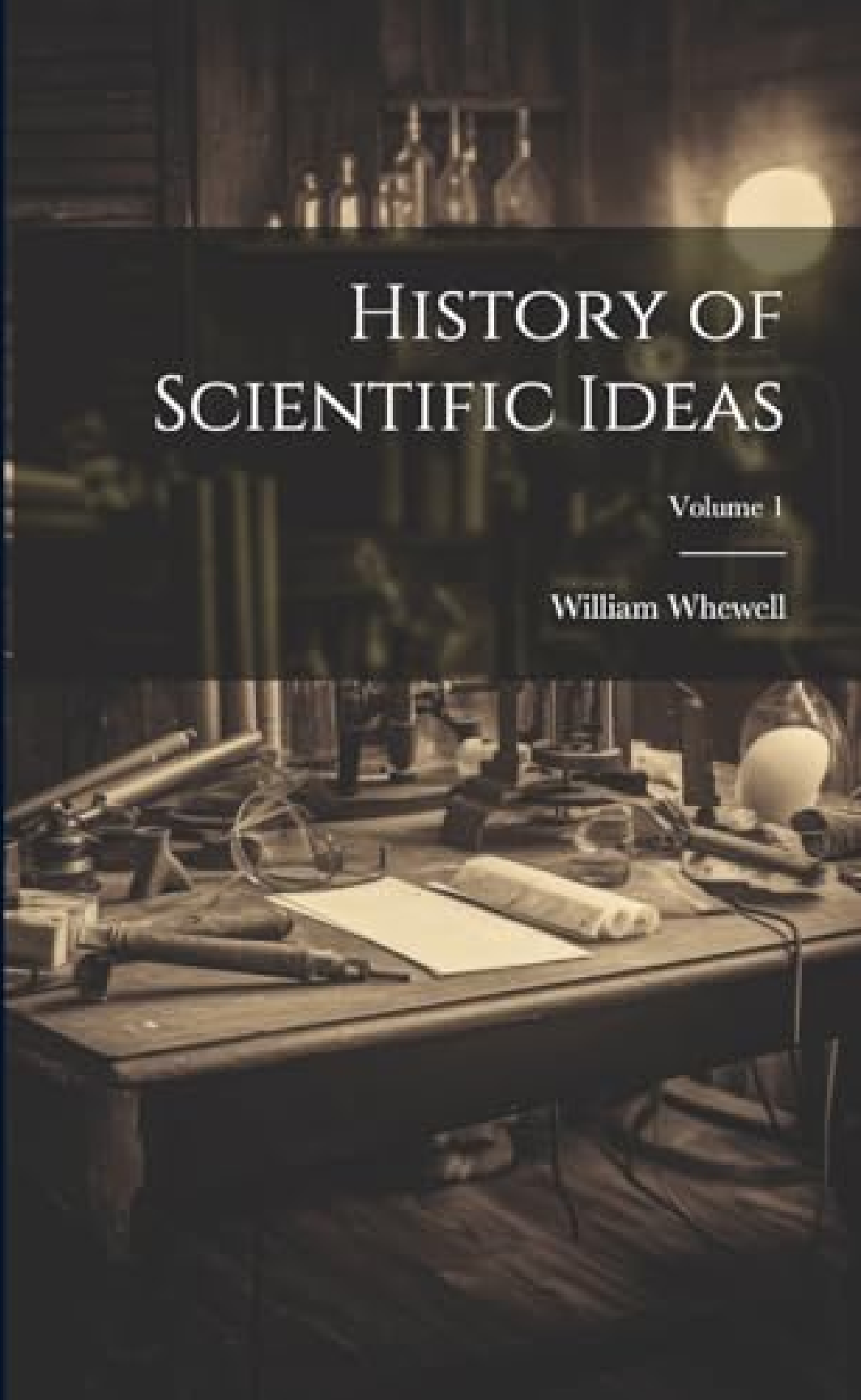


History of Scientific Ideas

Volume 1

William Whewell

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HISTORY
OF
SCIENTIFIC IDEAS.

VOLUME I.

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HISTORY

OF

SCIENTIFIC IDEAS.

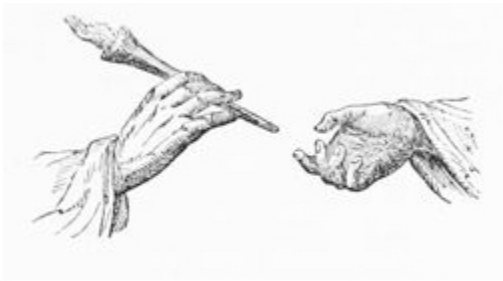
By WILLIAM WHEWELL, D.D.,

MASTER OF TRINITY COLLEGE, CAMBRIDGE, AND
CORRESPONDING MEMBER OF THE INSTITUTE OF FRANCE.

BEING THE FIRST PART OF THE PHILOSOPHY OF THE INDUCTIVE SCIENCES.

THE THIRD EDITION.

IN TWO VOLUMES.



ΛΑΜΠΑΔΙΑ ΕΧΟΝΤΕΣ ΔΙΑΔΩΣΟΥΣΙΝ ΑΛΛΗΛΟΙΣ

VOLUME I.

LONDON:
JOHN W. PARKER AND SON, WEST STRAND.
1858.

PREFACE TO THIS EDITION.

THE Chapters now offered to the Reader were formerly published as a portion of *The Philosophy of the Inductive Sciences, founded upon their History*: but the nature and subject of these Chapters are more exactly described by the present title, *The History of Scientific Ideas*. For this part of the work is mainly historical, and was, in fact, collected from the body of scientific literature, at the same time that the *History of the Inductive Sciences* was so collected. The present work contains the history of Science so far as it depends on *Ideas*; the former work contains the same history so far as it is derived from *Observation*. The leading features in *that* were Theories inferred from Facts; the leading features of *this* are Discussions of Theories tending to make them consistent with the conditions of human thought.

The Ideas of which the History is here given are mainly the following:
Space, *Time*, *Number*, *Motion*, *Cause*, *Force*,

Matter, *Medium*, *Intensity*, *Scale*, *Polarity*, *Element*, *Affinity*, *Substance*, *Atom*, *Symmetry*, *Likeness*, *Natural Classes*, *Species*, *Life*, *Function*, *Vital Forces*, *Final* vi *Causes*, *Historical Causation*, *Catastrophe and Uniformity*, *First Cause*.

The controversies to which the exact fixation of these Ideas and their properties have given occasion form a large and essential part of the History of Science: but they also form an important part of the Philosophy of Science, for no Philosophy of Science can be complete which does not solve the difficulties, antitheses, and paradoxes on which such controversies have turned. I have given a survey of such controversies, generally carried from their earliest origin to their latest aspect; and have stated what appeared to me the best solution of each problem. This has necessarily involved me in much thorny metaphysics; but such metaphysics is a necessary part of the progress of Science. The human mind deriving its knowledge of Truth from the observation of nature,

cannot evade the task of determining at every step how Truth is consistent with itself. This is the Metaphysics of Progressive Knowledge, and this is the matter of this present History.

Of the remaining part of what was formerly published as the Philosophy of the Inductive Sciences, an additional part, described in the Introduction to the present work, will shortly be published.

TRINITY LODGE,
May 24, 1858.

ERRATUM, p. 157, l. 11 from top, *for* sciences *read* science.

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THE

PHILOSOPHY

OF THE

INDUCTIVE SCIENCES.

INTRODUCTION.

INTRODUCTION.

THE PHILOSOPHY OF SCIENCE, if the phrase were to be understood in the comprehensive sense which most naturally offers itself to our thoughts, would imply nothing less than a complete insight into the essence and conditions of all real knowledge, and an exposition of the best methods for the discovery of new truths. We must narrow and lower this conception, in order to mould it into a form in which we may make it the immediate object of our labours with a good hope of success; yet still it may be a rational and useful undertaking, to endeavour to make some advance towards such a Philosophy, even according to the most ample conception of it which we can form. The present work has been written with a view of contributing, in some measure, however small it may be, towards

such an undertaking.

But in this, as in every attempt to advance beyond the position which we at present occupy, our hope of success must depend mainly upon our being able to profit, to the fullest extent, by the progress already made. We may best hope to understand the nature and conditions of real knowledge, by studying the nature and conditions of the most certain and stable portions of knowledge which we already possess: and we are most likely to learn the best methods of discovering truth, by examining how truths, now universally recognized, have really been discovered. Now there do exist among us doctrines of solid and acknowledged certainty, and truths of which the discovery has been received with universal applause. These constitute what we commonly term *Sciences*; and of these bodies of exact and enduring knowledge, we have within our 4 reach so large and varied a collection, that we may examine them, and the history of their formation, with a good prospect of deriving from the study

such instruction as we seek. We may best hope to make some progress towards the Philosophy of Science, by employing ourselves upon THE PHILOSOPHY OF THE SCIENCES.

The *Sciences* to which the name is most commonly and unhesitatingly given, are those which are concerned about the material world; whether they deal with the celestial bodies, as the sun and stars, or the earth and its products, or the elements; whether they consider the differences which prevail among such objects, or their origin, or their mutual operation. And in all these Sciences it is familiarly understood and assumed, that their doctrines are obtained by a common process of collecting general truths from particular observed facts, which process is termed *Induction*. It is further assumed that both in these and in other provinces of knowledge, so long as this process is duly and legitimately performed, the results will be real substantial truth. And although this process, with the conditions under which it is legitimate, and

the general laws of the formation of Sciences, will hereafter be subjects of discussion in this work, I shall at present so far adopt the assumption of which I speak, as to give to the Sciences from which our lessons are to be collected the name of *Inductive Sciences*. And thus it is that I am led to designate my work as THE PHILOSOPHY OF THE INDUCTIVE SCIENCES.

The views respecting the nature and progress of knowledge, towards which we shall be directed by such a course of inquiry as I have pointed out, though derived from those portions of human knowledge which are more peculiarly and technically termed *Sciences*, will by no means be confined, in their bearing, to the domain of such Sciences as deal with the material world, nor even to the whole range of Sciences now existing. On the contrary, we shall be led to believe that the nature of truth is in all subjects the same, and that its discovery involves, in all cases, the like 5 conditions. On one subject of human speculation after another, man's knowledge assumes that

exact and substantial character which leads us to term it *Science*; and in all these cases, whether inert matter or living bodies, whether permanent relations or successive occurrences, be the subject of our attention, we can point out certain universal characters which belong to truth, certain general laws which have regulated its progress among men. And we naturally expect that, even when we extend our range of speculation wider still, when we contemplate the world within us as well as the world without us, when we consider the thoughts and actions of men as well as the motions and operations of unintelligent bodies, we shall still find some general analogies which belong to the essence of truth, and run through the whole intellectual universe. Hence we have reason to trust that a just Philosophy of the Sciences may throw light upon the nature and extent of our knowledge in every department of human speculation. By considering what is the real import of our acquisitions, where they are certain and definite, we may learn something respecting

the difference between true knowledge and its precarious or illusory semblances; by examining the steps by which such acquisitions have been made, we may discover the conditions under which truth is to be obtained; by tracing the boundary-line between our knowledge and our ignorance, we may ascertain in some measure the extent of the powers of man's understanding.

But it may be said, in such a design there is nothing new; these are objects at which inquiring men have often before aimed. To determine the difference between real and imaginary knowledge, the conditions under which we arrive at truth, the range of the powers of the human mind, has been a favourite employment of speculative men from the earliest to the most recent times. To inquire into the original, certainty, and compass of man's knowledge, the limits of his capacity, the strength and weakness of his reason, has been the professed purpose of many of the most conspicuous and valued labours of the philosophers of 6 all periods

up to our own day. It may appear, therefore, that there is little necessity to add one more to these numerous essays; and little hope that any new attempt will make any very important addition to the stores of thought upon such questions, which have been accumulated by the profoundest and acutest thinkers of all ages.

To this I reply, that without at all disparaging the value or importance of the labours of those who have previously written respecting the foundations and conditions of human knowledge, it may still be possible to add something to what they have done. The writings of all great philosophers, up to our own time, form a series which is not yet terminated. The books and systems of philosophy which have, each in its own time, won the admiration of men, and exercised a powerful influence upon their thoughts, have had each its own part and functions in the intellectual history of the world; and other labours which shall succeed these may also have their proper office and useful

effect. We may not be able to do much, and yet still it may be in our power to effect something. Perhaps the very advances made by former inquirers may have made it possible for us, at present, to advance still further. In the discovery of truth, in the development of man's mental powers and privileges, each generation has its assigned part; and it is for us to endeavour to perform our portion of this perpetual task of our species. Although the terms which describe our undertaking may be the same which have often been employed by previous writers to express their purpose, yet our position is different from theirs, and thus the result may be different too. We have, as they had, to run our appropriate course of speculation with the exertion of our best powers; but our course lies in a more advanced part of the great line along which Philosophy travels from age to age. However familiar and old, therefore, be the design of such a work as this, the execution may have, and if it be performed in a manner suitable to the time, will have, something that is new and not unimportant.

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Indeed, it appears to be absolutely necessary, in order to check the prevalence of grave and pernicious errour, that the doctrines which are taught concerning the foundations of human knowledge and the powers of the human mind, should be from time to time revised and corrected or extended. Erroneous and partial views are promulgated and accepted; one portion of the truth is insisted upon to the undue exclusion of another; or principles true in themselves are exaggerated till they produce on men's minds the effect of falsehood. When evils of this kind have grown to a serious height, a *Reform* is requisite. The faults of the existing systems must be remedied by correcting what is wrong, and supplying what is wanting. In such cases, all the merits and excellencies of the labours of the preceding times do not supersede the necessity of putting forth new views suited to the emergency which has arrived. The new form which errour has assumed makes it proper to endeavour to give a new and

corresponding form to truth. Thus the mere progress of time, and the natural growth of opinion from one stage to another, leads to the production of new systems and forms of philosophy. It will be found, I think, that some of the doctrines now most widely prevalent respecting the foundations and nature of truth are of such a kind that a Reform is needed. The present age seems, by many indications, to be called upon to seek a sounder Philosophy of Knowledge than is now current among us. To contribute towards such a Philosophy is the object of the present work. The work is, therefore, like all works which take into account the most recent forms of speculative doctrine, invested with a certain degree of novelty in its aspect and import, by the mere time and circumstances of its appearance.

But, moreover, we can point out a very important peculiarity by which this work is, in its design, distinguished from preceding essays on like subjects; and this difference appears to be of such a kind as may well entitle us to expect some substantial

addition to our knowledge as the result of our labours. The peculiarity 8 of which I speak has already been announced;—it is this: that we purpose to collect our doctrines concerning the nature of knowledge, and the best mode of acquiring it, from a contemplation of the Structure and History of those Sciences (the Material Sciences), which are universally recognized as the clearest and surest examples of knowledge and of discovery. It is by surveying and studying the whole mass of such Sciences, and the various steps of their progress, that we now hope to approach to the true Philosophy of Science.

Now this, I venture to say, is a new method of pursuing the philosophy of human knowledge. Those who have hitherto endeavoured to explain the nature of knowledge, and the process of discovery, have, it is true, often illustrated their views by adducing special examples of truths which they conceived to be established, and by referring to the mode of their establishment. But these examples have, for

the most part, been taken at random, not selected according to any principle or system. Often they have involved doctrines so precarious or so vague that they confused rather than elucidated the subject; and instead of a single difficulty,—What is the nature of Knowledge? these attempts at illustration introduced two,—What was the true analysis of the Doctrines thus adduced? and,—Whether they might safely be taken as types of real Knowledge?

This has usually been the case when there have been adduced, as standard examples of the formation of human knowledge, doctrines belonging to supposed sciences other than the material sciences; doctrines, for example, of Political Economy, or Philology, or Morals, or the Philosophy of the Fine Arts. I am very far from thinking that, in regard to such subjects, there are no important truths hitherto established: but it would seem that those truths which have been obtained in these provinces of knowledge, have not yet been fixed by means of distinct and permanent

phraseology, and sanctioned by universal reception, and formed into a connected system, and traced through the steps of their gradual discovery and establishment, so as to make 9 them instructive examples of the nature and progress of truth in general. Hereafter we trust to be able to show that the progress of moral, and political, and philological, and other knowledge, is governed by the same laws as that of physical science. But since, at present, the former class of subjects are full of controversy, doubt, and obscurity, while the latter consist of undisputed truths clearly understood and expressed, it may be considered a wise procedure to make the latter class of doctrines the basis of our speculations. And on the having taken this course, is, in a great measure, my hope founded, of obtaining valuable truths which have escaped preceding inquirers.

But it may be said that many preceding writers on the nature and progress of knowledge have taken their examples abundantly from the Physical Sciences. It

would be easy to point out admirable works, which have appeared during the present and former generations, in which instances of discovery, borrowed from the Physical Sciences, are introduced in a manner most happily instructive. And to the works in which this has been done, I gladly give my most cordial admiration. But at the same time I may venture to remark that there still remains a difference between my design and theirs: and that I use the Physical Sciences as exemplifications of the general progress of knowledge in a manner very materially different from the course which is followed in works such as are now referred to. For the conclusions stated in the present work, respecting knowledge and discovery, are drawn from *a connected and systematic survey of the whole range of Physical Science and its History*; whereas, hitherto, philosophers have contented themselves with adducing detached examples of scientific doctrines, drawn from one or two departments of science. So long as we select our examples in this arbitrary and limited manner, we lose the best part of

that philosophical instruction, which the sciences are fitted to afford when we consider them as all members of one series, and as governed by rules which are the same for all. Mathematical and chemical truths, physical and physiological doctrines, the sciences of 10 classification and of causation, must alike be taken into our account, in order that we may learn what are the general characters of real knowledge. When our conclusions assume so comprehensive a shape that they apply to a range of subjects so vast and varied as these, we may feel some confidence that they represent the genuine form of universal and permanent truth. But if our exemplification is of a narrower kind, it may easily cramp and disturb our philosophy. We may, for instance, render our views of truth and its evidence so rigid and confined as to be quite worthless, by founding them too much on the contemplation of mathematical truth. We may overlook some of the most important steps in the general course of discovery, by fixing our attention too exclusively upon

some one conspicuous group of discoveries, as, for instance, those of Newton. We may misunderstand the nature of physiological discoveries, by attempting to force an analogy between them and discoveries of mechanical laws, and by not attending to the intermediate sciences which fill up the vast interval between these extreme terms in the series of material sciences. In these and in many other ways, a partial and arbitrary reference to the material sciences in our inquiry into human knowledge may mislead us; or at least may fail to give us those wider views, and that deeper insight, which should result from a systematic study of the whole range of sciences with this particular object.

The design of the following work, then, is to form a Philosophy of Science, by analyzing the substance and examining the progress of the existing body of the sciences. As a preliminary to this undertaking, a survey of the history of the sciences was necessary. This, accordingly, I have already performed; and the result of the labour thus

undertaken has been laid before the public as a *History of the Inductive Sciences*.

In that work I have endeavoured to trace the steps by which men acquired each main portion of that knowledge on which they now look with so much confidence and satisfaction. The events which that History relates, the speculations and controversies 11 which are there described, and discussions of the same kind, far more extensive, which are there omitted, must all be taken into our account at present, as the prominent and standard examples of the circumstances which attend the progress of knowledge. With so much of real historical fact before us, we may hope to avoid such views of the processes of the human mind as are too partial and limited, or too vague and loose, or too abstract and unsubstantial, to represent fitly the real forms of discovery and of truth.

Of former attempts, made with the same view of tracing the conditions of the progress of knowledge, that of Bacon is

perhaps the most conspicuous: and his labours on this subject were opened by his book on the *Advancement of Learning*, which contains, among other matter, a survey of the then existing state of knowledge. But this review was undertaken rather with the object of ascertaining in what quarters future advances were to be hoped for, than of learning by what means they were to be made. His examination of the domain of human knowledge was conducted rather with the view of discovering what remained undone, than of finding out how so much had been done. Bacon's survey was made for the purpose of tracing the boundaries, rather than of detecting the principles of knowledge. 'I will now attempt,' he says[1], 'to make a general and faithful perambulation of learning, with an inquiry what parts thereof lie fresh and waste, and not improved and converted by the industry of man; to the end that such a plot made and recorded to memory, may both minister light to any public designation, and also serve to excite voluntary endeavours.' Nor will it be foreign to our scheme also

hereafter to examine with a like purpose the frontier-line of man's intellectual estate. But the object of our perambulation in the first place, is not so much to determine the extent of the field, as the sources of its fertility. We would learn by what plan and rules 12 of culture, conspiring with the native forces of the bounteous soil, those rich harvests have been produced which fill our garners. Bacon's maxims, on the other hand, respecting the mode in which he conceived that knowledge was thenceforth to be cultivated, have little reference to the failures, still less to the successes, which are recorded in his Review of the learning of his time. His precepts are connected with his historical views in a slight and unessential manner. His Philosophy of the Sciences is not collected from the Sciences which are noticed in his survey. Nor, in truth, could this, at the time when he wrote, have easily been otherwise. At that period, scarce any branch of physics existed as a science, except Astronomy. The rules which Bacon gives for the conduct of scientific researches are obtained, as it were, by divination, from

the contemplation of subjects with regard to which no sciences as yet were. His instances of steps rightly or wrongly made in this path, are in a great measure cases of his own devising. He could not have exemplified his Aphorisms by references to treatises then extant, on the laws of nature; for the constant burden of his exhortation is, that men up to his time had almost universally followed an erroneous course. And however we may admire the sagacity with which he pointed the way along a better path, we have this great advantage over him;—that we can interrogate the many travellers who since his time have journeyed on this road. At the present day, when we have under our notice so many sciences, of such wide extent, so well established; a Philosophy of the Sciences ought, it must seem, to be founded, not upon conjecture, but upon an examination of many instances;—should not consist of a few vague and unconnected maxims, difficult and doubtful in their application, but should form a system of which every part has been repeatedly confirmed and

verified.

[1] *Advancement of Learning*, b. i. p. 74.

This accordingly it is the purpose of the present work to attempt. But I may further observe, that as my hope of making any progress in this undertaking is 13 founded upon the design of keeping constantly in view the whole result of the past history and present condition of science, I have also been led to draw my lessons from my examples in a manner more systematic and regular, as appears to me, than has been done by preceding writers. Bacon, as I have just said, was led to his maxims for the promotion of knowledge by the sagacity of his own mind, with little or no aid from previous examples. Succeeding philosophers may often have gathered useful instruction from the instances of scientific truths and discoveries which they adduced, but their conclusions were drawn from their instances casually and arbitrarily. They took for their moral any which the story might suggest. But such a proceeding as this cannot suffice for us, whose aim is to obtain a consistent

body of philosophy from a contemplation of the whole of Science and its History. For our purpose it is necessary to resolve scientific truths into their conditions and ingredients, in order that we may see in what manner each of these has been and is to be provided, in the cases which we may have to consider. This accordingly is necessarily the first part of our task:—*to analyse Scientific Truth into its Elements*. This attempt will occupy the earlier portion of the present work; and will necessarily be somewhat long, and perhaps, in many parts, abstruse and uninviting. The risk of such an inconvenience is inevitable; for the inquiry brings before us many of the most dark and entangled questions in which men have at any time busied themselves. And even if these can now be made clearer and plainer than of yore, still they can be made so only by means of mental discipline and mental effort. Moreover this analysis of scientific truth into its elements contains much, both in its principles and in its results, different from the doctrines most generally prevalent among us in recent times: but on that very

account this analysis is an essential part of the doctrines which I have now to lay before the reader: and I must therefore crave his indulgence towards any portion of it which may appear to him obscure or repulsive. 14

There is another circumstance which may tend to make the present work less pleasing than others on the same subject, in the nature of the examples of human knowledge to which I confine myself; all my instances being, as I have said, taken from the material sciences. For the truths belonging to these sciences are, for the most part, neither so familiar nor so interesting to the bulk of readers as those doctrines which belong to some other subjects. Every general proposition concerning politics or morals at once stirs up an interest in men's bosoms, which makes them listen with curiosity to the attempts to trace it to its origin and foundation. Every rule of art or language brings before the mind of cultivated men subjects of familiar and agreeable thought, and is dwelt upon with

pleasure for its own sake, as well as on account of the philosophical lessons which it may convey. But the curiosity which regards the truths of physics or chemistry, or even of physiology or astronomy, is of a more limited and less animated kind. Hence, in the mode of inquiry which I have prescribed to myself, the examples which I have to adduce will not amuse and relieve the reader's mind as much as they might do, if I could allow myself to collect them from the whole field of human knowledge. They will have in them nothing to engage his fancy, or to warm his heart. I am compelled to detain the listener in the chilly air of the external world, in order that we may have the advantage of full daylight.

But although I cannot avoid this inconvenience, so far as it is one, I hope it will be recollected how great are the advantages which we obtain by this restriction. We are thus enabled to draw all our conclusions from doctrines which are universally allowed to be eminently certain, clear, and definite. The portions of

knowledge to which I refer are well known, and well established among men. Their names are familiar, their assertions uncontested. Astronomy and Geology, Mechanics and Chemistry, Optics and Acoustics, Botany and Physiology, are each recognized as large and substantial collections of undoubted truths. Men are 15 wont to dwell with pride and triumph on the acquisitions of knowledge which have been made in each of these provinces; and to speak with confidence of the certainty of their results. And all can easily learn in what repositories these treasures of human knowledge are to be found. When, therefore, we begin our inquiry from such examples, we proceed upon a solid foundation. With such a clear ground of confidence, we shall not be met with general assertions of the vagueness and uncertainty of human knowledge; with the question, What truth is, and How we are to recognize it; with complaints concerning the hopelessness and unprofitableness of such researches. We have, at least, a definite problem before us. We have to examine the

structure and scheme, not of a shapeless mass of incoherent materials, of which we doubt whether it be a ruin or a natural wilderness, but of a fair and lofty palace, still erect and tenanted, where hundreds of different apartments belong to a common plan, where every generation adds something to the extent and magnificence of the pile. The certainty and the constant progress of science are things so unquestioned, that we are at least engaged in an intelligible inquiry, when we are examining the grounds and nature of that certainty, the causes and laws of that progress.

To this inquiry, then, we now proceed. And in entering upon this task, however our plan or our principles may differ from those of the eminent philosophers who have endeavoured, in our own or in former times, to illustrate or enforce the philosophy of science, we most willingly acknowledge them as in many things our leaders and teachers. Each reform must involve its own peculiar principles, and the result of our

attempts, so far as they lead to a result, must be, in some respects, different from those of former works. But we may still share with the great writers who have treated this subject before us, their spirit of hope and trust, their reverence for the dignity of the subject, their belief in the vast powers and boundless destiny of man. And we may once more venture to use the 16 words of hopeful exhortation, with which the greatest of those who have trodden this path encouraged himself and his followers when he set out upon his way.

‘Concerning ourselves we speak not; but as touching the matter which we have in hand, this we ask;—that men deem it not to be the setting up an Opinion, but the performing of a Work: and that they receive this as a certainty; that we are not laying the foundations of any sect or doctrine, but of the profit and dignity of mankind. Furthermore, that being well disposed to what shall advantage themselves, and putting off factions and prejudices, they take common counsel with us, to the end

that being by these our aids and appliances freed and defended from wanderings and impediments, they may lend their hands also to the labours which remain to be performed: and yet further, that they be of good hope; neither imagine to themselves this our Reform as something of infinite dimension, and beyond the grasp of mortal man, when in truth it is the end and true limit of infinite errour; and is by no means unmindful of the condition of mortality and humanity, not confiding that such a thing can be carried to its perfect close in the space of one single age, but assigning it as a task to a succession of generations.'

[The Philosophy of the Inductive Sciences, according to our view, must be founded upon the History of such Sciences; which history we have attempted in a former work. The events of that history may be described generally as the rise of Theories out of Facts. But besides this, which we may term the *external* history of Theories, there is an internal history of Theories, namely, the series of steps by

which the human mind becomes capable of forming each Theory. Hence to complete the History of the Sciences as derived from Facts, we require a history of the Ideas by which such derivation has been made possible: and thus, the *First Part* of our Philosophy must be a *History of Scientific Ideas*;—a labour no less historical than our former work, and concerned with the same events; but which has been purposely kept separate during the 17 composition, in order that it might be afterwards presented in a more systematic form, which I have here attempted to do.

Scientific Ideas are the Conditions of the derivation of Sciences from Facts: but can any method or methods be given by which such a Derivation can be ensured, or at least, aided? Many such methods have been proposed; of which the most celebrated is the *Novum Organon* of Bacon, of which the title was intended to imply that its scope goes much beyond the *Organon* of Aristotle. With the experience of the formation of Science which the world has had since

Bacon's time, it does not appear presumptuous to suppose that we can now improve or correct his methods; nor to term such an attempt *Novum Organon Renovatum*.

The Philosophy of the Inductive Sciences, then, contains these two parts, *The History of Scientific Ideas*, and the *Novum Organon Renovatum*.]

THE

PHILOSOPHY

OF THE

INDUCTIVE SCIENCES.

PART I.

HISTORY OF SCIENTIFIC IDEAS.

[We have just spoken of *Theories* and *Facts*, of *Ideas* and *Facts*, and of *Inductive* Sciences, which imply the opposition of *Induction* and *Deduction*. The explanation of these antitheses must be the starting point of our Philosophy.]

[Knowledge grows, and] through the ages one increasing purpose runs,

And the thoughts of men are widen’d with the process of the Suns.

BOOK I.

OF IDEAS IN GENERAL.

Quæ adhuc inventa sunt in Scientiis, ea hujusmodi sunt ut Notionibus Vulgaribus fere subjaceant: ut vero ad interiora et remotiora Naturæ penetretur, necesse est ut tam NOTIONES quam AXIOMATA magis certâ et munitâ viâ a particularibus abstrahantur; atque omnino melior et certior intellectûs adoperatio in usum veniat.

BACON, *Nov. Org.*, Lib. 1. Aphor. xviii.

BOOK I.

OF IDEAS IN GENERAL.

CHAPTER I.

Of the Fundamental Antithesis of Philosophy.

Sect. 1.—*Thoughts and Things.*

IN order that we may do something towards determining the nature and conditions of human knowledge, (which I have already stated as the purpose of this work,) I shall have to refer to an antithesis or opposition, which is familiar and generally recognized, and in which the distinction of the things

opposed to each other is commonly considered very clear and plain. I shall have to attempt to make this opposition sharper and stronger than it is usually conceived, and yet to shew that the distinction is far from being so clear and definite as it is usually assumed to be: I shall have to point the contrast, yet shew that the things which are contrasted cannot be separated:—I must explain that the antithesis is constant and essential, but yet that there is no fixed and permanent line dividing its members. I may thus appear, in different parts of my discussion, to be proceeding in opposite directions, but I hope that the reader who gives me a patient attention will see that both steps lead to the point of view to which I wish to lead him.

The antithesis or opposition of which I speak is denoted, with various modifications, by various pairs of terms: I shall endeavour to shew the connexion of these different modes of expression, and I will begin with that form which is the simplest and most idiomatic. 24

The simplest and most idiomatic expression of the antithesis to which I refer is that in which we oppose to each other THINGS and THOUGHTS. The opposition is familiar and plain. Our thoughts are something which belongs to ourselves; something which takes place within us; they are what we think; they are actions of our minds. Things, on the contrary, are something different from ourselves and independent of us; something which is without us; they *are*; we see them, touch them, and thus know that they exist; but we do not make them by seeing or touching them, as we make our *Thoughts* by thinking them; we are passive, and *Things* act upon our organs of perception.

Now what I wish especially to remark is this: that in all human KNOWLEDGE both Thoughts and Things are concerned. In every part of my knowledge there must be some *thing* about which I know, and an internal act of *me* who know. Thus, to take simple yet definite parts of our knowledge, if I know that a solar year consists of 365

days, or a lunar month of 30 days, I know something about the sun or the moon; namely, that those objects perform certain revolutions and go through certain changes, in those numbers of days; but I count such numbers and conceive such revolutions and changes by acts of my own thoughts. And both these elements of my knowledge are indispensable. If there were not such external Things as the sun and the moon I could not have any knowledge of the progress of time as marked by them. And however regular were the motions of the sun and moon, if I could not count their appearances and combine their changes into a cycle, or if I could not understand this when done by other men, I could not know anything about a year or a month. In the former case I might be conceived as a human being, possessing the human powers of thinking and reckoning, but kept in a dark world with nothing to mark the progress of existence. The latter is the case of brute animals, which see the sun and moon, but do not know how many days make a month or a year, because they have

not human powers of thinking and reckoning. 25

The two elements which are essential to our knowledge in the above cases, are necessary to human knowledge in all cases. In all cases, Knowledge implies a combination of Thoughts and Things. Without this combination, it would not be Knowledge. Without Thoughts, there could be no connexion; without Things, there could be no reality. Thoughts and Things are so intimately combined in our Knowledge, that we do not look upon them as distinct. One single act of the mind involves them both; and their contrast disappears in their union.

But though Knowledge requires the union of these two elements, Philosophy requires the separation of them, in order that the nature and structure of Knowledge may be seen. Therefore I begin by considering this separation. And I now proceed to speak of another way of looking at the antithesis of which I have spoken; and which I may, for

the reasons which I have just mentioned, call the FUNDAMENTAL ANTITHESIS OF PHILOSOPHY.

Sect. 2.—*Necessary and Experiential Truths.*

MOST persons are familiar with the distinction of *necessary* and *contingent* truths. The former kind are Truths which cannot but be true; as that 19 and 11 make 30;—that parallelograms upon the same base and between the same parallels are equal;—that all the angles in the same segment of a circle are equal. The latter are Truths which *it happens* (*contingit*) are true; but which, for anything which we can see, might have been otherwise; as that a lunar month contains 30 days, or that the stars revolve in circles round the pole. The latter kind of Truths are learnt by experience, and hence we may call them *Truths of Experience*, or, for the sake of convenience, *Experiential* Truths, in contrast with Necessary Truths.

Geometrical propositions are the most manifest examples of Necessary Truths. All

persons who have read and understood the elements of geometry, know that the propositions above stated (that parallelograms 26 upon the same base and between the same parallels are equal; that all the angles in the same segment of a circle are equal,) are necessarily true; not only they are true, but they *must be* true. The meaning of the terms being understood, and the proof being gone through, the truth of the propositions must be assented to. We learn these propositions to be true by demonstrations deduced from definitions and axioms; and when we have thus learnt them, we see that they could not be otherwise. In the same manner, the truths which concern numbers are necessary truths: 19 and 11 not only *do* make 30, but *must* make that number, and cannot make anything else. In the same manner, it is a necessary truth that half the sum of two numbers added to half their difference is equal to the greater number.

It is easy to find examples of Experiential Truths;—propositions which we know to be

true, but know by experience only. We know, in this way, that salt will dissolve in water; that plants cannot live without light; —in short, we know in this way all that we do know in chemistry, physiology, and the material sciences in general. I take the *Sciences* as my examples of human knowledge, rather than the common truths of daily life, or moral or political truths; because, though the latter are more generally interesting, the former are much more definite and certain, and therefore better starting-points for our speculations, as I have already said. And we may take elementary astronomical truths as the most familiar examples of Experiential Truths in the domain of science.

With these examples, the distinction of Necessary and Experiential Truths is, I hope, clear. The former kind, we see to be true by thinking about them, and see that they could not be otherwise. The latter kind, men could never have discovered to be true without looking at them; and having so discovered them, still no one will pretend to

say they might not have been otherwise. For aught we can see, the astronomical truths which express the motions and periods of the sun, moon and stars, might have been otherwise. If we had been placed in another part of the solar system, our 27 experiential truths respecting days, years, and the motions of the heavenly bodies, would have been other than they are, as we know from astronomy itself.

It is evident that this distinction of Necessary and Experiential Truths involves the same antithesis which we have already considered;—the antithesis of Thoughts and Things. Necessary Truths are derived from our own Thoughts: Experiential truths are derived from our observation of Things about us. The opposition of Necessary and Experiential Truths is another aspect of the Fundamental Antithesis of Philosophy.

Sect. 3.—*Deduction and Induction.*

I HAVE already stated that geometrical truths are established by demonstrations

deduced from definitions and axioms. The term *Deduction* is specially applied to such a course of demonstration of truths from definitions and axioms. In the case of the parallelograms upon the same base and between the same parallels, we prove certain triangles to be equal, by supposing them placed so that their two bases have the same extremities; and hence, referring to an Axiom respecting straight lines, we infer that the bases coincide. We combine these equal triangles with other equal spaces, and in this way make up both the one and the other of the parallelograms, in such a manner as to shew that they are equal. In this manner, going on step by step, deducing the equality of the triangles from the axiom, and the equality of the parallelograms from that of the triangles, we travel to the conclusion. And this process of successive deduction is the scheme of all geometrical proof. We begin with Definitions of the notions which we reason about, and with Axioms, or self-evident truths, respecting these notions; and we get, by reasoning from these, other

truths which are demonstratively evident; and from these truths again, others of the same kind, and so on. We begin with our own Thoughts, which supply us with Axioms to start from; and we reason from these, till we come to propositions 28 which are applicable to the Things about us; as for instance, the propositions respecting circles and spheres applicable to the motions of the heavenly bodies. This is *Deduction*, or *Deductive Reasoning*.

Experiential truths are acquired in a very different way. In order to obtain such truths, we begin with Things. In order to learn how many days there are in a year, or in a lunar month, we must begin by observing the sun and the moon. We must observe their changes day by day, and try to make the cycle of change fit into some notion of number which we supply from our own Thoughts. We shall find that a cycle of 30 days nearly will fit the changes of phase of the moon;—that a cycle of 365 days nearly will fit the changes of daily motion of the sun. Or, to go on to experiential truths

of which the discovery comes within the limits of the history of science—we shall find (as Hipparchus found) that the unequal motion of the sun among the stars, such as observation shews it to be, may be fitly represented by the notion of an *eccentric*;—a circle in which the sun has an equable annual motion, the spectator not being in the center of the circle. Again, in the same manner, at a later period, Kepler started from more exact observations of the sun, and compared them with a supposed motion in a certain ellipse; and was able to shew that, not a circle about an eccentric point, but an ellipse, supplied the mode of conception which truly agreed with the motion of the sun about the earth; or rather, as Copernicus had already shewn, of the earth about the sun. In such cases, in which truths are obtained by beginning from observation of external things and by finding some notion with which the Things, as observed, agree, the truths are said to be obtained by *Induction*. The process is an *Inductive Process*.

The contrast of the Deductive and Inductive process is obvious. In the former, we proceed at each step from general truths to particular applications of them; in the latter, from particular observations to a general truth which includes them. In the former case we may be said to reason *downwards*, in the latter case, 29 *upwards*; for general notions are conceived as standing above particulars. Necessary truths are proved, like arithmetical sums, by adding together the portions of which they consist. An inductive truth is proved, like the guess which answers a riddle, by its agreeing with the facts described. Demonstration is irresistible in its effect on the belief, but does not produce surprize, because all the steps to the conclusion are exhibited, before we arrive at the conclusion. Inductive inference is not demonstrative, but it is often more striking than demonstrative reasoning, because the intermediate links between the particulars and the inference are not shewn. Deductive truths are the results of relations among our own Thoughts. Inductive truths are relations

which we discern among existing Things; and thus, this opposition of Deduction and Induction is again an aspect of the Fundamental Antithesis already spoken of.

Sect. 4.—*Theories and Facts.*

GENERAL experiential Truths, such as we have just spoken of, are called *Theories*, and the particular observations from which they are collected, and which they include and explain, are called *Facts*. Thus Hipparchus's doctrine, that the sun moves in an eccentric about the earth, is *his Theory* of the Sun, or the *Eccentric Theory*. The doctrine of Kepler, that the Earth moves in an Ellipse about the Sun, is *Kepler's Theory* of the Earth, the Elliptical Theory. Newton's doctrine that this elliptical motion of the Earth about the Sun is produced and governed by the Sun's attraction upon the Earth, is the *Newtonian* theory, the *Theory of Attraction*. Each of these Theories was accepted, because it included, connected and explained the *Facts*; the Facts being, in the two former cases, the motions of the Sun as observed; and in the

other case, the elliptical motion of the Earth as known by Kepler's Theory. This antithesis of *Theory* and *Fact* is included in what has just been said of Inductive Propositions. A Theory is an Inductive Proposition, and the Facts 30 are the particular observations from which, as I have said, such Propositions are inferred by Induction. The Antithesis of Theory and Fact implies the fundamental Antithesis of Thoughts and Things; for a Theory (that is, a true Theory) may be described as a Thought which is contemplated distinct from Things and seen to agree with them; while a Fact is a combination of our Thoughts with Things in so complete agreement that we do not regard them as separate.

Thus the antithesis of Theory and Fact involves the antithesis of Thoughts and Things, but is not identical with it. Facts involve Thoughts, for we know Facts only by thinking about them. The Fact that the year consists of 365 days; the Fact that the month consists of 30 days, cannot be known to us, except we have the Thoughts of Time,

Number and Recurrence. But these Thoughts are so familiar, that we have the fact in our mind as a simple Thing without attending to the Thought which it involves. When we mould our Thoughts into a Theory, we consider the thought as distinct from the Facts; but yet, though distinct, not independent of them; for it is a true Theory, only by including and agreeing with the Facts.

Sect. 5.—*Ideas and Sensations.*

We have just seen that the antithesis of Theory and Fact, although it involves the antithesis of Thoughts and Things, is not identical with it. There are other modes of expression also, which involve the same Fundamental Antithesis, more or less modified. Of these, the pair of words which in their relations appear to separate the members of the antithesis most distinctly are *Ideas* and *Sensations*. We see and hear and touch external things, and thus perceive them by our senses; but in perceiving them, we connect the impressions of sense

according to relations of space, time, number, likeness, cause, &c. Now some at least of these kinds of connexion, as space, time, number, may be contemplated distinct from the things to which they are applied; and so contemplated, I term them *Ideas*. And 31 the other element, the impressions upon our senses which they connect, are called *Sensations*.

I term space, time, cause, &c., *Ideas*, because they are general relations among our sensations, apprehended by an act of the mind, not by the senses simply. These relations involve something beyond what the senses alone could furnish. By the sense of sight we see various shades and colours and shapes before us, but the *outlines* by which they are separated into distinct objects of definite forms, are the work of the mind itself. And again, when we conceive visible things, not only as surfaces of a certain form, but as *solid bodies*, placed at various distances in space, we again exert an act of the mind upon them. When we see a body move, we see it move in a path or

orbit, but this orbit is not itself seen; it is constructed by the mind. In like manner when we see the motions of a needle towards a magnet, we do not *see* the attraction or force which produces the effects; but we infer the force, by having in our minds the Idea of Cause. Such acts of thought, such *Ideas*, enter into our perceptions of external things.

But though our perceptions of external things involve some act of the mind, they must involve something else besides an act of the mind. If we must exercise an act of thought in order to see force exerted, or orbits described by bodies in motion, or even in order to see bodies existing in space, and to distinguish one kind of object from another, still the act of thought alone does not make the Bodies. There must be something besides, *on which* the thought is exerted. A colour, a form, a sound, are not produced by the mind, however they may be moulded, combined, and interpreted by our mental acts. A philosophical poet has spoken of

All the world

Of eye and ear, both what they half create,

And what perceive.

But it is clear, that though they *half* create, they do not wholly create: there must be an external world of colour and sound to give impressions to the eye and ear, as well as internal powers by which we perceive 32 what is offered to our organs. The mind is in some way passive as well as active: there are objects without as well as faculties within;—Sensations, as well as acts of Thought.

Indeed this is so far generally acknowledged, that according to common apprehension, the mind is passive *rather* than active in acquiring the knowledge which it receives concerning the material world. Its sensations are generally considered more distinct than its operations. The world without is held to be more clearly real than the faculties within. That there is something different from ourselves,

something external to us, something independent of us, something which no act of our minds can make or can destroy, is held by all men to be at least as evident, as that our minds can exert any effectual process in modifying and appreciating the impressions made upon them. Most persons are more likely to doubt whether the mind be always actively applying Ideas to the objects which it perceives, than whether it perceive them passively by means of Sensations.

But yet a little consideration will show us that an activity of the mind, and an activity according to certain Ideas, is requisite in all our knowledge of external objects. We see objects, of various solid forms, and at various distances from us. But we do not thus perceive them by sensation alone. Our visual impressions cannot, of themselves, convey to us a knowledge of solid form, or of distance from us. Such knowledge is inferred from what we see:—inferred by conceiving the objects as existing in space, and by applying to them the Idea of Space.

Again:—day after day passes, till they make up a year: but we do not know that the days are 365, except we count them; and thus apply to them our Idea of Number. Again:—we see a needle drawn to a magnet: but, in truth, the *drawing* is what we cannot see. We see the needle move, and infer the attraction, by applying to the fact our Idea of Force, as the cause of motion. Again:—we see two trees of different kinds; but we cannot know that they are so, except by applying to them our Idea of the resemblance 33 and difference which makes kinds. And thus Ideas, as well as Sensations, necessarily enter into all our knowledge of objects: and these two words express, perhaps more exactly than any of the pairs before mentioned, that Fundamental Antithesis, in the union of which, as I have said, all knowledge consists.

Sect. 6.—*Reflexion and Sensation.*

It will hereafter be my business to show what the Ideas are, which thus enter into our knowledge; and how each Idea has

been, as a matter of historical fact, introduced into the Science to which it especially belongs. But before I proceed to do this, I will notice some other terms, besides the phrases already noticed, which have a reference, more or less direct, to the Fundamental Antithesis of Ideas and Sensations. I will mention some of these, in order that if they should come under the reader's notice, he may not be perplexed as to their bearing upon the view here presented to him.

The celebrated doctrine of Locke, that all our 'Ideas,' (that is, in his use of the word, all our objects of thinking,) come from Sensation or Reflexion, will naturally occur to the reader as connected with the antithesis of which I have been speaking. But there is a great difference between Locke's account of Sensation and Reflexion, and our view of Sensation and Ideas. He is speaking of the origin of our knowledge;—we, of its nature and composition. He is content to say that all the knowledge which we do not receive directly by Sensation, we

obtain by Reflex Acts of the mind, which make up his Reflexion. But we hold that there is no Sensation without an act of the mind, and that the mind's activity is not only reflexly exerted upon itself, but directly upon objects, so as to perceive in them connexions and relations which are not Sensations. He is content to put together, under the name of Reflexion, everything in our knowledge which is not Sensation: we are to attempt to analyze all that is not Sensation; not only to say it consists of Ideas, but 34 to point out what those Ideas are, and to show the mode in which each of them enters into our knowledge. His purpose was, to prove that there are no Ideas, except the reflex acts of the mind: our endeavour will be to show that the acts of the mind, both direct and reflex, are governed by certain Laws, which may be conveniently termed Ideas. His procedure was, to deny that any knowledge could be derived from the mind alone: our course will be, to show that in every part of our most certain and exact knowledge, those who have added to our knowledge in

every age have referred to principles which the mind itself supplies. I do not say that my view is contrary to his: but it is altogether different from his. If I grant that all our knowledge comes from Sensation and Reflexion, still my task then is only begun; for I want further to determine, in each science, what portion comes, not from mere Sensation, but from those Ideas by the aid of which either Sensation or Reflexion can lead to Science.

Locke's use of the word 'idea' is, as the reader will perceive, different from ours. He uses the word, as he says, which 'serves best to stand for whatsoever is the object of the understanding when a man thinks.' 'I have used it,' he adds, 'to express whatever is meant by *phantasm*, *notion*, *species*, or whatever it is to which the mind can be employed about in thinking.' It might be shown that this separation of the *mind itself* from the ideal *objects* about which it is employed in thinking, may lead to very erroneous results. But it may suffice to observe that we use the word *Ideas*, in the

manner already explained, to express that element, supplied by the mind itself, which must be combined with Sensation in order to produce knowledge. For us, Ideas are not Objects of Thought, but rather Laws of Thought. Ideas are not synonymous with Notions; they are Principles which give to our Notions whatever they contain of truth. But our use of the term *Idea* will be more fully explained hereafter. 35

Sect. 7.—*Subjective and Objective.*

THE Fundamental Antithesis of Philosophy of which I have to speak has been brought into great prominence in the writings of modern German philosophers, and has conspicuously formed the basis of their systems. They have indicated this antithesis by the terms *subjective* and *objective*. According to the technical language of old writers, a thing and its qualities are described as *subject* and *attributes*; and thus a man's faculties and acts are attributes of which he is the *subject*. The mind is the *subject* in which ideas inhere. Moreover, the

man's faculties and acts are employed upon external *objects*; and from objects all his sensations arise. Hence the part of a man's knowledge which belongs to his own mind, is *subjective*: that which flows in upon him from the world external to him, is *objective*. And as in man's contemplation of nature, there is always some act of thought which depends upon himself, and some matter of thought which is independent of him, there is, in every part of his knowledge, a subjective and an objective element. The combination of the two elements, the subjective or ideal, and the objective or observed, is necessary, in order to give us any insight into the laws of nature. But different persons, according to their mental habits and constitution, may be inclined to dwell by preference upon the one or the other of these two elements. It may perhaps interest the reader to see this difference of intellectual character illustrated in two eminent men of genius of modern times, Göthe and Schiller.

Göthe himself gives us the account to

which I refer, in his history of the progress of his speculations concerning the Metamorphosis of Plants; a mode of viewing their structure by which he explained, in a very striking and beautiful manner, the relations of the different parts of a plant to each other; as has been narrated in the *History of the Inductive Sciences*. Göthe felt a delight in the passive contemplation of nature, unmingled with the desire of reasoning and theorizing; a delight such as naturally belongs to those poets who 36 merely embody the images which a fertile genius suggests, and do not mix with these pictures, judgments and reflexions of their own. Schiller, on the other hand, both by his own strong feeling of the value of a moral purpose in poetry, and by his adoption of a system of metaphysics in which the subjective element was made very prominent, was well disposed to recognize fully the authority of ideas over external impressions.

Göthe for a time felt a degree of estrangement towards Schiller, arising from

this contrariety in their views and characters. But on one occasion they fell into discussion on the study of natural history; and Göthe endeavoured to impress upon his companion his persuasion that nature was to be considered, not as composed of detached and incoherent parts, but as active and alive, and unfolding herself in each portion, in virtue of principles which pervade the whole. Schiller objected that no such view of the objects of natural history had been pointed out by observation, the only guide which the natural historians recommended; and was disposed on this account to think the whole of their study narrow and shallow. 'Upon this,' says Göthe, 'I expounded to him, in as lively a way as I could, the metamorphosis of plants, drawing on paper for him, as I proceeded, a diagram to represent that general form of a plant which shows itself in so many and so various transformations. Schiller attended and understood; and, accepting the explanation, he said, "This is not observation, but an idea." I replied,' adds Göthe, 'with some degree of irritation;

for the point which separated us was most luminously marked by this expression: but I smothered my vexation, and merely said, “I was happy to find that I had got ideas without knowing it; nay, that I saw them before my eyes.”’ Göthe then goes on to say, that he had been grieved to the very soul by maxims promulgated by Schiller, that no observed fact ever could correspond with an idea. Since he himself loved best to wander in the domain of external observation, he had been led to look with repugnance and hostility upon anything which professed to depend upon ideas. ‘Yet,’ he 37 observes, ‘it occurred to me that if my Observation was identical with his Idea, there must be some common ground on which we might meet.’ They went on with their mutual explanations, and became intimate and lasting friends. ‘And thus,’ adds the poet, by means of that mighty and interminable controversy between *object* and *subject*, we two concluded an alliance which remained unbroken, and produced much benefit to ourselves and others.’

The general diagram of a plant, of which Göthe here speaks, must have been a combination of lines and marks expressing the relations of position and equivalence among the elements of vegetable forms, by which so many of their resemblances and differences may be explained. Such a symbol is not an Idea in that general sense in which we propose to use the term, but is a particular modification of the general Ideas of symmetry, developement, and the like; and we shall hereafter see, according to the phraseology which we shall explain in the next chapter, how such a diagram might express the *ideal conception* of a plant.

The antithesis of *subjective* and *objective* is very familiar in the philosophical literature of Germany and France; nor is it uncommon in any age of our own literature. But though efforts have recently been made to give currency among us to this phraseology, it has not been cordially received, and has been much complained of as not of obvious meaning. Nor is the complaint without ground: for when we regard the mind as the

subject in which ideas inhere, it becomes for us an *object*, and the antithesis vanishes. We are not so much accustomed to use *subject* in this sense, as to make it a proper contrast to *object*. The combination '*ideal* and *objective*,' would more readily convey to a modern reader the opposition which is intended between the ideas of the mind itself, and the objects which it contemplates around it.

To the antitheses already noticed—Thoughts and Things; Necessary and Experiential Truths; Deduction and Induction; Theory and Fact; Ideas and Sensations; Reflexion and Sensation; Subjective and 38 Objective; we may add others, by which distinctions depending more or less upon the fundamental antithesis have been denoted. Thus we speak of the *internal* and *external* sources of our knowledge; of the world *within* and the world *without* us; of *Man* and *Nature*. Some of the more recent metaphysical writers of Germany have divided the universe into the *Me* and *Not-me* (Ich and Nicht-ich). Upon

such phraseology we may observe, that to have the fundamental antithesis of which we speak really understood, is of the highest consequence to philosophy, but that little appears to be gained by expressing it in any novel manner. The most weighty part of the philosopher's task is to analyze the operations of the mind; and in this task, it can aid us but little to call it, instead of the *mind*, the *subject*, or the *me*.

Sect. 8.—*Matter and Form.*

THERE are some other ways of expressing, or rather of illustrating, the fundamental antithesis, which I may briefly notice. The antithesis has been at different times presented by means of various images. One of the most ancient of these, and one which is still very instructive, is that which speaks of Sensations as the *Matter*, and Ideas as the *Form*, of our knowledge; just as ivory is the matter, and a cube the form, of a die. This comparison has the advantage of showing that two elements of an antithesis which cannot be separated in fact, may yet be

advantageously separated in our reasonings. For Matter and Form cannot by any means be detached from each other. All matter must have some form; all form must be the form of some material thing. If the ivory be not a cube, it must have a spherical or some other form. And the cube, in order to be a cube, must be of some material;—if not of ivory, of wood, or stone, for instance, A figure without matter is merely a geometrical conception;—a modification of the idea of space. Matter without figure is a mere abstract term;—a supposed union of certain sensible qualities which, so insulated 39 from others, cannot exist. Yet the distinction of Matter and Form is real; and, as a subject of contemplation, clear and plain. Nor is the distinction by any means useless. The speculations which treat of the two subjects, Matter and Figure, are very different. Matter is the subject of the sciences of Mechanics and Chemistry; Figure, of Geometry. These two classes of Sciences have quite different sets of principles. If we refuse to consider the Matter and the Form of bodies separately,

because we cannot exhibit Matter and Form separately, we shut the door to all philosophy on such subjects. In like manner, though Sensations and Ideas are necessarily united in all our knowledge, they can be considered as distinct; and this distinction is the basis of all philosophy concerning knowledge.

This illustration of the relation of Ideas and Sensations may enable us to estimate a doctrine which has been put forwards at various times. In a certain school of speculators there has existed a disposition to derive all our Ideas from our Sensations, the term *Idea*, being, in this school, used in its wider sense, so as to include all modifications and limitations of our Fundamental Ideas. The doctrines of this school have been summarily expressed by saying that 'Every Idea is a transformed Sensation.' Now, even supposing this assertion to be exactly true, we easily see, from what has been said, how little we are likely to answer the ends of philosophy by putting forward such a maxim as one of

primary importance. For we might say, in like manner, that every statue is but a transformed block of marble, or every edifice but a collection of transformed stones. But what would these assertions avail us, if our object were to trace the rules of art by which beautiful statues were formed, or great works of architecture erected? The question naturally occurs, What is the nature, the principle, the law of this Transformation? In what faculty resides the transforming power? What train of ideas of beauty, and symmetry, and stability, in the mind of the statuary or the architect, has produced those great works which 40 mankind look upon as among their most valuable possessions;—the Apollo of the Belvidere, the Parthenon, the Cathedral of Cologne? When this is what we want to know, how are we helped by learning that the Apollo is of Parian marble, or the Cathedral of basaltic stone? We must know much more than this, in order to acquire any insight into the principles of statuary or of architecture. In like manner, in order that we may make any progress in the

philosophy of knowledge, which is our purpose, we must endeavour to learn something further respecting ideas than that they are transformed sensations, even if they were this.

But, in reality, the assertion that our ideas are transformed sensations, is erroneous as well as frivolous. For it conveys, and is intended to convey, the opinion that our sensations have one form which properly belongs to them; and that, in order to become ideas, they are converted into some other form. But the truth is, that our sensations, of themselves, without some act of the mind, such as involves what we have termed an Idea, have no form. We cannot see one object without the idea of space; we cannot see two without the idea of resemblance or difference; and space and difference are not sensations. Thus, if we are to employ the metaphor of Matter and Form, which is implied in the expression to which I have referred, our sensations, from their first reception, have their Form not *changed*, but *given* by our Ideas. Without the

relations of thought which we here term *Ideas*, the sensations are matter without form. Matter without form cannot exist: and in like manner sensations cannot become perceptions of objects, without some formative power of the mind. By the very act of being received as perceptions, they have a formative power exercised upon them, the operation of which might be expressed, by speaking of them, not as *transformed*, but simply as *formed*;—as invested with form, instead of being the mere formless material of perception. The word *inform*, according to its Latin etymology, at first implied this process by which matter is 41 invested with form. Thus Virgil[1] speaks of the thunderbolt as *informed* by the hands of Brontes, and Steropes, and Pyracmon. And Dryden introduces the word in another place:—

> Let others better mould the running mass
>
> Of metals, or *inform* the breathing brass.

Even in this use of the word, the form is something superior to the brute manner,

and gives it a new significance and purpose. And hence the term is again used to denote the effect produced by an intelligent principle of a still higher kind:—

> . . . . He *informed*
>
> This ill-shaped body with a daring soul.

And finally even the soul itself, in its original condition, is looked upon as matter, when viewed with reference to education and knowledge, by which it is afterwards moulded; and hence these are, in our language, termed *information*. If we confine ourselves to the first of these three uses of the term, we may correct the erroneous opinion of which we have just been speaking, and retain the metaphor by which it is expressed, by saying, that ideas are not *transformed*, but *informed* sensations.

[1]

> Ferrum exercebant vasto Cyclopes in Antro
>
> Brontesque Steropesque et nudus membra Pyracmon;

His informatum manibus, jam parte polita

Fulmen erat.—*Æn.* viii. 424.

Sect. 9.—*Man the Interpreter of Nature.*

THERE is another image by which writers have represented the acts of thought through which knowledge is obtained from the observation of the external world. Nature is the Book, and Man is the *Interpreter*. The facts of the external world are marks, in which man discovers a meaning, and so reads them. Man is the Interpreter of Nature, and Science is the right Interpretation. And this image also is, in many respects, 42 instructive. It exhibits to us the necessity of both elements;—the marks which man has to look at, and the knowledge of the alphabet and language which he must possess and apply before he can find any meaning in what he sees. Moreover this image presents to us, as the ideal element, an activity of the mind of that very kind which we wish to point out.

Indeed the illustration is rather an example than a comparison of the composition of our knowledge. The letters and symbols which are presented to the Interpreter are really objects of sensation: the notion of letters as signs of words, the notion of connexions among words by which they have meaning, really are among our Ideas;—*Signs* and *Meaning* are Ideas, supplied by the mind, and added to all that sensation can disclose in any collection of visible marks. The Sciences are not figuratively, but really, Interpretations of Nature. But this image, whether taken as example or comparison, may serve to show both the opposite character of the two elements of knowledge, and their necessary combination, in order that there may be knowledge.

This illustration may also serve to explain another point in the conditions of human knowledge which we shall have to notice:—namely, the very different degrees in which, in different cases, we are conscious of the mental act by which our sensations are converted into knowledge. For the same

difference occurs in reading an inscription. If the inscription were entire and plain, in a language with which we were familiar, we should be unconscious of any mental act in reading it. We should seem to collect its meaning by the sight alone. But if we had to decipher an ancient inscription, of which only imperfect marks remained, with a few entire letters among them, we should probably make several suppositions as to the mode of reading it, before we found any mode which was quite successful; and thus, our guesses, being separate from the observed facts, and at first not fully in agreement with them, we should be clearly aware that the conjectured meaning, on the one hand, and the observed marks on the other, were distinct things, though these 43 two things would become united as elements of one act of knowledge when we had hit upon the right conjecture.

Sect. 10.—*The Fundamental Antithesis inseparable.*

THE illustration just referred to, as well as

other ways of considering the subject, may help us to get over a difficulty which at first sight appears perplexing. We have spoken of the common opposition of *Theory* and *Fact* as important, and as involving what we have called the Fundamental Antithesis of Philosophy. But after all, it may be asked, Is this distinction of Theory and Fact really tenable? Is it not often difficult to say whether a special part of our knowledge is a Fact or a Theory? Is it a Fact or a Theory that the stars revolve round the pole? Is it a Fact or a Theory that the earth is a globe revolving on its axis? Is it a Fact or a Theory that the earth travels in an ellipse round the sun? Is it a Fact or a Theory that the sun attracts the earth? Is it a Fact or a Theory that the loadstone attracts the needle? In all these cases, probably some persons would answer one way, and some persons the other. There are many persons by whom the doctrine of the globular form of the earth, the doctrine of the earth's elliptical orbit, the doctrine of the sun's attraction on the earth, would be called *theories*, even if they allowed them to be true theories. But yet if

each of these propositions be true, is it not a *fact*? And even with regard to the simpler facts, as the motion of the stars round the pole, although this may be a Fact to one who has watched and measured the motions of the stars, one who has not done this, and who has only carelessly looked at these stars from time to time, may naturally speak of the circles which the astronomer makes them describe as Theories. It would seem, then, that we cannot in such cases expect general assent, if we say, *This is a Fact and not a Theory*, or *This is a Theory and not a Fact*. And the same is true in a vast range of cases. It would seem, therefore, that we cannot rest any reasoning upon this distinction of Theory 44 and Fact; and we cannot avoid asking whether there is any real distinction in this antithesis, and if so, what it is.

To this I reply: the distinction between Theory (that is, true Theory) and Fact, is this: that in Theory the Ideas are considered as distinct from the Facts: in Facts, though Ideas may be involved, they are not, in our

apprehension, separated from the sensations. In a Fact, the Ideas are applied so readily and familiarly, and incorporated with the sensations so entirely, that we do not see *them*, we see *through them*. A person who carefully notes the motion of a star all night, sees the circle which it describes, as he sees the star, though the circle is, really, a result of his own Ideas. A person who has in his mind the measures of different lines and countries on the earth's surface, and who can put them, together into one conception, finds that they can make no figure but a globular one: to him, the earth's globular form is a Fact, as much as the square form of his chamber. A person to whom the grounds of believing the earth to travel round the sun are as familiar as the grounds for believing the movements of the mail-coaches in this country, looks upon the former event as a Fact, just as he looks upon the latter events as Facts. And a person who, knowing the Fact of the earth's annual motion, refers it distinctly to its mechanical cause, conceives the sun's attraction as a Fact, just as he conceives as a Fact, the

action of the wind which turns the sails of a mill. He cannot *see* the force in either case; he supplies it out of his own Ideas. And thus, a true Theory is a Fact; a Fact is a familiar Theory. That which is a Fact under one aspect, is a Theory under another. The most recondite Theories when firmly established are Facts: the simplest Facts involve something of the nature of Theory. Theory and Fact correspond, in a certain degree, with Ideas and Sensations, as to the nature of their opposition. But the Facts are Facts, so far as the Ideas have been combined with the Sensations and absorbed in them: the Theories are Theories, so far as the Ideas are kept distinct from the Sensations, and so far as it is 45 considered still a question whether those can be made to agree with these.

We may, as I have said, illustrate this matter by considering man as *interpreting* the phenomena which he sees. He often interprets without being aware that he does so. Thus when we see the needle move towards the magnet, we assert that the

magnet exercises an attractive force on the needle. But it is only by an interpretative act of our own minds that we ascribe this motion to attraction. That, in this case, a force is exerted—something of the nature of the pull which we could apply by our own volition—is our interpretation of the phenomena; although we may be conscious of the act of interpretation, and may then regard the attraction as a Fact.

Nor is it in such cases only that we interpret phenomena in our own way, without being conscious of what we do. We see a tree at a distance, and judge it to be a chestnut or a lime; yet this is only an inference from the colour or form of the mass according to preconceived classifications of our own. Our lives are full of such unconscious interpretations. The farmer recognizes a good or a bad soil; the artist a picture of a favourite master; the geologist a rock of a known locality, as we recognize the faces and voices of our friends; that is, by judgments formed on what we see and hear; but judgments in

which we do not analyze the steps, or distinguish the inference from the appearance. And in these mixtures of observation and inference, we speak of the judgment thus formed, as a Fact directly observed.

Even in the case in which our perceptions appear to be most direct, and least to involve any interpretations of our own,—in the simple process of seeing,—who does not know how much we, by an act of the mind, add to that which our senses receive? Does any one fancy that he sees a solid cube? It is easy to show that the solidity of the figure, the relative position of its faces and edges to each other, are inferences of the spectator; no more conveyed to his conviction by the eye alone, than they would be if he were looking at 46 a painted representation of a cube. The scene of nature is a picture without depth of substance, no less than the scene of art; and in the one case as in the other, it is the mind which, by an act of its own, discovers that colour and shape denote distance and solidity. Most men are

unconscious of this perpetual habit of reading the language of the external world, and translating as they read. The draughtsman, indeed, is compelled, for his purposes, to return back in thought from the solid bodies which he has inferred, to the shapes of surface which he really sees. He knows that there is a mask of theory over the whole face of nature, if it be *theory* to infer more than we *see*. But other men, unaware of this masquerade, hold it to be a fact that they see cubes and spheres, spacious apartments and winding avenues. And these things are facts to them, because they are unconscious of the mental operation by which they have penetrated nature's disguise.

And thus, we still have an intelligible distinction of Fact and Theory, if we consider Theory as a conscious, and Fact as an unconscious inference, from the phenomena which are presented to our senses.

But still, Theory and Fact, Inference and

Perception, Reasoning and Observation, are antitheses in none of which can we separate the two members by any fixed and definite line.

Even the simplest terms by which the antithesis is expressed cannot be separated. Ideas and Sensations, Thoughts and Things, Subject and Object, cannot in any case be applied absolutely and exclusively. Our Sensations require Ideas to bind them together, namely, Ideas of space, time, number, and the like. If not so bound together, Sensations do not give us any apprehension of Things or Objects. All Things, all Objects, must exist in space and in time—must be one or many. Now space, time, number, are not Sensations or Things. They are something different from, and opposed to Sensations and Things. We have termed them Ideas. It may be said they are *Relations* of Things, or of Sensations. But granting this form of expression, still a *Relation* is not a Thing or a 47 Sensation; and therefore we must still have another and opposite element, along with our

Sensations. And yet, though we have thus these two elements in every act of perception, we cannot designate any portion of the act as absolutely and exclusively belonging to one of the elements. Perception involves Sensation, along with Ideas of time, space, and the like; or, if any one prefers the expression, we may say, Perception involves Sensations along with the apprehension of Relations. Perception is Sensation, along with such Ideas as make Sensation into an apprehension of Things or Objects.

And as Perception of Objects implies Ideas,—as Observation implies Reasoning;—so, on the other hand, Ideas cannot exist where Sensation has not been; Reasoning cannot go on when there has not been previous Observation. This is evident from the necessary order of developement of the human faculties. Sensation necessarily exists from the first moments of our existence, and is constantly at work. Observation begins before we can suppose the existence of any Reasoning which is not involved in

Observation. Hence, at whatever period we consider our Ideas, we must consider them as having been already engaged in connecting our Sensations, and as having been modified by this employment. By being so employed, our Ideas are unfolded and defined; and such developement and definition cannot be separated from the Ideas themselves. We cannot conceive space, without boundaries or forms; now Forms involve Sensations. We cannot conceive time, without events which mark the course of time; but events involve Sensations. We cannot conceive number, without conceiving things which are numbered; and Things imply sensations. And the forms, things, events, which are thus implied in our Ideas, having been the objects of Sensation constantly in every part of our life, have modified, unfolded, and fixed our Ideas, to an extent which we cannot estimate, but which we must suppose to be essential to the processes which at present go on in our minds. We cannot say that Objects create Ideas; for to perceive Objects we must already have

Ideas. But we may 48 say, that Objects and the constant Perception of Objects have so far modified our Ideas, that we cannot, even in thought, separate our Ideas from the perception of Objects.

We cannot say of any Ideas, as of the Idea of space, or time, or number, that they are absolutely and exclusively Ideas. We cannot conceive what space, or time, or number, would be in our minds, if we had never perceived any Thing or Things in space or time. We cannot conceive ourselves in such a condition as never to have perceived any Thing or Things in space or time. But, on the other hand, just as little can we conceive ourselves becoming acquainted with space and time or numbers as objects of Sensation. We cannot reason without having the operations of our minds affected by previous Sensations; but we cannot conceive Reasoning to be merely a series of Sensations. In order to be used in Reasoning, Sensation must become Observation; and, as we have seen, Observation already involves Reasoning. In

order to be connected by our Ideas, Sensations must be Things or Objects, and Things or Objects already include Ideas. And thus, none of the terms by which the fundamental antithesis is expressed can be absolutely and exclusively applied.

I will make a remark suggested by the views which have thus been presented. Since, as we have just seen, none of the terms which express the fundamental antithesis can be applied absolutely and exclusively, the absolute application of the antithesis in any particular case can never be a conclusive or immoveable principle. This remark is the more necessary to be borne in mind, as the terms of this antithesis are often used in a vehement and peremptory manner. Thus we are often told that such a thing is *a Fact*; A FACT and not a Theory, with all the emphasis which, in speaking or writing, tone or italics or capitals can give. We see from what has been said, that when this is urged, before we can estimate the truth, or the value of the assertion, we must ask to whom is it a

Fact? what habits of thought, what previous information, what Ideas does it imply, to conceive the Fact as a Fact? 49 Does not the apprehension of the Fact imply assumptions which may with equal justice be called Theory, and which are perhaps false Theory? in which case, the Fact is no Fact. Did not the ancients assert it as a Fact, that the earth stood still, and the stars moved? and can any Fact have stronger apparent evidence to justify persons in asserting it emphatically than this had?

These remarks are by no means urged in order to show that no Fact can be certainly known to be true; but only, to show that no Fact can be certainly shown to be a Fact, merely by calling it a Fact, however emphatically. There is by no means any ground of general skepticism with regard to truth, involved in the doctrine of the necessary combination of two elements in all our knowledge. On the contrary, Ideas are requisite to the essence, and Things to the reality of our knowledge in every case. The proportions of Geometry and

Arithmetic are examples of knowledge respecting our Ideas of space and number, with regard to which there is no room for doubt. The doctrines of Astronomy are examples of truths not less certain respecting the Facts of the external world.

Sect. 11.—*Successive Generalization.*

IN the preceding pages we have been led to the doctrine, that though, in the Antithesis of Theory and Fact, there is involved an essential opposition; namely the opposition of the thoughts within us and the phenomena without us; yet that we cannot distinguish and define the members of this antithesis separately. Theories become Facts, by becoming certain and familiar: and thus, as our knowledge becomes more sure and more extensive, we are constantly transferring to the class of facts, opinions which were at first regarded as theories.

Now we have further to remark, that in the progress of human knowledge respecting any branch of speculation, there

may be several such steps in succession, each depending upon and including the preceding. 50 The theoretical views which one generation of discoverers establishes, become the facts from which the next generation advances to new theories. As men rise from the particular to the general, so, in the same manner, they rise from what is general to what is more general. Each induction supplies the materials of fresh inductions; each generalization, with all that it embraces in its circle, may be found to be but one of many circles, comprehended within the circuit of some wider generalization.

This remark has already been made, and illustrated, in the *History of the Inductive Sciences*[2]; and, in truth, the whole of the history of science is full of suggestions and exemplifications of this course of things. It may be convenient, however, to select a few instances which may further explain and confirm this view of the progress of scientific knowledge.

[2] *Hist. Inductive Sciences*, b. vii. c. ii. sect.

5.

The most conspicuous instance of this succession is to be found in that science which has been progressive from the beginning of the world to our own times, and which exhibits by far the richest collection of successive discoveries: I mean Astronomy. It is easy to see that each of these successive discoveries depended on those antecedently made, and that in each, the truths which were the highest point of the knowledge of one age were the fundamental basis of the efforts of the age which came next. Thus we find, in the days of Greek discovery, Hipparchus and Ptolemy combining and explaining the particular *facts* of the motion of the sun, moon, and planets, by means of the *theory* of epicycles and eccentrics;—a highly important step, which gave an intelligible connexion and rule to the motions of each of these luminaries. When these cycles and epicycles, thus truly representing the apparent motions of the heavenly bodies, had accumulated to an inconvenient

amount, by the discovery of many inequalities in the observed motions, Copernicus showed that their effects might all be more simply included, by making the sun the center of motion of the planets, instead of 51 the earth. But in this new view, he still retained the epicycles and eccentrics which governed the motion of each body. Tycho Brahe's observations, and Kepler's calculations, showed that, besides the vast number of facts which the epicyclical theory could account for, there were some which it would not exactly include, and Kepler was led to the persuasion that the planets move in ellipses. But this view of motion was at first conceived by Kepler as a modification of the conception of epicycles. On one occasion he blames himself for not sooner seeing that such a modification was possible. 'What an absurdity on my part!' he cries[3]; 'as if libration in the diameter of the epicycle might not come to the same thing as motion in the ellipse.' But again; Kepler's *laws* of the elliptical motion of the planets were established; and these laws immediately

became the *facts* on which the mathematicians had to found their mechanical theories. From these facts, Newton, as we have related, proved that the central force of the sun retains the planets in their orbits, according to the law of the inverse square of the distance. The same *law* was shown to prevail in the gravitation of the earth. It was shown, too, by induction from the motions of Jupiter and Saturn, that the planets attract each other; by calculations from the figure of the earth, that the parts of the earth attract each other; and, by considering the course of the tides, that the sun and moon attract the waters of the ocean. And all these curious discoveries being established as *facts*, the subject was ready for another step of generalization. By an unparalleled rapidity in the progress of discovery in this case, not only were all the inductions which we have first mentioned made by one individual, but the new advance, the higher flight, the closing victory, fell to the lot of the same extraordinary person.

[3] *Hist. Inductive Sciences*, b. v. c. iv. sect. 3.

The attraction of the sun upon the planets, of the moon upon the earth, of the planets on each other, of the parts of the earth on themselves, of the sun and 52 moon upon the ocean;—all these truths, each of itself a great discovery, were included by Newton in the higher *generalization*, of the universal gravitation of matter, by which each particle is drawn to every other according to the law of the inverse square: and thus this long advance from discovery to discovery, from truths to truths, each justly admired when new, and then rightly used as old, was closed in a worthy and consistent manner, by a truth which is the most worthy admiration, because it includes all the researches of preceding ages of Astronomy.

We may take another example of a succession of this kind from the history of a science, which, though it has made wonderful advances, has not yet reached its goal, as physical astronomy appears to have done, but seems to have before it a long

prospect of future progress. I now refer to Chemistry, in which I shall try to point out how the preceding discoveries afforded the materials of the succeeding; although this subordination and connexion is, in this case, less familiar to men's minds than in Astronomy, and is, perhaps, more difficult to present in a clear and definite shape. Sylvius saw, in the facts which occur, when an acid and an alkali are brought together, the evidence that they neutralize each other. But cases of neutralization, and acidification, and many other effects of mixture of the ingredients of bodies, being thus viewed as *facts*, had an aspect of unity and law given them by Geoffroy and Bergman[4], who introduced the *conception* of the Chemical Affinity or Elective Attraction, by which certain elements select other elements, as if by preference. That combustion, whether a chemical union or a chemical separation of ingredients, is of the same nature with acidification, was the doctrine of Beccher and Stahl, and was soon established as a truth which must form a part of every succeeding physical theory.

That the rules of affinity and chemical composition may include gaseous elements, was established by Black and Cavendish. And all these truths, thus brought to light by 53 chemical discoverers,—affinity, the identity of acidification and combustion, the importance of gaseous elements,—along with all the facts respecting the weight of ingredients and compounds which the balance disclosed,—were taken up, connected, and included as *particulars* in the oxygen *theory* of Lavoisier. Again, the results of this theory, and the quantity of the several ingredients which entered into each compound—(such results, for the most part, being now no longer mere theoretical speculations, but recognized facts)—were the *particulars* from which Dalton derived that wide law of chemical combination which we term the Atomic *Theory*. And this law, soon generally accepted among chemists, is already in its turn become one of the *facts* included in Faraday's *Theory* of the identity of Chemical Affinity and Electric Attraction.

[4] *Hist. Inductive Sciences*, b. xiv. c. iii.

It is unnecessary to give further exemplifications of this constant ascent from one step to a higher; this perpetual conversion of true theories into the materials of other and wider theories. It will hereafter be our business to exhibit, in a more full and formal manner, the mode in which this principle determines the whole scheme and structure of all the most exact sciences. And thus, beginning with the facts of sense, we gradually climb to the highest forms of human knowledge, and obtain from experience and observation a vast collection of the most wide and elevated truths.

There are, however, truths of a very different kind, to which we must turn our attention, in order to pursue our researches respecting the nature and grounds of our knowledge. But before we do this, we must notice one more feature in that progress of science which we have already in part described.

CHAPTER II.

Of Technical Terms.

1. It has already been stated that we gather knowledge from the external world, when we are able to apply, to the facts which we observe, some ideal conception, which gives unity and connexion to multiplied and separate perceptions. We have also shown that our conceptions, thus verified by facts, may themselves be united and connected by a new bond of the same nature; and that man may thus have to pursue his way from truth to truth through a long progression of discoveries, each resting on the preceding, and rising above it.

Each of these steps, in succession, is recorded, fixed, and made available, by some peculiar form of words; and such

words, thus rendered precise in their meaning, and appropriated to the service of science, we may call *Technical Terms*. It is in a great measure by inventing such Terms that men not only best express the discoveries they have made, but also enable their followers to become so familiar with these discoveries, and to possess them so thoroughly, that they can readily use them in advancing to ulterior generalizations.

Most of our ideal conceptions are described by exact and constant words or phrases, such as those of which we here speak. We have already had occasion to employ many of these. Thus we have had instances of technical Terms expressing geometrical conceptions, as *Ellipsis*, *Radius Vector*, *Axis*, *Plane*, the Proportion of the *Inverse Square*, and the like. Other Terms have described mechanical conceptions, as *Accelerating Force* and *Attraction*. Again, chemistry exhibits (as do all sciences) a series of Terms which mark the steps of our 55 progress. The views of the first real founders of the science are recorded by the

Terms which are still in use, *Neutral Salts*, *Affinity*, and the like. The establishment of Dalton's theory has produced the use of the word *Atom* in a peculiar sense, or of some other word, as *Proportion*, in a sense equally technical. And Mr. Faraday has found it necessary, in order to expound his electro-chemical theory, to introduce such terms as *Anode* and *Cathode*, *Anïon* and *Cathïon*.

2. I need not adduce any further examples, for my object at present is only to point out the use and influence of such language: its rules and principles I shall hereafter try, in some measure, to fix. But what we have here to remark is, the extraordinary degree in which the progress of science is facilitated, by thus investing each new discovery with a compendious and steady form of expression. These terms soon become part of the current language of all who take an interest in speculation. However strange they may sound at first, they soon grow familiar in our ears, and are used without any effort, or any recollection of the difficulty they once involved. They

become as common as the phrases which express our most frequent feelings and interests, while yet they have incomparably more precision than belongs to any terms which express feelings; and they carry with them, in their import, the results of deep and laborious trains of research. They convey the mental treasures of one period to the generations that follow; and laden with this, their precious freight, they sail safely across gulfs of time in which empires have suffered shipwreck, and the languages of common life have sunk into oblivion. We have still in constant circulation among us the Terms which belong to the geometry, the astronomy, the zoology, the medicine of the Greeks, and the algebra and chemistry of the Arabians. And we can in an instant, by means of a few words, call to our own recollection, or convey to the apprehension of another person, phenomena and relations of phenomena in optics, mineralogy, chemistry, which are so complex and abstruse, that it might seem to require the utmost subtlety of the human mind to 56 grasp them, even if that were made the sole

object of its efforts. By this remarkable effect of Technical Language, we have the results of all the labours of past times not only always accessible, but so prepared that we may (provided we are careful in the use of our instrument) employ what is really useful and efficacious for the purpose of further success, without being in any way impeded or perplexed by the length and weight of the chain of past connexions which we drag along with us.

By such means,—by the use of the Inductive Process, and by the aid of Technical Terms,—man has been constantly advancing in the path of scientific truth. In a succeeding part of this work we shall endeavour to trace the general rules of this advance, and to lay down the maxims by which it may be most successfully guided and forwarded. But in order that we may do this to the best advantage, we must pursue still further the analysis of knowledge into its elements; and this will be our employment in the first part of the work.

CHAPTER III.

Of Necessary Truths.

1. EVERY advance in human knowledge consists, as we have seen, in adapting new ideal conceptions to ascertained facts, and thus in superinducing the Form upon the Matter, the active upon the passive processes of our minds. Every such step introduces into our knowledge an additional portion of the ideal element, and of those relations which flow from the nature of Ideas. It is, therefore, important for our purpose to examine more closely this element, and to learn what the relations are which may thus come to form part of our knowledge. An inquiry into those Ideas which form the foundations of our sciences; —into the reality, independence, extent, and principal heads of the knowledge which we

thus acquire; is a task on which we must now enter, and which will employ us for several of the succeeding Books.

In this inquiry our object will be to pass in review all the most important Fundamental Ideas which our sciences involve; and to prove more distinctly in reference to each, what we have already asserted with regard to all, that there are everywhere involved in our knowledge acts of the mind as well as impressions of sense; and that our knowledge derives, from these acts, a generality, certainty, and evidence which the senses could in no degree have supplied. But before I proceed to do this in particular cases, I will give some account of the argument in its general form.

We have already considered the separation of our knowledge into its two elements,—Impressions of Sense and Ideas, —as evidently indicated by this; that all knowledge possesses characters which neither of these 58 elements alone could bestow. Without our ideas, our sensations

could have no connexion; without external impressions, our ideas would have no reality; and thus both ingredients of our knowledge must exist.

2. There is another mode in which the distinction of the two elements of knowledge appears, as I have already said (c. i. sect. 2): namely in the distinction of *necessary*, and *contingent* or *experiential*, truths. For of these two classes of truths, the difference arises from this;—that the one class derives its nature from the one, and the other from the other, of the two elements of knowledge. I have already stated briefly the difference of these two kinds of truths:—namely, that the former are truths which, we see, must be true:—the latter are true, but so far as we can see, might be otherwise. The former are true necessarily and universally: the latter are learnt from experience and limited by experience. Now with regard to the former kind of truths, I wish to show that the universality and necessity which distinguish them can by no means be derived from

experience; that these characters do in reality flow from the ideas which these truths involve; and that when the necessity of the truth is exhibited in the way of logical demonstration, it is found to depend upon certain fundamental principles, (Definitions and Axioms,) which may thus be considered as expressing, in some measure, the essential characters of our ideas. These fundamental principles I shall afterwards proceed to discuss and to exhibit in each of the principal departments of science.

I shall begin by considering Necessary Truths more fully than I have yet done. As I have already said, necessary truths are those in which we not only learn, that the proposition *is* true, but see that it *must be* true; in which the negation of the truth is not only false, but impossible; in which we cannot, even by an effort of imagination, or in a supposition, conceive the reverse of that which is asserted.

3. That there are such truths cannot be

doubted. We may take, for example, all relations of number. Three and Two added together make Five. We cannot 59 conceive it to be otherwise. We cannot, by any freak of thought, imagine Three and Two to make Seven.

It may be said that this assertion merely expresses what we mean by our words; that it is a matter of definition; that the proposition is an identical one.

But this is by no means so. The definition of Five is not Three and Two, but Four and One. How does it appear that Three and Two is the same number as Four and One? It is evident that it is so; but *why* is it evident?—not because the proposition is identical; for if that were the reason, all numerical propositions must be evident for the same reason. If it be a matter of definition that 3 and 2 make 5, it must be a matter of definition that 39 and 27 make 66. But who will say that the definition of 66 is 39 and 27? Yet the magnitude of the numbers can make no difference in the

ground of the truth. How do we know that the product of 13 and 17 is 4 less than the product of 15 and 15? We see that it is so, if we perform certain operations by the rules of arithmetic; but how do we know the truth of the rules of arithmetic? If we divide 123375 by 987 according to the process taught us at school, how are we assured that the result is correct, and that the number 125 thus obtained is really the number of times one number is contained in the other?

The correctness of the rule, it may be replied, can be rigorously demonstrated. It can be shown that the process must inevitably give the true quotient.

Certainly this can be shown to be the case. And precisely because it *can* be shown that the result must be true, we have here an example of a necessary truth; and this truth, it appears, is not *therefore* necessary because it is itself evidently identical, however it may be possible to prove it by reducing it to evidently identical propositions. And the same is the case with

all other numerical propositions; for, as we have said, the nature of all of them is the same.

Here, then, we have instances of truths which are not only true, but demonstrably and necessarily true. Now such truths are, in this respect at least, altogether 60 different from truths, which, however certain they may be, are learnt to be so only by the evidence of observation, interpreted, as observation must be interpreted, by our own mental faculties. There is no difficulty in finding examples of these merely observed truths. We find that sugar dissolves in water, and forms a transparent fluid, but no one will say that we can see any reason beforehand why the result *must* be so. We find that all animals which chew the cud have also the divided hoof; but could any one have predicted that this would be universally the case? or supposing the truth of the rule to be known, can any one say that he cannot conceive the facts as occurring otherwise? Water expands when it crystallizes, some other substances contract

in the same circumstances; but can any one know that this will be so otherwise than by observation? We have here propositions *rigorously* true, (we will assume,) but can any one say they are *necessarily* true? These, and the great mass of the doctrines established by induction, are actual, but so far as we can see, accidental laws; results determined by some unknown selection, not demonstrable consequences of the essence of things, inevitable and perceived to be inevitable. According to the phraseology which has been frequently used by philosophical writers, they are *contingent*, not necessary truths.

It is requisite to insist upon this opposition, because no insight can be obtained into the true nature of knowledge, and the mode of arriving at it, by any one who does not clearly appreciate the distinction. The separation of truths which are learnt by observation, and truths which can be seen to be true by a pure act of thought, is one of the first and most essential steps in our examination of the

nature of truth, and the mode of its discovery. If any one does not clearly comprehend this distinction of necessary and contingent truths, he will not be able to go along with us in our researches into the foundations of human knowledge; nor, indeed, to pursue with success any speculation on the subject. But, in fact, this distinction is one that can hardly fail to be at once understood. It 61 is insisted upon by almost all the best modern, as well as ancient, metaphysicians[5], as of primary importance. And if any person does not fully apprehend, at first, the different kinds of truth thus pointed out, let him study, to some extent, those sciences which have necessary truth for their subject, as geometry, or the properties of numbers, so as to obtain a familiar acquaintance with such truth; and he will then hardly fail to see how different the evidence of the propositions which occur in these sciences, is from the evidence of the facts which are merely learnt from experience. That the year goes through its course in 365 days, can only be known by observation of the

sun or stars: that 365 days is 52 weeks and a day, it requires no experience, but only a little thought to perceive. That bees build their cells in the form of hexagons, we cannot know without looking at them; that regular hexagons may be arranged so as to fill space, may be proved with the utmost rigour, even if there were not in existence such a thing as a material hexagon.

[5] Aristotle, Dr Whately, Dugald Stewart, &c.

4. As I have already said, one mode in which we may express the difference of necessary truths and truths of experience, is, that necessary truths are those of which we cannot distinctly conceive the contrary. We can very readily conceive the contrary of experiential truths. We can conceive the stars moving about the pole or across the sky in any kind of curves with any velocities; we can conceive the moon always appearing during the whole month as a luminous disk, as she might do if her light were inherent and not borrowed. But we cannot conceive one of the parallelograms

on the same base and between the same parallels larger than the other; for we find that, if we attempt to do this, when we separate the parallelograms into parts, we have to conceive one triangle larger than another, both having all their parts equal; which we cannot conceive at all, if we conceive the triangles distinctly. We make this impossibility more clear by conceiving 62 the triangles to be placed so that two sides of the one coincide with two sides of the other; and it is then seen, that in order to conceive the triangles unequal, we must conceive the two bases which have the same extremities both ways, to be different lines, though both straight lines. This it is impossible to conceive: we assent to the impossibility as an axiom, when it is expressed by saying, that two straight lines cannot inclose a space; and thus we cannot distinctly conceive the contrary of the proposition just mentioned respecting parallelograms.

But it is necessary, in applying this distinction, to bear in mind the terms of it;

—that we cannot *distinctly* conceive the contrary of a necessary truth. For in a certain loose, indistinct way, persons conceive the contrary of necessary geometrical truths, when they erroneously conceive false propositions to be true. Thus, Hobbes erroneously held that he had discovered a means of geometrically 'doubling the cube,' as it is called, that is, finding two mean proportionals between two given lines; a problem which cannot be solved by plane geometry. Hobbes not only proposed a construction for this purpose, but obstinately maintained that it was right, when it had been proved to be wrong. But then, the discussion showed how indistinct the geometrical conceptions of Hobbes were; for when his critics had proved that one of the lines in his diagram would not meet the other in the point which his reasoning supposed, but in another point near to it; he maintained, in reply, that one of these points was large enough to include the other, so that they might be considered as the same point. Such a mode of conceiving the opposite of a geometrical

truth, forms no exception to the assertion, that this opposite cannot be distinctly conceived.

In like manner, the indistinct conceptions of children and of rude savages do not invalidate the distinction of necessary and experiential truths. Children and savages make mistakes even with regard to numbers; and might easily happen to assert that 27 and 38 are equal to 63 or 64. But such mistakes cannot 63 make arithmetical truths cease to be necessary truths. When any person conceives these numbers and their addition distinctly, by resolving them into parts, or in any other way, he sees that their sum is necessarily 65. If, on the ground of the possibility of children and savages conceiving something different, it be held that this is not a necessary truth, it must be held on the same ground, that it is not a necessary truth that 7 and 4 are equal to 11; for children and savages might be found so unfamiliar with numbers as not to reject the assertion that 7 and 4 are 10, or even that 4 and 3 are 6, or 8. But I suppose that no

persons would on such grounds hold that these arithmetical truths are truths known only by experience.

5. I have taken examples of necessary truths from the properties of number and space; but such truths exist no less in other subjects, although the discipline of thought which is requisite to perceive them distinctly, may not be so usual among men with regard to the sciences of mechanics and hydrostatics, as it is with regard to the sciences of geometry and arithmetic. Yet every one may perceive that there are such truths in mechanics. If I press the table with my hand, the table presses my hand with an equal force: here is a self-evident and necessary truth. In any machine, constructed in whatever manner to increase the force which I can exert, it is certain that what I gain in force I must lose in the velocity which I communicate. This is not a contingent truth, borrowed from and limited by observation; for a man of sound mechanical views applies it with like confidence, however novel be the

construction of the machine. When I come to speak of the ideas which are involved in our mechanical knowledge, I may, perhaps, be able to bring more clearly into view the necessary truth of general propositions on such subjects. That reaction is equal and opposite to action, is as necessarily true as that two straight lines cannot inclose a space; it is as impossible theoretically to make a perpetual motion by mere mechanism as to make the diagonal of a square commensurable with the side. 64

6. Necessary truths must be *universal* truths. If any property belong to a right-angled triangle *necessarily*, it must belong to *all* right-angled triangles. And it shall be proved in the following Chapter, that truths possessing these two characters, of Necessity and Universality, cannot possibly be the mere results of experience.

[Necessary truths are not considered as a portion of the *Inductive* Sciences. They are Deductions from our Ideas. Thus the necessary truths which constitute the

Science of Geometry are Deductions from our Idea of Space: the necessary truths which constitute the Science of Arithmetic are Deductions from our notions of Number; which perhaps involves necessarily the Idea of Time. But though we do not call those Sciences *Inductive* which involve properties of Space, Number and Time alone, the properties of Space, Time and Number enter in many very important ways into the Inductive Sciences; and therefore the Ideas of Space, Time and Number require to be considered in the first place. And moreover the examination of these Ideas is an essential step towards the examination of other Ideas: and the conditions of the possibility and certainty of truth, which are exemplified in Geometry and Arithmetic, open to us important views respecting the conditions of the possibility and certainty of all Scientific Truth. We shall therefore in the next Book examine the Ideas on which the Pure Sciences, Geometry and Arithmetic, are founded. But we must first say a little more of Ideas in general.]

CHAPTER IV.

Of Experience.

1. THERE employ the term Experience in a more definite and limited sense than that which it possesses in common usage; for I restrict it to matters belonging to the domain of science. In such cases, the knowledge which we acquire, by means of experience, is of a clear and precise nature; and the passions and feelings and interests, which make the lessons of experience in practical matters so difficult to read aright, no longer disturb and confuse us. We may, therefore, hope, by attending to such cases, to learn what efficacy experience really has, in the discovery of truth.

That from *experience* (including intentional experience, or *observation*,) we obtain much knowledge which is highly

important, and which could not be procured from any other source, is abundantly clear. We have already taken several examples of such knowledge. We know by experience that animals which ruminate are cloven-hoofed; and we know this in no other manner. We know, in like manner, that all the planets and their satellites revolve round the sun from west to east. It has been found by experience that all meteoric stones contain chrome. Many similar portions of our knowledge might be mentioned.

Now what we have here to remark is this; —that in no case can experience prove a proposition to be *necessarily* or *universally* true. However many instances we may have observed of the truth of a proposition, yet if it be known merely by observation, there is nothing to assure us that the next case shall not be an exception to the rule. If it be strictly true that every ruminant animal yet known has cloven hoofs, we 66 still cannot be sure that some creature will not hereafter be discovered which has the first of these attributes without having the other. When

the planets and their satellites, as far as Saturn, had been all found to move round the sun in one direction, it was still possible that there might be other such bodies not obeying this rule; and, accordingly, when the satellites of Uranus were detected, they appeared to offer an exception of this kind. Even in the mathematical sciences, we have examples of such rules suggested by experience, and also of their precariousness. However far they may have been tested, we cannot depend upon their correctness, except we see some reason for the rule. For instance, various rules have been given, for the purpose of pointing out *prime numbers*; that is, those which cannot be divided by any other number. We may try, as an example of such a rule, this one—any odd power of the number two, diminished by one. Thus the third power of two, diminished by one, is seven; the fifth power, diminished by one, is thirty-one; the seventh power so diminished is one hundred and twenty-seven. All these are prime numbers: and we might be led to suppose that the rule is universal. But the next example

shows us the fallaciousness of such a belief. The ninth power of two, diminished by one, is five hundred and eleven, which is not a prime, being divisible by seven.

Experience must always consist of a limited number of observations. And, however numerous these may be, they can show nothing with regard to the infinite number of cases in which the experiment has not been made. Experience being thus unable to prove a fact to be universal, is, as will readily be seen, still more incapable of proving a truth to be necessary. Experience cannot, indeed, offer the smallest ground for the necessity of a proposition. She can observe and record what has happened; but she cannot find, in any case, or in any accumulation of cases, any reason for what must happen. She may see objects side by side; but she cannot see a reason why they must ever be side by side. She finds certain events to occur in succession; but the succession supplies, in its occurrence, no 67 reason for its recurrence. She contemplates external objects; but she cannot detect any

internal bond, which indissolubly connects the future with the past, the possible with the real. To learn a proposition by experience, and to see it to be necessarily true, are two altogether different processes of thought.

2. But it may be said, that we do learn by means of observation and experience many universal truths; indeed, all the general truths of which science consists. Is not the doctrine of universal gravitation learnt by experience? Are not the laws of motion, the properties of light, the general principles of chemistry, so learnt? How, with these examples before us, can we say that experience teaches no universal truths?

To this we reply, that these truths can only be known to be general, not universal, if they depend upon experience alone. Experience cannot bestow that universality which she herself cannot have, and that necessity of which she has no comprehension. If these doctrines *are* universally true, this universality flows from

the *ideas* which we apply to our experience, and which are, as we have seen, the real sources of necessary truth. How far these ideas can communicate their universality and necessity to the results of experience, it will hereafter be our business to consider. It will then appear, that when the mind collects from observation truths of a wide and comprehensive kind, which approach to the simplicity and universality of the truths of pure science; she gives them this character by throwing upon them the light of her own Fundamental Ideas.

But the truths which we discover by observation of the external world, even when most strikingly simple and universal, are not necessary truths. Is the doctrine of universal gravitation necessarily true? It was doubted by Clairaut (so far as it refers to the moon), when the progression of the apogee in fact appeared to be twice as great as the theory admitted. It has been doubted, even more recently, with respect to the planets, their mutual perturbations appearing to indicate a deviation from the

law. It is doubted still, by some 68 persons, with respect to the double stars. But suppose all these doubts to be banished, and the law to be universal; is it then proved to be necessary? Manifestly not: the very existence of these doubts proves that it is not so. For the doubts were dissipated by reference to observation and calculation, not by reasoning on the nature of the law. Clairaut's difficulty was removed by a more exact calculation of the effect of the sun's force on the motion of the apogee. The suggestion of Bessel, that the intensity of gravitation might be different for different planets, was found to be unnecessary, when Professor Airy gave a more accurate determination of the mass of Jupiter. And the question whether the extension of the law of the inverse square to the double stars be true, (one of the most remarkable questions now before the scientific world,) must be answered, not by any speculations concerning what the laws of attraction must necessarily be, but by carefully determining the actual laws of the motion of these curious objects, by means of the

observations such as those which Sir John Herschel has collected for that purpose, by his unexampled survey of both hemispheres of the sky. And since the extent of this truth is thus to be determined by reference to observed facts, it is clear that no mere accumulation of them can make its universality certain, or its necessity apparent.

Thus no knowledge of the necessity of any truths can result from the observation of what really happens. This being clearly understood, we are led to an important inquiry.

The characters of universality and necessity in the truths which form part of our knowledge, can never be derived from experience, by which so large a part of our knowledge is obtained. But since, as we have seen, we really do possess a large body of truths which are necessary, and because necessary, therefore universal, the question still recurs, from what source these characters of universality and necessity are

derived.

The answer to this question we will attempt to give in the next chapter.

CHAPTER V.

Of the Grounds of Necessary Truths.

1. To the question just stated, I reply, that the necessity and universality of the truths which form a part of our knowledge, are derived from the *Fundamental Ideas* which those truths involve. These ideas entirely shape and circumscribe our knowledge; they regulate the active operations of our minds, without which our passive sensations do not become knowledge. They govern these operations, according to rules which are not only fixed and permanent, but which may be expressed in plain and definite terms; and these rules, when thus expressed, may be made the basis of demonstrations by which the necessary relations imparted to our knowledge by our Ideas may be traced to

their consequences in the most remote ramifications of scientific truth.

These enunciations of the necessary and evident conditions imposed upon our knowledge by the Fundamental Ideas which it involves, are termed *Axioms*. Thus the Axioms of Geometry express the necessary conditions which result from the Idea of Space; the Axioms of Mechanics express the necessary conditions which flow from the Ideas of Force and Motion; and so on.

2. It will be the office of several of the succeeding Books of this work to establish and illustrate in detail what I have thus stated in general terms. I shall there pass in review many of the most important fundamental ideas on which the existing body of our science depends; and I shall endeavour to show, for each such idea in succession, that knowledge involves an active as well as a passive element; that it is not possible without an act of the mind, regulated by certain 70 laws. I shall further attempt to enumerate some of the principal

fundamental relations which each idea thus introduces into our thoughts, and to express them by means of definitions and axioms, and other suitable forms.

I will only add a remark or two to illustrate further this view of the ideal grounds of our knowledge.

3. To persons familiar with any of the demonstrative sciences, it will be apparent that if we state all the Definitions and Axioms which are employed in the demonstrations, we state the whole basis on which those reasonings rest. For the whole process of demonstrative or deductive reasoning in any science, (as in geometry, for instance,) consists entirely in combining some of these first principles so as to obtain the simplest propositions of the science; then combining these so as to obtain other propositions of greater complexity; and so on, till we advance to the most recondite demonstrable truths; these last, however intricate and unexpected, still involving no principles except the original definitions

and axioms. Thus, by combining the Definition of a triangle, and the Definitions of equal lines and equal angles, namely, that they are such as when applied to each other, coincide, with the Axiom respecting straight lines (that two such lines cannot inclose a space,) we demonstrate the equality of triangles, under certain assumed conditions. Again, by combining this result with the Definition of parallelograms, and with the Axiom that if equals be taken from equals the wholes are equal, we prove the equality of parallelograms between the same parallels and upon the same base. From this proposition, again, we prove the equality of the square on the hypotenuse of a triangle to the squares on the two sides containing the right angle. But in all this there is nothing contained which is not rigorously the result of our geometrical Definitions and Axioms. All the rest of our treatises of geometry consists only of terms and phrases of reasoning, the object of which is to connect those first principles, and to exhibit the effects of their combination in the shape of demonstration.

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4. This combination of first principles takes place according to the forms and rules of *Logic*. All the steps of the demonstration may be stated in the shape in which logicians are accustomed to exhibit processes of reasoning in order to show their conclusiveness, that is, in *Syllogisms*. Thus our geometrical reasonings might be resolved into such steps as the following:—

All straight lines drawn from the centre of a circle to its circumference are equal:

But the straight lines AB, AC, are drawn from the centre of a circle to its circumference:

Therefore the straight lines AB, AC, are equal.

Each step of geometrical, and all other demonstrative reasoning, may be resolved into three such clauses as these; and these three clauses are termed respectively, the *major premiss*, the *minor premiss*, and the *conclusion*; or, more briefly, the *major*, the *minor*, and the *conclusion*.

The principle which justifies the reasoning when exhibited in this syllogistic form, is this:—that a truth which can be asserted as generally, or rather as universally true, can be asserted as true also in each particular case. The *minor* only asserts a certain particular case to be an example of such conditions as are spoken of in the *major*; and hence the conclusion, which is true of the major by supposition, is true of the minor by consequence; and thus we proceed from syllogism to syllogism, in each one employing some general truth in some particular instance. Any proof which occurs in geometry, or any other science of demonstration, may thus be reduced to a series of processes, in each of which we pass from some general proposition to the narrower and more special propositions which it includes. And this process of deriving truths by the mere combination of general principles, applied in particular hypothetical cases, is called *deduction*; being opposed to *induction*, in which, as we have seen (chap. i. sect. 3), a new general principle is introduced at every step.

5. Now we have to remark that, this being so, however far we follow such deductive reasoning, we can 72 never have, in our conclusion any truth which is not virtually included in the original principles from which the reasoning started. For since at any step we merely take out of a general proposition something included in it, while at the preceding step we have taken this general proposition out of one more general, and so on perpetually, it is manifest that our last result was really included in the principle or principles with which we began. I say *principles*, because, although our logical conclusion can only exhibit the legitimate issue of our first principles, it may, nevertheless, contain the result of the combination of several such principles, and may thus assume a great degree of complexity, and may appear so far removed from the parent truths, as to betray at first sight hardly any relationship with them. Thus the proposition which has already been quoted respecting the squares on the sides of a right-angled triangle, contains the results of many elementary principles; as,

the definitions of parallels, triangle, and square; the axioms respecting straight lines, and respecting parallels; and, perhaps, others. The conclusion is complicated by containing the effects of the combination of all these elements; but it contains nothing, and can contain nothing, but such elements and their combinations.

This doctrine, that logical reasoning produces no new truths, but only unfolds and brings into view those truths which were, in effect, contained in the first principles of the reasoning, is assented to by almost all who, in modern times, have attended to the science of logic. Such a view is admitted both by those who defend, and by those who depreciate the value of logic. 'Whatever is established by reasoning, must have been contained and virtually asserted in the premises[6].' 'The only truth which such propositions can possess consists in conformity to the original principles.'

[6] Whately's *Logic*, pp. 237, 238.

In this manner the whole substance of our

geometry is reduced to the Definitions and Axioms which we employ in our elementary reasonings; and in like 73 manner we reduce the demonstrative truths of any other science to the definitions and axioms which we there employ.

6. But in reference to this subject, it has sometimes been said that demonstrative sciences do in reality depend upon Definitions only; and that no additional kind of principle, such as we have supposed Axioms to be, is absolutely required. It has been asserted that in geometry, for example, the source of the necessary truth of our propositions is this, that they depend upon definitions alone, and consequently merely state the identity of the same thing under different aspects.

That in the sciences which admit of demonstration, as geometry, mechanics, and the like, Axioms as well as Definitions are needed, in order to express the grounds of our necessary convictions, must be shown hereafter by an examination of each of these

sciences in particular. But that the propositions of these sciences, those of geometry for example, do not merely assert the identity of the same thing, will, I think, be generally allowed, if we consider the assertions which we are enabled to make. When we declare that ‘a straight line is the shortest distance between two points,’ is this merely an identical proposition? the definition of a straight line in another form? Not so: the definition of a straight line involves the notion of form only, and does not contain anything about magnitude; consequently, it cannot contain anything equivalent to ‘shortest.’ Thus the propositions of geometry are not merely identical propositions; nor have we in their general character anything to countenance the assertion, that they are the results of definitions alone. And when we come to examine this and other sciences more closely, we shall find that axioms, such as are usually in our treatises made the fundamental principles of our demonstrations, neither have ever been, nor can be, dispensed with. Axioms, as well as

Definitions, are in all cases requisite, in order properly to exhibit the grounds of necessary truth.

7. Thus the real logical basis of every body of demonstrated truths are the Definitions and Axioms 74 which are the first principles of the reasonings. But when we are arrived at this point, the question further occurs, what is the ground of the truth of these Axioms? It is not the logical, but the philosophical, not the formal, but the real foundation of necessary truth, which we are seeking. Hence this inquiry necessarily comes before us, What is the ground of the Axioms of Geometry, of Mechanics, and of any other demonstrable science?

The answer which we are led to give, by the view which we have taken of the nature of knowledge, has already been stated. The ground of the axioms belonging to each science is the *Idea* which the axiom involves. The ground of the Axioms of Geometry is the *Idea of Space*: the ground of

the Axioms of Mechanics is the *Idea of Force*, of *Action* and *Reaction*, and the like. And hence these Ideas are Fundamental Ideas; and since they are thus the foundations, not only of demonstration but of truth, an examination into their real import and nature is of the greatest consequence to our purpose.

8. Not only the Axioms, but the definitions which form the basis of our reasonings, depend upon our Fundamental Ideas. And the Definitions are not arbitrary definitions, but are determined by a necessity no less rigorous than the Axioms themselves. We could not think of geometrical truths without conceiving a circle; and we could not reason concerning such truths without defining a circle in some mode equivalent to that which is commonly adopted. The Definitions of parallels, of right angles, and the like, are quite as necessarily prescribed by the nature of the case, as the Axioms which these Definitions bring with them. Indeed we may substitute one of these kinds of principles

for another. We cannot always put a Definition in the place of an Axiom; but we may always find an Axiom which shall take the place of a Definition. If we assume a proper Axiom respecting straight lines, we need no Definition of a straight line. But in whatever shape the principle appear, as Definition or as Axiom, it has about it nothing casual or 75 arbitrary, but is determined to be what it is, as to its import, by the most rigorous necessity, growing out of the Idea of Space.

9. These principles,—Definitions, and Axioms,—thus exhibiting the primary developments of a fundamental idea, do in fact express the idea, so far as its expression in words forms part of our science. They are different views of the same body of truth; and though each principle, by itself, exhibits only one aspect of this body, taken together they convey a sufficient conception of it for our purposes. The Idea itself cannot be fixed in words; but these various lines of truth proceeding from it, suggest sufficiently to a fitly-prepared mind, the place where the

idea resides, its nature, and its efficacy.

It is true that these principles,—our elementary Definitions and Axioms,—even taken all together, express the Idea incompletely. Thus the Definitions and Axioms of Geometry, as they are stated in our elementary works, do not fully express the Idea of Space as it exists in our minds. For, in addition to these, other Axioms, independent of these, and no less evident, can be stated; and are in fact stated when we come to the Higher Geometry. Such, for instance, is the Axiom of Archimedes—that a curve line which joins two points is less than a broken line which joins the same points and includes the curve. And thus the Idea is disclosed but not fully revealed, imparted but not transfused, by the use we make of it in science. When we have taken from the fountain so much as serves our purpose, there still remains behind a deep well of truth, which we have not exhausted, and which we may easily believe to be inexhaustible.

CHAPTER VI.

The Fundamental Ideas are not Derived from Experience.

1. By the course of speculation contained in the last three Chapters, we are again led to the conclusion which we have already stated, that our knowledge contains an ideal element, and that this element is not derived from experience. For we have seen that there are propositions which are known to be necessarily true; and that such knowledge is not, and cannot be, obtained by mere observation of actual facts. It has been shown, also, that these necessary truths are the results of certain fundamental ideas, such as those of space, number, and the like. Hence it follows inevitably that these ideas and others of the same kind are not derived from experience. For these ideas

possess a power of infusing into their developments that very necessity which experience can in no way bestow. This power they do not borrow from the external world, but possess by their own nature. Thus we unfold out of the Idea of Space the propositions of geometry, which are plainly truths of the most rigorous necessity and universality. But if the idea of space were merely collected from observation of the external world, it could never enable or entitle us to assert such propositions: it could never authorize us to say that not merely some lines, but *all* lines, not only have, but *must* have, those properties which geometry teaches. Geometry in every proposition speaks a language which experience never dares to utter; and indeed of which she but half comprehends the meaning. Experience sees that the assertions are true, but she sees not how profound and absolute is their truth. 77 She unhesitatingly assents to the laws which geometry delivers, but she does not pretend to see the origin of their obligation. She is always ready to acknowledge the sway of pure scientific

principles as a matter of fact, but she does not dream of offering her opinion on their authority as a matter of right; still less can she justly claim to be herself the source of that authority.

David Hume asserted[7], that we are incapable of seeing in any of the appearances which the world presents anything of necessary connexion; and hence he inferred that our knowledge cannot extend to any such connexion. It will be seen from what we have said that we assent to his remark as to the fact, but we differ from him altogether in the consequence to be drawn from it. Our inference from Hume's observation is, not the truth of his conclusion, but the falsehood of his premises;—not that, therefore, we can know nothing of natural connexion, but that, therefore, we have some other source of knowledge than experience:—not, that we can have no idea of connexion or causation, because, in his language, it cannot be the copy of an impression; but that since we have such an idea, our ideas are not the

copies of our impressions.

[7] *Essays*, vol. ii. p. 70.

Since it thus appears that our fundamental ideas are not acquired from the external world by our senses, but have some separate and independent origin, it is important for us to examine their nature and properties, as they exist in themselves; and this it will be our business to do through a portion of the following pages. But it may be proper first to notice one or two objections which may possibly occur to some readers.

2. It may be said that without the use of our senses, of sight and touch, for instance, we should never have any idea of space; that this idea, therefore, may properly be said to be derived from those senses. And to this I reply, by referring to a parallel instance. Without light we should have no perception of visible 78 figure; yet the power of perceiving visible figure cannot be said to be derived from the light, but resides in the structure of the eye. If we had never

seen objects in the light, we should be quite unaware that we possessed a power of vision; yet we should not possess it the less on that account. If we had never exercised the senses of sight and touch (if we can conceive such a state of human existence) we know not that we should be conscious of an idea of space. But the light reveals to us at the same time the existence of external objects and our own power of seeing. And in a very similar manner, the exercise of our senses discloses to us, at the same time, the external world, and our own ideas of space, time, and other conditions, without which the external world can neither be observed nor conceived. That light is necessary to vision, does not, in any degree, supersede the importance of a separate examination of the laws of our visual powers, if we would understand the nature of our own bodily faculties and the extent of the information they can give us. In like manner, the fact that intercourse with the external world is necessary for the conscious employment of our ideas, does not make it the less essential for us to examine those ideas in their most

intimate structure, in order that we may understand the grounds and limits of our knowledge. Even before we see a single object, we have a faculty of vision; and in like manner, if we can suppose a man who has never contemplated an object in space or time, we must still assume him to have the faculties of entertaining the ideas of space and time, which faculties are called into play on the very first occasion of the use of the senses.

3. In answer to such remarks as the above, it has sometimes been said that to assume separate faculties in the mind for so many different processes of thought, is to give a mere verbal explanation, since we learn nothing concerning our idea of space by being told that we have a faculty of forming such an idea. It has been said that this course of explanation leads to an endless multiplication of elements in man's nature, without any advantage to our knowledge of his true 79 constitution. We may, it is said, assert man to have a faculty of walking, of standing, of breathing, of

speaking; but what, it is asked, is gained by such assertions? To this I reply, that we undoubtedly have such faculties as those just named; that it is by no means unimportant to consider them; and that the main question in such cases is, whether they are separate and independent faculties, or complex and derivative ones; and, if the latter be the case, what are the simple and original faculties by the combination of which the others are produced. In walking, standing, breathing, for instance, a great part of the operation can be reduced to one single faculty; the voluntary exercise of our muscles. But in breathing this does not appear to be the whole of the process. The operation is, in part at least, involuntary; and it has been held that there is a certain sympathetic action of the nerves, in addition to the voluntary agency which they transmit, which is essential to the function. To determine whether or no this sympathetic faculty is real and distinct, and if so, what are its laws and limits, is certainly a highly philosophical inquiry, and well deserving the attention which has been

bestowed upon it by eminent physiologists. And just of the same nature are the inquiries with respect to man's intellectual constitution, on which we propose to enter. For instance, man has a faculty of apprehending time, and a faculty of reckoning numbers: are these distinct, or is one faculty derived from the other? To analyze the various combinations of our ideas and observations into the original faculties which they involve; to show that these faculties are original, and not capable of further analysis: to point out the characters which mark these faculties and lead to the most important features of our knowledge;—these are the kind of researches on which we have now to enter, and these, we trust, will be found to be far from idle or useless parts of our plan. If we succeed in such attempts, it will appear that it is by no means a frivolous or superfluous step to distinguish separate faculties in the mind. If we do not learn much by being told that we have a faculty 80 of forming the idea of space, we at least, by such a commencement, circumscribe a certain

portion of the field of our investigations, which, we shall afterwards endeavour to show, requires and rewards a special examination. And though we shall thus have to separate the domain of our philosophy into many provinces, these are, as we trust it will appear, neither arbitrarily assigned, nor vague in their limits, nor infinite in number.

CHAPTER VII.

Of the Philosophy of the Sciences.

WE proceed, in the ensuing Books, to the closer examination of a considerable number of those Fundamental Ideas on which the sciences, hitherto most successfully cultivated, are founded. In this task, our objects will be to explain and analyze such Ideas so as to bring into view the Definitions and Axioms, or other forms, in which we may clothe the conditions to which our speculative knowledge is subjected. I shall also try to prove, for some of these Ideas in particular, what has been already urged respecting them in general, that they are not derived from observation, but necessarily impose their conditions upon that knowledge of which observation supplies the materials. I shall further, in

some cases, endeavour to trace the history of these Ideas as they have successively come into notice in the progress of science; the gradual development by which they have arrived at their due purity and clearness; and, as a necessary part of such a history, I shall give a view of some of the principal controversies which have taken place with regard to each portion of knowledge.

An exposition and discussion of the Fundamental Ideas of each Science may, with great propriety, be termed the PHILOSOPHY OF such SCIENCE. These ideas contain in themselves the elements of those truths which the science discovers and enunciates; and in the progress of the sciences, both in the world at large and in the mind of each individual student, the most important steps consist in apprehending these ideas clearly, and in bringing them into accordance with the observed facts. I shall, therefore, in a series of Books, 82 treat of the *Philosophy of the Pure Sciences*, the *Philosophy of the*

Mechanical Sciences, the *Philosophy of Chemistry*, and the like, and shall analyze and examine the ideas which these sciences respectively involve.

In this undertaking, inevitably somewhat long, and involving many deep and subtle discussions, I shall take, as a chart of the country before me, by which my course is to be guided, the scheme of the sciences which I was led to form by travelling over the history of each in order[8]. Each of the sciences of which I then narrated the progress, depends upon several of the Fundamental Ideas of which I have to speak: some of these Ideas are peculiar to one field of speculation, others are common to more. A previous enumeration of Ideas thus collected may serve both to show the course and limits of this part of our plan, and the variety of interest which it offers.

[8] *History of the Inductive Sciences.*

I shall, then, successively, have to speak Of the Ideas which are the foundation of Geometry and Arithmetic, (and which also

regulate all sciences depending upon these, as Astronomy and Mechanics;) namely, the Ideas of *Space*, *Time*, and *Number* (Book II.):

Of the Ideas on which the Mechanical Sciences (as Mechanics, Hydrostatics, Physical Astronomy) more peculiarly rest; the ideas of *Force* and *Matter*, or rather the idea of *Cause*, which is the basis of these (Book III.):

Of the Ideas which the Secondary Mechanical Sciences (Acoustics, Optics, and Thermotics) involve; namely, the Ideas of the *Externality* of objects, and of the *Media* by which we perceive their qualities (Book IV.):

Of the Ideas which are the basis of Mechanico-chemical and Chemical Science; *Polarity*, *Chemical Affinity*, and *Substance*; and the Idea of *Symmetry*, a necessary part of the Philosophy of Crystallography (Books V. VI.):

Of the Ideas on which the Classificatory Sciences proceed (Mineralogy, Botany, and Zoology); namely, 83 the Ideas of *Resemblance*, and of its gradations, and of *Natural Affinity* (Books VII. VIII.):

Finally, of those Ideas on which the Physiological Sciences are founded; the Ideas of separate Vital Powers, such as *Assimilation* and *Irritability*; and the Idea of *Final Cause* (Book IX.):

We have, besides these, the Palætiological Sciences, which proceed mainly on the conception of *Historical Causation* (Book X.):

It is plain that when we have proceeded so far as this, we have advanced to the verge of those speculations which have to do with mind as well as body. The extension of our philosophy to such a field, if it can be justly so extended, will be one of the most important results of our researches; but on that very account we must fully study the lessons which we learn in those fields of speculation where our doctrines are most secure, before we venture into a region where our principles will appear to be more precarious, and where they are inevitably less precise.

We now proceed to the examination of the above Ideas, and to such essays towards

the philosophy of each Science as this course of investigation may suggest.

BOOK II.

THE

PHILOSOPHY

OF THE

PURE SCIENCES.

The way in which we are led to regard human knowledge is like the way in which Copernicus was led to regard the heavens. When the explanation of the celestial motions could not be made to go right so long as he assumed that all the host of stars turns round the spectator, he tried whether it would not answer better if he made the spectator turn, and left the stars at rest. We may make a similar trial in Metaphysics, as to our way of looking at objects. If our view of them must be governed altogether by the properties of the

objects themselves, I see not how man can know anything about them *à priori*. But if the thing, as an object of the senses, is regulated by the constitution of our power of knowing, I can very readily represent to myself this possibility.

KANT, *Kritik d. R. V. Pref.*

BOOK II.

THE PHILOSOPHY OF THE PURE SCIENCES.

[The principal question discussed in the last Book was this (see chaps. V. and VI.): How are *necessary* and *universal* truths possible? And the answer then given was: that the necessity and universality of truths are derived from the *Fundamental Ideas* which they involve. And we proceed in this Book to exemplify this doctrine in the case of the truths of Geometry and Arithmetic, which derive their necessity and universality from the Fundamental Ideas of Space, and Time, or Number.

The question thus examined is that which Kant undertook to deal with in his celebrated work, *Kritik der reinen Vernunft*

(*Examination of the Pure Reason*): and our solution of the Problem, so far as the Ideas of Space and Time are concerned, agrees in the main with his. The arguments contained in chapters II. and VII. of this Book, are the leading arguments respecting Space and Time, in Kant's *Kritik*. Kant, however, instead of calling Space and Time *Ideas*, calls them the necessary *Forms* of our experience, as I have stated in the text.

But though I have adopted Kant's arguments as to Space and Time, all that follows in the succeeding Books, with regard to other Ideas, has no resemblance to any doctrines of Kant or his school (with the exception, perhaps, of some of the views on the Idea of *Cause*). The nature and character of the other Scientific Ideas which I have examined, in the succeeding Books, have been established by an analysis of the history of the several Sciences to which those Ideas are essential, and an examination of the writings of the principal discoverers in those Sciences.]

CHAPTER I.

Of the Pure Sciences.

1. ALL external objects and events which we can contemplate are viewed as having relations of Space, Time, and Number; and are subject to the general conditions which these Ideas impose, as well as to the particular laws which belong to each class of objects and occurrences. The special laws of nature, considered under the various aspects which constitute the different sciences, are obtained by a mixed reference to Experience and to the Fundamental Ideas of each science. But besides the sciences thus formed by the aid of special experience, the conditions which flow from those more comprehensive ideas first mentioned, Space, Time, and Number, constitute a body of science, applicable to

objects and changes of all kinds, and deduced without recurrence being had to any observation in particular. These sciences, thus unfolded out of ideas alone, unmixed with any reference to the phenomena of matter, are hence termed *Pure* Sciences. The principal sciences of this class are Geometry, Theoretical Arithmetic, and Algebra considered in its most general sense, as the investigation of the relations of space and number by means of general symbols.

2. These Pure Sciences were not included in our survey of the history of the sciences, because they are not *inductive* sciences. Their progress has not consisted in collecting laws from phenomena, true theories from observed facts, and more general from more limited laws; but in tracing the consequences of the ideas themselves, and in detecting the most general and intimate analogies and connexions which prevail 89 among such conceptions as are derivable from the ideas. These sciences have no principles besides

definitions and axioms, and no process of proof but *deduction*; this process, however, assuming here a most remarkable character; and exhibiting a combination of simplicity and complexity, of rigour and generality, quite unparalleled in other subjects.

3. The universality of the truths, and the rigour of the demonstrations of these pure sciences, attracted attention in the earliest times; and it was perceived that they offered an exercise and a discipline of the intellectual faculties, in a form peculiarly free from admixture of extraneous elements. They were strenuously cultivated by the Greeks, both with a view to such a discipline, and from the love of speculative truth which prevailed among that people: and the name *mathematics*, by which they are designated, indicates this their character of *disciplinal* studies.

4. As has already been said, the ideas which these sciences involve extend to all the objects and changes which we observe in the external world; and hence the

consideration of mathematical relations forms a large portion of many of the sciences which treat of the phenomena and laws of external nature, as Astronomy, Optics, and Mechanics. Such sciences are hence often termed *Mixed Mathematics*, the relations of space and number being, in these branches of knowledge, combined with principles collected from special observation; while Geometry, Algebra, and the like subjects, which involve no result of experience, are called *Pure Mathematics*.

5. Space, time, and number, may be conceived as *forms* by which the knowledge derived from our sensations is moulded, and which are independent of the differences in the *matter* of our knowledge, arising from the sensations themselves. Hence the sciences which have these ideas for their subject may be termed *Formal Sciences*. In this point of view, they are distinguished from sciences in which, besides these mere formal laws by which appearances are corrected, we endeavour to apply to the phenomena the idea of cause, 90 or some of

the other ideas which penetrate further into the principles of nature. We have thus, in the History, distinguished Formal Astronomy and Formal Optics from Physical Astronomy and Physical Optics.

We now proceed to our examination of the Ideas which constitute the foundation of these formal or pure mathematical sciences, beginning with the Idea of Space.

CHAPTER II.

Of the Idea of Space.

1. By speaking of space as an Idea, I intend to imply, as has already been stated, that the apprehension of objects as existing in space, and of the relations of position, &c., prevailing among them, is not a consequence of experience, but a result of a peculiar constitution and activity of the mind, which is independent of all experience in its origin, though constantly combined with experience in its exercise.

That the idea of space is thus independent of experience, has already been pointed out in speaking of ideas in general: but it may be useful to illustrate the doctrine further in this particular case.

I assert, then, that space is not a notion

obtained by experience. Experience gives us information concerning things without us: but our apprehending them *as* without us, takes for granted their existence in space. Experience acquaints us what are the form, position, magnitude of particular objects: but that they have form, position, magnitude, presupposes that they are in space. We cannot derive from appearances, by the way of observation, the habit of representing things to ourselves as in space; for no single act of observation is possible any otherwise than by beginning with such a representation, and conceiving objects as already existing in space.

2. That our mode of representing space to ourselves is not derived from experience, is clear also from this: that through this mode of representation we arrive at propositions which are rigorously universal and necessary. Propositions of such a kind could not possibly be obtained from experience; for experience can 92 only teach us by a limited number of examples, and therefore can never securely establish a universal

proposition: and again, experience can only inform us that anything is so, and can never prove that it must be so. That two sides of a triangle are greater than the third is a universal and necessary geometrical truth: it is true of all triangles; it is true in such a way that the contrary cannot be conceived. Experience could not prove such a proposition. And experience has not proved it; for perhaps no man ever made the trial as a means of removing doubts: and no trial could, in fact, add in the smallest degree to the certainty of this truth. To seek for proof of geometrical propositions by an appeal to observation proves nothing in reality, except that the person who has recourse to such grounds has no due apprehension of the nature of geometrical demonstration. We have heard of persons who convinced themselves by measurement that the geometrical rule respecting the squares on the sides of a right-angled triangle was true: but these were persons whose minds had been engrossed by practical habits, and in whom the speculative development of the idea of space had been stifled by other

employments. The practical trial of the rule may illustrate, but cannot prove it. The rule will of course be confirmed by such trial, because what is true in general is true in particular: but the rule cannot be proved from any number of trials, for no accumulation of particular cases makes up a universal case. To all persons who can see the force of any proof, the geometrical rule above referred to is as evident, and its evidence as independent of experience, as the assertion that sixteen and nine make twenty-five. At the same time, the truth of the geometrical rule is quite independent of numerical truths, and results from the relations of space alone. This could not be if our apprehension of the relations of space were the fruit of experience: for experience has no element from which such truth and such proof could arise.

3. Thus the existence of necessary truths, such as those of geometry, proves that the idea of space from 93 which they flow is not derived from experience. Such truths are inconceivable on the supposition of their

being collected from observation; for the impressions of sense include no evidence of necessity. But we can readily understand the necessary character of such truths, if we conceive that there are certain necessary conditions under which alone the mind receives the impressions of sense. Since these conditions reside in the constitution of the mind, and apply to every perception of an object to which the mind can attain, we easily see that their rules must include, not only all that has been, but all that can be, matter of experience. Our sensations can each convey no information except about itself; each can contain no trace of another additional sensation; and thus no relation and connexion between two sensations can be given by the sensations themselves. But the mode in which the mind perceives these impressions as objects, may and will introduce necessary relations among them: and thus by conceiving the idea of space to be a condition of perception in the mind, we can conceive the existence of necessary truths, which apply to all perceived objects.

4. If we consider the impressions of sense as the mere materials of our experience, such materials may be accumulated in any quantity and in any order. But if we suppose that this matter has a certain form given it, in the act of being accepted by the mind, we can understand how it is that these materials are subject to inevitable rules;—how nothing can be perceived exempt from the relations which belong to such a form. And since there are such truths applicable to our experience, and arising from the nature of space, we may thus consider space as a *form* which the materials given by experience necessarily assume in the mind; as an arrangement derived from the perceiving mind, and not from the sensations alone.

5. Thus this phrase,—that space is a *form* belonging to our perceptive power,—may be employed to express that we cannot perceive objects as in space, without an operation of the mind as well as of the senses—without active as well as passive faculties. This phrase, however, 94 is not

necessary to the exposition of our doctrines. Whether we call the conception of space a Condition of perception, a Form of perception, or an Idea, or by any other term, it is something originally inherent in the mind perceiving, and not in the objects perceived. And it is because the apprehension of all objects is thus subjected to certain mental conditions, forms or ideas, that our knowledge involves certain inviolable relations and necessary truths. The principles of such truths, so far as they regard space, are derived from the idea of space, and we must endeavour to exhibit such principles in their general form. But before we do this, we may notice some of the conditions which belong, not to our Ideas in general, but to this Idea of Space in particular.

CHAPTER III.

Of some Peculiarities of the Idea of Space.

1. SOME of the Ideas which we shall have to examine involve conceptions of certain relations of objects, as the idea of Cause and of Likeness; and may appear to be suggested by experience, enabling us to *abstract* this general relation from particular cases. But it will be seen that Space is not such a general conception of a relation. For we do not speak of *Spaces* as we speak of Causes and Likenesses, but of Space. And when we speak of *spaces*, we understand by the expression, parts of one and the same identical everywhere-extended Space. We conceive a universal Space; which is not made up of these partial spaces as its component parts, for it would remain if

these were taken away; and these cannot be conceived without presupposing absolute space. Absolute Space is essentially one; and the complication which exists in it, and the conception of various spaces, depends merely upon boundaries. Space must, therefore, be, as we have said, not a general conception abstracted from particulars, but a universal mode of representation, altogether independent of experience.

2. Space is infinite. We represent it to ourselves as an infinitely great magnitude. Such an idea as that of Likeness or Cause, is, no doubt, found in an infinite number of particular cases, and so far includes these cases. But these ideas do not include an infinite number of cases as parts of an infinite whole. When we say that all bodies and partial spaces exist *in* infinite space, we use an expression which is not applied in the same sense to any cases except those of Space and Time. 96

3. What is here said may appear to be a denial of the real existence of space. It must

be observed, however, that we do not deny, but distinctly assert, the existence of space as a real and necessary condition of all objects perceived; and that we not only allow that objects are seen external to us, but we found upon the fact of their being so seen, our view of the nature of space. If, however, it be said that we deny the reality of space as an object or thing, this is true. Nor does it appear easy to maintain that space exists as a thing, when it is considered that this thing is infinite in all its dimensions; and, moreover, that it is a thing, which, being nothing in itself, exists only that other things may exist in it. And those who maintain the real existence of space, must also maintain the real existence of time in the same sense. Now two infinite things, thus really existing, and yet existing only as other things exist in them, are notions so extravagant that we are driven to some other mode of explaining the state of the matter.

4. Thus space is not an object of which we perceive the properties, but a form of our

perception; not a thing which affects our senses, but an idea to which we conform the impressions of sense. And its peculiarities appear to depend upon this, that it is not only a form of sensation, but of *intuition*; that in reference to space, we not only perceive but *contemplate* objects. We see objects in space, side by side, exterior to each other; space, and objects in so far as they occupy space, have parts exterior to other parts; and have the whole thus made up by the juxtaposition of parts. This mode of apprehension belongs only to the ideas of space and time. Space and Time are made up of parts, but Cause and Likeness are not apprehended as made up of parts. And the term *intuition* (in its rigorous sense) is applicable only to that mode of contemplation in which we thus look at objects as made up of parts, and apprehend the relations of those parts at the same time and by the same act by which we apprehend the objects themselves.

5. As we have said, space limited by *boundaries* 97 gives rise to various

conceptions which we have often to consider. Thus limited, space assumes *form* or *figure*; and the variety of conceptions thus brought under our notice is infinite. We have every possible form of line, straight line, and curve; and of curves an endless number;—circles, parabolas, hyperbolas, spirals, helices. We have plane surfaces of various shapes,—parallelograms, polygons, ellipses; and we have solid figures,—cubes, cones, cylinders, spheres, spheroids, and so on. All these have their various properties, depending on the relations of their boundaries; and the investigation of their properties forms the business of the science of Geometry.

6. Space has three dimensions, or directions in which it may be measured; it cannot have more or fewer. The simplest measurement is that of a straight line, which has length alone. A surface has both length and breadth: and solid space has length, breadth, and thickness or depth. The origin of such a difference of dimensions will be seen if we reflect that each portion

of space has a boundary, and is extended both *in* the direction in which its boundary extends, and also in a direction *from* its boundary; for otherwise it would not be a boundary. A point has no dimensions. A line has but one dimension,—the distance from its boundary, or its *length*. A plane, bounded by a straight line, has the dimension which belongs to this line, and also has another dimension arising from the distance of its parts from this boundary line; and this may be called *breadth*. A solid, bounded by a plane, has the dimensions which this plane has; and has also a third dimension, which we may call *height* or *depth*, as we consider the solid extended above or below the plane; or *thickness*, if we omit all consideration of up and down. And no space can have any dimensions which are not resoluble into these three.

We may now proceed to consider the mode in which the idea of space is employed in the formation of Geometry.

CHAPTER IV.

Of the Definitions and Axioms which relate to Space.

1. THE relations of space have been apprehended with peculiar distinctness and clearness from the very first unfolding of man's speculative powers. This was a consequence of the circumstance which we have just noticed, that the simplest of these relations, and those on which the others depend, are seen by intuition. Hence, as soon as men were led to speculate concerning the relations of space, they assumed just principles, and obtained true results. It is said that the science of *geometry* had its origin in Egypt, before the dawn of the Greek philosophy: but the knowledge of the early Egyptians (exclusive of their mythology) appears to have been purely

practical; and, probably, their geometry consisted only in some maxims of *land-measuring*, which is what the term implies. The Greeks of the time of Plato, had, however, not only possessed themselves of many of the most remarkable elementary theorems of the science; but had, in several instances, reached the boundary of the science in its elementary form; as when they proposed to themselves the problems of doubling the cube and squaring the circle.

But the deduction of these theorems by a systematic process, and the primary exhibition of the simplest principles involved in the idea of space, which such a deduction requires, did not take place, so far as we are aware, till a period somewhat later. The *Elements of Geometry* of Euclid, in which this task was performed, are to this day the standard work on the subject: the author of this work taught mathematics with great applause at Alexandria, in the reign of Ptolemy Lagus, 99 about 280 years before Christ. The principles which Euclid makes the basis of his system have been

very little simplified since his time; and all the essays and controversies which bear upon these principles, have had a reference to the form in which they are stated by him.

2. *Definitions.*—The first principles of Euclid's geometry are, as the first principles of any system of geometry must be, definitions and axioms respecting the various ideal conceptions which he introduces; as straight lines, parallel lines, angles, circles, and the like. But it is to be observed that these definitions and axioms are very far from being arbitrary hypotheses and assumptions. They have their origin in the idea of space, and are merely modes of exhibiting that idea in such a manner as to make it afford grounds of deductive reasoning. The axioms are necessary consequences of the conceptions respecting which they are asserted; and the definitions are no less necessary limitations of conceptions; not requisite in order to arrive at this or that consequence; but necessary in order that it may be possible to draw any consequences, and to establish any general

truths.

For example, if we rest the end of one straight staff upon the middle of another straight staff, and move the first staff into various positions, we, by so doing, alter the angles which the first staff makes with the other to the right hand and to the left. But if we place the staff in that special position in which these two angles are equal, each of them is a right angle, according to Euclid; and this is the *definition* of a right angle, except that Euclid employs the abstract conception of straight lines, instead of speaking, as we have done, of staves. But this selection of the case in which the two angles are equal is not a mere act of caprice; as it might have been if he had selected a case in which these angles are unequal in any proportion. For the consequences which can be drawn concerning the cases of unequal angles, do not lead to general truths, without some reference to that peculiar case in which the angles are equal: and thus it becomes necessary to 100 single out and define that special case, marking it

by a special phrase. And this definition not only gives complete and distinct knowledge what a right angle is, to any one who can form the conception of an angle in general; but also supplies a principle from which all the properties of right angles may be deduced.

3. *Axioms.*—With regard to other conceptions also, as circles, squares, and the like, it is possible to lay down definitions which are a sufficient basis for our reasoning, so far as such figures are concerned. But, besides these definitions, it has been found necessary to introduce certain axioms among the fundamental principles of geometry. These are of the simplest character; for instance, that two straight lines cannot cut each other in more than one point, and an axiom concerning parallel lines. Like the definitions, these axioms flow from the Idea of Space, and present that idea under various aspects. They are different from the definitions; nor can the definitions be made to take the place of the axioms in the reasoning by

which elementary geometrical properties are established. For example, the definition of parallel straight lines is, that they are such as, however far continued, can never meet: but, in order to reason concerning such lines, we must further adopt some axiom respecting them: for example, we may very conveniently take this axiom; that two straight lines which cut one another are not both of them parallel to a third straight line[1]. The definition and the axiom are seen to be inseparably connected by our intuition of the properties of space; but the axiom cannot be proved from the definition, by any rigorous deductive demonstration. And if we were to take any other definition of two parallel straight lines, (as that they are both perpendicular to a third straight line,) we should still, at some point or other of our progress, fall in with the same difficulty of demonstratively establishing their properties without some further assumption.

[1] This axiom is simpler and more convenient than that of Euclid. It is employed by the late Professor Playfair in his *Geometry*.

101 4. Thus the elementary properties of figures, which are the basis of our geometry, are necessary results of our Idea of Space; and are connected with each other by the nature of that idea, and not merely by our hypotheses and constructions. Definitions and axioms must be combined, in order to express this idea so far as the purposes of demonstrative reasoning require. These verbal enunciations of the results of the idea cannot be made to depend on each other by logical consequence; but have a mutual dependence of a more intimate kind, which words cannot fully convey. It is not possible to resolve these truths into certain *hypotheses*, of which all the rest shall be the necessary logical consequence. The necessity is not hypothetical, but intuitive. The axioms require not to be granted, but to be seen. If any one were to assent to them without seeing them to be true, his assent would be of no avail for purposes of reasoning: for he would be also unable to see in what cases they might be applied. The clear possession of the Idea of Space is the first requisite for all geometrical

reasoning; and this clearness of idea may be tested by examining whether the axioms offer themselves to the mind as evident.

5. The necessity of ideas added to sensations, in order to produce knowledge, has often been overlooked or denied in modern times. The ground of necessary truth which ideas supply being thus lost, it was conceived that there still remained a ground of necessity in definitions;—that we might have necessary truths, by asserting especially what the definition implicitly involved in general. It was held, also, that this was the case in geometry:—that all the properties of a circle, for instance, were implicitly contained in the definition of a circle. That this alone is not the ground of the necessity of the truths which regard the circle,—that we could not in this way unfold a definition into proportions, without possessing an intuition of the relations to which the definition led,—has already been shown. But the insufficiency of the above account of the grounds of necessary geometrical truth appeared in another way

also. It was found impossible to lay 102 down a system of definitions out of which alone the whole of geometrical truth could be evolved. It was found that axioms could not be superseded. No definition of a straight line could be given which rendered the axiom concerning straight lines superfluous. And thus it appeared that the source of geometrical truths was not definition alone; and we find in this result a confirmation of the doctrine which we are here urging, that this source of truth is to be found in the form or conditions of our perception;—in the idea which we unavoidably combine with the impressions of sense;—in the activity, and not in the passivity of the mind[2].

[2] I formerly stated views similar to these in some ‘Remarks’ appended to a work which I termed *The Mechanical Euclid*, published in 1837. These Remarks, so far as they bear upon the question here discussed, were noticed and controverted in No. 135 of the *Edinburgh Review*. As an examination of the reviewer’s objections may serve further to illustrate the subject, I shall annex to this chapter an answer to the article to which I have referred.

6. This will appear further when we come to consider the mode in which we exercise our observation upon the relations of space. But we may, in the first place, make a remark which tends to show the connexion between our conception of a straight line, and the axiom which is made the foundation of our reasonings concerning space. The axiom is this;—that two straight lines, which have both their ends joined, cannot have the intervening parts separated so as to inclose a space. The necessity of this axiom is of exactly the same kind as the necessity of the definition of a right angle, of which we have already spoken. For as the line standing on another makes *right angles* when it makes the angles on the two sides of it equal; so a line is a *straight line* when it makes the two portions of space, on the two sides of it, similar. And as there is only a single position of the line first mentioned, which can make the angles equal, so there is only a single form of a line which can make the spaces near the line similar on one side and on the other: and 103 therefore there cannot be two straight lines, such as the

axiom describes, which, between the same limits, give two different boundaries to space thus separated. And thus we see a reason for the axiom. Perhaps this view may be further elucidated if we take a leaf of paper, double it, and crease the folded edge. We shall thus obtain a straight line at the folded edge; and this line divides the surface of the paper, as it was originally spread out, into two similar spaces. And that these spaces are similar so far as the fold which separates them is concerned, appears from this;—that these two parts coincide when the paper is doubled. And thus a fold in a sheet of paper at the same time illustrates the definition of a straight line according to the above view, and confirms the axiom that two such lines cannot inclose a space.

If the separation of the two parts of space were made by any other than a straight line; if, for instance, the paper were cut by a concave line; then, on turning one of the parts over, it is easy to see that the edge of one part being concave one way, and the edge of the other part concave the other

way, these two lines would enclose a space. And each of them would divide the whole space into two portions which were not similar; for one portion would have a concave edge, and the other a convex edge. Between any two points, there might be innumerable lines drawn, some, convex one way, and some, convex the other way; but the straight line is the line which is not convex either one way or the other; it is the single medium standard from which the others may deviate in opposite directions.

Such considerations as these show sufficiently that the singleness of the straight line which connects any two points is a result of our fundamental conceptions of space. But yet the above conceptions of the similar form of the two parts of space on the two sides of a line, and of the form of a line which is intermediate among all other forms, are of so vague a nature, that they cannot fitly be made the basis of our elementary geometry; and they are far more conveniently replaced, as they have been in almost all treatises of 104 geometry, by the

axiom, that two straight lines cannot inclose a space.

7. But we may remark that, in what precedes, we have considered space only under one of its aspects:—as a plane. The sheet of paper which we assumed in order to illustrate the nature of a straight line, was supposed to be perfectly *plane* or *flat*: for otherwise, by folding it, we might obtain a line not straight. Now this assumption of a plane appears to take for granted that very conception of a straight line which the sheet was employed to illustrate; for the definition of a plane given in the Elements of Geometry is, that it is a surface on which lie all straight lines drawn from one point of the surface to another. And thus the explanation above given of the nature of a straight line,—that it divides a plane space into similar portions on each side,—appears to be imperfect or nugatory.

To this we reply, that the explanation must be rendered complete and valid by deriving the conception of a plane from

considerations of the same kind as those which we employed for a straight line. Any portion of solid space may be divided into two portions by surfaces passing through any given line or boundaries. And these surfaces may be convex either on one side or on the other, and they admit of innumerable changes from being convex on one side to being convex on the other in any degree. So long as the surface is convex either way, the two portions of space which it separates are not similar, one having a convex and the other a concave boundary. But there is a certain intermediate position of the surface, in which position the two portions of space which it divides have their boundaries exactly similar. In this position, the surface is neither convex nor concave, but plane. And thus a plane surface is determined by this condition—of its being that single surface which is the intermediate form among all convex and concave surfaces by which solid space can be divided,—and of its separating such space into two portions, of which the boundaries, though they are the same surface in two

opposite positions, are exactly similar. 105

Thus a plane is the simplest and most symmetrical boundary by which a solid can be divided; and a straight line is the simplest and most symmetrical boundary by which a plane can be separated. These conceptions are obtained by considering the boundaries of an interminable space, capable of imaginary division in every direction. And as a limited space may be separated into two parts by a plane, and a plane again separated into two parts by a straight line, so a line is divided into two portions by a point, which is the common boundary of the two portions; the end of the one and the beginning of the other portion having itself no magnitude, form, or parts.

8. The geometrical properties of planes and solids are deducible from the first principles of the Elements, without any new axioms; the definition of a plane above quoted,—that all straight lines joining its points lie in the plane,—being a sufficient basis for all reasoning upon these subjects.

And thus, the views which we have presented of the nature of space being verbally expressed by means of certain definitions and axioms, become the groundwork of a long series of deductive reasoning, by which is established a very large and curious collection of truths, namely, the whole science of Elementary Plane and Solid Geometry.

This science is one of indispensable use and constant reference, for every student of the laws of nature; for the relations of space and number are the *alphabet* in which those laws are written. But besides the interest and importance of this kind which geometry possesses, it has a great and peculiar value for all who wish to understand the foundations of human knowledge, and the methods by which it is acquired. For the student of geometry acquires, with a degree of insight and clearness which the unmathematical reader can but feebly imagine, a conviction that there are necessary truths, many of them of a very complex and striking character; and that a

few of the most simple and self-evident truths which it is possible for the mind of man to apprehend, may, by systematic deduction, lead to the most remote and unexpected results. 106

In pursuing such philosophical researches as that in which we are now engaged, it is of great advantage to the speculator to have cultivated to some extent the study of geometry; since by this study he may become fully aware of such features in human knowledge as those which we have mentioned. By the aid of the lesson thus learned from the contemplation of geometrical truths, we have been endeavouriug to establish those further doctrines;—that these truths are but different aspects of the same Fundamental Idea, and that the grounds of the necessity which these truths possess reside in the Idea from which they flow, this Idea not being a derivative result of experience, but its primary rule. When the reader has obtained a clear and satisfactory view of these doctrines, so far as they are applicable to

our knowledge concerning space, he has, we may trust, overcome the main difficulty which will occur in following the course of the speculations now presented to him. He is then prepared to go forwards with us; to see over how wide a field the same doctrines are applicable: and how rich and various a harvest of knowledge springs from these seemingly scanty principles.

But before we quit the subject now under our consideration, we shall endeavour to answer some objections which have been made to the views here presented; and shall attempt to illustrate further the active powers which we have ascribed to the mind.

CHAPTER V.

Of some Objections which have been made to the Doctrines stated in the previous Chapter.[3]

[3] In order to render the present chapter more intelligible, it may be proper to state briefly the arguments which gave occasion to the review. After noticing Stewart's assertions, that the certainty of mathematical reasoning arises from its depending upon definitions, and that mathematical truth is hypothetical; I urged,—that no one has yet been able to construct a system of mathematical truths by the aid of definitions alone; that a definition would not be admissible or applicable except it agreed with a distinct conception in the mind; that the definitions which we employ in mathematics are not arbitrary or hypothetical, but necessary definitions; that if Stewart had taken as his examples of axioms the peculiar geometrical axioms, his assertions would have been obviously erroneous; and that the real foundation of the truths of mathematics is

> the Idea of Space, which may be expressed (for purposes of demonstration) partly by definitions and partly by axioms.

THE *Edinburgh Review*, No. cxxxv., contains a critique on a work termed *The Mechanical Euclid*, in which opinions were delivered to nearly the same effect as some of those stated in the last chapter, and hereafter in Chapter xi. Although I believe that there are no arguments used by the reviewer to which the answers will not suggest themselves in the mind of any one who has read with attention what has been said in the preceding chapters (except, perhaps, one or two remarks which have reference to mechanical ideas), it may serve to illustrate the subject if I reply to the objections directly, taking them as the reviewer has stated them.

I. I had dissented from Stewart's assertion that mathematical truth is hypothetical, or depends upon arbitrary definitions; since we understand by an 108 hypothesis a supposition, not only which we may make, but may abstain from making, or may

replace by a different supposition; whereas the definitions and hypotheses of geometry are necessarily such as they are, and cannot be altered or excluded. The reviewer (p. 84) informs us that he understands Stewart, when he speaks of hypotheses and definitions being the foundation of geometry, to speak of the hypothesis that real objects correspond to our geometrical definitions. '*If* a crystal be an exact hexahedron, the geometrical properties of the hexahedron may be predicated of that crystal.' To this I reply,—that such hypotheses as this are the grounds of our applications of geometrical truths to real objects, but can in no way be said to be the foundation of the truths themselves;—that I do not think that the sense which the reviewer gives was Stewart's meaning;—but that if it was, this view of the use of mathematics does not at all affect the question which both he and I proposed to discuss, which was, the ground of mathematical certainty. I may add, that whether a crystal be an exact hexahedron, is a matter of observation and measurement,

not of definition. I think the reader can have no difficulty in seeing how little my doctrine is affected by the connexion on which the reviewer thus insists. I have asserted that the proposition which affirms the square on the diagonal of a rectangle to be equal to the squares on two sides, does not rest upon arbitrary hypotheses; the objector answers, that the proposition that the square on the diagonal *of this page* is equal to the squares on the sides, depends upon the arbitrary hypothesis that the page is a rectangle. Even if this fact were a matter of arbitrary hypothesis, what could it have to do with the general geometrical proposition? How could a single fact, observed or hypothetical, affect a universal and necessary truth, which would be equally true if the fact were false? If there be nothing arbitrary or hypothetical in geometry till we come to such steps in its application, it is plain that the truths themselves are not hypothetical; which is the question for us to decide. 109

2. The reviewer then (p. 85) considers the

doctrine that axioms as well as definitions are the foundations of geometry; and here he strangely narrows and confuses the discussion by making himself the advocate of Stewart, instead of arguing the question itself. I had asserted that some axioms are necessary as the foundations of mathematical reasoning, in addition to the definitions. If Stewart did not intend to discuss this question, I had no concern with what he had said about axioms. But I had every reason to believe that this was the question which Stewart did intend to discuss. I conceive there is no doubt that he intended to give an opinion upon the grounds of mathematical reasoning in general. For he begins his discussions (*Elements*, vol. ii. p. 38) by contesting Reid's opinion on this subject, which is stated generally; and he refers again to the same subject, asserting in general terms, that the first principles of mathematics are not axioms but definitions. If, then, afterwards, he made his proof narrower than his assertion;—if having declared that no axioms are necessary, he afterwards limited

himself to showing that seven out of twelve of Euclid's axioms are barren truisms, it was no concern of mine to contest this assertion, which left my thesis untouched. I had asserted that the proper geometrical axioms (that two straight lines cannot inclose a space, and the axiom about parallel lines) are indispensable in geometry. What account the reviewer gives of these axioms we shall soon see; but if Stewart allowed them to be axioms necessary to geometrical reasoning, he overturned his own assertion as to the foundations of such reasoning; and if he said nothing decisive about these axioms, which are the points on which the battle must turn, he left his assertion altogether unproved; nor was it necessary for me to pursue the war into a barren and unimportant corner, when the metropolis was surrendered. The reviewer's exultation that I have not contested the first seven axioms is an amusing example of the self-complacent zeal of advocacy.

3. But let us turn to the material point,—the proper geometrical axioms. What is the

reviewer's account of 110 these? Which side of the alternative does he adopt? Do they depend upon the definitions, and is he prepared to show the dependence? Or are they superfluous, and can he erect the structure of geometry without their aid? One of these two courses, it would seem, he must take. For we both begin by asserting the excellence of geometry as an example of demonstrated truth. It is precisely this attribute which gives an interest to our present inquiry. How, then, does the reviewer explain this excellence on his views? How does he reckon the foundation courses of the edifice which we agree in considering as a perfect example of intellectual building?

I presume I may take, as his answer to this question, his hypothetical statement of what Stewart would have said (p. 87), on the supposition that there had been, among the foundations of geometry, self-evident indemonstrable truths: although it is certainly strange that the reviewer should not venture to make up his mind as to the

truth or falsehood of this supposition. If there were such truths they would be, he says, 'legitimate filiations' of the definitions. They would be involved in the definitions. And again he speaks of the foundation of the geometrical doctrine of parallels as a flaw, and as a truth which requires, but has not received demonstration. And yet again, he tells us that each of these supposed axioms (Euclid's twelfth, for instance) is 'merely an indication of the point at which geometry fails to perform that which it undertakes to perform' (p. 91); and that in reality her truths are not yet demonstrated. The amount of this is, that the geometrical axioms are to be held to be *legitimate filiations* of the definitions, because though certainly true, they cannot be proved from the definitions; that they are involved in the definitions, although they cannot be evolved out of them; and that rather than admit that they have any other origin than the definitions, we are to proclaim that geometry has failed to perform what she undertakes to perform.

To this I reply—that I cannot understand what is meant by ‘legitimate filiations’ of principles, if the 111 phrase do not mean consequences of such principles established by rigorous and formal demonstrations;—that the reviewer, if he claims any real signification for his phrase, must substantiate the meaning of it by such a demonstration; he must establish his ‘legitimate filiation’ by a genealogical table in a satisfactory form. When this cannot be done, to assert, notwithstanding, that the propositions are involved in the definitions, is a mere begging the question; and to excuse this defect by saying that geometry fails to perform what she has promised, is to calumniate the character of that science which we profess to make our standard, rather than abandon an arbitrary and unproved assertion respecting the real grounds of her excellence. I add, further, that if the doctrine of parallel lines, or any other geometrical doctrine of which we see the truth, with the most perfect insight of its necessity, have not hitherto received demonstration to the satisfaction of any

school of reasoners, the defect must arise from their erroneous views of the nature of demonstrations, and the grounds of mathematical certainty.

4. I conceive, then, that the reviewer has failed altogether to disprove the doctrine that the axioms of geometry are necessary as a part of the foundations of the science. I had asserted further that these axioms supply what the definitions leave deficient; and that they, along with definitions, serve to present the idea of space under such aspects that we can reason logically concerning it. To this the reviewer opposes (p. 96) the common opinion that a perfect definition is a complete explanation of a name, and that the test of its perfection is, that we may substitute the definition for the name wherever it occurs. I reply, that my doctrine, that a definition expresses a part, but not the whole, of the essential characters of an idea, is certainly at variance with an opinion sometimes maintained, that a definition merely explains a word, and should explain it so

fully that it may always replace it. The error of this common opinion may, I think, be shown from considerations such as these;—that if 112 we undertake to explain one word by several, we may be called upon, on the same ground, to explain each of these several by others, and that in this way we can reach no limit nor resting-place;—that in point of fact, it is not found to lead to clearness, but to obscurity, when in the discussion of general principles, we thus substitute definitions for single terms;—that even if this be done, we cannot reason without conceiving what the terms mean;—and that, in doing this, the relations of our conceptions, and not the arbitrary equivalence of two forms of expression, are the foundations of our reasoning.

5. The reviewer conceives that some of the so-called axioms are really definitions. The axiom, that ‘magnitudes which coincide with each other, that is, which fill the same space, are equal,’ is a definition of geometrical *equality*: the axiom, that ‘the whole is greater than its part,’ is a definition

of *whole* and *part*. But surely there are very serious objections to this view. It would seem more natural to say, if the former axiom is a definition of the word *equal*, that the latter is a definition of the word *greater*. And how can one short phrase define two terms? If I say, 'the heat of summer is greater than the heat of winter,' does this assertion define anything, though the proposition is perfectly intelligible and distinct? I think, then, that this attempt to reduce these axioms to definitions is quite untenable.

6. I have stated that a definition can be of no use, except we can conceive the possibility and truth of the property connected with it; and that if we do conceive this, we may rightly begin our reasonings by stating the property as an axiom; which Euclid does, in the case of straight lines and of parallels. The reviewer inquires (p. 92), whether I am prepared to extend this doctrine to the case of circles, for which the reasoning is usually rested upon the definition;—whether I would

replace this definition by an axiom, asserting the possibility of such a circle. To this I might reply, that it is not at all incumbent upon me to assent to such a change; for I have all along stated that it is indifferent 113 whether the fundamental properties from which we reason be exhibited as definitions or as axioms, provided the necessity be clearly seen. But I am ready to declare that I think the form of our geometry would be not at all the worse, if, instead of the usual definition of a circle, —'that it is a figure contained by one line, which is called the circumference, and which is such, that all straight lines drawn from a certain point within the circumference are equal to one another,'— we were to substitute an axiom and a definition, as follows:—

Axiom. If a line be drawn so as to be at every point equally distant from a certain point, this line will return into itself or will be *one* line including a space.

Definition. The space is called a *circle*, the line the *circumference*, and the point the *center*.

And this being done, it would be true, as the reviewer remarks, that geometry cannot stir *one* step without resting on an axiom. And I do not at all hesitate to say, that the above axiom, expressed or understood, is no less necessary than the definition, and is tacitly assumed in every proposition into which circles enter.

7. I have, I think, now disposed of the principal objections which bear upon the proper axioms of geometry. The principles which are stated as the first seven axioms of Euclid's *Elements*, need not, as I have said, be here discussed. They are principles which refer, not to Space in particular, but to Quantity in general: such, for instance, as these; 'If equals be added to equals the wholes are equal;'—'If equals be taken from equals the remainders are equal.' But I will make an observation or two upon them before I proceed.

Both Locke and Stewart have spoken of these axioms as barren truisms: as propositions from which it is not possible to

deduce a single inference: and the reviewer asserts that they are not first principles, but laws of thought (p. 88). To this last expression I am 114 willing to assent; but I would add, that not only these, but all the principles which express the fundamental conditions of our knowledge, may with equal propriety be termed laws of thought; for these principles depend upon our ideas, and regulate the active operations of the mind, by which coherence and connexion are given to its passive impressions. But the assertion that no conclusions can be drawn from simple axioms, or laws of human thought, which regard quantity, is by no means true. The whole of arithmetic,—for instance, the rules for the multiplication and division of large numbers, the rule for finding a common measure, and, in short, a vast body of theory respecting numbers,—rests upon no other foundation than such axioms as have been just noticed, that if equals be added to equals the wholes will be equal. And even when Locke's assertion, that from these axioms no truths can be deduced, is modified by Stewart and the

reviewer, and limited to *geometrical* truths, it is hardly tenable (although, in fact, it matters little to our argument whether it is or no). For the greater part of the Seventh Book of Euclid's *Elements*, (on Commensurable and Incommensurable Quantities,) and the Fifth Book, (on Proportion,) depend upon these axioms, with the addition only of the definition or axiom (for it may be stated either way) which expresses the idea of proportionality in numbers. So that the attempt to disprove the necessity and use of axioms, as principles of reasoning, fails even when we take those instances which the opponents consider as the more manifestly favourable to their doctrine.

8. But perhaps the question may have already suggested itself to the reader's mind, of what use can it be formally to state such principles as these, (for example, that if equals be added to equals the wholes are equal,) since, whether stated or no, they will be assumed in our reasoning? And how can such principles be said to be necessary,

when our proof proceeds equally well without any reference to them? And the answer is, that it is precisely because these are the 115 common principles of reasoning, which we naturally employ without specially contemplating them, that they require to be separated from the other steps and formally stated, when we *analyse* the demonstrations which we have obtained. In every mental process many principles are combined and abbreviated, and thus in some measure concealed and obscured. In analysing these processes, the combination must be resolved, and the abbreviation expanded, and thus the appearance is presented of a pedantic and superfluous formality. But that which is superfluous for proof, is necessary for the analysis of proof. In order to exhibit the conditions of demonstration distinctly, they must be exhibited formally. In the same manner, in demonstration we do not usually express every step in the form of a syllogism, but we see the grounds of the conclusiveness of a demonstration, by resolving it into syllogisms. Neither axioms nor syllogisms

are necessary for conviction; but they are necessary to display the conditions under which conviction becomes inevitable. The application of a single one of the axioms just spoken of is so minute a step in the proof, that it appears pedantic to give it a marked place; but the very essence of demonstration consists in this, that it is composed of an indissoluble succession of such minute steps. The admirable circumstance is, that by the accumulation of such apparently imperceptible advances, we can in the end make so vast and so sure a progress. The completeness of the analysis of our knowledge appears in the smallness of the elements into which it is thus resolved. The minuteness of any of these elements of truth, of axioms for instance, does not prevent their being as essential as others which are more obvious. And any attempt to assume one kind of element only, when the course of our analysis brings before us two or more kinds, is altogether unphilosophical. Axioms and definitions are the proximate constituent principles of our demonstrations; and the intimate bond

which connects together a definition and an axiom on the same subject is not truly expressed 116 by asserting the latter to be derived from the former. This bond of connexion exists in the mind of the reasoner, in his conception of *that* to which both definition and axiom refer, and consequently in the general Fundamental Idea of which that conception is a modification.

CHAPTER VI.

Of the Perception of Space.

1. According to the views above explained, certain of the impressions of our senses convey to us the perception of objects as existing in space; inasmuch as by the constitution of our minds we cannot receive those impressions otherwise than in a certain form, involving such a manner of existence. But the question deserves to be asked, *What* are the impressions of sense by which we thus become acquainted with space and its relations? And as we have seen that this idea of space implies an act of the mind as well as an impression on the sense, what manifestations do we find of this activity of the mind, in our observation of the external world?

It is evident that sight and touch are the

senses by which the relations of space are perceived, principally or entirely. It does not appear that an odour, or a feeling of warmth or cold, would, independently of experience, suggest to us the conception of a space surrounding us. But when we *see* objects, we see that they are extended and occupy space; when we *touch* them, we feel that they are in a space in which we also are. We have before our eyes any object, for instance, a board covered with geometrical diagrams; and we distinctly perceive, by vision, those lines of which the relations are the subjects of our mathematical reasoning. Again, we see before us a solid object, a cubical box for instance; we see that it is within reach; we stretch out the hand and perceive by the touch that it has sides, edges, corners, which we had already perceived by vision. 118

2. Probably most persons do not generally apprehend that there is any material difference in these two cases;—that there are any different acts of mind concerned in perceiving by sight a mathematical diagram

upon paper, and a solid cube lying on a table. Yet it is not difficult to show that, in the latter case at least, the perception of the shape of the object is not immediate. A very little attention teaches us that there is an act of judgment as well as a mere impression of sense requisite, in order that we may see any solid object. For there is no visible appearance which is inseparably connected with solidity. If a picture of a cube be rightly drawn, in perspective and skilfully shaded, the impression upon the sense is the same as if it were a real cube. The picture may be mistaken for a solid object. But it is clear that, in this case, the solidity is given to the object by an act of mental judgment. All that is seen is outline and shade, figures and colours on a flat board. The solid angles and edges, the relation of the faces of the figure by which they form a cube, are matters of inference. This, which is evident in the case of the pictured cube, is true in all vision whatever. We see a scene before us on which are various figures and colours, but the eye cannot see more. It sees length and breadth, but no third dimension. In

order to know that there are solids, we must infer as well as see. And this we do readily and constantly; so familiarly, indeed, that we do not perceive the operation. Yet we may detect this latent process in many ways; for instance, by attending to cases in which the habit of drawing such inferences misleads us. Most persons have experienced this delusion in looking at a scene in a theatre, and especially that kind of scene which is called a *diorama*, when the interior of a building is represented. In these cases, the perspective representations of the various members of the architecture and decoration impress us almost irresistibly with the conviction that we have before us a space of great extent and complex form, instead of a flat painted canvass. Here, at least, the space is our own creation; but yet here, it is 119 manifestly created by the same act of thought as if we were really in the palace or the cathedral of which the halls and aisles thus seem to inclose us. And the act by which we thus create space of three dimensions out of visible extent of length and breadth, is constantly and

imperceptibly going on. We are perpetually interpreting in this manner the language of the visible world. From the appearances of things which we directly see, we are constantly inferring that which we cannot directly see,—their distance from us, and the position of their parts.

3. The characters which we thus interpret are various. They are, for instance, the visible forms, colours, and shades of the parts, understood according to the maxims of perspective; (for of perspective every one has a practical knowledge, as every one has of grammar;) the effort by which we fix both our eyes on the same object, and adjust each eye to distinct vision; and the like. The right interpretation of the information which such circumstances give us respecting the true forms and distances of things, is gradually learned; the lesson being begun in our earliest infancy, and inculcated upon us every hour during which we use our eyes. The completeness with which the lesson is mastered is truly admirable; for we forget that our conclusion is obtained indirectly,

and mistake a judgment on evidence for an intuitive perception. We see the breadth of the street, as clearly and readily as we see the house on the other side of it; and we see the house to be square, however obliquely it be presented to us. This, however, by no means throws any doubt or difficulty on the doctrine that in all these cases we do interpret and infer. The rapidity of the process, and the unconsciousness of the effort, are not more remarkable in this case than they are when we understand the meaning of the speech which we hear, or of the book which we read. In these latter cases we merely hear noises or see black marks; but we make, out of these elements, thought and feeling, without being aware of the act by which we do so. And by an exactly similar process we see a variously-coloured 120 expanse, and collect from it a space occupied by solid objects. In both cases the act of interpretation is become so habitual that we can hardly stop short at the mere impression of sense.

4. But yet there are various ways in which

we may satisfy ourselves that these two parts of the process of seeing objects are distinct. To separate these operations is precisely the task which the artist has to execute, in making a drawing of what he sees. He has to recover the consciousness of his real and genuine sensations, and to discern the lines of objects as they appear. This at first he finds difficult; for he is tempted to draw what he knows of the forms of visible objects, and not what he sees: but as he improves in his art, he learns to put on paper what he sees only, separated from what he infers, in order that thus the inference, and with it a conception like that of the reality, may be left to the spectator. And thus the natural process of vision is the habit of seeing that which cannot be seen; and the difficulty of the art of drawing consists in learning not to see more than is visible.

5. But again; even in the simplest drawing we exhibit something which we do not see. However slight is our representation of objects, it contains something which we

create for ourselves. For we draw an *outline*. Now an outline has no existence in nature. There are no visible lines presented to the eye by a group of figures. We separate each figure from the rest, and the boundary by which we do this is the outline of the figure; and the like may be said of each member of every figure. A painter of our own times has made this remark in a work upon his art[4]: 'The effect which natural objects produce upon our sense of vision is that of a number of parts, or distinct masses of form and colour, and not of lines. But when we endeavour to represent by painting the objects which are before us, or which invention supplies to our minds, 121 the first and the simplest means we resort to is this picture, by which we separate the form of each object from those that surround it, marking its boundary, the extreme extent of its dimensions in every direction, as impressed on our vision: and this is termed drawing its outline.'

[4] Phillips *On Painting*.

6. Again, there are other ways in which

we see clear manifestations of the act of thought by which we assign to the parts of objects their relations in space, the impressions of sense being merely subservient to this act. If we look at a medal through a glass which inverts it, we see the figures upon it become concave depressions instead of projecting convexities; for the light which illuminates the nearer side of the convexity will be transferred to the opposite side by the apparent inversion of the medal, and will thus imply a hollow in which the side nearest the light gathers the shade. Here our decision as to which part is nearest to us, has reference to the side from which the light comes. In other cases the decision is more spontaneous. If we draw black outlines, such as represent the edges of a cube seen in perspective, certain of the lines will cross each other; and we may make this cube appear to assume two different positions, by determining in our own mind that the lines which belong to one end of the cube shall be understood to be before or to be behind those which they cross. Here an act of the will, operating

upon the same sensible image, gives us two cubes, occupying two entirely different positions. Again, many persons may have observed that when a windmill in motion at a distance from us, (so that the outline of the sails only is seen,) stands obliquely to the eye, we may, by an effort of thought, make the obliquity assume one or the other of two positions; and as we do this, the sails, which in one instance appear to turn from right to left, in the other case turn from left to right. A person a little familiar with this mental effort, can invert the motion as often as he pleases, so long as the conditions of form and light do not offer a manifest contradiction to either position. 122.

Thus we have these abundant and various manifestations of the activity of the mind, in the process by which we collect from vision the relations of solid space of three dimensions. But we must further make some remarks on the process by which we perceive mere visible figure; and also, on the mode in which we perceive the relations

of space by the touch; and first, of the latter subject.

7. The opinion above illustrated, that our sight does not give us a direct knowledge of the relations of solid space, and that this knowledge is acquired only by an inference of the mind, was first clearly taught by the celebrated Bishop Berkeley[5], and is a doctrine now generally assented to by metaphysical speculators.

[5] *Theory of Vision.*

But does the sense of *touch* give us directly a knowledge of space? This is a question which has attracted considerable notice in recent times; and new light has been thrown upon it in a degree which is very remarkable, when we consider that the philosophy of perception has been a prominent subject of inquiry from the earliest times. Two philosophers, advancing to this inquiry from different sides, the one a metaphysician, the other a physiologist, have independently arrived at the conviction that the long current opinion,

according to which we acquire a knowledge of space by the sense of touch, is erroneous. And the doctrine which they teach instead of the ancient errour, has a very important bearing upon the principle which we are endeavouring to establish,—that our knowledge of space and its properties is derived rather from the active operations than from the passive impressions of the percipient mind.

Undoubtedly the persuasion that we acquire a knowledge of form by the touch is very obviously suggested by our common habits. If we wish to know the form of any body in the dark, or to correct the impressions conveyed by sight, when we suspect them to be false, we have only, it seems to us, at least at first, to stretch forth the hand and touch the object; and we learn its 123 shape with, no chance of errour. In these cases, form appears to be as immediate a perception of the sense of touch, as colour is of the sense of sight.

8. But is this perception really the result

of the passive sense of touch merely? Against such an opinion Dr. Brown, the metaphysician of whom I speak, urges[6] that the feeling of touch alone, when any object is applied to the hand, or any other part of the body, can no more convey the conception of form or extension, than the sensation of an odour or a taste can do, except we have already some knowledge of the relative position of the parts of our bodies; that is, except we are already in possession of an idea of space, and have, in our minds, referred our limbs to their positions; which is to suppose the conception of form already acquired.

[6] *Lectures*, Vol. i. p. 459, (1824).

9. By what faculty then do we originally acquire our conceptions of the relations of position? Brown answers by the *muscular sense*; that is, by the conscious exertions of the various muscles by which we move our limbs. When we feel out the form and position of bodies by the hand, our knowledge is acquired, not by the mere touch of the body, but by perceiving the

course the fingers must take in order to follow the surface of the body, or to pass from one body to another. We are conscious of the slightest of the volitions by which we thus feel out form and place; we know whether we move the finger to the right or left, up or down, to us or from us, through a large or a small space; and all these conscious acts are bound together and regulated in our minds by an idea of an extended space in which they are performed. That this idea of space is not borrowed from the sight, and transferred to the muscular feelings by habit, is evident. For a man born blind can feel out his way with his staff, and has his conceptions of position determined by the conditions of space, no less than one who has the use of his eyes. And the muscular consciousness which reveals to us the position of objects and parts of objects, 124 when we feel them out by means of the hand, shows itself in a thousand other ways, and in all our limbs: for our habits of standing, walking, and all other attitudes and motions, are regulated by our feeling of our position and that of

surrounding objects. And thus, we cannot touch any object without learning something respecting its position; not that the sense of touch directly conveys such knowledge; but we have already learnt, from the muscular sense, constantly exercised, the position of the limb which the object thus touches.

10. The justice of this distinction will, I think, be assented to by all persons who attend steadily to the process itself, and might be maintained by many forcible reasons. Perhaps one of the most striking evidences in its favour is that, as I have already intimated, it is the opinion to which another distinguished philosopher, Sir Charles Bell, has been led, reasoning entirely upon physiological principles. From his researches it resulted that besides the nerves which convey the impulse of the will from the brain to the muscle, by which every motion of our limbs is produced, there is another set of nerves which carry back to the brain a sense of the condition of the muscle, and thus regulate its activity;

and give us the consciousness of our position and relation to surrounding objects. The motion of the hand and fingers, or the consciousness of this motion, must be combined with the sense of touch properly so called, in order to make an inlet to the knowledge of such relations. This consciousness of muscular exertion, which he has called a sixth sense[7], is our guide, Sir C. Bell shows, in the common practical government of our motions; and he states that having given this explanation of perception as a physiological doctrine, he had afterwards with satisfaction seen it confirmed by Dr. Brown's speculations.

[7] *Bridgewater Treatise*, p. 195. *Phil. Trans.* 1826, Pt. ii. p. 167.

11. Thus it appears that our consciousness of the relations of space is inseparably and fundamentally connected with our own actions in space. We perceive 125 only while we act; our sensations require to be interpreted by our volitions. The apprehension of extension and figure is far from being a process in which we are inert

and passive. We draw lines with our fingers; we construct surfaces by curving our hands; we generate spaces by the motion of our arms. When the geometer bids us form lines, or surfaces, or solids by motion, he intends his injunction to be taken as hypothetical only; we need only conceive such motions. But yet this hypothesis represents truly the origin of our knowledge; we perceive spaces by motion at first, as we conceive spaces by motion afterwards:—or if not always by actual motion, at least by potential. If we perceive the length of a staff by holding its two ends in our two hands without running the finger along it, this is because by habitual motion we have already acquired a measure of the distance of our hands in any attitude of which we are conscious. Even in the simplest case, our perceptions are derived not from the touch, but from the sixth sense; and this sixth sense at least, whatever may be the case with the other five, implies an active mind along with the passive sense.

12. Upon attentive consideration, it will

be clear that a large portion of the perceptions respecting space which appear at first to be obtained by sight alone, are, in fact, acquired by means of this sixth sense. Thus we consider the visible sky as a single surface surrounding us and returning into itself, and thus forming a hemisphere. But such a mode of conceiving an object of vision could never have occurred to us, if we had not been able to turn our heads, to follow this surface, to pursue it till we find it returning into itself. And when we have done this, we necessarily present it to ourselves as a concave inclosure within which we are. The sense of sight alone, without the power of muscular motion, could not have led us to view the sky as a vault or hemisphere. Under such circumstances, we should have perceived only what was presented to the eye in one position; and if different appearances had been presented in succession, we could 126 not have connected them as parts of the same picture, for want of any perception of their relative position. They would have been so many detached and incoherent

visual sensations. The muscular sense connects their parts into a whole, making them to be only different portions of one universal scene[8].

[8] It has been objected to this view that we might obtain a conception of the sky as a hemisphere, by being ourselves turned round, (as on a music-stool, for instance,) and thus seeing in succession all parts of the sky. But this assertion I conceive to be erroneous. By being thus turned round, we should see a number of pictures which we should put together as parts of a plane picture; and when we came round to the original point, we should have no possible means of deciding that it was the *same* point: it would appear only as a *repetition* of the picture. That sight, of itself, can give us only a plane picture, the doctrine of Berkeley, appears to be indisputable; and, no less so, the doctrine that it is the consciousness of our own action in space which puts together these pictures so that they cover the surface of a solid body. We can see length and breadth with our eyes, but we must thrust out our arm towards the flat surface, in order that we may, in our thoughts, combine a third dimension with the other two.

13. These considerations point out the fallacy of a very curious representation made by Dr. Reid, of the convictions to

which man would be led, if he possessed vision without the sense of touch. To illustrate this subject, Reid uses the fiction of a nation whom he terms the *Idomenians*, who have no sense except that of sight. He describes their notions of the relations of space as being entirely different from ours. The axioms of their geometry are quite contradictory to our axioms. For example, it is held to be self-evident among them that two straight lines which intersect each other once, must intersect a second time; that the three angles of any triangle are *greater* than two right angles; and the like. These paradoxes are obtained by tracing the relations of lines on the surface of a concave sphere, which surrounds the spectator, and on which all visible appearances may be supposed to be presented to him. But from what is said above it appears that the notion of such a sphere, and such a connexion of visible objects which are seen in different 127 directions, cannot be arrived at by sight alone. When the spectator combines in his conception the relations of long-drawn lines and large figures, as he sees them by

turning his head to the right and to the left, upwards and downwards, he ceases to be an Idomenian. And thus our conceptions of the properties of space, derived through the exercise of one mode of perception, are not at variance with those obtained in another way; but all such conceptions, however produced or suggested, are in harmony with each other; being, as has already been said, only different aspects of the same idea.

14. If our perceptions of the position of objects around us do not depend on the sense of vision alone, but on the muscular feeling brought into play when we turn our head, it will obviously follow that the same is true when we turn the eye instead of the head. And thus we may learn the form of objects, not by looking at them with a fixed gaze, but by following the boundary of them with the eye. While the head is held perfectly still, the eye can rove along the outlines of visible objects, scrutinize each point in succession, and leap from one point to another; each such act being accompanied by a muscular consciousness

which makes us aware of the direction in which the look is travelling. And we may thus gather information concerning the figures and places which we trace out with the visual ray, as the blind man learns the forms of things which he traces out with his staff, being conscious of the motions of his hand.

15. This view of the mode in which the eye perceives position, which is thus supported by the analogy of other members employed for the same purpose, is further confirmed by Sir Charles Bell by physiological reasons. He teaches us that[9] when an object is seen we employ two senses: there is an impression on the retina; but we receive also the idea of position or relation in space, which it is not the office of the retina to give, by our consciousness of the efforts of the voluntary 128 muscles of the eye: and he has traced in detail the course of the nerves by which these muscles convey their information. The constant *searching* motion of the eye, as he terms it[10], is the means by which we become aware of

the position of objects about us.

[9] *Phil. Trans.* 1823. On the Motions of the Eye.

[10] *Bridgewater Treatise*, p. 282. I have adopted, in writing the above, the views and expressions of Sir Charles Bell. The essential part of the doctrine there presented is, that the eye constantly makes efforts to turn, so that the image of an object to which our attention is drawn, shall fall upon a certain particular point of the retina; and that when the image falls upon any other point, the eye turns away from this oblique into the direct position. Other writers have maintained that the eye thus turns not because the point on which the image falls in direct vision is the most *sensible* point, but that it is the point of *greatest distinctness* of vision. They urge that a small star, which disappears when the eye is turned full upon it, may often be seen by looking a little away from it: and hence, they infer that the parts of the retina removed from the spot of direct vision, are more sensible than it is. The facts are very curious, however they be explained, but they do not disturb the doctrine delivered in the text.

16. It is not to our present purpose to follow the physiology of this subject; but we may notice that Sir C. Bell has examined the special circumstances which belong to this

operation of the eye. We learn from him that the particular point of the eye which thus traces the forms of visible objects is a part of the retina which has been termed the *sensible spot*; being that part which is *most distinctly* sensible to the impressions of light and colour. This part, indeed, is not a spot of definite size and form, for it appears that proceeding from a certain point of the retina, the distinct sensibility diminishes on every side by degrees. And the searching motion of the eye arises from the desire which we instinctively feel of receiving upon the sensible spot the image of the object to which the attention is directed. We are uneasy and impatient till the eye is turned so that this is effected. And as our attention is transferred from point to point of the scene before us, the eye, and this point of the eye in particular, travel along with the thoughts; and the muscular sense, which tells us of these movements of the organ of 129 vision, conveys to us a knowledge of the forms and places which we thus successively survey.

17. How much of activity there is in the process by which we perceive the outlines of objects appears further from the language by which we describe their forms. We apply to them not merely adjectives of form, but verbs of motion. An abrupt hill *starts* out of the plain; a beautiful figure has a *gliding* outline. We have

> The windy summit, wild and high,
>
> Roughly *rushing* on the sky.

These terms express the course of the eye as it follows the lines by which such forms are bounded and marked. In like manner another modern poet[11] says of Soracte, that it

> From out the plain
>
> *Heaves* like a long-swept wave about to break,
>
> And on the curl *hangs pausing*.

[11] Byron, *Ch. Har.* vi. st. 75.

Thus the muscular sense, which is inseparably connected with an act

originating in our own mind, not only gives us all that portion of our perceptions of space in which we use the sense of touch, but also, at least in a great measure, another large portion of such perceptions, in which we employ the sense of sight. As we have before seen that our *knowledge* of solid space and its properties is not conceivable in any other way than as the result of a mental act, governed by conditions depending on its own nature; so it now appears that our *perceptions* of visible figure are not obtained without an act performed under the same conditions. The sensations of touch and sight are subordinated to an idea which is the basis of our speculative knowledge concerning space and its relations; and this same idea is disclosed to our consciousness by its practically regulating our intercourse with the external world.

By considerations such as have been adduced and referred to, it is proved beyond doubt, that in a great 130 number of cases our knowledge of form and position is acquired from the muscular sense, and not

from sight directly:—for instance, in all cases in which we have before us objects so large and prospects so extensive that we cannot see the whole of them in one position of the eye[12].

[12] The expression in the first edition was ‘large objects and extensive spaces.’ In the text as now given, I state a definite size and extent, within which the sight by itself can judge of position and figure.

The doctrine, that we require the assistance of the muscular sense to enable us to perceive space of three dimensions, is not at all inconsistent with this other doctrine, that within the space which is seen by the fixed eye, we perceive the relative positions of points directly by vision, and that, consequently, we have a perception of *visible figure*.

Sir Charles Bell has said, (*Phil. Trans.* 1823, p. 181,) ‘It appears to me that the utmost ingenuity will be at a loss to devise an explanation of that power by which the eye becomes acquainted with the position and relation of objects, if the sense of muscular activity be excluded which accompanies the motion of the eyeball.’ But surely we should have no difficulty in perceiving the relation of the sides and angles of a small triangle, placed before the eye, even if the muscles of the eyeball were severed. This subject is resumed b. iv. c. ii. sect. 11.

We now quit the consideration of the properties of Space, and consider the Idea of Time.

CHAPTER VII.

Of the Idea of Time.

1. RESPECTING the Idea of Time, we may make several of the same remarks which we made concerning the idea of space, in order to show that it is not borrowed from experience, but is a bond of connexion among the impressions of sense, derived from a peculiar activity of the mind, and forming a foundation both of our experience and of our speculative knowledge.

Time is not a notion obtained by experience. Experience, that is, the impressions of sense and our consciousness of our thoughts, gives us various perceptions; and different successive perceptions considered together exemplify the notion of change. But this very

connexion of different perceptions,—this successiveness,—presupposes that the perceptions exist *in time*. That things happen either together, or one after the other, is intelligible only by assuming time as the condition under which they are presented to us.

Thus time is a necessary condition in the presentation of all occurrences to our minds. We cannot conceive this condition to be taken away. We can conceive time to go on while nothing happens in it; but we cannot conceive anything to happen while time does not go on.

It is clear from this that time is not an impression derived from experience, in the same manner in which we derive from experience our information concerning the objects which exist, and the occurrences which take place in time. The objects of experience can easily be conceived to be, or not to be:—to be absent as well as present. Time always is, and always is 132 present, and even in our thoughts we cannot form

the contrary supposition.

2. Thus time is something distinct from the *matter* or substance of our experience, and may be considered as a necessary *form* which that matter (the experience of change) must assume, in order to be an object of contemplation to the mind. Time is one of the necessary conditions under which we apprehend the information which our senses and consciousness give us. By considering time as a form which belongs to our power of apprehending occurrences and changes, and under which alone all such experience can be accepted by the mind, we explain the necessity, which we find to exist, of conceiving all such changes as happening in time; and we thus see that time is not a property perceived as existing in objects, or as conveyed to us by our senses; but a condition impressed upon our knowledge by the constitution of the mind itself; involving an act of thought as well as an impression of sense.

3. We showed that space is an idea of the

mind, or form of our perceiving power, independent of experience, by pointing out that we possess necessary and universal truths concerning the relations of space, which could never be given by means of experience; but of which the necessity is readily conceivable, if we suppose them to have for their basis the constitution of the mind. There exist also respecting number, many truths absolutely necessary, entirely independent of experience and anterior to it; and so far as the conception of number depends upon the idea of time, the same argument might be used to show that the idea of time is not derived from experience, but is a result of the native activity of the mind: but we shall defer all views of this kind till we come to the consideration of Number.

4. Some persons have supposed that we obtain the notion of time from the perception of motion. But it is clear that the perception of motion, that is, change of place, presupposes the conception of time, and is not capable of being presented to the

mind in any other 133 way. If we contemplate the same body as being in different places at different times, and connect these observations, we have the conception of motion, which thus presupposes the necessary conditions that existence in time implies. And thus we see that it is possible there should be necessary truths concerning all Motion, and consequently, concerning those motions which are the objects of experience; but that the source of this necessity is the Ideas of Time and Space, which, being universal conditions of knowledge residing in the mind, afford a foundation for necessary truths.

CHAPTER VIII.

Of some Peculiarities of the Idea of Time.

1. THE Idea of Time, like the Idea of Space, offers to our notice some characters which do not belong to our fundamental ideas generally, but which are deserving of remark. These characters are, in some respects, closely similar with regard to Time and to Space, while, in other respects, the peculiarities of these two ideas are widely different. We shall point out some of these characters.

Time is not a general *abstract* notion collected from experience; as, for example, a certain general conception of the relations of things. For we do not consider particular *times* as examples of Time in general, (as we consider particular causes to be examples of

Cause,) but we conceive all particular times to be parts of a single and endless Time. This continually-flowing and endless time is what offers itself to us when we contemplate any series of occurrences. All actual and possible times exist as Parts, in this original and general Time. And since all particular times are considered as derivable from time in general, it is manifest that the notion of time in general cannot be derived from the notions of particular times. The notion of time in general is therefore not a general conception gathered from experience.

2. Time is infinite. Since all actual and possible times exist in the general course of time, this general time must be infinite. All limitation merely divides, and does not terminate, the extent of absolute time. Time has no beginning and no end; but the beginning and the end of every other existence takes place in it.

3. Time, like space, is not only a form of perception, but of *intuition*. We contemplate

events as 135 taking place *in* time. We consider its parts as added to one another, and events as filling a larger or smaller extent of such parts. The time which any event takes up is the sum of all such parts, and the relation of the same to time is fully understood when we can clearly see what portions of time it occupies, and what it does not. Thus the relation of known occurrences to time is perceived by intuition; and time is a form of intuition of the external world.

4. Time is conceived as a quantity of one dimension; it has great analogy with a line, but none at all with a surface or solid. Time may be considered as consisting of a series of instants, which are before and after one another; and they have no other relation than this, of *before* and *after*. Just the same would be the case with a series of points taken along a line; each would be after those on one side of it, and before those on another. Indeed the analogy between time, and space of one dimension, is so close, that the same terms are applied to both ideas,

and we hardly know to which they originally belong. Times and lines are alike called *long* and *short*; we speak of the *beginning* and *end* of a line; of a *point* of time, and of the *limits* of a portion of duration.

5. But, as has been said, there is nothing in time which corresponds to more than one dimension in space, and hence nothing which has any obvious analogy with figure. Time resembles a line indefinitely extended both ways; all partial times are portions of this line; and no mode of conceiving time suggests to us a line making any angle with the original line, or any other combination which might give rise to figures of any kind. The analogy between time and space, which in many circumstances is so clear, here disappears altogether. Spaces of two and of three dimensions, planes and solids, have nothing to which we can compare them in the conceptions arising out of time.

6. As figure is a conception solely appropriate to space, there is also a

conception which peculiarly belongs to time, namely, the conception of recurrence of times similarly marked; or, as it may be termed, 136 *rhythm*, using this word in a general sense. The term rhythm is most commonly used to designate the recurrence of times marked by the syllables of a verse, or the notes of a melody: but it is easy to see that the general conception of such a recurrence does not depend on the mode in which it is impressed upon the sense. The forms of such recurrence are innumerable. Thus in such a line as

Quádrupedánte putrém sonitú quatit úngula cámpum,

we have alternately one long or forcible syllable, and two short or light ones, recurring over and over. In like manner in our own language, in the line

At the clóse of the dáy when the hámlet is still,

we have two light and one strong syllable repeated four times over. Such repetition is the essence of versification. The same kind of rhythm is one of the main elements of

music, with this difference only, that in music the forcible syllables are made so for the purposes of rhythm by their length only or principally; for example, if either of the above lines were imitated by a melody in the most simple and obvious manner, each strong syllable would occupy exactly twice as much time as two of the weaker ones. Something very analogous to such rhythm may be traced in other parts of poetry and art, which we need not here dwell upon. But in reference to our present subject, we may remark that by the introduction of such rhythm, the flow of time, which appears otherwise so perfectly simple and homogeneous, admits of an infinite number of varied yet regular modes of progress. All the kinds of versification which occur in all languages, and the still more varied forms of recurrence of notes of different lengths, which are heard in all the varied strains of melodies, are only examples of such modifications, or configurations as we may call them, of time. They involve relations of various portions of time, as figures involve relations of various portions of space. But

yet the analogy between rhythm and figure is by no means very close; for in rhythm we have relations of quantity alone in the parts of time, whereas in figure we have 137 relations not only of quantity, but of a kind altogether different,—namely, of position. On the other hand, a *repetition* of similar elements, which does not necessarily occur in figures, is quite essential in order to impress upon us that measured progress of time of which we here speak. And thus the ideas of time and space have each its peculiar and exclusive relations; position and figure belonging only to space, while repetition and rhythm are appropriate to time.

7. One of the simplest forms of recurrence is *alternation*, as when we have alternate strong and slight syllables. For instance,—

Awáke, aríse, or bé for éver fáll'n.

Or without any subordination, as when we reckon numbers, and call them in succession, *odd*, *even*, *odd*, *even*.

8. But the simplest of all forms of recurrence is that which has no variety;—in which a series of units, each considered as exactly similar to the rest, succeed each other; as *one*, *one*, *one*, and so on. In this case, however, we are led to consider each unit with reference to all that have preceded; and thus the series *one*, *one*, *one*, and so forth, becomes *one*, *two*, *three*, *four*, *five*, and so on; a series with which all are familiar, and which may be continued without limit.

We thus collect from that repetition of which time admits, the conception of *Number*.

9. The relations of position and figure are the subject of the science of geometry; and are, as we have already said, traced into a very remarkable and extensive body of truths, which rests for its foundations on axioms involved in the Idea of Space. There is, in like manner, a science of great complexity and extent, which has its foundation in the Idea of Time. But this

science, as it is usually pursued, applies only to the conception of Number, which is, as we have said, the simplest result of repetition. This science is *Theoretical Arithmetic*, or the speculative doctrine of the properties and relations of numbers; and we must say a few words concerning the principles which it is requisite to assume as the basis of this science.

CHAPTER IX.

Of the Axioms which relate to Number.

1. THE foundations of our speculative knowledge of the relations and properties of Number, as well as of Space, are contained in the mode in which we represent to ourselves the magnitudes which are the subjects of our reasonings. To express these foundations in axioms in the case of number, is a matter requiring some consideration, for the same reason as in the case of geometry; that is, because these axioms are principles which we assume as true, without being aware that we have made any assumption; and we cannot, without careful scrutiny, determine when we have stated, in the form of axioms, all that is necessary for the formation of the

science, and no more than is necessary. We will, however, attempt to detect the principles which really must form the basis of theoretical arithmetic.

2. Why is it that three and two are equal to four and one? Because if we look at five things of any kind, we see that it is so. The five are four and one; they are also three and two. The truth of our assertion is involved in our being able to conceive the number five at all. We perceive this truth by *intuition*, for we cannot see, or imagine we see, five things, without perceiving also that the assertion above stated is true.

But how do we state in words this fundamental principle of the doctrine of numbers? Let us consider a very simple case. If we wish to show that seven and two are equal to four and five, we say that seven are four and three, *therefore* seven and two are four and three and two; and because three and two are 139 five, this is four and five. Mathematical reasoners justify the first inference (marked by the conjunctive word

therefore), by saying that “When equals are added to equals the wholes are equal,” and that thus, since seven is equal to three and four, if we add two to both, seven and two are equal to four and three and two.

3. Such *axioms* as this, that when equals are added to equals the wholes are equal, are, in fact, expressions of the general condition of intuition, by which a whole is contemplated as made up of parts, and as identical with the aggregate of the parts. And a yet more general form in which we might more adequately express this condition of intuition would be this; that ‘Two magnitudes are equal when they can be divided into parts which are equal, each to each.’ Thus in the above example, seven and two are equal to four and five, because each of the two sums can be divided into the parts, four, three, and two.

4. In all these cases, a person who had never seen such axioms enunciated in a verbal form would employ the same reasoning as a practised mathematician, in

order to satisfy himself that the proposition was true. The steps of the reasoning, being seen to be true by intuition, would carry an entire conviction, whether or not the argument were made verbally complete. Hence the axioms may appear superfluous, and on this account such axioms have often been spoken contemptuously of, as empty and barren assertions. In fact, however, although they cannot supply the deficiency of the clear intuition of number and space in the reasoner himself, and although when he possesses such a faculty, he will reason rightly if he have never heard of such axioms, they still have their place properly at the beginning of our treatises on the science of quantity; since they express, as simply as words can express, those conditions of the intuition of magnitudes on which all reasoning concerning quantity must be based; and are necessary when we want, not only to see the truth of the elementary reasonings on these subjects, but to put such reasonings in a formal and logical shape. 140

5. We have considered the above-mentioned axioms as the basis of all arithmetical operations of the nature of *addition*. But it is easily seen that the same principle may be carried into other cases; as for instance, *multiplication*, which is merely a repeated addition, and admits of the same kind of evidence. Thus five times three are equal to three times five; why is this? If we arrange fifteen things in five rows of three, it is seen by looking, or by imaginary looking, which is *intuition*, that they may also be taken as three rows of five. And thus the principle that those wholes are equal which can be resolved into the same partial magnitudes, is immediately applicable in this as in the other case.

6. We may proceed to higher numbers, and may find ourselves obliged to use artificial nomenclature and notation in order to represent and reckon them; but the reasoning in these cases also is still the same. And the usual artifice by which our reasoning in such instances is assisted is, that the number which is the root of our

scale of notation (which is *ten* in our usual system), is alternately separated into parts and treated as a single thing. Thus 47 and 35 are 82; for 47 is four tens and seven; 35 is three tens and five; whence 47 and 35 are seven tens and twelve; that is, 7 tens, 1 ten, and 2; which is 8 tens and 2, or 82. The like reasoning is applicable in other cases. And since the most remote and complex properties of numbers are obtained by a prolongation of a course of reasoning exactly similar to that by which we thus establish the most elementary propositions, we have, in the principles just noticed, the foundation of the whole of Theoretical Arithmetic.

CHAPTER X.

Of the Perception of Time and Number.

1. OUR perception of the passage of time involves a series of acts of memory. This is easily seen and assented to, when large intervals of time and a complex train of occurrences are concerned. But since memory is requisite in order to apprehend time in such cases, we cannot doubt that the same faculty must be concerned in the shortest and simplest cases of succession; for it will hardly be maintained that the process by which we contemplate the progress of time is different, when small, and when large intervals are concerned. If memory be absolutely requisite to connect two events which begin and end a day, and to perceive a tract of time between them, it must be equally indispensable to connect the

beginning and end of a minute, or a second; though in this case the effort may be smaller, and consequently more easily overlooked. In common cases, we are unconscious of the act of thought by which we recollect the preceding instant, though we perceive the effort when we recollect some distant event. And this is analogous to what happens in other instances. Thus, we walk without being conscious of the volitions by which we move our muscles; but, in order to leap, a distinct and manifest exertion of the same muscles is necessary. Yet no one will doubt that we walk as well as leap by an act of the will exerted through the muscles; and in like manner, our consciousness of small as well as large intervals of time involves something of the nature of an act of memory.

2. But this constant and almost imperceptible kind of memory, by which we connect the beginning and 142 end of each instant as it passes, may very fitly be distinguished in common cases from manifest acts of recollection, although it

may be difficult or impossible to separate the two operations in general. This perpetual and latent kind of memory may be termed a *sense of successiveness*; and must be considered as an internal sense by which we perceive ourselves existing in time, much in the same way as by our external and muscular sense we perceive ourselves existing in space. And both our internal thoughts and feelings, and the events which take place around us, are apprehended as objects of this internal sense, and thus as taking place in time.

3. In the same manner in which our interpretation of the notices of the muscular sense implies the power of moving our limbs, and of touching at will this object or that; our apprehension of the relations of time, by means of the internal sense of successiveness, implies a power of recalling what has past, and of retaining what is passing. We are able to seize the occurrences which have just taken place, and to hold them fast in our minds so as mentally to measure their distance in time

from occurrences now present. And thus, this sense of successiveness, like the muscular sense with which we have compared it, implies activity of the mind itself, and is not a sense passively receiving impressions.

4. The conception of *Number* appears to require the exercise of the same sense of succession. At first sight, indeed, we seem to apprehend Number without any act of memory, or any reference to time: for example, we look at a horse, and see that his legs are four; and this we seem to do at once, without reckoning them. But it is not difficult to see that this seeming instantaneousness of the perception of small numbers is an illusion. This resembles the many other cases in which we perform short and easy acts so rapidly and familiarly that we are unconscious of them; as in the acts of seeing, and of articulating our words. And this is the more manifest, since we begin our acquaintance with number by counting even the 143 smallest numbers. Children and very rude savages must use an

effort to reckon even their five fingers, and find a difficulty in going further. And persons have been known who were able by habit, or by a peculiar natural aptitude, to count by dozens as rapidly as common persons can by units. We may conclude, therefore, that when we appear to catch a small number by a single glance of the eye, we do in fact count the units of it in a regular, though very brief succession. To count requires an act of memory. Of this we are sensible when we count very slowly, as when we reckon the strokes of a church-clock; for in such a case we may forget in the intervals of the strokes, and *miscount*. Now it will not be doubted that the nature of the process in counting is the same whether we count fast or slow. There is no definite speed of reckoning at which the faculties which it requires are changed; and therefore memory, which is requisite in some cases, must be so in all[13].

[13] I have considered Number as involving the exercise of the sense of succession, because I cannot draw any line between those cases of large numbers, in which, the process of counting being performed,

> there is a manifest apprehension of succession; and those cases of small numbers, in which we seem to see the number at one glance. But if any one holds Number to be apprehended by a direct act of intuition, as Space and Time are, this view will not disturb the other doctrines delivered in the text.

The act of counting, (*one*, *two*, *three*, and so on,) is the foundation of all our knowledge of number. The intuition of the relations of number involves this act of counting; for, as we have just seen, the conception of number cannot be obtained in any other way. And thus the whole of theoretical arithmetic depends upon an act of the mind, and upon the conditions which the exercise of that act implies. These have been already explained in the last chapter.

5. But if the apprehension of number be accompanied by an act of the mind, the apprehension of *rhythm* is so still more clearly. All the forms of versification and the *measures* of melodies are the creations of man, who thus realizes in words and sounds the 144 forms of recurrence which rise within his own mind. When we hear in a

quiet scene any rapidly-repeated sound, as those made by the hammer of the smith or the saw of the carpenter, every one knows how insensibly we throw these noises into a rhythmical form in our own apprehension. We do this even without any suggestion from the sounds themselves. For instance, if the beats of a clock or watch be ever so exactly alike, we still reckon them alternately tick-*tack*, tick-*tack*. That this is the case, may be proved by taking a watch or clock of such a construction that the returning swing of the pendulum is silent, and in which therefore all the beats are rigorously alike: we shall find ourselves still reckoning its sounds as tick-*tack*. In this instance it is manifest that the rhythm is entirely of our own making. In melodies, also, and in verses in which the rhythm is complex, obscure and difficult, we perceive something is required on our part; for we are often incapable of contributing our share, and thus lose the sense of the measure altogether. And when we consider such cases, and attend to what passes within us when we catch the measure, even of the

simplest and best-known air, we shall no longer doubt that an act of our own thoughts is requisite in such cases, as well as impressions on the sense. And thus the conception of this peculiar modification of time, which we have called *rhythm*, like all the other views which we have taken of the subject, shows that we must, in order to form such conceptions, supply a certain idea by our own thoughts, as well as merely receive by senses, whether external or internal, the impressions of appearances and collections of appearances.

NOTE TO CHAPTER X.

I HAVE in the last ten chapters described Space, Time, and Number by various expressions, all intended to point out their office as exemplifying the Ideal Element of human knowledge. I have called them *Fundamental Ideas*; *Forms of Perception*; *Forms of Intuition*; and perhaps other names. I might add yet other phrases. I might say that the properties of Space, Time, and Number are *Laws of the Mind's Activity* in apprehending what is. For the mind cannot apprehend any thing or event except

conformably to the properties of space, time, and number. It is not only that it *does* not, but it *can* not: and this impossibility shows that the law is a law of the mind, and not of objects extraneous to the mind.

It is usual for some of those who reject the doctrines here presented to say that the axioms of geometry, and of other sciences, are obtained by Induction from facts constantly presented by experience. But I do not see how Induction can prove that a proposition *must* be true. The only intelligible usage of the word *Induction* appears to me to be, that in which it is applied to a proposition which, being separable from the facts in our apprehension, and being compared with them, is seen to agree with them. But in the cases now spoken of, the proposition is not separable from the facts. We cannot infer by induction that two straight lines cannot inclose a space, because we cannot contemplate special cases of two lines inclosing a space, in which it remains to be determined whether or not the proposition, that both are straight, is true.

I do not deny that the activity of the mind by which it perceives objects and events as related according to the laws of space, time, and number, is awakened and developed by being constantly exercised; and that we cannot imagine a stage of human existence in which the powers have not been awakened and 146 developed by such exercise. In this way, experience and observation are necessary conditions and prerequisites of our apprehension of

geometrical (and other) axioms. We cannot see the truth of these axioms without some experience, because we cannot see any thing, or be human beings, without some experience. This might be expressed by saying that such truths are acquired necessarily *in the course of* all experience; but I think it is very undesirable to apply, to such a case, the word *Induction*, of which it is so important to us to keep the scientific meaning free from confusion. Induction cannot give demonstrative proofs, as I have already stated in Book 1. C. i. sect. 3, and therefore cannot be the ground of necessary truths.

Another expression which may be used to describe the Fundamental Ideas here spoken of is suggested by the language of a very profound and acute Review of the former edition. The Reviewer holds that we pass from special experiences to universal truths in virtue of 'the inductive propensity—the irresistible impulse of the mind to generalize *ad infinitum*.' I have already given reasons why I cannot adopt the former expression; but I do not see why space, time, number, cause, and the rest, may not be termed *different forms* of the *impulse of the mind to generalize*. But if we put together all the Fundamental Ideas as results of the Generalizing Impulse, we must still separate them as different modes of action of that Impulse, showing themselves in various characteristic ways in the axioms and modes of reasoning which belong to different sciences. The Generalizing Impulse in one case proceeds according

to the Idea of Space; in another, according to the Idea of Mechanical Cause; and so in other subjects.

CHAPTER XI.

Of Mathematical Reasoning.

1. *Discursive Reasoning.*—We have thus seen that our notions of space, time, and their modifications, necessarily involve a certain activity of the mind; and that the conditions of this activity form the foundations of those sciences which have the relations of space, time, and number, for their object. Upon the fundamental principles thus established, the various sciences which are included in the term *Pure Mathematics*, (Geometry, Algebra, Trigonometry, Conic Sections, and the rest of the Higher Geometry, the Differential Calculus, and the like,) are built up by a series of reasonings. These reasonings are subject to the rules of Logic, as we have already remarked; nor is it necessary here to

dwell long on the nature and rules of such processes. But we may here notice that such processes are termed *discursive*, in opposition to the operations by which we acquire our fundamental principles, which are, as we have seen, *intuitive*. This opposition was formerly very familiar to our writers; as Milton,—

> . . . Thus the soul reason receives,
>
> Discursive or intuitive.—*Paradise Lost*, v. 438.

For in such reasonings we obtain our conclusions, not by looking at our conceptions steadily in one view, which is *intuition*, but by passing from one view to another, like those who run from place to place (*discursus*). Thus a straight line may be at the same time a side of a triangle and a radius of a circle: and in the first proposition of Euclid a line is considered, first in one of these relations, and then in the other, and thus the sides of a certain triangle are proved to be equal. And by this 'discourse of reason,' as by our older 148 writers it was termed, we set forth from

those axioms which we perceive by intuition, travel securely over a vast and varied region, and become possessed of a copious store of mathematical truths.

2. *Technical Terms of Reasoning.*—The reasoning of mathematics, thus proceeding from a few simple principles to many truths, is conducted according to the rules of Logic. If it be necessary, mathematical proofs may be reduced to logical forms, and expressed in Syllogisms, consisting of major, minor, and conclusion. But in most cases the syllogism is of that kind which is called by logical writers an *Enthymeme*; a word which implies something existing in the thoughts only, and which designates a syllogism in which one of the premises is understood, and not expressed. Thus we say in a mathematical proof, 'because the point C is the center of the circle AB, AC is equal to BC;' not stating the *major*,—that all lines drawn from the center of a circle to the circumference are equal; or introducing it only by a transient reference to the definition of a circle. But the enthymeme is

so constantly used in all habitual forms of reasoning, that it does not occur to us as being anything peculiar in mathematical works.

The propositions which are proved to be generally true are termed *Theorems*: but when anything is required to be done, as to draw a line or a circle under given conditions, this proposition is a *Problem*. A theorem requires demonstration; a problem, solution. And for both purposes the mathematician usually makes a *Construction*. He directs us to draw certain lines, circles, or other curves, on which is to be founded his demonstration that his theorem is true, or that his problem is solved. Sometimes, too, he establishes some *Lemma*, or preparatory proposition, before he proceeds to his main task; and often he deduces from his demonstration some conclusion in addition to that which was the professed object of his proposition; and this is termed a *Corollary*.

These technical terms are noted here, not

as being very important, but in order that they may not sound 149 strange and unintelligible if we should have occasion to use some of them. There is, however, one technical distinction more peculiar, and more important.

3. *Geometrical Analysis and Synthesis.*—In geometrical reasoning such as we have described, we introduce at every step some new consideration; and it is by combining all these considerations, that we arrive at the conclusion, that is, the demonstration of the proposition. Each step tends to the final result, by exhibiting some part of the figure under a new relation. To what we have already proved, is added something more; and hence this process is called *Synthesis*, or *putting together*. The proof flows on, receiving at every turn new contributions from different quarters; like a river fed and augmented by many tributary streams. And each of these tributaries flows from some definition or axiom as its fountain, or is itself formed by the union of smaller rivulets which have sources of this kind. In

descending along its course, the synthetical proof gathers all these accessions into one common trunk, the proposition finally proved.

But we may proceed in a different manner. We may begin from the formed river, and ascend to its sources. We may take the proposition of which we require a proof, and may examine what the supposition of its truth implies. If this be true, then something else may be seen to be true; and from this, something else, and so on. We may often, in this way, discover of what simpler propositions our theorem or solution is compounded, and may resolve these in succession, till we come to some proposition which is obvious. This is geometrical *Analysis*. Having succeeded in this analytical process, we may invert it; and may descend again from the simple and known propositions, to the proof of a theorem, or the solution of a problem, which was our starting-place.

This process resembles, as we have said,

tracing a river to its sources. As we ascend the stream, we perpetually meet with bifurcations; and some sagacity is needed to enable us to see which, in each case, is the main stream: but if we proceed in our research, we 150 exhaust the unexplored valleys, and finally obtain a clear knowledge of the place whence the waters flow. *Analytical* is sometimes confounded with *symbolical* reasoning, on which subject we shall make a remark in the next chapter. The object of that chapter is to notice certain other fundamental principles and ideas, not included in those hitherto spoken of, which we find thrown in our way as we proceed in our mathematical speculations. It would detain us too long, and involve us in subtle and technical disquisitions, to examine fully the grounds of these principles; but the Mathematics hold so important a place in relation to the inductive sciences, that I shall briefly notice the leading ideas which the ulterior progress of the subject involves.

CHAPTER XII.

Of the Foundations of the Higher Mathematics.

1. *The Idea of a Limit.*—The general truths concerning relations of space which depend upon the axioms and definitions contained in Euclid's *Elements*, and which involve only properties of straight lines and circles, are termed Elementary Geometry: all beyond this belongs to the Higher Geometry. To this latter province appertain, for example, all propositions respecting the lengths of any portions of curve lines; for these cannot be obtained by means of the principles of the Elements alone. Here then we must ask to what other principles the geometer has recourse, and from what source these are drawn. Is there any origin of geometrical truth which we have not yet explored?

The *Idea of a Limit* supplies a new mode of establishing mathematical truths. Thus with regard to the length of any portion of a curve, a problem which we have just mentioned; a curve is not made up of straight lines, and therefore we cannot by means of any of the doctrines of elementary geometry measure the length of any curve. But we may make up a figure nearly resembling any curve by putting together many short straight lines, just as a polygonal building of very many sides may nearly resemble a circular room. And in order to approach nearer and nearer to the curve, we may make the sides more and more small, more and more numerous. We may then possibly find some mode of measurement, some relation of these small lines to other lines, which is not disturbed by the multiplication of the sides, however far it be carried. And thus, we may do what is equivalent to measuring the curve itself; for by multiplying the 152 sides we may approach more and more closely to the curve till no appreciable difference remains. The curve line is the *Limit* of the polygon;

and in this process we proceed on the *Axiom*, that 'What is true up to the Limit is true at the Limit.'

This mode of conceiving mathematical magnitudes is of wide extent and use; for every curve may be considered as the limit of some polygon; every varied magnitude, as the limit of some aggregate of simpler forms; and thus the relations of the elementary figures enable us to advance to the properties of the most complex cases.

A Limit is a peculiar and fundamental conception, the use of which in proving the propositions of the Higher Geometry cannot be superseded by any combination of other hypotheses and definitions[14]. The axiom just noticed, that what is true up to the limit is true at the limit, is involved in the very conception of a Limit: and this principle, with its consequences, leads to all the results which form the subject of the higher mathematics, whether proved by the consideration of evanescent triangles, by the processes of the Differential Calculus, or in

any other way.

> [14] This assertion cannot be fully proved and illustrated without a reference to mathematical reasonings which would not be generally intelligible. I have shown the truth of the assertion in my *Thoughts on the Study of Mathematics*, annexed to the *Principles of English University Education*. The proof is of this kind:—The ultimate equality of an arc of a curve and the corresponding periphery of a polygon, when the sides of the polygon are indefinitely increased in number, is *evident*. But this truth cannot be proved from any other axiom. For if we take the supposed axiom, that a curve is always less than the including broken line, this is not true, except with a condition; and in tracing the import of this condition, we find its necessity becomes evident only when we introduce a reference to a Limit. And the same is the case if we attempt to supersede the notion of a Limit in proving any other simple and evident proposition in which that notion is involved. Therefore these evident truths are *self*-evident, *in virtue of the Idea of a Limit*.

The ancients did not expressly introduce this conception of a Limit into their mathematical reasonings; although in the application of what is termed the 153 *Method of Exhaustions*, (in which they show

how to *exhaust* the *difference* between a polygon and a curve, or the like,) they were in fact proceeding upon an obscure apprehension of principles equivalent to those of the Method of Limits. Yet the necessary fundamental principle not having, in their time, been clearly developed, their reasonings were both needlessly intricate and imperfectly satisfactory. Moreover they were led to put in the place of axioms, assumptions which were by no means self-evident; as when Archimedes assumed, for the basis of his measure of the circumference of the circle, the proposition that a circular arc is necessarily less than two lines which inclose it, joining its extremities. The reasonings of the older mathematicians, which professed to proceed upon such assumptions, led to true results in reality, only because they were guided by a latent reference to the limiting case of such assumptions. And this latent employment of the conception of a Limit, reappeared in various forms during the early period of modern mathematics; as for example, in the *Method of Indivisibles* of Cavalleri, and the

Characteristic Triangle of Barrow; till at last, Newton distinctly referred such reasonings to the conception of a Limit, and established the fundamental principles and processes which that conception introduces, with a distinctness and exactness which required little improvement to make it as unimpeachable as the demonstrations of geometry. And when such processes as Newton thus deduced from the conception of a Limit, are represented by means of general algebraical symbols instead of geometrical diagrams, we have then before us the *Method of Fluxions*, or the *Differential Calculus*; a mode of treating mathematical problems justly considered as the principal weapon by which the splendid triumphs of modern mathematics have been achieved.

2. *The Use of General Symbols.*—The employment of algebraical symbols, of which we have just spoken, has been another of the main instruments to which the successes of modern mathematics are owing. And here again the processes by which we obtain our 154 results depend for

their evidence upon a fundamental conception,—the conception of *arbitrary symbols* as the *Signs* of quantity and its relations; and upon a corresponding axiom, that 'The interpretation of such symbols must be perfectly general.' In this case, as in the last, it was only by degrees that mathematicians were led to a just apprehension of the grounds of their reasoning. For symbols were at first used only to represent numbers considered with regard to their numerical properties; and thus the science of Algebra was formed. But it was found, even in cases belonging to common algebra, that the symbols often admitted of an interpretation which went beyond the limits of the problem, and which yet was not unmeaning, since it pointed out a question closely analogous to the question proposed. This was the case, for example, when the answer was a *negative quantity*; for when Descartes had introduced the mode of representing curves by means of algebraical relations among the symbols of the *co-ordinates*, or distances of each of their points from fixed lines, it was found that negative

quantities must be dealt with as not less truly significant than positive ones. And as the researches of mathematicians proceeded, other cases also were found, in which the symbols, although destitute of meaning according to the original conventions of their institution, still pointed out truths which could be verified in other ways; as in the cases in which what are called *impossible quantities* occur. Such processes may usually be confirmed upon other principles, and the truth in question may be established by means of a demonstration in which no such seeming fallacies defeat the reasoning. But it has also been shown in many such cases, that the process in which some of the steps appear to be without real meaning, does in fact involve a valid proof of the proposition. And what we have here to remark is, that this is not true accidentally or partially only, but that the results of systematic symbolical reasoning must *always* express general truths, by their nature; and do not, for their justification, require each of the steps of the process to represent 155 some definite

operation upon quantity. *The absolute universality of the interpretation of symbols* is the fundamental principle of their use. This has been shown very ably by Dr. Peacock in his *Algebra*. He has there illustrated, in a variety of ways, this principle: that 'If general symbols express an identity when they are supposed to be of any special nature, they must also express an identity when they are general in their nature.' And thus, this universality of symbols is a principle in addition to those we have already noticed; and is a principle of the greatest importance in the formation of mathematical science, according to the wide generality which such science has in modern times assumed.

3. *Connexion of Symbols and Analysis.*— Since in our symbolical reasoning our symbols thus reason for us, we do not necessarily here, as in geometrical reasoning, go on adding carefully one known truth to another, till we reach the desired result. On the contrary, if we have a theorem to prove or a problem to solve

which can be brought under the domain of our symbols, we may at once state the given but unproved truth, or the given combination of unknown quantities, in its symbolical form. After this first process, we may then proceed to trace, by means of our symbols, what other truth is involved in the one just stated, or what the unknown symbols must signify; resolving step by step the symbolical assertion with which we began, into others more fitted for our purpose. The former process is a kind of *synthesis*, the latter is termed *analysis*. And although symbolical reasoning does not necessarily imply such analysis; yet the connexion is so familiar, that the term *analysis* is frequently used to designate symbolical reasoning.

CHAPTER XIII.

The Doctrine of Motion.

1. *Pure Mechanism.*—The doctrine of Motion, of which we have here to speak, is that in which motion is considered quite independently of its cause, force; for all consideration of force belongs to a class of ideas entirely different from those with which we are here concerned. In this view it may be termed the *pure* doctrine of motion, since it has to do solely with space and time, which are the subjects of pure mathematics. (See c. i. of this book.) Although the doctrine of motion in connexion with force, which is the subject of mechanics, is by far the most important form in which the consideration of motion enters into the formation of our sciences, the Pure Doctrine of Motion, which treats of

space, time, and velocity, might be followed out so as to give rise to a very considerable and curious body of science. Such a science is the science of Mechanism, independent of force, and considered as the solution of a problem which may be thus enunciated: ‘To communicate any given motion from a first mover to a given body.’ The science which should have for its object to solve all the various cases into which this problem would ramify, might be termed *Pure Mechanism*, in contradistinction to *Mechanics Proper*, or *Machinery*, in which Force is taken into consideration. The greater part of the machines which have been constructed for use in manufactures have been practical solutions of some of the cases of this problem. We have also important contributions to such a science in the works of Mathematicians; for example, the various investigations and demonstrations which have been published respecting the form of the Teeth 157 of Wheels, and Mr. Babbage’s memoir[15] on the Language of Machinery. There are also several works which contain collections of the mechanical contrivances

which have been invented for the purpose of transmitting and modifying motion, and these works may be considered as treatises on the science of Pure Mechanism. But this science has not yet been reduced to the systematic simplicity which is desirable, nor indeed generally recognized as a separate science. It has been confounded, under the common name of *Mechanics*, with the other science, Mechanics Proper, or Machinery, which considers the effect of *force* transmitted by Mechanism from one part of a material combination to another. For example, the *Mechanical Powers*, as they are usually termed, (the Lever, the Wheel and Axle, the Inclined Plane, the Wedge, and the Screw,) have almost always been treated with reference to the relation between the *Power* and the *Weight*, and not primarily as a mode of changing the velocity and kind of the motion. The science of pure motion has not generally been separated from the science of motion viewed with reference to its causes.

[15] *On a Method of expressing by Signs the action of Machinery. Phil. Trans.* 1826, p.

250.

Recently, indeed, the necessity of such a separation has been seen by those who have taken a philosophical view of science. Thus this necessity has been urged by M. Ampère, in his *Essai sur la Philosophie des Sciences* (1834): 'Long,' he says, (p. 50,) 'before I employed myself upon the present work, I had remarked that it is usual to omit, in the beginning of all books treating of sciences which regard motion and force, certain considerations which, duly developed, must constitute a special science: of which science certain parts have been treated of, either in memoirs or in special works; such, for example, as that of Carnot upon Motion considered Geometrically, and the essay of Lanz and Betancourt upon the Composition of Machines.' He then proceeds to describe this science nearly as we have 158 done, and proposes to term it *Kinematics* (*Cinématique*), from κίνημα, motion.

2. *Formal Astronomy.*—I shall not attempt here further to develop the form which such a science must assume. But I may notice one

very large province which belongs to it. When men had ascertained the apparent motions of the sun, moon, and stars, to a moderate degree of regularity and accuracy, they tried to conceive in their minds some mechanism by which these motions might be produced; and thus they in fact proposed to themselves a very extensive problem in *Kinematics*. This, indeed, was the view originally entertained of the nature of the science of astronomy. Thus Plato in the seventh Book of his *Republic*[16], speaks of astronomy as the doctrine of the motion of solids, meaning thereby, spheres. And the same was a proper description of the science till the time of Kepler, and even later: for Kepler endeavoured in vain to conjoin with the knowledge of the motions of the heavenly bodies, those true mechanical conceptions which converted formal into physical astronomy[17].

[16] P. 528.

[17] *Hist. Induc. Sc.* ii. 130.

The astronomy of the ancients admitted

none but uniform circular motions, and could therefore be completely cultivated by the aid of their elementary geometry. But the pure science of motion might be extended to all motions, however varied as to the speed or the path of the moving body. In this form it must depend upon the doctrine of limits; and the fundamental principle of its reasonings would be this: That velocity is measured by the Limit of the *space* described, considered with reference to the *time* in which it is described. I shall not further pursue this subject; and in order to complete what I have to say respecting the Pure Sciences, I have only a few words to add respecting their bearing on Inductive Science in general.

CHAPTER XIV.

Of the Application of Mathematics to the Inductive Sciences.

1. ALL objects in the world which can be made the subjects of our contemplation are subordinate to the conditions of Space, Time, and Number; and on this account, the doctrines of pure mathematics have most numerous and extensive applications in every department of our investigations of nature. And there is a peculiarity in these Ideas, which has caused the mathematical sciences to be, in all cases, the first successful efforts of the awakening speculative powers of nations at the commencement of their intellectual progress. Conceptions derived from these Ideas are, from the very first, perfectly precise and clear, so as to be fit elements of

scientific truths. This is not the case with the other conceptions which form the subjects of scientific inquiries. The conception of *statical force,* for instance, was never presented in a distinct form till the works of Archimedes appeared: the conception of *accelerating force* was confused, in the mind of Kepler and his contemporaries, and only became clear enough for purposes of sound scientific reasoning in the succeeding century: the just conception of chemical *composition* of elements gradually, in modern times, emerged from the erroneous and vague notions of the ancients. If we take works published on such subjects before the epoch when the foundations of the true science were laid, we find the knowledge not only small, but worthless. The writers did not see any evidence in what we now consider as the axioms of the science; nor any inconsistency where we now see self-contradiction. But this was never the case with speculations concerning 160 space and number. From their first rise, these were true as far as they went. The Geometry and

Arithmetic of the Greeks and Indians, even in their first and most scanty form, contained none but true propositions. Men's intuitions upon these subjects never allowed them to slide into error and confusion; and the truths to which they were led by the first efforts of their faculties, so employed, form part of the present stock of our mathematical knowledge.

2. But we are here not so much concerned with mathematics in their pure form, as with their application to the phenomena and laws of nature. And here also the very earliest history of civilization presents to us some of the most remarkable examples of man's success in his attempts to attain to science. Space and time, position and motion, govern all visible objects; but by far the most conspicuous examples of the relations which arise out of such elements, are displayed by the ever-moving luminaries of the sky, which measure days, and months, and years, by their motions, and man's place on the earth by their position. Hence the sciences of space and number

were from the first cultivated with peculiar reference to Astronomy. I have elsewhere[18] quoted Plato's remark,—that it is absurd to call the science of the relations of space *geometry*, the measure of the earth, since its most important office is to be found in its application to the heavens. And on other occasions also it appears how strongly he, who may be considered as the representative of the scientific and speculative tendencies of his time and country, had been impressed with the conviction, that the formation of a science of the celestial motions must depend entirely upon the progress of mathematics. In the Epilogue to the Dialogue on the Laws[19], he declares mathematical knowledge to be the first and main requisite for the astronomer, and describes the portions of it which he holds necessary for astronomical speculators to cultivate. These seem to be, Plane Geometry, Theoretical Arithmetic, the Application of Arithmetic 161 to planes and to solids, and finally the doctrine of Harmonics. Indeed the bias of Plato appears to be rather to consider

mathematics as the essence of the science of astronomy, than as its instrument; and he seems disposed, in this as in other things, to disparage observation, and to aspire after a science founded upon demonstration alone. 'An astronomer,' he says in the same place, 'must not be like Hesiod and persons of that kind, whose astronomy consists in noting the settings and risings of the stars; but he must be one who understands the revolutions of the celestial spheres, each performing its proper cycle.'

[18] *Hist. Ind. Sc.* b. iii. c. ii.

[19] *Epinomis*, p. 990.

A large portion of the mathematics of the Greeks, so long as their scientific activity continued, was directed towards Astronomy. Besides many curious propositions of plane and solid Geometry, to which their astronomers were led, their Arithmetic, though very inconvenient in its fundamental assumptions (as being sexagesimal not decimal), was cultivated to a great extent; and the science of Trigonometry, in which

problems concerning the relations of space were resolved by means of tables of numerical results previously obtained, was created. Menelaus of Alexandria wrote six Books on Chords, probably containing methods of calculating Tables of these quantities; such Tables were familiarly used by the later Greek astronomers. The same author also wrote three Books on Spherical Trigonometry, which are still extant.

3. The Greeks, however, in the first vigour of their pursuit of mathematical truth, at the time of Plato and soon after, had by no means confined themselves to those propositions which had a visible bearing on the phenomena of nature; but had followed out many beautiful trains of research, concerning various kinds of figures, for the sake of their beauty alone; as for instance in their doctrine of Conic Sections, of which curves they had discovered all the principal properties. But it is curious to remark, that these investigations, thus pursued at first as mere matters of curiosity and intellectual gratification, were destined, two thousand

years later, to play a very important part in 162 establishing that system of the celestial motions which succeeded the Platonic scheme of cycles and epicycles. If the properties of the conic sections had not been demonstrated by the Greeks, and thus rendered familiar to the mathematicians of succeeding ages, Kepler would probably not have been able to discover those laws respecting the orbits and motions of the planets which were the occasion of the greatest revolution that ever happened in the history of science.

4. The Arabians, who, as I have elsewhere said, added little of their own to the stores of science which they received from the Greeks, did however make some very important contributions in those portions of pure mathematics which are subservient to astronomy. Their adoption of the Indian mode of computation by means of the Ten Digits, 1, 2, 3, 4, 5, 6, 7, 8, 9, 0, and by the method of Local Values, instead of the cumbrous sexagesimal arithmetic of the Greeks, was an improvement by which the

convenience and facility of numerical calculations were immeasurably augmented. The Arabians also rendered several of the processes of trigonometry much more commodious, by using the Sine of an arc instead of the Chord; an improvement which Albategnius appears to claim for himself[20]; and by employing also the Tangents of arcs, or, as they called them[21], *upright shadows*.

[20] Delambre, *Ast., M. A.*, p. 12.

[21] *Ibid.* p. 17.

5. The constant application of mathematical knowledge to the researches of Astronomy, and the mutual influence of each science on the progress of the other, has been still more conspicuous in modern times. Newton's Method of Prime and Ultimate Ratios, which we have already noticed as the first correct exposition of the doctrine of a Limit, is stated in a series of Lemmas, or preparatory theorems, prefixed to his *Treatise on the System of the World*. Both the properties of curve lines and the

doctrines concerning force and motion, which he had to establish, required that the common mathematical processes should be methodized and extended. If Newton had not been a most 163 expert and inventive mathematician, as well as a profound and philosophical thinker, he could never have made any one of those vast strides in discovery of which the rapid succession in his work strikes us with wonder[22]. And if we see that the great task begun by him, goes on more slowly in the hands of his immediate successors, and lingers a little before its full completion, we perceive that this arises, in a great measure, from the defect of the mathematical methods then used. Newton's synthetical modes of investigation, as we have elsewhere observed, were an instrument[23], powerful indeed in his mighty hand, but too ponderous for other persons to employ with effect. The countrymen of Newton clung to it the longest, out of veneration for their master; and English cultivators of physical astronomy were, on that very account, left behind the progress of mathematical science

in France and Germany, by a wide interval, which they have only recently recovered. On the Continent, the advantages offered by a familiar use of symbols, and by attention to their symmetry and other relations, were accepted without reserve. In this manner the Differential Calculus of Leibnitz, which was in its origin and signification identical with the Method of Fluxions of Newton, soon surpassed its rival in the extent and generality of its application to problems. This Calculus was applied to the science of mechanics, to which it, along with the symmetrical use of co-ordinates, gave a new form; for it was soon seen that the most difficult problems might in general be reduced to finding integrals, which is the reciprocal process of that by which differentials are found; so that all difficulties of physical astronomy were reduced to difficulties of symbolical calculation, these, indeed, being often sufficiently stubborn. Clairaut, Euler, and D'Alembert employed the increased resources of mathematical science upon the Theory of the Moon, and other questions relative to the system of the

world; and thus began to pursue such inquiries in the course in which mathematicians 164 are still labouring up to the present day. This course was not without its checks and perplexities. We have elsewhere quoted[24] Clairaut's expression when he had obtained the very complex differential equations which contain the solution of the problem of the moon's motion: 'Now integrate them who can!' But in no very long time they were integrated, at least approximately; and the methods of approximation have since then been improved; so that now, with a due expenditure of labour, they may be carried to any extent which is thought desirable. If the methods of astronomical observation should hereafter reach a higher degree of exactness than they now profess, so that irregularities in the motions of the sun, moon, and planets, shall be detected which at present escape us, the mathematical part of the theory of universal gravitation is in such a condition that it can soon be brought into comparison with the newly-observed facts. Indeed at present the mathematical

theory is in advance of such observations. It can venture to suggest what may afterwards be detected, as well as to explain what has already been observed. This has happened recently; for Professor Airy has calculated the law and amount of an inequality depending upon the mutual attraction of the Earth and Venus; of which inequality (so small is it,) it remains to be determined whether its effect can be traced in the series of astronomical observations.

[22] *Hist. Ind. Sc.* b. vii. c. ii.

[23] *Ibid.* p. 175.

[24] *Hist. Ind. Sc.* b. vi. c. vi. sect. 7.

6. As the influence of mathematics upon the progress of astronomy is thus seen in the cases in which theory and observation confirm each other, so this influence appears in another way, in the very few cases in which the facts have not been fully reduced to an agreement with theory. The most conspicuous case of this kind is the state of our knowledge of the Tides. This is a portion of astronomy: for the Newtonian

theory asserts these curious phenomena to be the result of the attraction of the sun and moon. Nor can there be any doubt that this is true, as a general statement; yet the subject is up to the present time a blot 165 on the perfection of the theory of universal gravitation; for we are very far from being able in this, as in the other parts of astronomy, to show that theory will exactly account for the time, and magnitude, and all other circumstances of the phenomenon at every place on the earth's surface. And what is the portion of our mathematics which is connected with this solitary signal defect in astronomy? It is the mathematics of the Motion of Fluids; a portion in which extremely little progress has been made, and in which all the more general problems of the subject have hitherto remained entirely insoluble. The attempts of the greatest mathematicians, Newton, Maclaurin, Bernoulli, Clairaut, Laplace, to master such questions, all involve some gratuitous assumption, which is introduced because the problem cannot otherwise be mathematically dealt with: these

assumptions confessedly render the result defective, and how defective, it is hard to say. And it was probably precisely the absence of a theory which could be reasonably expected to agree with the observations, which made Observations of this very curious phenomenon, the Tides, to be so much neglected as till very recently they were. Of late years such observations have been pursued, and their results have been resolved into empirical laws, so that the rules of the phenomena have been ascertained, although the dependence of these rules upon the lunar and solar forces has not been shown. Here then we have a portion of our knowledge relating to facts undoubtedly dependent upon universal gravitation, in which Observation has outstripped Theory in her progress, and is compelled to wait till her usual companion overtakes her. This is a position of which Mathematical Theory has usually been very impatient, and we may expect that she will be no less so in the present instance.

7. It would be easy to show from the

history of other sciences, for example, Mechanics and Optics, how essential the cultivation of pure mathematics has been to their progress. The parabola was already familiar among mathematicians when Galileo discovered that it was the theoretical path of a Projectile; and the 166 extension and generalization of the Laws of Motion could never have been effected, unless the Differential and Integral Calculus had been at hand, ready to trace the results of every hypothesis which could be made. D'Alembert's mode of expressing the Third Law of Motion in its most general form[25], if it did not prove the law, at least reduced the application of it to analytical processes which could be performed in most of those cases in which they were needed. In many instances the demands of mechanical science suggested the extension of the methods of pure analysis. The problem of Vibrating Strings gave rise to the Calculus of Partial Differences, which was still further stimulated by its application to the motions of fluids and other mechanical problems. And we have in the writings of Lagrange

and Laplace other instances equally remarkable of new analytical methods, to which mechanical problems, and especially cosmical problems, have given occasion.

[25] *Hist. Ind. Sc.* b. vi. c. vi. sect. 7.

8. The progress of Optics as a science has, in like manner, been throughout dependent upon the progress of pure mathematics. The first rise of Geometry was followed by some advances, slight ones no doubt, in the doctrine of Reflection and in Perspective. The law of Refraction was traced to its consequences by means of Trigonometry, which indeed was requisite to express the law in a simple form. The steps made in Optical science by Descartes, Newton, Euler, and Huyghens, required the geometrical skill which those philosophers possessed. And if Young and Fresnel had not been, each in his peculiar way, persons of eminent mathematical endowments, they would not have been able to bring the Theory of Undulations and Interferences into a condition in which it could be tested by experiments. We may see how unexpectedly

recondite parts of pure mathematics may bear upon physical science, by calling to mind a circumstance already noticed in the History of Science[26];—that Fresnel obtained one of the 167 most curious confirmations of the theory (the laws of Circular Polarization by reflection) through an interpretation of an algebraical expression, which, according to the original conventional meaning of the symbols, involved an impossible quantity. We have already remarked, that in virtue of the principle of the generality of symbolical language, such an interpretation may often point out some real and important analogy.

[26] *Hist. Ind. Sc.* b. ix. c. xiii. sect. 2.

9. From this rapid sketch it may be seen how important an office in promoting the progress of the physical sciences belongs to mathematics. Indeed in the progress of many sciences, every step has been so intimately connected with some advance in mathematics, that we can hardly be surprised if some persons have considered mathematical reasoning to be the most essential part of such sciences; and have

overlooked the other elements which enter into their formation. How erroneous this view is we shall best see by turning our attention to the other Ideas besides those of space, number, and motion, which enter into some of the most conspicuous and admired portions of what is termed exact science; and by showing that the clear and distinct development of such Ideas is quite as necessary to the progress of exact and real knowledge as an acquaintance with arithmetic and geometry.

BOOK III.

THE

PHILOSOPHY

OF THE

MECHANICAL SCIENCES.

> It is only because we subject trains of phenomena, that is, all change whatever, to the law of causality—to the relation of cause and effect—that experience or empirical knowledge becomes possible.
>
> KANT, *Kr. d. R. V.* 11 Th. 1 Abth. 11 Buch. 2 Haupt.

> Quicquid premit vel trahit alterum, tantundem ab eo premitur vel trahitur … Si corpus aliquod in corpus aliud impingens motum ejus vi suâ quomodocunque mutaverit,

idem quoque vicissim in motu proprio eandem mutationem in partem contrariam vi alterius (ob æqualitatem pressionis, mutuæ) subibit … Obtinet etiam hæc Lex in attractionibus.

NEWTON, *Princip.* ad init.

BOOK III.

THE PHILOSOPHY OF THE MECHANICAL SCIENCES.

CHAPTER I.

Of the Mechanical Sciences.

In the History of the Sciences, that class of which we here speak occupies a conspicuous and important place; coming into notice immediately after those parts of Astronomy which require for their cultivation merely the ideas of space, time, motion, and number. It appears from our History, that certain truths concerning the *equilibrium* of bodies were established by

Archimedes;—that, after a long interval of inactivity, his principles were extended and pursued further in modern times:—and that to these doctrines concerning equilibrium and the forces which produce it, (which constitute the science *Statics*,) were added many other doctrines concerning the *motions* of bodies, considered also as produced by forces, and thus the science of *Dynamics* was produced. The assemblage of these sciences composes the province of *Mechanics*. Moreover, philosophers have laboured to make out the laws of the equilibrium of *fluid* as well as solid bodies; and hence has arisen the science of *Hydrostatics*. And the doctrines of Mechanics have been found to have a most remarkable bearing upon the motions of the heavenly bodies; with reference to which, indeed, they were at first principally studied. The explanation of those cosmical facts by means of mechanical 172 principles and their consequences, forms the science of *Physical Astronomy*. These are the principal examples of mechanical science; although some other portions of Physics, as

Magnetism and Electrodynamics, introduce mechanical doctrines very largely into their speculations.

Now in all these sciences we have to consider *Forces*. In all mechanical reasonings forces enter, either as producing motion, or as prevented from doing so by other forces. Thus force, in its most general sense, is the *cause* of motion, or of tendency to motion; and in order to discover the principles on which the mechanical sciences truly rest, we must examine the nature and origin of our knowledge of Causes.

In these sciences, however, we have not to deal with Cause in its more general acceptation, in which it applies to all kinds of agency, material or immaterial;—to the influence of thought and will, as well as of bodily pressure and attractive force. Our business at present is only with such causes as immediately operate upon matter. We shall nevertheless, in the first place, consider the nature of Cause in its most general form; and afterwards narrow our

speculations so as to direct them specially to the mechanical sciences.

CHAPTER II.

Of the Idea of Cause.

1. We see in the world around us a constant succession of causes and effects connected with each other. The laws of this connexion we learn in a great measure from experience, by observation of the occurrences which present themselves to our notice, succeeding one another. But in doing this, and in attending to this succession of appearances, of which we are aware by means of our senses, we supply from our own minds the Idea of Cause. This Idea, as we have already shown with respect to other Ideas, is not derived from experience, but has its origin in the mind itself;—is introduced into our experience by the active, and not by the passive part of our nature.

By Cause we mean some quality, power, or efficacy, by which a state of things produces a succeeding state. Thus the motion of bodies from rest is produced by a cause which we call *Force*: and in the particular case in which bodies fall to the earth, this force is termed *Gravity*. In these cases, the Conceptions of Force and Gravity receive their meaning from the Idea of Cause which they involve: for Force is conceived as the Cause of Motion. That this Idea of Cause is not derived from experience, we prove (as in former cases) by this consideration: that we can make assertions, involving this idea, which are rigorously necessary and universal; whereas knowledge derived from experience can only be true as far as experience goes, and can never contain in itself any evidence whatever of its necessity. We assert that 'Every event must have a cause:' and this proposition we know to be true, not only probably, and generally, and as far as we can see: 174 but we cannot suppose it to be false in any single instance. We are as certain of it as of the truths of arithmetic or

geometry. We cannot doubt that it must apply to all events past and future, in every part of the universe, just as truly as to those occurrences which we have ourselves observed. *What* causes produce what effects; —what is the cause of any particular event; —what will be the effect of any peculiar process;—these are points on which experience may enlighten us. Observation and experience may be requisite, to enable us to judge respecting such matters. But that every event has *some* cause, Experience cannot prove any more than she can disprove. She can add nothing to the evidence of the truth, however often she may exemplify it. This doctrine, then, cannot have been acquired by her teaching; and the Idea of Cause, which the doctrine involves, and on which it depends, cannot have come into our minds from the region of observation.

2. That we do, in fact, apply the Idea of Cause in a more extensive manner than could be justified, if it were derived from experience only, is easily shown. For from

the principle that everything must have a cause, we not only reason concerning the succession of the events which occur in the progress of the world, and which form the course of experience; but we infer that the world itself must have a cause;—that the chain of events connected by common causation, must have a First Cause of a nature different from the events themselves. This we are entitled to do, if our Idea of Cause be independent of, and superior to, experience: but if we have no Idea of Cause except such as we gather from experience, this reasoning is altogether baseless and unmeaning.

3. Again; by the use of our powers of observation, we are aware of a succession of appearances and events. But none of our senses or powers of external observation can detect in these appearances the power or quality which we call Cause. Cause is that which connects one event with another; but no sense or perception discloses to us, or can disclose, any connexion 175 among the events which we observe. We see that one

occurrence follows another, but we can never see anything which shows that one occurrence *must* follow another. We have already noticed[1], that this truth has been urged by metaphysicians in modern times, and generally assented to by those who examine carefully the connexion of their own thoughts. The arguments are, indeed, obvious enough. One ball strikes another and causes it to move forwards. But by what compulsion? Where is the necessity? If the mind can see any circumstance in this case which makes the result inevitable, let this circumstance be pointed out. But, in fact, there is no such discoverable necessity; for we can conceive this event not to take place at all. The struck ball may stand still, for aught we can see. 'But the laws of motion will not allow it to do so.' Doubtless they will not. But the laws of motion are learnt from experience, and therefore can prove no necessity. Why should not the laws of motion be other than they are? Are they necessarily true? That they are necessarily such as do actually regulate the impact of bodies, is at least no obvious truth; and

therefore this necessity cannot be, in common minds, the ground of connecting the impact of one ball with the motion of another. And assuredly, if this fail, no other ground of such necessary connexion can be shown. In this case, then, the events are not seen to be necessarily connected. But if this case, where one ball moves another by impulse, be not an instance of events exhibiting a necessary connexion, we shall look in vain for any example of such a connexion. There is, then, no case in which events can be observed to be necessarily connected: our idea of causation, which implies that the event is necessarily connected with the cause, cannot be derived from observation.

[1] Book 3. chap. ii.

4. But it may be said, we have not any such Idea of Cause, implying necessary connexion with effect, and a quality by which this connexion is produced. 176 We see nothing but the succession of events; and by *cause* we mean nothing but a certain succession of events;—namely, a constant,

unvarying succession. Cause and effect are only two events of which the second invariably follows the first. We delude ourselves when we imagine that our idea of causation involves anything more.

To this I reply by asking, what then is the meaning of the maxim above quoted, and allowed by all to be universally and necessarily true, that every event must have a cause? Let us put this maxim into the language of the explanation just noticed; and it becomes this:—'Every event must have a certain other event invariably preceding it.' But why must it? Where is the necessity? Why must like events always be preceded by like, except so far as other events interfere? That there is such a necessity, no one can doubt. All will allow that if a stone ascend because it is thrown upwards in one case, a stone which ascends in another case has also been thrown upwards, or has undergone some equivalent operation. All will allow that in this sense, every kind of event must have some other specific kind of event preceding it. But this

turn of men's thoughts shows that they see in events a connexion which is not mere succession. They see in cause and effect, not merely what does, often or always, precede and follow, but what *must* precede and follow. The events are not only conjoined, they are connected. The cause is more than the prelude, the effect is more than the sequel, of the fact. The cause is conceived not as a mere occasion; it is a power, an efficacy, which has a real operation.

5. Thus we have drawn from the maxim, that Every Effect must have a Cause, arguments to show that we have an Idea of Cause which is not borrowed from experience, and which involves more than mere succession. Similar arguments might be derived from any other maxims of universal and necessary validity, which we can obtain concerning Cause: as, for example, the maxims that Causes are measured by their Effects, and that Reaction is equal and opposite to 177 Action. These maxims we shall soon have to examine; but we may observe here, that the necessary

truth which belongs to them, shows that they, and the Ideas which they involve, are not the mere fruits of observation; while their meaning, including, as it does, something quite different from the mere conception of succession of events, proves that such a conception is far from containing the whole import and signification of our Idea of Cause.

The progress of the opinions of philosophers on the points discussed in this chapter, has been one of the most remarkable parts of the history of Metaphysics in modern times: and I shall therefore briefly notice some of its features.

CHAPTER III.

Modern Opinions respecting the Idea of Cause.

1. TOWARDS the end of the seventeenth century there existed in the minds of many of the most vigorous and active speculators of the European literary world, a strong tendency to ascribe the whole of our Knowledge to the teaching of Experience. This tendency, with its consequences, including among them the reaction which was produced when the tenet had been pushed to a length manifestly absurd, has exercised a very powerful influence upon the progress of metaphysical doctrines up to the present time. I proceed to notice some of the most prominent of the opinions which have thus obtained prevalence among philosophers, so far as the Idea of Cause is

concerned.

Locke was one of the metaphysicians who produced the greatest effect in diffusing this opinion, of the exclusive dependence of our knowledge upon experience. Agreeably to this general system, he taught[2] that our ideas of Cause and Effect are got from observation of the things about us. Yet notwithstanding this tenet of his, he endeavoured still to employ these ideas in reasoning on subjects which are far beyond all limits of experience: for he professed to prove, from our idea of Causation, the existence of the Deity[3].

[2] *Essay on the Human Understanding*, b. ii. c. xxvi.

[3] B. iv. c. x.

Hume noticed this obvious inconsistency; but declared himself unable to discover any remedy for a defect so fatal to the most important parts of our knowledge. He could see, in our belief of the succession of cause and effect, nothing but the habit of associating in our minds what had often

been 179 associated in our experience. He therefore maintained that we could not, with logical propriety, extend our belief of such a succession to cases entirely distinct from all those of which our experience consisted. We see, he said, an actual *conjunction* of two events; but we can in no way detect a necessary *connexion*; and therefore we have no means of inferring cause from effect, or effect from cause[4]. The only way in which we recognize Cause and Effect in the field of our experience, is as an unfailing Sequence: we look in vain for anything which can assure us of an infallible Consequence. And since experience is the only source of our knowledge, we cannot with any justice assert that the world in which we live must necessarily have had a cause.

[4] Hume's *Phil. of the Human Mind*, vol. i. p. 94.

2. This doctrine, taken in conjunction with the known skepticism of its author on religious points, produced a considerable fermentation in the speculative world. The

solution of the difficulty thus thrown before philosophers, was by no means obvious. It was vain to endeavour to find in experience any other property of a Cause, than a constant sequence of the effect. Yet it was equally vain to try to persuade men that they had no idea of Cause; or even to shake their belief in the cogency of the familiar arguments concerning the necessity of an original cause of all that is and happens. Accordingly these hostile and apparently irreconcilable doctrines,—the indispensable necessity of a cause of every event, and the impossibility of our knowing such a necessity,—were at last allowed to encamp side by side. Reid, Beattie, and others, formed one party, who showed how widely and constantly the idea of a cause pervades all the processes of the human mind: while another sect, including Brown, and apparently Stewart, maintained that this idea is always capable of being resolved into a constant sequence; and these latter reasoners tried to obviate the dangerous and shocking inferences which some persons might try to draw from their opinion, by

declaring the 180 maxim that “Every event must have a cause,” to be an instinctive law of belief, or a fundamental principle of the human mind[5].

[5] Stewart’s *Active Powers*, vol. i. p. 347. Browne’s *Lectures*, vol. i. p. 115.

3. While this series of discussions was going on in Britain, a great metaphysical genius in Germany was unravelling the perplexity in another way. Kant’s speculations originated, as he informs us, in the trains of thought to which Hume’s writings gave rise; and the *Kritik der Reinen Vernunft*, or *Examination of the Pure Reason*, was published in 1787, with the view of showing the true nature of our knowledge.

Kant’s solution of the difficulties just mentioned differs materially from that above stated. According to Brown[6], succession observed and cause inferred,—the memory of past conjunctions of events and the belief of similar future conjunctions,—are facts, independent, so far as we can discover, but inseparably combined by a law

of our mental nature. According to Kant, causality is an inseparable condition of our experience: a connexion in events is requisite to our apprehending them *as* events. Future occurrences must be connected by causation as the past have been, because we cannot think of past, present, and future, without such connexion. We cannot fix the mind upon occurrences, without including these occurrences in a series of causes and effects. The relation of Causation is a condition under which we think of events, as the relations of space are a condition under which we see objects.

[6] *Lectures*, vol. i. p. 114.

4. On a subject so abstruse, it is not easy to make our distinctions very clear. Some of Brown's illustrations appear to approach very near to the doctrine of Kant. Thus he says[7], 'The *form* of bodies is the relation of their elements to each other in space,—the *power* of bodies is their relation to each other in time.' Yet notwithstanding such approximations in expression, the Kantian

doctrine appears to be different from 181 the views of Stewart and Brown, as commonly understood. According to the Scotch philosophers, the cause and the effect are two things, connected in our minds by a law of our nature. But this view requires us to suppose that we can conceive the law to be absent, and the course of events to be unconnected. If we can understand what is the special force of this law, we must be able to imagine what the case would be if the law were non-existing. We must be able to conceive a mind which does not connect effects with causes. The Kantian doctrine, on the other hand, teaches that we cannot imagine events liberated from the connexion of cause and effect: this connexion is a condition of our conceiving any real occurrences: we cannot think of a real sequence of things, except as involving the operation of causes. In the Scotch system, the past and the future are in their nature independent, but bound together by a rule; in the German system, they share in a common nature and mutual relation, by the act of thought which makes them past

and future. In the former doctrine cause is a tie which binds; in the latter it is a character which pervades and shapes events. The Scotch metaphysicians only assert the *universality* of the relation; the German attempts further to explain its *necessity*.

[7] *Lectures*, vol. i. p. 127.

This being the state of the case, such illustrations as that of Dr. Brown quoted above, in which he represents *cause* as a relation of the same kind with *form*, do not appear exactly to fit his opinions. Can the relations of figure be properly said to be connected with each other by a law of our nature, or a tendency of our mental constitution? Can we ascribe it to a law of our thoughts, that we believe the three angles of a triangle to be equal to two right angles? If so, we must give the same reason for our belief that two straight lines cannot inclose a space; or that three and two are five. But will any one refer us to an ultimate law of our constitution for the belief that three and two are five? Do we not see that they are so, as plainly as we see that they

are three and two? Can we imagine laws of our constitution abolished, so that three and two shall 182 make something different from five;—so that an inclosed space shall lie between two straight lines;—so that the three angles of a plane triangle shall be greater than two right angles? We cannot conceive this. If the numbers *are* three and two; if the lines *are* straight; if the triangle *is* a rectilinear triangle, the consequences are inevitable. We cannot even imagine the contrary. We do not want a law to direct that things should be what they are. The relation, then, of cause and effect, being of the same kind as the necessary relations of figure and number, is not properly spoken of as established in our minds by a special law of our constitution: for we reject that loose and inappropriate phraseology which speaks of the relations of figure and number as ‘determined by laws of belief.’

5. In the present work, we accept and adopt, as the basis of our inquiry concerning our knowledge, the existence of necessary truths concerning causes, as there exist

necessary truths concerning figure and number. We find such truths universally established and assented to among the cultivators of science, and among speculative men in general. All mechanicians agree that reaction is equal and opposite to action, both when one body presses another, and when one body communicates motion to another. All reasoners join in the assertion, not only that every observed change of motion has had a cause, but that every change of motion must have a cause. Here we have certain portions of substantial and undoubted knowledge. Now the essential point in the view which we must take of the idea of cause is this,—that our view must be such as to form a solid basis for our knowledge. We have, in the Mechanical Sciences, certain universal and necessary truths on the subject of causes. Now any view which refers our belief in causation to mere experience or habit, cannot explain the possibility of such necessary truths, since experience and habit can never lead to a perception of necessary connexion. But a view which teaches us to

acknowledge axioms concerning cause, as we acknowledge axioms 183 concerning space, will lead us to look upon the science of mechanics as equally certain and universal with the science of geometry; and will thus materially affect our judgment concerning the nature and claims of our scientific knowledge.

Axioms concerning Cause, or concerning Force, which as we shall see, is a modification of Cause, will flow from an Idea of Cause, just as axioms concerning space and number flow from the ideas of space and number or time. And thus the propositions which constitute the science of Mechanics prove that we possess an idea of cause, in the same sense in which the propositions of geometry and arithmetic prove our possession of the ideas of space and of time or number.

6. The idea of cause, like the ideas of space and time, is a part of the *active* powers of the mind. The relation of cause and effect is a relation or condition under

which events are apprehended, which relation is not given by observation, but supplied by the mind itself. According to the views which explain our apprehension of cause by reference to habit, or to a supposed law of our mental nature, causal connexion is a consequence of agencies which the mind passively obeys; but according to the view to which we are led, this connexion is a result of faculties which the mind actively exercises. And thus the relation of cause and effect is a condition of our apprehending successive events, a part of the mind's constant and universal activity, a source of necessary truths; or, to sum all this in one phrase, a Fundamental Idea.

CHAPTER IV.

Of the Axioms which relate to the Idea of Cause.

1. *Causes are abstract Conceptions.*—We have now to express, as well as we can, the fundamental character of that Idea of Cause of which we have just proved the existence. This may be done, at least for purposes of reasoning, in this as in former instances, by means of axioms. I shall state the principal axioms which belong to this subject, referring the reader to his own thoughts for the axiomatic evidence which belongs to them.

But I must first observe, that in order to express general and abstract truths concerning cause and effect, these terms, *cause* and *effect*, must be understood in a general and abstract manner. When one

event gives rise to another, the first *event* is, in common language, often called the cause, and the second the effect. Thus the meeting of two billiard-balls may be said to be the cause of one of them turning aside out of the path in which it was moving. For our present purposes, however, we must not apply the term cause to such occurrences as this meeting and turning, but to a certain conception, *force*, abstracted from all such special events, and considered as a quality or property by which one body affects the motion of the other. And in like manner in other cases, cause is to be conceived as some abstract quality, power, or efficacy, by which change is produced; a quality not identical with the events, but disclosed by means of them. Not only is this abstract mode of conceiving force and cause useful in expressing the fundamental principles of science; but it supplies us with the only mode by which such principles can be 185 stated in a general manner, and made to lead to substantial truth and real knowledge.

Understanding *cause*, therefore, in this sense, we proceed to our Axioms.

2. First Axiom. *Nothing can take place without a Cause.*

Every event, of whatever kind, must have a cause in the sense of the term which we have just indicated; and that it must, is a universal and necessary proposition to which we irresistibly assent as soon as it is understood. We believe each appearance to come into existence,—we conceive every change to take place,—not only with something preceding it, but something by which it is made to be what it is. An effect without a cause;—an event without a preceding condition involving the efficacy by which the event is produced;—are suppositions which we cannot for a moment admit. That the connexion of effect with cause is universal and necessary, is a universal and constant conviction of mankind. It persists in the minds of all men, undisturbed by all the assaults of sophistry and skepticism; and, as we have seen in the

last chapter, remains unshaken, even when its foundations seem to be ruined. This axiom expresses, to a certain extent, our Idea of Cause; and when that idea is clearly apprehended, the axiom requires no proof, and indeed admits of none which makes it more evident. That notwithstanding its simplicity, it is of use in our speculations, we shall hereafter see; but in the first place, we must consider the other axioms belonging to this subject.

3. Second Axiom. *Effects are proportional to their Causes, and Causes are measured by their Effects.*

We have already said that *cause* is that quality or power, in the circumstances of each case, by which the effect is produced; and this power, an abstract property of the condition of things to which it belongs, can in no way fall directly under the cognizance of the senses. Cause, of whatever kind, is not apprehended as including objects and events which share its nature by being co-extensive with certain portions of it, as

space and time are. It cannot therefore, like them, be 186 measured by repetition of its own parts, as space is measured by repetition of inches, and time by repetition of minutes. Causes may be greater or less; as, for instance, the force of a man is greater than the force of a child. But how much is the one greater than the other? How are we to compare the abstract conception, force, in such cases as these?

To this, the obvious and only answer is, that we must compare causes by means of their effects;—that we must compare force by something which force can do. The child can lift one fagot; the man can lift ten such fagots: we have here a means of comparison. And whether or not the rule is to be applied in this manner, that is, by the number of things operated on, (a question which we shall have to consider hereafter,) it is clear that this form of rule, namely, a reference to some effect or other as our measure, is the right, because the only possible form. The cause determines the effect. The cause being the same, the effect

must be the same. The connexion of the two is governed by a fixed and inviolable rule. It admits of no ambiguity. Every degree of intensity in the cause has some peculiar modification of the effect corresponding to it. Hence the effect is an unfailing index of the amount of the cause; and if it be a measurable effect, gives a measure of the cause. We can have no other measure; but we need no other, for this is exact, sufficient and complete.

It may be said, that various effects are produced by the same cause. The sun's heat melts wax and expands quicksilver. The force of gravity causes bodies to move downwards if they are free, and to press down upon their supports if they are supported. Which of the effects is to be taken as the measure of heat, or of gravity, in these cases? To this we reply, that if we had merely different states of the same cause to compare, any of the effects might be taken. The sun's heat on different days might be measured by the expansion of quicksilver, or by the quantity of wax

melted. The force of gravity, if it were different at different places, might be measured by the spaces through which a given weight would bend an elastic 187 support, or by the spaces through which a body would fall in a given time. All these measures are consistent with the general character of our idea of cause.

4. *Limitation of the Second Axiom.*—But there may be circumstances in the nature of the case which may further determine the kind of effect which we must take for the measure of the cause. For example, if causes are conceived to be of such a nature as to be capable of addition, the effects taken as their measure must conform to this condition. This is the case with mechanical causes. The weights of two bodies are the causes of the pressure which they exert downwards; and these weights are capable of addition. The weight of the two is the sum of the weight of each. We are therefore not at liberty to say that weights shall be measured by the spaces through which they bend a certain elastic support, except we

have first ascertained that the whole weight bends it through a space equal to the sum of the inflections produced by the separate weights. Without this precaution, we might obtain inconsistent results. Two weights, each of the magnitude 3 as measured by their effects, might, if we took the inflections of a spring for the effects, be together equal to 5 or to 7 by the same kind of measurement. For the inflection produced by two weights of 3 might, for aught we can see beforehand, be more or less than twice as great as the inflection produced by one weight of 3. That forces are capable of addition, is a condition which limits, and, as we shall see, in some cases rigorously fixes, the kind of effects which are to be taken as their measures.

Causes which are thus capable of addition are to be measured by the repeated addition of equal quantities. Two such causes are *equal* to each other when they produce exactly the same effect. So far our axiom is applied directly. But these two causes can be *added* together; and being thus added,

they are *double* of one of them; and the cause composed by addition of *three* such, is *three* times as great as the first; and so on for any measure whatever. By this means, and by this 188 means only, we have a complete and consistent measure of those causes which are so conceived as to be subject to this condition of being added and multiplied.

Causes are, in the present chapter, to be understood in the widest sense of the term; and the axiom now under our consideration applies to them, whenever they are of such a nature as to admit of any measure at all. But the cases which we have more particularly in view are *mechanical* causes, the causes of the motion and of the equilibrium of bodies. In these cases, forces are conceived as capable of addition; and what has been said of the measure of causes in such cases, applies peculiarly to mechanical forces. Two weights, placed together, may be considered as a single weight, equal to the *sum* of the two. Two pressures, pushing a body in the same

direction at the same point, are identical in all respects with some single pressure, their *sum*, pushing in like manner; and this is true whether or not they put the body in motion. In the cases of mechanical forces, therefore, we take some certain effect, velocity generated or weight supported, which may fix the *unit* of force; and we then measure all other forces by the successive repetition of this unit, as we measure all spaces by the successive repetition of our unit of lineal measure.

But these steps in the formation of the science of Mechanics will be further explained, when we come to follow our axioms concerning cause into their application in that science. At present we have, perhaps, sufficiently explained the axiom that causes are measured by their effects, and we now proceed to a third axiom, also of great importance.

5. Third Axiom. *Reaction is equal and opposite to Action.*

In the case of mechanical forces, the

action of a cause often takes place by an operation of one body upon another; and in this case, the action is always and inevitably accompanied by an *opposite* action. If I press a stone with my hand, the stone presses my hand in return. If one ball strike another and put it in motion, the second ball diminishes the motion of 189 the first. In these cases the operation is mutual; the Action is accompanied by a Reaction. And in all such cases the Reaction is a force of exactly the same nature as the Action, exerted in an opposite direction. A pressure exerted upon a body at rest is resisted and balanced by another pressure; when the pressure of one body puts another in motion, the body, though it yields to the force, nevertheless exerts upon the pressing body a force like that which it suffers.

Now the axiom asserts further, that this Reaction is *equal*, as well as opposite, to the Action. For the Reaction is an effect of the Action, and is determined by it. And since the two, Action and Reaction, are forces of the same nature, each may be considered as

cause and as effect; and they must, therefore, determine each other by a common rule. But this consideration leads necessarily to their equality: for since the rule is mutual, if we could for an instant suppose the Reaction to be less than the Action, we must, by the same rule, suppose the Action to be less than the Reaction. And thus Action and Reaction, in every such case, are rigorously equal to each other.

It is easily seen that this axiom is not a proposition which is, or can be, proved by experience; but that its truth is anterior to special observation, and depends on our conception of Action and Reaction. Like our other axioms, this has its source in an Idea; namely, the Idea of Cause, under that particular condition in which cause and effect are mutual. The necessary and universal truth which we cannot help ascribing to the axiom, shows that it is not derived from the stores of experience, which can never contain truths of this character. Accordingly, it was asserted with equal confidence and generality by those who did

not refer to experience for their principles, and by those who did. Leonicus Tomæus, a commentator of Aristotle, whose work was published in 1552, and therefore at a period when no right opinions concerning mechanical reaction were current, at least in his school, says, in his remarks on the Author's Questions concerning the communication of motion, that 'Reaction is equal and 190 contrary to Action.' The same principle was taken for granted by all parties, in all the controversies concerning the proper measure of force, of which we shall have to speak: and would be rigorously true, as a law of motion, whichever of the rival interpretations of the measure of the term 'Action' we were to take.

6. *Extent of the Third Axiom.*—It may naturally be asked whether this third Axiom respecting causation extends to any other cases than those of mechanical action, since the notion of Cause in general has certainly a much wider extent. For instance, when a hot body heats a cold one, is there necessarily an equal reaction of the second

body upon the first? Does the snowball cool the boy's hand exactly as much as the hand heats the snow? To this we reply, that, in every case in which one body acts upon another by its physical qualities, there must be some reaction. No body can affect another without being itself also affected. But in any physical change the *action* exerted is an abstract term which may be variously understood. The hot hand may *melt* a cool body, or may *warm* it: which kind of effect is to be taken as action? This remains to be determined by other considerations.

In all cases of physical change produced by one body in another, it is generally possible to assume such a meaning of action, that the reaction shall be of the same nature as the action; and when this is done, the third axiom of causation, that reaction is equal to action, is universally true. Thus if a hot body heat a cold one, the change may be conceived as the transfer of a certain substance, *heat* or *caloric*, from the first body to the second. On this supposition, the

first body *loses* just as much heat as the other *gains*; action and reaction are equal. But if the reaction be of a different kind to the action we can no longer apply the axiom. If a hot body *melt* a cold one, the latter *cools* the former: here, then, is reaction; but so long as the action and reaction are stated in this form, we cannot assert any equality between them.

In treating of the secondary mechanical sciences, we 191 shall see further in what way we may conceive the physical action of one body upon another, so that the same axioms which are the basis of the science of Mechanics shall apply to changes not at first sight manifestly mechanical.

The three axioms of causation which we have now stated are the fundamental maxims of all reasoning concerning causes as to their quantities; and it will be shown in the sequel that these axioms form the basis of the science of Mechanics, determining its form, extent, and certainty. We must, however, in the first place,

consider how we acquire those conceptions upon which the axioms now established are to be employed.

[2d Ed.] [The Axiom that *Reaction is equal and opposite to Action*, may appear to be at variance with a maxim concerning Cause which is commonly current; namely, that the 'Cause precedes Effect, and Effect follows Cause.' For it may be said, if *A*, the Action, and *R*, the Reaction, can be considered as mutually the cause of each other, *A* must precede *R*, and yet must follow it, which is impossible. But to this I reply, that in those cases of direct Causation to which the maxim applies, the Cause and Effect are not successive, but simultaneous. If I press against some obstacle, the obstacle resists and returns the pressure at the instant it is exerted, not after any interval of time, however small. The common maxim, that the effect follows the cause, has arisen from the practice of considering, as examples of cause and effect, not instantaneous forces or causes, and the instantaneous changes which they produce;

but taking, instead of this latter, the *cumulative* effects produced in the course of time, and compared with like results occurring without the action of the cause. Thus, if we alter the length of a clock-pendulum, this change produces, as its effect, a subsequent change of rate in the clock: because the rate is measured by the accumulated effects of the pendulum's gravity, before and after the change. But the pendulum produces its mechanical effect upon the escapement, at the moment of its contact, and each wheel upon the next, at the moment of *its* contact. As has 192 been said in a Review of this work, 'The time lost in cases of indirect physical causation is consumed in the movements which take place among the parts of the mechanism in action, by which the active forces so transformed into momentum are transported over intervals of space to new points of action, the motion of matter in such cases being regarded as a mere carrier of force.' (*Quarterly Rev.* No. cxxxv. p. 212.)

This subject I have further treated in the

Memoirs of the Cambridge Philosophical Society, vol. vii. part iii.] [In this Third Edition I add this discussion.]

Discussion of the Question:—Are Cause and Effect successive or simultaneous?

I HAVE at various times laid before this Society dissertations on the metaphysical grounds and elements of our knowledge, and especially on the foundations of the science of mechanics. As these speculations have not failed to excite some attention, both here and elsewhere, I am tempted to bring forward in the same manner some additional disquisitions of the same kind. Indeed, the immediate occasion of the present memoir is of itself an evidence that such subjects are not supposed to be without their interest for the general reader; for I am led to the views and reasonings which I am now about to lay before the Society, by some remarks in one of our most popular Reviews, (*The Quarterly Review*, Article on the *History* and *Philosophy of the Inductive Sciences*, June 1841). A writer of singular acuteness and comprehensiveness of view has there made remarks upon the doctrines which I had delivered in the *Philosophy of the Inductive Sciences*, which remarks appear to me in the highest degree instructive and philosophical. I am not, however, going here to discuss fully the doctrines contained in this critique. With respect to its general tendency, I will only observe, that the author does not accept, in

the form in which I had given it, the account of the origin and ground of necessary and universal truths. I had stated that our knowledge is derived from Sensations and Ideas; and that Ideas, which are the conditions of perception, such as *space*, *time*, *likeness*, *cause*, make universal and necessary knowledge possible; whereas, if knowledge were derived from Sensation alone, it could not have those characters. I have moreover 193 enumerated a long series of Fundamental Ideas as the bases of a corresponding series of sciences, of which sciences I have shown also, by an historical survey, that they claim to possess universal truths, and have their claims allowed. I have gone further: for I have stated the Axioms which flow from these Fundamental Ideas, and which are the logical grounds of necessity and universality in the truths of each science, when the science is presented in the form of a demonstrated system. The Reviewer does not assent to this doctrine, nor to the argument by which it is supported; namely, that Experience cannot lead to universal truths, except by means of a universal Idea supplied by the mind, and infused into the particular facts which observation ministers. He considers that the existence of universal truths in our knowledge may be explained otherwise. He holds that it is a sufficient account of the matter to say that we pass from special experience to universal truth in virtue of 'the inductive propensity—the irresistible impulse of the mind to generalize *ad infinitum*.' I shall not here dwell upon very strong reasons which may be

assigned, as I conceive, for not accepting this as a full and satisfactory explanation of the difficulty. Instead of doing so, I shall here content myself with remarking, that even if we adopt the Reviewer's expressions, we must still contend that there are *different forms* of the *impulse of the mind to generalize*, corresponding to each of the Fundamental Ideas of our system. These Fundamental Ideas, if they be nothing else, must at least be accepted as a classification of the modes of action of the Inductive Propensity,—as so many different paths and tendencies of the Generalizing Impulse: and the Axioms which I have stated as the express results of the Fundamental Ideas, and as the steps by which those Ideas make universal truths possible, are still no less worthy of notice, if they are stated as the results of our Generalizing Impulse; and as the steps by which that Impulse, in its many various forms, makes universal truths possible. The Generalizing Impulse in that operation by which it leads us to the Axioms of Geometry, and to those of Mechanics, takes very different courses; and these courses may well deserve to be separately studied. And perhaps, even if we accept this view of the philosophy of our knowledge, no simpler or clearer way can be found of describing and distinguishing these fundamentally different operations of the Inductive Propensity, than by saying, 194 that in the one case it proceeds according to the Idea of Space, in another according to the Idea of Mechanical Cause; and the like phraseology may be employed for all the other cases.

This then being understood, my present object is to consider some very remarkable, and, as appears to me, novel views of the Idea of Cause which the Reviewer propounds. And these may be best brought under our discussion by considering them as an attempt to solve the question, Whether, according to our fundamental apprehensions of the relation of Cause and Effect, effect follows cause in the order of time, or is simultaneous with it.

At first sight, this question may seem to be completely decided by our fundamental convictions respecting cause and effect, and by the axioms which have been propounded by almost all writers, and have obtained universal currency among reasoners on this subject. That the cause must precede the effect,—that the effect must follow the cause,—are, it might seem, self-evident truths, assumed and assented to by all persons in all reasonings in which those notions occur. Such a doctrine is commonly asserted in general terms, and seems to be verified in all the applications of the idea of cause. A heavy body produces motion by its weight; the motion produced is subsequent in time to the pressure which the weight exerts. In a machine, bodies push or strike each other, and so produce a series of motions; each motion, in this case, is the result of the motions and configurations which have preceded it. The whole series of such motions employs time; and this time is filled up and measured by the series of causes and effects, the effects being, in their turn, causes of

other effects. This is the common mode of apprehending the universal course of events, in which the chain of causation, and the progress of time, are contemplated as each the necessary condition and accompaniment of the other.

But this, the Critic remarks, is not true in *direct* causation. 'If the antecedence and consequence in question be understood as the interposition of an interval of time, however small, between the action of the cause and the production of the effect, we regard it as inadmissible. In the production of motion by force, for instance, though the effect be cumulative with continued exertion of the cause, yet each elementary or individual action is, to our apprehension, *instanter* accompanied with its corresponding increment of momentum in the body moved. In all dynamical 195 reasonings no one has ever thought of interposing an instant of time between the action and its resulting momentum; nor does it appear necessary.' This is so evident, that it appears strange it should have the air of novelty; yet, so far as I am aware, the matter has never before been put in the same point of view. But this being the case, the question occurs, how it is that time *seems* to be employed in the progress from cause to effect? How is it that the opinion of the effect being subsequent to the cause has generally obtained? And to this the Critic's answer is obvious:—it is so in cases of indirect or of *cumulative* effect. If a ball *A* strikes another, *B*, and puts it in motion, and *B*

strikes *C*, and puts it in motion, *A*‘s impact may be considered as the cause, though not the direct cause, of *C*‘s motion. Now time, namely the time of *B*‘s motion after it is struck by *A*, and before it strikes *C*, intervenes between *A*‘s impact and the beginning of *C*‘s motion: that is, between the cause and its effect. In this sense, the effect is subsequent to the cause. Again, if a body be put in motion by a series of impulses acting at finite intervals of time, all in the same direction, the motion at the end of all these intervals is the effect of all the impulses, and exists after they have all acted. It is the accumulated effect, and subsequent to each separate action of the cause. But in this case, each impulse produces its effect instantaneously, and the time is employed, not in the transition from any cause to its effect, but in the intervals between the action of the several causes, during which intervals the body goes on with the velocity already communicated to it. In each impulse, force produces motion: and the motion goes on till a new change takes place, by the same kind of action. The force may be said, in the language employed by the Critic, to be transformed into momentum; and in the successive impulses, successive portions of force are thus transformed; while in the intervening intervals, the force thus transformed into momentum is carried by the body from one place to another, where a new change awaits it. ‘The cause is absorbed and transformed into effect, and therein treasured up.’ Hence, as the Writer says, ‘The time lost in cases of indirect

physical causation is that consumed in the movements which take place among the parts of the mechanism set in action, by which the active forces so transformed into mechanism are transported over intervals of space to new points of action, the motion of matter in such cases being 196 regarded as a mere carrier of force':—and when force is directly counteracted by force, their mutual destruction must be conceived, as the Reviewer says, to be instantaneous. We can therefore hardly resist his conclusion, that men have been misled in assuming sequence as a feature in the relation of cause and effect; and we may readily assent to his suggestion, that sequence, when observed, is to be held as a sure indication of indirect action, accompanied with a movement of parts.

But yet if we turn for a moment to other kinds of causation, we seem to be compelled at every step to recognize the truth of the usual maxim upon this subject, that effects are subsequent to causes. Is not poison, taken at a certain moment, the cause of disorder and death which follow at a *subsequent* period? Is not a man's early prudence often the cause of his prosperity in *later* life, and his folly, though for a moment it may produce gratification, *finally* the cause of his ruin? And even in the case of mechanism, if, in a clock which goes rightly, we alter the length of the pendulum, is not this alteration the cause of an alteration which *afterwards* takes place in the rate of the clock's going? Are not all these, and

innumerable other cases, instances in which the usual notion of the effect following the cause is verified? and are they not irreconcileable with the new doctrine of cause and effect being simultaneous?

In order to disentangle this apparent confusion, let us first consider the case last mentioned, of a clock, in which some alteration is made which affects the rate of going.

So long as the parts of the clock remain unaltered, its rate will remain unaltered; and any part which is considered as capable of alteration, may be considered as, if we please, the cause of the unaltered rate, by being itself unaltered. But we do not usually introduce the positive idea of cause, to correspond with this negation of change. If we speak of the rate as unaltered, we may also say that it is so because there is *no cause* of alteration. The steady rate is the indication of the absence of any cause of alteration; and the rate of going measures the progress of time, in a state of things in which causes of change are thus excluded. If an alteration takes place in any part of the clock, once for all, the rate is altered; but the new rate is steady as the old rate was, and, like it, measures the uniform progress of time. But the difference between the new rate and the old is occasioned by 197 the difference of the parts of the clock; and the new rate may very properly be said to be caused by the change of the parts, and to be subsequent to it: for it does prevail after the change, and does not prevail before.

But how is this view to be reconciled with the one just quoted from the Reviewer, and, as it appeared, satisfactorily proved by him; according to which all mechanical effects are simultaneous with their causes, and not subsequent to them? We have here the two views in close contact, and in seeming opposition.

In the going of a clock, the parts are in motion; and these motions are determined by forces arising from the form and connexion of the parts of the mechanism. Each of the forces thus exerted at any instant produces its effect at the same instant; and thus, so far as the term *cause* refers to such instantaneous forces, the cause and the effect are simultaneous. But if such instantaneous forces act at successive intervals of time, the motion during each interval is unaltered, and by its uniform progress measures the progress of time. And thus the motion of the machine consists of a series of intervals, during each of which the motion is uniform, and measures the time; separated from each other by a series of changes, at each of which the change measures the instantaneous force, and is simultaneous with it. And if, in this case, we suppose, at any point of time, the instantaneous forces to cease, the succession of them being terminated, from that point of time the motion would be uniform. And since the rate of the motion in each interval of time is determined by the instantaneous force which last acted and by the

preceding motion, the rate of the motion in each interval of time is determined by all the preceding instantaneous forces. Hence, when the series of instantaneous forces stops, the rate at which the motion goes on permanently, from that point of time, is determined by the antecedent series of such forces, which series may be considered as an aggregate cause; and hence it appears, that the *permanent* effect is determined by the *aggregate* cause; and in this sense the effect is subsequent to the cause.

Thus we obtain, in this case, a solution of the difficulty which is placed before us. The instantaneous effect or change is simultaneous with the instantaneous force or cause by which it is 198 produced. But if we consider a series of such instantaneous forces as a single aggregate cause, and the final condition as a permanent effect of this cause, the effect is subsequent to the cause. In this case, the cause is immediately succeeded by the effect. The cause acts in time: the effect goes on in time. The times occupied by the cause and by the effect succeed each other, the one ending at the point of time at which the other begins. But the time which the cause occupies is really composed of a series of instants of uniform motion interposed between instantaneous forces; and during the time that this series of causes is going on, to make up the aggregate cause, a series of effects is going on to make up the final effect. There is a progressive cause and a progressive effect which go on together, and

occupy the same finite time; and this simultaneous progression is composed of all the simultaneous instantaneous steps of cause and effect. The aggregate cause is the sum of the progression of causes; the final effect is the last term of the progression of effects. At each step, as the Reviewer says, cause is transformed into effect; and it is treasured up in the results during the intermediate intervals; and the time occupied is not the time which intervenes between cause and effect at each step, but the time which intervenes between these transformations.

I have supposed forces to act at distinct instants, and to cease to act in the intervals between; and then, the aggregate of such intervals to make up a finite time, during which an aggregate force acts. But if the action of the force be rigorously continuous, it will easily be seen that all the consequences as to cause and effect will be the same; the discontinuous action being merely the usual artifice by which, in mathematical reasonings, we obtain results respecting continuous changes. It will still be true, that the uniform motion which takes place after a continuous force has acted, is the effect subsequent to the cause; while the change which takes place at any instant by the action of the force, is the instantaneous effect simultaneous with the cause.

It may be objected, that this solution does not appear immediately to apply: for the motion of a clock is not uniform during any portion of the time.

The parts move by intervals of varied motion and of rest; or by oscillations backwards and forwards; and the succession of forces which acts during any 199 oscillation, or any cycle of motion, is repeated during the succeeding oscillation or cycle, and so on indefinitely; and if an alteration be made in the parts, it is not a change once for all, but recurs in its operation in every cycle of the motion.

But it will be found that this circumstance does not prevent the same explanation from being still applicable with a slight modification. Instead of uniform motion in the intervals of causation, we shall have to speak of *steady going*: and instead of considering all the forces which affect the motion as causes of change of uniform motion, we shall have to speak of changes in the parts of the mechanism as causes of *change of rate of going*. With this modification, it will still be true, that any instantaneous cause produces its instantaneous effect simultaneously, while the permanent effect is subsequent to the change which is its cause. The steady going of the clock is assumed as a normal condition, in which it measures the progress of time; and in this assumption, the notion of cause and effect is not brought into view. But a steady rate thus denoting the mean passage of time, a change in the rate indicates a cause of change. The *change of rate*, as an instantaneous *transition* from one rate to another, is *simultaneous* with the change in the parts. But then the *changed rate* as a continued *condition* in

which, no new change supervening, the rate again measures the progress of time, is *subsequent* to the change of parts, for it begins when that ends, and continues when the progress of that has ceased.

If, however, this be a satisfactory solution of the difficulty in the case of mechanism, how shall we apply the same views to the other cases? Growth, the effect of food, is subsequent to the act of taking food; disorder, the effect of poison, is subsequent to the introduction of poison into the system. Can we say that the animal would continue unchanged if it were not to take food; and that food is the cause of a change, namely, of growth? This is manifestly false; for if the animal were not to take food, it would soon perish. But the analogy of the former case, of the clock, will enable us to avoid this perplexity. As we assumed a steady rate of going in the clock to be the measure of time when we considered the effect of mechanism, so we assume a steady rate of action in the animal functions to be the measure of the progress of time when we consider the causes which act upon the 200 development and health of animals. Digestion, and of course nutrition, are a part of this normal condition; they are involved in the steady going of the animal mechanism, and we must suppose these functions to go regularly on, in order that the animal may preserve its character of animal. Food and digestion may be considered as causes of the continued existence of the animal, in the same way in which the form of the parts of a clock is the

cause of the steady going of a clock. And when we come to consider causes of change, this kind of causation, which produces a normal condition of things, merely measuring the flow of time, is left out of our account. We can conceive an uniform condition of animal existence, the animal neither growing nor wasting. This being taken as the normal condition, any deviation from this condition indicates a cause, and is taken as the evidence and measure of the cause of change. And thus, in a growing animal, the food partly keeps the animal in continued animal existence, and partly, and in addition to this, causes its growth. Food, in the former view, is always circulating in the system, and is supposed to be uniformly administered; the cycles of nutrition being merged in the notion of uniform existence, as the oscillations of the pendulum in a clock are merged in the notion of uniform going; and the elementary steps of nutrition which are, in this view, supposed to take place at each instant, produce their instantaneous effect, for they are requisite in the cycle of animal processes which goes on from instant to instant. But on the other hand, in considering growth, we compare the state of an animal with a preceding state, and consider the nutriment taken in the intervening time as the cause of the change: hence this nutriment, as an aggregate, is considered as the cause of growth of the animal; and in this view the effect is subsequent to the cause. But yet here, as in the case of mechanism, the progressive effect is simultaneous, step by step, with

the progressive cause. There is a series of operations; as for instance, intussusception, digestion, assimilation, growth: each of these is a progressive operation; and in the progress of each operation, the steps of the effect and the instantaneous forces are simultaneous. But the end of one operation is the beginning of the next, or at least in part, and hence we have time occupied by the succession. The end of intussusception is the beginning of digestion, the end of digestion the beginning of assimilation, 201 and so on. These aggregate effects succeed each other; and hence growth is subsequent to the taking of food; though each instantaneous force of animal life, no less than of mechanism, produces an effect simultaneous with its action. Each of these separate operations is an aggregate operation, and occupies time; and each aggregate effect is a condition of the action of the cause in the next operation.

Again; if an animal in a permanent condition, neither waxing nor wasting, may be taken as the normal state in which the functions of life measure time, in order that we may consider growth as an effect, to be referred to food as cause; we may, for other purposes, consider, as the normal condition, an animal waxing and then wasting, according to the usual law of animal life: and we must take this, the healthy progress of an animal, as our normal condition, if we have to consider causes which produce disease. If we have to refer the morbid condition of an animal to the influence of poison, for

example, we must consider how far the condition deviates from what it would have been if the poison had not been taken into the frame. The usual progress of the animal functions including its growth, is the measure of time; the deviation from this usual progress is the indication of cause; and the effect of the poison is subsequent to the cause, because the poison acts through the cycle of the animal functions just mentioned, which occupies time; and because the taking the poison into the system, not any subsequent action of the animal forces in the system, is considered as the event which we must contemplate as a cause. To resume the analogy of the clock: the rate of the clock is altered by altering the parts; but this alteration itself may occupy time; as if we alter the rate of a clock by applying a drop of acid, which gradually eats off a part of the pendulum, the corrosion, as an aggregate effect, occupies time; and the rates before and after the change are separated by this time. But the application of the drop is the cause; and thus, in this case the final effect is subsequent to the cause, though here, as in the case of mechanism, the instantaneous forces always produce a simultaneous effect.

Thus we have in every case a *uniform* state, or a state which is considered as uniform, or at least *normal*; and which is taken as the indication and measure of *time*; and we have also *change*, 202 which is contemplated as a deviation from uniformity, and

is taken as the indication and measure of *cause*. The uniform state may be one which never exists, being purely imaginary; as the case in which no forces act; and the case in which animal functions go on permanently, the animal neither growing nor wasting. The normal state may also be a state in which change is constantly taking place, as, in fact, even a state of motion is a state of change; such states also are, in a further sense, that of a clock going by starts, and that of an animal constantly growing: in these cases the changes are all merged in a wider view of uniformity, so that these are taken as the normal states. And in all these cases, successive changes which take place are separated by intervals of time, measured by the normal progress; and each change is produced by some *simultaneous* instantaneous cause. But taking the cause in a larger sense, we group these instantaneous causes, and perhaps omit in our contemplation some of the intervening intervals; and thus assign the cause to a *preceding*, and the effect to a *succeeding* time.

I may observe further, as a corollary from what has been said, that the measure of time is different, when we consider different kinds of causation; and in each case, is *homogeneous* with the changes which causation effects. In the consideration of mechanical causes, we measure time by mechanical changes;—by uniform motion, or uniform succession of cycles of motion; by the rotation of a wheel, or the oscillation of a pendulum. But if we have to consider

physiological changes, the progress of time is physiologically measured;—by the normal progress of vital operations; by the circulation, digestion or development of the organized body; by the pulse, or by the growth. These different measures of time give to time, so far as it is exhibited by facts and events, a different character in the different cases. Phenomenal time has a different nature and essence according to the kind of the changes which we consider, and which gives us our sole phenomenal indication of cause.

I fear that I am travelling into matters too abstruse and metaphysical for the occasion: but before I conclude, I will present one other aspect of the subject.

In stating the difficulty, I referred to cases of moral as well as physical causation; as when prudence produces prosperity, or 203 when folly produces ruin. It may be asked, whether we are here to apply the same explanation;—whether we are to assume a normal condition of human existence, in which neither prudence nor folly are displayed, neither prosperity nor adversity produced;—whether we are to conceive the progress of such a state to measure the progress of time, and deviations from it to denote causes of the kind mentioned. It may be asked further, whether, if we do make this supposition, we can resolve the influence of such causes as prudence or imprudence into instantaneous acts, which produce their effects immediately: and which occupy

time only by being separated by intervals of the inactive normal moral condition. To this I must here reply, that the discussion of such questions would carry me too far, and would involve speculations not included within the acknowledged domain of this Society, from which I therefore abstain. But I may say, before quitting the subject, that I do not think the suppositions above suggested are untenable; and that in order to include moral causation under the maxims of causation in general, we must necessarily make some such hypothesis. The peculiarity of that kind of causation which the will and the character exert, and which is exerted upon the will and the character, would make this case far more complex and difficult than those already considered; but, at the same time, would offer us the means of explaining what may seem harsh, in the above analogy. For instance, we should have to assume such a maxim as this: that in moral causation, time is not to be measured by the flow of mechanical or physiological events;—not by the clock, or by the pulse. Moral causation has its own clock, its own pulse, in the progress of man's moral being; and by this measure of time is the relation of moral cause and effect to be defined.

That in estimating moral causation, the progress of time is necessarily estimated by moral changes, and not by machinery,—by the progress of events, and not by the going of the clock,—is a truth familiar as a practical maxim to all who give their thoughts to

dramatic or narrative fictions. Who feels any thing incongruous or extravagantly hurried in the progress of events in that great exhibition of moral causation, the tragedy of Othello? If we were asked what time those vast and terrible 204 and complex changes of the being and feelings of the characters occupy, we should say, that, measured on its own scale, the event is of great extent;—that the transaction is of considerable magnitude in all ways. But if, with previous critics, we look into the progress of time by the day and the hour—what is the measure of this history? Forty-eight hours.

CHAPTER V.

Of the Origin of our Conceptions of Force and Matter.

1. *Force.*—When the faculties of observation and thought are developed in man, the idea of causation is applied to those changes which we see and feel in the state of rest and motion of bodies around us. And when our abstract conceptions are thus formed and named, we adopt the term *Force*, and use it to denote that property which is the cause of motion produced, changed, or prevented. This conception is, it would seem, mainly and primarily suggested by our consciousness of the exertions by which we put bodies in motion. The Latin and Greek words for *Force*, Vis, Fὶς, were probably, like all abstract terms, derived at first from some sensible object.

The original meaning of the Greek word was a *muscle* or *tendon*. Its first application as an abstract term is accordingly to muscular force:

> Δεύτερος αὖτ' Αἴας πολὺ μείζονα λᾶαν ἀείρας
>
> ἦκ' ἐπιδινήσας, ἐπέρεισε δὲ FΙΝ' ἀπέλεθρον.
>
> Then Ajax a far heavier stone upheaved,
>
> He whirled it, and impressing Force intense
>
> Upon the mass, dismist it.

The property by which bodies affect each other's motions, was naturally likened to that energy which we exert upon them with similar effect: and thus the labouring horse, the rushing torrent, the descending weight, the elastic bow, were said to exert force. 206 Homer[8] speaks of the *force* of the river, Fὶς ποταμοῖο; and Hesiod[9] of the *force* of the north wind, Fὶς ἀνέμου βορέαο.

[8] *Il.* xxi.

[9] *Op. et D.*

Thus man's general notion of force was probably first suggested by his muscular exertions, that is, by an act depending upon that muscular sense, to which, as we have already seen, the perception of space is mainly due. And this being the case, it will be easily understood that the *Direction* of the force thus exerted is perceived by the muscular sense, at the same time that the force itself is perceived; and that the direction of any other force is understood by comparison with force which man must exert to produce the same effect, in the same manner as force itself is so understood.

This abstract notion of Force long remained in a very vague and obscure condition, as may be seen by referring to the History for the failures of attempts at a science of force and motion, made by the ancients and their commentators in the middle ages. By degrees, in modern times, we see the scientific faculty revive. The conception of Force becomes so far distinct and precise that it can be reasoned upon in

a consistent manner, with demonstrated consequences; and a genuine science of Mechanics comes into existence. The foundations of this science are to be found in the Axioms concerning causation which we have already stated; these axioms being interpreted and fixed in their application by a constant reference to observed facts, as we shall show. But we must, in the first place, consider further those primary processes of observation by which we acquire the first materials of thought on such subjects.

2. *Matter.*—The conception of Force, as we have said, arises with our consciousness of our own muscular exertions. But we cannot imagine such exertions without also imagining some bodily substance against which they are exercised. If we press, we press something: if we thrust or throw, there must be something 207 to resist the thrust or to receive the impulse. Without body, muscular force cannot be exerted, and force in general is not conceivable.

Thus Force cannot exist without *Body* on

which it acts. The two conceptions, Force and Matter, are co-existent and correlative. Force implies resistance; and the force is effective only when the resistance is called into play. If we grasp a stone, we have no hold of it till the closing of the hand is resisted by the solid texture of the stone. If we push open a gate, we must surmount the opposition which it exerts while turning on its hinges. However slight the resistance be, there must be some resistance, or there would be no force. If we imagine a state of things in which objects do not resist our touch, they must also cease to be influenced by our strength. Such a state of things we sometimes imagine in our dreams; and such are the poetical pictures of the regions inhabited by disembodied spirits. In these, the figures which appear are conspicuous to the eye, but impalpable like shadow or smoke; and as they do not resist the corporeal impressions, so neither do they obey them. The spectator tries in vain to strike or to grasp them.

Et ni docta comes tenues sine corpore vitas

Admoneat volitare cavâ sub imagine formæ,

Irruat ac frustra ferro diverberet umbras.

The Sibyl warns him that there round him fly

Bodiless things, but substance to the eye;

Else had he pierced those shapes with life-like face,

And smitten, fierce, the unresisting space.

Neque illum

Prensantem nequicquam umbras et multa volentem

Dicere, preterea vidit.

He grasps her form, and clutches but the shade.

Such may be the circumstances of the unreal world of dreams, or of poetical fancies approaching to dreams: for in these worlds our imaginary perceptions are bound by no rigid conditions of force and reaction. In 208 such cases, the mind casts off the empire of the idea of cause, as it casts off even the still more familiar sway of the

ideas of space and time. But the character of the material world in which we live when awake is, that we have at every instant and at every place, force operating on matter and matter resisting force.

3. *Solidity.*—From our consciousness of muscular exertion, we derive, as we have seen, the conception of force, and with that also the conception of matter. We have already shown, in a former chapter, that the same part of our frame, the muscular system, is the organ by which we perceive extension and the relations of space. Thus the same organ gives us the perception of body as resisting force, and as occupying space; and by combining these conditions we have the conception of *solid* extended bodies. In reality, this resistance is inevitably presented to our notice in the very facts from which we collect the notion of extension. For the action of the hand and arm by which we follow the forms of objects, implies that we apply our fingers to their surface; and we are stopped there by the resistance which the body offers. This

resistance is precisely that which is requisite in order to make us conscious of cur muscular effort[10]. Neither touch, nor any other mere passive sensation, could produce the perception of extent, as we have already urged: nor could the muscular sense lead to such a perception, except the extension of the muscles were felt to be resisted. And thus the perception of resistance enters the mind along with the perception of extended bodies. All the objects with which we have to do are not only extended but solid.

[10] Brown's *Lectures*, i. 466.

This sense of the term *solidity*, (the general property of all matter,) is different to that in which we oppose *solidity* to *fluidity*. We may avoid ambiguity by opposing *rigid* to *fluid* bodies. By solid bodies, as we now speak of them, we mean only such as resist the pressure which we exert, so long as their parts continue in their places. By fluid bodies, we mean those 209 whose parts are, by a slight pressure, removed out of their places. A drop of water ceases to prevent the contact of our two

hands, not by ceasing to have solidity in this sense, but by being thrust out of the way. If it could remain in its place, it could not cease to exercise its resistance to our pressure, except by ceasing to be matter altogether.

The perception of solidity, like the perception of extension, implies an act of the mind, as well as an impression of the senses: as the perception of extension implies the idea of space, so the perception of solidity implies the idea of action and reaction. That an Idea is involved in our knowledge on this subject, appears, as in other instances, from this consideration, that the convictions of persons, even of those who allow of no ground of knowledge but experience, do in fact go far beyond the possible limits of experience. Thus Locke says[11], that 'the bodies which we daily handle hinder by an *insurmountable* force the approach of the parts of our hands that press them.' Now it is manifest that our observation can never go to this length. By our senses we can only perceive that bodies

resist the greatest actual forces that we exert upon them. But our conception of force carries us further: and since, so long as the body is there to receive the action of the force, it must suffer the whole of that action, and must react as much as it suffers: it is therefore true, that so long as the body remains there, the force which is exerted upon it can never surmount the resistance which the body exercises. And thus this doctrine, that bodies resist the intrusion of other bodies by an insurmountable force, is, in fact, a consequence of the axiom that the reaction is always equal to the action.

[11] *Essay*, b. ii. c. 4.

4. *Inertia.*—But this principle of the equality of action and reaction appears also in another way. Not only when we exert force upon bodies at rest, but when, by our exertions, we put them in motion, they react. If we set a large stone in motion, the stone 210 resists; for the operation requires an effort. By increasing the effort, we can increase the effect, that is, the motion produced; but the resistance still remains.

And the greater the stone moved, the greater is the effort requisite to move it. There is, in every case, a resistance to motion, which shows itself, not in preventing the motion, but in a reciprocal force, exerted backwards upon the agent by which the motion is produced. And this resistance resides in each portion of matter, for it is increased as we add one portion of matter to another. We can push a light boat rapidly through the water; but we may go on increasing its freight, till we are barely able to stir it. This property of matter, then, by which it resists the reception of motion, or rather by which it reacts and requires an adequate force in order that any motion may result, is called its inertness, or *inertia*. That matter has such a property, is a conviction flowing from that idea of a reaction equal and opposite to the action, which the conception of all force involves. By what laws this inertia depends on the magnitude, form, and material of the body, must be the subject of our consideration hereafter. But that matter has this inertia, in virtue of which, as the matter is greater, the

velocity which the same effort can communicate to it is less, is a principle inseparable from the notion of matter itself.

Hermann says that Kepler first introduced this ‘most significant’ *inertia*. Whether it is to be found in earlier writers I know not; Kepler certainly does use it familiarly in those attempts to assign physical reasons for the motions of the planets which were among the main occasions of the discovery of the true laws of mechanics. He assumes the slowness of the motions of the planets to increase, (other causes remaining the same,) as the inertia increases; and though, even in this assumption, there is an errour involved, (if we adopt that interpretation of the term *inertia* to which subsequent researches led,) the introduction of such a word was one step in determining and expressing those laws of motion which depend on the fundamental principle of the equality of action and reaction. 211

5. We have thus seen, I trust in a satisfactory manner, the origin of our

conceptions of Force, Matter, Solidity, and Inertness. It has appeared that the organ by which we obtain such conceptions is that very muscular frame, which is the main instrument of our perceptions of space; but that, besides bodily sensations, these ideal conceptions, like all the others which we have hitherto considered, involve also an habitual activity of the mind, giving to our sensations a meaning which they could not otherwise possess. And among the ideas thus brought into play, is an idea of action with an equal and opposite reaction, which forms a foundation for universal truths to be hereafter established respecting the conceptions thus obtained.

We must now endeavour to trace in what manner these fundamental principles and conceptions are unfolded by means of observation and reasoning, till they become an extensive yet indisputable science.

CHAPTER VI.

Of the Establishment of the Principles of Statics.

1. *Object of the Chapter.*—In the present and the succeeding chapters we have to show how the general axioms of Causation enable us to construct the science of Mechanics. We have to consider these axioms as moulding themselves, in the first place, into certain fundamental mechanical principles, which are of evident and necessary truth in virtue of their dependence upon the general axioms of Causation; and thus as forming a foundation for the whole structure of the science;—a system of truths no less necessary than the fundamental principles, because derived from these by rigorous demonstration.

This account of the construction of the

science of Mechanics, however generally treated, cannot be otherwise than technical in its details, and will probably be imperfectly understood by any one not acquainted with Mechanics as a mathematical science.

I cannot omit this portion of my survey without rendering my work incomplete; but I may remark that the main purpose of it is to prove, in a more particular manner, what I have already declared in general, that there are, in Mechanics no less than in Geometry, fundamental principles of axiomatic evidence and necessity;—that these principles derive their axiomatic character from the Idea which they involve, namely, the Idea of Cause;—and that through the combination of principles of this kind, the whole science of Mechanics, including its most complex and remote results, exists as a body of solid and universal truths. 213

2. *Statics and Dynamics.*—We must first turn our attention to a technical distinction

of Mechanics into two portions, according as the forces about which we reason produce rest, or motion; the former portion is termed *Statics*, the latter *Dynamics*. If a stone fall, or a weight put a machine in motion, the problem belongs to Dynamics; but if the stone rest upon the ground, or a weight be merely supported by a machine, without being raised higher, the question is one of Statics.

3. *Equilibrium.*—In Statics, forces *balance* each other, or keep each other *in equilibrium*. And forces which directly balance each other, or keep each other in equilibrium, are necessarily and manifestly equal. If we see two boys pull at two ends of a rope so that neither of them in the smallest degree prevails over the other, we have a case in which two forces are in equilibrium. The two forces are evidently equal, and are a statical exemplification of action and reaction, such as are spoken of in the third axiom concerning causes. Now the same exemplification occurs in every case of equilibrium. No point or body can be kept

at rest except in virtue of opposing forces acting upon it; and these forces must always be equal in their opposite effect. When a stone lies on the floor, the weight of the stone downwards is opposed and balanced by an equal pressure of the floor upwards. If the stone rests on a slope, its tendency to slide is counteracted by some equal and opposite force, arising, it may be, from the resistance which the sloping ground opposes to any motion along its surface. Every case of rest is a case of equilibrium: every case of equilibrium is a case of equal and opposite forces.

The most complex frame-work on which weights are supported, as the roof of a building, or the cordage of a machine, are still examples of equilibrium. In such cases we may have many forces all combining to balance each other; and the equilibrium will depend on various conditions of direction and magnitude among the forces. And in order to understand what are these conditions, we must ask, in the first place, what 214 we understand by the magnitude

of such forces;—what is the measure of statical forces.

4. *Measure of Statical Forces.*—At first we might expect, perhaps, that since statical forces come under the general notion of Cause, the mode of measuring them would be derived from the second axiom of Causation, that causes are measured by their effects. But we find that the application of this axiom is controlled by the limitation which we noticed, after stating that axiom; namely, the condition that the causes shall be capable of addition. Further, as we have seen, a statical force produces no other effect than this, that it balances some other statical force; and hence the measure of statical forces is necessarily dependent upon their balancing, that is, upon the equality of action and reaction.

That *statical forces are capable of addition* is involved in our conception of such forces. When two men pull at a rope in the same direction, the forces which they exert are

added together. When two heavy bodies are put into a basket suspended by a string, their weights are added, and the sum is supported by the string.

Combining these considerations, it will appear that the measure of statical forces is necessarily given at once by the fundamental principle of the equality of action and reaction. Since two opposite forces which balance each other are equal, each force is measured by that which it balances; and since forces are capable of addition, a force of any magnitude is measured by adding together a proper number of such equal forces. Thus a heavy body which, appended to some certain elastic branch of a tree, would bend it down through one inch, may be taken as a unit of weight. Then if we remove this first body, and find a second heavy body which will also bend the branch through the same space, this is also a unit of weight; and in like manner we might go on to a third and a fourth equal body; and adding together the two, or the three, or the four heavy bodies,

we have a force twice, or three times, or four times the unit of weight. And with 215 such a collection of heavy bodies, or *weights*, we can readily measure all other forces; for the same principle of the equality of action and reaction leads at once to this maxim, that any statical force is measured by the weight which it would support.

As has been said, it might at first have been supposed that we should have to apply, in this case, the axiom that causes are measured by their effects in another manner; that thus, if that body were a unit of weight which bent the bough of a tree through one inch, *that* body would be *two* units which bent it through *two* inches, and so on. But, as we have already stated, the measures of weight must be subject to this condition, that they are susceptible of being added: and therefore we cannot take the deflexion of the bough for our measure, till we have ascertained, that which experience alone can teach us, that under the burden of two equal weights, the deflexion will be twice as great as it is with one weight,

which is not true, or at least is neither obviously nor necessarily true. In this, as in all other cases, although causes must be measured by their effects, we learn from experience only how the effects are to be interpreted, so as to give a true and consistent measure.

With regard, however, to the measure of statical force, and of weight, no difficulty really occurred to philosophers from the time when they first began to speculate on such subjects; for it was easily seen that if we take any uniform material, as wood, or stone, or iron, portions of this which are geometrically equal, must also be equal in statical effect; since this was implied in the very hypothesis of a uniform material And a body ten times as large as another of the same substance, will be of ten times the weight. But before men could establish by reasoning the conditions under which weights would be in equilibrium, some other principles were needed in addition to the mere measure of forces. The principles introduced for this purpose still resulted

from the conception of equal action and reaction; but it required no small clearness of thought to select them rightly, and to employ them 216 successfully. This, however, was done, to a certain extent, by the Greeks; and the treatise of Archimedes *On the Center of Gravity*, is founded on principles which may still be considered as the genuine basis of statical reasoning. I shall make a few remarks on the most important principle among those which Archimedes thus employs.

5. *The Center of Gravity.*—The most important of the principles which enter into the demonstration of Archimedes is this: that "Every body has a center of gravity;" meaning by the center of gravity, a point at which the whole matter of the body may be supposed to be collected, to all intents and purposes of statical reasoning. This principle has been put in various forms by succeeding writers: for instance, it has been thought sufficient to assume a case much simpler than the general one; and to assert that two *equal* bodies have their center of gravity in

the point midway between them. It is to be observed, that this assertion not only implies that the two bodies will *balance* upon a support placed at that midway point, but also, that they will exercise, upon such a support, a *pressure equal to their sum*; for this point being the center of gravity, the whole matter of the two bodies may be conceived to be collected there, and therefore the whole weight will press there. And thus the principle in question amounts to this, that *when two equal heavy bodies are supported on the middle point between them, the pressure upon the support is equal to the sum of the weights of the bodies*.

A clear understanding of the nature and grounds of this principle is of great consequence: for in it we have the foundation of a large portion of the science of Mechanics. And if this principle can be shown to be necessarily true, in virtue of our Fundamental Ideas, we can hardly doubt that there exist many other truths of the same kind, and that no sound view of the evidence and extent of human

knowledge can be obtained, so long as we mistake the nature of these, its first principles. 217

The above principle, that the pressure on the support is equal to the sum of the bodies supported, is often stated as an axiom in the outset of books on Mechanics. And this appears to be the true place and character of this principle, in accordance with the reasonings which we have already urged. The axiom depends upon our conception of action and reaction. That the two weights are supported, implies that the supporting force must be equal to the force or weight supported.

In order further to show the foundation of this principle, we may ask the question:—If it be not an axiom, deriving its truth from the fundamental conception of equal action and reaction, which equilibrium always implies, what is the origin of its certainty? The principle is never for an instant denied or questioned: it is taken for granted, even before it is stated. No one will doubt that it

is not only true, but true with the same rigour and universality as the axioms of Geometry. Will it be said, that it is borrowed from experience? Experience could never prove a principle to be universally and rigorously true. Moreover, when from experience we prove a proposition to possess great exactness and generality, we approach by degrees to this proof: the conviction becomes stronger, the truth more secure, as we accumulate trials. But nothing of this kind is the case in the instance before us. There is no gradation from less to greater certainty;—no hesitation which precedes confidence. From the first, we know that the axiom is exactly and certainly true. In order to be convinced of it, we do not require many trials, but merely a clear understanding of the assertion itself.

But in fact, not only are trials not necessary to the proof, but they do not strengthen it. Probably no one ever made a trial for the purpose of showing that the pressure upon the support is equal to the

sum of the two weights. Certainly no person with clear mechanical conceptions ever wanted such a trial to convince him of the truth; or thought the truth clearer after the trial had been made. If to such a person, an 218 experiment were shown which seemed to contradict the principle, his conclusion would be, not that the principle was doubtful, but that the apparatus was out of order. Nothing can be less like collecting truth from experience than this.

We maintain, then, that this equality of mechanical action and reaction, is one of the principles which do not flow from, but regulate our experience. To this principle, the facts which we observe must conform; and we cannot help interpreting them in such a manner that they shall be exemplifications of the principle. A mechanical pressure not accompanied by an equal and opposite pressure, can no more be given by experience, than two unequal right angles. With the supposition of such inequalities, space ceases to be space, force ceases to be force, matter ceases to be

matter. And this equality of action and reaction, considered in the case in which two bodies are connected so as to act on a single support, leads to the axiom which we have stated above, and which is one of the main foundations of the science of Mechanics.

[2d ed.] [To the doctrine that mechanical principles, such as the one here under consideration (that the pressure on the point of support is equal to the sum of the weights), are derived from our Ideas, and do not flow from but regulate our experience, objections are naturally made by those who assert all our knowledge to be derived from experience. How, they ask, can we know the properties of pressures, levers and the like, except from experience? What but experience can possibly inform us that a force applied transversely to a lever will have any tendency to turn the lever on its center? This cannot be, except we suppose in the lever tenacity, rigidity and the like, which are qualities known only by experience. And it is obvious that this line

of argument might be carried on through the whole subject.

My answer to this objection is a remark of the same kind as one which I have made respecting the Ideas of Space, Time, and Number, in the last Book. The mind, in apprehending events as causes 219 and effects, is governed by Laws of its own Activity; and these Laws govern the results of the mind's action; and make these results conform to the Axioms of Causation. But this activity of the mind is awakened and developed by being exercised; and in dealing with the examples of cause and effect here spoken of, (namely, pressure and resistance, force and motion,) the mind's activity is necessarily governed also by the bodily powers of perception and action. We are human beings only in so far as we have existed in space and time; and of our human faculties, developed by our existence in space and time, space and time are necessary conditions. In like manner, we are human beings only in so far as we have bodies, and bodily organs; and our bodies

necessarily imply material objects external to us. And hence our human faculties, developed by our bodily existence in a material world, have the conditions of matter for their necessary Laws. I have already said (chap. v.) that our conception of Force arises with our consciousness of our own muscular exertions;—that Force cannot be conceived without Resistance to exercise itself upon;—and that this resistance is supplied by Matter. And thus the conception of Matter, and of the most general modes in which Matter receives, resists, and transmits force, are parts of our constitution which, though awakened and unfolded by our being in a material world, are not distinguishable from the original structure of the mind. I do not ascribe to the mind *innate* Ideas—Ideas which it would have, even if it had no intercourse with the world of space, time, and matter; because we cannot imagine a mind in such a state. But I attempt to point out and classify those Conditions of all Experience, to which the intercourse of all minds with the material world has necessarily given rise in all.

Truths *thus* necessarily acquired in the course of all experience, cannot be said to be learnt *from experience*, in the same sense in which particular facts, at definite times, are learnt from experience—learnt by some persons and not by others—learnt with more or less of certainty. These latter *special truths of* 220 *experience* will be very important subjects of our consideration; but our whole chance of discussing them with any profit depends upon our keeping them distinct from the *necessary and universal conditions of experience*. Here, as everywhere, we must keep in view the fundamental antithesis of Ideas and Facts.]

6. *Oblique Forces.*—By the aid of the above axiom and a few others, the Greeks made some progress in the science of Statics. But after a short advance, they arrived at another difficulty, that of Oblique Forces, which they never overcame; and which no mathematician mastered till modern times. The unpublished manuscripts of Leonardo da Vinci, written in the fifteenth century, and the works of Stevinus

and Galileo, in the sixteenth, are the places in which we find the first solid grounds of reasoning on the subject of forces acting obliquely to each other. And from that period, mathematicians, having thus become possessed of all the mechanical principles which are requisite in problems respecting equilibrium, soon framed a complete science of Statics. Succeeding writers presented this science in forms variously modified; for it was found, in Mechanics as in Geometry, that various propositions might be taken as the starting points; and that the collection of truths which it was the mechanician's business to include in his course, might thus be traversed by various routes, each path offering a series of satisfactory demonstrations. The fundamental conceptions of force and resistance, like those of space and number, could be contemplated under different aspects, each of which might be made the basis of axioms, or of principles employed as axioms. Hence the grounds of the truth of Statics may be stated in various ways; and it would be a

task of some length to examine all these completely, and to trace them to their Fundamental Ideas. This I shall not undertake here to do; but the philosophical importance of the subject makes it proper to offer a few remarks on some of the main principles involved in the different modes of presenting Statics as a rigorously demonstrated science. 221

7. *A Force may be supposed to act at any Point of its Direction.*—It has been stated in the history of Mechanics[12], that Leonardo da Vinci and Galileo obtained the true measure of the effect of oblique forces, by reasonings which were, in substance, the same. The principle of these reasonings is that expressed at the head of this paragraph; and when we have a little accustomed ourselves to contemplate our conceptions of force, and its action on matter, in an abstract manner, we shall have no difficulty in assenting to the principle in this general form. But it may, perhaps, be more obvious at first in a special case.

[12] *Hist. Ind. Sc.* b. vi. c. i. sect. 2.

If we suppose a wheel, moveable about its axis, and carrying with it in its motion a weight, (as, for example, one of the wheels by means of which the large bells of a church are rung,) this weight may be supported by means of a rope (not passing along the circumference of the wheel, as is usual in the case of bells,) but fastened to one of the spokes of the wheel. Now the principle which is enunciated above asserts, that if the rope pass in a straight line across several of the spokes of the wheel, it makes no difference in the mechanical effect of the force applied, for the purpose of putting the bell in motion, to *which* of these spokes the rope is *fastened*. In each case, the fastening of the rope to the wheel merely serves to enable the force to produce motion about the center; and so long as the force acts in the same line, the effect is the same, at whatever point of the rope the line of action finishes.

This axiom very readily aids us in estimating the effect of oblique forces. For when a force acts on one of the arms of a

lever at any oblique angle, we suppose another arm projecting from the center of motion, like another spoke of the same wheel, so situated that it is perpendicular to the force. This arm we may, with Leonardo, call the *virtual lever*; for, by the axiom, we may suppose the force to act where the line of its direction meets this arm; and thus we reduce the case 222 to that in which the force acts perpendicularly on the arm.

The ground of this axiom is, that matter, in Statics, is necessarily conceived as *transmitting* force. That force can be transmitted from one place to another, by means of matter;—that we can push with a rod, pull with a rope,—are suppositions implied in our conceptions of force and matter. Matter is, as we have said, that which receives the impression of force, and the modes just mentioned, are the simplest ways in which that impression operates. And since, in any of these cases, the force might be resisted by a reaction equal to the force itself, the reaction in each case would be equal, and, therefore, the action in each

case is necessarily equal; and thus the forces must be transmitted, from one point to another, without increase or diminution.

This property of matter, of transmitting the action of force, is of various kinds. We have the coherence of a rope which enables us to pull, and the rigidity of a staff, which enables us to push with it in the direction of its length; and again, the same staff has a rigidity of another kind, in virtue of which we can use it as a lever; that is, a rigidity to resist flexure, and to transmit the force which turns a body round a fulcrum. There is, further, the rigidity by which a solid body resists *twisting*. Of these kinds of rigidity, the first is that to which our axiom refers; but in order to complete the list of the elementary principles of Statics, we ought also to lay down axioms respecting the other kinds of rigidity[13]. These, however, I shall not here state, as they do not involve any new principle. Like the one just considered, they form part of our fundamental conception of matter; they are not the results of any experience, but are

the hypotheses to which we are irresistibly led, when we would liberate our reasonings concerning force and matter from a dependence on the special results of experience. We cannot even 223 conceive (that is, if we have any clear mechanical conceptions at all) the force exerted by the point of a staff and resisting the force which we steadily impress on the head of it, to be different from the impressed force.

[13] Such axioms are given in a little work (*The Mechanical Euclid*) which I published on the Elements of Mechanics.

8. *Forces may have equivalent Forces substituted for them. The Parallelogram of Forces.*—It has already been observed, that in order to prove the doctrines of Statics, we may take various principles as our starting points, and may still find a course of demonstration by which the leading propositions belonging to the subject may be established. Thus, instead of beginning our reasonings, as in the last section we supposed them to commence, with the case in which forces act upon different points of

the same body in the same line of force, and counteract each other in virtue of the intervening matter by which the effect of force is transferred from one point to another; we may suppose different forces to act at the same point, and may thus commence our reasonings with a case in which we have to contemplate force, without having to take into our account the resistance or rigidity of matter. Two statical forces, thus acting at a mathematical point, are equivalent, in all respects, to some single force acting at the same point; and would be kept in equilibrium by a force equal and opposite to that single force. And the rule by which the single force is derived from the two, is commonly termed *the parallelogram of forces*; the proposition being this,—That if the two forces be represented in magnitude and direction by the two sides of a parallelogram, the resulting force will be represented in the same manner by the diagonal of the parallelogram. This proposition has very frequently been made, by modern writers, the commencement of the science of Mechanics: a position for

which, by its simplicity, it is well suited; although, in order to deduce from it the other elementary propositions of the science, as, for instance, those respecting the lever, we require the axiom stated in the last section.

9. *The Parallelogram of Forces is a necessary Truth.*—In the series of discussions in which we are 224 here engaged, our main business is to ascertain the nature and grounds of the certainty of scientific truths. We have, therefore, to ask whether this proposition, the parallelogram of forces, be a necessary truth; and if so, on what grounds its necessity ultimately rests. We shall find that this, like the other fundamental doctrines of Statics, justly claim a demonstrative certainty. Daniel Bernoulli, in 1726, gave the first proof of this important proposition on pure statical principles; and thus, as he says[14], 'proved that statical theorems are not less necessarily true than geometrical are.' If we examine this proof of Bernoulli, in order to discover what are the principles on which it

rests, we shall find that the reasoning employs in its progress such axioms as this; —That if from forces which are in equilibrium at a point be taken away other forces which are in equilibrium at the same point, the remainder will be in equilibrium; and generally;—That if forces can be resolved into other equivalent forces, these may be separated, grouped, and recombined, in any new manner, and the result will still be identical with what it was at first. Thus in Bernoulli's proof, the two forces to be compounded are represented by P and Q; P is resolved into two other forces, X and U; and Q into two others, Y and V, under certain conditions. It is then assumed that these forces may be grouped into the pairs X, Y, and U, V: and when it has been shown that X and Y are in equilibrium, they may, by what has been said, be removed, and the forces, P, Q, are equivalent to U, V; which, being in the same direction by the course of the construction, have a result equal to their sum.

[14] *Comm. Petrop.* vol. i.

It is clear that the principles here assumed are genuine axioms, depending upon our conception of the nature of equivalence of forces, and upon their being capable of addition and composition. If the forces, P, Q, be *equivalent* to forces X, U, Y, V, they are equivalent to these forces added and compounded in any order; just as a geometrical figure is, by our conception of 225 space, equivalent to its parts added together in any order. The apprehension of forces as having magnitude, as made up of parts, as capable of composition, leads to such axioms in Statics, in the same manner as the like apprehension of space leads to the axioms of Geometry. And thus the truths of Statics, resting upon such foundations, are independent of experience in the same manner in which geometrical truths are so.

The proof of the parallelogram of forces thus given by Daniel Bernoulli, as it was the first, is also one of the most simple proofs of that proposition which have been devised up to the present day. Many other demonstrations, however, have been given

of the same proposition. Jacobi, a German mathematician, has collected and examined eighteen of these[15]. They all depend either upon such principles as have just been stated; That forces may in every way be replaced by those which are equivalent to them;—or else upon those previously stated, the doctrine of the lever, and the transfer of a force from one point to another of its direction. In either case, they are necessary results of our statical conceptions, independent of any observed laws of motion, and indeed, of the conception of actual motion altogether.

[15] These are by the following mathematicians; D. Bernoulli (1726); Lambert (1771); Scarella (1756); Venini (1764); Araldi (1806); Wachter (1815); Kaestner; Marini; Eytelwein; Salimbeni; Duchayla; two different proofs by Foncenex (1760); three by D'Alembert; and those of Laplace and M. Poisson.

There is another class of alleged proofs of the parallelogram of forces, which involve the consideration of the motion produced by the forces. But such reasonings are, in fact, altogether irrelevant to the subject of

Statics. In that science, forces are not measured by the motion which they produce, but by the forces which they will balance, as we have already seen. The combination of two forces employed in producing motion in the same body, either simultaneously or successively, 226 belongs to that part of Mechanics which has motion for its subject, and is to be considered in treating of the laws of motion. The composition of motion, (as when a man moves in a ship while the ship moves through the water,) has constantly been confounded with the composition of force. But though it has been done by very eminent mathematicians, it is quite necessary for us to keep the two subjects distinct, in order to see the real nature of the evidence of truth in either case. The conditions of equilibrium of two forces on a lever, or of three forces at a point, can be established without any reference whatever to any motions which the forces might, under *other* circumstances, produce. And because this can be done, to do so is the only scientific procedure. To prove such

propositions by any other course, would be to support truth by extraneous and inconclusive reasons; which would be foreign to our purpose, since we seek not only knowledge, but the grounds of our knowledge.

10. *The Center of gravity seeks the lowest place.*—The principles which we have already mentioned afford a sufficient basis for the science of Statics in its most extensive and varied applications; and the conditions of equilibrium of the most complex combinations of machinery may be deduced from these principles with a rigour not inferior to that of geometry. But in some of the more complex cases, the results of long trains of reasoning may be foreseen, in virtue of certain maxims which appear to us self-evident, although it may not be easy to trace the exact dependence of these maxims upon our fundamental conceptions of force and matter. Of this nature is the maxim now stated;—That in any combination of matter any how supported, the Center of Gravity will descend into the lowest position which

the connexion of the parts allows it to assume by descending. It is easily seen that this maxim carries to a much greater extent the principle which the Greek mathematicians assumed, that every body has a Center of Gravity, that is, a point in which, if the whole matter of the body be collected, the effect will remain unchanged. For the Greeks asserted this of a 227 single rigid mass only; whereas, in the maxim now under our notice, it is asserted of any masses, connected by strings, rods, joints, or in any manner. We have already seen that more modern writers on mechanics, desirous of assuming as fundamental no wider principles than are absolutely necessary, have not adopted the Greek axiom in all its generality, but have only asserted that two *equal* weights have a center of gravity midway between them. Yet the principle that every body, however irregular, has a center of gravity, and will be supported if that center is supported, and not otherwise, is so far evident, that it might be employed as a fundamental truth, if we could not resolve it into any simpler

truths: and, historically speaking, it was assumed as evident by the Greeks. In like manner the still wider principle, that a collection of bodies, as, for instance, a flexible chain hanging upon one or more supports, has a center of gravity; and that this point will descend to the lowest possible situation, as a single body would do, has been adopted at various periods in the history of mechanics; and especially at conjunctures when mathematical philosophers have had new and difficult problems to contend with. For in almost every instance it has only been by repeated struggles that philosophers have reduced the solution of such problems to a clear dependence upon the most simple axioms.

11. *Stevinus's Proof for Oblique Forces.*— We have an example of this mode of dealing with problems, in Stevinus's mode of reasoning concerning the Inclined Plane; which, as we have stated in the History of Mechanics, was the first correct published solution of that problem. Stevinus supposes a loop of chain, or a loop of string loaded

with a series of equal balls at equal distances, to hang over the Inclined Plane; and his reasoning proceeds upon this assumption,—That such a loop so hanging will find a certain position in which it will rest: for otherwise, says he[16], its motion must go on for ever, which is absurd. It may be asked how 228 this absurdity of a perpetual motion appears; and it will perhaps be added, that although the impossibility of a machine with such a condition may be proved as a remote result of mechanical principles, this impossibility can hardly be itself recognized as a self-evident truth. But to this we may reply, that the impossibility is really evident in the case contemplated by Stevinus; for we cannot conceive a loop of chain to go on through all eternity, sliding round and round upon its support, by the effect of its own weight. And the ground of our conviction that this cannot be, seems to be this consideration; that when the chain moves by the effect of its weight, we consider its motion as the result of an effort to reach some certain position, in which it can rest; just as a single

ball in a bowl moves till it comes to rest at the lowest point of the bowl. Such an effect of weight in the chain, we may represent to ourselves by conceiving all the matter of the chain to be collected in one single point, and this single heavy point to hang from the support in some way or other, so as fitly to represent the mode of support of the chain. In whatever manner this heavy point (the center of gravity of the chain) be supported and controlled in its movements, there will still be some position of rest which it will seek and find. And thus there will be some corresponding position of rest for the chain; and the interminable shifting from one position to another, with no disposition to rest in any position, cannot exist.

[16] Stevin. *Statique*, livre i. prop. 19.

Thus the demonstration of the property of the Inclined Plane by Stevinus, depends upon a principle which, though far from being the simplest of those to which the case can be reduced, is still both true and evident: and the evidence of this principle, depending upon the assumption of a center

of gravity, is of the same nature as the evidence of the Greek statical demonstrations, the earliest real advances in the science.

12. *Principle of Virtual Velocities.*—We have referred above to an assertion often made, that we may, from the simple principles of Mechanics, demonstrate the impossibility of a perpetual motion. In reality, 229 however, the simplest proof of that impossibility, in a machine acted upon by weight only, arises from the very maxim above stated, that the center of gravity seeks and finds the lowest place; or from some similar proposition. For if, as is done by many writers, we profess to prove the impossibility of a perpetual motion by means of that proposition which includes the conditions of equilibrium, and is called the *Principle of Virtual Velocities*[17], we are under the necessity of first proving in a general manner that principle. And if this be done by a mere enumeration of cases, (as by taking those five cases which are called the *Mechanical Powers*,) there may remain some

doubts whether the enumeration of possible mechanical combinations be complete. Accordingly, some writers have attempted independent and general proofs of the Principle of Virtual Velocities; and these proofs rest upon assumptions of the same nature as that now under notice. This is, for example, the case with Lagrange's proof, which depends upon what he calls the *Principle of Pulleys*. For this principle is,—That a weight any how supported, as by a string passing round any number of pulleys any how placed, will be at rest then only, when it cannot get lower by any small motion of the pulleys. And thus the maxim that a weight will descend if it can, is assumed as the basis of this proof.

[17] See *Hist. Ind Sc.* b. vi. c. ii. sect. 4.

There is, as we have said, no need to assume such principles as these for the foundation of our mechanical science. But it is, on various accounts, useful to direct our attention to those cases in which truths, apprehended at first in a complex and derivative form, have afterwards been

reduced to their simpler elements;—in which, also, sagacious and inventive men have fixed upon those truths as self-evident, which now appear to us only certain in virtue of demonstration. In these cases we can hardly doubt that such men were led to assert the doctrines which they discovered, not by any capricious conjecture of arbitrary selection, but by having a keener and deeper insight than other persons 230 into the relations which were the object of their contemplation; and in the science now spoken of, they were led to their assumptions by possessing clearly and distinctly the conceptions of mechanical cause and effect,—action and reaction,—force, and the nature of its operation.

13. *Fluids press Equally in all Directions.*—The doctrines which concern the equilibrium of fluids depend on principles no less certain and simple than those which refer to the equilibrium of solid bodies; and the Greeks, who, as we have seen, obtained a clear view of some of the principles of Statics, also made a beginning in the

kindred subject of Hydrostatics. We still possess a treatise of Archimedes *On Floating Bodies*, which contains correct solutions of several problems belonging to this subject, and of some which are by no means easy. In this treatise, the fundamental assumption is of this kind: 'Let it be assumed that the nature of a fluid is such, that the parts which are less pressed yield to those which are more pressed.' In this assumption or axiom it is implied that a pressure exerted upon a fluid in one direction produces a pressure in another direction; thus, the weight of the fluid which arises from a downward force produces a lateral pressure against the sides of the containing vessel. Not only does the pressure thus diverge from its original direction into all other directions, but the pressure is in all directions exactly equal, an equal extent of the fluid being taken. This principle, which was involved in the reasoning of Archimedes, is still to the present day the basis of all hydrostatical treatises, and is expressed, as above, by saying that *fluids press equally in all directions*.

Concerning this, as concerning previously-noticed principles, we have to ask whether it can rightly be said to be derived from experience. And to this the answer must still be, as in the former cases, that the proposition is not one borrowed from experience in any usual or exact sense of the phrase. I will endeavour to illustrate this. There are many elementary propositions in physics, our knowledge of which 231 indisputably depends upon experience; and in these cases there is no difficulty in seeing the evidence of this dependence. In such cases, the *experiments* which prove the law are prominently stated in treatises upon the subject: they are given with exact measures, and with an account of the means by which errours were avoided: the experiments of more recent times have either rendered more certain the law originally asserted, or have pointed out some correction of it as requisite: and the names, both of the discoverers of the law and of its subsequent reformers, are well known. For instance, the proposition that ‘The elastic force of air varies as the

density,' was first proved by Boyle, by means of operations of which the detail is given in his *Defence* of his *Pneumatical Experiments*[18]; and by Mariotte in his *Traité de l'Équilibre des Liquides*, from whom it has generally been termed Mariotte's law. After being confirmed by many other experimenters, this law was suspected to be slightly inaccurate, and a commission of the French Academy of Sciences was appointed, consisting of several distinguished philosophers[19], to ascertain the truth or falsehood of this suspicion. The result of their investigations appeared to be, that the law is exact, as nearly as the inevitable inaccuracies of machinery and measures will allow us to judge. Here we have an example of a law which is of the simplest kind and form; and which yet is not allowed to rest upon its simplicity or apparent probability, but is rigorously tested by experience. In this case, the assertion, that the law depends upon experience, contains a reference to plain and notorious passages in the history of science.

[18] Shaw's *Boyle*, Vol. ii. p. 671.

[19] The members were Prony, Arago, Ampère, Girard, and Dulong. The experiments were extended to a pressure of twenty-seven atmospheres; and in no instance did the difference between the observed and calculated elasticity amount to one-hundredth of the whole; nor did the difference appear to increase with the increase of pressure.—Fechner, *Repertorium*, i. 110.

Now with regard to the principle that fluids press equally in all directions, the case is altogether different. 232 It is, indeed, often asserted in works on hydrostatics, that the principle is collected from experience, and sometimes a few experiments are described as exhibiting its effect; but these are such as to illustrate and explain, rather than to prove, the truth of the principle: they are never related to have been made with that exactness of precaution and measurement, or that frequency of repetition, which are necessary to establish a purely experimental truth. Nor did such experiments occur as important steps in the history of science. It does not appear that Archimedes thought experiment necessary

to confirm the truth of the law as he employed it: on the contrary, he states it in exactly the same shape as the axioms which he employs in statics, and even in geometry; namely, as an assumption. Nor does any intelligent student of the subject find any difficulty in assenting to this fundamental principle of hydrostatics as soon as it is propounded to him. Experiment was not requisite for its discovery; experiment is not necessary for its proof at present; and we may add, that experiment, though it may make the proposition the more readily intelligible, can add nothing to our conviction of its truth when it is once understood.

14. *Foundation of the above Axiom.*—But it will naturally be asked, What then is the ground of our conviction of this doctrine of the equal pressure of a fluid in all directions? And to this I reply, that the reasons of this conviction are involved in our idea of a fluid, which is considered as matter, and therefore as capable of receiving, resisting, and transmitting force

according to the general conception of matter; and which is also considered as matter which has its parts perfectly moveable among one another. For it follows from these suppositions, that if the fluid be confined, a pressure which thrusts in one side of the containing vessel, may cause any other side to bulge outwards, if there be a part of the surface which has not strength to resist this pressure from within. And that this pressure, when thus transferred into a direction different from the original one, is not altered in intensity, 233 depends upon this consideration; that any difference in the two pressures would be considered as a defect of *perfect* fluidity, since the fluidity would be still more complete, if this entire and undiminished transmission of pressure in all directions were supposed. If, for instance, the lateral pressure were less than the vertical, this could be conceived no other way than as indicating some rigidity or adhesion of the parts of the fluid. When the fluidity is perfect, the two pressures which act in the two different parts of the fluid exactly balance each other: they are

the action and the reaction; and must hence be equal by the same necessity as two directly opposite forces in statics.

But it may be urged, that even if we grant that this conception of a perfect fluid, as a body which has its parts perfectly moveable among each other, leads us necessarily to the principle of the equality of hydrostatic pressure in all directions, still this conception itself is obtained from experience, or suggested by observation. And to this we may reply, that the conception of a fluid, as contemplated in mechanical theory, cannot be said to be derived from experience, except in the same manner as the conception of a solid and rigid body may be said to be acquired by experience. For if we imagine a vessel full of small, smooth spherical balls, such a collection of balls would approach to the nature of a fluid, in having its parts moveable among each other; and would approach to perfect fluidity, as the balls became smoother and smaller. And such a collection of balls would also possess the

statical properties of a fluid; for it would transmit pressure out of a vertical into a lateral (or any other) direction, in the same manner as a fluid would do. And thus a collection of solid bodies has the same property which a fluid has; and the science of Hydrostatics borrows from experience no principles beyond those which are involved in the science of Statics respecting solids. And since in this latter portion of science, as we have already seen, none of the principles depend for their evidence upon any special experience, the doctrines of Hydrostatics also are not 234 proved by experience, but have a necessary truth borrowed from the relations of our ideas.

It is hardly to be expected that the above reasoning will, at first sight, produce conviction in the mind of the reader, except he have, to a certain extent, acquainted himself with the elementary doctrines of the science of Hydrostatics as usually delivered; and have followed, with clear and steady apprehension, some of the trains of reasoning by which the pressures of fluids

are determined; as, for instance, the explanation of what is called *the Hydrostatic Paradox*. The necessity of such a discipline in order that the reader may enter fully into this part of our speculations, naturally renders them less popular; but this disadvantage is inevitable in our plan. We cannot expect to throw light upon philosophy by means of the advances which have been made in the mathematical and physical sciences, except we really understand the doctrines which have been firmly established in those sciences. This preparation for philosophizing may be somewhat laborious; but such labour is necessary if we would pursue speculative truth with all the advantages which the present condition of human knowledge places within our reach.

We may add, that the consequences to which we are directed by the preceding opinions, are of very great importance in their bearing upon our general views respecting human knowledge. I trust to be able to show, that some important

distinctions are illustrated, some perplexing paradoxes solved, and some large anticipations of the future extension of our knowledge suggested, by means of the conclusions to which the preceding discussions have conducted us. But before I proceed to these general topics, I must consider the foundations of some of the remaining portions of the science of Mechanics.

CHAPTER VII.

Of the Establishment of the Principles of Dynamics.

1. In the History of Mechanics, I have traced the steps by which the three Laws of Motion and the other principles of mechanics were discovered, established, and extended to the widest generality of form and application. We have, in these laws, examples of principles which were, historically speaking, obtained by reference to experience. Bearing in mind the object and the result of the preceding discussions, we cannot but turn with much interest to examine these portions of science; to inquire whether there be any real difference in the grounds and nature between the knowledge thus obtained, and those truths which we have already contemplated; and which, as

we have seen, contain their own evidence, and do not require proof from experiment.

2. *The First Law of Motion.*—The first law of motion is, that *When a body moves not acted upon by any force, it will go on perpetually in a straight line, and with a uniform velocity*. Now what is the real ground of our assent to this proposition? That it is not at first sight a self-evident truth, appears to be clear; since from the time of Aristotle to that of Galileo the opposite assertion was held to be true; and it was believed that all bodies in motion had, by their own nature, a constant tendency to move more and more slowly, so as to stop at last. This belief, indeed, is probably even now entertained by most persons, till their attention is fixed upon the arguments by which the first law of motion is established. It is, however, not difficult to lead any person of a speculative habit 236 of thought to see that the retardation which constantly takes place in the motion of all bodies when left to themselves, is, in reality, the effect of extraneous forces which

destroy the velocity. A top ceases to spin because the friction against the ground and the resistance of the air gradually diminish its motion, and not because its motion has any internal principle of decay or fatigue. This may be shown, and was, in fact, shown by Hooke before the Royal Society, at the time when the laws of motion were still under discussion, by means of experiments in which the weight of the top is increased, and the resistance to motion offered by its support, is diminished; for by such contrivances its motion is made to continue much longer than it would otherwise do. And by experiments of this nature, although we can never remove the whole of the external impediments to continued motion, and although, consequently, there will always be some retardation; and an end of the motion of a body left to itself, however long it may be delayed, must at last come; yet we can establish a conviction that if all resistance could be removed, there would be no diminution of velocity, and thus the motion would go on for ever.

If we call to mind the axioms which we formerly stated, as containing the most important conditions involved in the idea of Cause, it will be seen that our conviction in this case depends upon the first axiom of Causation, that nothing can happen without a cause. Every change in the velocity of the moving body must have a cause; and if the change can, in any manner, be referred to the presence of other bodies, these are said to exert *force* upon the moving body: and the conception of force is thus evolved from the general idea of cause. *Force is any cause which has motion, or change of motion, for its effect*; and thus, all the change of velocity of a body which can be referred to extraneous bodies,—as the air which surrounds it, or the support on which it rests,—is considered as the effect of forces; and this consideration is looked upon as explaining the difference between the motion which really takes places in the experiment, and that motion 237 which, as the law asserts, would take place if the body were not acted on by any forces.

Thus the truth of the first law of motion depends upon the axiom that no change can take place without a cause; and follows from the definition of force, if we suppose that there can be none but an *external* cause of change. But in order to establish the law, it was necessary further to be assured that there is no *internal* cause of change of velocity belonging to all matter whatever, and operating in such a manner that the mere progress of time is sufficient to produce a diminution of velocity in all moving bodies. It appears from the history of mechanical science, that this latter step required a reference to observation and experiment; and that the first law of motion is so far, historically at least, dependent upon our experience.

But notwithstanding this historical evidence of the need which we have of a reference to observed facts, in order to place this first law of motion out of doubt, it has been maintained by very eminent mathematicians and philosophers, that the law is, in truth, evident of itself, and does

not really rest upon experimental proof. Such, for example, is the opinion of d'Alembert[20], who offers what is called an *à priori* proof of this law; that is, a demonstration derived from our ideas alone. When a body is put in motion, either, he says, the cause which puts it in motion at first, suffices to make it move one foot, or the continued action of the cause during this foot is requisite for the motion. In the first case, the same reason which made the body proceed to the end of the first foot will hold for its going on through a second, a third, a fourth foot, and so on for any number. In the second case, the same reason which made the force continue to act during the first foot, will hold for its acting, and therefore for the body moving during each succeeding foot. And thus the body, once beginning to move, must go on moving for ever.

[20] *Dynamique.*

238 It is obvious that we might reply to this argument, that the reasons for the body proceeding during each succeeding foot may

not necessarily be all the same; for among these reasons may be the time which has elapsed; and thus the velocity may undergo a change as the time proceeds: and we require observation to inform us that it does not do so.

Professor Playfair has presented nearly the same argument, although in a different and more mathematical form[21]. If the velocity change, says he, it must change according to some expression of calculation depending upon the time, or, in mathematical language, must be a *function* of the time. If the velocity diminish as the time increases, this may be expressed by stating the velocity in each case as a certain number, from which another quantity, or *term*, increasing as the time increases, is subtracted. But, Playfair adds, there is no condition involved in the nature of the case, by which the *coefficients*, or numbers which are to be employed, along with the number representing the time, in calculating this second term, can be determined to be of one magnitude rather than of any other.

Therefore he infers there can be no such coefficients, and that the velocity is in each case equal to some constant number, independent of the time; and is therefore the same for all times.

[21] *Outlines of Natural Philosophy*, p. 26.

In reply to this we may observe, that the circumstance of *our not seeing* in the nature of the case anything which determines for us the coefficients above spoken of, cannot prove that they have not some certain value *in nature*. We do not see in the nature of the case anything which should determine a body to fall sixteen feet in a second of time, rather than one foot or one hundred feet: yet in fact the space thus run through by falling bodies is determined to a certain magnitude. It would be easy to assign a mathematical expression for the velocity of a body, implying that one-hundredth of the velocity, or any other 239 fraction, is lost in each second[22]: and where is the absurdity of supposing such an expression really to represent the velocity?

[22] This would be the case, if, t being the number of seconds elapsed, and C some constant quantity, the velocity were expressed by this mathematical formula, $C(^{99}/_{100})^t$.

Most modern writers on mechanics have embraced the opposite opinion, and have ascribed our knowledge of this first law of motion to experience. Thus M. Poisson, one of the most eminent of the mathematicians who have written on this subject, says[23], "We cannot affirm *à priori* that the velocity communicated to a body will not become slower and slower of itself, and end by being entirely extinguished. It is only by experience and induction that this question can be decided."

[23] Poisson, *Dynamique*, ed. 2, art. 113.

Yet it cannot be denied that there is much force in those arguments by which it is attempted to show that the First Law of Motion, such as we find it, is more consonant to our conceptions than any other would be. The Law, as it exists, is the most simple that we can conceive. Instead

of having to determine by experiments what is the law of the natural change of velocity, we find the Law to be that it does not change at all. To a certain extent, the Law depends upon the evident axiom, that no change can take place without a cause. But the question further occurs, whether the mere lapse of time may not be a cause of change of velocity. In order to ensure this, we have recourse to experiment; and the result is that time alone does not produce any such change. In addition to the conditions of change which we collect from our own Ideas, we ask of Experience what other conditions and circumstances she has to offer; and the answer is, that she can point out none; When we have removed the alterations which external causes, in our very conception of them, occasion, there are no longer any alterations. Instead of having to guide ourselves 240 by experience, we learn that on this subject she has nothing to tell us. Instead of having to take into account a number of circumstances, we find that we have only to reject all circumstances. The velocity of a body

remains unaltered by time alone, of whatever kind the body itself be.

But the doctrine that time alone is not a cause of change of velocity in any body is further recommended to us by this consideration;—that time is conceived by us not as a cause, but only as a condition of other causes producing their effects. Causes operate in time; but it is only when the cause exists, that the lapse of time can give rise to alterations. When therefore all external causes of change of velocity are supposed to be removed, the velocity must continue identical with itself, whatever the time which elapses. An eternity of negation can produce no positive result.

Thus, though the discovery of the First Law of Motion was made, historically speaking, by means of experiment, we have now attained a point of view in which we see that it might have been certainly known to be true independently of experience. This law in its ultimate form, when completely simplified and steadily contemplated,

assumes the character of a self-evident truth. We shall find the same process to take place in other instances. And this feature in the progress of science will hereafter be found to suggest very important views with regard both to the nature and prospects of our knowledge.

3. *Gravity is a Uniform Force.*—We shall find observations of the same kind offering themselves in a manner more or less obvious, with regard to the other principles of Dynamics. The determination of the laws according to which bodies fall downwards by the common action of gravity, has already been noticed in the History of Mechanics[24], as one of the earliest positive advances in the doctrine of motion. These laws were first rightly stated by Galileo, and 241 established by reasoning and by experiment, not without dissent and controversy. The amount of these doctrines is this: That gravity is a uniform accelerating force; such a *uniform force* having this for its character, that it *makes the velocity increase in exact proportion to the*

time of motion. The relation which the spaces described by the body bear to the times in which they are described, is obtained by mathematical deduction from this definition of the force.

[24] *Hist. Ind. Sc.* b. vi. c. ii. sect. 2.

The clear Definition of a uniform accelerating force, and the Proposition that gravity is such a force, were co-ordinate and contemporary steps in this discovery. In defining accelerating force, reference, tacit or express, was necessarily made to the second of the general axioms respecting causation,—That causes are measured by their effects. Force, in the cases now under our notice, is conceived to be, as we have already stated, (p. 236,) any cause which, acting from without, changes the motion of a body. It must, therefore, in this acceptation, be measured by the magnitude of the changes which are produced. But in what manner the changes of motion are to be employed as the measures of force, is learnt from observation of the facts which we see taking place in the world. Experience

interprets the axiom of causation, from which otherwise we could not deduce any real knowledge. We may assume, in virtue of our general conceptions of force, that under the same circumstances, a greater change of motion implies a greater force producing it; but what are we to expect when the circumstances change? The weight of a body makes it fall from rest at first, and causes it to move more quickly as it descends lower. We may express this by saying, that gravity, the universal force which makes all terrestrial bodies fall when not supported, by its continuous action first *gives* velocity to the body when it has none, and afterwards *adds* velocity to that which the body already has. But how is the velocity added proportioned to the velocity which already exists? Force acting on a body at rest, and on a body in motion, appears under very different 242 conditions; —how are the effects related? Let the force be conceived to be in both cases the same, since force is conceived to depend upon the extraneous bodies, and not upon the condition of the moving mass itself. But the

force being the same, the effects may still be different. It is at first sight conceivable that the body, acted upon by the same gravity, may receive a less addition of velocity when it is already moving in the direction in which this gravity impels it; for if we ourselves push a body forwards, we can produce little additional effect upon it when it is already moving rapidly away from us. May it not be true, in like manner, that although gravity be always the same force, its effect depends upon the velocity which the body under its influence already possesses?

Observation and reasoning combined, as we have said, enabled Galileo to answer these questions. He asserted and proved that we may consistently and properly measure a force by the velocity which is by it generated in a body, in some certain time, as one second; and further, that if we adopt this measure, gravity will be a force of the same value under all circumstances of the body which it affects; since it appeared that, in fact, a falling body does receive equal

increments of velocity in equal times from first to last.

If it be asked whether we could have known, anterior to, or independent of, experiment, that gravity is a uniform force in the sense thus imposed upon the term; it appears clear that we must reply, that we could not have attained to such knowledge, since other laws of the motion of bodies downwards are easily conceivable, and nothing but observation could inform us that one of these laws does not prevail in fact. Indeed, we may add, that the assertion that the force of gravity is uniform, is so far from being self-evident, that it is not even true; for gravity varies according to the distance from the center of the earth; and although this variation is so small as to be, in the case of falling bodies, imperceptible, it negatives the rigorous uniformity of the force as completely, though 243 not to the same extent, as if the weight of a body diminished in a marked degree, when it was carried from the lower to the upper room of a house. It cannot, then, be a truth

independent of experience, that gravity is uniform.

Yet, in fact, the assertion that gravity is uniform was assented to, not only before it was proved, but even before it was clearly understood. It was readily granted by all, that bodies which fall freely are *uniformly* accelerated; but while some held the opinion just stated, that uniformly accelerated motion is that in which the velocity increases in proportion to the *time*, others maintained, that *that* is uniformly accelerated motion, in which the velocity increases in proportion to the *space*; so that, for example, a body in falling vertically through twenty feet should acquire twice as great a velocity as one which falls through ten feet.

These two opinions are both put forward by the interlocutors of Galileo's Dialogue on this subject[25]. And the latter supposition is rejected, the author showing, not that it is inconsistent with experience, but that it is impossible in itself: inasmuch as it would

inevitably lead to the conclusion, that the fall through a large and a small vertical space would occupy exactly the same time.

[25] *Dialogo*, iii. p. 95.

Indeed, Galileo assumes his definition of uniformly accelerated motion as one which is sufficiently recommended by its own simplicity. ‘If we attend carefully,’ he says, ‘we shall find that no mode of increase of velocity is more simple than that which adds equal increments in equal times. Which we may easily understand if we consider the close affinity of time and motion: for as the uniformity of motion is defined by the equality of spaces described in equal times, so we may conceive the uniformity of acceleration to exist when equal velocities are added in equal times.’

Galileo’s mode of supporting his opinion, that bodies falling by the action of gravity are thus uniformly 244 accelerated, consists, in the first place, in adducing the maxim that nature always employs the most simple means[26]. But he is far from considering this

a decisive argument. ‘I,’ says one of his speakers, ‘as it would be very unreasonable in me to gainsay this or any other definition which any author may please to make, since they are all arbitrary, may still, without offence, doubt whether such a definition, conceived and admitted in the abstract, fits, agrees, and is verified in that kind of accelerated motion which bodies have when they descend naturally.’

[26] *Dialogo*, iii. p. 91.

The experimental proof that bodies, when they fall downwards, are uniformly accelerated, is (by Galileo) derived from the inclined plane; and therefore assumes the proposition, that if such uniform acceleration prevail in vertical motion, it will also hold when a body is compelled to describe an oblique rectilinear path. This proposition may be shown to be true, if (assuming by anticipation the Third Law of Motion, of which we shall shortly have to speak,) we introduce the conception of a uniform statical force as the cause of uniform acceleration. For the force on the

inclined plane bears a constant proportion to the vertical force, and this proportion is known from statical considerations. But in the work of which we are speaking, Galileo does not introduce this abstract conception of force as the foundation of his doctrines. Instead of this, he proposes, as a postulate sufficiently evident to be made the basis of his reasonings, That bodies which descend down inclined planes of different inclinations, but of the same vertical height, all acquire the same velocity[27]. But when this postulate has been propounded by one of the persons of the dialogue, another interlocutor says, 'You discourse very probably; but besides this likelihood, I wish to augment the probability so far, that it shall be almost as complete as a necessary demonstration.' He then proceeds to describe a very ingenious and simple experiment, which shows that when a body is made to swing upwards at the end of 245 a string, it attains to the same height, whatever is the path it follows, so long as it starts from the lowest point with the same velocity. And thus Galileo's postulate is

experimentally confirmed, so far as the force of gravity can be taken as an example of the forces which the postulate contemplates: and conversely, gravity is proved to be a uniform force, so far as it can be considered clear that the postulate is true of uniform forces.

[27] *Dialogo*, iii. p. 36.

When we have introduced the conception and definition of accelerating force, Galileo's postulate, that bodies descending down inclined planes of the same vertical height, acquire the same velocity, may, by a few steps of reasoning, be demonstrated to be true of uniform forces: and thus the proof that gravity, either in vertical or oblique motion, is a uniform force, is confirmed by the experiment above mentioned; as it also is, on like grounds, by many other experiments, made upon inclined planes and pendulums.

Thus the propriety of Galileo's conception of a uniform force, and the doctrine that gravity is a uniform force, were confirmed

by the same reasonings and experiments. We may make here two remarks; *First*, that the conception, when established and rightly stated, appears so simple as hardly to require experimental proof; a remark which we have already made with regard to the First Law of Motion: and *Second*, that the discovery of the real law of nature was made by assuming propositions which, without further proof, we should consider as very precarious, and as far less obvious, as well as less evident, than the law of nature in its simple form.

4. *The Second Law of Motion.*—When a body, instead of falling downwards from rest, is thrown in any direction, it describes a curve line, till its motion is stopped. In this, and in all other cases in which a body describes a curved path in free space, its motion is determined by the Second Law of Motion. The law, in its general form, is as follows:—When a body is thus cast forth and acted upon by a force in a direction 246 transverse to its motion, the result is, That *there is combined with the motion with which*

the body is thrown, another motion, exactly the same as that which the same force would have communicated to a body at rest.

It will readily be understood that the basis of this law is the axiom already stated, that effects are measured by their causes. In virtue of this axiom, the effect of gravity acting upon a body in a direction transverse to its motion, must measure the accelerative or deflective force of gravity under those circumstances. If this effect vary with the varying velocity and direction of the body thus acted upon, the deflective force of gravity also will vary with those circumstances. The more simple supposition is, that the deflective force of gravity is the same, whatever be the velocity and direction of the body which is subjected to its influence: and this is the supposition which we find to be verified by facts. For example, a ball let fall from the top of a ship's upright mast, when she is sailing steadily forward, will fall at the foot of the mast, just as if it were let fall while the ship were at rest; thus showing that the motion

which gravity gives to the ball is compounded with the horizontal motion which the ball shares with the ship from the first. This general and simple conception of motions as *compounded* with one another, represents, it is proved, the manner in which the motion produced by gravity modifies any other motion which the body may previously have had.

The discussions which terminated in the general reception of this Second Law of Motion among mechanical writers, were much mixed up with the arguments for and against the Copernican system, which system represented the earth as revolving upon its axis. For the obvious argument against this system was, that if each point of the earth's surface were thus in motion from west to east, a stone dropt from the top of a tower would be left behind, the tower moving away from it: and the answer was, that by this law of motion, the stone would have the earth's motion impressed upon it, as well as that motion which would 247 arise from its gravity to the earth; and that

the motion of the stone relative to the tower would thus be the same as if both earth and tower were at rest. Galileo further urged, as a presumption in favour of the opinion that the two motions,—the circular motion arising from the rotation of the earth, and the downward motion arising from the gravity of the stone, would be compounded in the way we have described, (neither of them disturbing or diminishing the other,) —that the first motion was in its own nature not liable to any change or diminution[28], as we learn from the First Law of Motion. Nor was the subject lightly dismissed. The experiment of the stone let fall from the top of the mast was made in various forms by Gassendi; and in his Epistle, *De Motu impresso a Motore translato*, the rule now in question is supported by reference to these experiments. In this manner, the general truth, the Second Law of Motion, was established completely and beyond dispute.

[28] *Dialogo*, ii. p. 114.

But when this law had been proved to be true in a general sense, with such accuracy

as rude experiments, like those of Galileo and Gassendi, would admit, it still remained to be ascertained (supposing our knowledge of the law to be the result of experience alone,) whether it were true with that precise and rigorous exactness which more refined modes of experimenting could test. We so willingly believe in the simplicity of laws of nature, that the rigorous accuracy of such a law, known to be at least approximately true, was taken for granted, till some ground for suspecting the contrary should appear. Yet calculations have not been wanting which might confirm the law as true to the last degree of accuracy. Laplace relates (*Syst. du Monde*, livre iv. chap. 16,) that at one time he had conceived it possible that the effect of gravity upon the moon might be slightly modified by the moon's direction and velocity; and that in this way an explanation might be found for the moon's *acceleration* (a deviation of her observed from her calculated place, which long 248 perplexed mathematicians). But it was after some time discovered that this feature in the moon's

motion arose from another cause; and the second law of motion was confirmed as true in the most rigorous sense.

Thus we see that although there were arguments which might be urged in favour of this law, founded upon the necessary relations of ideas, men became convinced of its truth only when it was verified and confirmed by actual experiment. But yet in this case again, as in the former ones, when the law had been established beyond doubt or question, men were very ready to believe that it was not a mere result of observation, —that the truth which it contained was not derived from experience,—that it might have been assumed as true in virtue of reasonings anterior to experience,—and that experiments served only to make the law more plain and intelligible, as visible diagrams in geometry serve to illustrate geometrical truths; our knowledge not being (they deemed) in mechanics, any more than in geometry, borrowed from the senses. It was thought by many to be self-evident, that the effect of a force in any direction

cannot be increased or diminished by any motion transverse to the direction of the force which the body may have at the same time: or, to express it otherwise, that if the motion of the body be compounded of a horizontal and vertical motion, the vertical motion alone will be affected by the vertical force. This principle, indeed, not only has appeared evident to many persons, but even at the present day is assumed as an axiom by many of the most eminent mathematicians. It is, for example, so employed in the *Mécanique Céleste* of Laplace, which may be looked upon as the standard of mathematical mechanics in our time; and in the *Mécanique Analytique* of Lagrange, the most consummate example which has appeared of subtilty of thought on such subjects, as well as of power of mathematical generalization[29]. And 249 thus we have here another example of that circumstance which we have already noticed in speaking of the First Law of Motion, (Art. 2 of this chapter,) and of the Law that Gravity is a uniform Force, (Art. 3); namely, that the law, though historically

established by experiments, appears, when once discovered and reduced to its most simple and general form, to be self-evident. I am the more desirous of drawing attention to this feature in various portions of the history of science, inasmuch as it will be found to lead to some very extensive and important views, hereafter to be considered.

[29] I may observe that the rule that we may *compound* motions, as the Law supposes, is involved in the step of *resolving* them; which is done in the passage to which I refer. (*Méc. Analyt.* ptie. i. sect. i. art. 3. p. 225.) 'Si on conçoit que le mouvement d'un corps et les forces qui le sollicitent soient *decomposées* suivant trois lignes droites perpendiculaires entre elles, on pourra considérer séparément les mouvemens et les forces relatives à chacun de ces trois directions. Car à cause de la perpendicularité des directions il est visible que chacun de ces mouvemens partiels peut être regardé comme indépendant des deux autres, et qu'il ne peut recevoir d'altération que de la part de la force qui agit dans la direction de ce mouvement; l'on peut conclure que ces trois mouvemens doivent suivre, chacun en particulier, les lois des mouvemens rectilignes accélérés ou retardés par les forces données.' Laplace makes the same assumption in effect, (*Méc. Cél.* p. i. liv. i.

art. 7), by resolving the forces which act upon a point in three rectangular directions, and reasoning separately concerning each direction. But in his mode of treating the subject is involved a principle which belongs to the Third Law of Motion, namely, the doctrine that the velocity is as the force, of which we shall have to speak elsewhere.

5. *The Third Law of Motion.*—We have, in the definition of Accelerating Force, a measure of Forces, so far as they are concerned in producing motion. We had before, in speaking of the principles of statics, defined the measure of Forces or Pressures, so far as they are employed in producing equilibrium. But these two aspects of Force are closely connected; and we require a law which shall lay down the rule of their connexion. By the same kind of muscular exertion by which we can support a heavy stone, we can also put it in motion. The question then occurs, how is the rate and manner of its motion determined? The answer to this question is contained in the Third Law 250 of Motion, and it is to this effect: that the *Momentum* which any pressure produces in the mass in a given

time is proportional to the pressure. By *Momentum* is meant the product of the numbers which express the velocity and the mass of the body: and hence, if the mass of the body be the same in the instances which we compare, the rule is,—That *the velocity is as the force which produces it*; and this is one of the simplest ways of expressing the Third Law of Motion.

In agreement with our general plan, we have to ask, What is the ground of this rule? What is the simplest and most satisfactory form to which we can reduce the proof of it? Or, to take an instance; if a double pressure be exerted against a given mass, so disposed as to be capable of motion, why must it produce twice the velocity in the same time?

To answer this question, suppose the double pressure to be resolved into two single pressures: one of these will produce a certain velocity; and the question is, why an equal pressure, acting upon the same mass, will produce an equal velocity *in addition* to

the former? Or, stating the matter otherwise, the question is, why each of the two forces will produce its separate effect, unaltered by the simultaneous action of the other force?

This statement of the case makes it seem to approach very near to such cases as are included in the Second Law of Motion, and therefore it might appear that this Third Law has no grounds distinct from the Second. But it must be recollected that the word *force* has a different meaning in this case and in that; in this place it signifies *pressure*; in the statement of the Second Law its import was *accelerative* or *deflective force*, measured by the velocity or deflexion generated. And thus the Third Law of Motion, so far as our reasonings yet go, appears to rest on a foundation different from the Second.

Accordingly, that part of the Third Law of Motion which we are now considering, that the velocity generated is as the force, was obtained, in fact, by a separate train of

research. The first exemplification of this 251 law which was studied by mathematicians, was the motion of bodies upon inclined planes: for the force which urges a body down an inclined plane is known by statics, and hence the velocity of its descent was to be determined. Galileo originally[30] in his attempts to solve this problem of the descent of a body down an inclined plane, did not proceed from the principle which we have stated, (the determination of the force which acts down the inclined plane from statical considerations,) obvious as it may seem; but assumed, as we have already seen, a proposition apparently far more precarious; —namely, that a body sliding down a smooth inclined plane acquires always the same velocity, so long as the *vertical* height fallen through is the same. And this conjecture (for at first it was nothing more than a conjecture) he confirmed by an ingenious experiment; in which bodies acquired or lost the same velocity by descending or ascending through the same height, although their paths were different

in other respects.

[30] *Dial. della Sc. Nuov.* iii. p. 96. See *Hist. Ind. Sci.* b. vi. c. ii. sect. 5.

This was the form in which the doctrine of the motion of bodies down inclined planes was at first presented in Galileo's *Dialogues* on the Science of Motion. But his disciple Viviani was dissatisfied with the assumption thus introduced; and in succeeding editions of the *Dialogues*, the apparent chasm in the reasoning was much narrowed, by making the proof depend upon a principle nearly identical with the third law of motion as we have just stated it. In the proof thus added, 'We are agreed,' says the interlocutor[31], 'that in a moving body the impetus, energy, momentum, or propension to motion, is as great as is the force or least resistance which suffices to sustain it;' and the impetus or momentum, in the course of the proof, being taken to be as the velocity produced in a given time, it is manifest that the principle so stated amounts to this; that the velocity produced is as the statical force. And thus this law of

motion appears, 252 in the school of Galileo, to have been suggested and established at first by experiment, but afterwards confirmed and demonstrated by *à priori* considerations.

[31] *Dialogo*, p. 104.

We see, in the above reasoning, a number of abstract terms introduced which are not, at first at least, very distinctly defined, as *impetus*, *momentum*, &c. Of these, *momentum* has been selected, to express that quantity which, in a moving body, measures the statical force impressed upon the body. This quantity is, as we have just seen, proportional to the velocity in a given body. It is also, in different bodies, proportional to the mass of the body. This part of the third law of motion follows from our conception of matter in general as consisting of parts capable of addition. A double pressure must be required to produce the same velocity in a double mass; for if the mass be halved, each half will require an equal pressure; and the addition, both of the pressures and of the masses, will take place without

disturbing the effects.

The measure of the quantity of matter of a body considered as affecting the velocity which pressure produces in the body, is termed its *inertia*, as we have already stated (c. v.). Inertia is the property by which a large mass of matter requires a greater force than a small mass, to give it an equal velocity. It belongs to each portion of matter; and portions of inertia are added whenever portions of matter are added. Hence *inertia is as the quantity of matter*; which is only another way of expressing this third law of motion, so far as quantity of matter is concerned.

But how do we know the quantity of matter of a body? We may reply, that we take the weight as the measure of the quantity of matter: but we may then be again asked, how it appears that the weight is proportional to the inertia; which it must be, in order that the quantity of matter may be proportional to both one and the other. We answer, that this appears to be true

experimentally, because all bodies fall with equal velocities by gravity, when the known causes of difference are removed. The observations of falling 253 bodies, indeed, are not susceptible of much exactness: but experiments leading to the same result, and capable of great precision, were made upon pendulums by Newton; as he relates in his *Principia*, Book III. prop. 6. They all agreed, he says, with perfect accuracy: and thus the weight and the inertia are proportional in all cases, and therefore each proportional to the quantity of matter as measured by the other.

The conception of inertia, as we have already seen in chapter v., involves the notion of action and reaction; and thus the laws which involve inertia depend upon the idea of mutual causation. The rule, that the velocity is as the force, depends upon the principle of causation, that the effect is proportional to the cause; the effect being here so estimated as to be consistent both with the other laws of motion and with experiment.

But here, as in other cases, the question occurs again; Is experiment really requisite for the proof of this law? If we look to authorities, we shall be not a little embarrassed to decide. D'Alembert is against the necessity of experimental proof. 'Why,' says he[32], 'should we have recourse to this principle employed, at the present day, by everybody, that the force is proportional to the velocity? … a principle resting solely upon this vague and obscure axiom, that the effect is proportional to the cause. We shall not examine here,' he adds, 'if this principle is necessarily true; we shall only avow that the proofs which have hitherto been adduced do not appear to us unexceptionable: nor shall we, with some geometers, adopt it as a purely contingent truth; which would be to ruin the certainty of mechanics, and to reduce it to be nothing more than an experimental science. We shall content ourselves with observing,' he proceeds, 'that certain or doubtful, clear or obscure, it is useless in mechanics, and consequently ought to be banished from the science.' Though D'Alembert rejects the

third law of motion in this form, he accepts one of 254 equivalent import, which appears to him to possess axiomatic certainty; and this procedure is in consistence with the course which he takes, of claiming for the science of mechanics more than mere experimental truth. On the contrary, Laplace considers this third law as established by experiment. ‘Is the force,’ he says’[33], ‘proportioned to the velocity? This,’ he replies, ‘we cannot know *à priori*, seeing that we are in ignorance of the nature of moving force: we must therefore, for this purpose, recur to experience; for all which is not a necessary consequence of the few data we have respecting the nature of things, is, for us, only a result of observation.’ And again he says[34], ‘Here, then, we have two laws of motion,—the law of inertia [the first law of motion], and the law of the force proportional to the velocity, —which are given by observation. They are the most natural and the most simple laws which we can imagine, and without doubt they flow from the very nature of matter; but this nature being unknown, they are, for

us, only observed facts: the only ones, however, which Mechanics borrows from experience.'

[32] *Dynamique*, Pref. p. x.

[33] *Méc. Cél.* p. 15.

[34] p. 18.

It will appear, I think, from the views given in this and several other parts of the present work, that we cannot with justice say that we have very 'few data respecting the nature of things,' in speculating concerning the laws of the universe; since all the consequences which flow from the relations of our fundamental ideas, necessarily regulate our knowledge of things, so far as we have any such knowledge. Nor can we say that the nature of matter is unknown to us, in any sense in which we can conceive knowledge as possible. The nature of matter is no more unknown than the nature of space or of number. In our conception of matter, as of space and of number, are involved certain relations, which are the necessary

groundwork of our knowledge; and anything which is independent of these relations, is not unknown, but inconceivable. 255

It must be already clear to the reader, from the phraseology employed by these two eminent mathematicians, that the question respecting the formation of the third law of motion can only be solved by a careful consideration of what we mean by observation and experience, nature and matter. But it will probably be generally allowed, that, taking into account the explanations already offered of the necessary conditions of experience and of the conception of inertia, this law of motion, that the inertia is as the quantity of matter, is almost or altogether self-evident.

6. *Action and Reaction are Equal in Moving Bodies.*—When we have to consider bodies as acting upon one another, and influencing each other's motions, the third law of motion is still applied; but along with this, we also employ the general principle that

action and reaction are equal and opposite. Action and reaction are here to be understood as momentum produced and destroyed, according to the measure of action established by the Third Law of Motion: and the cases in which this principle is thus employed form so large a portion of those in which the third law of motion is used, that some writers (Newton at the head of them) have stated the equality of action and reaction as the third law of motion.

The third law of motion being once established, the equality of action and reaction, in the sense of momentum gained and lost, necessarily follows. Thus, if a weight hanging by a string over the edge of a smooth level table draw another weight along the table, the hanging weight moves more slowly than it would do if not so connected, and thus loses velocity by the connexion; while the other weight gains by the connexion all the velocity which it has, for if left to itself it would rest. And the pressures which restrain the descent of the

first body and accelerate the motion of the second, are equal at all instants of time, for each of these pressures is the tension of the string: and hence, by the third law of motion, the momentum gained by the one body, and the momentum lost by the other in virtue of the action of this string, are equal. And similar 256 reasoning may be employed in any other case where bodies are connected.

The case where one body does not push or draw, but *strikes* another, appeared at first to mechanical reasoners to be of a different nature from the others; but a little consideration was sufficient to show that a blow is, in fact, only a short and violent pressure; and that, therefore, the general rule of the equality of momentum lost and gained applies to this as well as to the other cases.

Thus, in order to determine the case of the direct action of bodies upon one another, we require no new law of motion. The equality of action and reaction, which

enters necessarily into every conception of mechanical operation, combined with the measure of action as given by the third law of motion, enables us to trace the consequences of every case, whether of pressure or of impact.

7. *D'Alembert's Principle.*—But what will be the result when bodies do not act directly upon each other, but are *indirectly* connected in any way by levers, strings, pulleys, or in any other manner, so that one part of the system has a mechanical advantage over another? The result must still be determined by the principle that action and reaction balance each other. The action and reaction, being pressures in one sense, must balance each other by the laws of statics, for these laws determine the equilibrium of pressure. Now action and reaction, according to their measures in the Third Law of Motion, are momentum gained and lost, when the action is direct; and except the indirect action introduce some modification of the law, they must have the same measure still. But, in fact, we cannot

well conceive any modification of the law to take place in this case; for direct action is only one (the ultimate) case of indirect action. Thus if two heavy bodies act at different points of a lever, the action of each on the other is indirect; but if the two points come together, the action becomes direct. Hence the rule must be that which we have already stated; for if the rule were false for indirect action, it would 257 also be false for direct action, for which case we have shown it to be true. And thus we obtain the general principle, that in any system of bodies which act on each other, action and reaction, estimated by momentum gained and lost, balance each other according to the laws of equilibrium. This principle, which is so general as to supply a key to the solution of all possible mechanical problems, is commonly called *D'Alembert's Principle*. The experimental proofs which convinced men of the truth of the Third Law of Motion were, many or most of them, proofs of the law in this extended sense. And thus the proof of D'Alembert's Principle, both from the idea of mechanical

action and from experience, is included in the proof of the law already stated.

8. *Connexion of Dynamical and Statical Principles.*—The principle of equilibrium of D'Alembert just stated, is the law which he would substitute for the Third Law of Motion; and he would thus remove the necessity for an independent proof of that law. In like manner, the Second Law of Motion is by some writers derived from the principle of the composition of statical forces; and they would thus supersede the necessity of a reference to experiment in that case. Laplace takes this course, and thus, as we have seen, rests only the First and Third Law of Motion upon experience. Newton, on the other hand, recognizes the same connexion of propositions, but for a different purpose; for he derives the composition of statical forces from the Second Law of Motion.

The close connexion of these three principles, the composition of (statical) forces, the composition of (accelerating)

forces with velocities, and the measure of (moving) forces by velocities, cannot be denied; yet it appears to be by no means easy to supersede the necessity of independent proofs of the last two of these principles. Both may be proved or illustrated by experiment: and the experiments which prove the one are different from those which establish the other. For example, it appears by easy calculations, that when we apply our principles to the oscillations of a pendulum, 258 the Second Law is proved by the fact, that the oscillations take place at the same rate in an east and west, and in a north and south direction: under the same circumstances, the Third Law is proved by our finding that the time of a small oscillation is proportional to the square root of the length of a pendulum; and similar differences might be pointed out in other experiments, as to their bearing upon the one law or the other.

9. *Mechanical Principles become gradually more simple and more evident.*—I will again

point out in general two circumstances which I have already noticed in particular cases of the laws of motion.—Truths are often at first assumed in a form which is far from being the most obvious or simple;—and truths once discovered are gradually simplified, so as to assume the appearance of self-evident truths.

The former circumstance is exemplified in several of the instances which we have had to consider. The assumption, that a perpetual motion is impossible, preceded the knowledge of the first law of motion. The assumed equality of the velocities acquired down two inclined planes of the same height, was afterwards reduced to the third law of motion by Galileo himself. In the History[35], we have noted Huyghens's assumption of the equality of the actual descent and potential ascent of the center of gravity: this was afterwards reduced by Herman and the Bernoullis, to the statical equivalence of the solicitations of gravity and the vicarious solicitations of the effective forces which act on each point;

and finally to the principle of D'Alembert, which asserts that the motions gained and lost balance each other.

[35] B. vi. c. v. sect. 2.

This early assertion of principles which now appear neither obvious nor self-evident, is not to be considered as a groundless assumption on the part of the discoverers by whom it was made. On the contrary, it is evidence of the deep sagacity and clear thought which were 259 requisite in order to make such discoveries. For these results are really rigorous consequences of the laws of motion in their simplest form: and the evidence of them was probably present, though undeveloped, in the minds of the discoverers. We are told of geometrical students, who, by a peculiar aptitude of mind, perceived the evidence of some of the more advanced propositions of geometry without going through the introductory steps. We must suppose a similar aptitude for mechanical reasonings, which, existing in the minds of Stevinus, Galileo, Newton, and Huyghens, led them to

make those assumptions which finally resolved themselves into the laws of motion.

We may observe further, that the simplicity and evidence which the laws of mechanics have at length assumed, are much favoured by the usage of words among the best writers on such subjects. Terms which originally, and before the laws of motion were fully known, were used in a very vague and fluctuating sense, were afterwards limited and rendered precise, so that assertions which at first appear identical propositions become distinct and important principles. Thus *force*, *motion*, *momentum*, are terms which were employed, though in a loose manner, from the very outset of mechanical speculation. And so long as these words retained the vagueness of common language, it would have been a useless and barren truism to say that 'the momentum is proportional to the force,' or that 'a body loses as much motion as it communicates to another.' But when 'momentum' and 'quantity of motion' are defined to mean the product of mass and

velocity, these two propositions immediately become distinct statements of the third law of motion and its consequences. In like manner, the assertion that ‘gravity is a uniform force’ was assented to, before it was settled what a uniform force was; but this assertion only became significant and useful when that point had been properly determined. The statement that ‘when different motions are communicated to the same body their effects are 260 compounded,’ becomes the second law of motion, when we define what composition of motions is. And the same process may be observed in other cases.

And thus we see how well the form which science ultimately assumes is adapted to simplify knowledge. The definitions which are adopted, and the terms which become current in precise senses, produce a complete harmony between the matter and the form of our knowledge; so that truths which were at first unexpected and recondite, became familiar phrases, and after a few generations sound, even to

common ears, like identical propositions.

10. *Controversy of the Measure of Force.*— In the History of Mechanics[36], we have given an account of the controversy which, for some time, occupied the mathematicians of Europe, whether the forces of bodies in motion should be reckoned proportional to the velocity, or to the square of the velocity. We need not here recall the events of this dispute; but we may remark, that its history, as a metaphysical controversy, is remarkable in this respect, that it has been finally and completely settled; for it is now agreed among mathematicians that both sides were right, and that the results of mechanical action may be expressed with equal correctness by means of *momentum* and of *vis viva*. It is, in one sense, as D'Alembert has said[37], a dispute about words; but we are not to infer that, on that account, it was frivolous or useless; for such disputes are one principal means of reducing the principles of our 261 knowledge to their utmost simplicity and clearness. The terms which are employed in

the science of mechanics are now liberated for ever, in the minds of mathematicians, from that ambiguity which was the battleground in the war of the *vis viva*.

[36] B. vi. c. v. sect. 2.

[37] D'Alembert has also remarked (*Dynamique*, Pref. xxi.) that this controversy 'shows how little justice and precision there is in the pretended axiom that causes are proportional to their effects.' But this reflection is by no means well founded. For since both measures are true, it appears that causes may be *justly* measured by their effects, even when very different kinds of effects are taken. That the axiom does not point out one *precise* measure, till illustrated by experience or by other considerations, we grant: but the same thing occurs in the application of other axioms also.

But we may observe that the real reason of this controversy was exactly that tendency which we have been noticing;—the disposition of man to assume in his speculations certain general propositions as true, and to fix the sense of terms so that they shall fall in with this truth. It was agreed, on all hands, that in the mutual

action of bodies the same quantity of force is always preserved; and the question was, by which of the two measures this rule could best be verified. We see, therefore, that the dispute was not concerning a definition merely, but concerning a definition combined with a general proposition. Such a question may be readily conceived to have been by no means unimportant; and we may remark, in passing, that such controversies, although they are commonly afterwards stigmatized as quarrels about words and definitions, are, in reality, events of considerable consequence in the history of science; since they dissipate all ambiguity and vagueness in the use of terms, and bring into view the conditions under which the fundamental principles of our knowledge can be most clearly and simply presented.

It is worth our while to pause for a moment on the prospect that we have thus obtained, of the advance of knowledge, as exemplified in the history of Mechanics. The general transformation of our views from

vague to definite, from complex to simple, from unexpected discoveries to self-evident truths, from seeming contradictions to identical propositions, is very remarkable, but it is by no means peculiar to our subject. The same circumstances, more or less prominent, more or less developed, appear in the history of other sciences, according to the point of advance which each has reached. They bear upon very important doctrines respecting the prospects, the 262 limits, and the very nature of our knowledge. And though these doctrines require to be considered with reference to the whole body of science, yet the peculiar manner in which they are illustrated by the survey of the history of Mechanics, on which we have just been engaged, appears to make this a convenient place for introducing them to the reader.

CHAPTER VIII.

Of the Paradox of Universal Propositions obtained from Experience.

1. IT was formerly stated[38] that experience cannot establish any universal or necessary truths. The number of trials which we can make of any proposition is necessarily limited, and observation alone cannot give us any ground of extending the inference to untried cases. Observed facts have no visible bond of necessary connexion, and no exercise of our senses can enable us to discover such connexion. We can never acquire from a mere observation of facts, the right to assert that a proposition is true in all cases, and that it could not be otherwise than we find it to be.

[38] B. i. c. iv. Of Experience

Yet, as we have just seen in the history of the laws of motion, we may go on collecting our knowledge from observation, and enlarging and simplifying it, till it approaches or attains to complete universality and seeming necessity. Whether the laws of motion, as we now know them, can be rigorously traced to an absolute necessity in the nature of things, we have not ventured absolutely to pronounce. But we have seen that some of the most acute and profound mathematicians have believed that, for these laws of motion, or some of them, there was such a demonstrable necessity compelling them to be such as they are, and no other. Most of those who have carefully studied the principles of Mechanics will allow that some at least of the primary laws of motion approach very near to this character of necessary truth; and will confess that it would be difficult to imagine any other consistent 264 scheme of fundamental principles. And almost all mathematicians will allow to these laws an absolute universality; so that we may apply them without scruple or misgiving, in cases

the most remote from those to which our experience has extended. What astronomer would fear to refer to the known laws of motion, in reasoning concerning the double stars; although these objects are at an immeasurably remote distance from that solar system which has been the only field of our observation of mechanical facts? What philosopher, in speculating respecting a magnetic fluid, or a luminiferous ether, would hesitate to apply to it the mechanical principles which are applicable to fluids of known mechanical properties? When we assert that the quantity of motion in the world cannot be increased or diminished by the mutual actions of bodies, does not every mathematician feel convinced that it would be an unphilosophical restriction to limit this proposition to such modes of action as we have tried?

Yet no one can doubt that, in historical fact, these laws were collected from experience. That such is the case, is no matter of conjecture. We know the time, the persons, the circumstances, belonging to

each step of each discovery. I have, in the History, given an account of these discoveries; and in the previous chapters of the present work, I have further examined the nature and the import of the principles which were thus brought to light.

Here, then, is an apparent contradiction. Experience, it would seem, has done that which we had proved that she cannot do. She has led men to propositions, universal at least, and to principles which appear to some persons necessary. What is the explanation of this contradiction, the solution of this paradox? Is it true that Experience can reveal to us universal and necessary truths? Does she possess some secret virtue, some unsuspected power, by which she can detect connexions and consequences which we have declared to be out of her sphere? Can she see more than mere appearances, and observe more than mere facts? Can 265 she penetrate, in some way, to the nature of things?—descend below the surface of phenomena to their causes and origins, so as to be able to say

what can and what can not be;—what occurrences are partial, and what universal? If this be so, we have indeed mistaken her character and powers; and the whole course of our reasoning becomes precarious and obscure. But, then, when we return upon our path we cannot find the point at which we deviated, we cannot detect the false step in our deduction. It still seems that by experience, strictly so called, we cannot discover necessary and universal truths. Our senses can give us no evidence of a necessary connexion in phenomena. Our observation must be limited, and cannot testify concerning anything which is beyond its limits. A general view of our faculties appears to prove it to be impossible that men should do what the history of the science of mechanics shows that they have done.

2. But in order to try to solve this Paradox, let us again refer to the History of Mechanics. In the cases belonging to that science, in which propositions of the most unquestionable universality, and most

approaching to the character of necessary truths, (as, for instance, the laws of motion,) have been arrived at, what is the source of the axiomatic character which the propositions thus assume? The answer to this question will, we may hope, throw some light on the perplexity in which we appear to be involved.

Now the answer to this inquiry is, that the laws of motion borrow their axiomatic character from their being merely *interpretations* of the Axioms of Causation. Those axioms, being exhibitions of the Idea of Cause under various aspects, are of the most rigorous universality and necessity. And so far as the laws of motion are exemplifications of those axioms, these laws must be no less universal and necessary. How these axioms are to be understood;—in what sense *cause* and *effect*, *action* and *reaction*, are to be taken, experience and observation did, in fact, teach inquirers on this subject; and without this teaching, the laws of motion could never have been distinctly known. If two forces 266 act

together, each must produce its effect, by the axiom of causation; and, therefore, the effects of the separate forces must be *compounded*. But a long course of discussion and experiment must instruct men of what kind this *composition* of forces is. Again; action and reaction must be equal; but much thought and some trial were needed to show what *action* and *reaction* are. Those metaphysicians who enunciated Laws of motion without reference to experience, propounded only such laws as were vague and inapplicable. But yet these persons manifested the indestructible conviction, belonging to man's speculative nature, that there exist Laws of motion, that is, universal formulæ, connecting the causes and effects when motion takes place. Those mechanicians, again, who, observed facts involving equilibrium and motion, and stated some narrow rules, without attempting to ascend to any universal and simple principle, obtained laws no less barren and useless than the metaphysicians; for they could not tell in what new cases, or whether in any, their laws would be

verified;—they needed a more general rule, to show them the limits of the rule they had discovered. They went wrong in each attempt to solve a new problem, because their interpretation of the terms of the axioms, though true, perhaps, in certain cases, was not right in general.

Thus Pappus erred in attempting to interpret as a case of the lever, the problem of supporting a weight upon an inclined plane; thus Aristotle erred in interpreting the doctrine that the weight of bodies is the cause of their fall; thus Kepler erred in interpreting the rule that the velocity of bodies depends upon the force; thus Bernoulli[39] erred in interpreting the equality of action and reaction upon a lever in motion. In each of these instances, true doctrines, already established, (whether by experiment or otherwise,) were erroneously applied. And the error was corrected by further reflection, which pointed out that another mode of interpretation was requisite, in order that the axiom 267 which, was appealed to in each case might retain

its force in the most general sense. And in the reasonings which avoided or corrected such errors, and which led to substantial general truths, the object of the speculator always was to give to the acknowledged maxims which the Idea of Cause suggested, such a signification as should be consistent with their universal validity. The rule was not accepted as particular at the outset, and afterwards generalized more and more widely; but from the very first, the universality of the rule was assumed, and the question was, how it should be understood so as to be universally true. At every stage of speculation, the law was regarded as a general law. This was not an aspect which it gradually acquired, by the accumulating contributions of experience, but a feature of its original and native character. *What* should happen universally, experience might be needed to show: but that what happened should happen *universally*, was implied in the nature of knowledge. The universality of the laws of motion was not gathered from experience, however much the laws themselves might

be so.

[39] *Hist. Ind. Sc.* b. vi. c. v. sect. 2.

3. Thus we obtain the solution of our Paradox, so far as the case before us is concerned. The laws of motion borrow their *form* from the Idea of Causation, though their *matter* may be given by experience: and hence they possess a universality which experience cannot give. They are certainly and universally valid; and the only question for observation to decide is, how they are to be understood. They are like general mathematical formulæ, which are known to be true, even while we are ignorant what are the unknown quantities which they involve. It must be allowed, on the other hand, that so long as these formulæ are not interpreted by a real study of nature, they are not only useless but prejudicial; filling men's minds with vague general terms, empty maxims, and unintelligible abstractions, which they mistake for knowledge. Of such perversion of the speculative propensities of man's nature, the world has seen too much in all ages. Yet we

must not, on that account, despise these forms of 268 truth, since without them, no general knowledge is possible. Without general terms, and maxims, and abstractions, we can have no science, no speculation; hardly, indeed, consistent thought or the exercise of reason. The course of real knowledge is, to obtain from thought and experience the right interpretation of our general terms, the real import of our maxims, the true generalizations which our abstractions involve.

4. If it be asked, How Experience is able to teach us to interpret aright the general terms which the Axioms of Causation involve;—whence she derives the light which she is to throw on these general notions; the answer is obvious;—namely, that the relations of causation are the *conditions* of Experience;—that the general notions are *exemplified* in the particular cases of which she takes cognizance. The events which take place about us, and which are the objects of our observation, we

cannot conceive otherwise than as subject to the laws of cause and effect. Every event must have a cause;—Every effect must be determined by its cause;—these maxims are true of the phenomena which form the materials of our experience. It is precisely to them, that these truths apply. It is in the world which we have before our eyes, that these propositions are universally verified; and it is therefore by the observation of what we see, that we must learn how these propositions are to be understood. Every fact, every experiment, is an example of these statements; and it is therefore by attention to and familiarity with facts and experiments, that we learn the signification of the expressions in which the statements are made; just as in any other case we learn the import of language by observing the manner in which it is applied in known cases. Experience is the interpreter of nature; it being understood that she is to make her interpretation in that comprehensive phraseology which is the genuine language of science.

5. We may return for an instant to the objection, that experience cannot give us general truths, since, after any number of trials confirming a rule, we may for aught we can foresee, have one which violates the 269 rule. When we have seen a thousand stones fall to the ground, we may see one which does not fall under the same apparent circumstances. How then, it is asked, can experience teach us that *all* stones, rigorously speaking, will fall if unsupported? And to this we reply, that it is not true that we can conceive one stone to be suspended in the air, while a thousand others fall, without believing some peculiar cause to support it; and that, therefore, such a supposition forms no exception to the law, that gravity is a force by which *all* bodies are urged downwards. Undoubtedly we can conceive a body, when dropt or thrown, to move in a line quite different from other bodies: thus a certain missile[40] used by the natives of Australia, and lately brought to this country, when thrown from the hand in a proper manner, describes a curve, and returns to the place from whence it was

thrown. But did any one, therefore, even for an instant suppose that the laws of motion are different for this and for other bodies? On the contrary, was not every person of a speculative turn immediately led to inquire how it was that the known causes which modify motion, the resistance of the air and the other causes, produced in this instance so peculiar an effect? And if the motion had been still more unaccountable, it would not have occasioned any uncertainty whether it were consistent with the agency of gravity and the laws of motion. If a body suddenly alter its direction, or move in any other unexpected manner, we never doubt that there is a cause of the change. We may continue quite ignorant of the nature of this cause, but this ignorance never occasions a moment's doubt that the cause exists and is exactly suited to the effect. And thus experience can prove or discover to us general rules, but she can never prove that general rules do not exist. Anomalies, exceptions, unexplained phenomena, may remind us that we have much still to learn, but they can never make us suppose that

truths are not universal. We may observe facts that show us we have not fully 270 understood the meaning of our general laws, but we can never find facts which show our laws to have no meaning. Our experience is bound in by the limits of cause and effect, and can give us no information concerning any region where that relation does not prevail. The whole series of external occurrences and objects, through all time and space, exists only, and is conceived only, as subject to this relation; and therefore we endeavour in vain to imagine to ourselves when and where and how exceptions to this relation may occur. The assumption of the connexion of cause and effect is essential to our experience, as the recognition of the maxims which express this connexion is essential to our knowledge.

[40] Called the Bo-me-rang.

6. I have thus endeavoured to explain in some measure how, at least in the field of our mechanical knowledge, experience can discover universal truths, though she cannot

give them their universality; and how such truths, though borrowing their form from our ideas, cannot be understood except by the actual study of external nature. And thus with regard to the laws of motion, and other fundamental principles of Mechanics, the analysis of our ideas and the history of the progress of the science well illustrate each other.

If the paradox of the discovery of universal truths by experience be thus solved in one instance, a much wider question offers itself to us;—How far the difficulty, and how far the solution, are applicable to other subjects. It is easy to see that this question involves most grave and extensive doctrines with regard to the whole compass of human knowledge: and the views to which we have been led in the present Book of this work are, we trust, fitted to throw much light upon the general aspect of the subject. But after discussions so abstract, and perhaps obscure, as those in which we have been engaged for some chapters, I willingly postpone to a future

occasion an investigation which may perhaps appear to most readers more recondite and difficult still. And we have, in fact, many other special fields of knowledge to survey, before we are led by the order of our subject, to 271 those general questions and doctrines, those antitheses brought into view and again resolved, which a view of the whole territory of human knowledge suggests, and by which the nature and conditions of knowledge are exhibited.

Before we quit the subject of mechanical science we shall make a few remarks on another doctrine which forms part of the established truths of the science, namely, the doctrine of universal gravitation.

CHAPTER IX.

Of the Establishment of the Law of Universal Gravitation.

THE doctrine of universal gravitation is a feature of so much importance in the history of science that we shall not pass it by without a few remarks on the nature and evidence of the doctrine.

1. To a certain extent the doctrine of the attraction of bodies according to the law of the inverse square of the distance, exhibits in its progress among men the same general features which we have noticed in the history of the laws of motion. This doctrine was maintained *à priori* on the ground of its simplicity, and was asserted positively, even before it was clearly understood:—notwithstanding this anticipation, its establishment on the ground of facts was a

task of vast labour and sagacity:—when it had been so established in a general way, there occurred at later periods, an occasional suspicion that it might be approximately true only:—these suspicions led to further researches, which showed the rule to be rigorously exact:—and at present there are mathematicians who maintain, not only that it is true, but that it is a necessary property of matter. A very few words on each of these points will suffice.

2. I have shown in the *History of Science*[41], that the attraction of the sun according to the inverse square of the distance, had been divined by Bullialdus, Hooke, Halley, and others, before it was proved by Newton. Probably the reason which suggested this conjecture was, that gravity might be considered 273 as a sort of emanation; and that thus, like light or any other effect diffused from a center, it must follow the law just stated, the efficacy of the force being weakened in receding from the center, exactly in proportion to the space through which it is diffused. It cannot be

denied that such a view appears to be strongly recommended by analogy.

[41] B. vii. c. i.

When it had been proved by Newton that the planets were really retained in their elliptical orbits by a central force, his calculations also showed that the above-stated *law* of the force must be at least very approximately correct, since otherwise the aphelia of the orbits could not be so nearly at rest as they were. Yet when it seemed as if the motion of the moon's apogee could not be accounted for without some new supposition, the *à priori* argument in favour of the inverse square did not prevent Clairaut from trying the hypothesis of a small term added to that which expressed the ancient law: but when, in order to test the accuracy of this hypothesis, the calculation of the motion of the moon's apogee was pushed to a greater degree of exactness than had been obtained before, it was found that the new term vanished of itself; and that the inverse square now accounted for the whole of the motion. And

thus, as in the case of the second law of motion, the most scrupulous examination terminated in showing the simplest rule to be rigorously true.

3. Similar events occurred in the history of another part of the law of gravitation: namely, that the attraction is proportional to the quantity of matter attracted. This part of the law may also be thus stated, That the weight of bodies arising from gravity is proportional to their inertia; and thus, that the *accelerating force* on all bodies under the same circumstances is the same. Newton made experiments which proved this with regard to terrestrial bodies; for he found that, at the end of equal strings, balls of all substances, gold, silver, lead, glass, wood, &c., oscillated in equal times[42]. But a few years ago, doubts 274 arose among the German astronomers whether this law was rigorously true with regard to the planetary bodies. Some calculations appeared to prove, that the attraction of Jupiter as shown by the perturbations which he produces in the small planets Juno, Vesta,

and Pallas, was different from the attraction which he exerts on his own satellites. Nor did there appear to these philosophers anything inconceivable in the supposition that the attraction of a planet might be thus *elective*. But when Mr. Airy obtained a more exact determination of the mass of Jupiter, as indicated by his effect on his satellites, it was found that this suspicion was unfounded; and that there was, in this case, no exception to the universality of the rule, that this cosmical attraction is in the proportion of the attracted mass.

[42] *Prin.* lib. iii. prop. 6.

4. Again: when it had thus been shown that a mutual attraction of parts, according to the law above mentioned, prevailed throughout the extent of the solar system, it might still be doubted whether the same law extended to other regions of the universe. It might have been perhaps imagined that each fixed star had its peculiar law of force. But the examination of the motions of double stars about each other, by the two Herschels and others,

appears to show that these bodies describe ellipses as the planets do; and thus extends the law of the inverse squares to parts of the universe immeasurably distant from the whole solar system.

5. Since every doubt which has been raised with regard to the universality and accuracy of the law of gravitation, has thus ended in confirming the rule, it is not surprizing that men's minds should have returned with additional force to those views which had at first represented the law as a necessary truth, capable of being established by reason alone. When it had been proved by Newton that gravity is really a *universal* attribute of matter as far as we can learn, his pupils were not content without maintaining it to be an *essential* quality. This is the doctrine held by Cotes in the preface to the second edition of the *Principia* (1712): 275 'Gravity,' he says, 'is a primary quality of bodies, as extension, mobility, and impenetrability are.' But Newton himself by no means went so far. In his second Letter to Bentley (1693), he says,

‘You sometimes speak of gravity as essential and inherent to matter; pray do not ascribe that notion to me. The cause of gravity,’ he adds, ‘I do not pretend to know, and would take more time to consider of it.’

Cotes maintains his opinion by urging, that we learn by *experience* that all bodies possess gravity, and that we do not learn in any other way that they are extended, moveable, or solid. But we have already seen, that the ideas of space, time, and reaction, on which depend extension, mobility, and solidity, are not results, but conditions, of experience. We cannot conceive a body except as extended; we cannot conceive it to exert mechanical action except with some kind of solidity. But so far as our conceptions of body have hitherto been developed, we find no difficulty in conceiving two bodies which do not attract each other.

6. Newton lays down, in the second edition of the *Principia*, this ‘Rule of Philosophizing’ (book iii.); that ‘The

qualities of bodies which cannot be made more or less intense, and which belong to all bodies on which we are able to make experiments, are to be held to be qualities of all bodies in general.' And this Rule is cited in the sixth Proposition of the Third Book of the *Principia*, (Cor. 2,) in order to prove that gravity, proportional to the quantity of matter, may be asserted to be a quality of all bodies universally. But we may remark that a Rule of Philosophizing, itself of precarious authority, cannot authorize us in ascribing universality to an empirical result. Geometrical and statical properties are seen to be necessary, and *therefore* universal: but Newton appears disposed to assert a like universality of gravity, quite unconnected with any necessity. It would be a very inadequate statement, indeed a false representation, of statical truth, if we were to say, that because every body which has hitherto been tried *has been found* to have a center of gravity, we venture to assert that all bodies whatever 276 have a center of gravity. And if we are ever able to assert the absolute universality of the law of

gravitation, we shall have to rest this truth upon the clearer development of our ideas of matter and force; not upon a Rule of Philosophizing, which, till otherwise proved, must be a mere rule of prudence, and which the opponent may refuse to admit.

7. Other persons, instead of asserting gravity to be in its own nature essential to matter, have made hypotheses concerning some mechanism or other, by which this mutual attraction of bodies is produced[43]. Thus the Cartesians ascribed to a vortex the tendency of bodies to a center; Newton himself seems to have been disposed to refer this tendency to the elasticity of an ether; Le Sage propounded a curious hypothesis, in which this attraction is accounted for by the impulse of infinite streams of particles flowing constantly through the universe in all directions. In these speculations, the force of gravity is resolved into the pressure or impulse of solids or fluids. On the other hand, hypotheses have been propounded, in which the solidity, and other physical

qualities of bodies, have been explained by representing the bodies as a collection of points, from which points, repulsive, as well as attractive, forces emanate. This view of the constitution of bodies was maintained and developed by Boscovich, and is hence termed 'Boscovich's Theory:' and the discussion of it will more properly come under our review at a future period, when we speak of the question whether bodies are made up of atoms. But we may observe, that Newton himself appears to have inclined, as his followers certainly did, to this mode of contemplating the physical properties of bodies. In his Preface to the *Principia*, after speaking of the central forces which are exhibited in cosmical phenomena, he says: 'Would that we could derive the other phenomena of Nature from mechanical principles by the same mode of reasoning. For many things move me 277 so that I suspect all these phenomena may depend upon certain forces, by which the particles of bodies, through causes not yet known, are either impelled to each other and cohere according to regular figures, or are repelled

and recede from each other: which forces being unknown, philosophers have hitherto made their attempts upon nature in vain.'

[43] See Vince, *Observations on the Hypothesis respecting Gravitation*, and the Critique of that work, *Edinb. Rev.* vol. xiii.

8. But both these hypotheses;—that by which cohesion and solidity are reduced to attractive and repulsive forces, and that by which attraction is reduced to the impulse and pressure of media;—are hitherto merely modes of representing mechanical laws of nature; and cannot, either of them, be asserted as possessing any evident truth or peremptory authority to the exclusion of the other. This consideration may enable us to estimate the real weight of the difficulty felt in assenting to the mutual attraction of bodies not in contact with each other; for it is often urged that this attraction of bodies at a distance is an absurd supposition.

The doctrine is often thus stigmatized, both by popular and by learned writers. It was long received as a maxim in philosophy (as Monboddo informs us[44]), that a body

cannot act *where* it is not, any more than *when* it is not. But to this we reply, that time is a necessary condition of our conception of causation, in a different manner from space. The action of force can only be conceived as taking place in a succession of moments, in each of which cause and effect immediately succeed each other: and thus the interval of time between a cause and its remote effect is filled up by a continuous succession of events connected by the same chain of causation. But in space, there is no such visible necessity of continuity; the action and reaction may take place at a distance from each other; all that is necessary being that they be equal and opposite.

[44] *Ancient Metaphysics*, vol. ii. p. 175.

Undoubtedly the existence of attraction is rendered more acceptable to common apprehension by supposing 278 some intermediate machinery,—a cord, or rod, or fluid,—by which the forces may be conveyed from one point to another. But such images are rather fitted to satisfy those prejudices which arise from the earlier

application of our ideas of force, than to exhibit the real nature of those ideas. If we suppose two bodies to pull each other by means of a rod or cord, we only suppose, in addition to those equal and opposite forces acting upon the two bodies, (which forces are alone essential to mutual attraction) a certain power of resisting transverse pressure at every point of the intermediate line: which additional supposition is entirely useless, and quite unconnected with the essential conditions of the case. When the Newtonians were accused of introducing into philosophy an unknown cause which they termed *attraction*, they justly replied that they knew as much respecting attraction as their opponents did about impulse. In each case we have a knowledge of the conception in question so far as we clearly apprehend it under the conditions of those axioms of mechanical causation which form the basis of our science on such subjects.

Having thus examined the degree of certainty and generality to which our

knowledge of the law of universal gravitation has been carried, by the progress of mechanical discovery and speculation up to the present time, we might proceed to the other branches of science, and examine in like manner their grounds and conditions. But before we do this, it will be worth our while to attend for a moment to the effect which the progress of mechanical ideas among mathematicians and mechanical philosophers has produced upon the minds of other persons, who share only in an indirect and derivative manner in the influence of science.

CHAPTER X.

Of the general Diffusion of clear Mechanical Ideas.

1. WE have seen how the progress of knowledge upon the subject of motion and force has produced, in the course of the world's history, a great change in the minds of acute and speculative men; so that such persons can now reason with perfect steadiness and precision upon subjects on which, at first, their thoughts were vague and confused; and can apprehend, as truths of complete certainty and evidence, laws which it required great labour and time to discover. This *complete* development and clear manifestation of mechanical ideas has taken place only among mathematicians and philosophers. But yet a progress of thought upon such subjects,—an advance from the

obscure to the clear, and from errour to truth,—may be traced in the world at large, and among those who have not directly cultivated the exact sciences. This diffused and collateral influence of science manifests itself, although in a wavering and fluctuating manner, by various indications, at various periods of literary history. The opinions and reasonings which are put forth upon mechanical subjects, and above all, the adoption, into common language, of terms and phrases belonging to the prevalent mechanical systems, exhibit to us the most profound discoveries and speculations of philosophers in their effect upon more common and familiar trains of thought. This effect is by no means unimportant, and we shall point out some examples of such indications as we have mentioned.

2. The discoveries of the ancients in speculative mechanics were, as we have seen, very scanty; and 280 hardly extended their influence to the unmathematical world. Yet the familiar use of the term

‘center of gravity’ preserved and suggested the most important part of what the Greeks had to teach. The other phrases which they employed, as *momentum*, *energy*, *virtue*, *force*, and the like, never had any exact meaning, even among mathematicians; and therefore never, in the ancient world, became the means of suggesting just habits of thought. I have pointed out, in the History of Science, several circumstances which appear to denote the general confusion of ideas which prevailed upon mechanical subjects during the times of the Roman empire. I have there taken as one of the examples of this confusion, the fable narrated by Pliny and others concerning the echineïs, a small fish, which was said to stop a ship merely by sticking to it[45]. This story was adduced as betraying the absence of any steady apprehension of the equality of action and reaction; since the fish, except it had some immoveable obstacle to hold by, must be pulled forward by the ship, as much as it pulled the ship backward. If the writers who speak of this wonder had shown any perception of the necessity of a

reaction, either produced by the rapid motion of the fish's fins in the water, or in any other way, they would not be chargeable with this confusion of thought; but from their expressions it is, I think, evident that they saw no such necessity[46]. Their idea of mechanical action was not sufficiently distinct to enable them to see the absurdity of 281 supposing an intense pressure with no obstacle for it to exert itself against.

[45] *Hist. Ind. Sc.* b. iv. c. i. sect. 2.

[46] See Prof. Powell, *On the Nature and Evidence of the Laws of Motion. Reports of the Ashmolean Society*. Oxford. 1837. Professor Powell has made an objection to my use of this instance of confusion of thought; the remark in the text seems to me to justify what I said in the History. As an evidence that the fish was not supposed to produce its effect by its muscular power acting on the water, we may take what Pliny says, *Nat. Hist.* xxxii. 1, 'Domat mundi rabiem, nullo suo labore; non retinendo, aut alio modo quam adhærendo:' and also what he states in another place (ix. 41), that when it is preserved in pickle, it may be used in recovering gold which has fallen into a deep well. All this implies adhesion alone,

with no conception of reaction.

3. We may trace, in more modern times also, indications of a general ignorance of mechanical truths. Thus the phrase of shooting at an object ‘point-blank,’ implies the belief that a cannon-ball describes a path of which the first portion is a straight line. This errour was corrected by the true mechanical principles which Galileo and his followers brought to light; but these principles made their way to popular notice, principally in consequence of their application to the motions of the solar system, and to the controversies which took place respecting those motions. Thus by far the most powerful argument against the reception of the Copernican system of the universe, was that of those who asked, Why a stone dropt from a tower was not left behind by the motion of the earth? The answer to this question, now universally familiar, involves a reference to the true doctrine of the composition of motions. Again; Kepler’s persevering and strenuous attempts[47] to frame a physical theory of the

universe were frustrated by his ignorance of the first law of motion, which informs us that a body will retain its velocity without any maintaining force. He proceeded upon the supposition that the sun's force was requisite to *keep up* the motion of the planets, as well as to deflect and modify it; and he was thus led to a system which represented the sun as carrying round the planets in their orbits by means of a *vortex*, produced by his revolution. The same neglect of the laws of motion presided in the formation of Descartes' system of vortices. Although Descartes had enunciated in words the laws of motion, he and his followers showed that they had not the practical habit of referring to these mechanical principles; and dared not trust the planets to move in free space without some surrounding machinery to support them[48].

[47] *Hist. Ind. Sc.* b. v. c. iv. and b. vii. c. i.

[48] I have, in the History, applied to Descartes the character which Bacon gives to Aristotle, 'Audax simul et pavidus:' though he was bold enough to enunciate

> the laws of motion without knowing them aright, he had not the courage to leave the planets to describe their orbits by the agency of those laws, without the machinery of contact.

282 4. When at last mathematicians, following Newton, had ventured to consider the motion of each planet as a mechanical problem not different in its nature from the motion of a stone cast from the hand; and when the solution of this problem and its immense consequences had become matters of general notoriety and interest; the new views introduced, as is usual, new terms, which soon became extensively current. We meet with such phrases as ‘flying off in the tangent,’ and ‘deflexion from the tangent;’ with antitheses between ‘centripetal’ and ‘centrifugal force,’ or between ‘projectile’ and ‘central force.’ ‘Centers of force,’ ‘disturbing forces,’ ‘perturbations,’ and ‘perturbations of higher orders,’ are not unfrequently spoken of: and the expression ‘to gravitate,’ and the term ‘universal gravitation,’ acquired a permanent place in the language.

Yet for a long time, and even up to the present day, we find many indications that false and confused apprehensions on such subjects are by no means extirpated. Arguments are urged against the mechanical system of the universe, implying in the opponents an absence of all clear mechanical notions. Many of this class of writers retrograde to Kepler's point of view. This is, for example, the case with Lord Monboddo, who, arguing on the assumption that force is requisite to maintain, as well as to deflect motion, produced a series of attacks upon the Newtonian philosophy; which he inserted in his *Ancient Metaphysics*, published in 1779 and the succeeding years. This writer (like Kepler), measures force by the velocity which the body *has*[49], not by that which it *gains*. Such a use of language would prevent our obtaining any laws of motion at all. Accordingly, the author, in the very next page to that which I have just quoted, abandons this measure of force, and, in curvilinear motion, measures 283 force by 'the fall from the extremity of the arc.' Again; in his objections to the received

theory, he denies that curvilinear motion is compounded, although his own mode of considering such motion assumes this composition in the only way in which it was ever intended by mathematicians. Many more instances might be adduced to show that a want of cultivation of the mechanical ideas rendered this philosopher incapable of judging of a mechanical system.

[49] *Anc. Met.* vol. ii. b. v. c. vi. p. 413.

The following extract from the *Ancient Metaphysics*, may be sufficient to show the value of the author's criticism on the subjects of which we are now speaking. His object is to prove that there do not exist a centripetal and a centrifugal force in the case of elliptical motion. 'Let any man move in a circular or elliptical line described to him; and he will find no tendency in himself either to the center or from it, much less both. If indeed he attempt to make the motion with great velocity, or if he do it carelessly and inattentively, he may go out of the line, either towards the center or from it: but this is to be ascribed, not to the

nature of the motion, but to our infirmity; or perhaps to the animal form, which is more fitted for progressive motion in a right line than for any kind of curvilinear motion. But this is not the case with a sphere or spheroid, which is equally adapted to motion in all directions[50].' We need hardly remind the reader that the manner in which a man running round a small circle, finds it necessary to lean inwards, in order that there may be a centripetal inclination to counteract the centrifugal force, is a standard example of our mechanical doctrines; and this fact (quite familiar in practice as well as theory) is in direct contradiction of Lord Monboddo's assertion.

[50] *Anc. Met.* vol. i. b. ii. c. 19, p. 264.

5. A similar absence of distinct mechanical thought appears in some of the most celebrated metaphysicians of Germany. I have elsewhere noted[51] the opinion expressed by Hegel, that the glory which belongs to 284 Kepler has been unjustly transferred to Newton; and I have suggested, as the explanation of this mode

of thinking, that Hegel himself, in the knowledge of mechanical truth, had not advanced beyond Kepler's point of view. Persons who possess conceptions of space and number, but who have not learnt to deal with ideas of force and causation, may see more value in the discoveries of Kepler than in those of Newton. Another exemplification of this state of mind may be found in Professor Schelling's speculations; for instance, in his *Lectures on the Method of Academical Study*. In the twelfth Lecture, on the study of Physics and Chemistry, he says, (p. 266,) 'What the mathematical natural philosophy has done for the knowledge of the laws of the universe since the time that they were discovered by his (Kepler's) godlike genius, is, as is well known, this: it has attempted a construction of those laws which, according to its foundations, is altogether empirical. We may assume it as a general rule, that in any proposed construction, that which is not a pure general form cannot have any scientific import or truth. The foundation from which the centrifugal motion of the bodies of the

world is derived, is no necessary form, it is an empirical fact. The Newtonian attractive force, even if it be a necessary assumption for a merely reflective view of the subject, is still of no significance for the Reason, which recognizes only absolute relations. The grounds of the Keplerian laws can be derived, without any empirical appendage, purely from the doctrine of Ideas, and of the two Unities, which are in themselves one Unity, and in virtue of which each being, while it is absolute in itself, is at the same time in the absolute, and reciprocally.'

[51] *Hist. Ind. Sc.* b. vii. c. ii. sect. 5.

It will be observed, that in this passage our mechanical laws are objected to because they are not necessary results of our ideas; which, however, as we have seen, according to the opinion of some eminent mechanical philosophers, they are. But to assume this evident necessity as a condition of every advance in science, is to mistake the last, perhaps unattainable step, for the first, which lies before our feet. And, 285 without inquiring further about 'the Doctrine of the

two Unities,' or the manner in which from that doctrine we may deduce the Keplerian laws, we may be well convinced that such a doctrine cannot supply any sufficient reason to induce us to quit the inductive path by which all scientific truth up to the present time has been acquired.

6. But without going to schools of philosophy opposed to the Inductive School, we may find many loose and vague habits of thinking on mechanical subjects among the common classes of readers and reasoners. And there are some familiar modes of employing the phraseology of mechanical science, which are, in a certain degree, chargeable with inaccuracy, and may produce or perpetuate confusion. Among such cases we may mention the way in which the centripetal and centrifugal forces, and also the projectile and central forces of the planets, are often compared or opposed. Such antitheses sometimes proceed upon the false notion that the two members of these pairs of forces are of the same kind: whereas on the contrary the *projectile* force is a

hypothetical impulsive force which may, at some former period, have caused the motion to begin; while the *central* force is an actual force, which must act continuously and during the whole time of the motion, in order that the motion may go on in the curve. In the same manner the *centrifugal* force is not a distinct force in a strict sense, but only a certain result of the first law of motion, measured by the portion of *centripetal* force which counteracts it. Comparisons of quantities so heterogeneous imply confusion of thought, and often suggest baseless speculations and imagined reforms of the received opinions.

7. I might point out other terms and maxims, in addition to those already mentioned, which, though formerly employed in a loose and vague manner, are now accurately understood and employed by all just thinkers; and thus secure and diffuse a right understanding of mechanical truths. Such are *momentum*, *inertia*, *quantity of matter*, *quantity of motion*; that *force is proportional to its effects*; that *action and* 286

reaction are equal; that *what is gained in force by machinery is lost in time*; that *the quantity of motion in the world cannot be either increased or diminished*. When the expression of the truth thus becomes easy and simple, clear and convincing, the meanings given to words and phrases by discoverers glide into the habitual texture of men's reasonings, and the effect of the establishment of true mechanical principles is felt far from the school of the mechanician. If these terms and maxims are understood with tolerable clearness, they carry the influence of truth to those who have no direct access to its sources. Many an extravagant project in practical machinery, and many a wild hypothesis in speculative physics, has been repressed by the general currency of such maxims as we have just quoted.

8. Indeed so familiar and evident are the elementary truths of mechanics when expressed in this simple form, that they are received as truisms; and men are disposed to look back with surprise and scorn at the speculations which were carried on in

neglect of them. The most superficial reasoner of modern times thinks himself entitled to speak with contempt and ridicule of Kepler's hypothesis concerning the physical causes of the celestial motions: and gives himself credit for intellectual superiority, because he sees, as self-evident, what such a man could not discover at all. It is well for such a person to recollect, that the real cause of his superior insight is not the pre-eminence of his faculties, but the successful labours of those who have preceded him. The language which he has learnt to use unconsciously, has been adapted to, and moulded on, ascertained truths. When he talks familiarly of "accelerating forces" and "deflexions from the tangent," he is assuming that which Kepler did not know, and which it cost Galileo and his disciples so much labour and thought to establish. Language is often called an instrument of thought; but it is also the nutriment of thought; or rather, it is the atmosphere in which thought lives: a medium essential to the activity of our speculative power, although invisible 287

and imperceptible in its operation; and an element modifying, by its qualities and changes, the growth and complexion of the faculties which it feeds. In this way the influence of preceding discoveries upon subsequent ones, of the past upon the present, is most penetrating and universal, though most subtle and difficult to trace. The most familiar words and phrases are connected by imperceptible ties with the reasonings and discoveries of former men and distant times. Their knowledge is an inseparable part of ours; the present generation inherits and uses the scientific wealth of all the past. And this is the fortune, not only of the great and rich in the intellectual world: of those who have the key to the ancient storehouses, and who have accumulated treasures of their own;—but the humblest inquirer, while he puts his reasonings into words, benefits by the labours of the greatest discoverers. When he counts his little wealth, he finds that he has in his hands coins which bear the image and superscription of ancient and modern intellectual dynasties; and that in virtue of

this possession, acquisitions are in his power, solid knowledge within his reach, which none could ever have attained to, if it were not that the gold of truth, once dug out of the mine, circulates more and more widely among mankind.

9. Having so fully examined, in the preceding instances, the nature of the progress of thought which science implies, both among the peculiar cultivators of science, and in that wider world of general culture which receives only an indirect influence from scientific discoveries, we shall not find it necessary to go into the same extent of detail with regard to the other provinces of human knowledge. In the case of the Mechanical Sciences, we have endeavoured to show, not only that Ideas are requisite in order to form into a science the Facts which nature offers to us, but that we can advance, almost or quite, to a complete identification of the Facts with the Ideas. In the sciences to which we now proceed, we shall not seek to fill up the chasm by which Facts and Ideas are

separated; but we shall endeavour to detect the Ideas which our 288 knowledge involves, to show how essential these are; and in some respects to trace the mode in which they have been gradually developed among men.

10. The motions of the heavenly bodies, their laws, their causes, are among the subjects of the first division of the Mechanical Sciences; and of these sciences we formerly sketched the history, and have now endeavoured to exhibit the philosophy. If we were to take any other class of motions, *their* laws and causes might give rise to sciences which would be mechanical sciences in exactly the same sense in which Physical Astronomy is so. The phenomena of magnets, of electrical bodies, of galvanical apparatus, seem to form obvious materials for such sciences; and if they were so treated, the philosophy of such branches of knowledge would naturally come under our consideration at this point of our progress.

But on looking more attentively at the sciences of Electricity, Magnetism, and Galvanism, we discover cogent reasons for transferring them to another part of our arrangement; we find it advisable to associate them with Chemistry, and to discuss their principles when we can connect them with the principles of chemical science. For though the first steps and narrower generalizations of these sciences depend upon mechanical ideas, the highest laws and widest generalizations which we can reach respecting them, involve chemical relations. The progress of these portions of knowledge is in some respects opposite to the progress of Physical Astronomy. In this, we begin with phenomena which appear to indicate peculiar and various qualities in the bodies which we consider, (namely, the heavenly bodies,) and we find in the end that all these qualities resolve themselves into one common mechanical property, which exists alike in all bodies and parts of bodies. On the contrary, in studying magnetical and electrical laws, we appear at first to have a

single extensive phenomenon, attraction and repulsion: but in our attempts to generalize this phenomenon, we find that it is governed by conditions depending upon something quite separate from the bodies themselves, upon 289 the presence and distribution of peculiar and transitory agencies; and, so far as we can discover, the general laws of these agencies are of a *chemical* nature, and are brought into action by peculiar properties of special substances. In cosmical phenomena, everything, in proportion as it is referred to mechanical principles, tends to simplicity,—to permanent uniform forces,—to one common, positive, property. In magnetical and electrical appearances, on the contrary, the application of mechanical principles leads only to a new complexity, which requires a new explanation; and this explanation involves changeable and various forces,—gradations and oppositions of qualities. The doctrine of the universal gravitation of matter is a simple and ultimate truth, in which the mind can acquiesce and repose. We rank gravity

among the mechanical attributes of matter, and we see no necessity to derive it from any ulterior properties. Gravity belongs to matter, independent of any conditions. But the *conditions* of magnetic or electrical activity require investigation as much as the *laws* of their action. Of these conditions no mere mechanical explanation can be given; we are compelled to take along with us chemical properties and relations also: and thus magnetism, electricity, galvanism, are *mechanico-chemical sciences*.

11. Before considering these, therefore, I shall treat of what I shall call *Secondary Mechanical Sciences*; by which expression I mean the sciences depending upon certain qualities which our senses discover to us in bodies;—*Optics*, which has visible phenomena for its subject; *Acoustics*, the science of hearing; the doctrine of *Heat*, a quality which our touch recognizes: to this last science I shall take the liberty of sometimes giving the name *Thermotics*, analogous to the names of the other two. If our knowledge of the phenomena of Smell

and Taste had been successfully cultivated and systematized, the present part of our work would be the place for the philosophical discussion of those sensations as the subjects of science.

The branches of knowledge thus grouped in one class involve common Fundamental Ideas, from which 290 their principles are derived in a mode analogous, at least in a certain degree, to the mode in which the principles of the mechanical sciences are derived from the fundamental ideas of causation and reaction. We proceed now to consider these Fundamental Ideas, their nature, development, and consequences.

BOOK IV.

THE

PHILOSOPHY

OF THE

SECONDARY

MECHANICAL SCIENCES.

Πάσχοντος γάρ τι τοῦ αἰσθητικοῦ γίνεται τὸ ὁρᾶν ὑπ' αὐτοῦ μὲν οὖν τοῦ ὁρωμένου χρωματος ἀδύνατον· λείπεται δὴ ὑπὸ τοῦ μεταξύ, ὥστ' ἀναγκαῖόν τι εἶναι μεταξύ κενοῦ δὲ γενομένου οὐχ ὅτι ἀκριβῶς, ἀλλ' ὅλως οὐθὲν ὀφθήσεται. δι' ἣν μὲν οὖν αἰτίαν τὸ χρῶμα ἀναγκαῖον ἐν φωτὶ ὁρᾶσθαι, εἴρηται. πῦρ δὲ ἐν ἀμφοῖν ὁρᾶται,

καὶ ἐν σκότει καὶ ἐν φωτί, καὶ τοῦτο ἐξ ἀναγκης· τὸ γὰρ διαφανὲς ὑπὸ τούτου γίνεται διαφανές. ὁ δ' αὐτὸς λόγος καὶ περὶ ψόφου καὶ ὀσμῆς ἐστιν· οὐθὲν γὰρ αὐτῶν ἁπτόμενον τοῦ αἰσθητηρίου ποιεῖ τὴν αἰσθησιν, ἀλλ' ὑπὸ μὲν ὀσμῆς καὶ ψόφου τὸ μεταξὺ κινεῖται, ὑπὸ δὲ τούτου τῶν αἰσθητηρίων ἑκάτερον· ὅταν δ' ἐπ' αὐτό τις ἐπιθῇ τὸ αἰσθητήριον τὸ ψοφοῦν ἢ τὸ ὄζον, οὐδεμίαν αἴσθησιν ποιήσει.

ARISTOT. *De Anima*, II. 7.

BOOK IV.

THE PHILOSOPHY OF THE SECONDARY MECHANICAL SCIENCES.

CHAPTER I.

Of the Idea of a Medium as commonly employed.

1. *Of Primary and Secondary Qualities.*—In the same way in which the mechanical sciences depend upon the Idea of Cause, and have their principles regulated by the development of that Idea, it will be found that the sciences which have for their subject Sound, Light, and Heat, depend for *their* principles upon the Fundamental Idea

of Media by means of which we perceive those qualities. Like the idea of cause, this idea of a medium is unavoidably employed, more or less distinctly, in the common, unscientific operations of the understanding; and is recognized as an express principle in the earliest speculative essays of man. But here also, as in the case of the mechanical sciences, the development of the idea, and the establishment of the scientific truths which depend upon it, was the business of a succeeding period, and was only executed by means of long and laborious researches, conducted with a constant reference to experiment and observation.

Among the most prominent manifestations of the influence of the idea of a medium of which we have now to speak, is the distinction of the qualities of bodies into *primary*, and *secondary* qualities. This distinction has 294 been constantly spoken of in modern times: yet it has often been a subject of discussion among metaphysicians whether there be really such

a distinction, and what the true difference is. Locke states it thus[1]: Original or Primary qualities of bodies are ‘such as are utterly inseparable from the body in what estate soever it may be,—such as sense constantly finds in every particle of matter which has bulk enough to be perceived, and the mind finds inseparable from every particle of matter, though less than to make itself singly perceived by our senses:’ and he enumerates them as solidity, extension, figure, motion or rest, and number. Secondary qualities, on the other hand, are such ‘which in truth are nothing in the objects themselves, but powers to produce various sensations in us by their primary qualities, *i.e.* by the bulk, figure, texture, and motion of their insensible parts, as colours, sounds, tastes, &c.’

[1] *Essay*, b. ii. ch. viii. s. 9, 10.

Dr. Reid[2], reconsidering this subject, puts the difference in another way. There is, he says, a real foundation for the distinction of Primary and Secondary qualities, and it is this: ‘That our senses give us a direct and

distinct notion of the primary qualities, and inform us what they are in themselves; but of the secondary qualities, our senses give us only a relative and obscure notion. They inform us only that they are qualities that affect us in a certain manner, that is, produce in us a certain sensation; but as to what they are in themselves, our senses leave us in the dark.'

[2] *Essays*, b. ii. c. xvii.

Dr. Brown[3] states the distinction somewhat otherwise. We give the name of Matter, he observes, to that which has extension and resistance: these, therefore, are Primary qualities of matter, because they compose our definition of it. All other qualities are Secondary, since they are ascribed to bodies only because we find them associated with the primary qualities which form our notion of those bodies.

[3] *Lectures*, ii. 12.

295 It is not necessary to criticize very strictly these various distinctions. If it were, it would be easy to find objections to them.

Thus Locke, it may be observed, does not point out any *reason* for believing that his secondary qualities are produced by the primary. How are we to learn that the colour of a rose arises from the bulk, figure, texture, and motion of its particles? Certainly our senses do not teach us this; and in what other way, on Locke's principles, can we learn it? Reid's statement is not more free from the same objection. How does it appear that our notion of Warmth is relative to our own sensations more than our notion of Solidity? And if we take Brown's account, we may still ask whether our selection of certain qualities to form our idea and definition of Matter be arbitrary and without reason? If it be, how can it make a real distinction? if it be not, what is the reason?

I do not press these objections, because I believe that any of the above accounts of the distinction of Primary and Secondary qualities is right in the main, however imperfect it may be. The difference between such qualities as Extension and Solidity on

the one hand, and Colour or Fragrance on the other, is assented to by all, with a conviction so firm and indestructible, that there must be some fundamental principle at the bottom of the belief, however difficult it may be to clothe the principle in words. That successive efforts to express the real nature of the difference were made by men so clear-sighted and acute as those whom I have quoted, even if none of them are satisfactory, shows how strong and how deeply-seated is the perception of truth which impels us to such attempts.

The most obvious mode of stating the difference of Primary and Secondary qualities, as it naturally offers itself to speculative minds, appears to be that employed by Locke, slightly modified. Certain of the qualities of bodies, as their bulk, figure, and motion, are perceived immediately in the bodies themselves. Certain other qualities as sound, colour, heat, are 296 perceived by means of some medium. Our conviction that this is the case is spontaneous and irresistible; and this

difference of qualities immediately and mediately perceived is the distinction of Primary and Secondary qualities. We proceed further to examine this conviction.

2. *The Idea of Externality.*—In reasoning concerning the Secondary Qualities of bodies, we are led to assume the bodies to be external to us, and to be perceived by means of some Medium intermediate between us and them. These assumptions are fundamental conditions of perception, inseparable from perception even in thought.

That objects are *external* to us, that they are *without* us, that they have *outness*, is as clear as it is that these words have any meaning at all. This conviction is, indeed, involved in the exercise of that faculty by which we perceive all things as existing in space; for by this faculty we place ourselves and other objects in one common space, and thus they are exterior to us. It may be remarked that this apprehension of objects as external to us, although it assumes the

idea of space, is far from being implied in the idea of space. The objects which we contemplate are considered as existing in space, and by that means become invested with certain mutual relations of position; but when we consider them as existing without *us*, we make the additional step of supposing *ourselves* and the objects to exist in one common space. The question respecting the Ideal Theory of Berkeley has been mixed up with the recognition of this condition of the externality of objects. That philosopher maintained, as is well known, that the perceptible qualities of bodies have no existence except in a perceiving mind. This system has often been understood as if he had imagined the world to be a kind of optical illusion, like the images which we see when we shut our eyes, appearing to be without us, though they are only in our organs; and thus this Ideal System has been opposed to a belief in an external world. In truth, however, no such opposition exists. The Ideal System is an attempt to explain the 297 mental process of perception, and to get over the difficulty of mind being

affected by matter. But the author of that system did not deny that objects were perceived under the conditions of space and mechanical causation;—that they were *external* and *material* so far as those words describe perceptible qualities. Berkeley's system, however visionary or erroneous, did not prevent his entertaining views as just, concerning optics or acoustics, as if he had held any other doctrine of the nature of perception.

But when Berkeley's theory was understood as a denial of the existence of objects without us, how was it answered? If we examine the answers which are given by Reid and other philosophers to this hypothesis, it will be found that they amount to this: that objects *are* without us, since we *perceive* that they are so; that we perceive them to be external, by the same act by which we perceive them to be objects. And thus, in this stage of philosophical inquiry, the externality of objects is recognized as one of the inevitable conditions of our perception of them; and

hence the Idea of Externality is adopted as one of the necessary foundations of all reasoning concerning all objects whatever.

3. *Sensation by a Medium.*—Objects, as we have just seen, are necessarily apprehended as *without* us; and in general, as removed from us by a great or small distance. Yet they affect our bodily senses; and this leads us irresistibly to the conviction that they are perceived by means of something intermediate. Vision, or hearing, or smell, or the warmth of a fire, must be communicated to us by some Medium of Sensation. This unavoidable belief appears in all attempts, the earliest and the latest alike, to speculate upon such subjects. Thus, for instance, Aristotle says[4], 'Seeing takes place in virtue of some action which the sentient organ suffers: now it cannot suffer action from the colour of the object directly: the only remaining possible case then is, that it is acted upon by an 298 intervening Medium; there must then be an intervening Medium.' 'And the same may be said,' he adds, 'concerning sounding and odorous bodies;

for these do not produce sensation by touching the sentient organ, but the intervening Medium is acted on by the sound or the smell, and the proper organ, by the Medium … In sound the Medium is air; in smell we have no name for it.' In the sense of taste, the necessity of a Medium is not at first so obviously seen, because the object tasted is brought into contact with the organ; but a little attention convinces us that the taste of a solid body can only be perceived when it is conveyed in some liquid vehicle. Till the fruit is crushed, and till its juices are pressed out, we do not distinguish its flavour. In the case of heat, it is still more clear that we are compelled to suppose some invisible fluid, or other means of communication, between the distant body which warms us and ourselves.

[4] Περὶ Ψυχῆς. ii. 7. See the motto to this Book.

It may appear to some persons that the assumption of an intermedium between the object perceived and the sentient organ results from the principles which form the

basis of our mechanical reasonings,—that every change must have a cause, and that bodies can act upon each other only by contact. It cannot be denied that this principle does offer itself very naturally as the ground of our belief in media of sensation; and it appears to be referred to for this purpose by Aristotle in the passage quoted above. But yet we cannot but ask, Does the principle, that matter produces its effect by contact only, manifestly apply here? When we so apply it, we include *sensation* among the *effects* which material contact produces;—a case so different from any merely mechanical effect, that the principle, so employed, appears to acquire a new signification. May we not, then, rather say that we have here a new axiom,—That sensation implies a material cause immediately acting on the organ,—than a new application of our former proposition, —That all mechanical change implies contact?

The solution of this doubt is not of any material consequence to our reasonings; for

whatever be the 299 ground of the assumption, it is certain that we do assume the existence of media by which the sensations of sight, hearing, and the like, are produced; and it will be seen shortly that principles inseparably connected with this assumption are the basis of the sciences now before us.

This assumption makes its appearance in the physical doctrines of all the schools of philosophy. It is exhibited perhaps most prominently in the tenets of the Epicureans, who were materialists, and extended to all kinds of causation the axiom of the existence of a corporeal mechanism by which alone the effect is produced. Thus, according to them, vision is produced by certain images or material films which flow from the object, strike upon the eyes, and so become sensible. This opinion is urged with great detail and earnestness by Lucretius, the poetical expositor of the Epicurean creed among the Romans. His fundamental conviction of the necessity of a material medium is obviously the basis of his

reasoning, though he attempts to show the existence of such a medium by facts. Thus he argues[5], that by shouting loud we make the throat sore; which shows, he says, that the voice must be material, so that it can hurt the passage in coming out.

> Haud igitur dubium est quin voces verbaque constent
>
> Corporeis e principiis ut lædere possint.

[5] *De Rerum Naturâ*, Lib. iv. 529.

4. *The Process of Perception of Secondary Qualities.*—The likenesses or representatives of objects by which they affect our senses were called by some writers *species*, or *sensible species*, a term which continued in use till the revival of science. It may be observed that the conception of these *species* as films cast off from the object, and retaining its shape, was different, as we have seen, from the view which Aristotle took, though it has sometimes been called the Peripatetic doctrine[6]. We may add that the expression was latterly applied to express the supposition of an emanation of

any kind, and implied little 300 more than that supposition of a Medium of which we are now speaking. Thus Bacon, after reviewing the phenomena of sound, says[7], 'Videntur motus soni fieri per *species spirituales*: ita enim loquendum donec certius quippiam inveniatur.'

[6] Brown, vol. ii. p. 98.

[7] *Hist. Son. et Aud.* vol. ix. p. 87.

Though the fundamental principles of several sciences depend upon the assumption of a Medium of Perception, these principles do not at all depend upon any special view of the Process of our perceptions. The mechanism of that process is a curious subject of consideration; but it belongs to physiology, more properly than either to metaphysics, or to those branches of physics of which we are now speaking. The general nature of the process is the same for all the senses. The object affects the appropriate intermedium; the medium, through the proper organ, the eye, the ear, the nose, affects the nerves of the particular

sense; and, by these, in some way, the sensation is conveyed to the mind, But to treat the *impression* upon the nerves as the *act* of sensation which we have to consider, would be to mistake our object, which is not the constitution of the human body, but of the human mind. It would be to mistake one link of the chain for the power which holds the end of the chain. No anatomical analysis of the corporeal conditions of vision, or hearing, or feeling warm, is necessary to the sciences of Optics, or Acoustics, or Thermotics.

Not only is this physiological research an extraneous part of our subject, but a partial pursuit of such a research may mislead the inquirer. We perceive objects *by means of* certain media, and *by means of* certain impressions on the nerves: but we cannot with propriety say that we perceive either the media or the impressions on the nerves. What person in the act of seeing is conscious of the little coloured spaces on the retina? or of the motions of the bones of the auditory apparatus whilst he is hearing?

Surely, no one. This may appear obvious enough, and yet a writer of no common acuteness, Dr. Brown, has put forth several 301 very strange opinions, all resting upon the doctrine that the coloured spaces on the retina are the *objects* which we perceive; and there are some supposed difficulties and paradoxes on the same subject which have become quite celebrated (as upright vision with inverted images), arising from the same confusion of thought.

As the consideration of the difficulties which have arisen respecting the Philosophy of Perception may serve still further to illustrate the principles on which we necessarily reason respecting the secondary qualities of bodies, I shall here devote a few pages to that subject.

CHAPTER II.

On Peculiarities in the Perceptions of the Different Senses.

1. We cannot doubt that we perceive all secondary qualities by means of immediate impressions made, through the proper medium of sensation, upon our organs. Hence all the senses are sometimes vaguely spoken of as modifications of the sense of feeling. It will, however, be seen, on reflection, that this mode of speaking identifies in words things which in our conceptions have nothing in common. No impression on the organs of touch can be conceived as having any resemblance to colour or smell. No effort, no ingenuity, can enable us to describe the impressions of one sense in terms borrowed from another.

The senses have, however, each its

peculiar powers, and these powers may be in some respects compared, so as to show their leading resemblances and differences, and the characteristic privileges and laws of each. This is what we shall do as briefly as possible.

SECT. I.—*Prerogatives of Sight.*

THE sight distinguishes colours, as the hearing distinguishes tones; the sight estimates degrees of brightness, the ear, degrees of loudness; but with several resemblances, there are most remarkable differences between these two senses.

2. *Position.*—The sight has this peculiar prerogative, that it apprehends the *place* of its objects directly and primarily. We see *where* an object is at the same instant that we see what it is. If we see two objects, we see their relative position. We cannot help 303 perceiving that one is above or below, to the right or to the left of the other, if we perceive them at all.

There is nothing corresponding to this in sound. When we hear a noise, we do not necessarily assign a place to it. It may easily happen that we cannot tell from which side a thunder-clap comes. And though we often can judge in what direction a voice is heard, this is a matter of secondary impression, and of inference from concomitant circumstances, not a primary fact of sensation. The judgments which we form concerning the position of sounding bodies are obtained by the conscious or unconscious comparison of the impressions made on the two ears, and on the bones of the head in general; they are not inseparable conditions of hearing. We may hear sounds, and be uncertain whether they are 'above, around, or underneath!' but the moment anything visible appears, however unexpected, we can say, 'see *where* it comes!'

Since we can see the relative position of things, we can see *figure*, which is but the relative position of the different parts of the boundary of the object. And thus the whole

visible world exhibits to us a scene of various shapes, coloured and shaded according to their form and position, but each having relations of position to all the rest; and altogether, entirely filling up the whole range which the eye can command.

3. *Distance.*—The distance of objects from us is no matter of immediate perception, but is a judgment and inference formed from our sensations, in something of the same way as our judgment of position by the ear, though more precise. That this is so, was most distinctly shown by Berkeley, in his *New Theory of Vision*. The elements on which we form our judgment are, the effort by which we fix both eyes on the same object, the effort by which we adjust each eye to distinct vision, and the known forms, colours, and parts of objects, as compared with their appearance. The right interpretation of the information which these circumstances give us respecting the true distances and forms of things, is gradually learnt by experience, the lesson being begun in our earliest infancy, and

inculcated upon us every hour during which we 304 use our eyes. The completeness with which the lesson is learnt is truly admirable; for we forget that our conclusion is obtained indirectly, and mistake a judgment on evidence for an intuitive perception. This, however, is not more surprizing than the rapidity and unconsciousness of effort with which we understand the meaning of the speech that we hear, or the book that we read. In both cases, the habit of interpretation is become as familiar as the act of perception. And this is the case with regard to vision. We see the breadth of the street as clearly and readily as we see the house on the other side of it. We see the house to be square, however obliquely it be presented to us. Indeed the difficulty is, to recover the consciousness of our real and original sensations;—to discover what is the *apparent* relation of the lines which appear before us. As we have already said, (book ii. chap. 6) in the common process of vision we suppose ourselves to see that which cannot be seen; and when we would make a picture of an object, the difficulty is to represent

what is visible and no more.

But perfect as is our habit of interpreting what we perceive, we could not interpret if we did not perceive. If the eye did not apprehend visible position, it could not infer actual position, which is collected from visible position as a consequence: if we did not see apparent figure, we could not arrive at any opinion concerning real form. The perception of place, which is the prerogative of the eye, is the basis of all its other superiority.

The precision with which the eye can judge of apparent position is remarkable. If we had before us two stars distant from each other by one-twentieth of the moon's diameter, we could easily decide the apparent direction of the one from the other, as above or below, to the right or left. Yet eight millions of stars might be placed in the visible hemisphere of the sky at such distances from each other; and thus the eye would recognize the relative position in a portion of its range not greater than one

eight-millionth of the whole. Such is the accuracy of the sense of vision in this 305 respect; and, indeed, we might with truth have stated it much higher. Our judgment of the position of distant objects in a landscape depends upon features far more minute than the magnitude we have here described.

As our object is to point out principally the differences of the senses, we do not dwell upon the delicacy with which we distinguish tints and shades, but proceed to another sense.

SECT. II.—*Prerogatives of Hearing.*

THE sense of hearing has two remarkable prerogatives; it can perceive a definite and peculiar relation between certain tones, and it can clearly perceive two tones together; in both these circumstances it is distinguished from vision, and from the other senses.

4. *Musical intervals.*—We perceive that two tones have, or have not, certain definite relations to each other, which we call

Concords: one sound is a *Fifth*, an *Octave*, &c., above the other. And when this is the case, our perception of the relation is extremely precise. It is easy to perceive when a fifth is out of tune by one-twentieth of a tone; that is, by one-seventieth of itself. To this there is nothing analogous in vision. Colours have certain vague relations to one another; they look well together, by contrast or by resemblance; but this is an indefinite, and in most cases a casual and variable feeling. The relation of *complementary* colours to one another, as of red to green, is somewhat more definite; but still, has nothing of the exactness and peculiarity which belongs to a musical concord. In the case of the two sounds, there is an exact point at which the relation obtains; when by altering one note we pass this point, the concord does not gradually fade away, but instantly becomes a discord; and if we go further still, we obtain another concord of quite a different character.

We learn from the theory of sound that concords occur when the times of vibration

of the notes have exact simple ratios; an octave has these times as 1 to 306 2; a fifth, as 2 to 3. According to the undulatory theory of light, such ratios occur in colours, yet the eye is not affected by them in any peculiar way. The times of the undulations of certain red and certain violet rays are as 2 to 3, but we do not perceive any peculiar harmony or connexion between those colours.

5. *Chords.*—Again, the ear has this prerogative, that it can apprehend two notes together, yet distinct. If two notes, distant by a fifth from each other, are sounded on two wind instruments, both they and their musical relation are clearly perceived. There is not a mixture, but a concord, a musical interval. In colours, the case is otherwise. If blue and yellow fall on the same spot, they form green; the colour is simple to the eye; it can no more be decomposed by the vision than if it were the simple green of the prismatic spectrum: it is impossible for us, by sight, to tell whether it is so or not.

These are very remarkable differences of the two senses: two colours can be compounded into an apparently simple one; two sounds cannot: colours pass into each other by gradations and intermediate tints; sounds pass from one concord to another by no gradations: the most intolerable discord is that which is near a concord. We shall hereafter see how these differences affect the *scales* of sound and of colour.

6. *Rhythm.*—We might remark, that as we see objects in *space*, we hear sounds in *time*; and that we thus introduce an arrangement among sounds which has several analogies with the arrangement of objects in space. But the conception of time does not seem to be peculiarly connected with the sense of hearing; a faculty of apprehending tone and time, or in musical phraseology *tune* and *rhythm*, are certainly very distinct. I shall not, therefore, here dwell upon such analogies.

The other Senses have not any peculiar prerogatives, at least none which bear on

the formation of science. I may, however, notice, in the feeling of heat, this circumstance; that it presents us with two opposites, heat and cold, which graduate into each other. This 307 is not quite peculiar, for vision also exhibits to us white and black, which are clearly opposites, and which pass into each other by the shades of gray.

SECT. III.—*The Paradoxes of Vision.*

7. *First Paradox of Vision. Upright Vision.*—All our senses appear to have this in common; That they act by means of organs, in which a bundle of nerves receives the impression of the appropriate medium of the sense. In the construction of these organs there are great differences and peculiarities, corresponding, in part at least, to the differences in the information given. Moreover, in some cases, as we have noted in the case of audible position and visible distance, that which seems to be a perception is really a judgment founded on perceptions of which we are not directly

aware. It will be seen, therefore, that with respect to the peculiar powers of each sense, it may be asked;—whether they can be explained by the construction of the peculiar organ;—whether they are acquired judgments and not direct perceptions;—or whether they are inexplicable in either of these ways, and cannot, at present at least, be resolved into anything but conditions of the intellectual act of perception.

Two of these questions with regard to vision, have been much discussed by psychological writers: the cause of our seeing objects upright by inverted images on the retina; and of our seeing single with two such images.

Physiologists have very completely explained the exquisitely beautiful mechanism of the eye, considered as analogous to an optical instrument; and it is indisputable that by means of certain transparent lenses and humours, an inverted image of the objects which are looked at is formed upon the *retina*, or fine net-work of

nerve, with which the back of the eye is lined. We cannot doubt that the impression thus produced on these nerves is essential to the act of vision; and so far as we consider the nerves 308 themselves to feel or perceive by contact, we may say that they perceive this image, or the affections of light which it indicates. But we cannot with any propriety say that *we* perceive, or that our mind perceives, this image; for we are not conscious of it, and none but anatomists are aware of its existence: we perceive *by means* of it.

A difficulty has been raised, and dwelt upon in a most unaccountable manner, arising from the neglect of this obvious distinction. It has been asked, how is it that we see an object, a man for instance, upright, when the immediate object of our sensation, the image of the man on our retina, is inverted? To this we must answer, that we see him upright *because* the image is inverted; that the inverted image is the necessary means of seeing an upright object. This is granted, and where then is the

difficulty? Perhaps it may be put thus: How is it that we do not judge the man to be inverted, since the sensible image is so? To this we may reply, that we have no notion of *upright* or inverted, except that which is founded on experience, and that all our experience, without exception, must have taught us that such a sensible image belongs to a man who is in an upright position. Indeed, the contrary judgment is not conceivable; a man is upright whose head is upwards and his feet downwards. But what are the sensible images of *upwards* and *downwards*? Whatever be our standard of up and down, the sensible representation of *up* will be an image moving on the retina towards the lower side, and the sensible representation of *down* will be a motion towards the upper side. The head of the man's image is towards the image of the sky, its feet are towards the image of the ground; how then should it appear otherwise than upright? Do we expect that the whole world should appear inverted? Be it so: but if the whole be inverted, how is the relation of the parts altered? Do we

expect that we should think our own persons in particular? This cannot be, for we look at them as we do at other objects. Do we expect that things should appear to fall 309 upwards? Surely not. For what do we know of upwards, except that it is the direction in which bodies do *not* fall? In short, the whole of this difficulty, though it has in no small degree embarrassed metaphysicians, appears to result from a very palpable confusion of ideas; from an attempt at comparison of what *we* see, with that which the retina feels, as if they were separately presentable. It is a sufficient explanation to say, that we do not see the image on the retina, but see by *means* of it. The perplexity does not require much more skill to disentangle, than it does to see that a word written in *black* ink, may signify *white*[8].

[8] The explanation of our seeing objects erect when the image is inverted has been put very simply, by saying, 'We *call* that the *lower* end of an object which is next the ground.' The observer cannot look into his own eye; he knows *by experience* what kind of image corresponds to a man in an

> upright position. The anatomist tells him that this image is *inverted*: but this does not disturb the process of judging by experience. It does not appear why any one should be perplexed at the notion of seeing objects erect by means of inverted images, rather than at the notion of seeing objects large by means of small images; or cubical and pyramidal, by means of images on a spherical surface; or green and red, by means of images on a black surface. Indeed some persons have contrived to perplex themselves with these latter questions, as well as the first.
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> The above explanation is not at all affected, as to its substance, if we adopt Sir David Brewster's expression, and say that the *line of visible direction* is a line passing through the center of the spherical surface of the retina, and therefore of course perpendicular to the surface. In speaking of 'the inverted image,' it has always been supposed to be determined by such lines; and though the point where they intersect may not have been ascertained with exactness by previous physiologists, the philosophical view of the matter was not in any degree vitiated by this imperfection.

8. *Second Paradox of Vision. Single Vision.* —(1.) *Small or Distant Objects.*—The other difficulty, why with two images on the retina we see only one object, is of a much

more real and important kind. This effect is manifestly limited by certain circumstances of a very precise nature; for if we direct our eyes at an object which is very near the eye, we see 310 all other objects double. The fact is not, therefore, that we are incapable of receiving two impressions from the two images, but that, under certain conditions, the two impressions form one. A little attention shows us that these conditions are, that with both eyes we should look at the same object; and again, we find that to look at an object with either eye, is to direct the eye so that the image falls on or near a particular point about the middle of the retina. Thus these middle points in the two retinas correspond, and we see an image single when the two images fall on the corresponding points.

Again, as each eye judges of position, and as the two eyes judge similarly, an object will be seen in the same place by one eye and by the other, when the two images which it produces are *similarly situated* with regard to the *corresponding points* of the

retina[9].

[9] The explanation of single vision with two eyes may be put in another form. Each eye judges immediately of the relative position of all objects within the field of its direct vision. Therefore when we look with both eyes at a *distant* prospect (so distant that the distance between the eyes is small in comparison) the two prospects, being similar collections of forms, will coincide altogether, if a corresponding point in one and in the other coincide. If this be the case, the two images of every object will fall upon corresponding points of the retina, and will appear single.

If the two prospects seen by the two eyes do not exactly coincide, in consequence of nearness of the objects, or distortion of the eyes, but if they nearly coincide, the stronger image of an object absorbs the weaker, and the object is seen single; yet modified by the combination, as will be seen when we speak of the single vision of near objects. When the two images of an object are considerably apart, we see it double.

This explanation is not different in substance from the one given in the text; but perhaps it is better to avoid the assertion that the law of corresponding points is ‘a distinct and original principle of our constitution,’ as I had stated in the first edition. The simpler mode of stating the law of our constitution appears to be to say, that each eye determines similarly

> the position of objects; and that when the positions of an object, as seen by the two eyes, coincide (or nearly coincide) the object is seen single.

This is the Law of Single Vision, at least so far as regards small objects; namely, objects so small that in contemplating them we consider their position only, 311 and not their solid dimensions. Single vision in such cases is a result of the law of vision simply: and it is a mistake to call in, as some have done, the influence of habit and of acquired judgments, in order to determine the result in such cases.

To ascribe the apparent singleness of objects to the impressions of vision corrected by the experience of touch[10], would be to assert that a person who had not been in the habit of handling what he saw, would see all objects double; and also, to assert that a person beginning with the double world which vision thus offers to him, would, by the continued habit of handling objects, gradually and at last learn to see them single. But all the facts of the

case show such suppositions to be utterly fantastical. No one can, in this case, go back from the habitual judgment of the singleness of objects, to the original and direct perception of their doubleness, as the draughtsman goes back from judgments to perception, in representing solid distances and forms by means of perspective pictures. No one can point out any case in which the habit is imperfectly formed; even children of the most tender age look at an object with both eyes, and see it as one.

[10] See Brown, vol. ii. p. 81.

In cases when the eyes are distorted (in squinting), one eye only is used, or if both are employed, there is double vision; and thus any derangement of the correspondence of motion in the two eyes will produce double-sightedness.

Brown is one of those[11] who assert that two images suggest a single object because we have *always found* two images to belong to a single object. He urges as an illustration, that the *two* words ‘he

conquered,' by custom excite exactly the same notion as the *one* Latin word 'vicit;' and thus that two visual images, by the effect of habit, produce the same belief of a single object as one tactual impression. But in order to make this pretended illustration of any value, it ought to be true that when a person has thoroughly learnt the Latin language, he can no longer distinguish 312 any separate meaning in 'he' and in 'conquered.' We can by no effort perceive the double sensation, when we look *at* the object with the two eyes. Those who squint, learn by habit to see objects single: but the habit which they acquire is that of attending to the impressions of one eye only at once, not of combining the two impressions. It is obvious, that if each eye spreads before us the same visible scene, with the same objects and the same relations of place, then, if one object in each scene coincide, the whole of the two visible impressions will be coincident. And here the remarkable circumstance is, that not only each eye judges for itself of the relations of position which come within its field of view; but that

there is a superior and more comprehensive faculty which combines and compares the two fields of view; which asserts or denies their coincidence; which contemplates, as in a relative position to one another, these two visible worlds, in which all other relative position is given. This power of confronting two sets of visible images and figured spaces before a purely intellectual tribunal, is one of the most remarkable circumstances in the sense of vision.

[11] *Lectures*, vol. ii. p. 81.

9. (2.) *Near Objects.*—We have hitherto spoken of the singleness of objects whose images occupy corresponding positions on the retina of the two eyes. But here occurs a difficulty. If an object of moderate size, a small thick book for example, be held at a little distance from the eyes, it produces an image on the retina of each eye; and these two images are perspective representations of the book from different points of view, (the positions of the two eyes,) and are therefore of different forms. Hence the two images cannot occupy corresponding points

of the retina throughout their whole extent. If the central parts of the two images occupy corresponding points, the boundaries of the two wall not correspond. How is it then consistent with the law above stated that in this case the object appears single?

It may be observed, that the two images in such a case will differ most widely when the object is not a 313 mere surface, but a solid. If a book, for example, be held with one of its upright edges towards the face, the right eye will see one side more directly than the left eye, and the left eye will see another side more directly, and the outline of the two images upon the two retinas will exhibit this difference. And it may be further observed, that this difference in the images received by the two eyes, is a plain and demonstrative evidence of the solidity of the object seen; since nothing but a solid object could (without some special contrivance) produce these different forms of the images in the two eyes.

Hence the absence of exact coincidence in

the two images on the retina is the necessary condition of the solidity of the object seen, and must be one of the indications by means of which our vision apprehends an object as solid. And that this is so, Mr. Wheatstone has proved experimentally, by means of some most ingenious and striking contrivances. He has devised[12] an instrument (the *stereoscope*) by which two images (drawn in outline) differing exactly as much as the two images of a solid body seen near the face would differ, are conveyed, one to one eye, and the other to the other. And it is found that when this is effected, the object which the images represent is not only seen single, but is apprehended as solid with a clearness and reality of conviction quite distinct from any impression which a mere perspective representation can give.

[12] *Phil. Trans.* 1839.

At the same time it is found that the object is then only apprehended as single when the two images are such as are capable of being excited by one single

object placed in solid space, and seen by the two eyes. If the images differ more or otherwise than this condition allows, the result is, that both are seen, their lines crossing and interfering with one another.

It may be observed, too, that if an object be of such large size as not to be taken in by a single glance of the eyes, it is no longer apprehended as single by a direct act of perception; but its parts are looked at 314 separately and successively, and the impressions thus obtained are put together by a succeeding act of the mind. Hence the objects which are directly seen as solid, will be of moderate size; in which case it is not difficult to show that the outlines of the two images will differ from each other only slightly.

Hence we are led to the following, as the Law of Single Vision for *near* objects:— When the two images in the two eyes are situated (part for part) nearly, but not exactly, upon corresponding points, the object is apprehended as single, if the two

images are such as are or would be given by a single solid object seen by the two eyes separately: and in this case the object is necessarily apprehended as solid.

This law of vision does not contradict that stated above for distant objects: for when an object is removed to a considerable distance, the images in the two eyes coincide exactly, and the object is seen as single, though without any direct apprehension of its solidity. The first law is a special case of the second. Under the condition of *exactly* corresponding points, we have the perception of singleness, but no evidence of solidity. Under the condition of *nearly* corresponding points, we may have the perception of singleness, and with it, of solidity.

We have before noted it as an important feature in our visual perception, that while we have two distinct impressions upon the sense, which we can contemplate separately and alternately, (the impressions on the two eyes,) we have a higher perceptive faculty

which can recognize these two impressions, exactly similar to each other, as only two images of one and the same assemblage of objects. But we now see that the faculty by which we perceive visible objects can do much more than this:—it can not only unite two impressions, and recognize them as belonging to one object in virtue of their coincidence, but it can also unite and identify them, even when they do not exactly coincide. It can correct and adjust their small difference, so that they are both apprehended as representations of the same figure. It can infer from them a real form, not 315 agreeing with either of them; and a solid space, which they are quite incapable of exemplifying. The visual faculty decides whether or not the two ocular images can be pictures of the same solid object, and if they can, it undoubtingly and necessarily accepts them as being so. This faculty operates as if it had the power of calling before it all possible solid figures, and of ascertaining by trial whether any of those will, at the same time, fit both the outlines which are given by the sense. It assumes the

reality of solid space, and, if it be possible, reconciles the appearances with that reality. And thus an activity of the mind of a very remarkable and peculiar kind is exercised in the most common act of seeing.

10. It may be said that this doctrine, of such a visual faculty as has been described, is very vague and obscure, since we are not told what are its limits. It adjusts and corrects figures which *nearly* coincide, so as to identify them. But *how* nearly, it may be asked, must the figures approach each other, in order that this adjustment may be possible? What discrepance renders impossible the reconcilement of which we speak? Is it not impossible to give a definite answer to these questions, and therefore impossible to lay down definitely such laws of vision as we have stated? To this I reply, that the indefiniteness thus objected to us, is no new difficulty, but one with which philosophers are familiar, and to which they are already reconciled. It is, in fact, no other than the indefiniteness of the limits of distinct vision. How near to the face must

an object be brought, so that we shall cease to see it distinctly? The distance, it will be answered, is indefinite: it is different for different persons; and for the same person, it varies with the degree of effort, attention, and habit. But this indefiniteness is only the indefiniteness, in another form, of the deviation of the two ocular images from one another: and in reply to the question concerning them we must still say, as before, that in doubtful cases, the power of apprehending an object as single, when this *can* be done, will vary with effort, attention, and habit. The assumption 316 that the apparent object exists as a real figure, in real space, is to be verified, if possible; but, in extreme cases, from the unfitness of the point of view, or from any other cause of visual confusion or deception, the existence of a real object corresponding to the appearance may be doubtful; as in any other kind of perception it may be doubtful whether our senses, under disadvantageous circumstances, give us true information. The vagueness of the limits, then, within which this visual faculty can be successfully

exercised, is no valid argument against the existence of the faculty, or the truth of the law which we have stated concerning its action.

SECT. IV.—*The Perception of Visible Figure.*

11. *Visible Figure.*—There is one tenet on the subject of vision which appears to me so extravagant and unphilosophical, that I should not have thought it necessary to notice it, if it had not been recently promulgated by a writer of great acuteness in a book which has obtained, for a metaphysical work, considerable circulation. I speak of Brown's opinion[13] that we have no immediate perception of visible figure. I confess myself unable to comprehend fully the doctrine which he would substitute in the place of the one commonly received. He states it thus[14]: 'When the simple affection of sight is blended with the ideas of suggestion [those arising from touch, &c.] in what are termed the acquired perceptions of vision, as, for example, in the perception of a sphere, it is

colour only which is blended with the large convexity, and not a small coloured plane.' The doctrine which Brown asserts in this and similar passages, appears to be, that we do not by vision perceive *both* colour and *figure*; but that the colour which we see is blended with the figure which we learn the existence of by other means, as by touch. But if this were possible when we can call in other perceptions, how is it possible when we cannot or do not touch the object? 317 Why does the moon appear round, gibbous, or horned? What sense besides vision suggests to us the idea of her figure? And even in objects which we can reach, what is that circumstance in the sense of vision which suggests to us that the colour belongs to the sphere, except that we see the colour where we see the sphere? If we do not see figure, we do not see position; for figure is the relative position of the parts of a boundary. If we do not see position, why do we ascribe the yellow colour to the sphere on our left, rather than to the cube on our right? We *associate* the colour with the object, says Dr. Brown; but if his opinion

were true, we could not associate two colours with two objects, for we could not apprehend the colours as occupying two different places.

[13] *Lectures*, vol. ii. p. 82.

[14] *Ib.* vol. ii. p. 90.

The whole of Brown's reasoning on this subject is so irreconcilable with the first facts of vision, that it is difficult to conceive how it could proceed from a person who has reasoned with great acuteness concerning touch. In order to prove his assertion, he undertakes to examine the only reasons which, he says[15], he can imagine for believing the immediate perception of visible figure: (1) That it is absolutely impossible, in our present sensations of sight, to separate colour from extension; and (2) That there are, in fact, figures on the retina corresponding to the apparent figures of objects.

[15] *Lectures*, vol. ii. p. 83.

On the subject of the first reason, he says,

that the figure which we perceive as associated with colour, is the real, and not the apparent figure. ‘Is there,’ he asks, ‘the slightest consciousness of a perception of visible figure, corresponding to the affected portion of the retina?’ To which, though he seems to think an affirmative answer impossible, we cannot hesitate to reply, that there is undoubtedly such a consciousness; that though obscured by being made the ground of habitual inference as to the real figure, this consciousness is constantly referred to by the draughtsman, and easily recalled by any one. We may separate colour, he says 318 again[16], from the figures on the retina, as we may separate it from length, breadth, and thickness, which we do not see. But this is altogether false: we cannot separate colour from length, breadth, and thickness, *in any other way*, than by transferring it to the visible figure which we do see. He cannot, he allows, separate the colour from the visible form of the trunk of a large oak; but just as little, he thinks, can he separate it from the convex mass of the trunk, which (it is allowed on

all hands) he does not immediately see. But in this he is mistaken: for if he were to make a *picture* of the oak, he would separate the colour from the convex shape, which he does not imitate, but he could not separate it from the visible figure, which he does imitate; and he would then perceive that the fact that he *has not* an immediate perception of the convex form, is necessarily connected with the fact that he *has* an immediate perception of the apparent figure; so far is the rejection of immediate perception in the former case from being a reason for rejecting it in the latter.

[16] *Lectures*, vol. ii. p. 84.

Again, with regard to the second argument. It does not, he says, follow, that because a certain figured portion of the retina is affected by light, we should see such a figure; for if a certain figured portion of the olfactory organ were affected by odours, we should not acquire by smell any perception of such figure[17]. This is merely to say, that because we do not perceive position and figure by one sense, we cannot

do so by another sense. But this again is altogether erroneous. It is an office of our sight to inform us of position, and consequently of figure; for this purpose, the organ is so constructed that the position of the object determines the position of the point of the retina affected. There is nothing of this kind in the organ of smell; objects in different positions and of different forms do not affect different parts of the olfactory nerve, or portions of different shape. Different objects, remote from each other, if perceived by smell, affect the same 319 part of the olfactory organs. This is all quite intelligible; for it is not the office of smell to inform us of position. Of what use or meaning would be the curious and complex structure of the eye, if it gave us only such vague and wandering notions of the colours and forms of the flowers in a garden, as we receive from their odours when we walk among them blindfold? It is, as we have said, the *prerogative* of vision to apprehend position: the places of objects on the retina give this information. We do not suppose that the affection of a certain shape of

nervous expanse will necessarily and in all cases give us the impression of figure; but we know that in vision it does; and it is clear that if we did not acquire our acquaintance with visible figure in this way, we could not acquire it in any way[18].

[17] *Ib.* p. 87.

[18] When Brown says further (p. 87), that we can indeed show the image in the dissected eye; but that 'it is not in the dissected eye that vision takes place;' it is difficult to see what his drift is. Does he doubt that there is an image formed in the living as completely as in the dissected eye?

The whole of this strange mistake of Brown's appears to arise from the fault already noticed;—that of considering the image on the retina as the *object* instead of the *means* of vision. This indeed is what he says: 'the true object of vision is not the distant body itself, but the light that has reached the expansive termination of the optic nerve[19].' Even if this were so, we do not see why we should not perceive the position of the impression on this expanded

nerve. But as we have already said, the impression on the nerve is the means of vision, and enables us to assign a place, or at least a direction, to the object from which the light proceeds, and thus makes vision possible. Brown, indeed, pursues his own peculiar view till he involves the subject in utter confusion. Thus he says[20], 'According to the common theory [that figure can be perceived by the eye,] a visible sphere is at once to my perception convex and plane; and if the sphere be a one, it is perceived at once to be a sphere of 320 many feet in diameter, and a plane circular surface of the diameter of a quarter of an inch.' It is easy to deduce these and greater absurdities, if we proceed on his strange and baseless supposition that the object and the image on the retina are *both* perceived. But who is conscious of the image on the retina in any other way than as he sees the object by means of it?

[19] *Lectures*, vol. ii. p. 57.

[20] *Ib.* vol. ii. p. 89.

Brown seems to have imagined that he was analysing the perception of figure ‘in the same manner in which Berkeley had analyzed the perception of distance. He ought to have recollected that such an undertaking, to be successful, required him to show *what* elements he analyzed it *into*. Berkeley analyzed the perception of real figure into the interpretation of visible figure according to certain rules which he distinctly stated. Brown analyzes the perception of visible figure into no elements. Berkeley says, that we do not directly perceive distance, but that we perceive something else, from which we infer distance, namely, visible figure and colour, and our own efforts in seeing; Brown says, that we do not see figure, but infer it; what then do we see, which we infer it from? To this he offers no answer. He asserts the seeming perception of visible figure to be a result of ‘association;’—of ‘suggestion.’ But what meaning can we attach to this? Suggestion requires something which suggests; and not a hint is given what it is which suggests position.

Association implies two things associated; what is the sensation which we associate with form? What is that visual perception which is not figure, and which we mistake for figure? What perception is it that suggests a square to the eye? What impressions are those which have been associated with a visible triangle, so that the revival of the impressions revives the notion of the triangle? Brown has nowhere pointed out such perceptions and impressions; nor indeed was it possible for him to do so; for the only visual perceptions which he allows to remain, those of colour, most assuredly do not suggest visible figures by their differences; red is not associated with square rather than with round, or with round rather than square. On the contrary, the 321 eye, constructed in a very complex and wonderful manner in order that it may give to us directly the perception of position as well as of colour, has it for one of its prerogatives to give us this information; and the perception of the relative position of each part of the visible boundary of an object constitutes the perception of its

apparent figure; which faculty we cannot deny to the eye without rejecting the plain and constant evidence of our senses, making the mechanism of the eye unmeaning, confounding the object with the means of vision, and rendering the mental process of vision utterly unintelligible.

Having sufficiently discussed the processes of perception, I now return to the consideration of the Ideas which these processes assume.

CHAPTER III.

Successive Attempts at the Scientific Application of the Idea of a Medium.

1. In what precedes, we have shown by various considerations that we necessarily and universally assume the perception of secondary qualities to take place by means of a medium interjacent between the object and the person perceiving. Perception is affected by various peculiarities, according to the nature of the quality perceived: but in all cases a medium is equally essential to the process.

This principle, which, as we have seen, is accepted as evident by the common understanding of mankind, is confirmed by all additional reflection and discipline of the mind, and is the foundation of all the theories which have been proposed

concerning the processes by which the perception takes place, and concerning the modifications of the qualities thus perceived. The medium, and the mode in which the impression is conveyed through the medium, seem to be different for different qualities; but the existence of the medium leads to certain necessary conditions or alternatives, which have successively made their appearance in science, in the course of the attempts of men to theorize concerning the principal secondary qualities, sound, light, and heat. We must now point out some of the ways, at first imperfect and erroneous, in which the consequences of the fundamental assumption were traced.

2. *Sound.*—In all cases the medium of sensation, whatever it is, is supposed to produce the effect of conveying secondary qualities to our perception by means of its primary qualities. It was conceived to operate 323 by the size, form, and motion of its parts. This is a fundamental principle of the class of sciences of which we have at

present to speak.

It was assumed from the first, as we have seen in the passage lately quoted from Aristotle[21], that in the conveyance of *sound*, the medium of communication was the air. But although the first theorists were right so far, that circumstance did not prevent their going entirely wrong when they had further to determine the nature of the process. It was conceived by Aristotle that the air acted after the manner of a rigid body;—like a staff, which, receiving an impulse at one end, transmits it to the other. Now this is altogether an erroneous view of the manner in which the air conveys the impulse by which sound is perceived. An approach was made to the true view of this process, by assimilating it to the diffusion of the little circular waves which are produced on the surface of still water when a stone is dropt into it. These little waves begin from the point thus disturbed, and run outwards, expanding on every side, in concentric circles, till they are lost. The propagation of sound through the air from the point where

it is produced, was compared by Vitruvius to this diffusion of circular waves in water; and thus the notion of a propagation of impulse by the *waves* of a fluid was introduced, in the place of the former notion of the impulse of an unyielding body.

[21] *Supr.* p. 297.

But though, taking an enlarged view of the nature of the progress of a wave, this is a just representation of the motion of air in conveying sound, we cannot suppose that the process was, at the period of which we speak, rightly understood. For the waves of water were contemplated only as affecting the surface of the water; and as the air has no surface, the communication must take place by means of an internal motion, which can bear only a remote and obscure resemblance to the waves which we see. And even with regard to the waves of water, the mechanism by which they are 324 produced and transferred was not at all understood; so that the comparison employed by Vitruvius must be considered rather as a loose analogy than as an exact

scientific explanation.

No correct account of such motions was given, till the formation of the science of Mechanics in modern times had enabled philosophers to understand more distinctly the mode in which motion is propagated through a fluid, and to discern the forces which the process calls into play, so as to continue the motion once begun. Newton introduced into this subject the exact and rigorous conception of an *Undulation*, which is the true key to the explanation of impulses conveyed through a fluid.

Even at the present day, the right apprehension of the nature of an Undulation transmitted through a fluid is found to be very difficult for all persons except those whose minds have been duly disciplined by mathematical studies. When we see a wave run along the surface of water, we are apt to imagine at first that a portion of the fluid is transferred bodily from one place to another. But with a little consideration we may easily satisfy ourselves that this is not

so: for if we look at a field of standing corn, when a breeze blows over it, we see waves like those of water run along its surface. Yet it is clear that in this case the separate stalks of corn only bend backwards and forwards, and no portion of the grain is really conveyed from one part of the field to the other. This is obvious even to popular apprehension. The poet speaks of

> . . . . The rye,
>
> That stoops its head when whirlwinds rave
>
> And springs again in eddying wave
>
> As each wild gust sweeps by.

Each particle of the mass in succession has a small motion backwards and forwards; and by this means a large ridge made by many such particles runs along the mass to any distance. This is the true conception of an undulation in general.

Thus, when an Undulation is propagated in a fluid, it is not *matter*, but *form*, which is transmitted from 325 one place to another.

The particles along the line of each wave assume a certain arrangement, and this arrangement passes from one part to another, the particles changing their places only within narrow limits, so as to lend themselves successively to the arrangements by which the successive waves, and the intervals between the waves, are formed.

When such an Undulation is propagated through air, the wave is composed, not, as in water, of particles which are higher than the rest, but of particles which are closer to each other than the rest. The wave is not a ridge of elevation, but a line of condensation; and as in water we have alternately elevated and depressed lines, we have in air lines alternately condensed and rarefied. And the motion of the particles is not, as in water, up and down, in a direction transverse to that of the wave which runs forwards; in the motion of an undulation through air the motion of each particle is alternately forwards and backwards, while the motion of the undulation is constantly forwards.

This precise and detailed account of the Undulatory Motion of air by which sound is transmitted was first given by Newton. He further attempted to determine the motions of the separate particles, and to point out the force by which each particle affects the next, so as to continue the progress of the undulation once begun. The motions of each particle must be oscillatory; he assumed the oscillations to be governed by the simplest law of oscillation which had come under the notice of mathematicians, (that of small vibrations of a pendulum;) and he proved that in this manner the forces which are called into play by the contraction and expansion of the parts of the elastic fluid are such as the continuance of the motion requires.

Newton’s proof of the exact law of Oscillatory Motion of the aërial particles was not considered satisfactory by succeeding mathematicians; for it was found that the same result, the development of forces adequate to continue the motion, would follow if any other law of the motion

were assumed. Cramer proved this by a sort of *parody* on Newton's proof, in which, by the 326 alteration of a few phrases in this formula of demonstration, it was made to establish an entirely different conclusion.

But the general conception of an Undulation as presented by Newton was, as from its manifest mechanical truth it could not fail to be, accepted by all mathematicians; and in proportion as the methods of calculating the motions of fluids were further improved, the necessary consequences of this conception, in the communication of sound through air, were traced by unexceptionable reasoning. This was especially done by Euler and Lagrange, whose memoirs on such motions of fluids are some of the most admirable examples which exist, of refined mathematical methods applied to the solution of difficult mechanical problems.

But the great step in the formation of the theory of sound was undoubtedly that which we have noticed, the introduction of

the Conception of an Undulation such as we have attempted to describe it:—a state, condition, or arrangement of the particles of a fluid, which is transferred from one part of space to another by means of small motions of the particles, altogether distinct from the movement of the Undulation itself. This is a conception which is not obvious to common apprehension. It appears paradoxical at first sight to speak of a large *wave* (as the tide-wave) running up a river at the rate of twenty miles an hour, while the *stream* of the river is all the while flowing downwards. Yet this is a very common fact. And the conception of such a motion must be fully mastered by all who would reason rightly concerning the mechanical transmission of impressions through a medium.

We have described the motion of sound as produced by small motions of the particle forwards and backwards, while the waves, or condensed and rarefied lines, move constantly forwards. It may be asked what right we have to suppose the motion to be

of this kind, since when sound is heard, no such motions of the particles of air can be observed, even by refined methods of observation. Thus Bacon declares himself against the hypothesis of such a vibration, since, as he remarks, it 327 cannot be perceived in any visible impression upon the flame of a candle. And to this we reply, that the supposition of this Vibration is made in virtue of a principle which is involved in the original assumption of a medium; namely, That *a Medium, in conveying Secondary qualities, operates by means of its Primary qualities*, the bulk, figure, motion, and other mechanical properties of its parts. This is an Axiom belonging to the Idea of a Medium. In virtue of this axiom it is demonstrable that the motion of the air, when any how disturbed, must be such as is supposed in our acoustical reasonings. For the elasticity of the parts of the air, called into play by its expansion and contraction, lead, by a mechanical necessity, to such a motion as we have described. We may add that, by proper contrivances, this motion may be

made perceptible in its visible effects. Thus the theory of sound, as an impression conveyed through air, is established upon evident general principles, although the mathematical calculations which are requisite to investigate its consequences are, some of them, of a very recondite kind.

3. *Light.*—The early attempts to explain Vision represented it as performed by means of material rays proceeding *from* the eye, by the help of which the eye felt out the form and other visible qualities of an object, as a blind man might do with his staff. But this opinion could not keep its ground long: for it did not even explain the fact that light is necessary to vision. Light, as a peculiar medium, was next assumed as the machinery of vision; but the mode in which the impression was conveyed through the medium was left undetermined, and no advance was made towards sound theory, on that subject, by the ancients.

In modern times, when the prevalent philosophy began to assume a mechanical

turn (as in the theories of Descartes), light was conceived to be a material substance which is emitted from luminous bodies, and which is also conveyed from all bodies to the eye, so as to render them visible. The various changes of direction by which the rays of light are affected, (reflection, 328 refraction, &c.,) Descartes explained, by considering the particles of light as small globules, which change their direction when they impinge upon other bodies, according to the laws of Mechanics. Newton, with a much more profound knowledge of Mechanics than Descartes possessed, adopted, in the most mature of his speculations, nearly the same view of the nature of light; and endeavoured to show that reflection, refraction, and other properties of light, might be explained as the effects which certain forces, emanating from the particles of bodies, produce upon the luminiferous globules.

But though some of the properties of light could thus be accounted for by the assumption of particles emitted from

luminous bodies, and reflected or refracted by forces, other properties came into view which would not admit of the same explanation. The phenomena of *diffraction* (the fringes which accompany shadows) could never be truly represented by such an hypothesis, in spite of many attempts which were made. And the *colours of thin plates*, which show the rays of light to be affected by an alternation of two different conditions at small intervals along their length, led Newton himself to incline, often and strongly, to some hypothesis of undulation. The *double refraction* of Iceland spar, a phenomenon in itself very complex, could, it was found by Huyghens, be expressed with great simplicity by a certain hypothesis of undulations.

Two hypotheses of the nature of the luminiferous medium were thus brought under consideration; the one representing Light as Matter emitted from the luminous object, the other, as Undulations propagated through a fluid. These two hypotheses remained in presence of each other during

the whole of the last century, neither of them gaining any material advantage over the other, though the greater part of mathematicians, following Newton, embraced the emission theory. But at the beginning of the present century, an additional class of phenomena, those of the *interference* of two rays of light, were brought under 329 consideration by Dr. Young; and these phenomena were strongly in favour of the undulatory theory, while they were irreconcilable with the hypothesis of emission. If it had not been for the original bias of Newton and his school to the other side, there can be little doubt that from this period light as well as sound would have been supposed to be propagated by undulations; although in this case it was necessary to assume as the vehicle of such undulations a special medium or *ether*. Several points of the phenomena of vision no doubt remained unexplained by the undulatory theory, as absorption, and the natural colours of bodies; but such facts, though they did not confirm, did not evidently contradict the theory of a

Luminiferous Ether; and the facts which such a theory did explain, it explained with singular happiness and accuracy.

But before this Undulatory Theory could be generally accepted, it was presented in an entirely new point of view by being combined with the facts of *polarization*. The general idea of polarization must be illustrated hereafter; but we may here remark that Young and Fresnel, who had adopted the undulatory theory, after being embarrassed for some time by the new facts which were thus presented to their notice, at last saw that these facts might be explained by conceiving the vibrations to be transverse to the ray, the motions of the particles being not backwards and forwards in the line in which the impulse travels, but to the right and left of that line. This conception of *transverse vibrations*, though quite unforeseen, had nothing in it which was at all difficult to reconcile with the general notion of an undulation. We have described an undulation, or wave, as a certain condition or arrangement of the

particles of the fluid successively transferred from one part of space to another: and it is easily conceivable that this arrangement or wave may be produced by a lateral transfer of the particles from their quiescent positions. This conception of transverse vibrations being accepted, it was found that the explanation of the phenomena of polarization and of those of interference led to the same theory 330 with a correspondence truly wonderful; and this coincidence in the views, collected from two quite distinct classes of phenomena, was justly considered as an almost demonstrative evidence of the truth of this undulatory theory.

It remained to be considered whether the doctrine of transverse vibrations in a fluid could be reconciled with the principles of Mechanics. And it was found that by making certain suppositions, in which no inherent improbability existed, the hypothesis of transverse vibrations would explain the laws, both of interference and of polarization of light, in air and in crystals of

all kinds, with a surprizing fertility and fidelity.

Thus the Undulatory Theory of Light, like the Undulatory Theory of Sound, is recommended by its conformity to the fundamental principle of the Secondary Mechanical Sciences, that the medium must be supposed to transmit its peculiar impulses according to the laws of Mechanics. Although no one had previously dreamt of qualities being conveyed through a medium by such a process, yet when it is once suggested as the only mode of explaining some of the phenomena, there is nothing to prevent our accepting it entirely, as a satisfactory theory for all the known laws of Light.

4. *Heat.*—With regard to Heat as with regard to Light, a fluid medium was necessarily assumed as the vehicle of the property. During the last century, this medium was supposed to be an emitted fluid. And many of the ascertained Laws of Heat, those which prevail with regard to its

radiation more especially, were well explained by this hypothesis[22]. Other effects of heat, however, as for instance *latent heat*[23], and the change of *consistence* of bodies[24], were not satisfactorily brought into connexion with the hypothesis; while 331 *conduction*[25], which at first did not appear to result from the fundamental assumption, was to a certain extent explained as internal radiation.

[22] See the Account of the Theory of Exchanges, *Hist. Ind. Sc.* b. x. c. i. sect. 2.

[23] *Ib.* c. ii. sect. 3.

[24] *Ib.* c. ii. sect. 2.

[25] *Ib.* c. i. sect. 7.

But it was by no means clear that an Undulatory Theory of Heat might not be made to explain these phenomena equally well. Several philosophers inclined to such a theory; and finally, Ampère showed that the doctrine that the heat of a body consists in the undulations of its particles propagated by means of the undulations of a medium, might be so adjusted as to explain all which

the theory of emission could explain, and moreover to account for facts and laws which were out of the reach of that theory. About the same time it was discovered by Prof. Forbes and M. Nobili that radiant heat is, under certain circumstances, polarized. Now polarization had been most satisfactorily explained by means of transverse undulations in the case of light; while all attempts to modify the emission theory so as to include polarization in it, had been found ineffectual. Hence this discovery was justly considered as lending great countenance to the opinion that Heat consists in the vibrations of its proper medium.

But what is this medium? Is it the same by which the impressions of Light are conveyed? This is a difficult question; or rather it is one which we cannot at present hope to answer with certainty. No doubt the connexion between Light and Heat is so intimate and constant, that we can hardly refrain from considering them as affections of the same medium. But instead of

attempting to erect our systems on such loose and general views of connexion, it is rather the business of the philosophers of the present day to determine the laws of the operation of heat, and its real relation to light, in order that we may afterwards be able to connect the theories of the two qualities. Perhaps in a more advanced state of our knowledge we may be able to state it as an Axiom, that two Secondary Qualities, which are intimately connected in their causes and effects, must be affections of the same Medium. 332 But at present it does not appear safe to proceed upon such a principle, although many writers, in their speculations both concerning Light and Heat, and concerning other properties, have not hesitated to do so.

Some other consequences follow from the Idea of a Medium which must be the subject of another chapter.

CHAPTER IV.

Of the Measure of Secondary Qualities.

Sect. I.—*Scales of Qualities in general.*

THE ultimate object of our investigation in each of the Secondary Mechanical Sciences, is the nature of the processes by which the special impressions of sound, light, and heat, are conveyed, and the modifications of which these processes are susceptible. And of this investigation, as we have seen, the necessary basis is the principle, that these impressions are transmitted by means of a medium. But before we arrive at this ultimate object, we may find it necessary to occupy ourselves with several intermediate objects: before we discover the *cause*, it may be necessary to determine the *laws* of the

phenomena. Even if we cannot immediately ascertain the mechanism of light or heat, it may still be interesting and important to arrange and measure the effects which we observe.

The idea of a Medium affects our proceeding in this research also. We cannot measure Secondary qualities in the same manner in which we measure Primary qualities, by a mere addition of parts. There is this leading and remarkable difference, that while both classes of qualities are susceptible of changes of magnitude, primary qualities increase by addition of *extension*, secondary, by augmentation of *intensity*. A space is doubled when another equal space is placed by its side; one weight joined to another makes up the sum of the two. But when one degree of warmth is combined with another, or one shade of red colour with another, we cannot in like manner talk of the *sum*. The component parts do not evidently retain their 334 separate existence; we cannot separate a strong green colour into two weaker ones,

as we can separate a large force into two smaller. The increase is absorbed into the previous amount, and is no longer in evidence as a part of the whole. And this is the difference which has given birth to the two words *extended*, and *intense*. That is extended which has 'partes extra partes,' parts outside of parts: that is intense which becomes stronger by some indirect and unapparent increase of agency, like the stretching of the internal springs of a machine, as the term *intense* implies. Extended magnitudes can at will be resolved into the parts of which they were originally composed, or any other which the nature of their extension admits; their proportion is apparent; they are directly and at once subject to the relations of number. Intensive magnitudes cannot be resolved into smaller magnitudes; we can see that they differ, but we cannot tell in what proportion; we have no direct measure of their quantity. How many times hotter than blood is boiling water? The answer cannot be given by the aid of our feelings of heat alone.

The difference, as we have said, is connected with the fundamental principle that we do not perceive Secondary qualities directly, but through a Medium. We have no natural apprehension of light, or sound, or heat, as they exist in the bodies from which they proceed, but only as they affect our organs. We can only measure them, therefore, by some *Scale* supplied by their effects. And thus while extended magnitudes, as space, time, are measurable directly and of themselves; intensive magnitudes, as brightness, loudness, heat, are measurable only by artificial means and conventional scales. Space, time, measure themselves: the repetition of a smaller space, or time, while it composes a larger one, measures it. But for light and heat we must have Photometers and Thermometers, which measure something which is assumed to be an indication of the quality in question. In the one case, the mode of applying the measure, and the meaning of the number resulting, are seen by intuition; in the 335 other, they are consequences of assumption and reasoning. In the one case,

they are *Units*, of which the extension is made up; in the other, they are *Degrees* by which the intensity ascends.

2. When we discover any property in a sensible quality, which at once refers us to number or space, we readily take this property as a measure; and thus we make a transition from quality to quantity. Thus Ptolemy in the third chapter of the First Book of his *Harmonics* begins thus: 'As to the differences which exist in sounds both in *quality* and in *quantity*, if we consider that difference which refers to the acuteness and graveness, we cannot at once tell to which of the above two classes it belongs, till we have considered the causes of such symptoms.' But at the end of the chapter, having satisfied himself that grave sounds result from the magnitude of the string or pipe, other things being equal, he infers, 'Thus the difference of acute and grave appears to be a difference of *quantity*.'

In the same manner, in order to form Secondary Mechanical Sciences respecting

any of the other properties of bodies, we must reduce these properties to a dependence upon quantity, and thus make them subject to measurement. We cannot obtain any sciential truths respecting the comparison of sensible qualities, till we have discovered measures and scales of the qualities which we have to consider; and accordingly, some of the most important steps in such sciences have been the establishment of such measures and scales, and the invention of the requisite instruments.

The formation of the mathematical sciences which rest upon the measures of the intensity of sensible qualities took place mainly in the course of the last century. Perhaps we may consider Lambert, a mathematician who resided in Switzerland, and published about 1750, as the person who first clearly felt the importance of establishing such sciences. His Photometry, Pyrometry, and Hygrometry, are examples of the systematic reduction of sensible qualities (light, heat, moisture) to modes of

numerical measurement. 336

We now proceed to speak of such modes of measurement with regard to the most obvious properties of bodies.

SECT. II.—*The Musical Scale.*

3. THE establishment of the *Harmonic Canon*, that is, of a Scale and Measure of the musical place of notes, in the relation of *high* and *low*, was the first step in the science of Harmonics. The perception of the differences and relations of musical sounds is the office of the sense of hearing; but these relations are fixed, and rendered accurately recognizable by artificial means. 'Indeed, in all the senses,' as Ptolemy truly says in the opening of his Harmonics, 'the sense discovers what is approximately true, and receives accuracy from another quarter: the reason receives the approximately-true from another quarter, and discovers the accurate truth.' We can have no measures of sensible qualities which do not ultimately refer to the sense;—whether they do this

immediately, as when we refer Colours to an assumed Standard; or mediately, as when we measure Heat by Expansion, having previously found by an appeal to sense that the expansion increases with the heat. Such relations of sensible qualities cannot be described in words, and can only be apprehended by their appropriate faculty. The faculty by which the relations of sounds are apprehended is a *musical ear* in the largest acceptation of the term. In this signification the faculty is nearly universal among men; for all persons have musical ears sufficiently delicate to understand and to imitate the modulations corresponding to various emotions in speaking; which modulations depend upon the succession of acuter and graver tones. These are the relations now spoken of, and these are plainly perceived by persons who have very imperfect musical ears, according to the common use of the phrase. But the relations of tones which occur in speaking are somewhat indefinite; and in forming that musical scale which is the basis of our science upon the subject, we 337 take the

most definite and marked of such relations of notes; such as occur, not in speaking but in singing. Those musical relations of two sounds which we call the *octave*, the *fifth*, the *fourth*, the *third*, are recognized after a short familiarity with them. These *chords* or *intervals* are perceived to have each a peculiar character, which separates them from the relations of two sounds taken at random, and makes it easy to know them when sung or played on an instrument; and for most persons, not difficult to sing the sounds in succession exactly, or nearly correct. These musical relations, or *concords*, then, are the groundwork of our musical series of sounds. But how are we to name these indescribable sensible characters? how to refer, with unerring accuracy, to a type which exists only in our own perceptions? We must have for this purpose a *Scale* and a *Standard*.

The Musical Scale is a series of eight notes, ascending by certain steps from the first or key-note to the octave above it, each of the notes being fixed by such

distinguishable musical relations as we have spoken of above. We may call these notes C, D, E, F, G, A, B, *c*; and we may then say that G is determined by its being a fifth above C; D by its being a fourth below G; E by its being a third above C; and similarly of the rest. It will be recollected that the terms a *fifth*, a *fourth*, a *third*, have hitherto been introduced as expressing certain simple and indescribable musical relations among sounds, which might have been indicated by any other names. Thus we might call the fifth the *dominant*, and the fourth the *subdominant*, as is done in one part of musical science. But the names we have used, which are the common ones, are in fact derived from the number of notes which these intervals include in the scale obtained in the above manner. The notes, C, D, E, F, G, being five, the interval from C to G is a fifth, and so of the rest. The fixation of this scale gave the means of describing exactly any note which occurs in the scale, and the method is easily applicable to notes above and below this range; for in a series of sounds higher or lower by an octave than

338 this standard series, the ear discovers a recurrence of the same relations so exact, that a person may sometimes imagine he is producing the same notes as another when he is singing the same air an octave higher. Hence the next eight notes may be conveniently denoted by a repetition of the same letters, as the first; thus, C, D, E, F, G, A, B, *c*, *d*, *e*, *f*, *g*, *a*, *b*; and it is easy to devise a continuation of such cycles. And other admissible notes are designated by a further modification of the standard ones, as by making each note *flat* or *sharp*; which modification it is not necessary here to consider, since our object is only to show how a standard is attainable, and how it serves the ends of science.

We may observe, however, that the above is not an exact account of the first, or early Greek scale; for that scale was founded on a primary division of the interval of two octaves (the extreme range which it admitted) into five *tetrachords*, each tetrachord including the interval of a fourth. All the notes of this series had different

names borrowed from this division[26]; thus *mese* was the middle or key-note; the note below it was *lichanos mesôn*, the next below was *parypate mesôn*, the next lower, *hypate mesôn*. The fifth above *mese* was *nete diazeugmenôn*, the octave was *nete hyperbolæôn*.

[26] Burney's *History of Music*, vol. i. p. 28.

4. But supposing a complete system of such denominations established, how could it be with certainty and rigour applied? The human ear is fallible, the organs of voice imperfectly obedient; if this were not so, there would be no such thing as a *good* ear or a *good* voice. What means can be devised of finding at will a *perfect* concord, a fifth or a fourth? Or supposing such concords fixed by an acknowledged authority, how can they be referred to, and the authority adduced? How can we enact a Standard of sounds?

A Standard was discovered in the *Monochord*. A musical string properly stretched, may be made to produce different

notes, in proportion as we intercept a longer or shorter portion, and make this portion 339 vibrate. The relation of the length of the strings which thus sound the two notes G and C is fixed and constant, and the same is true of all other notes. Hence the musical interval of any notes of which we know the places in the musical scale, may be reproduced by measuring the lengths of string which are known to give them. If C be of the length 180, D is 160, E is 144, F is 135, G is 120; and thus the musical relations are reduced to numerical relations, and the monochord is a complete and perfect *Tonometer*.

We have here taken the length of the string as the measure of the tone: but we may observe that there is in us a necessary tendency to assume that the ground of this measure is to be sought in some ulterior cause; and when we consider the matter further, we find this cause in the frequency of these vibrations of the string. The truth that the same note must result from the same frequency of vibration is readily

assented to on a slight suggestion of experience. Thus Mersenne[27], when he undertakes to determine the frequency of vibrations of a given sound, says 'Supponendum est quoscunque nervos et quaslibet chordas unisonum facientes eundem efficere numerum recursuum eodem vel equali tempore, quod perpetuâ constat experientiâ.' And he proceeds to apply it to cases where experience could not verify this assertion, or at least had not verified it, as to that of pipes.

[27] *Harmonia*, lib. ii. prop. 19.

The pursuit of these numerical relations of tones forms the science of Harmonics; of which here we do not pretend to give an account, but only to show, how the invention of a Scale and Nomenclature, a Standard and Measure of the tone of sounds, is its necessary basis. We will therefore now proceed to speak of another subject; *colour*.

SECT. III.—*Scales of Colour.*

5. *The Prismatic Scale of Colour.*—A SCALE

of Colour must depend originally upon differences 340 discernible by the eye, as a scale of notes depends on differences perceived by the ear. In one respect the difficulty is greater in the case of the visible qualities, for there are no relations of colour which the eye peculiarly singles out and distinguishes, as the ear selects and distinguishes an octave or a fifth. Hence we are compelled to take an arbitrary scale; and we have to find one which is fixed, and which includes a proper collection of colours. The *prismatic spectrum*, or coloured image produced when a small beam of light passes obliquely through any transparent surface (as the surface of a prism of glass,) offers an obvious Standard as far as it is applicable. Accordingly colours have, for various purposes, been designated by their place in the spectrum, ever since the time of Newton; and we have thus a means of referring to such colours as are included in the series *red*, *orange*, *yellow*, *green*, *blue*, *violet*, *indigo*, and the intermediate tints.

But this scale is not capable of numerical

precision. If the spectrum could be exactly defined as to its extremities, and if these colours occupied always the same proportional part of it, we might describe any colour in the above series by the measure of its position. But the fact is otherwise. The spectrum is too indefinite in its boundaries to afford any distinct point from which we may commence our measures; and moreover the spectra produced by different transparent bodies differ from each other. Newton had supposed that the spectrum and its parts were the same, so long as the refraction was the same; but his successors discovered that, with the same amount of refraction in different kinds of glass, there are different magnitudes of the spectrum; and what is still worse with reference to our present purpose, that the spectra from different glasses have the colours distributed in different proportions. In order, therefore, to make the spectrum the scale of colour, we must assume some fixed substance; for instance, we may take water, and thus a series approaching to the colours of the

rainbow will be our standard. But we should still have an extreme difficulty in applying such a rule. The distinctions of 341 colour which the terms of common language express, are not used with perfect unanimity or with rigorous precision. What one person calls *bluish green* another calls *greenish blue*. Nobody can say what is the precise boundary between red and orange. Thus the prismatic scale of colour was incapable of mathematical exactness, and this inconvenience was felt up to our own times.

But this difficulty was removed by a curious discovery of Wollaston and Fraunhofer; who found that there are, in the solar spectrum, certain fine black Lines which occupy a definite place in the series of colours, and can be observed with perfect precision. We have now no uncertainty as to what coloured light we are speaking of, when we describe it as that part of the spectrum in which Fraunhofer's Line C or D occurs. And thus, by this discovery, the prismatic spectrum of sunlight became, for certain purposes, an exact *Chromatometer*.

6. *Newton's Scale of Colours.*—Still, such a standard, though definite, is arbitrary and seemingly anomalous. The lines A, B, C, D, &c., of Fraunhofer's spectrum are distributed without any apparent order or law; and we do not, in this way, obtain numerical measures, which is what, in all cases, we desire to have. Another discovery of Newton, however, gives us a spectrum containing the same colours as the prismatic spectrum, but produced in another way, so that the colours have a numerical relation. I speak of the laws of the *Colours of Thin Plates*. The little rainbows which we sometimes see in the cracks of broken glass are governed by fixed and simple laws. The kind of colour produced at any point depends on the thickness of the thin plate of air included in the fissure. If the thickness be eight-millionths of an inch, the colour is orange, if fifteen-millionths of an inch, we have green, and so on; and thus these numbers, which succeed each other in a regular order from red to indigo, give a numerical measure of each colour; which measure, when we pursue the subject, we

find is one of the bases of all optical theory. The series of colours obtained from plates of air of gradually increasing thickness is called 342 *Newton's Scale of Colours*; but we may observe that this is not precisely what we are here speaking of, a scale of *simple* colours; it is a series produced by certain combinations, resulting from the repetition of the first spectrum, and is mainly useful as a standard for similar phenomena, and not for colour in general. The real scale of colour is to be found, as we have said, in the numbers which express the thickness of the producing film;—in the length of a *fit* in Newton's phraseology, or the length of an *undulation* in the modern theory.

7. *Scales of Impure Colours.*—The standards just spoken of include (mainly at least) only pure and simple colours; and however complete these standards may be for certain objects of the science of optics, they are insufficient for other purposes. They do not enable us to put in their place mixed and impure colours. And there is, in the case of colour, a difficulty already

noticed, which does not occur in the case of sound; two notes, when sounded together, are not necessarily heard as one; they are recognized as still two, and as forming a concord or a discord. But two colours form a single colour; and the eye cannot, in any way, distinguish between a green compound of blue and yellow, and the simple, undecomposable green of the spectrum. By composition of three or more colours, innumerable new colours may be generated which form no part of the prismatic series; and by such compositions is woven the infinitely varied web of colour which forms the clothing of nature. How are we to classify and arrange all the possible colours of objects, so that each shall have a place and name? How shall we find a *chromatometer* for impure as well as for pure colour?

Though no optical investigations have depended on a scale of impure colours, such a scale has been wanted and invented for other purposes; for instance, in order to identify and describe objects of natural

history. Not to speak of earlier essays, we may notice Werner's Nomenclature of Colours, devised for the purpose of describing minerals. This scale of colour was far superior to any which had previously been promulgated. 343 It was, indeed, arbitrary in the selection of its degrees, and in a great measure in their arrangement; and the colours were described by the usual terms, though generally with some added distinction; as *blackish green*, *bluish green*, *apple-green*, *emerald-green*. But the great merit of the scale was its giving a *fixed* conventional meaning to these terms, so that they lost much of their usual vagueness. Thus *apple-green* did not mean the colour of any green apple casually taken; but a certain definite colour which the student was to bear in mind, whether or not he had ever seen an apple of that exact hue. The words were not a description, but a *record* of the colour: the memory was to retain a *sensation*, not a name.

The imperfection of the system (arising from its arbitrary form) was its

incompleteness: however well it served for the reference of the colours which it did contain, it was applicable to no others; and thus though Werner's enumeration extended to more than a hundred colours, there occur in nature a still greater number which cannot be exactly described by means of it.

In such cases the unclassed colour is, by the Wernerians, defined by stating it as intermediate between two others: thus we have an object described as *between emerald-green and grass-green*. The eye is capable of perceiving a gradation from one colour to another; such as may be produced by a gradual mixture in various ways. And if we image to ourselves such a mixture, we can compare with it a given colour. But in employing this method we have nothing to tell us in what part of the scale we must seek for an approximation to our unclassed colour. We have no rule for discovering where we are to look for the boundaries of the definition of a colour which the Wernerian series does not supply. For it is not always between contiguous members of

the series that the undescribed colour is found. If we place emerald-green between apple-green and grass-green, we may yet have a colour intermediate between emerald-green and leek-green; and, in fact, the Wernerian series of colours is destitute 344 of a principle of self-arrangement and gradation; and is thus necessarily and incurably imperfect.

8. We should have a complete Scale of Colours, if we could form a series including all colours, and arranged so that each colour was intermediate in its tint between the adjacent terms of the series; for then, whether we took many or few of the steps of the series for our standard terms, the rest could be supplied by the law of continuity; and any given colour would either correspond to one of the steps of our scale or fall between two intermediate ones. The invention of a Chromatometer for Impure Colours, therefore, requires that we should be able to form all possible colours by such intermediation in a systematic manner; that is, by the mixture or combination of certain

elementary colours according to a simple rule: and we are led to ask whether such a process has been shown to be possible.

The colours of the prismatic spectrum obviously do form a continuous series; green is intermediate between its neighbours yellow and blue, orange between red and yellow; and if we suppose the two ends of the spectrum bent round to meet each other, so that the arrangement of the colours may be circular, the violet and indigo will find their appropriate place between the blue and red. And all the interjacent tints of the spectrum, as well as the ones just named, will result from such an arrangement. Thus all the *pure* colours are produced by combinations two and two of three primary colours, Red, Yellow, and Blue: and the question suggests itself whether these three are not really the only Primary Colours, and whether all the impure colours do not arise from mixtures of the three in various proportions. There are various modes in which this suggestion may be applied to the construction of a

scale of colours; but the simplest, and the one which appears really to verify the conjecture that all possible colours may be so exhibited, is the following. A certain combination of red, yellow, and blue, will produce black, or pure grey, and when diluted, will give all the shades of grey which intervene between 345 black and white. By adding various shades of grey, then, to pure colours, we may obtain all the possible ternary combinations of red, yellow, and blue; and in this way it is found that we exhaust the range of colours. Thus the circle of pure colours of which we have spoken may be accompanied by several other circles, in which these colours are tinged with a less or greater shade of grey; and in this manner it is found that we have a perfect chromatometer; every possible colour being exhibited either exactly or by means of approximate and contiguous limits. The arrangement of colours has been brought into this final and complete form by M. Merimée, whose Chromatic Scale is published by M. Mirbel in his *Elements of Botany*. We may observe that such a

standard affords us a numerical exponent for every colour by means of the proportions of the three primary colours which compose it; or, expressing the same result otherwise, by means of the pure colour which is involved, and the proportion of grey by which it is rendered impure. In such a scale the fundamental elements would be the precise tints of red, yellow, and blue which are found or assumed to be primary; the numerical exponents of each colour would depend upon the arbitrary number of degrees which we interpose between each two primary colours; and between each pure colour and absolute blackness. No such numerical scale has, however, as yet, obtained general acceptation[28].

[28] The reference to *Fraunhofer's Lines*, as a means of determining the place of a colour in the prismatic series, has been objected to, because, as is asserted, the colours which are in the neighbourhood of each line vary with the position of the sun, state of the atmosphere and the like. It is very evident that coloured light refracted by the prism will not give the same spectrum as white light. The

spectrum given by white light is of course the one here meant. It is an usual practice of optical experimenters to refer to the colours of such a spectrum, defining them by Fraunhofer's Lines.

I do not know whether it needs explanation that the 'first spectrum' in Newton's rings is a ring of the prismatic colours.

I have not had an opportunity of consulting Lambert's *Photometria, sive de mensura et gradibus luminis, colorum, et umbræ*, published in 1760, nor Mayer's *Commentatio de Affinitate Colorum*, (1758), in which, I believe, he describes a chromatometer. The present work is not intended to be complete as a history; and I hope I have given sufficient historical detail to answer its philosophical purpose.

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SECT. IV.—*Scales of Light.*

9. *Photometer.*—ANOTHER instrument much needed in optical researches is a *Photometer*, a measure of the intensity of light. In this case, also, the organ of sense, the eye, is the ultimate judge; nor has any effect of light, as light, yet been discovered which we can substitute for such a judgment. All instruments, such as that of

Leslie, which employ the heating effect of light, or at least all that have hitherto been proposed, are inadmissible as photometers. But though the eye can judge of two surfaces illuminated by light of the same colour, and can determine when they are equally bright, or which is the brighter, the eye can by no means decide at sight the proportion of illumination. How much in such judgments we are affected by contrast, is easily seen when we consider how different is the apparent brightness of the moon at mid-day and at midnight, though the light which we receive from her is, in fact, the same at both periods. In order to apply a scale in this case, we must take advantage of the known numerical relations of light. We are certain that if all other illumination be excluded, two equal luminaries, under the same circumstances, will produce an illumination twice as great as one does; and we can easily prove, from mathematical considerations, that if light be not enfeebled by the medium through which it passes, the illumination on a given surface will diminish as the square of the distance

of the luminary increases. If, therefore, we can by taking a fraction thus known of the illuminating effect of one luminary, make it equal to the total effect of another, of which equality the eye is a competent judge, we compare the effects of the two luminaries. In order to make this comparison we may, with Rumford, look at the shadows of the same object made by the two lights, 347 or with Ritchie, we may view the brightness produced on two contiguous surfaces, framing an apparatus so that the equality may be brought about by proper adjustment; and thus a measure will become practicable. Or we may employ other methods as was done by Wollaston[29], who reduced the light of the sun by observing it as reflected from a bright globule, and thus found the light of the sun to be 10,000,000,000 times that of Sirius, the brightest fixed star. All these methods are inaccurate, even as methods of comparison; and do not offer any fixed or convenient numerical standard; but none better have yet been devised[30].

[29] *Phil. Trans.* 1820, p. 19.

[30] Improved Photometers have been devised by Professor Wheatstone, Professor Potter, and Professor Steinheil; but they depend upon principles similar to those mentioned in the text.

10. *Cyanometer.*—As we thus measure the brightness of a colourless light, we may measure the intensity of any particular colour in the same way; that is, by applying a standard exhibiting the gradations of the colour in question till we find a shade which is seen to agree with the proposed object. Such an instrument we have in the *Cyanometer*, which was invented by Saussure for the purpose of measuring the intensity of the blue colour of the sky. We may introduce into such an instrument a numerical scale, but the numbers in such a scale will be altogether arbitrary.

SECT. V.—*Scales of Heat.*

11. *Thermometers.*—WHEN we proceed to the sensation of heat, and seek a measure of that quality, we find, at first sight, new

difficulties. Our sensations of this kind are more fluctuating than those of vision; for we know that the same object may feel warm to one hand and cold to another at the same instant, if the hands have been previously cooled and warmed respectively. Nor can we obtain here, as in the case of light, self-evident numerical relations of the heat communicated in given circumstances; for we know that the 348 effect so produced will depend on the warmth of the body to be heated, as well as on that of the source of heat; the summer sun, which warms our bodies, will not augment the heat of a red-hot iron. The cause of the difference of these cases is, that bodies do not receive the whole of their heat, as they receive the whole of their light, from the immediate influence of obvious external agents. There is no readily-discovered absolute cold, corresponding to the absolute darkness which we can easily produce or imagine. Hence we should be greatly at a loss to devise a *Thermometer*, if we did not find an indirect effect of heat sufficiently constant and measurable to answer this purpose. We

discover, however, such an effect in the *expansion* of bodies by the effect of heat.

12. Many obvious phenomena show that air, under given circumstances, expands by the effect of heat; the same is seen to be true of liquids, as of water, and spirit of wine; and the property is found to belong also to the metallic fluid, quicksilver. A more careful examination showed that the increase of bulk in some of these bodies by increase of Heat was a fact of a nature sufficiently constant and regular to afford a means of measuring that previously intangible quality; and the Thermometer was invented. There were, however, many difficulties to overcome, and many points to settle, before this instrument was fit for the purposes of science.

An explanation of the way in which this was done necessarily includes an important chapter of the history of Thermotics. We must now, therefore, briefly notice historically the progress of the Thermometer. The leading steps of this

progress, after the first invention of the instrument, were—The establishment of *fixed points* in the thermometric scale—The *comparison of the scales* of different substances—And the reconcilement of these differences by some method of interpreting them as indications of the absolute *quantity of heat*.

13. It would occupy too much space to give in detail the history of the successive attempts by which 349 these steps were effected. A thermometer is described by Bacon under the title *Vitrum Calendare*; this was an air thermometer. Newton used a thermometer of linseed oil, and he perceived that the first step requisite to give value to such an instrument was to fix its scale; accordingly he proposed his *Scala Graduum Caloris*[31]. But when thermometers of different liquids were compared, it appeared, from their discrepancies, that this fixation of the scale of heat was more difficult than had been supposed. It was, however, effected. Newton had taken freezing water, or rather thawing snow, as

the zero of his scale, which is really a fixed point; Halley and Amontons discovered (in 1693 and 1702) that the heat of boiling water is another fixed point; and Daniel Gabriel Fahrenheit, of Dantzig, by carefully applying these two standard points, produced, about 1714, thermometers, which were constantly consistent with each other. This result was much admired at the time, and was, in fact, the solution of the problem just stated, the *fixation of the scale of heat*.

[31] *Phil. Trans.* 1701.

14. But the scale thus obtained is a conventional not a natural scale. It depends upon the fluid employed for the thermometer. The progress of expansion from the heat of freezing to that of boiling water is different for mercury, oil, water, spirit of wine, air. A degree of heat which is half-way between these two standard points according to a mercurial thermometer, will be below the half-way point in a spirit thermometer, and above it in an air thermometer. Each liquid has its own *march* in the course of its expansion. Deluc and

others compared the marches of various liquids, and thus made what we may call a *concordance* of thermometers of various kinds.

15. Here the question further occurs: Is there not some *natural measure* of the degrees of heat? It appears certain that there must be such a measure, and that by means of it all the scales of different liquids must be reconciled. Yet this does not seem to have occurred at once to men's minds. Deluc, in speaking 350 of the researches which we have just mentioned, says[32], 'When I undertook these experiments, it never once came into my thoughts that they could conduct me with any probability to a table of real degrees of heat. But hope grows with success, and desire with hope.' Accordingly he pursued this inquiry for a long course of years.

[32] *Modif. de l'Atmosph.* 1782, p. 303.

What are the principles by which we are to be guided to the true measure of heat? Here, as in all the sciences of this class, we

have the general principle, that the secondary quality, Heat, must be supposed to be perceived in some way by a material Medium or Fluid. If we take that which is, perhaps, the simplest form of this hypothesis, that the heat depends upon the *quantity* of this fluid, or *Caloric*, which is present, we shall find that we are led to propositions which may serve as a foundation for a natural measure of heat. The *Method of Mixtures* is one example of such a result. If we mix together two pints of water, one hot and one cold, is it not manifest that the temperature of the mixture must be midway between the two? Each of the two portions brings with it its own heat. The whole heat, or caloric, of the mixture is the sum of the two; and the heat of each half must be the half of this sum, and therefore its temperature must be intermediate between the temperatures of the equal portions which were mixed. Deluc made experiments founded upon this principle, and was led by them to conclude that ‘the dilatations of mercury follow an accelerated march for successive equal

augmentations of heat.’

But there are various circumstances which prevent this method of mixtures from being so satisfactory as at first sight it seems to promise to be. The different capacities for heat of different substances, and even of the same substance at different temperatures, introduce much difficulty into the experiments; and this path of inquiry has not yet led to a satisfactory result. 351

16. Another mode of inquiring into the natural measure of heat is to seek it by researches on the *law of cooling* of hot bodies. If we assume that the process of cooling of hot bodies consists in a certain material heat flying off, we may, by means of certain probable hypotheses, determine mathematically the law according to which the temperature decreases as time goes on; and we may assume *that* to be the true measure of temperature which gives to the experimental law of cooling the most simple and probable form.

It appears evident from the most obvious

conceptions which we can form of the manner in which a body parts with its superabundant heat, that the hotter a body is, the faster it cools; though it is not clear without experiment, by what law the rate of cooling will depend upon the heat of the body. Newton took for granted the most simple and seemingly natural law of this dependence: he supposed the rate of cooling to be *proportional* to the temperature, and from this supposition he could deduce the temperature of a hot iron, calculating from the original temperature and the time during which it had been cooling. By calculation founded on such a basis, he graduated his thermometer.

17. But a little further consideration showed that the rate of cooling of a hot body depended upon the temperature of the surrounding bodies, as well as upon its own temperature. Prevost's *Theory of Exchanges*[33] was propounded with a view of explaining this dependence, and was generally accepted. According to this theory, all bodies radiate heat to one another, and are

thus constantly giving and receiving heat; and a body which is hotter than surrounding bodies, cools itself, and warms the surrounding, bodies, by an exchange of heat for heat, in which they are the gainers. Hence if θ be the temperature of the bodies, or of the space, by which the hot body is surrounded, and $\theta + t$ the temperature of the hot body, the rate of cooling will depend 352 upon the excess of the radiation for a temperature $\theta + t$, above the radiation for a temperature θ.

[33] *Recherches sur la Chaleur*, 1791. *Hist. Ind. Sc.* b. x. c. i. sect. 2.

Accordingly, in the admirable researches of MM. Dulong and Petit upon the cooling of bodies, it was assumed that the rate of cooling of the hot body was represented by the excess of $F(\theta + t)$ above $F(\theta)$; where F represented some mathematical *function*, that is, some expression obtained by arithmetical operations from the temperatures $\theta + t$ and θ; although what these operations are to be, was left undecided, and was in fact determined by

the experiments. And the result of their investigations was, that the function is of this kind: when the temperature increases by equal intervals, the function increases in a continued geometric proportion[34]. This was, in fact, the same law which had been assumed by Newton and others, with this difference, that *they* had neglected the term which depends upon the temperature of the surrounding space.

[34] The formula for the rate of cooling is $ma^{\theta + t} - ma^{\theta}$, where the quantity m depends upon the nature of the body, the state of its surface, and other circumstances.—*Ann. Chim.* vii. 150.

18. This law falls in so well with the best conceptions we can form of the mechanism of cooling upon the supposition of a radiant fluid caloric, that it gives great probability to the scale of temperature on which the simplicity of the result depends. Now the temperatures in the formulæ just referred to were expressed by means of the *air thermometer*. Hence MM. Dulong and Petit justly state, that while all different substances employed as thermometers give

different laws of thermotical phenomena, their own success in obtaining simple and general laws by means of the air thermometer, is a strong recommendation of that as the *natural scale of heat*. They add[35], 'The well-known uniformity of the principal physical properties of all gases, and especially the perfect identity of their laws of dilatation by heat, [a very important discovery of 353 Dalton and Gay Lussac[36],] make it very probable that in this class of bodies the disturbing causes have not the same influence as in solids and liquids; and consequently that the changes of bulk produced by the action of heat are here in a more immediate dependence on the force which produces them.'

[35] *Annales de Chimie*, vii. 153.

[36] *Hist. Ind. Sc.* b. x. c. ii. sect. 1.

19. Still we cannot consider this point as settled till we obtain a more complete theoretical insight into the nature of heat itself. If it be true that heat consists in the vibrations of a fluid, then, although, as

Ampère has shown[37], the laws of radiation will, on mathematical grounds, be the same as they are on the hypothesis of emission, we cannot consider the natural scale of heat as determined, till we have discovered some means of measuring the caloriferous vibrations as we measure luminiferous vibrations. We shall only know what the quantity of heat is when we know what heat itself is;—when we have obtained a theory which satisfactorily explains the manner in which the substance or medium of heat produces its effects. When we see how radiation and conduction, dilatation and liquefaction, are all produced by mechanical changes of the same fluid, we shall then see what the nature of that change is which dilatation really measures, and what relation it bears to any more proper standard of heat.

[37] *Ib.* c. iv.

We may add, that while our thermotical theory is still so imperfect as it is, all attempts to divine the true nature of the relation between light and heat are

premature, and must be in the highest degree insecure and visionary. Speculations in which, from the general assumption of a caloriferous and luminiferous medium, and from a few facts arbitrarily selected and loosely analysed, a general theory of light and heat is asserted, are entirely foreign to the course of inductive science, and cannot lead to any stable and substantial truth.

20. *Other Instruments for measuring Heat.*—It does not belong to our present purpose to speak of 354 instruments of which the object is to measure, not sensible qualities, but some effect or modification of the cause by which such qualities are produced: such, for instance, are the *Calorimeter*, employed by Lavoisier and Laplace, in order to compare the *Specific Heat* of different substances; and the *Actinometer*, invented by Sir John Herschel, in order to determine the *effect of the Sun's Rays* by means of the heat which they communicate in a given time; which effect is, as may readily be supposed, very different under different circumstances of atmosphere and position. The laws of such

effects may be valuable contributions to our knowledge of heat, but the interpretation of them must depend on a previous knowledge of the relations which temperature bears to heat, according to the views just explained.

SECT. VI.—*Scales of other Qualities.*

21. BEFORE quitting the subject of the measures of sensible qualities, we may observe that there are several other such qualities for which it would be necessary to have scales and means of measuring, in order to make any approach to science on such subjects. This is true, for instance, of Tastes and Smells. Indeed some attempts have been made towards a classification of the Tastes of sapid substances, but these have not yet assumed any satisfactory or systematic character; and I am not aware that any instrument has been suggested for *measuring* either the Flavour or the Odour of bodies which possess such qualities.

22. *Quality of Sounds.*—The same is true of that kind of difference in sounds which is

peculiarly termed their *Quality*; that character by which, for instance, the sound of a flute differs from that of a hautbois, when the note is the same; or a woman's voice from a boy's.

23. *Articulate Sounds.*—There is also in sounds another difference, of which the nature is still obscure, but in reducing which to rule, and consequently to measure, some progress has nevertheless been made. 355 I speak of the differences of sound considered as *articulate*. Classifications of the sounds of the usual alphabets have been frequently proposed; for instance, that which arranges the *Consonants* in the following groups:

Sharp.	Flat.	Sharp Aspirate.	Flat Aspirate.	Nas
p	b	ph (*f*)	bh (*v*)	m
k	g (hard)	kh	gh	n
t	d	th (sharp)	th (flat)	n
s	z	sh	zh	

It is easily perceived that the relations of the sounds in each of these horizontal lines are

analogous; and accordingly the rules of derivation and modification of words in several languages proceed upon such analogies. In the same manner the *Vowels* may be arranged in an order depending on their sound. But to make such arrangements fixed and indisputable, we ought to know the mechanism by which such modifications are caused. Instruments have been invented by which some of these sounds can be imitated; and if such instruments could be made to produce the above series of articulate sounds, by connected and regular processes, we should find, in the process, a *measure* of the sound produced. This has been in a great degree effected for the Vowels by Professor Willis's artificial mode of imitating them. For he finds that if a musical reed be made to sound through a cylindrical pipe, we obtain by gradually lengthening the cylindrical pipe, the series of vowels I, E, A, O, U, with intermediate sounds[38]. In this instrument, then, the length of the pipe would determine the vowel, and might be used numerically to express it. Such an instrument so employed would be a

measure of vowel quality, and might be called a *Phthongometer*.

[38] *Camb. Trans.* vol. iii. p. 239.

Our business at present, however, is not with instruments which might be devised for measuring sensible qualities, but with those which have been so used, and have thus been the basis of the sciences in which 356 such qualities are treated of; and this we have now done sufficiently for our present purpose.

24. There is another Idea which, though hitherto very vaguely entertained, has had considerable influence in the formation, both of the sciences spoken of in the present Book, and on others which will hereafter come under our notice: namely, the Idea of Polarity. This Idea will be the subject of the ensuing Book. And although this Idea forms a part of the basis of various other extensive portions of science, as Optics and Chemistry, it occupies so peculiarly conspicuous a place in speculations belonging to what I have termed the

Mechanico-Chemical Sciences, (Magnetism and Electricity,) that I shall designate the discussion of the Idea of Polarity as the Philosophy of those Sciences.

BOOK V.

THE

PHILOSOPHY

OF THE

MECHANICO-CHEMICAL SCIENCES.

En donnant à ces côtés le nom de *poles*, j'appelerai *polarisation* la modification qui donne à la lumière des propriétés relatives à ces poles. J'ai tardé jusqu'à présent à admettre ce terme dans la description des phénomènes physiques dont il est question; je n'ai pas osé l'introduire dans les mémoires où j'ai publié mes dernières expériences; mais les variétés qu'offre ce nouveau phénomène, et la difficulté de les décrire, me forcent à admettre cette

nouvelle expression, qui signifie simplement la modification que la lumière a subie en acquérant de nouvelles propriétés qui ne sont pas relatives à la direction du rayon, mais seulement à ses côtés considérés à angles droits et dans un plan perpendiculaire à sa direction.

Malus (1811), *Mém. de Inst.* tom. xi. p. 106.

BOOK V.

THE PHILOSOPHY OF THE MECHANICO-CHEMICAL SCIENCES.

CHAPTER I.

Attempts at the Scientific Application of the Idea of Polarity.

1. In some of the mechanical sciences, as Magnetism and Optics, the phenomena are found to depend upon position (the position of the magnet, or of the ray of light,) in a peculiar alternate manner. This dependence, as it was first apprehended, was represented by means of certain conceptions of space

and force, as for instance by considering the two *Poles* of a magnet. But in all such modes of representing these alternations by the conceptions borrowed from other ideas, a closer examination detected something superfluous and something defective; and in proportion as the view which philosophers took of this relation was gradually purified from these incongruous elements, and was rendered more general and abstract by the discovery of analogous properties in new cases, it was perceived that the relation could not be adequately apprehended without considering it as involving a peculiar and independent Idea, which we may designate by the term *Polarity*.

We shall trace some of the forms in which this Idea has manifested itself in the history of science. In doing so we shall not begin, as in other Books of this work we have done, by speaking of the notion as it is 360 employed in common use: for the relation of Polarity is of so abstract and technical a nature, that it is not employed, at least in any distinct and obvious manner, on any

ordinary or practical occasions. The idea belongs peculiarly to the region of speculation: in persons of common habits of thought it is probably almost or quite undeveloped; and even most of those whose minds have been long occupied by science, find a difficulty in apprehending it in its full generality and abstraction, and stript of all irrelevant hypothesis.

2. *Magnetism.*—The name and the notion of *Poles* were first adopted in the case of a magnet. If we have two magnets, their extremities attract and repel each other alternatively. If the first end of the one attract the first end of the other, it repels the second end, and conversely. In order to express this rule conveniently, the two ends of each magnet are called the *north pole* and the *south pole* respectively, the denominations being borrowed from the poles of the earth and heavens. 'These poles,' as Gilbert says[1], 'regulate the motions of the celestial spheres and of the earth. In like manner the magnet has its poles, a northern and a southern one; certain and

determined points constituted by nature in the stone, the primary terms of its motions and effects, the limits and governors of many actions and virtues.'

[1] *De Magn.* lib. i. c. iii.

The nature of the opposition of properties of which we speak may be stated thus:

The North pole of one magnet attracts the South pole of another magnet.

The North pole of one magnet *repels* the North pole of another magnet.

The South pole of one magnet repels the South pole of another magnet.

The South pole of one magnet *attracts* the North pole of another magnet.

It will be observed that the contrariety of position which is indicated by putting the South pole for the North pole in either magnet, is accompanied by the 361 opposition of mechanical effect which is expressed by changing attraction into repulsion and repulsion into attraction: and thus we have the general feature of Polarity, —A contrast of properties corresponding to

a contrast of positions.

3. *Electricity*.—When the phenomena of Electricity came to be studied, it appeared that they involved relations in some respects Analogous to those of magnetism.

Two kinds of electricity were distinguished, the positive and the negative; and it appeared that two bodies electrized positively, or two electrized negatively, repelled each other, like two north or two south magnetic poles; while a positively and a negatively electrized body attracted each other, like the north and south poles of two magnets. In conductors of an oblong form, the electricity could easily be made to distribute itself so that one end should be positively and one end negatively electrized; and then such conductors acted on each other exactly as magnets would do.

But in conductors, however electrized, there is no peculiar point which can permanently be considered as the *pole*. The distribution of electricity in the conductor depends upon external circumstances: and

thus, although the phenomena offer the general character of *polarity*—alternative results corresponding to alternative positions,—they cannot be referred to poles. Some other mode of representing the forces must be adopted than that which makes them emanate from permanent points as in a magnet.

The phenomena of attraction and repulsion in electrized bodies were conveniently represented by means of the hypothesis of *two* electric *fluids*, a positive and a negative one, which were supposed to be distributed in the bodies. Of these fluids, it was supposed that each repelled its own parts and attracted those of the opposite fluid: and it was found that this hypothesis explained all the obvious laws of electric action. Here then we have the phenomena of polarization explained by a new kind of machinery:—two opposite fluids 362 distributed in bodies, and supplying them, so to speak, with their polar forces. This hypothesis not only explains electrical attraction, but also the electrical spark:

namely, thus: when two bodies, of which the neighbouring surfaces are charged with the two opposite fluids, approach near to each other, the mutual attraction of the fluids becomes more and more intense, till at last the excess of fluid on the one body breaks through the air and rushes to the other body, in a form accompanied by light and noise. When this transfer has taken place, the attraction ceases, the positive and the negative fluid having neutralized each other. Their effort was to unite; and this union being effected, there is no longer any force in action. Bodies in their natural unexcited condition may be considered as occupied by a combination of the two fluids: and hence we see how the production of either kind of electricity is necessarily accompanied with the production of an equivalent amount of the opposite kind.

4. *Voltaic Electricity.*—Such is the case in Franklinic electricity,—that which is excited by the common electrical machine. In studying Voltaic electricity, we are led to

the conviction that the fluid which is in a condition of momentary *equilibrium* in electrized conductors, exists in the state of a *Current* in the voltaic circuit. And here we find polar relations of a new kind existing among the forces. Two voltaic Currents *attract* each other when they are moving in the *same*, and *repel* each other when they are moving in *opposite*, directions.

But we find, in addition to these, other polar relations of a more abstruse kind, and which the supposition of two fluids does not so readily explain. For instance, if such fluids existed, distinct from each other, it might be expected that it would be possible to exhibit one of them separate from the other. Yet in all the phenomena of electromotive currents, we attempt in vain to obtain one kind of electricity separately. 'I have not,' says Mr. Faraday[2], 'been able to find a 363 single fact which could be adduced to prove the theory of two electricities rather than one, in electric currents; or, admitting the hypothesis of two electricities, have I been able to perceive the

slightest grounds that one electricity can be more powerful than the other, or that it can be present without the other, or that it can be varied or in the slightest degree affected without a corresponding variation in the other.' 'Thus,' he adds, 'the polar character of the powers is rigorous and complete.' Thus, we too may remark, all the superfluous and precarious parts gradually drop off from the hypothesis which we devise in order to represent polar phenomena; and the abstract notion of Polarity—of equal and opposite powers called into existence by a common condition—remains unincumbered with extraneous machinery.

[2] *Researches*, 516.

5. *Light.*—Another very important example of the application of the Idea of Polarity is that supplied by the discovery of the polarization of light. A ray of light may, by various processes, be modified, so that it has different properties according to its different *sides*, although this difference is not perceptible by any common effects. If,

for instance, a ray thus modified, pass perpendicularly through a circular glass, and fall upon the eye, we may turn the glass round and round in its frame, and we shall make no difference in the brightness of the spot which we see. But if, instead of a glass, we look through a longitudinal slice of tourmaline, the spot is alternately dark and bright as we turn the crystal through successive quadrants. Here we have a contrast of Properties (dark and bright) corresponding to a contrast of positions, (the position of a line east and west being contrasted with the position north and south,) which, as we have said, is the general character of Polarity. It was with a view of expressing this character that the term *Polarization* was originally introduced. Malus was forced by his discoveries into the use of this expression. 'We find,' he says, in 1811, 'that light acquires properties which are relative only to the sides of the ray,—which are the same for the north and south sides of the ray, (using 364 the points of the compass for description's sake only,) and which are different when we go from the

north and south to the east or to the west sides of the ray. I shall give the name of *poles* to these sides of the ray, and shall call *polarization* the modification which gives to light these properties relative to these poles. I have *put off* hitherto the admission of this term into the description of the physical phenomena with which we have to do: I did not *dare* to introduce it into the Memoirs in which I published my last observations: but the variety of forms in which this new phenomenon appears, and the difficulty of describing them, compel me to admit this new expression; which signifies simply the modification which light has undergone in acquiring new properties which are not relative to the direction of the ray, but only to its sides considered at right angles to each other, and in a plane perpendicular to its direction.'

The theory which represents light as an emission of particles was in vogue at the time when Malus published his discoveries; and some of his followers in optical research conceived that the phenomena which he

thus described rendered it necessary to ascribe poles and an axis to each particle of light. On this hypothesis, light would be polarized when the axes of all the particles were in the same direction: and, making such a supposition, it may easily be conceived capable of transmission through a crystal whose axis is parallel to that of the luminous particles, and intransmissible when the axis of the crystal is in a position transverse to that of the particles.

The hypothesis of particles possessing *poles* is a rude and arbitrary assumption, in this as in other cases; but it serves to convey the general notion of polarity, which is the essential feature of the phenomena. The term ‘polarization of light has sometimes been complained of in modern times as hypothetical and obscure. But the real cause of obscurity was, that the Idea of Polarity was, till lately, very imperfectly developed in men’s minds. As we have seen, the general notion of Polarity,—opposite properties in opposite 365 directions,—exactly describes the character of the optical

phenomena to which the term is applied.

It is to be recollected that in optics we never speak of the *poles*, but of the *plane of polarization* of a ray. The word *sides*, which Newton and Malus have used, neither of them appears to have been satisfied with; Newton, in employing it, had recourse to the strange Gallicism of speaking of the *coast* of usual and of unusual refraction of a crystal.

The modern theory of optics represents the plane of polarization of light as depending, not on the position in which the axes of the luminiferous particles lie, but on the *direction* of those *transverse vibrations* in which light consists. This theory is, as we have stated in the History, recommended by an extraordinary series of successes in accounting for the phenomena. And this hypothesis of transverse vibrations shows us another mechanical mode, (besides the hypothesis of particles with axes,) by which we may represent the polarity of a ray. But we may remark that the general notion of

Polarity, as applied to light in such cases, would subsist, even if the undulatory theory were rejected. The idea is, as we have before said, independent of all hypothetical machinery.

I need not here refer to the various ways in which light may be polarized; as, for instance, by being reflected from the surface of water, or of glass, at certain angles, by being transmitted, through crystals, and in other ways. In all cases the modification produced, the polarization, is identically the same property. Nor need I mention the various kinds of phenomena which appear as contrasts in the result; for these are not merely light and dark, or white and black, but red and green, and generally, a colour and its *complementary* colour, exhibited in many complex and varied configurations. These multiplied modes in which polarized light presents itself add nothing to the original conception of Polarization: and I shall therefore pass on to another subject.

6. *Crystallization.*—Bodies which are

perfectly crystallized exhibit the most complete regularity and 366 symmetry of form; and this regularity not only appears in their outward shape, but pervades their whole texture, and manifests itself in their cleavage, their transparency, and in the uniform and determinate optical properties which exist in every part, even in the smallest fragment of the mass. If we conceive crystals as composed of particles, we must suppose these particles to be arranged in the most regular manner; for example, if we suppose each particle to have an axis, we must suppose all these axes to be parallel; for the direction of the axis of the particles is indicated by the physical and optical properties of the crystal, and therefore this direction must be the same for every portion of the crystal. This parallelism of the axes of the particles may be conceived to result from the circumstance of each particle having poles, the opposite poles attracting each other. In virtue of forces acting as this hypothesis assumes, a collection of small *magnetic* particles would arrange themselves in parallel positions; and

such a collection of magnetic particles offers a sort of image of a crystal. Thus we are led to conceive the particles of crystals as polarized, and as determined in their crystalline positions by polar forces. This mode of apprehending the constitution of crystals has been adopted by some of our most eminent philosophers. Thus Berzelius says[3], 'It is demonstrated, that the regular forms of bodies presuppose an effort of their atoms to touch each other by preference in certain points; that is, they are founded upon a Polarity;'—he adds, 'a polarity which can be no other than an electric or magnetic polarity.' In this latter clause we have the identity of different kinds of polarity asserted; a principle which we shall speak of in the next chapter. But we may remark, that even without dwelling upon this connexion, any notion which we can form of the structure of Crystals necessarily involves the idea of Polarity. Whether this polarity necessarily requires us to believe crystals to be composed of Atoms which exert an effort to touch 367 each other in certain points by preference, is another

question. And, in agreement with what has been said respecting other kinds of polarity, we shall probably find, on a more profound examination of the subject, that while the Idea of Polarity is essential, the machinery by which it is thus expressed is precarious and superfluous.

[3] *Essay on the Theory of Chemical Properties*, 1820, p. 113.

7. *Chemical Affinity.*—We shall have, in the next Book, to speak of Chemical Affinity at some length; but since the ultimate views to which philosophers have been led, induce them to consider the forces of Affinity as Polar Forces, we must enumerate these among the examples of Polarity. In chemical processes, opposites tend to unite, and to neutralize each other by their union. Thus an *acid* or an *alkali* combine with vehemence, and form a compound, a neutral salt, which is neither acid nor alkaline.

This conception of contrariety and mutual neutralization, involves the Idea of Polarity.

In the conception as entertained by the earlier chemists, the Idea enters very obscurely: but in the attempts which have more recently been made to connect this relation (of acid and base), with other relations, the chemical elements have been conceived as composed of particles which possess poles; *like* poles repelling, and *unlike* attracting each other, as they do in magnetic and electric phenomena. This is, however, a rude and arbitrary way of expressing Polarity, and, as may be easily shown, involves many difficulties which do not belong to the Idea itself. Mr. Faraday, who has been led by his researches to a conviction of the polar nature of the forces of chemical affinity, has expressed their character in a more general manner, and without any of the machinery of particles indued with poles. According to his view, chemical synthesis and analysis must always be conceived as taking place in virtue of equal and opposite forces, by which the particles are united or separated. These forces, by the very circumstance of their being polar, may be transferred from point

to point. For if we conceive a string of particles, and if the positive force of the first particle 368 be liberated and brought into action, its negative force also must be set free: this negative force neutralizes the positive force of the next particle, and therefore the negative force of this particle (before employed in neutralizing its positive force) is set free: this is in the same way transferred to the next particle, and so on. And thus we have a positive force active at one extremity of a line of particles, corresponding to a negative force at the other extremity, all the intermediate particles reciprocally neutralizing each other's action. This conception of the transfer of chemical action was indeed at an earlier period introduced by Grotthus[4], and confirmed by Davy. But in Mr. Faraday's hands we see it divested of all that is superfluous, and spoken of, not as a line of particles, but as 'an axis of power, having [at every point] contrary forces, exactly equal, in opposite directions.'

[4] DUMAS, *Leçons sur la Philosophie Chimique*, p. 401.

8. *General Remarks.*—Thus, as we see, the notion of Polarity is applicable to many large classes of phenomena. Yet the Idea in a distinct and general form is only of late growth among philosophers. It has gradually been abstracted and refined from many extraneous hypotheses which were at first supposed to be essential to it. We have noticed some of these hypotheses;—as the poles of a *body*; the poles of the *particles* of a fluid; *two* opposite fluids; a single fluid in *excess* and *defect*; transverse *vibrations*. To these others might be added. Thus Dr. Prout[5] assumes that the polarity of molecules results from their *rotation* on their axes, the opposite motions of contiguous molecules being the cause of opposite (positive and negative) polarities.

[5] *Bridgewater Treatise*, p. 559.

But none of these hypotheses can be proved by the fact of Polarity alone; and they have been in succession rejected when they had been assumed on that ground. Thus Davy, in 1826, speaking of chemical forces says[6], ‘In assuming the idea of two

ethereal, subtile, elastic 369 fluids, attractive of the particles of each other, and repulsive as to their own particles, capable of combining in different proportions with bodies, and according to their proportions giving them their specific qualities and rendering them equivalent masses, it would be natural to refer the action of the poles to the repulsions of the substances combined with the excess of one fluid, and the attractions of those united to the excess of the other fluid; and a history of the phenomena, not unsatisfactory to the reason, might in this way be made out. But as it is possible likewise to take an entirely different view of the subject, on the idea of the dependence of the results upon the primary attractive powers of the parts of the combination on a single subtile fluid, I shall not enter into any discussion on this obscure part of the theory.' Which of these theories will best represent the case, will depend upon the consideration of other facts, in combination with the polar phenomena, as we see in the history of optical theory. In like manner Mr. Faraday proved by

experiment[7] the errour of all theories which ascribe electro-chemical decomposition to the attraction of the poles of the voltaic battery.

[6] *Phil. Tr.* 1826, p. 415.

[7] *Researches*, p. 495, &c.

In order that they may distinctly image to themselves the Idea of Polarity, men clothe it in some of the forms of machinery above spoken of; yet every new attempt shows them the unnecessary difficulties in which they thus involve themselves. But on the other hand it is difficult to apprehend this Idea divested of all machinery; and to entertain it in such a form that it shall apply at the same time to magnetism and electricity, galvanism and chemistry, crystalline structure and light. The Idea of *Polarity* becomes most pure and genuine, when we entirely reject the conception of *Poles*, as Faraday has taught us to do in considering electro-chemical decomposition; but it is only by degrees and by effort that we can reach this point of abstraction and

generality. 370

9. There is one other remark which we may here make. It was a maxim commonly received in the ancient schools of philosophy, that ‘Like attracts Like:’ but as we have seen, the universal maxim of Polar Phenomena is, that Like *repels* Like, and attracts Unlike. The north pole attracts the south pole, the positive fluid attracts the negative fluid; opposite elements rush together; opposite motions reduce each other to rest. The permanent and stable course of things is that which results from the balance and neutralization of contrary tendencies. Nature is constantly labouring after repose by the effect of such tendencies; and so far as Polar Forces enter into her economy, she seeks harmony by means of discord, and unity by opposition.

Although the Idea of Polarity is as yet somewhat vague and obscure, even in the minds of the cultivators of physical science, it has nevertheless given birth to some general principles which have been

accepted as evident, and have had great influence on the progress of science. These we shall now consider.

CHAPTER II.

Of the Connexion of Polarities.

1. It has appeared in the preceding chapter that in cases in which the phenomena suggest to us the idea of Polarity, we are also led to assume some material machinery as the mode in which the polar forces are exerted. We assume, for instance, globular particles which possess poles, or the vibrations of a fluid, or two fluids attracting each other; in every case, in short, some hypothesis by which the existence and operation of the Polarity is embodied in geometrical and mechanical properties of a medium; nor is it possible for us to avoid proceeding upon the conviction that some such hypothesis must be true; although the nature of the connexion between the mechanism and the phenomena

must still be indefinite and arbitrary.

But since each class of Polar Phenomena is thus referred to an ulterior cause, of which we know no more than that it has a polar character, it follows that *different* Polarities may result from the *same* cause manifesting its polar character under different aspects. Taking, for example, the hypothesis of globular particles, if electricity result from an action dependent upon the *poles* of each globule, magnetism may depend upon an action in the *equator* of each globule; or taking the supposition of transverse vibrations, if polarized light result directly from such *vibrations*, crystallization may have reference to the *axes* of the elasticity of the *medium* by which the vibrations are rendered transverse,—so far as the polar character only of the phenomena is to be accounted for. I say this *may* be so, *in so far* only as the polar character of the phenomena is concerned; for whether the relation of 372 electricity to magnetism, or of crystalline forces to light, can really be explained by such hypotheses,

remains to be determined by the facts themselves. But since the first necessary feature of the hypothesis is, that it shall give polarity, and since an hypothesis which does this, may, by its mathematical relations, give polarities of different kinds and in different directions, any two co-existent kinds of polarity may result from the same cause, manifesting itself in various manners.

The conclusion to which we are led by these general considerations is, that two co-existing classes of polar phenomena *may* be effects of the same cause. But those who have studied such phenomena more deeply and attentively have, in most or in all cases, arrived at the conviction that the various kinds of Polarity in such cases *must* be connected and fundamentally identical. As this conviction has exercised a great influence, both upon the discoveries of new facts and upon the theoretical speculations of modern philosophers, and has been put forward by some writers as a universal principle of science, I will consider some of

the cases in which it has been thus applied.

2. *Connexion of Magnetic and Electric Polarity.*—The polar phenomena of electricity and magnetism are clearly analogous in their laws: and obvious facts showed at an early period that there was some connexion between the two agencies. Attempts were made to establish an evident and definite relation between the two kinds of force, which attempts proceeded upon the principle now under consideration;—namely, that in such cases, the two kinds of Polarity must be connected. Professor Œrsted, of Copenhagen, was one of those who made many trials founded upon this conviction: yet all these were long unsuccessful. At length, in 1820, he discovered that a galvanic current, passing at right angles near to a magnetic needle, exercises upon it a powerful deflecting force. The connexion once detected between magnetism and galvanism was soon recognized as constant and universal. It was represented in different hypothetical modes by different persons; some considering the

galvanic 373 current as the primitive axis, and the magnet as constituted of galvanic currents passing round it at right angles to the magnetic axis; while others conceived the magnetic axis as the primitive one, and the electric current as implying a magnetic current round the wire. So far as many of the general relations of these two kinds of force were concerned, either mode of representation served to express them; and thus the assumption that the two Polarities, the magnetic and the electric, were fundamentally identical, was verified, so far as the phenomena of magnetic attraction, and the like, were concerned.

I need not here mention how this was further confirmed by the experiments in which, by means of the forces thus brought into view, a galvanic wire was made to revolve round a magnet, and a magnet round a galvanic wire;—in which artificial magnets were constructed of coils of galvanic wire;—and finally, in which the galvanic spark was obtained from the magnet. The identity which sagacious

speculators had divined even before it was discovered, and which they had seen to be universal as soon as it was brought to light, was completely manifested in every imaginable form.

The relation of the electric and magnetic Polarities was found to be, that they were *transverse* to each other, and this relation exhibited under various conditions of form and position of the apparatus, gave rise to very curious and unexpected perplexities. The degree of complication which this relation may occasion, may be judged of from the number of constructions and modes of conception offered by Œrsted, Wollaston, Faraday, and others, for the purpose of framing a technical memory of the results. The magnetic polarity gives us the north and south poles of the needle; the electric polarity makes the current positive and negative; and these pairs of opposites are connected by relations of situation, as above and below, right and left; and give rise to the resulting motion of the needle one way or the other. 374

3. Ampère, by framing his hypotheses of the action of voltaic currents and the constitution of magnets, reduced all these technical rules to rigorous deductions from one general principle. And thus the vague and obscure persuasion that there *must* be *some* connexion between Electricity and Magnetism, so long an idle and barren conjecture, was unfolded into a complete theory, according to which magnetic and electromotive actions are only two different manifestations of the same forces; and all the above-mentioned complex relations of polarities are reduced to one single polarity, that of the electro-dynamic current.

4. As the Idea of Polarity was thus firmly established and clearly developed, it became an instrument of reasoning. Thus it led Ampère to maintain that the original or elementary forces in electro-dynamic action could not be as M. Biot thought they were, a statical *couple*, but must be directly opposite to each other. The same idea enabled Mr. Faraday to carry on with confidence such reasonings as the following[8]: 'No other

known power has like direction with that exerted between an electric current and a magnetic pole; it is tangential, while all other forces acting at a distance are direct. Hence if a magnetic pole on one side of a revolving plate follow its course by reason of its obedience to the tangential force exerted upon it by the very current of electricity which it has itself caused; a similar pole on the other side of the plate should immediately set it free from this force; for the currents which have to be formed by the two poles are in contrary directions.' And in Article 1114 of his *Researches*, the same eminent philosopher infers that if electricity and magnetism are considered as the results of a peculiar agent or condition, exerted in determinate directions perpendicular to each other, one must be by some means convertible into the other; and this he was afterwards able to prove to be the case in fact.

[8] *Researches*, 244.

375 Thus the principle that the Co-existent Polarities of magnetism and

electricity are connected and fundamentally identical, is not only true, but is far from being either vague or barren. It has been a fertile source both of theories which have, at present, a very great probability, and of the discovery of new and striking facts. We proceed to consider other similar cases.

5. *Connexion of Electrical and Chemical Polarities.*—The doctrine that the chemical forces by which the elements of bodies are held together or separated, are identical with the polar forces of electricity, is a great discovery of modern times; so great and so recent, indeed, that probably men of science in general have hardly yet obtained a clear view and firm hold of this truth. This doctrine is now, however, entirely established in the minds of the most profound and philosophical chemists of our time. The complete development and confirmation of this as of other great truths, was preceded by more vague and confused opinions gradually tending to this point; and the progress of thought and of research was impelled and guided, in this as in

similar cases, by the persuasion that these co-existent polarities could not fail to be closely connected with each other. While the ultimate and exact theory to which previous incomplete and transitory theories tended is still so new and so unfamiliar, it must needs be a matter of difficulty and responsibility for a common reader to describe the steps by which truth has advanced from point to point. I shall, therefore, in doing this, guide myself mainly by the historical sketches of the progress of this great theory, which, fortunately for us, have been given us by the two philosophers who have played by far the most important parts in the discovery, Davy and Faraday.

It will be observed that we are concerned here with the progress of theory, and not of experiment, except so far as it is confirmatory of theory. In Davy's Memoir[9] of 1826, on the Relations of Electrical and 376 Chemical Changes, he gives the historical details to which I have alluded. Already in 1802 he had conjectured that all chemical decompositions might be polar. In

1806 he attempted to confirm this conjecture, and succeeded, to his own satisfaction, in establishing[10] that the combinations and decompositions by electricity were referable to the law of electrical attractions and repulsions; and advanced the hypothesis (as he calls it), that chemical and electrical attractions were produced by the same cause, acting in one case on particles, in the other on masses. This hypothesis was most strikingly confirmed by the author's being able to use electrical agency as a more powerful means of chemical decomposition than any which had yet been applied. 'Believing,' he adds, 'that our philosophical systems are exceedingly imperfect, I never attached much importance to this hypothesis; but having formed it after a copious induction of facts, and having gained by the application of it a number of practical results, and considering myself as much the author of it as I was of the decomposition of the alkalies, and having developed it in an elementary work as far as the present state of chemistry seemed to allow, I have never,'

he says, ‘criticised or examined the manner in which different authors have adopted or explained it, contented, if in the hands of others, it assisted the arrangements of chemistry or mineralogy, or became an instrument of discovery.’ When the doctrine had found an extensive acceptance among chemists, attempts were made to show that it had been asserted by earlier writers: and though Davy justly denies all value to these pretended anticipations, they serve to show, however dimly, the working of that conviction of the Connexion of Co-existent Properties which all along presided in men’s minds during this course of investigation. ‘Ritter and Winterl have been quoted,’ Davy says[11], ‘among other persons, as having imagined or anticipated the relation between electrical powers and chemical affinities before the discovery of the pile 377 of Volta. But whoever will read with attention Ritter’s “Evidence that Galvanic action exists in organised nature,” and Winterl's *Prolusiones ad Chemiam sæculi decimi noni*, will find nothing to justify this opinion.’ He then refers to the Queries of

Newton at the end of his Optics. ‘These,’ he says, ‘contain more grand and speculative views that might be brought to bear upon this question than any found in the works of modern electricians; but it is very unjust to the experimentalists who by the laborious application of new instruments, have discovered novel facts and analogies, to refer them to any such suppositions as that all attractions, chemical, electrical, magnetical, and gravitative, may depend upon the same cause.’ It is perfectly true, that such vague opinions, though arising from that tendency to generalize which is the essence of science, are of no value except so far as they are both rendered intelligible, and confirmed by experimental research.

[9] *Phil. Trans.* 1826, p. 383.

[10] *Phil. Trans.* 1826, p. 389.

[11] *Ibid.* p. 384.

The phenomena of chemical decomposition by means of the voltaic pile, however, led other persons to views very

similar to those of Davy. Thus Grotthus in 1805[12] published an hypothesis of the same kind. ‘The pile of Volta,’ he says, ‘is an electrical magnet, of which each element, that is, each pair of plates, has a positive and a negative pole. The consideration of this polarity suggested to me the idea that a similar polarity may come into play between the elementary particles of water when acted upon by the same electrical agent; and I avow that this thought was for me a flash of light.’

[12] *Ann. Chim.* lxviii. 54.

6. The thought, however, though thus brought into being, was very far from being as yet freed from vagueness, superfluities, and errours. I have elsewhere noticed[13] Faraday’s remark on Davy’s celebrated Memoir of 1806; that ‘the mode of action by which the effects take place is stated very generally, so generally, indeed, that probably a dozen precise schemes of electro-chemical action might be drawn up, differing 378 essentially from each other, yet all agreeing with the statement there

given.' When Davy and others proceeded to give a little more definiteness and precision to the statement of their views, they soon introduced into the theory features which it was afterwards found necessary to abandon. Thus[14] both Davy, Grotthus, Riffault, and Chompré, ascribed electrical decomposition to the action of the *poles*, and some of them even pretended to assign the proportion in which the force of the pole diminishes as the distance from it increases. Faraday, as I have already stated, showed that the polarity must be considered as residing not only in what had till then been called the *poles*, but at every point of the circuit. He ascribed[15] electro-chemical decomposition to internal forces, residing in the *particles* of the matter under decomposition, not to external forces, exerted by the poles. Hence he shortly afterwards[16] proposed to reject the word *poles* altogether, and to employ instead, the term *electrode*, meaning the doors or passages (of whatever surface formed) by which the decomposed elements pass out. What have been called the *positive* and *negative* poles he further termed the

Anode and *Cathode*; and he introduced some other changes in nomenclature connected with these. He then, as I have related in the History[17], invented the Volta-electrometer, which enabled him to measure the quantity of voltaic action, and this he found to be identical with the quantity of chemical affinity; and he was thus led to the clearest view of the truth towards which he and his predecessors had so long been travelling, that electrical and chemical forces are identical[18].

[13] *Hist. Ind. Sc.* b. xiv. c. ix. sect. 1.

[14] See Faraday's Historical Sketch, *Researches*, 481–492.

[15] Art. 524.

[16] In 1834. Eleventh Series of Researches. Art. 662.

[17] *Hist. Ind. Sc.* b. xiv. c. ix. sect. 2.

[18] Arts. 915, 916, 917.

7. It will, perhaps, be said that this beautiful train of discovery was entirely due to experiment, and not to any *à priori* conviction that co-existent polarities 379

must be connected. I trust I have sufficiently stated that such an *à priori* principle could not be proved, nor even understood, without a most laborious and enlightened use of experiment; but yet I think that the doctrine, when once fully unfolded, exhibited clearly, and established as true, takes possession of the mind with a more entire conviction of its certainty and universality, in virtue of the principle we are now considering. When the theory has assumed so simple a form, it appears to derive immense probability (to say the least) from its simplicity. Like the laws of motion, when stated in its most general form, it appears to carry with it its own evidence. And thus this great theory borrows something of its character from the Ideas which it involves, as well as from the Experiments by which it was established.

8. We may find in many of Mr. Faraday's subsequent reasonings, clear evidence that this idea of the Connexion of Polarities, as now developed, is not limited in its application to facts already known

experimentally, but, like other ideas, determines the philosopher's researches into the unknown, and gives us the *form* of knowledge even before we possess the *matter*. Thus, he says, in his Thirteenth Series[19], 'I have long sought, and still seek, for an effect or condition which shall be to statical electricity what magnetic force is to current electricity; for as the lines of discharge are associated with a certain transverse effect, so it appeared to me impossible but that the lines of tension or of inductive action, which of necessity precede the discharge, should also have their correspondent transverse condition or effect.' Other similar passages might be found.

[19] Art. 1658.

I will now consider another case to which we may apply the Principle of Connected Polarities.

9. *Connexion of Chemical and Crystalline Polarities.*—The close connexion between the Chemical Affinity and the Crystalline

Attraction of elements cannot be overlooked. Bodies never crystallize but when their elements combine chemically; and solid bodies which 380 combine, when they do it most completely and exactly, also crystallize. The forces which *hold together* the elements of a crystal of alum are the same forces which make it a *crystal*. There is no distinguishing between the two sets of forces.

Both *chemical* and *crystalline* forces are *polar*, as we stated in the last chapter; but the polarity in the two cases is of a different kind. The polarity of chemical forces is then put in the most distinct form, when it is identified with electrical polarity; the polarity of the particles of crystals has reference to their geometrical form. And it is clear that these two kinds of polarity must be connected. Accordingly, Berzelius expressly asserts[20] the necessary identity of these two polarities. 'The regular forms of bodies suppose a polarity which *can be* no other than an electric or magnetic polarity.' This being so seemingly inevitable, we

might expect to find the electric forces manifesting some relation to the definite directions of crystalline forms. Mr. Faraday tried, but in vain, to detect some such relation. He attempted to ascertain[21] whether a cube of rock crystal transmitted the electrical force of tension with different intensity along and across the axis of the crystal. In the first specimen there seemed to be some difference; but in other experiments, made both with rock crystal and with calc spar, this difference disappeared. Although therefore we may venture to assert that there must be some very close connexion between electrical and crystalline forces, we are, as yet, quite ignorant what the nature of the connexion is, and in what kind of phenomena it will manifest itself.

[20] *Essay on Chemical Prop.* 113.

[21] *Researches*. Art. 1689.

10. *Connexion of Crystalline and Optical Polarities.*—Crystals present to us *optical* phenomena which have a manifestly polar

character. The double refraction, both of uniaxal and of biaxal crystals, is always accompanied with opposite polarization of the two rays; and in this and in other ways light is polarized in directions dependent upon the axes of the crystalline form, that is, on the directions of the polarities of the 381 crystalline particles. The identity of these two kinds of polarity (crystalline and optical) is too obvious to need insisting on; and it is not necessary for us here to decide by what hypothesis this identity may most properly be represented. We may hereafter perhaps find ourselves justified in considering the crystalline forces as determining the *elasticity* of the luminiferous ether to be different in *different directions* within the crystal, and thus as determining the refraction and polarization of the light which the crystal transmits. But at present we merely note this case as an additional example of the manifest connexion and fundamental identity of two co-existent polarities.

11. *Connexion of Polarities in general.*—

Thus we find that the Connexion of different kinds of Polarities, magnetic, electric, chemical, crystalline, and optical, is certain as a truth of experimental science. We have attempted to show further that in the minds of several of the most eminent discoverers and philosophers, such a conviction is something more than a mere empirical result: it is a principle which has regulated their researches while it was still but obscurely seen and imperfectly unfolded, and has given to their theories a character of generality and self-evidence which experience alone cannot bestow.

It will, perhaps, be said that these doctrines,—that scientific researches may usefully be directed by principles in themselves vague and obscure;—that theories may have an evidence superior to and anterior to experience;—are doctrines in the highest degree dangerous, and utterly at variance with the soundest maxims of modern times respecting the cultivation of science.

In the justice and wisdom of this caution I entirely agree: and although I have shown that this principle of the *Connexion of Polarities*, rightly interpreted and established in each case by experiment, involves profound and comprehensive truths; I think it no less important to remark that, at least in the present stage of our knowledge, we can make no use of this principle without taking care, at every step, to determine by 382 clear and decisive experiments, its proper meaning and application. All endeavours to proceed otherwise have led, and must lead, to ignorance and confusion. Attempts to deduce from our bare Idea of Polarity, and our fundamental convictions respecting the connexion of polarities, theories concerning the forces which really exist in nature, can hardly have any other result than to bewilder men's minds, and to misdirect their efforts.

So far, indeed, as this persuasion of a connexion among apparently different kinds of agencies, impels men, engaged in the pursuit of knowledge, to collect

observations, to multiply, repeat, and vary experiments, and to contemplate the result of these in all aspects and relations, it may be an occasion of the most important discoveries. Accordingly we find that the great laws of phenomena which govern the motions of the planets about the sun, were first discovered by Kepler, in consequence of his scrutinizing the recorded observations with an intense conviction of the existence of geometrical and arithmetical harmonies in the solar system. Perhaps we may consider the discovery of the connexion of magnetism and electricity by Professor Œrsted in 1820, as an example somewhat of the same kind; for he also was a believer in certain comprehensive but undefined relations among the properties of bodies; and in consequence of such views entertained great admiration for the *Prologue to the Chemistry of the Nineteenth Century*, of Winterl, already mentioned. M. Œrsted, in 1803, published a summary of this work; and in so doing, praised the views of Winterl as far more profound and comprehensive than those of Lavoisier. Soon

afterwards a Review of this publication appeared in France[22], in which it was spoken of as a work only fit for the dark ages, and as the indication of a sect which had for some time ‘ravaged Germany,’ and inundated that country with extravagant and unintelligible mysticism. It was, therefore, a kind of triumph to M. Œrsted to be, after 383 some years’ labour, the author of one of the most remarkable and fertile physical discoveries of his time.

[22] *Ann. Chim.*, Tom. 1. (1804), p. 191.

12. It was not indeed without some reason that certain of the German philosophers were accused of dealing in doctrines vast and profound in their aspect, but, in reality, indefinite, ambiguous, and inapplicable. And the most prominent of such doctrines had reference to the principle now under our consideration; they represented the properties of bodies as consisting in certain polarities, and professed to deduce, from the very nature of things, with little or no reference to experiment, the existence and connexion of

these polarities. Thus Schelling, in his *Ideas towards a Philosophy of Nature*, published in 1803, says[23], 'Magnetism is the universal act of investing Multiplicity with Unity; but the universal form of the reduction of Multiplicity to Unity is the Line, pure Longitudinal Extension: hence Magnetism is determination of pure Longitudinal Extension; and as this manifests itself by absolute Cohesion, Magnetism is the determination of absolute Cohesion.' And as Magnetism was, by such reasoning, conceived to be proved as a universal property of matter, Schelling asserted it to be a confirmation of his views when it was discovered that other bodies besides iron are magnetic. In like manner he used such expressions as the following[24]: 'The threefold character of the Universal, the Particular, and the Indifference of the two, —as expressed in their Identity, is Magnetism, as expressed in their Difference, is Electricity, and as expressed in the Totality, is Chemical Process. Thus these forms are only one form; and the Chemical Process is a mere transfer of the three Points

of Magnetism into the Triangle of Chemistry.’

[23] P. 223.

[24] P. 486.

It was very natural that the chemists should refuse to acknowledge, in this fanciful and vague language, (delivered, however, it is to be recollected, in 1803,) an anticipation of Davy’s doctrine of the identity of electrical and chemical forces, or of Œrsted’s 384 electro-magnetic agency. Yet it was perhaps no less natural that the author of such assertions should look upon every great step in the electro-chemical theory as an illustration of his own doctrines. Accordingly we find Schelling welcoming, with a due sense of their importance, the discoveries of Faraday. When he heard of the experiment in which electricity was produced from common magnetism, he fastened with enthusiasm upon the discovery, even before he knew any of its details, and proclaimed it at a public meeting of a scientific body[25] as one

of the most important advances of modern science. We have (he thus reasoned) three effects of polar forces;—Electro-chemical Decomposition, Electrical Action, Magnetism. Volta and Davy had confirmed experimentally the identity of the two former agencies: Œrsted showed that a closed voltaic circuit acquired magnetic properties: but in order to exhibit the identity of electric and magnetic action it was requisite that electric forces should be extricated from magnetic. This great step Faraday, he remarked, had made, in producing the electric spark by means of magnets.

[25] Ueber Faraday's *Neueste Entdeckung*. München. 1832.

13. Although conjectures and assertions of the kind thus put forth by Schelling involve a persuasion of the pervading influence and connexion of polarities, which persuasion has already been confirmed in many instances, they involve this principle in a manner so vague and ambiguous that it can rarely, in such a form, be of any use or

value. Such views of polarity can never teach us in what cases we are and in what we are not expected to find polar relations; and indeed tend rather to diffuse error and confusion, than to promote knowledge. Accordingly we cannot be surprized to find such doctrines put forward by their authors as an evidence of the small value and small necessity of experimental science. This is done by the celebrated metaphysician Hegel, in his *Encyclopædia*[26]. 'Since,' 385 says he, 'the plane of incidence and of reflection in simple reflection is the same plane, when a second reflector is introduced which further distributes the illumination reflected from the first, the position of the first plane with respect to the second plane, containing the direction of the first reflection and of the second, has its influence upon the position, illumination or darkening of the object as it appears by the second reflection. This influence must be the strongest when the two planes are what we must call *negatively* related to each other:—that is, when they are at right angles.' 'But,' he adds, 'when men infer (as

Malus has done) from the modification which is produced by this situation, in the illumination of the reflection, that the molecules of light in themselves, that is, on their different sides, possess different physical energies; and when on this foundation, along with the phenomena of entoptical colours therewith connected, a wide labyrinth of the most complex theory is erected; we have then one of the most remarkable examples of the *inferences* of physics from experiment.' If Hegel's reasoning prove anything, it must prove that polarization always accompanies reflection under such circumstances as he describes: yet all physical philosophers know that in the case of metals, in which the reflection is most complete, light is not completely polarized at any angle; and that in other substances the polarization depends upon various circumstances which show how idle and inapplicable is the account which he thus gives of the property. His self-complacent remark about the inferences of physics from experiment, is intended to recommend by comparison his own method

of considering the nature of ‘things in themselves;’ a mode of obtaining physical truth which had been more than exhausted by Aristotle, and out of which no new attempts have extracted anything of value since his time.

[26] Sec. 278.

14. Thus the general conclusion to which we are led on this subject, is, that the persuasion of the existence and Connexion or Identity of various Polarities in nature, although very naturally admitted, and in many 386 cases interpreted and confirmed by observed facts, is of itself, so far as we at present possess it, a very insecure guide to scientific doctrines. When it is allowed to dictate our theories, instead of animating and extending our experimental researches, it leads only to errour, confusion, obscurity, and mysticism.

This Fifth Book, on the subject of Polarities, is a short one compared with most of the others. This arises in a great measure from the circumstance that the

Idea of Polarity has only recently been apprehended and applied, with any great degree of clearness, among physical philosophers; and is even yet probably entertained in an obscure and ambiguous manner by most experimental inquirers. I have been desirous of not attempting to bring forward any doctrines upon the subject, except such as have been fully illustrated and exemplified by the acknowledged progress of the physical sciences. If I had been willing to discuss the various speculations which have been published respecting the universal prevalence of Polarities in the universe, and their results in every province of nature, I might easily have presented this subject in a more extended form; but this would not have been consistent with my plan of tracing the influence of scientific Ideas only so far as they have really aided in disclosing and developing scientific truths. And as the influence of this Idea is clearly distinguishable both from those which precede and those which follow, in the character of the sciences to which it gives

rise, and as it appears likely to be hereafter of great extent and consequence, it seemed better to treat of it in a separate Book, although of a brevity disproportioned to the rest.

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BOOK VI.

THE

PHILOSOPHY

OF

CHEMISTRY.

A PHILOSOPHER was asked:—How much does smoke weigh? He answered: Subtract from the weight of the fuel the weight of the ashes, and thou hast the weight of the smoke. Thus he assumed as incontrovertible that, even in the fire, the Substance does not perish, only its Form undergoes a change. In like manner the proposition, *Nothing can come of Nothing* was only another consequence of the Principle of Permanence, or rather of the Principle of the

Enduring Existence of the same subject with different appearances.

Kant, *Kritik d. r. Vern.*

BOOK VI.

THE PHILOSOPHY OF CHEMISTRY.

CHAPTER I.

Attempts to conceive Elementary Composition.

1. We have now to bring into view, if possible, the Ideas and General Principles which are involved in Chemistry,—the science of the composition of bodies. For in this as in other parts of human knowledge, we shall find that there are certain Ideas, deeply seated in the mind, though shaped and unfolded by external observation, which are necessary conditions of the

existence of such a science. These Ideas it is, which impel man to such a knowledge of the Composition of bodies, which give *meaning* to *facts* exhibiting this composition, and *universality* to *special* truths discovered by experience. These are the Ideas of *Element* and of *Substance*.

Unlike the Idea of Polarity, of which we treated in the last Book, these Ideas have been current in men's minds from very early times, and formed the subject of some of the first speculations of philosophers. It happened however, as might have been expected, that in the first attempts they were not clearly distinguished from other notions, and were apprehended and applied in an obscure and confused manner. We cannot better exhibit the peculiar character and meaning of these Ideas than by tracing the form which they have assumed 4 and the efficacy which they have exerted in these successive essays. This, therefore, I shall endeavour to do, beginning with the Idea of Element.

2. That bodies are composed or made up of certain parts, elements, or principles, is a conception which has existed in men's minds from the beginning of the first attempts at speculative knowledge. The doctrine of the Four Elements, Earth, Air, Fire and Water, of which all things in the universe were supposed to be constituted, is one of the earliest forms in which this conception was systematized; and this doctrine is stated by various authors to have existed as early as the times of the ancient Egyptians[1]. The words usually employed by Greek writers to express these elements are ἀρχὴ a *principle* or *beginning*, and στοιχεῖον, which probably meant a *letter* (of a word) before it meant an *element* of a compound. For the resolution of a word into its letters is undoubtedly a remarkable instance of a successful analysis performed at an early stage of man's history; and might very naturally supply a metaphor to denote the analysis of substances into their intimate parts, when men began to contemplate such an analysis as a subject of speculation. The Latin word *elementum* itself, though by its

form it appears to be a derivative abstract term, comes from some root now obsolete; probably[2] from a word signifying *to grow* or *spring up*.

[1] Gilbert's *Phys.* 1. i. c. iii.

[2] Vossius *in voce*. "Conjecto esse ab antiqua voco *eleo* pro *oleo*, id est *cresco*: a qua signiflcatione proles, *suboles*, *adolescens*: ut ab *juratum*, *juramentum*; ab *adjutum*, *adjumentum*: sic ab *eletum*, *elementum*: quia inde omnia crescunt ac nascuntur."

The mode in which elements form the compound bodies and determine their properties was at first, as might be expected, vaguely and variously conceived. It will, I trust, hereafter be made clear to the reader that the relation of the elements to the compound involves a peculiar and appropriate Fundamental Idea, not susceptible of being correctly represented by any comparison or combination of other ideas, and guiding us to clear and definite results only when it is illustrated 5 and nourished by an abundant supply of experimental facts. But at first the peculiar

and special notion which is required in a just conception of the constitution of bodies was neither discerned nor suspected; and up to a very late period in the history of chemistry, men went on attempting to apprehend the constitution of bodies more clearly by substituting for this obscure and recondite idea of Elementary Composition, some other idea more obvious, more luminous, and more familiar, such as the ideas of Resemblance, Position, and mechanical Force. We shall briefly speak of some of these attempts, and of the errours which were thus introduced into speculations on the relations of elements and compounds.

3. *Compounds assumed to resemble their Elements.*—The first notion was that compounds derive their qualities from their elements by *resemblance*:—they are hot in virtue of a hot element, heavy in virtue of a heavy element, and so on. In this way the doctrine of the *four elements* was framed; for every body is either hot or cold, moist or dry; and by combining these qualities in all

possible ways, men devised four elementary substances, as has been stated in the History[3].

[3] *Hist. Ind. Sc.* b. i. c. ii. sec. 2.

This assumption of the derivation of the qualities of bodies from similar qualities in the elements was, as we shall see, altogether baseless and unphilosophical, yet it prevailed long and universally. It was the foundation of medicine for a long period, both in Europe and Asia; disorders being divided into hot, cold, and the like; and remedies being arranged according to similar distinctions. Many readers will recollect, perhaps, the story[4] of the indignation which the Persian physicians felt towards the European, when he undertook to cure the ill effects of cucumber upon the patient, by means of mercurial medicine: for cucumber, which is cold, could not be counteracted, they maintained, by mercury, which in their classification is cold also. Similar views of the operation of medicines might 6 easily be traced in our own country. A moment's reflection may

convince us that when drugs of any kind are subjected to the chemistry of the human stomach and thus made to operate on the human frame, it is utterly impossible to form the most remote conjecture what the result will be, from any such vague notions of their qualities as the common use of our senses can give. And in like manner the common operations of chemistry give rise, in almost every instance, to products which bear no resemblance to the materials employed. The results of the furnace, the alembic, the mixture, frequently have no visible likeness to the ingredients operated upon. Iron becomes steel by the addition of a little charcoal; but what visible trace of the charcoal is presented by the metal thus modified? The most beautiful colours are given to glass and earthenware by minute portions of the ores of black or dingy metals, as iron and manganese. The worker in metal, the painter, the dyer, the vintner, the brewer, all the artisans in short who deal with practical chemistry, are able to teach the speculative chemist that it is an utter mistake to expect that the qualities of

the elements shall be still discoverable, in an unaltered form, in the compound. This first rude notion of an element, that it determines the properties of bodies *by resemblance*, must be utterly rejected and abandoned before we can make any advance towards a true apprehension of the constitution of bodies.

[4] See *Hadji Baba*.

4. This step accordingly was made, when the hypothesis of the four elements was given up, and the doctrine of the *three Principles*, Salt, Sulphur, and Mercury, was substituted in its place. For in making this change, as I have remarked in the History[5], the real advance was the acknowledgment of the changes, produced by the chemist's operations, as results to be accounted for by the union and separation of substantial elements, however great the changes, and however unlike the product might be to the materials. And this step once made, chemists went on constantly 7 advancing towards a truer view of the nature of an element, and consequently, towards a more

satisfactory theory of chemical operations.

[5] *Hist. Ind. Sc.* b. iv. c. 1.

5. Yet we may, I think, note one instance, even in the works of eminent modern chemists, in which this maxim, that we have no right to expect any resemblance between the elements and the compound, is lost sight of. I speak of certain classifications of mineral substances. Berzelius, in his System of Mineral Arrangement, places *sulphur* next to the *sulphurets*. But surely this is an errour, involving the ancient assumption of the resemblance of elements and compounds; as if we were to expect the sulphurets to bear a resemblance to sulphur. All classifications are intended to bring together things resembling each other: the sulphurets of metals have certain general resemblances to each other which make them a tolerably distinct, well determined, class of bodies. But sulphur has no resemblances with these, and no analogies with them, either in physical or even in chemical properties. It is a simple body; and both its resemblances and its analogies direct us to place it along

with other simple bodies, (selenium, and phosphorus,) which, united with metals, produce compounds not very different from the sulphurets. Sulphur cannot be, nor approach to being, a sulphuret; we must not confound what it *is* with what it *makes*. Sulphur has its proper influence in determining the properties of the compound into which it enters; but it does not do this according to resemblance of qualities, or according to any principle which properly leads to propinquity in classification.

6. *Compounds assumed to be determined by the Figure of Elements.*—I pass over the fanciful modes of representing chemical changes which were employed by the Alchemists; for these strange inventions did little in leading men towards a juster view of the relations of elements to compounds. I proceed for an instant to the attempt to substitute another obvious conception for the still obscure notion of elementary composition. It was imagined that all the properties of bodies and their mutual operations might be 8 accounted for by

supposing them constituted of *particles* of various *forms*, round or angular, pointed or hooked, straight or spiral. This is a very ancient hypothesis, and a favourite one with many casual speculators in all ages. Thus Lucretius undertakes to explain why wine passes rapidly through a sieve and oil slowly, by telling us that the latter substance has its particles either larger than those of the other, or more hooked and interwoven together. And he accounts for the difference of sweet and bitter by supposing the particles in the former case to be round and smooth, in the latter sharp and jagged[6]. Similar assumptions prevailed in modern times on the revival of the mechanical philosophy, and constitute a large part of the physical schemes of Descartes and Gassendi. They were also adopted to a considerable extent by the chemists. Acids were without hesitation assumed to consist of sharp pointed particles; which, 'I hope,' Lemery says[7], 'no one will dispute, seeing every one's experience does demonstrate it: he needs but taste an acid to be satisfied of it, for it

pricks the tongue like anything keen and finely cut.' Such an assumption is not only altogether gratuitous and useless, but appears to be founded in some degree upon a confusion in the metaphorical and literal use of such words as *keen* and *sharp*. The assumption once made, it was easy to accommodate it, in a manner equally arbitrary, to other facts. 'A demonstrative and convincing proof that an acid does consist of pointed parts is, that not only all acid salts do crystallize into edges, but all dissolutions of different things, caused by acid liquors, do assume this figure in their crystallization. These crystals consist of points differing both in length and bigness one from another, and this diversity must be attributed to the keener or blunter edges of the different sorts of acids: and so likewise this difference of the points in subtilty is the cause that one acid can penetrate and dissolve with one sort of *mixt*, that another can't rarify at all: Thus *vinegar* dissolves *lead*, 9 which *aqua fortis* can't: *aqua fortis* dissolves *quicksilver*, which *vinegar* will not touch; *aqua regalis* dissolves *gold*, whenas

aqua fortis cannot meddle with it; on the contrary, *aqua fortis* dissolves *silver*, but can do nothing with gold, and so of the rest.'

[6] *De Rerum Natura*, ii. 390 sqq.

[7] *Chemistry*, p. 25.

The leading fact of the vehement combination and complete union of acid and alkali readily suggested a fit form for the particles of the latter class of substances. 'This effect,' Lemery adds, 'may make us reasonably conjecture that an alkali is a terrestrious and solid matter whose forms are figured after such a manner that the acid points entering in do strike and divide whatever opposes their motion.' And in a like spirit are the speculations in Dr. Mead's *Mechanical Account of Poisons* (1745). Thus he explains the poisonous effect of *corrosive sublimate* of mercury by saying[8] that the particles of the salt are a kind of lamellæ or blades to which the mercury gives an additional weight. If resublimed with three-fourths the quantity of mercury, it loses its corrosiveness, (becoming *calomel*,) which

arises from this, that in sublimation ‘the crystalline blades are divided every time more and more by the force of the fire:’ and ‘the broken pieces of the crystals uniting into little masses of differing figures from their former make, those cutting points are now so much smaller that they cannot make wounds deep enough to be equally mischievous and deadly: and therefore do only vellicate and twitch the sensible membranes of the stomach.’

[8] P. 199.

7. Among all this very fanciful and gratuitous assumption we may notice one true principle clearly introduced, namely, that the suppositions which we make respecting the forms of the elementary particles of bodies and their mode of combination must be such as to explain the facts of crystallization, as well as of mere chemical change. This principle we shall hereafter have occasion to insist upon further.

I now proceed to consider a more refined

form of assumption respecting the constitution of bodies, yet 10 still one in which a vain attempt is made to substitute for the peculiar idea of chemical composition a more familiar mechanical conception.

8. *Compounds assumed to be determined by the Mechanical Attraction of the Elements.*— When, in consequence of the investigations and discoveries of Newton and his predecessors, the conception of mechanical force had become clear and familiar, so far as the action of external forces upon a body was concerned, it was very natural that the mathematicians who had pursued this train of speculation should attempt to apply the same conception to that mutual action of the internal parts of a body by which they are held together. Newton himself had pointed the way to this attempt. In the Preface to the *Principia*, after speaking of what he has done in calculating the effects of forces upon the planets, satellites, &e., he adds, ‘Would it were permitted us to deduce the other phenomena of nature from

mechanical principles by the same kind of reasoning. For many things move me to suspect that all these phenomena depend upon certain forces, by which the particles of bodies, through causes not yet known, are either urged towards each other, and cohere according to regular figures, or are repelled and recede from each other; which forces being unknown, philosophers have hitherto made their attempts upon nature in vain.' The same thought is at a later period followed out further in one of the Queries at the end of the Opticks[9]. 'Have not the small particles of bodies certain Powers, Virtues, or Forces, by which they act at a distance, not only upon the rays of light for reflecting, refracting and inflecting them, but also upon one another for producing a great part of the phenomena of nature?' And a little further on he proceeds to apply this expressly to chemical changes. 'When Salt of Tartar runs *per deliquium* [or as we now express it, deliquesces] is not this done by an *attraction* between the particles of the Salt of Tartar and the particles of the water which float in the air in 11 the form of

vapours? And why does not common salt, or saltpetre, or vitriol, run *per deliquium*, but for want of such an attraction? or why does not Salt of Tartar draw more water out of the air than in a certain proportion to its quantity, but for want of an attractive force after it is saturated with water?' He goes on to put a great number of similar cases, all tending to the same point, that chemical combinations cannot be conceived in any other way than as an attraction of particles.

[9] Query 31.

9. Succeeding speculators in his school attempted to follow out this view. Dr. Frend, of Christ Church, in 1710, published his *Prælectiones Chymicæ, in quibus omne fere Operationes Chymicæ ad vera Principia ex ipsius Naturæ Legibus rediguntur. Oxonii habitæ*. This book is dedicated to Newton, and in the dedication, the promise of advantage to chemistry from the influence of the Newtonian discoveries is spoken of somewhat largely,—much more largely, indeed, than has yet been justified by the sequel. After declaring in strong terms that

the only prospect of improving science consists in following the footsteps of Newton, the author adds, ‘That force of attraction, of which you first so successfully traced the influence in the heavenly bodies, operates in the most minute corpuscles, as you long ago hinted in your *Principia*, and have lately plainly shown in your *Opticks*; and this force we are only just beginning to perceive and to study. Under these circumstances I have been desirous of trying what is the result of this view in chemistry.’ The work opens formally enough, with a statement of general mechanical principles, of which the most peculiar are these:—‘That there exists an attractive force by which particles when at very small distances from each other, are drawn together;—that this force is different, according to the different figure and density of the particles;—that the force may be greater on one side of a particle than on the other;—that the force by which particles cohere together arises from attraction, and is variously modified according to the quantity of contacts.’ But these principles are not 12 applied in any

definite manner to the explanation of specific phenomena. He attempts, indeed, the question of special solvents[10]. Why does *aqua fortis* dissolve silver and not gold, while *aqua regia* dissolves gold and not silver? which, he says, is the most difficult question in chemistry, and which is certainly a fundamental question in the formation of chemical theory. He solves it by certain assumptions respecting the forces of attraction of the particles, and also the diameter of the particles of the acids and the pores of the metals, all which suppositions are gratuitous.

[10] P. 54.

10. We may observe further, that by speaking, as I have stated that he does, of the figure of particles, he mixes together the assumption of the last section with the one which we are considering in this. This combination is very unphilosophical, or, to say the least, very insufficient, since it makes a new hypothesis necessary. If a body be composed of cubical particles, held together by their mutual attraction, by what

force are the parts of each cube held together? In order to understand their structure, we are obliged again to assume a cohesive force of the second order, binding together the particles of each particle. And therefore Newton himself says[11], very justly, 'The parts of all homogeneal hard bodies which fully touch each other, stick together very strongly: and for explaining how this is, some have invented hooked atoms, *which is begging the question*.' For (he means to imply,) how do the parts of the hook stick together?

[11] *Opticks*, p. 364.

The same remark is applicable to all hypotheses in which particles of a complex structure are assumed as the constituents of bodies: for while we suppose bodies and their known properties to result from the mutual actions of these particles, we are compelled to suppose the parts of each particle to be held together by forces still more difficult to conceive, since they are disclosed only by the properties of these particles, which as yet are unknown. Yet

Newton himself has not abstained from such hypotheses: thus he says[12], 'A particle of 13 a salt may be compared to a chaos, being dense, hard, dry, and earthy in the center, and moist and watery in the circumference.'

[12] *Opticks*, p. 362.

Since Newton's time the use of the term *attraction*, as expressing the cause of the union of the chemical elements of bodies, has been familiarly continued; and has, no doubt, been accompanied in the minds of many persons with an obscure notion that chemical attraction is, in some way, a kind of mechanical attraction of the particles of bodies. Yet the doctrine that *chemical* 'attraction' and *mechanical* attraction are forces of the same kind has never, so far as I am aware, been worked out into a system of chemical theory; nor even applied with any distinctness as an explanation of any particular chemical phenomena. Any such attempt, indeed, could only tend to bring more clearly into view the entire inadequacy of such a mode of explanation. For the leading phenomena of chemistry are

all of such a nature that no mechanical combination can serve to express them, without an immense accumulation of additional hypotheses. If we take as our problem the changes of colour, transparency, texture, taste, odour, produced by small changes in the ingredients, how can we expect to give a mechanical account of these, till we can give a mechanical account of colour, transparency, texture, taste, odour, themselves? And if our mechanical hypothesis of the elementary constitution of bodies does not explain *such* phenomena as those changes, what can it explain, or what can be the value of it? I do not here insist upon a remark which will afterwards come before us, that even crystalline form, a phenomenon of a far more obviously mechanical nature than those just alluded to, has never yet been in any degree explained by such assumptions as this, that bodies consist of elementary particles exerting forces of the same nature as the central forces which we contemplate in Mechanics.

When therefore Newton asks, ‘When some stones, as spar of lead, dissolved in proper menstruums, become salts, do not these things show that salts are dry earth and watery acid united by *attraction*?’ we may 14 answer, that this mode of expression appears to be intended to identify chemical combination with mechanical attraction;—that there would be no objection to any such identification, if we could, in that way, explain, or even classify well, a collection of chemical facts; but that this has never yet been done by the help of such expressions. Till some advance of this kind can be pointed out, we must necessarily consider the power which produces chemical combination as a peculiar principle, a special relation of the elements, not rightly expressed in mechanical terms. And we now proceed to consider this relation under the name by which it is most familiarly known.

CHAPTER II.

Establishment and Development of the Idea of Chemical Affinity.

1. THE earlier chemists did not commonly involve themselves in the confusion into which the mechanical philosophers ran, of comparing chemical to mechanical forces. Their attention was engaged, and their ideas were moulded, by their own pursuits. They saw that the connexion of elements and compounds with which they had to deal, was a peculiar relation which must be studied directly; and which must be understood, if understood at all, in itself, and not by comparison with a different class of relations. At different periods of the progress of chemistry, the conception of this relation, still vague and obscure, was expressed in various manners;

and at last this conception was clothed in tolerably consistent phraseology, and the principles which it involved were, by the united force of thought and experiment, brought into view.

2. The power by which the elements of bodies combine chemically, being, as we have seen, a peculiar agency, different from mere mechanical connexion or attraction, it is desirable to have it designated by a distinct and peculiar name; and the term *Affinity* has been employed for that purpose by most modern chemists. The word ‘affinity’ in common language means, sometimes resemblance, and sometimes relationship and ties of family. It is from the latter sense that the metaphor is borrowed when we speak of ‘chemical affinity.’ By the employment of this term we do not indicate a resemblance, but a disposition to unite. Using the word in a common unscientific manner, we might say that chlorine, bromine, and iodine, have a great 16 *natural affinity* with each other, for there are considerable resemblances and analogies

among them; but these bodies have very little *chemical* Affinity for each other. The use of the word in the *former* sense, of resemblance, can be traced in earlier chemists; but the word does not appear to have acquired its peculiar chemical meaning till after Boerhaave's time. Boerhaave, however, is the writer in whom we first find a due apprehension of the peculiarity and importance of the Idea which it now expresses. When we make a chemical solution[13], he says, not only are the particles of the dissolved body separated from each other, but they are closely united to the particles of the solvent. When *aqua regia* dissolves gold, do you not see, he says to his hearers, that there must be between each particle of the solvent and of the metal, a mutual virtue by which each loves, unites with, and holds the other (*amat*, *unit*, *retinet*)? The opinion previously prevalent had been that the solvent merely separates the parts of the body dissolved: and most philosophers had conceived this separation as performed by mechanical operations of the particles, resembling, for instance, the

operation of wedges breaking up a block of timber. But Boerhaave forcibly and earnestly points out the insufficiency of the conception. This, he says, does not account for what we see. We have not only a separation, but a new combination. There is a force by which the particles of the solvent associate to themselves the parts dissolved, not a force by which they repel and dissever them. We are here to imagine not mechanical action, not violent impulse, not antipathy, but love, at least if love be the desire of uniting. (Non igitur hic etiam actiones mechanicæ, non propulsiones violentæ, non inimicitiæ cogitandæ, sed amicitiæ, si amor dicendus copulæ cupido.) The novelty of this view is evidenced by the mode in which he apologizes for introducing it. 'Fateor, paradoxa hæc assertio.' To Boerhaave, therefore, (especially considering his great influence as a teacher of chemistry,) we may 17 assign the merit of first diffusing a proper view of Chemical Affinity as a peculiar force, the origin of almost all chemical changes and operations.

[13] *Elementa Chemiæ*, Lugd. Bat. 1732, p. 677.

3. To Boerhaave is usually assigned also the credit of introducing the *word* 'Affinity' among chemists; but I do not find that the word is often used by him in this sense; perhaps not at all[14]. But however this may be, the term is, on many accounts, well worthy to be preserved, as I shall endeavour to show. Other terms were used in the same sense during the early part of the eighteenth century. Thus when Geoffroy, in 1718, laid before the Academy of Paris his Tables of Affinities, which perhaps did more than any other event to fix the Idea of Affinity, he termed them 'Tables of the Relations of Bodies;' '*Tables des Rapports*:' speaking however, also, of their 'disposition to unite,' and using other phrases of the same import.

[14] See Dumas, *Leçons de Phil. Chim.* p. 364. Rees' *Cyclopædia*, Art. Chemistry. In the passage of Boerhaave to which I refer above, *affinitas* is rather opposed to, than identified with, chemical combination. When, he says, the parts of the body to be dissolved are dissevered by the solvent, why do they remain united to the particles of the solvent, and why do not rather both

> the particles of the solvent and of the dissolved body collect into homogeneous bodies by their *affinity*? ‘denuo se affinitate suæ naturæ colligant in corpora homogenea?‘ And the answer is, because they possess another force which counteracts this affinity of homogeneous particles, and makes compounds of different elements. Affinity, in chemistry, now means the tendency of *different* kinds of matter to unite: but it appears, as I have said, to have acquired this sense since Boerhaave’s time.

The term *attraction*, having been recommended by Newton as a fit word to designate the force which produces chemical combination, continued in great favour in England, where the Newtonian philosophy was looked upon as applicable to every branch of science. In France, on the contrary, where Descartes still reigned triumphant, ‘attraction,’ the watch-word of the enemy, was a sound never uttered but with dislike and suspicion. In 1718 (in the notice of Geoffroy’s Table,) the Secretary of the Academy, after pointing out some of the peculiar circumstances of chemical 18 combinations, says, ‘Sympathies and attractions would suit well here, if there

were such things,' 'Les sympathies, les attractions conviendroient bien ici, si elles étaient quelque chose.' And at a later period, in 1731, having to write the *éloge* of Geoffroy after his death, he says, 'He gave, in 1718, a singular system, and a Table of *Affinities*, or Relations of the different substances in chemistry. These affinities gave an easiness to some persons, who feared that they were *attractions in disguise*, and all the more dangerous in consequence of the seductive forms which clever people have contrived to give them. It was found in the sequel that this scruple might be got over.'

This is the earliest published instance, so far as I am aware, in which the word 'Affinity' is distinctly used for the cause of chemical composition; and taking into account the circumstances, the word appears to have been adopted in France in order to avoid the word *attraction*, which had the taint of Newtonianism. Accordingly we find the word *affinité* employed in the works of French chemists from this time.

Thus, in the *Transactions of the French Academy* for 1746, in a paper of Macquer's upon Arsenic, he says[15], 'On peut facilement rendre raison de ces phenomènes par le moyen des affinités que les différens substances qui entrent dans ces combinaisons, ont les uns avec les autres:' and he proceeds to explain the facts by reference to Geoffroy's Table. And in Macquer's *Elements of Chemistry*, which appeared a few years later, the 'Affinity of Composition' is treated of as a leading part of the subject, much in the same way as has been practised in such books up to the present time. From this period, the word appears to have become familiar to all European chemists in the sense of which we are now speaking. Thus, in the year 1758, the Academy of Sciences at Rouen offered a prize for the best dissertation on Affinity. The prize was shared between M. Limbourg of Theux, near Liege, and M. Le Sage 19 of Geneva[16]. About the same time other persons (Manherr[17], Nicolai[18], and others) wrote on the same subject, employing the same name.

[15] *A. P.* 1746, p. 201.

[16] Thomson's *Chemistry*, iii. 10. Limbourg's Dissertation was published at Liege, in 1761; and Le Sage's at Geneva.

[17] *Dissertatio de Affinitate Corporum*. Vindob. 1762.

[18] *Progr.* I. II. *de Affinitate Corporum Chimica*. Jen. 1775, 1776.

Nevertheless, in 1775, the Swedish chemist Bergman, pursuing still further this subject of Chemical Affinities, and the expression of them by means of Tables, returned again to the old Newtonian term; and designated the disposition of a body to combine with one rather than another of two others as *Elective Attraction*. And as his work on *Elective Attractions* had great circulation and great influence, this phrase has obtained a footing by the side of *Affinity*, and both one and the other are now in common use among chemists.

4. I have said above that the term *Affinity* is worthy of being retained as a technical term. If we use the word *attraction* in this case, we identify or compare chemical with

mechanical attraction; from which identification and comparison, as I have already remarked, no one has yet been able to extract the means of expressing any single scientific truth. If such an identification or comparison be not intended, the use of the same word in two different senses can only lead to confusion; and the proper course, recommended by all the best analogies of scientific history, is to adopt a peculiar term for that peculiar relation on which chemical composition depends. The word *Affinity*, even if it were not rigorously proper according to its common meaning, still, being simple, familiar, and well established in this very usage, is much to be preferred before any other.

But further, there are some analogies drawn from the common meaning of this word, which appear to recommend it as suitable for the office which it has to discharge. For common mechanical attractions and 20 repulsions, the forces by which one body considered as a *whole* acts

upon another external to it, are, as we have said, to be distinguished from those more intimate ties by which the *parts* of each body are held together. Now this difference is implied, if we compare the former relations, the attractions and repulsions, to alliances and wars between States, and the latter, the internal union of particles, to those bonds of affinity which connect the citizens of the same state with one another, and especially to the ties of Family. We have seen that Boerhaave compares the union of two elements of a compound to their marriage; 'we must allow,' says an eminent chemist of our own time[19], 'that there is some truth in this poetical comparison.' It contains this truth,—that the two become one to most intents and purposes, and that the Unit thus formed (the Family) is not a mere juxtaposition of the component parts. And thus the Idea of Affinity as the peculiar principle of chemical composition, is established among chemists, and designated by a familiar and appropriate name.

[19] Dumas, *Leçons de Phil. Chim.* p. 363.

5. *Analysis is possible.*—We must, however, endeavour to obtain a further insight into this Idea, thus fixed and named. We must endeavour to extricate, if not from the Idea itself, from the processes by which it has obtained acceptation and currency among chemists, some principles which may define its application, some additional specialities in the relations which it implies. This we shall proceed to do.

The Idea of Affinity, as already explained, implies a disposition to combine. But this combination is to be understood as admitting also of a possibility of separation. Synthesis implies Analysis as conceivable: or to recur to the image which we have already used, Divorce is possible when the Marriage has taken place.

That there is this possibility, is a conviction implied in all the researches of chemists, ever since the true notion of composition began to predominate in their investigations. One of the first persons who

clearly 21 expressed this conviction was Mayow, an English physician, who published his *Medico-Physical Tracts* in 1674. The first of them *De Sale-Nitro et Spiritu Nitro-Aerio*, contains a clear enunciation of this principle. After showing how, in the combinations of opposite elements, as acid and alkali, their properties entirely disappear, and a new substance is formed not at all resembling either of the ingredients, he adds[20], 'Although these salts thus mixed appear to be destroyed it is still possible for them to be separated from each other, with their powers still entire.' He proceeds to exemplify this, and illustrates it by the same image which I have already alluded to: 'Salia acida a salibus volatilibus discedunt, ut cum sale fixo tartari, tanquam *sponso* magis idoneo, *conjugium* strictius ineunt.' This idea of a synthesis which left a complete analysis still possible, was opposed to a notion previously current, that when two heterogeneous bodies united together and formed a third body, the two constituents were entirely destroyed, and the result formed out of their ruins[21]. And

this conception of Synthesis and Analysis, as processes which are possible successively and alternately, and each of which supposes the possibility of the other, has been the fundamental and regulative principle of the operations and speculations of analytical chemistry from the time of Mayow to the present day.

[20] Cap. xiv. p. 233.

[21] Thomson's *Chemistry*, iii. 8.

6. *Affinity is Elective.*—When the idea of chemical affinity, or disposition to unite, was brought into view by the experiments and reasonings of chemists, they found it necessary to consider this disposition as *elective*;—each element *chose* one rather than another of the elements which were presented to it, and quitted its union with one to unite with another which it preferred. This has already appeared in the passage just quoted from Mayow. He adds in the same strain, 'I have no doubt that fixed salts choose one acid rather than another, in order that they may coalesce

with it 22 in a more intimate union.'—'Nullus dubito salia fixa acidum unum præ aliis *eligere*, ut cum eodem arctiore unione coalescant.' The same thought is expressed and exemplified by other chemists: they notice innumerable cases in which, when an ingredient is combined with a liquid, if a new substance be immersed which has a greater affinity for the liquid, the liquid combines with the new substance by election, and the former ingredient is *precipitated*. Thus Stahl says[22], 'In spirit of nitre dissolve silver; put in copper and the silver is thrown down; put in iron and the copper goes down; put in zinc, the iron precipitates; put in volatile alkali, the zinc is separated; put in fixed alkali, the volatile quits its hold.'—As may be seen in this example, we have in such cases, not only a preference, but a long gradation of preferences. The spirit of nitre will combine with silver, but it prefers copper; prefers iron more; zinc still more; volatile alkali yet more; fixed alkali the most.

[22] *Zymotechnia*, 1697, p. 117.

The same thing was proved to obtain with regard to each element; and when this was ascertained, it became the object of chemists to express these degrees of preference, by lists in which substances were arranged according to their disposition to unite with another substance. In this manner was formed Geoffroy's Table of Affinities (1718), which we have already mentioned. This Table was further improved by other writers, as Gellert (1751) and Limbourg (1761). Finally Bergman improved these Tables still further, taking into account not only the order of affinities of each element for others, but the *sum* of the tendencies to unite of each two elements, which sum, he held, determined the resulting combination when several elements were in contact with each other.

7. As we have stated in the History[23], when the doctrine of elective affinities had assumed this very definite and systematic form, it was assailed by Berthollet, who maintained, in his *Essai de Statique* 23 *Chimique*, (1803,) that chemical affinities

are *not* elective:—that, when various elements are brought together, their combinations do not depend upon the kind of elements alone, but upon the quantity of each which is present, that which is most abundant always entering most largely into the resulting compounds. It may seem strange that it should be possible, at so late a period of the science, to throw doubt upon a doctrine which had presided over and directed its progress so long. Proust answered Berthollet, and again maintained that chemical affinity is elective. I have, in the History, given the judgment of Berzelius upon this controversy. 'Berthollet,' he says, 'defended himself with an acuteness which makes the reader hesitate in his judgment; but the great mass of facts finally decided the point in favour of Proust.' I may here add the opinion pronounced upon this subject by Dr. Turner[24]: 'Bergman erred in supposing the result of the chemical action to be in every case owing to elective affinity [for this power is modified in its effects by various circumstances]: but Berthollet ran into the opposite extreme in declaring that

the effects formerly ascribed to that power are never produced by it. That chemical attraction is exerted between different bodies with different degrees of energy, is, I apprehend, indisputable.' And he then proceeds to give many instances of differences in affinity which cannot be accounted for by the operation of any modifying causes. Still more recently, M. Dumas has taken a review of this controversy; and, speaking with enthusiasm of the work of Berthollet, as one which had been of inestimable service to himself in his early study of chemistry, he appears at first disposed to award to him the victory in this dispute. But his final verdict leaves undamaged the general principle now under our consideration, that chemical affinity is elective. 'For my own part,' he says[25], 'I willingly admit the notions of Berthollet when we have to do with acids or 24 with bases, of which the energy is nearly equal: but when bodies endued with very energetic affinities are in presence of other bodies of which the affinities are very feeble, I propose to adopt the following rule: In a

solution, everything remaining dissolved, the strong affinities satisfy themselves, leaving the weak affinities to arrange matters with one another. The strong acids take the strong bases, and the weak acids can only unite with the weak bases. The known facts are perfectly in accordance with this practical rule.' It is obvious that this recognition of a distinction between strong and weak affinities, which operates to such an extent as to determine entirely the result, is a complete acknowledgement of the Elective nature of Affinity, as far as any person acquainted with chemical operations could contend for it. For it must be allowed by all, that solubility, and other collateral circumstances, influence the course of chemical combinations, since they determine whether or not there shall take place that contact of elements without which affinity cannot possibly operate.

[23] *Hist. Ind. Sc.* b. xiv. c. iii.

[24] *Chemistry*, p. 199. 6th edition.

[25] *Leçons de Philosophie Chimique*, p. 386.

8. *Affinity is Definite as to quantity.*—In proportion as chemists obtained a clearer view of the products of the laboratory as results of the composition of elements, they saw more and more clearly that these results were definite; that one element not only preferred to combine with another of a certain kind, but also would combine with it to a certain extent and no further, thus giving to the result not an accidental and variable, but a fixed and constant character. Thus salts being considered as the result of the combination of two opposite principles, acid and alkali, and being termed *neutral* when these principles exactly balanced each other, Rouelle (who was Royal Professor at Paris in 1742) admits of neutral salts with excess of acid, neutral salts with excess of base, and perfect neutral salts. Beaume maintained[26] against him that there were no salts except those perfectly neutral, the other classes being the results of mixture and imperfect 25 combination. But this question was not adequately treated till chemists made every experiment with the balance in their hands. When this was done,

they soon discovered that, in each neutral salt, the proportional weights of the ingredients which composed it were always the same. This was ascertained by Wenzel, whose *Doctrine of the Affinities of Bodies* appeared in 1777. He not only ascertained that the proportions of elements in neutral chemical compounds are definite, but also that they are reciprocal; that is, (to express his results in a manner now employed by chemists), that if A, a certain weight of a certain acid, neutralize *m*, a certain weight of a certain base, and B, a certain weight of a certain other acid, neutralize *n*, a certain weight of a certain other base; the compound of A and *n* will also be neutral; as also that of B and *m*. The same views were again presented by Richter in 1792, in his *Principles of the Measure of Chemical Elements*. And along with these facts, that of the combination of elements in multiple proportions being also taken into account, the foundations of the Atomic Theory were laid; and that Theory was propounded in 1803 by Mr. Dalton. That theory, however, rests upon the Idea of Substance, as well as

upon that Idea of Chemical Affinity which we are here considering; and the discussion of its evidence and truth must be for the present deferred.

[26] Dumas, *Phil. Chim.* p. 198.

9. The two principles just explained,—that Affinity is Definite as to the Kind, and as to the Quantity of the elements which it unites,—have here been stated as results of experimental investigation. That they could never have been clearly understood, and therefore never firmly established, without laborious and exact experiments, is certain; but yet we may venture to say that being once fully known, they may seem to thoughtful men to possess an evidence beyond that of mere experiment. For how, in fact, can we conceive combinations, otherwise than as definite in kind and quantity? If we were to suppose each element ready to combine with any other indifferently, and indifferently in any quantity, we should have a world in 26 which all would be confusion and indefiniteness. There would be no fixed

kinds of bodies. Salts, and stones, and ores, would approach to and graduate into each other by insensible degrees. Instead of this, we know that the world consists of bodies distinguishable from each other by definite differences, capable of being classified and named, and of having general propositions asserted concerning them. And as we cannot conceive a world in which this should not be the case, it would appear that we cannot conceive a state of things in which the laws of the combination of elements should not be of that definite and measured kind which we have above asserted.

This will, perhaps, appear more clearly by stating our fundamental convictions respecting chemical composition in another form, which I shall, therefore, proceed to do.

10. *Chemical Composition determines Physical Properties.*—However obscure and incomplete may be our conception of the internal powers by which the ultimate particles of bodies are held together, it

involves, at least, this conviction:—that these powers are what determine bodies to be bodies, and therefore contain the reason of all the properties which, as bodies, they possess. The forces by which the particles of a body are held together, also cause it to be hard or soft, heavy or light, opake or transparent, black or red; for if these forces are not the cause of these peculiarities, what can be the cause? By the very supposition which we make respecting these forces, they include all the relations by which the parts are combined into a whole, and therefore they, and they only, must determine all the attributes of the whole. The foundation of all our speculations respecting the intimate constitution of bodies must be this principle, that their composition determines their properties.

Accordingly we find our chemists reasoning from this principle with great confidence, even in doubtful cases. Thus Davy, in his researches concerning the diamond, says: ‘That some chemical difference must exist between the hardest

and most beautiful of the 27 gems and charcoal, between a non-conductor and a conductor of electricity, it is scarcely possible to doubt: and it seems reasonable to expect that a very refined or perfect chemistry will confirm *the analogies of nature*; and show that bodies cannot be the same in their composition or chemical nature, and yet totally different in their chemical properties.' It is obvious that the principle here assumed is so far from being a mere result of experience, that it is here appealed to to prove that all previous results of experience on this subject must be incomplete and inaccurate; and that there must be some chemical difference between charcoal and diamond, though none had hitherto been detected.

11. In what manner, according to what rule, the chemical composition shall determine the kind of the substance, we cannot reasonably expect to determine by mere conjecture or assumption, without a studious examination of natural bodies and artificial compounds. Yet even in the most

recent times, and among men of science, we find that an assumption of the most arbitrary character has in one case been mixed up with this indisputable principle, that the elementary composition determines the kind of the substance. In the classification of minerals, one school of mineralogists have rightly taken it as their fundamental principle that the chemical composition shall decide the position of the mineral in the system. But they have appended to this principle, arbitrarily and unjustifiably, the maxim that the element which is *largest in quantity* shall fix the class of the substance. To make such an assumption is to renounce, at once, all hope of framing a system which shall be governed by the resemblances of the things classified; for how can we possibly know beforehand that fifty-five per cent, of iron shall give a substance its predominant properties, and that forty-five per cent, shall not? Accordingly, the systems of mineralogical arrangement which have been attempted in this way, (those of Haüy, Phillips, and others,) have been found

inconsistent with themselves, ambiguous, and incapable of leading to any general truths. 28

12. *Chemical Composition and Crystalline Form correspond.*—Thus the physical properties of bodies depend upon their chemical composition, but in a manner which a general examination of bodies with reference to their properties and their composition can alone determine. We may, however, venture to assert further, that the more definite the properties are, the more distinct may we expect to find this dependence. Now the most definite of the properties of bodies are those constant properties which involve relations of space; that is, their figure. We speak not, however, of that external figure, derived from external circumstances, which, so far from being constant and definite, is altogether casual and arbitrary; but of that figure which arises from their internal texture, and which shows itself not only in the regular forms which they spontaneously assume, but in the disposition of the parts to

separate in definite directions, and no others. In short, the most definite of the properties of perfect chemical compounds is their *crystalline structure*; and therefore it is evident that the crystalline structure of each body, and the forms which it affects, must be in a most intimate dependence upon its chemical composition.

Here again we are led to the brink of another theory;—that of crystalline structure, which has excited great interest among philosophers ever since the time of Haüy. But this theory involves, besides that idea of chemical composition with which we are here concerned, other conceptions, which enter into the relations of figure. These conceptions, governed principally by the Idea of Symmetry, must be unfolded and examined before we can venture to discuss any theory of crystallization: and we shall proceed to do this as soon as we have first duly considered the Idea of Substance and its consequences.

CHAPTER III.

Of the Idea of Substance.

1. *Axiom of the Indestructibility of Substance.*—We now come to an Idea of which the history is very different from those of which we have lately been speaking. Instead of being gradually and recently brought into a clear light, as has been the case with the Ideas of Polarity and Affinity, the Idea of Substance has been entertained in a distinct form from the first periods of European speculation. That this is so, is proved by our finding a principle depending upon this Idea current as an axiom among the early philosophers of Greece:—namely, that *nothing can be produced out of nothing*. Such an axiom, more fully stated, amounts to this: that the substance of which a body consists is

incapable of being diminished (and consequently incapable of being augmented) in quantity, whatever apparent changes it may undergo. Its forms, its distribution, its qualities, may vary, but the substance itself is identically the same under all these variations.

The axiom just spoken of was the great principle of the physical philosophy of the Epicurean school, as it must be of every merely material philosophy. The reader of Lucretius will recollect the emphasis with which it is repeatedly asserted in his poem:

> E nilo nil gigni, in nilum nil posse reverti;
>
> Nought comes of nought, nor ought returns to nought.

Those who engaged in these early attempts at physical speculation were naturally much pleased with the clearness which was given to their notions of change, composition, and decomposition, by keeping steadily hold of the Idea of Substance, as marked by this 30 fundamental axiom. Nor

has its authority ever ceased to be acknowledged. A philosopher was asked[27], What is the weight of smoke? He answered, 'Subtract the weight of the ashes from the weight of the wood which is burnt, and you have the weight of the smoke.' This reply would be assented to by all; and it assumes as incontestable that even under the action of fire, the material, the substance, does not perish, but only changes its form.

[27] Kant, *Kritik der R. V.* p. 167.

This principle of the indestructibility of substance might easily be traced in many reasonings and researches, ancient and modern. For instance, when the chemist works with the *retort*, he places the body on which he operates in one part of an inclosed cavity, which, by its bendings and communications, separates at the same time that it confines, the products which result from the action of fire: and he assumes that this process is an analysis of the body into its ingredients, not a creation of anything which did not exist before, or a destruction of anything which previously existed. And

he assumes further, that the total quantity of the substance thus analysed is the sum of the quantities of its ingredients. This principle is the very basis of chemical speculation, as we shall hereafter explain more fully.

2. *The Idea of Substance.*—The axiom above spoken of depends upon the Idea of Substance, which is involved in all our views of external objects. We unavoidably assume that the qualities and properties which we observe are properties of *things*;—that the adjective implies a substantive;—that there is, besides the external characters of things, something *of which* they are the characters. An apple which is red, and round, and hard, is not merely redness, and roundness, and hardness: these circumstances may all alter while the apple remains the same apple. Behind or under the appearances which we see, we conceive something of which we think; or, to use the metaphor which obtained currency among the ancient philosophers, the 31 attributes and qualities which we observe are

supported by and inherent in something: and this something is hence called a *substratum* or *substance*,—that which stands *beneath* the apparent qualities and supports them.

That we have such an *Idea*, using the term 'Idea' in the sense in which I have employed it throughout these disquisitions, is evident from what has been already said. The Axiom of the Indestructibility of Substance proves the existence of the Idea of Substance, just as the Axioms of Geometry and Arithmetic prove the existence of the Ideas of Space and Number. In the case of Substance, as of space or number, the ideas cannot be said to be borrowed from experience, for the axioms have an authority of a far more comprehensive and demonstrative character than any which experience can bestow. The axiom that nothing can be produced from nothing and nothing destroyed, is so far from being a result of experience, that it is apparently contradicted by the most obvious observation. It has, at first, the air of a

paradox; and by those who refer to it, it is familiarly employed to show how fallacious common observation is. The assertion is usually made in this form;—that nothing is created and nothing annihilated, *notwithstanding* that the common course of our experience appears to show the contrary. The principle is not an empirical, but a necessary and universal truth;—is collected, not from the evidence of our senses, but from the operation of our ideas. And thus the universal and undisputed authority of the axiom proves the existence of the Idea of Substance.

3. *Locke's Denial of the Idea of Substance.*—I shall not attempt to review the various opinions which have been promulgated respecting this Idea: but it may be worth our while to notice briefly the part which it played in the great controversy concerning the origin of our ideas which Locke's *Essay* occasioned. Locke's object was to disprove the existence of all ideas not derived from Sensation or Reflection: and since the idea of substance as distinct from external

qualities, is 32 manifestly not derived directly from sensation, nor by any very obvious or distinct process from reflection, Locke was disposed to exclude the idea as much as possible. Accordingly, in his argumentation against Innate Ideas[28], he says plainly, ‘the idea of substance, which we neither have nor can have by sensation or reflection.’ And the inference which he draws is, ‘that we have no such clear idea at all.’ What then, it may be asked, do we mean by the word *substance*? This also he answers, though somewhat strangely, ‘We signify nothing by the word *substance*, but only an uncertain supposition of we know not what, *i. e.* of something whereof we have no particular distinct positive idea, which we take to be the substratum, or support, of those ideas we know.’ That while he indulged in this tautological assertion of our ignorance and uncertainty, he should still have been compelled to acknowledge that the word substance had some meaning, and should have been driven to explain it by the identical metaphors of ‘substratum’ and ‘support,’ is a curious proof

how impossible it is entirely to reject this idea.

[28] *Essay*, b. i. c. iv. s. 18.

But as we have already seen, the supposition of the existence of substance is so far from being uncertain, that it carries with it irresistible conviction, and substance is necessarily conceived as something which cannot be produced or destroyed. It may be easily supposed, therefore, that when the controversy between Locke and his assailants came to this point, he would be in some difficulty. And, indeed, though with his accustomed skill in controversy, he managed to retain a triumphant tone, he was driven from his main points. Thus he repels the charge that he took the being of substance to be doubtful[29]. He says, 'Having everywhere affirmed and built upon it that man is a substance, I cannot be supposed to question or doubt of the being of substance, till I can question or doubt of my own being.' He attempts to make a stand by saying that *being* of things does not depend upon our 33 *ideas*; but if he had been asked

how, without having an *idea* of substance, he *knew* substance to *be*, it is difficult to conceive what answer he could have made. Again, he had said that our idea of substance arises from our 'accustoming ourselves to suppose' a substratum of qualities. Upon this his adversary, Bishop Stillingfleet, very properly asks, Is this custom grounded upon true reason or no? To which Locke replies, that it is grounded upon this: That we cannot conceive how simple ideas of sensible qualities should subsist alone; and therefore we suppose them to exist in, and to be supported by some common subject, which support we denote by the name substance. Thus he allows, not only that we necessarily assume the reality of substance, but that we cannot conceive qualities without substance; which are concessions so ample as almost to include all that any advocate for the Idea of Substance need desire.

[29] *Essay*, b. ii. c. ii. and *First Letter to the Bishop of Worcester*.

Perhaps Locke, and the adherents of

Locke, in denying that we have an idea of substance in general, were latently influenced by finding that they could not, by any effort of mind, call up any *image* which could be considered as an image of substance in general. That in this sense we have no idea of substance, is plain enough; but in the same sense we have no idea of space in general, or of time, or number, or cause, or resemblance. Yet we certainly have such a power of representing to our minds space, time, number, cause, resemblance, as to arrive at numerous truths by means of such representations. These general representations I have all along called Ideas, nor can I discover any more appropriate word; and in this sense, we have also, as has now been shown, an Idea of Substance.

4. *Is all Material Substance heavy?*—The principle that the quantity of the substance of any body remains unchanged by our operations upon it, is, as we have said, of universal validity. But then the question occurs, how are we to ascertain the quantity

of substance, and thus, to apply the principle in particular cases. In the case above mentioned, where 34 smoke was to be weighed, it was manifestly assumed that the quantity of the substance might be known by its weight; and that the total quantity being unchanged, the total weight also would remain the same. Now on what grounds do we make this assumption? Is all material substance heavy? and if we can assert this to be so, on what grounds does the truth of the assertion rest? These are not idle questions of barren curiosity; for in the history of that science (Chemistry) to which the Idea of Substance is principally applicable, nothing less than the fate of a comprehensive and long established theory (the Phlogiston theory) depended upon the decision of this question. When it was urged that the reduction of a metal from a calcined to a metallic form could not consist in the *addition* of phlogiston, because the metal was lighter than the calx had been; it was replied by some, that this was not conclusive, for that phlogiston was a principle of levity, diminishing the weight

of the body to which it was added. This reply was, however, rejected by all the sounder philosophers, and the force of the argument finally acknowledged. But why was this suggestion of a substance having no weight, or having absolute levity, repudiated by the most reflective reasoners? It is assumed, it appears, that all matter must be heavy; what is the ground of this assumption?

The ground of such an assumption appears to be the following. Our idea of substance includes in it this:—that substance is a quantity capable of addition; and thus capable of making up, by composition, a sum equal to all its parts. But substance, and the quantity of substance, can be known to us only by its attributes and qualities. And the qualities which are capable constantly and indefinitely of increase and diminution by increase and diminution of the parts, must be conceived inseparable from the substance. For the qualities, if removable from the substance at all, must be removable by some operation

performed upon the substance; and by the idea of substance, all such operations are only equivalent to separation, junction, and union of parts. Hence those characters 35 which thus universally increase and diminish by addition and subtraction of the things themselves, belong to the substance of the things. They are measures of its quantity, and are not merely its separable qualities.

The weight of bodies is such a character. However we compound or divide bodies, we compound and divide their weight in the same manner. We may dismember a body into the minutest parts; but the sum of the weights of the parts is always equal to the whole weight of the body. The weight of a body can be in no way increased or diminished, except by adding something to it or taking something from it. If we bake a brick, we do not conceive that the change of colour or of hardness, implies that anything has been created or destroyed. It may easily be that the parts have only assumed a new arrangement; but if the brick have lost

weight, we suppose that something (moisture for instance) has been removed elsewhere.

Thus weight is apprehended as essential to matter. In considering the dismemberment or analysis of bodies, we assume that there must be some criterion of the quantity of substance; and this criterion can possess no other properties than their weight possesses. If we assume an element which has no weight, or the weight of which is negative, as some of the defenders of phlogiston attempted to do, we put an end to all speculation on such subjects. For if weight is not the criterion of the quantity of one element, phlogiston for instance, why is weight the criterion of the quantity of any other element? We may, by the same right, assume any other real or imaginary element to have levity instead of gravity; or to have a peculiar intensity of gravity which makes its weight no index of its quantity. In short, if we do this, we deprive of all possibility of application our notions of element, analysis, and composition; and violate the postulates

on which the questions are propounded which we thus attempt to decide.

We must, then, take a constant and quantitative property of matter, such as weight is, to be an index 36 of the quantity of matter or of substance to which it belongs. I do not here speak of the question which has sometimes been proposed, whether the *weight* or the *inertia* of bodies be the more proper measure of the quantity of matter. For the measure of inertia is regulated by the same assumption as that of substance:—that the quantity of the whole must be equal to the quantity of all the parts: and inertia is measured by weight, for the same reason that substance is so.

Having thus established the certainty, and ascertained the interpretation of the fundamental principle which the Idea of Substance involves, we are prepared to consider its application in the science upon which it has a peculiar bearing.

NOTE TO CHAPTER III.

[3rd Ed.]—[THE doctrine here propounded, that All Matter is Heavy, has been opposed by Sir William Hamilton of Edinburgh. (*Works of Reid*, note, p. 853.) This writer is a man of unquestionable acuteness and of very extensive reading; but his acuteness shows itself in barren ontological distinctions, which appear to me to be of the same character as the speculations of the eminent Schoolmen of the most sterile periods of the dark ages. That he should have no conception of progressive or inductive science is not wonderful, when we recollect that he holds, as an important part of his philosophy, that the study of mathematics perverts and obscures the mind. But it may be of some interest to consider his objections to the doctrine here maintained.

He says, 1st, that our reasoning assumes that we must necessarily have it in our power to ascertain the Quantity of Matter; whereas this may be a problem out of the reach of human determination.

To this I reply, that my reasoning *does* assume that there is a science, or sciences, which make assertions concerning the Quantity of Matter: Mechanics and Chemistry are such sciences. My assertion is, that to make such sciences possible, Quantity of Matter must be proportional to Weight. If my opponent deny that Mechanics and Chemistry can exist as sciences, he may invalidate my proof; but not otherwise.

2. He says that there are two conceivable ways of estimating the Quantity of Matter: by the Space occupied, and by the Weight or Inertia; and that I assume the second measure gratuitously.

To which I reply, that the most elementary steps in Mechanics and in Chemistry contradict the notion that 38 the

Quantity of Matter is proportionate to the Space. They proceed necessarily on a distinction between Space and Matter:—between mere Extension and material Substance.

3. He allows that we cannot make the Extension of a body the measure of the Quantity of Matter, because, he says, we do not know if ‘the compressing force’ is such as to produce ‘the closest compression.’ That is, he assumes a compressing force, assumes a closest compression, assumes a peculiar (and very improbable) atomic hypothesis; and all this to supply a reason why we are not to believe the first simple principle of Mechanics and Chemistry.

4. He speaks of ‘a series of apparent fluids (as Light or its vehicle, the Calorific, the Electro-galvanic, and Magnetic agents) which we can neither denude of their character of substance, nor clothe with the attribute of weight.’

To which my reply is, that precisely because I cannot ‘clothe’ these agents with

the attribute of Weight, I *do* 'denude them of the character of Substance.' They are not substances, but agencies. These Imponderable Agents are not properly called 'Imponderable Fluids.' This I conceive that I have proved; and the proof is not shaken by denying the conclusion without showing any defect in the reasoning.

5. Finally, my critic speaks about 'a logical canon,' and about 'a criterion of truth, subjectively necessary and objectively certain;' which matters I shall not waste the reader's time by discussing.]

CHAPTER IV.

Application of the Idea of Substance in Chemistry.

1. *A Body is Equal to the Sum of its Elements.*—From the earliest periods of chemistry the balance has been familiarly used to determine the proportions of the ingredients and of the compound; and soon after the middle of the last century, this practice was so studiously followed, that Wenzel and Richter were thereby led to the doctrine of Definite Proportions. But yet the full value and significance of the balance, as an indispensable instrument in chemical researches, was not understood till the gaseous, as well as solid and fluid ingredients were taken into the account. When this was done, it was found that the principle, that the whole is equal to the sum

of its parts, of which, as we have seen, the necessary truth, in such cases, flows from the idea of substance, could be applied in the most rigorous manner. And conversely, it was found that by the use of the balance, the chemist could decide, in doubtful cases, which was a whole, and which were parts.

For chemistry considers all the changes which belong to her province as compositions and decompositions of elements; but still the question may occur, whether an observed change be the one or the other. How can we distinguish whether the process which we contemplate be composition or decomposition?—whether the new body be formed by addition of a new, or subtraction of an old element? Again; in the case of decomposition, we may inquire, What are the ultimate limits of our analysis? If we decompound bodies into others more and more simple, how far can we carry this succession 40 of processes? How far can we proceed in the road of analysis? And in our actual course, what evidence have we that our progress, as far

as it has gone, has carried us from the more complex to the more simple?

To this we reply, that the criterion which enables us to distinguish, decidedly and finally, whether our process have been a mere analysis of the proposed body into its ingredients, or a synthesis of some of them with some new element, is the principle stated above, that the weight of the whole is equal to the weight of all the parts. And no process of chemical analysis or synthesis can be considered complete till it has been verified by this fact;—by finding that the weight of the compound is the weight of its supposed ingredients; or, that if there be an element which we think we have detached from the whole, its loss is betrayed by a corresponding diminution of weight.

I have already noticed what an important part this principle has played in the great chemical controversy which ended in the establishment of the oxygen theory. The calcination of a metal was decided to be the union of oxygen with the metal, and not the

separation of phlogiston from it, because it was found that in the process of calcination, the weight of the metal increased, and increased exactly as much as the weight of ambient air diminished. When oxygen and hydrogen were exploded together, and a small quantity of water was produced, it was held that this was really a synthesis of water, because, when very great care was taken with the process, the weight of the water which resulted was equal to the weight of the gases which disappeared.

2. *Lavoisier.*—It was when gases came to be considered as entering largely into the composition of liquid and solid bodies, that extreme accuracy in weighing was seen to be so necessary to the true understanding of chemical processes. It was in this manner discovered by Lavoisier and his contemporaries that oxygen constitutes a large ingredient of calcined metals, of acids, and of water. A countryman of Lavoisier[30] [41] has not only given most just praise to that great philosopher for having constantly tested all his processes by a careful and

skilful use of the balance, but has also claimed for him the merit of having introduced the maxim, that in chemical operations nothing is created and nothing lost. But I think it is impossible to deny that this maxim is assumed in all the attempts at analysis made by his contemporaries, as well as by him. This maxim is indeed included in any clear notion of analysis: it could not be the result of the researches of any one chemist, but was the governing principle of the reasonings of all. Lavoisier, however, employed this principle with peculiar assiduity and skill. In applying it, he does not confine himself to mere additions and subtractions of the quantities of ingredients; but often obtains his results by more complex processes. In one of his investigations he says, 'I may consider the ingredients which are brought together, and the result which is obtained as an algebraical equation; and if I successively suppose each of the quantities of this equation to be unknown, I can obtain its value from the rest: and thus I can rectify the experiment by the calculation, and the

calculation by the experiment. I have often taken advantage of this method, in order to correct the first results of my experiments, and to direct me in repeating them with proper precautions.'

[30] M. Dumas, *Leçons de la Philosophie Chimique*. 1837. p. 157.

The maxim, that the whole is equal to the sum of all its parts, is thus capable of most important and varied employment in chemistry. But it may be applied in another form to the exclusion of a class of speculations which are often put forwards.

3. *Maxim respecting Imponderable Elements.* —Several of the phenomena which belong to bodies, as heat, light, electricity, magnetism, have been explained hypothetically by assuming the existence of certain fluids; but these fluids have never been shown to have weight. Hence such hypothetical fluids have been termed *imponderable elements*. It is however plain, that so long as these fluids appear to be without weight, they are not *elements* of

bodies in the same 42 sense as those elements of which we have hitherto been speaking. Indeed we may with good reason doubt whether those phenomena depend upon transferable fluids at all. We have seen strong reason to believe that light is not matter, but only motion; and the same thing appears to be probable with regard to heat. Nor is it at all inconceivable that a similar hypothesis respecting electricity and magnetism should hereafter be found tenable. Now if heat, light, and those other agents, be not matter, they are not *elements* in such a sense as to be included in the principle referred to above, That the body is equal to the sum of its elements. Consequently the maxim just stated, that in chemical operations nothing is created, nothing annihilated, does not apply to Light and Heat. They are not *things*. And whether heat can be produced where there was no heat before, and light struck out from darkness, the ideas of which we are at present treating do not enable us to say. In reasoning respecting chemical synthesis and analysis therefore, we shall only make

confusion by attempting to include in our conception the Light and Heat which are produced and destroyed. Such phenomena may be very proper subjects of study, as indeed they undoubtedly are; but they cannot be studied to advantage by considering them as sharing the nature of composition and decomposition.

Again: in all attempts to explain the processes of nature, the proper course is, first to measure the facts with precision, and then to endeavour to understand their cause. Now the facts of chemical composition and decomposition, the weights of the ingredients and of the compounds, are facts measurable with the utmost precision and certainty. But it is far otherwise with the light and heat which accompany chemical processes. When combustion, deflagration, explosion, takes place, how can we measure the light or the heat? Even in cases of more tranquil action, though we can apply the thermometer, what does the thermometer tell us respecting the *quantity* of the heat? Since then we have no

measure which is of any value as 43 regards such circumstances in chemical changes, if we attempt to account for these phenomena *on chemical principles*, we introduce, into investigations in themselves perfectly precise and mathematically rigorous, another class of reasonings, vague and insecure, of which the only possible effect is to vitiate the whole reasoning, and to make our conclusions inevitably erroneous.

We are led then to this maxim: that *imponderable fluids are* not *to be admitted as chemical elements of bodies*[31].

[31] See the answer to Sir William Hamilton's objections, at the end of the last chapter.

Since we are thus warned by a sound view of the nature of science, from considering chemical affinity as having any hold upon imponderable elements, we are manifestly still more decisively prohibited from supposing mechanical impulse or pressure to have any effect upon such elements. To make this supposition, is to connect the most subtle and incorporeal objects which we know in nature by the most gross material ties. This remark seems to be applicable to M. Poisson's hypothesis that the electric fluid

> is retained at the surface of bodies by the pressure of the atmosphere.

4. It appears, I think, that our best and most philosophical chemists have proceeded upon this principle in their investigations. In reasoning concerning the constitution of bodies and the interpretation of chemical changes, the attempts to include in these interpretations the heat or cold produced, by the addition or subtraction of a certain hypothetical ‘caloric,’ have become more and more rare among men of science. Such statements, and the explanations often put forwards of the light and heat which appear under various circumstances in the form of fire, must be considered as unessential parts of any sound theory. Accordingly we find Mr. Faraday gradually relinquishing such views. In January, 1834, he speaks generally of an hypothesis of this kind[32]: ‘I cannot refrain from recalling here the beautiful idea put forth, I believe by Berzelius, in his development of his views of the electro-chemical theory of affinity, that the heat and light evolved during cases of powerful combination 44 are the

consequence of the electric discharge which is at that moment taking place.' But in April of the same year[33], he observes, that in the combination of oxygen and hydrogen to produce water, electric powers to a most enormous amount are for the time active, but that the flame which is produced gives but feeble traces of such powers. 'Such phenomena,' therefore, he adds, 'may not, cannot, be taken as evidences of the nature of the action; but are merely incidental results, incomparably small in relation to the forces concerned, and supplying no information of the way in which the particles are active on each other, or in which their forces are finally arranged.'

[32] *Researches*, 870.

[33] *Researches*, 960.

In pursuance of this maxim, we must consider as an unessential part of the oxygen theory that portion of it, much insisted upon by its author at the time, in which when sulphur, for instance, combined with oxygen to produce sulphuric acid, the

combustion was accounted for by means of the *caloric* which was supposed to be *liberated* from its combination with oxygen.

5. *Controversy of the Composition of Water.*—There is another controversy of our times to which we may with great propriety apply the maxim now before us. After the glory of having first given a true view of the composition of water had long rested tranquilly upon the names of Cavendish and Lavoisier, a claim was made in favour of James Watt as the real author of this discovery by his son, (Mr J. Watt,) and his eulogist, (M. Arago[34]). It is not to our purpose here to discuss the various questions which have arisen on this subject respecting priority of publication, and respecting the translation of opinions published at one time into the language of another period. But if we look at Watt's own statement of his views, given soon after those of Cavendish had been published, we shall perceive that it is marked by a violation of this maxim: we shall find that he does admit imponderable fluids 45 as

chemical elements; and thus shows a vagueness and confusion in his idea of chemical composition. With such imperfection in his views, it is not surprising that Watt, not only did not anticipate, but did not apprehend quite precisely the discovery of Cavendish and Lavoisier. Watt's statement of his views is as follows[35]:—'Are we not authorized to conclude that water is composed of dephlogisticated air and phlogiston deprived of part of their latent or elementary heat; that dephlogisticated or pure air is composed of water deprived of its phlogiston and united to elementary heat and light; and that the latter are contained in it in a latent state, so as not to be sensible to the thermometer or to the eye; and if light be only a modification of heat, or a circumstance attending it, or a component part of the inflammable air, then pure or dephlogisticated air is composed of water deprived of its phlogiston and united to elementary heat?'

[34] Éloge de James Watt, *Annuaire du Bur. des Long.* 1839.

[35] *Phil. Trans.* 1784, p. 332.

When we compare this doubtful and hypothetical statement, involving so much that is extraneous and heterogeneous, with the conclusion of Cavendish, in which there is nothing hypothetical or superfluous, we may confidently assent to the decision which has been pronounced by one[36] of our own time in favour of Cavendish. And we may with pleasure recognize, in this enlightened umpire, a due appreciation of the value of the maxim on which we are now insisting. 'Cavendish,' says Mr. Vernon Harcourt, 'pared off 46 from the hypotheses their theories of combustion, and *affinities of imponderable for ponderable matter*, as complicating chemical with physical considerations.'

[36] The Rev. W. Vernon Harcourt, Address to the British Association, 1839.—Since the first edition of this work was published, and also since the second edition of the *History of the Inductive Sciences*, Mr. Watt's correspondence bearing upon the question of the Composition of Water has been published by Mr. Muirhead. I do not find, in this publication, any reason for withdrawing

> what I have stated in the text above: but with reference to the statement in the *History*, it appears that Mr. Cavendish's claim to the discovery was not uncontested in his own time. Mr. Watt had looked at the composition of water, as a problem to be solved, perhaps more distinctly than Mr. Cavendish had done; and he conceived himself wronged by Mr. Cavendish's putting forwards his experiment as the first solution of this problem.

6. *Relation of Heat to Chemistry.*—But while we thus condemn the attempts to explain the thermotical phenomena of chemical processes by means of chemical considerations, it may be asked if we are altogether to renounce the hope of understanding such phenomena? It is plain, it may be said, that heat generated in chemical changes is always a very important circumstance, and can sometimes be measured, and perhaps reduced to laws; are we prohibited from speculating concerning the causes of such circumstances and such laws? And to this we reply, that we may properly attempt to connect chemical with thermotical processes, *so far*

as we have obtained a clear and probable view of the nature of the thermotical processes. When our theory of Thermotics is tolerably complete and certain, we may with propriety undertake to connect it with our theory of Chemistry. But at present we are not far enough advanced in our knowledge of heat to make this attempt with any hope of success. We can hardly expect to understand the part which heat plays in the union of two bodies, when we cannot as yet comprehend in what manner it produces the liquefaction or vaporization of one body. We cannot look to account for Gay Lussac and Dalton's Law, that all gases expand equally by heat, till we learn how heat causes a gas to expand. We cannot hope to see the grounds of Dulong and Petit's Law, that the specific heat of all atoms is the same, till we know much more, not only about atoms, but about specific heat. We have as yet no thermotical theory which even professes to account for all the prominent facts of the subject[37]: and the theories which have been proposed are of the most diverse kind. Laplace assumes

particles of bodies surrounded by atmospheres of caloric[38]; Cauchy makes heat consist in longitudinal vibrations of the ether of which transverse vibrations 47 produce light: in Ampère's theory[39], heat consists in the vibrations of the particles of bodies. And so long as we have nothing more certain in our conceptions of heat than the alternative of these and other precarious hypotheses, how can we expect to arrive at any real knowledge, by connecting the results of such hypotheses with the speculations of Chemistry, of which science the theory is at least equally obscure?

[37] *Hist. Ind. Sci.* b. x. c. 4.

[38] *Ib.*

[39] *Hist. Ind. Sci.* b. x. c. 4.

The largest attempts at chemical theory have been made in the form of the Atomic Theory, to which I have just had occasion to allude. I must, therefore, before quitting the subject, say a few words respecting this theory.

CHAPTER V.

The Atomic Theory.

1. *The Atomic Theory considered on Chemical Grounds.*—We have already seen that the combinations which result from chemical affinity are definite, a certain quantity of one ingredient uniting, not with an uncertain, but with a certain quantity of another ingredient. But it was found, in addition to this principle, that one ingredient would often unite with another in different proportions, and that, in such cases, these proportions are multiples one of another. In the three salts formed by potassa with oxalic acid, the quantities of acid which combine with the same quantity of alkali are exactly in the proportion of the numbers 1, 2, 4. And the same rule of the existence of multiple proportions is found to

obtain in other cases.

It is obvious that such results will be accounted for, if we suppose that the base and the acid consist each of numerous definite equal particles, and that the formation of the salts above mentioned consists in the combination of one particle of the base with one particle of acid, with two particles of acid, and with four particles of acid, respectively. But further; as we have already stated, chemical affinity is not only definite, but reciprocal. The proportions of potassa and soda which form neutral salts being 590 and 391 in one case, they are so in all cases. These numbers represent *proportions* of weight in which the two bases, potassa and soda, enter into analogous combinations; 590 of potassa is *equivalent* to 391 of soda. These facts with regard to combination are still expressed by the above supposition of equal particles, assuming that the weights of a 49 particle of potassa and of soda are in the proportion of 590 to 391.

But we pursue our analysis further. We find that potassa is a compound of a metallic base, potassium, and of oxygen, in the proportion of 490 to 100; we suppose, then, that the particle of potassa consists of a particle of potassium and a particle of oxygen; and these latter particles, since we see no present need to suppose them divided, potassium and oxygen being simple bodies, we may call *atoms*, and assume to be indivisible. And by supposing all simple bodies to consist of such atoms, and compounds to be formed by the union of two, or three, or more of such atoms, we explain the occurrence of definite and multiple proportions, and we construct the Atomic Theory.

2. *Hypothesis of Atoms.*—So far as the assumption of such atoms as we have spoken of serves to express those laws of chemical composition which we have referred to, it is a clear and useful generalization. But if the Atomic Theory be put forwards (and its author, Dr. Dalton, appears to have put it forwards with such

an intention,) as asserting that chemical elements are really composed of *atoms*, that is, of such particles not further divisible, we cannot avoid remarking, that for such a conclusion, chemical research has not afforded, nor can afford, any satisfactory evidence whatever. The smallest observable quantities of ingredients, as well as the largest, combine according to the laws of proportions and equivalence which have been cited above. How are we to deduce from such facts any inference with regard to the existence of certain smallest possible particles? The Theory, when dogmatically taught as a physical truth, asserts that all observable quantities of elements *are* composed of proportional numbers of particles which can no further be subdivided; but all which observation teaches us is, that if there be such particles, they are smaller than the smallest observable quantities. In chemical experiment, at least, there is not the slightest positive evidence for the existence of such atoms. The assumption of *indivisible* particles, smaller than the smallest 50

observable, which combine, particle with particle, will explain the phenomena; but the assumption of particles bearing this proportion, but *not* possessing the property of indivisibility, will explain the phenomena at least equally well. The decision of the question, therefore, whether the Atomic Hypothesis be the proper way of conceiving the chemical combinations of substances, must depend, not upon chemical facts, but upon our conception of Substance. In this sense the question is an ancient and curious controversy, and we shall hereafter have to make some remarks upon it.

3. *Chemical Difficulties of the Hypothesis.*— But before doing this, we may observe that there is no small difficulty in reconciling this hypothesis with the facts of chemistry. According to the theory, all salts, compounded of an acid and a base, are analogous in their atomic constitution; and the number of atoms in one such compound being known or assumed, the number of atoms in other salts may be determined. But when we proceed in this course of reasoning

to other bodies, as metals, we find ourselves involved in difficulties. The protoxide of iron is a base which, according to all analogy, must consist of one atom of iron and one of oxygen: but the peroxide of iron is also a base, and it appears by the analysis of this substance that it must consist of *two-thirds* of an atom of iron and one atom of oxygen. Here, then, our indivisible atoms must be divisible, even upon chemical grounds. And if we attempt to evade this difficulty by making the peroxide of iron consist of two atoms of iron and three of oxygen, we have to make a corresponding alteration in the theoretical constitution of all bodies analogous to the protoxide; and thus we overturn the very foundation of the theory. Chemical facts, therefore, not only do not prove the Atomic Theory as a physical truth, but they are not, according to any modification yet devised of the theory, reconcileable with its scheme.

Nearly the same conclusions result from the attempts to employ the Atomic Hypothesis in expressing another important

chemical law;—the law of the 51 combinations of gases according to definite proportions of their volumes, experimentally established by Gay Lussac[40]. In order to account for this law, it has been very plausibly suggested that all gases, under the same pressure, contain an equal number of atoms in the same space; and that when they combine, they unite atom to atom. Thus one volume of chlorine unites with one volume of hydrogen, and forms hydrochloric acid[41]. But then this hydrochloric acid occupies the space of the two volumes; and therefore the proper number of particles cannot be supplied, and the uniform distribution of atoms in all gases maintained, without dividing into two each of the compound particles, constituted of an atom of chlorine and an atom of hydrogen. And thus in this case, also, the Atomic Theory becomes untenable if it be understood to imply the indivisibility of the atoms.

[40] *Hist. Ind. Sc.* b. xiv. c. 8.

[41] Dumas, *Phil. Chim.* 263.

In all these attempts to obtain distinct physical conception of chemical union by the aid of the Atomic Hypothesis, the atoms are conceived to be associated by certain forces of the nature of mechanical attractions. But we have already seen[42] that no such mode of conception can at all explain or express the facts of chemical combination; and therefore it is not wonderful that when the Atomic Theory attempts to give an account of chemical relations by contemplating them under such an aspect, the facts on which it grounds itself should be found not to authorize its positive doctrines; and that when these doctrines are tried upon the general range of chemical observation, they should prove incapable of even expressing, without self-contradiction, the laws of phenomena.

[42] See Chapter I. of this book.

4. *Grounds of the Atomic Doctrine.*—Yet the doctrine of atoms, or of substance as composed of indivisible particles, has in all ages had great hold upon the minds of physical speculators; nor would this

doctrine ever have suggested itself so readily, or have been maintained so tenaciously, as the true mode of 52 conceiving chemical combinations, if it had not been already familiar to the minds of those who endeavour to obtain a general view of the constitution of nature. The grounds of the assumption of the atomic structure of substance are to be found rather in the idea of substance itself, than in the experimental laws of chemical affinity. And the question of the existence of atoms, thus depending upon an idea which has been the subject of contemplation from the very infancy of philosophy, has been discussed in all ages with interest and ingenuity. On this very account it is unlikely that the question, so far as it bears upon chemistry, should admit of any clear and final solution. Still it will be instructive to look back at some of the opinions which have been delivered respecting this doctrine.

5. *Ancient Prevalence of the Atomic Doctrine.*—The doctrine that matter consists of minute, simple, indivisible, indestructible

particles as its ultimate elements, has been current in all ages and countries, whenever the tendency of man to wide and subtle speculations has been active. I need not attempt to trace the history of this opinion in the schools of Greece and Italy. It was the leading feature in the physical tenets of the Epicureans, and was adopted by their Roman disciples, as the poem of Lucretius copiously shows us. The same tenet had been held at still earlier periods, in forms more or less definite, by other philosophers. It is ascribed to Democritus, and is said to have been by him derived from Leucippus. But this doctrine is found also, we are told[43], among the speculations of another intellectual and acute race, the Hindoos. According to some of their philosophical writers, the ultimate elements of matter are atoms, of which it is proved by certain reasonings, that they are each one-sixth of one of the motes that float in the sunbeam.

[43] By Mr. Colebrook. *Asiatic Res.* 1824.

This early prevalence of controversies of the widest and deepest kind, which even in

our day remain undecided, has in it nothing which need surprize us; or, at least, it has in it nothing which is not in conformity 53 with the general course of the history of philosophy. As soon as any ideas are clearly possessed by the human mind, its activity and acuteness in reasoning upon them are such, that the fundamental antitheses and ultimate difficulties which belong to them are soon brought into view. The Greek and Indian philosophers had mastered completely the Idea of Space, and possessed the Idea of Substance in tolerable distinctness. They were, therefore, quite ready, with their lively and subtle minds, to discuss the question of the finite and infinite divisibility of matter, so far as it involved only the ideas of space and of substance, and this accordingly they did with great ingenuity and perseverance.

But the ideas of Space and of Substance are far from being sufficient to enable men to form a complete general view of the constitution of matter. We must add to these ideas, that of mechanical Force with its

antagonist Resistance, and that of the Affinity of one kind of matter for another. Now the former of these ideas the ancients possessed in a very obscure and confused manner; and of the latter they had no apprehension whatever. They made vague assumptions respecting the impact and pressure of atoms on each other; but of their mutual attraction and repulsion they never had any conception, except of the most dim and wavering kind; and of an affinity different from mere local union they did not even dream. Their speculations concerning atoms, therefore, can have no value for us, except as a part of the history of science. If their doctrines appear to us to approach near to the conclusions of our modern philosophy, it must be because our modern philosophy is that philosophy which has not fully profited by the additional light which the experiments and meditations of later times have thrown upon the constitution of matter.

6. *Bacon.*—Still, when modern philosophers look upon the Atomic Theory

of the ancients in a general point of view merely, without considering the special conditions which such a theory must fulfil, in order to represent the discoveries of modern times, they are 54 disposed to regard it with admiration. Accordingly we find Francis Bacon strongly expressing such a feeling. The Atomic Theory is selected and dwelt upon by him as the chain which connects the best parts of the physical philosophy of the ancient and the modern world. Among his works is a remarkable dissertation *On the Philosophy of Democritus, Parmenides, and Telesius*: the last mentioned of whom was one of the revivers of physical science in modern times. In this work he speaks of the atomic doctrine of Democritus as a favourable example of the exertions of the undisciplined intellect. 'Hæc ipsa placita, quamvis paulo emendatiora, talia sunt qualia esse possunt illa quæ ab intellectu sibi permisso, nec continenter et gradatim sublevato, profecta videntur.'—'These doctrines, thus [in an ancient fable] presented in a better form, are such glimpses of truth as can be

obtained by the intellect left to its own natural impulses, and not ascending by successive and connected steps,' [as the Baconian philosophy directs]. 'Accordingly,' he adds, 'the doctrine of Atoms, from its going a step beyond the period in which it was advanced, was ridiculed by the vulgar, and severely handled in the disputations of the learned, notwithstanding the profound acquaintance with physical science by which its author was allowed to be distinguished, and from which he acquired the character of a magician.'

'However,' he continues, 'neither the hostility of Aristotle, with all his skill and vigour in disputation, (though, like the Ottoman sultans, he laboured to destroy all his brother philosophers that he might rest undisputed master of the throne of science,) nor the majestic and lofty authority of Plato, could effect the subversion of the doctrine of Democritus. And while the opinions of Plato and Aristotle were rehearsed with loud declamation and professorial pomp in the schools, this of Democritus was always

held in high honour by those of a deeper wisdom, who followed in silence a severer path of contemplation. In the days of Roman speculation it kept its ground and its favour; Cicero everywhere speaks of its author with the 55 greatest praise; and Juvenal, who, like poets in general, probably expressed the prevailing judgment of his time, proclaims his merit as a noble exception to the general stupidity of his countrymen.

. . . . Cujus prudentia monstrat

Magnos posse viros et magna exempla daturos

Vervecum in patriâ crassoque sub aere nasci.

‘The destruction of this philosophy was not effected by Aristotle and Plato, but by Genseric and Attila, and their barbarians. For then, when human knowledge had suffered shipwreck, those fragments of the Aristotelian and Platonic philosophy floated on the surface like things of some lighter and emptier sort, and so were preserved; while more solid matters went to the

bottom, and were almost lost in oblivion.'

7. *Modern Prevalence of the Atomic Doctrine.*—It is our business here to consider the doctrine of Atoms only in its bearing upon existing physical sciences, and I must therefore abstain from tracing the various manifestations of it in the schemes of hypothetical cosmologists;—its place among the *vortices* of Descartes, its exhibition in the *monads* of Leibnitz. I will, however, quote a passage from Newton to show the hold it had upon his mind.

At the close of his *Opticks* he says, 'All these things being considered, it seems probable to me that God, in the beginning, formed matter in solid, massy, hard, impenetrable, moveable particles, of such sizes and figures, and with such other properties, and in such proportions to space, as most conduced to the end for which He formed them; and that the primitive particles, being solids, are incomparably harder than any porous bodies compounded of them, even so very hard as never to wear

or break in pieces; no ordinary power being able to divide what God had made one in the first creation. While the particles continue entire, they may compose bodies of one and the same nature and texture in all ages: but should they wear away or break in pieces, the nature of things depending on them would be changed. Water and earth composed 56 of old worn particles and fragments of particles would not be of the same nature and texture now with water and earth composed of entire particles in the beginning. And therefore that nature may be lasting, the changes of corporeal things are to be placed only in the various separations and new associations and motions of these permanent particles; compounded bodies being apt to break, not in the midst of solid particles, but where those particles are laid together and only touch in a few points.'

We shall hereafter see how extensively the atomic doctrine has prevailed among still more recent philosophers. Not only have the chemists assumed it as the fittest

form for exhibiting the principles of multiple proportions; but the physical mathematicians, as Laplace and Poisson, have made it the basis of their theories of heat, electricity, capillary action; and the crystallographers have been supposed to have established both the existence and the arrangement of such ultimate molecules.

In the way in which it has been employed by such writers, the hypothesis of ultimate particles has been of great use, and is undoubtedly permissible. But when we would assert this theory, not as a convenient hypothesis for the expression or calculation of the laws of nature, but as a philosophical truth respecting the constitution of the universe, we find ourselves checked by difficulties of reasoning which we cannot overcome, as well as by conflicting phenomena which we cannot reconcile. I will attempt to state briefly the opposing arguments on this question.

8. *Arguments for and against Atoms.*—The

leading arguments on the two sides of the question, in their most general form, may be stated as follows:

For the Atomic Doctrine.—The appearances which nature presents are compounded of many parts, but if we go on resolving the larger parts into smaller, and so on successively, we must at last come to something simple. For that which is compound can be so no otherwise than by composition of what is simple; and if we suppose all composition to be removed, which 57 hypothetically we may do, there can remain nothing but a number of simple substances, capable of composition, but themselves not compounded. That is, matter being dissolved, resolves itself into atoms.

Against the Atomic Doctrine.—Space is divisible without limit, as may be proved by Geometry; and matter occupies space, therefore matter is divisible without limit, and no portion of matter is indivisible, or an *atom*.

And to the argument on the other side

just stated, it is replied that we cannot even hypothetically divest a body of composition, if by composition we mean the relation of point to point in space. However small be a particle, it is compounded of parts having relation in space.

The Atomists urge again, that if matter be infinitely divisible, a finite body consists of an infinite number of parts, which is a contradiction. To this it is replied, that the finite body consists of an infinite number of parts in the same sense in which the parts are infinitely small, which is no contradiction.

But the opponents of the Atomists not only rebut, but retort this argument drawn from the notion of infinity. Your atoms, they say, are indivisible by any finite force; therefore they are infinitely hard; and thus your finite particles possess infinite properties. To this the Atomists are wont to reply, that they do not mean the hardness of their particles to be infinite, but only so great as to resist all usual natural forces. But

here it is plain that their position becomes untenable; for, in the first place, their assumption of this precise degree of hardness in the particles is altogether gratuitous; and in the next place, if it were granted, such particles are not atoms, since in the next moment the forces of nature may be augmented so as to divide the particle, though hitherto undivided.

Such are the arguments for and against the Atomic Theory in its original form. But when these atoms are conceived, as they have been by Newton, and commonly by his followers, to be solid, hard particles exerting attractive and repulsive forces, a new set of 58 arguments come into play. Of these, the principal one may be thus stated: According to the Atomic Theory thus modified, the properties of bodies depend upon the attractions and repulsions of the particles. Therefore, among other properties of bodies, their hardness depends upon such forces. But if the hardness *of the bodies* depends upon the forces, the repulsion, for instance, of the particles, upon what does

the hardness *of the particles* depend? what progress do we make in explaining the properties of bodies, when we assume the same properties in our explanation? and to what purpose do we assume that the particles are hard?

9. *Transition to Boscovich's Theory*.—To this difficulty it does not appear easy to offer any reply. But if the hardness and solidity of the particles be given up as an incongruous and untenable appendage to the Newtonian view of the Atomic Theory, we are led to the theory of Boscovich, according to which matter consists not of solid particles, but of mere mathematical centers of force. According to this theory, each body is composed of a number of geometrical points from which emanate forces, following certain mathematical laws in virtue of which the forces become, at certain small distances attractive, at certain other distances repulsive, and at greater distances attractive again. From these forces of the points arise the cohesion of the parts of the same body, the resistance which it

exerts against the pressure of another body, and finally the attraction of gravitation which it exerts upon bodies at a distance.

This theory is at least a homogeneous and consistent theory, and it is probable that it may be used as an instrument for investigating and expressing true laws of nature; although, as we have already said, the attempt to identify the forces by which the particles of bodies are bound together with mechanical attraction, appears to be a confusion of two separate ideas[44].

[44] 'Boscovich's Theory,' that all bodies may be considered as consisting of a mere collection of centers of forces, may be so conceived as possibly to involve an explanation of all the powers which their parts exert, (such powers, namely, as those which produce optical, thermotical and chemical phenomena;) but this theory cannot supply an explanation of the mechanical properties of a body as a whole, especially of its *inertia*. A collection of mere centers of force can have no inertia. If two bodies are considered as two collections of centers of force, the one attracting the other, there is in this view nothing to limit or determine the velocity with which the one body will approach the other. A world composed of such

> bodies is not a *material* world: for matter (as we have already seen in book iii. chapter v.) implies not only force, but something which resists the action of force.

59 10. *Use of the Molecular Hypothesis.*—In this form, representing matter as a collection of molecules or centers of force, the Atomic Theory has been abundantly employed in modern times as an hypothesis on which calculations respecting the elementary forces of bodies might be conducted. When thus employed it is to be considered as expressing the principle that the properties of bodies depend upon forces emanating from immovable points of their mass. This view of the way in which the properties of bodies are to be treated by the mechanical philosopher was introduced by Newton, and was a natural sequel to the success which he had obtained by reasoning concerning central forces on a large scale. I have already quoted his Preface to the *Principia*, in which he says, 'Many things induce me to believe that the rest of the phenomena of nature, as well as those of

astronomy, may depend upon certain forces by which the particles of bodies, in virtue of causes not yet known, are urged towards each other and cohere in regular figures, or are mutually repelled and recede; and philosophers, knowing nothing of these forces, have hitherto failed in their examination of nature.' Since the time of Newton, this line of speculation has been followed with great assiduity, and by some mathematicians with great success. In particular Laplace has shown that the hypothesis may, in many instances, be made a much closer representation of nature, if we suppose the forces exerted by the particles to decrease so rapidly with the increasing distance from them, that 60 the force is finite only at distances imperceptible to our senses, and vanishes at all remoter points. He has taught the method of expressing and calculating such forces, and he and other mathematicians of his school have applied this method to many of the most important questions of physics; as capillary action, the elasticity of solids, the conduction and radiation of heat.

The explanation of many apparently unconnected and curious observed facts by these mathematical theories gives a strong assurance that its essential principles are true. But it must be observed that the actual constitution of bodies as composed of distinct and separate particles is by no means proved by these coincidences. The assumption, in the reasoning, of certain centers of force acting at a distance, is to be considered as nothing more than a method of reducing to calculation that view of the constitution of bodies which supposes that they exert force at *every* point. It is a mathematical artifice of the same kind as the hypothetical division of a body into infinitesimal parts, in order to find its center of gravity; and no more implies a physical reality than that hypothesis does.

11. *Poisson's Inference.*—When, therefore, M. Poisson, in his views of Capillary Action, treats this hypothetical distribution of centers of force as if it were a physical fact, and blames Laplace for not taking account of their different distribution at the surface

of the fluid and below it[45], he appears to push the claims of the molecular hypothesis too far. The only ground for the assumption of separate centers, is that we can thus explain the action of the whole mass. The intervals between the centers nowhere enter into this explanation: and therefore we can have no reason for assuming these intervals different in one part of the fluid and in the other. M. Poisson asserts that the density of the fluid diminishes when we approach very near the surface; but he allows that this diminution is not detected by experiment, and that the formulæ on 61 his supposition, so far as the results go, are identical with those of Laplace. It is clear, then, that his doctrine consists merely in the assertion of the necessary truth of a part of the hypothesis which cannot be put to the test of experiment. It is true, that so long as we have before us the hypothesis of separate centers, the particles very near the surface are not in a condition symmetrical with that of the others: but it is also true that this hypothesis is only a step of calculation. There results, at one period of the process of

deduction, a stratum of smaller density at the surface of the fluid; but at a succeeding point of the reasoning the thickness of this stratum vanishes; it has no physical existence.

[45] Poisson, *Théorie de l'Action Capillaire*.

Thus the *molecular* hypothesis, as used in such cases, does not differ from the doctrine of forces acting at *every point* of the mass; and this principle, which is common to both the opposite views, is the true part of each.

12. *Wollaston's Argument.*—An attempt has been made in another case, but depending on nearly the same arguments, to bring the doctrine of ultimate atoms to the test of observation. In the case of the air, we know that there *is* a diminution of density in approaching the upper surface of the atmosphere, if it have a surface: but it is held by some that except we allow the doctrine of ultimate molecules, it will not be bounded by any surface, but will extend to an infinite distance. This is the reasoning of Wollaston[46]. 'If air consists of any ultimate

particles no longer divisible, then must the expansion of the medium composed of them cease at that distance where the force of gravity downwards is equal to the resistance arising from the repulsive force of the medium.' But if there be no such ultimate particles, every stratum will require a stratum beyond it to prevent by its weight a further expansion, and thus the atmosphere 62 must extend to an infinite distance. And Wollaston conceived that he could learn from observation whether the atmosphere was thus diffused through all space; for if so, it must, he argued, be accumulated about the larger bodies of the system, as Jupiter and the Sun, by the law of universal gravitation; and the existence of an atmosphere about these bodies, might, he remarked, be detected by its effects in producing refraction. His result is, that 'all the phenomena accord entirely with the supposition that the earth's atmosphere is of finite extent, limited by the weight of ultimate atoms of definite magnitude, no longer divisible by repulsion of their parts.'

[46] *Phil. Trans.* 1822, p. 89.

A very little reflection will show us that such a line of reasoning cannot lead to any result. For we know nothing of the law which connects the density with the compressing force, in air so extremely rare as we must suppose it to be near the boundary of the atmosphere. Now there are possible laws of dependence of the density upon the compressing force such that the atmosphere would terminate in virtue of the law without any assumption of atoms. This may be proved by mathematical reasoning. If we suppose the density of air to be as the square root of the compressing force, it will follow that at the very limits of the atmosphere, the strata of equal thickness may observe in their densities such a law of proportion as is expressed by the numbers 7, 5, 3, 1[47].

[47] For the compressing force on each being as the whole weight beyond it, it will be for the four highest strata, 16, 9, 4 and 1, of which the square roots are as 4, 3, 2, 1, or, as 8, 6, 4, 2; and though these numbers are not exactly as the densities 7, 5, 3, 1, those who are a little acquainted with

> mathematical reasoning, will see that the difference arises from taking so small a number of strata. If we were to make the strata indefinitely thin, as to avoid error we ought to do, the coincidence would be exact; and thus, according to this law, the series of strata terminates as we ascend, without any consideration of atoms.

If it be asked how, on this hypothesis, the density of the highest stratum can be as 1, since there is 63 nothing to compress it, we answer that the upper part of the highest stratum compresses the lower, and that the density diminishes continually to the surface, so that the need of compression and the compressing weight vanish together.

The fallacy of concluding that because the height of the atmosphere is finite, the weight of the highest stratum must be finite, is just the same as the fallacy of those who conclude that when we project a body vertically upwards, because it occupies only a finite time in ascending to the highest point, the velocity at the last instant of the ascent must be finite. For it might be said, if the last velocity of ascent be not finite, how can the body describe the last particle of

space in a finite time? and the answer is, that there is no last finite particle of space, and therefore no last finite velocity.

13. *Permanence of Properties of Bodies.*— We have already seen that, in explaining the properties of matter as we find them in nature, the assumption of solid, hard, indestructible particles is of no use or value. But we may remark, before quitting the subject, that Newton appears to have had another reason for assuming such particles, and one well worthy of notice. He wished to express, by means of this hypothesis, the doctrine that the laws of nature do not alter with the course of time. This we have already seen in the quotation from Newton. 'The ultimate particles of matter are indestructible, unalterable, impenetrable; for if they could break or wear, the structure of material bodies now would be different from that which it was when the particles were new.' No philosopher will deny the truth which is thus conveyed by the assertion of atoms; but it is obviously equally easy for a person who rejects the

atomic view, to state this truth by saying that the forces which matter exerts do not vary with time, but however modified by the new modifications of its form, are always unimpaired in quantity, and capable of being restored to their former mode of action. 64

We now proceed to speculations in which the fundamental conceptions may, perhaps, be expressed, at least in some cases, by means of the arrangement of atoms; but in which the philosophy of the subject appears to require a reference to a new Fundamental Idea.

BOOK VII.

THE

PHILOSOPHY

OF

MORPHOLOGY,

INCLUDING

CRYSTALLOGRAPHY.

CRYSTALLIZATION exhibits to us the effects of the natural arrangement of the ultimate particles of various compound bodies; but we are scarcely yet sufficiently acquainted with chemical synthesis and analysis to understand the rationale of this process. The rhomboidal

form may arise from the proper position of 4, 6, 8 or 9 globular particles, the cubic form from 8 particles, the triangular form from 3, 6 or 10 particles, the hexahedral prism from 7 particles, &c. Perhaps, in due time we may be enabled to ascertain the number and order of elementary particles, constituting any given compound element, and from that determine the figure which it will prefer on crystallization, and *vice versâ*.

JOHN DALTON, *Chemical Philosophy* (1808), p. 210.

BOOK VII.

THE PHILOSOPHY OF MORPHOLOGY, INCLUDING CRYSTALLOGRAPHY.

CHAPTER I.

Explication of the Idea of Symmetry.

1. WE have seen in the History of the Sciences, that the principle which I have there termed[1] the Principle of Developed and Metamorphosed Symmetry, has been extensively applied in botany and physiology, and has given rise to a province of science termed Morphology. In order to understand clearly this principle, it is necessary to obtain a clear idea of the Symmetry of which we thus speak. But this Idea of Symmetry is applicable in the inorganic, as well as in the organic

kingdoms of nature; it is presented to our eyes in the forms of minerals, as well as of flowers and animals; we must, therefore, take it under our consideration here, in order that we may complete our view of Mineralogy, which, as I have repeatedly said, is an essential part of Chemical science. I shall accordingly endeavour to unfold the Idea of Symmetry with which we here have to do.

[1] *Hist. Ind. Sc.* b. xvii. c. vi.

It will of course be understood that by the term *Symmetry* I here intend, not that more indefinite attribute of form which belongs to the domain of the fine arts, as when we speak of the 'symmetry' of an edifice 68 or of a sculptured figure, but a certain definite relation or property, no less rigorous and precise than other relations of number and position, which is thus one of the sure guides of the scientific faculty, and one of the bases of our exact science.

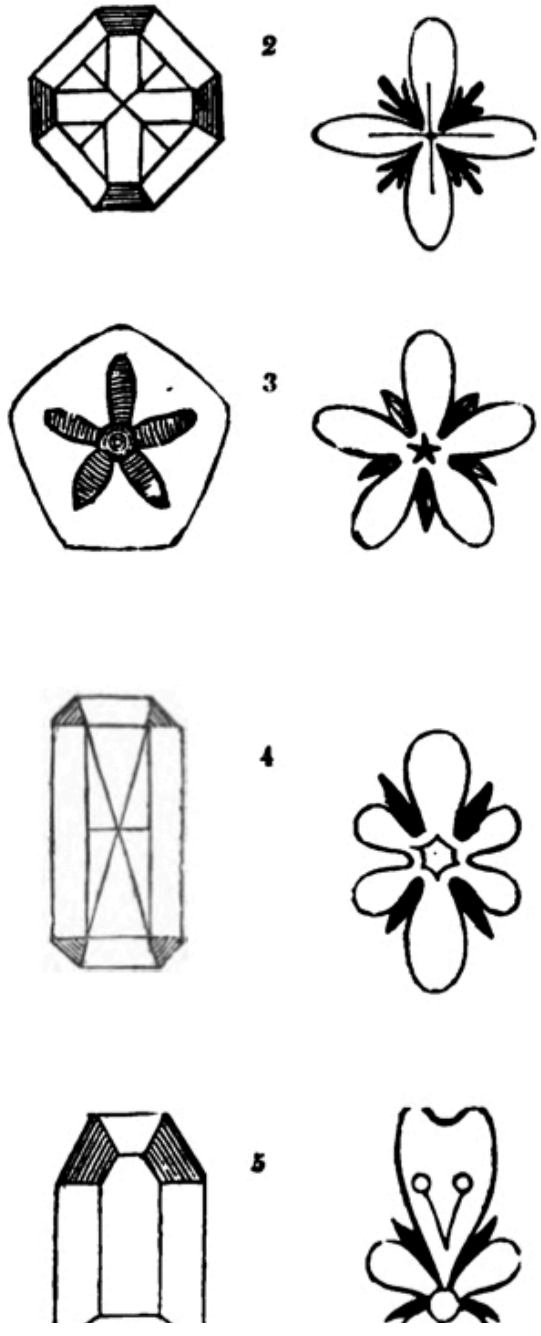
2. In order to explain what Symmetry is in this sense, let the reader recollect that the

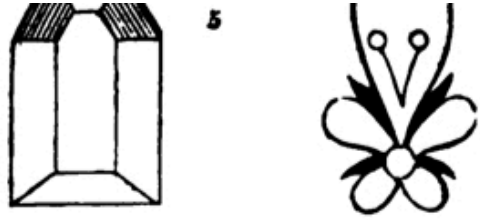
bodies of animals consist of *two* equal and similar sets of members, the right and the left side;—that some flowers consist of three or of five equal sets of organs, similarly and regularly disposed, as the iris has *three* straight petals, and three reflexed ones, alternately disposed, the rose has *five* equal and similar sepals of the calyx, and alternate with these, as many petals of the corolla. This orderly and exactly similar distribution of two, or three, or five, or any other number of parts, is Symmetry; and according to its various modifications, the forms thus determined are said to be *symmetrical* with various numbers of members. The classification of these different kinds of symmetry has been most attended to in Crystallography, in which science it is the highest and most general principle by which the classes of forms are governed. Without entering far into the technicalities of the subject, we may point out some of the features of such classes.



The first of the figures (1) in the margin may represent the summit of a crystal as it appears to an eye looking directly down upon it; the center of the figure represents the summit of a pyramid, and the spaces of various forms which diverge from this point represent sloping sides of the pyramid. Now it will be observed that the figure consists of three portions exactly similar to one another, and that each part or member is repeated in each of these portions. The faces, or pairs of faces, are repeated in *threes*, with exactly similar forms and angles. This figure is said to be *three-membered*, or to have *triangular* symmetry. The same kind of 69 symmetry may exist in a flower, as presented in the accompanying figure, and does, in fact, occur in a large class of flowers, as for example, all the lily

tribe.


2
3
4
5



The next pair of figures (2) have four equal and similar portions, and have their members or pairs of members four times repeated. Such figures are termed *four-membered*, and are said to have *square* or *tetragonal* symmetry. The *pentagonal* symmetry, formed by *five* similar *members*, is represented in the next figures (3). It occurs abundantly in the vegetable world, but never among crystals; for the pentagonal figures which crystals sometimes assume, are never exactly regular. But there is still another kind of symmetry (4) in which the opposite ends are exactly similar to each other and also the opposite sides; this is *oblong*, or *two-and-two-membered* symmetry. And finally, we have the case of *simple* symmetry (5) in which the two sides of the object are exactly alike (in opposite positions) without any further repetition.

3. These different kinds of symmetry

occur in various ways in the animal, vegetable, and mineral kingdom. Vertebrate animals have a right and a 70 left side exactly alike, and thus possess *simple* symmetry. The same kind of symmetry (simple symmetry) occurs very largely in the forms of vegetables, as in most leaves, in *papilionaceous*, *personate*, and *labiate* flowers. Among minerals, crystals which possess this symmetry are called *oblique-prismatic*, and are of very frequent occurrence. The *oblong*, or *two-and-two-membered* symmetry belongs to *right-prismatic* crystals; and may be seen in *cruciferous* flowers, for though these are cross-shaped, the cross has two longer and two shorter arms, or pairs of arms. The *square* or *tetragonal* symmetry occurs in crystals abundantly; to the vegetable world it appears to be less congenial; for though there are flowers with four exactly similar and regularly-disposed petals, as the herb Paris (*Paris quadrifolia*), these flowers appear, from various circumstances, to be deviations from the usual type of vegetable forms. The *trigonal*, or *three-membered* symmetry is found abundantly both in

plants and in crystals, while the *pentagonal* symmetry, on the other hand, though by far the most common among flowers, nowhere occurs in minerals, and does not appear to be a possible form of crystals. This pentagonal form further occurs in the animal kingdom, which the oblong, triangular, and square forms do not. Many of Cuvier's *radiate* animals appear in this pentagonal form, as *echini* and *pentacrinites*, which latter have hence their name.

4. The regular, or as they may be called, the *normal* types of the vegetable world appear to be the forms which possess triangular and pentagonal symmetry; from these the others may be conceived to be derived, by transformations resulting from the expansion of one or more parts. Thus it is manifest that if in a three-membered or five-membered flower, one of the petals be expanded more than the other, it is immediately reduced from pentagonal or trigonal, to simple symmetry. And the oblong or two-and-two-membered symmetry of the flowers of cruciferous

plants, (in which the stamens are four large and two small ones, arranged in regular opposition,) is held by botanists to result 71 from a normal form with ten stamens; Meinecke explaining this by adhesion, and Sprengel by the metamorphosis of the stamens into petals[2].

[2] Sprengel, *Gesch. d. Bot.* ii. 304.

It is easy to see that these various kinds of symmetry include relations both of form and of number, but more especially of the latter kind; and as this symmetry is often an important character in various classes of natural objects, such classes have often curious numerical properties. One of the most remarkable and extensive of these is the distinction which prevails between monocotyledonous and dicotyledonous plants; the number *three* being the ground of the symmetry of the former, and the number *five*, of the latter. Thus liliaceous and bulbous plants, and the like, have flowers of three or six petals, and the other organs follow the same numbers: while the vast majority of plants are pentandrous, and

with their five stamens have also their other parts in fives. This great numerical distinction corresponding to a leading difference of physiological structure cannot but be considered as a highly curious fact in phytology. Such properties of numbers, thus connected in an incomprehensible manner with fundamental and extensive laws of nature, give to numbers an appearance of mysterious importance and efficacy. We learn from history how strongly the study of such properties, as they are exhibited by the phenomena of the heavens, took possession of the mind of Kepler; perhaps it was this which, at an earlier period, contributed in no small degree to the numerical mysticism of the Pythagoreans in antiquity, and of the Arabians and others in the middle ages. In crystallography, numbers are the primary characters in which the properties of substances are expressed;—they appear, first, in that classification of forms which depends on the degree of symmetry, that is, upon the number of correspondencies; and next, in the laws of derivation, which, for the most part, appear to be common in their

occurrence in proportion to the numerical simplicity of their expression. But the manifestation 72 of a governing numerical relation in the organic world strikes us as more unexpected; and the selection of the number *five* as the index of the symmetry of dicotyledonous plants and radiated animals, (a number which is nowhere symmetrically produced in inorganic bodies,) makes this a new and remarkable illustration of the constancy of numerical relations. We may observe, however, that the moment one of these radiate animals has one of its five members expanded, or in any way peculiarly modified, (as happens among the echini), it is reduced to the common type of animals simply symmetrical, with a right and left side.

5. It is not necessary to attempt to enumerate all the kinds of Symmetry, since our object is only to explain what Symmetry is, and for this purpose enough has probably been said already. It will be seen, as soon as the notion of Symmetry in general is well apprehended, that it is or includes a

peculiar Fundamental Idea, not capable of being resolved into any of the ideas hitherto examined. It may be said, perhaps, that the Idea of Symmetry is a modification or derivative of our ideas of space and number; —that a symmetrical shape is one which consists of parts exactly similar, repeated a certain number of times, and placed so as to correspond with each other. But on further reflection it will be seen that this repetition and correspondence of parts in symmetrical figures are something peculiar; for it is not *any* repetition or any correspondence of parts to which we should give the name of symmetry, in the manner in which we are now using the term. Symmetrical arrangements may, no doubt, be concerned with space and position, time and number; but there appears to be implied in them a Fundamental Idea of regularity, of completeness, of complex simplicity, which is not a mere modification of other ideas.

6. It is, however, not necessary, in this and in similar cases, to determine whether the idea which we have before us be a

peculiar and independent Fundamental Idea or a modification of other ideas, provided we clearly perceive the evidence of those Axioms by 73 means of which the Idea is applied in scientific reasonings. Now in the application of the Idea of Symmetry to crystallography, phytology and zoology, we must have this idea embodied in some principle which asserts more than a mere geometrical or numerical accordance of members. We must have it involved in some vital or productive action, in order that it may connect and explain the facts of the organic world. Nor is it difficult to enunciate such a principle. We may state it in this manner. *All the symmetrical members of a natural product are, under like circumstances, alike affected by the natural formative power.* The parts which we have termed *symmetrical*, resemble each other, not only in their form and position, but also in the manner in which they are produced and modified by natural causes. And this principle we assume to be necessarily true, however unknown and inconceivable may be the causes which determine the

phenomena. Thus it has not yet been found possible to discover or represent to ourselves, in any intelligible manner, the forces by which the various faces of a crystal are consequent upon its primary form: for the hypothesis of their being built up of integrant molecules, as Haüy held, cannot be made satisfactory. But though the mechanism of crystals is still obscure, there is no doubt as to the principle which regulates their modifications. The whole of crystallography rests upon this principle, that if one of the primary planes or axes be modified in any manner, all the symmetrical planes and axes must be modified in the same manner. And though accidental mechanical or other causes may interfere with the actual exhibition of such faces, we do not the less assume their crystallographical reality, as inevitably implied in the law of symmetry of the crystal[3]. And we apply similar considerations to organized beings. We assume that in a regular flower, each of the similar 74 members has the same organization and similar powers of

developement; and hence if among these similar parts some are much less developed than others, we consider them as *abortive*; and if we wish to remove doubts as to what are symmetrical members in such a case, we make the inquiry by tracing the anatomy of these members, or by following them in their earlier states of developement, or in cases where their capabilities are magnified by monstrosity or otherwise. The power of developement may be modified by external causes, and thus we may pass from one kind of symmetry to another; as we have already remarked. Thus a regular flower with pentagonal symmetry, growing on a lateral branch, has one petal nearest to the axis of the plant: if this petal be more or less expanded than the others, the pentagonal symmetry is interfered with, and the flower may change to a symmetry of another kind. But it is easy to see that all such conceptions of expansion, abortion, and any other kind of metamorphosis, go upon the supposition of identical faculties and tendencies in each similar member, in so far as such tendencies have any relation to the symmetry. And

thus the principle we have stated above is the basis of that which, in the History, we termed the Principle of Developed and Metamorphosed Symmetry.

[3] Some crystalline forms, instead of being *holohedral* (provided with their whole number of faces), are *hemihedral* (provided with only half their number of faces). But in these hemihedral forms the half of the faces are still *symmetrically* suppressed.

We shall not at present pursue the other applications of this Idea of Symmetry, but we shall consider some of the results of its introduction into Crystallography.

CHAPTER II.

Application of the Idea of Symmetry to Crystals.

1. MINERALS and other bodies of definite chemical composition often exhibit that marked regularity of form and structure which we designate by terming them *Crystals*; and in such crystals, when we duly study them, we perceive the various kinds of symmetry of which we have spoken in the previous chapter. And the different kinds of symmetry which we have there described are now usually distinguished from each other, by writers on crystallography. Indeed it is mainly to such writers that we are indebted for a sound and consistent classification of the kinds and degrees of symmetry of which forms are capable. But this classification was by no

means invented as soon as mineralogists applied themselves to the study of crystals. These first attempts to arrange crystalline forms were very imperfect; those, for example, of Linnæus, Werner, Romé de Lisle, and Haüy. The essays of these writers implied a classification at once defective and superfluous. They reduced all crystals to one or other of certain *fundamental forms*; and this procedure might have been a perfectly good method of dividing crystalline forms into classes, if the fundamental forms had been selected so as to exemplify the different kinds of symmetry. But this was not the case. Haüy's fundamental or 'primitive' forms, were, for instance, the following: the *parallelepiped*, the *octahedron*, the *tetrahedron*, the *regular hexagonal prism*, the *rhombic dodecahedron*, and the *double hexagonal pyramid*. Of these, the *octahedron*, the *tetrahedron*, the *rhombic dodecahedron*, all belong to the 76 same kind of symmetry (the TESSULAR systems); also the *hexagonal prism* and the *hexagonal pyramid* both belong to the RHOMBIC system; while the *parallelepiped* is so

employed as to include all kinds of symmetry.

It is, however, to be recollected that Haüy, in his selection of primitive forms, not only had an eye to the external form of the crystal and to its degree and kind of regularity, but also made his classification with an especial reference to the *cleavage* of the mineral, which he considered as a primary element in crystalline analysis. There can be no doubt that the cleavage of a crystal is one of its most important characters: it is a relation of form belonging to the interior, which is to be attended to no less than the form of the exterior. But still, the cleavage is to be regarded only as determining the degree of geometrical symmetry of the body, and not as defining a special geometrical figure to which the body *must* be referred. To have looked upon it in the latter light, was a mistake of the earlier crystallographic speculators, on which we shall shortly have to remark.

2. I have said that the reference of

crystals to Primitive Forms might have been well employed as a mode of expressing a just classification of them. This follows as a consequence from the application of the Principle stated in the last chapter, that *all symmetrical members are alike affected*. Thus we may take an upright triangular prism as the representative of the rhombic system, and if we then suppose one of the upper edges to be cut off, or truncated, we must, by the Principle of Symmetry, suppose the other two upper edges to be truncated in precisely the same manner. By this truncation we may obtain the upper part of a rhombohedron; and by truncations of the same kind, symmetrically affecting all the analogous parts of the figure, we may obtain any other form possessing three-membered symmetry. And the same is true of any of the other kinds of symmetry, provided we make a proper selection of a fundamental form. And this was really the method employed by Demeste, Werner, and Romé de Lisle. They 77 assumed a Primitive Form, and then conceived other forms, such as they found in nature, to be derived from

the Primitive Form by truncation of the edges, acumination of the corners, and the like processes. This mode of conception was a perfectly just and legitimate expression of the general Idea of Symmetry.

3. The true view of the degrees of symmetry was, as I have already said, impeded by the attempts which Haüy and others made to arrive at primitive forms by the light which cleavage was supposed to throw upon the structure of minerals. At last, however, in Germany, as I have narrated in the History of Mineralogy[4], Weiss and Mohs introduced a classification of forms implying a more philosophical principle, dividing the forms into Systems; which, employing the terms of the latter writer, we shall call the *tessular*, the *pyramidal* or *square pyramidal*, the *prismatic* or *oblong*, and the *rhombohedral* systems.

[4] *Hist. Ind. Sc.* b. xv. c. iv.

Of these forms, the three latter may be at once referred to those kinds of symmetry of which we have spoken in the last chapter.

The *rhombohedral* system has *triangular* symmetry, or is three-membered: the *pyramidal* has *square* symmetry, or is four-membered: the *prismatic* has *oblong* symmetry, and is two-and-two-membered. But the kinds of symmetry which were spoken of in the former chapter, do not exhaust the idea when applied to minerals. For the symmetry which was there explained was such only as can be exhibited on a surface, whereas the forms of crystals are solid. Not only have the right and left parts of the upper surface of a crystal relations to each other; but the upper surface and the lateral faces of the crystal have also their relations; they may be different, or they may be alike. If we take a cube, and hold it so that four of its faces are vertical, not only are all these four sides exactly similar, so as to give square symmetry; but also we may turn the cube, so that any one of these four sides shall become the top, and still the four sides which are thus made vertical, though 78 not the same which were vertical before, are still perfectly symmetrical. Thus this cubical

figure possesses more than square symmetry. It possesses square symmetry in a vertical as well as in a horizontal sense. It possesses a symmetry which has the same relation to a *cube* which four-membered symmetry has to a *square*. And this kind of symmetry is termed the *cubical* or *tessular* symmetry. All the other kinds of symmetry have reference to an axis, about which the corresponding parts are disposed; but in tessular symmetry the horizontal and vertical axes are also symmetrical, or interchangeable; and thus the figure may be said to have no axis at all.

4. It has already been repeatedly stated that, by the very idea of symmetry, all the incidents of form must affect alike all the corresponding parts. Now in crystals we have, among these incidents, not only external figure, but *cleavage*, which may be considered as internal figure. Cleavage, then, must conform to the degree of symmetry of the figure. Accordingly cleavage, no less than form, is to be attended to in determining to what system a

mineral belongs. If a crystal were to occur as a square prism or pyramid, it would not on that account necessarily belong to the square pyramidal system. If it were found that it was cleavable parallel to one side of the prism, but not in the transverse direction, it has only oblong symmetry; and the equality of the sides which makes it square is only accidental.

Thus no cleavage is admissible in any system of crystallization which does not agree with the degree of symmetry of the system. On the other hand, *any* cleavage which *is* consistent with the symmetry of the system, is (hypothetically at least) allowable. Thus in the oblong prismatic system we may have a cleavage parallel to one side only of the prism; or parallel to both, but of different distinctness; or parallel to the two diagonals of the prism but of the same distinctness; or we may have both these cleavages together. In the rhombohedral system, the cleavage may be parallel to the sides of the rhombohedron, as in Calc 79 Spar: or, in the same system,

the cleavage, instead of being thus oblique to the axis, may be along the axis in those directions which make equal angles with each other: this cleavage easily gives either a triangular or a hexagonal prism. Again, in the tessular system, the cleavage may be parallel to the surface of the cube, which is thus readily separable into other cubes, as in Galena; or the cleavage may be such as to cut off the solid angle of the cube, and since there are eight of these, such cleavage gives us an octahedron, which, however, may be reduced to a tetrahedron, by rejecting all parallel faces, as being mere repetitions of the same cleavage; this is the case with Fluor Spar: or the cube of the tessular system may be cleavable in planes which truncate all the edges of the cube; and as these are twelve, we thus obtain the dodecahedron with rhombic faces: this occurs in Zinc Blende. And thus we see the origin of Haüy's various primitive forms, the tetrahedron, octahedron, and rhombic dodecahedron, all belonging to the tessular system:—they are, in fact, different cleavage forms of that system.

5. I do not dwell upon other incidents of crystals which have reference to form, nor upon the lustre, smoothness, and striation of the surfaces. To all such incidents the general principle applies, that similar parts are similarly affected; and hence, if any parts are found to be constantly and definitely different from other parts of the same sort, they are not similar parts; and the symmetry is to be interpreted with reference to this difference.

We have now to consider the inferences which have been drawn from these incidents of crystallization, with regard to the intimate structure of bodies.

CHAPTER III.

Speculations founded upon the Symmetry of Crystals.

1. WHEN a crystal, as, for instance, a crystal of Galena, (sulphuret of lead,) is readily divisible into smaller cubes, and these into smaller ones, and so on without limit, it is very natural to represent to ourselves the original cube as really consisting of small cubical elements; and to imagine that it is a philosophical account of the physical structure of such a substance to say that it is made up of cubical molecules. And when the Galena crystal has externally the form of a cube, there is no difficulty in such a conception; for the surface of the crystal is also conceived as made up of the surfaces of its cubical molecules. We conceive the crystal so constituted, as we

conceive a wall built of bricks.

But if, as often happens, the Galena crystal be an octahedron, a further consideration is requisite in order to understand its structure, pursuing still the same hypothesis. The mineral is still, as in the other case, readily cleavable into small cubes, having their corners turned to the faces of the octahedron. Therefore these faces can no longer be conceived as made up of the faces of cubical elements of which the whole is constituted. If we suppose a pile of such small cubes to be closely built together, but with decreasing width above, so as to form a pyramid, the face of such a pyramid will no longer be plane; it will consist of a great number of the corners or edges of the small elementary cubes. It would appear at first sight, therefore, that such a face cannot represent the smooth polished surface of a crystal. 81

But when we come to look more closely, this difficulty disappears. For how large are these elementary cubes? We cannot tell,

even supposing they really have any size. But we know that they must be, at any rate, very small; so small as to be inappreciable by our senses, for our senses find no limit to the divisibility of minerals by cleavage. Hence the surface of the pyramid above described would not consist of visible corners or edges, but would be roughened by specks of imperceptible size; or rather, by supposing these specks to become still smaller, the roughness becomes smoothness. And thus we may have a crystal with a smooth surface, made up of small cubes in such a manner that their surfaces are all oblique to the surface of the crystal.

Haüy, struck by some instances in which the supposition of such a structure of crystals appeared to account happily for several of their relations and properties, adopted and propounded it as a general theory. The small elements, of which he supposed crystals to be thus built up, he termed *integrant molecules*. The form of these molecules might or might not be the same as the *primitive form* with which his

construction was supposed to begin; but there was, at any rate, a close connexion between these forms, since both of them were founded on the cleavage of the mineral. The tenet that crystals are constituted in the manner which I have been describing, I shall call the *Theory of Integrant Molecules*, and I have now to make some remarks on the grounds of this theory.

2. In the case of which I have spoken, the mineral used as the example, Galena, readily splits into cubes, and cubes are easily placed together so as to fit each other, and fill the space which they occupy. The same is the case in the mineral which suggested to Haüy his theory, namely, Calc Spar. The crystals of this substance are readily divisible into rhombohedrons, a form like a brick with oblique angles; and such bricks can be built together so as to produce crystals of all the immense varieties of form which Calc Spar presents. This kind of masonry is equally possible in many other 82 minerals; but as we go through the mineral kingdom in our survey, we soon

find cases which offer difficulties. Some minerals cleave only in two directions, some in one only; in such cases we cannot by cleavage obtain an integrant molecule of definite form; one of its dimensions, at least, must remain indeterminate and arbitrary. Again, in some instances, we have more than three different planes of cleavage, as in Fluor Spar, where we have four. The solid, bounded by four planes, is a tetrahedron; or if we take four *pairs* of parallel faces, an octahedron. But if we attempt to take either of these forms for our integrant molecule, we are met by this difficulty: that a collection of such forms will not fill space. Perhaps this difficulty will be more readily conceived by the general reader if it be contemplated with reference to plane figures. It will readily be seen that a number of equal squares may be put together so as to fill the space which they occupy; but if we take a number of equal regular octagons, we may easily convince ourselves that no possible arrangement can make them cover a flat space without leaving blank spots between. In like manner octahedrons or

tetrahedrons cannot be arranged in solid space so as to fill it. They necessarily leave vacancies. Hence the structure of Fluor Spar, and similar crystals, was a serious obstacle in the way of the theory of integrant molecules. That theory had been adopted in the first instance because portions of the crystal, obtained by cleavage, could be built up into a solid mass; but this ground of the theory failed altogether in such instances as I have described, and hence the theory, even upon the representations of its adherents, had no longer any claim to assent.

The doctrine of Integral Molecules, however, was by no means given up at once, even in such instances. In this and in other subjects, we may observe that a theory, once constructed and carried into detail, has such a hold upon the minds of those who have been in the habit of applying it, that they will attempt to uphold it by introducing suppositions inconsistent with 83 the original foundations of the theory. Thus those who assert the Atomic

Theory, reconcile it with facts by taking the *halves of atoms*; and thus the Theory of Integrant Molecules was maintained for Fluor Spar, by representing the elementary octahedrons of which crystals are built up, as touching each other only by the *edges*. The contact of surface with surface amongst integrant molecules had been the first basis of the theory; but this supposition being here inapplicable, was replaced by one which made the theory no longer a representation of the facts (the cleavages), but a mere geometrical construction. Although, however, the inapplicability of the theory to such cases was thus, in some degree, disguised to the disciples of Haüy, it was plain that, in the face of such difficulties, the Theory of Integrant Molecules could not hold its place as a philosophical truth. But it still answered the purpose (a very valuable one, and one to which crystallography is much indebted,) of an instrument for calculating the geometrical relations of the parts of crystals to each other: for the integrant molecules were supposed to be placed layer above

layer, each layer as we ascend, *decreasing* by a certain number of molecules and rows of molecules; and the calculation of these *laws of decrement* was, in fact, the best mode then known of determining the positions of the faces. The Theory of Decrements served to express and to determine, in a great number of the most obvious cases, *the laws of phenomena* in crystalline forms, though the Theory of Integrant Molecules could not be maintained as a just view of the structure of crystals.

3. The Theory of Integrant Molecules, however, involved this just and important principle: that a true view of the intimate structure of crystals must include and explain the facts of crystallization, that is, crystalline form and cleavage; and that it must take these into account, according to their degree of *Symmetry*. So far all theories concerning the elements of crystals must agree. And it was soon seen that this was, in reality, all that had been established by the investigations of Haüy and his school. I have already, in the 84 *History*, quoted

Weiss's reflections on making this step. 'When in 1809,' he says[5], 'I published my Dissertation, I shared the common opinion as to the necessity of the assumption, and the reality of the existence of a primitive form, at least in a sense not very different from the usual sense of the expression.' He then proceeds to relate that he sought a ground for such an opinion, independent of the doctrine of *Atoms*, which he, in common with a great number of philosophers of that time in his own country, was disposed to reject, inclining to believe that the properties of bodies were determined by *Forces* which acted in them, and not by *Molecules* of which they were composed. He adds, that in pursuing this train of thought, he found, 'that out of his Primitive Forms there was gradually unfolded to his hands that which really governs them, and is not affected by their casual fluctuations; namely, the Fundamental Relations of their Dimensions,' or as we now may call them, *Axes of Symmetry*. With reference to these Axes, he found, as he goes on to say, that 'a multiplicity of internal Oppositions,

necessarily and mutually interdependent, are developed in the crystalline mass, each Relation having its own Polarity; so that the Crystalline Character is co-extensive with these Polarities.' The character of these polarities, whether manifested in crystalline faces, cleavage, or any other incidents of crystallization, is necessarily displayed in the degree and kind of Symmetry which the crystal possesses: and thus this Symmetry, in all our speculations concerning the structure of crystals, necessarily takes the place of that enumeration of Primitive Forms which were rejected as inconsistent with observed facts, and destitute of sound scientific principle.

[5] *Acad. Berlin.* 1816, p. 307.

I may just notice here what I have stated in the History of Mineralogy[6], that the distinction of systems of crystallization, as introduced by Weiss and Mohs, was strikingly confirmed by Sir David Brewster's discoveries respecting the optical properties of minerals. 85 The splendid phenomena which were produced by passing polarized

light through crystals, were found to vary according as the crystals were of the Rhombohedral, Square Pyramidal, Oblong Prismatic, or Tessular System. The Optical Symmetry exactly corresponded with the Geometrical Symmetry. In the two former Systems were crystals *uniaxal* in respect of their optical properties; the oblong prismatic, was *biaxal*; while in the tessular, the want of a predominant axis prevented the phenomena here spoken of from occurring at all. The optical experiments must have led, and would have led, to a classification of crystals into the above systems or something nearly equivalent, even had they not been already so arranged by attention to their forms.

[6] *Hist. Ind. Sc.* b. xv. c. v.

4. While in Germany Weiss and Mohs with their disciples, were gradually rejecting what was superfluous in the previous crystallographical hypotheses, philosophers in England were also trying to represent to themselves the constitution of crystals in a manner which should be free

from the obviously arbitrary and untenable fictions of the Haüyian school. These attempts, however, were not crowned with much success. One mode of representing the structure of crystals which suggested itself, was to reject the polyhedral forms which Haüy gave to his integrant molecules, and to conceive the elements of crystals as *spheres*, the properties of the crystal being determined not by the *surfaces*, but by the *position* of the elements. This was done by Wollaston, in the *Philosophical Transactions* for 1813. He applied this view to the tessular system, in which, indeed, the application is not difficult; and he showed that octahedral and tetrahedral figures may be deduced from symmetrical arrangements of equal spherules. But though in doing this, he manifested a perception of the conditions of the problem, he appeared to lose his hold on the real question when he tried to pass on to other systems of crystallization. For he accounted for the rhombohedral system by supposing the spheres changed into *spheroids*. Such a procedure involved him in a gratuitous and useless hypothesis: for to

what purpose do we introduce the 86 arrangement of atoms (instead of their figure,) as a mode of explaining the symmetry of the crystallization, when at the next step we ascribe to the atom, by an arbitrary fiction, a symmetry of figure of the same kind as that which we have to explain? It is just as easy, and as allowable, to assume an elementary rhombohedron, as to assume elementary spheroids, of which the rhombohedrons are constructed.

5. Many hypotheses of the same kind might be adduced, devised both by mineralogists and chemists. But almost all such speculations have been pursued with a most surprising neglect of the principle which obviously is the only sound basis on which they can proceed. The principle is this:—that *All hypotheses concerning the arrangement of the elementary atoms of bodies in space must be constructed with reference to the general facts of crystallization*. The truth and importance of this principle can admit of no doubt. For if we make any hypothesis concerning the mode of connexion of the

elementary particles of bodies, this must be done with the view of representing to ourselves the forces which connect them, and the results of these forces as manifested in the properties of the bodies. Now the forces which connect the particles of bodies so as to make them crystalline, are manifestly chemical forces. It is only definite chemical compounds which crystallize; and in crystals the force of cohesion by which the particles are held together cannot in any way be distinguished or separated from the chemical force by which their elements are combined. The elements are understood to be combined, precisely because the result is a definite, apparently homogeneous substance. The properties of the compound bodies depend upon the elements and their mode of combination; for, in fact, these include everything on which they can depend. There are no other circumstances than these which can affect the properties of a body. Therefore all those properties which have reference to space, namely, the crystalline properties, cannot depend upon anything

else than the arrangement of the elementary molecules in space. These 87 properties are the facts which any hypothesis of the arrangement of molecules must explain, or at least render conceivable; and all such hypotheses, all constructions of bodies by supposed arrangements of molecules, can have no other philosophical object than to account for facts of this kind. If they do not do this, they are mere arbitrary geometrical fictions, which cannot be in any degree confirmed or authorised by an examination of nature, and are therefore not deserving of any regard.

6. Those philosophers who have endeavoured to represent the mode in which bodies are constructed by the combination of their chemical atoms, have often undertaken to show, not only that the atoms are combined, but also in what positions and configurations they are combined. And it is truly remarkable, as I have already said, that they have done this, almost in every instance, without any consideration of the crystalline character of

the resulting combinations; from which alone we receive any light as to the relation of their elements in space. Thus Dr. Dalton, in his *Elements of Chemistry*, in which he gave to the world the Atomic Theory as a representation of the doctrine of definite and multiple proportions, also published a large collection of Diagrams, exhibiting what he conceived to be the configuration of the atoms in a great number of the most common combinations of chemical elements. Now these hypothetical diagrams do not in any way correspond, as to the nature of their symmetry, with the compounds, as we find them displaying their symmetry when they occur crystallized. Carbonate of lime has in reality a triangular symmetry, since it belongs to the rhombohedral system; Dr. Dalton's carbonate of lime would be an oblique rhombic prism or pyramid. Sulphate of baryta is really two-and-two membered; Dr. Dalton's diagram makes it two-and-one membered. Alum is really octahedral or tessular; but according to the diagram it could not be so, since the two ends of the

atom are not symmetrical. And the same want of correspondence between the facts and the hypothesis runs through the whole 88 system. It need not surprise us that the theoretical arrangement of atoms does not explain the facts of *crystallization*; for to produce such an explanation would be a second step in science quite as great as the first, the discovery of the atomic theory in its *chemical* sense. But we may allow ourselves to be surprised that an utter discrepance between all the facts of crystallization and the figures assumed in the theory, did not suggest any doubt as to the soundness of the mode of philosophizing by which this part of the theory was constructed.

7. Some little accordance between the hypothetical arrangements of chemical atoms and the facts of crystallization, does appear to have been arrived at by some of the theorists to whom we here refer, although by no means enough to show a due conviction of the importance of the principle stated above. Thus Wollaston, in

the Essay above noticed, after showing that a symmetrical arrangement of equal spherules would give rise to octahedral and other tessular figures, remarks, very properly, that the metals, which are simple bodies, crystallize in such forms. M. Ampère[7] also, in 1814, published a brief account of an hypothesis of a somewhat similar nature, and stated himself to have developed this speculation in a Memoir which has not yet, so far as I am aware, been published. In this notice he conceives bodies to be compounded of *molecules*, which, arranged in a polyhedral form, constitute *particles*. These *representative forms* of the particles depend on chemical laws. Thus the particles of oxygen, of hydrogen, and of azote, are composed each of four molecules. Hence it is collected that the particles of nitrous gas are composed of two molecules of oxygen and two of azote; and similar conclusions are drawn respecting other substances. These conclusions, though expressed by means of the polyhedrons thus introduced, are supported by chemical, rather than by

crystallographical comparisons. The author does, indeed, appeal to the crystallization of sal 89 ammoniac as an argument[8]; but as *all* the forms which he introduces appear to belong to the *tessular* system of crystallization, there is, in his reasonings, nothing distinctive; and therefore nothing, crystallographically speaking, of any weight on the side of this theory.

[7] *Ann. de Chimie*, tom. xc. p. 43.

[8] *Ann. de Chimie*, tom. xc. p. 83.

8. Any hypothesis which should introduce any principle of chemical order among the actual forms of minerals, would well deserve attention. At first sight, nothing can appear more anomalous than the forms which occur. We have, indeed, one broad fact, which has an encouraging aspect, the tessular forms in which the pure metals crystallize. The highest degree of chemical and of geometrical simplicity coincide: irregularity disappears precisely where it is excluded by the consideration above stated, that the symmetry of chemical composition

must determine the symmetry of crystalline form[9].

[9] Inasmuch as this law, that the simple metals crystallize in tessular forms, is the most signal example of that connexion between the chemical nature of a body and its crystalline form, I in the former Edition stated it with as much generality as I could find any ground for, and I should have been glad if I could have added confirmation of the law, derived from later observations. But the most recent investigations of crystallographers appear to have afforded exceptions rather than examples of the rule. Arsenic and Tellurium are said to be *rhombohedral*. Antimony, stated by Haüy to be octahedral (and therefore tessular), has been found by more modern observers to be *rhombohedral*. Tin has been obtained by Professor Miller in beautiful crystals belonging to the *pyramidal* system. Professor Nöggerath has observed in Zinc, after cooling from fusion, hexagonal cleavage, rendering it probable that the mineral crystallized in *rhombohedrons* having their axes vertical, like ice. G. Rose conceives it highly probable that Osmium and Iridium are *rhombohedral*. (Poggendorf. Bd. liv.)

But all the more perfect metals are tessular; namely, Gold, Silver, Mercury, Platinum, Iron, Copper; also Bismuth [?] Perhaps the observation in which the crystallization of Zinc is affected by its

> position is, on that very account, no sufficient evidence of its free crystallization. We can hardly conceive a collection of perfectly simple, similar particles to crystallize so as to have one pre-eminent axis, without some extraneous action affecting them.

But if we go on to any other class of crystalline forms, we soon find ourselves lost in our attempts to 90 follow any thread of order. We have indeed many large groups connected by obvious analogies; as the rhombohedral carbonates of lime, magnesia, iron, manganese;—the prismatic carbonates and sulphates of lime, baryta, strontia, lead. But even in these, we cannot form any plausible hypothesis of the arrangement of the elements; and in other cases to which we naturally turn, we can find nothing but confusion. For instance, if we examine the oxides of metals:—those of iron are rhombohedral and tessular; those of copper, tessular; those of tin, of titanium, of manganese, square pyramidal; those of antimony, prismatic; and we have other forms for other substances.

It may be added, that if we take account of the optical properties which, as we have already stated, have constant relations to the crystalline forms, the confusion is still further increased; for the optical dimensions vary in amount, though not in symmetry, where chemistry can trace no difference of composition.

9. We will not quit the subject, however, without noticing the much more promising aspect which it has assumed by the detection of such groups as are referred to in the last article; or in other words, by Mitscherlich's discovery of *Isomorphism*. According to that discovery, there are various elements which may take the place of each other in crystalline bodies, either without any alteration of the crystalline form, or at most with only a slight alteration of its dimensions. Such a group of elements we have in the earths lime and magnesia, the protoxides of iron and manganese: for the carbonates of all these bases occur crystallized in forms of the rhombohedral system, the characteristic

angle being nearly the same in all. Now lime and magnesia, by the discoveries of modern chemistry, are really oxides of metals; and therefore all these carbonates have a similar chemical constitution, while they have also a similar crystalline form. Whether or no we can devise any arrangement of molecules by which this connexion of the chemical and the geometrical property can be represented, we cannot help 91 considering the connexion as an extremely important fact in the constitution of bodies; and such facts are more likely than any other to give us some intelligible view of the relations of the ultimate parts of bodies. The same may be said of all the other isomorphous or plesiomorphous groups[10]. For instance, we have a number of minerals which belong to the same system of crystallization, but in which the chemical composition appears at first sight to be very various: namely, spinelle, pleonaste, gahnite, franklinite, chromic iron oxide, magnetic iron oxide: but Abich has shown that all these may be reduced to a common chemical formula;—

they are bioxides of one set of bases, combined with trioxides of another set. Perhaps some mathematician may be able to devise some geometrical arrangement of such a group of elements which may possess the properties of the tessular system. Hypothetical arrangements of atoms, thus expressing both the chemical and the crystalline symmetry which we know to belong to the substance, would be valuable steps in analytical science; and when they had been duly verified, the hypotheses might easily be divested of their atomic character.

[10] See *Hist. Ind. Sc.* b. xv. c. vi.

Thus, as we have already said, mineralogy, understood in its wider sense, as the counterpart of chemistry, has for one of its main objects to discover those Relations of the Elements of bodies which have reference to Space. In this research, the foundation of all sound speculation is the kind and degree of Symmetry of form which we find in definite chemical compounds: and the problem at present

before the inquirer is, to devise such arrangements of molecules as shall answer the conditions alike of Chemistry and of Crystallography.

We now proceed to the Classificatory Sciences, of which Mineralogy is one, though hitherto by far the least successful.

BOOK VIII.

THE

PHILOSOPHY

OF THE

CLASSIFICATORY SCIENCES.

> WHERE a certain apparent difference between things (although perhaps in itself of little moment) answers to we know not what number of other differences, pervading not only their known properties but properties yet undiscovered, it is not optional but imperative to recognise this difference as the foundation of a specific distinction.
>
> JOHN S. MILL, *System of Logic*, b. 1, ch. vii. § 4.

BOOK VIII.

THE PHILOSOPHY OF THE CLASSIFICATORY SCIENCES.

CHAPTER I.

The Idea of Likeness as Governing the Use of Common Names.

1. *Object of the Chapter.*—Not only the Classificatory Sciences, but the application of names to things in the rudest and most unscientific manner, depends upon our apprehending them as *like* each other. We must therefore endeavour to trace the influence and operation of the Idea of Likeness in the common use of language,

before we speak of the conditions under which it acquires its utmost exactness and efficacy.

It will be my object to show in this, as in previous cases, that the impressions of sense are apprehended by acts of the mind; and that these mental acts necessarily imply certain relations which may be made the subjects of speculative reasoning. We shall have, if we can, to seize and bring into clear view the principles which the relation of *like* and *unlike* involves, and the mode in which these principles have been developed.

2. *Unity of the Individual.*—But before we can attend to several things as like or unlike, we must be able to apprehend each of these by itself as *one thing*. 96 It may at first sight perhaps appear that this apprehension results immediately from the impressions on our senses, without any act of our thoughts. A very little attention, however, enables us to see that thus to single out special objects requires a mental operation as well as a sensation. How, for

example, without an exertion of mental activity, can we see one tree, in a forest where there are many? We have, spread before us, a collection of colours and forms, green and brown, dark and light, irregular and straight: this is all that sensation gives or can give. But we associate one brown trunk with one portion of the green mass, excluding the rest, although the neighbouring leaves are both nearer in contiguity and more similar in appearance than is the stem. We thus have before us one tree; but this unity is given by the mind itself. We see the green and the brown, but we must *make* the tree before we can see *it*.

That this composition of our sensations so as to form *one thing* implies an act of our own, will perhaps be more readily allowed, if we once more turn our attention to the manner in which we sometimes attempt to imitate and record the objects of sight, by drawing. When we do this, as we have already observed, we mark this unity of each object, by drawing a line to separate the parts which we include from those

which we exclude;—an *Outline*. This line corresponds to nothing which we see; the beginner in drawing has great difficulty in discerning it; he has in fact to make it. It is, as has been said by a painter of our own time[1], a fiction: but it is a fiction employed to mark a real act of the mind; to designate the singleness of the object in our conception. As we have said elsewhere, we see lines, but especially outlines, by mentally drawing them ourselves.

[1] Phillips *On Painting*,—Design.

The same act of conception which the outline thus represents and commemorates in visible objects,—the same combination of sensible impressions into a unit,—is exercised also with regard to the objects of all 97 our senses: and the singleness thus given to each object, is a necessary preliminary to its being named or represented in any other way.

But it may be said, Is it then by an arbitrary act of our own that we put together the branches of the same tree, or

the limbs of the same animal? Have we equally the power and the right to make the branch of the fir a part of the neighbouring oak? Can we include in the outline of a man any object with which he happens to be in contact?

Such suppositions are manifestly absurd. And the answer is, that though we give unity to objects by an act of thought, it is not by an *arbitrary* act; but by a process subject to certain conditions;—to conditions which exclude such incongruous combinations as have just been spoken of.

What are these conditions which regulate our apprehension of an object as one?—which determine what portion of our impressions does, and what portion does not belong to the same thing?

3. *Condition of Unity.*—I reply, that the primary and fundamental condition is, that we must be able to make intelligible assertions respecting the object, and to entertain that belief of which assertions are the exposition. A tree *grows*, *sheds* its leaves

in autumn, and *buds* again in the spring, *waves* in the wind, or *falls* before the storm. And to the tree belong all those parts which must be included in order that such declarations, and the thought which they convey, shall have a coherent and permanent meaning. Those are *its* branches which wave and fall with *its* trunk; those are *its* leaves which grow on *its* branches. The permanent connexions which we observe,—permanent, among unconnected changes which affect the surrounding appearances,—are what we bind together as belonging to one object. This permanence is the condition of our conceiving the object *as* one. The connected changes may always be described by means of assertions; and the connexion is seen in the identity of the subject of successive predications; in the possibility of applying many verbs to one substantive. We may 98 therefore express the condition of the unity of an object to be this: that *assertions concerning the object shall be possible*: or rather we should say, that the acts of belief which such assertions enunciate shall be possible.

It may seem to be superfluous to put in a form so abstract and remote, the grounds of a process apparently so simple as our conceiving an object to be one. But the same condition to which we have thus been led, as the essential principle of the unity of objects, namely, that propositions shall be possible, will repeatedly occur in the present chapter; and it may serve to illustrate our views, to show that this condition pervades even the simplest cases.

4. *Kinds.*—The mental synthesis of which we have thus spoken, gives us our knowledge of *individual* things; it enables me to apprehend that particular tree or man which I now see, or, by the help of memory, the tree or the man I saw yesterday. But the knowledge with which we have mainly here to do is not a knowledge of individuals but of kinds; of such classes as are indicated by common names. We have to make assertions concerning a tree or a man in general, without regarding what is peculiar to this man or that tree.

Now it is clear that certain individual objects are all called *man*, or all called *tree*, in virtue of some resemblance which they have. If we had not the power of perceiving in the appearances around us, likeness and unlikeness, we could not consider objects as distributed into kinds at all. The impressions of sense would throng upon us, but being uncompared with each other, they would flow away like the waves of the sea, and each vanish from our contemplation when the sensation faded. That we do apprehend surrounding objects as belonging to permanent kinds, as being men and horses, oaks and roses, arises from our having the idea of likeness, and from our applying it habitually, and so far as such a classification requires.

Not only can we employ the idea of likeness in this manner, but we apply it incessantly and universally to 99 the whole mass and train of our sensations. For we have no external sensations to which we cannot apply some language or other; and all language necessarily implies recognition

of resemblances. We cannot call an object *green* or *round* without comparing in our thoughts its colour or its shape, with a shape and a colour seen in other objects. All our sensations, therefore, without any exception of kind or time, are subject to this constant process of classification; and the idea of likeness is perpetually operating to distribute them into kinds, at least so far as the use of language requires.

We come then again to the question, Upon what principle, under what conditions, is the Idea of Likeness thus operative? What are the limits of the classes thus formed? Where does that similarity end, which induces and entitles us to call a thing a *tree*? What universal rule is there for the application of common names, so that we may not apply them wrongly?

5. *Not made by Definitions.*—Perhaps some one might expect in answer to these inquiries a definition or a series of definitions;—might imagine that some description of a tree might be given which

might show when the term was applicable and when it was not; and that we might construct a body of rules to which such descriptions must conform. But on consideration it will be clear that the real solution of our difficulty cannot be obtained in such a manner. For *first*; such descriptions must be given in words, and must therefore suppose that we have already satisfied ourselves how words are to be used. If we define a tree to be 'a living thing without the power of voluntary motion,' we shall be called upon to define 'a living thing;' and it is manifest that this renewal of the demand for definition might be repeated indefinitely; and, therefore, we cannot in this way come to a final principle. And in the *next* place, most of those who use language, even with great precision and consistency, would find it difficult or impossible to give good definitions even of a few of the general names which they use; and therefore their practice cannot be regulated by any 100 tacit reference to such definitions. That definitions of terms are of great use and importance in their right

place, we shall soon see; but their place is not to regulate the use of common language.

What then, once more, is this regulative principle? What rules do men follow in the use of words, so as commonly to avoid confusion and ambiguity? How do they come to understand each other so well as they ordinarily do, respecting the limits of classes never defined, and which they cannot define? What is the common Convention, or Condition to which they conform?

6. *Condition of the Use of Terms.*—To this we reply, that the Condition which regulates the use of language, is, that it shall be capable of being used;—that is, that general assertions shall be possible. The term *tree* is applicable as far as it is useful in expressing our knowledge concerning trees:—thus we know that trees are fixed in the ground, have a solid stem, branches, leaves, and many other properties. With regard to all the objects which surround us, we have an

immense store of knowledge of such properties, and we employ the names of the objects in such a manner as enables us to express these properties.

But the connexion of such properties is variable and indefinite. Some properties are constantly combined, others occasionally only. The leaves of different oaks resemble each other, the branches resemble far less, and may differ very widely. The term *oak* does not enable us to say that all oaks have straight branches or all crooked. Terms can only express properties as far as they are constant. Not only, therefore, the accumulation of a vast mass of knowledge of the properties and attributes of objects, but also an observation of the habitual *connexion* of such properties is needed, to direct us to the consistent application of terms:—to enable us to apply them so as to express truths. But here again we are largely provided with the requisite knowledge and observation by the common course of our existence. The unintermitting stream of experience supplies us with an incalculable

101 amount of such observed connexions. All men have observed that the associations of the same form of leaves are more constant than of the same form of branches; —that though persons walk in different attitudes, none go on all fours; and thus the term *oak* is so applied as to include those cases in which the leaves are alike in form though the branches be unlike; and though we should refuse to apply the term *man* to a class of creatures which habitually and without compulsion used four legs, we make no scruple of affixing it to persons of very different figures. The whole of human experience being composed of such observed connexions, we have thus materials even for the immense multiplicity of names which human language contains; all which names are, as we have said, regulated in their application by the condition of their expressing such experience.

Thus amid the countless combinations of properties and divisions of classes which the structure of language implies, scarcely any

are arbitrary or capricious. A word which expressed a mere wanton collection of unconnected attributes could hardly be called a *word*; for of such a collection of properties no truth could be asserted, and the word would disappear, for want of some occasion on which it could be used. Though much of the fabric of language appears, not unnaturally, fantastical and purely conventional, it is in fact otherwise. The associations and distinctions of phraseology are not more fanciful than is requisite to make them correspond to the apparent caprices of nature or of thought; and though much in language may be called conventional, the conventions exist for the sake of expressing some truth or opinion, and not for their own sake. The principle, that *the condition of the use of terms is the possibility of general, intelligible, consistent assertions*, is true in the most complete and extensive sense.

7. *Terms may have different Uses.*—The Terms with which we are here most concerned are Names of Classes of natural

objects; and when we say that the principle and the limit of such Names are their use in expressing propositions concerning the classes, it is 102 clear that much will depend on the kind of propositions which we mainly have to express: and that the same name may have different limits, according to the purpose we have in view. For example, is the *whale* properly included in the general term *fish*? When men are concerned in catching marine animals, the main features of the process are the same however the animals may differ; hence whales are classed with fishes, and we speak of the *whale-fishery*. But if we look at the analogies of organization, we find that, according to these, the whale is clearly not a fish, but a *beast*, (confining this term, for the sake of distinctness, to suckling beasts or *mammals*). In Natural History, therefore, the whale is not included among fish. The indefinite and miscellaneous propositions which language is employed to enunciate in the course of common practical life, are replaced by a more coherent and systematic collection of properties, when we come to

aim at scientific knowledge. But we shall hereafter consider the principle of the classifications of Natural History; our present subject is the application of the Idea of Likeness in common practice and common language.

8. *Gradation of Kinds.*—Common names, then, include many individuals associated in virtue of resemblances, and of permanently connected properties; and such names are applicable as far as they serve to express such properties. These collections of individuals are termed *Kinds*, *Sorts*, *Classes*.

But this association of particulars is capable of degrees. As individuals by their resemblances form Kinds, so kinds of things, though different, may resemble each other so as to be again associated in a higher Class; and there may be several successive steps of such classification. *Man*, *horse*, *tree*, *stone*, are each a name of a Kind; but *animal* includes the two first and excludes the others; *living thing* is a term which includes *animal* and *tree* but not *stone*; *body* includes

all the four. And such a subordination of kinds may be traced very widely in the arrangements of language. 103

The condition of the use of the wider is the same as that of the narrower Names of Classes;—they are good as far as they serve to express true propositions. In common language, though such an order of generality may in a variety of instances be easily discerned, it is not systematically and extensively referred to; but this subordination and graduated comprehensiveness is the essence of the methods and nomenclatures of Natural History, as we shall soon have to show.

But such subordination is not without its use, even in common cases, and when it is expressed in the terms of common language. Thus *organized body* is a term which includes plants and animals; *animal* includes beasts, birds, fishes; *beast* includes horses and dogs; *dogs*, again, are greyhounds, spaniels, terriers.

9. *Characters of Kinds.*—Now when we

have such a Series of Names and Classes, we find that we take for granted irresistibly that each class has some *Character* which distinguishes it from other classes included in the superior division. We ask what kind of beast a dog is; what kind of animal a beast is; and we assume that such questions admit of answer;—that each kind has some mark or marks by which it may be described. And such descriptions may be given: an animal is an organized body *having sensation and volition*; man is a *reasonable* animal. Whether or no we assent to the exactness of these definitions, we allow the propriety of their form. If we maintain these definitions to be wrong, we must believe some others to be right, however difficult it may be to hit upon them. We entertain a conviction that there must be, among things so classed and named, a possibility of defining each.

Now what is the foundation of this postulate? What is the ground of this assumption, that there must exist a definition which we have never seen, and

which perhaps no one has seen in a satisfactory form? The knowledge of this definition is by no means necessary to our using the word with propriety; for any one can make true assertions about dogs, but who can define a 104 dog? And yet if the definition be not necessary to enable us to use the word, why is it necessary at all? I allow that we possess an indestructible conviction that there must be such a character of each kind as will supply a definition; but I ask, on what this conviction rests.

I reply, that our persuasion that there must needs be characteristic marks by which things can be defined in words, is founded on the assumption of *the necessary possibility of reasoning*.

The reference of any object or conception to its class without definition, may give us a persuasion that it shares the properties of its class, but such classing does not enable us to reason upon those properties. When we consider man as an animal, we ascribe to

him in thought the appetites, desires, affections, which we habitually include in our notion of animal: but except we have expressed these in some definition or acknowledged description of the term *animal*, we can make no use of the persuasion in ratiocination. But if we have described animals as 'being impelled to action by appetites and passions,' we can not only think, but say, 'man is an animal, and therefore he is impelled to act by appetites and passions.' And if we add a further definition, that 'man is a reasonable animal,' and if it appear that 'reason implies conformity to a rule of action,' we can then further infer that man's nature is to conform the results of animal appetite and passion to a rule of action.

The possibility of pursuing any such train of reasoning as this, depends on the definitions, of *animal* and of *man*, which we have introduced; and the possibility of reasoning concerning the objects around us being inevitably assumed by us from the constitution of our nature, we assume

consequently the possibility of such definitions as may thus form part of our deduction, and the existence of such defining characters.

10. *Difficulty of Definitions.*—But though men are, on such grounds, led to make constant and importunate *demands* for definitions of the terms which they employ in their speculations, they are, in fact, far 105 from being able to carry into complete effect the postulate on which they proceed, that they must be able to find definitions which by logical consequence shall lead to the truths they seek. The postulate overlooks the process by which our classes of things are formed and our names applied. This process consisting, as we have already said, in observing permanent connexions of properties, and in fixing them by the attribution of names, is of the nature of the process of Induction, of which we shall afterwards have to speak. And the postulate is so far true, that this process of induction being once performed, its result may usually be expressed by means of a few definitions,

and may thus lead by a deduction to a train of real truths.

But in the subjects where we principally find such a subordination of classes as we have spoken of, this process of deduction is rarely of much prominence: for example, in the branches of natural history. Yet it is in these subjects that the existence and importance of these characteristic marks, which we have spoken of, principally comes into view. In treating of these marks, however, we enter upon methods which are technical and scientific, not popular and common. And before we make this transition, we have a remark to make on the manner in which writers, without reference to physics or natural history, have spoken of kinds, their subordination, and their marks.

11. '*The Five Words.*'—These things,—the Nature and Relations of Classes,—were, in fact, the subjects of minute and technical treatment by the logicians of the school of Aristotle. Porphyry wrote an Introduction to the *Categories* of that philosopher, which is

entitled *On the Five Words*. The 'Five Words' are *Genus*, *Species*, *Difference*, *Property*, *Accident*. Genus and Species are superior and inferior classes, and are stated[2] to be capable of repeated subordination. The 'most 106 general Genus' is the widest class; the 'most special Species' the narrowest. Between these are intermediate classes, which are Genera with regard to those below, and Species with regard to those above them. Thus Being is the most general Genus; under this is Body; under Body is Living Body; under this again Animal; under Animal is Rational Animal, or Man; under Man are Socrates and Plato, and other individual men.

[2] Porphyr. *Isagog.* c. 23.

The *Difference* is that which is added to the genus to make the species; thus Rational is the Difference by which the genus Animal is made the species Man; the Difference in this Technical sense is the 'Specific,' or species-making Difference[3]. It forms the Definition for the purposes of logic, and corresponds to the 'Character' (specific or

generic) of the Natural Historians. Indeed several of them, as, for instance, Linnæus, in his *Philosophia Botanica*, always call these Characters the *Difference*, by a traditional application of the Peripatetic terms of art.

[3] εἰδοποιός.

Of the other two words, the Property is that which though not employed in defining the class, belongs to every part of it[4]: it is, 'What happens to all the class, to it alone, and at all times; as *to be capable of laughing* is a Property of man.'

[4] *Isagog.* c. 4

The Accident is that which may be present and absent without the destruction of the subject, as to sleep is an Accident (a thing which happens) to man.

I need not dwell further on this system of technicalities. The most remarkable points in it are those which I have already noticed; the doctrine of the successive Subordination of genera, and the fixing attention upon the Specific Difference. These doctrines, though

invented in order to make reasoning more systematic, and at a period anterior to the existence of any Classificatory Science, have, by a curious contrast with the intentions of their founders, been of scarcely 107 any use in sciences of *Reasoning*, but have been amply applied and developed in the *Natural History* which arose in later times.

We must now treat of the principles on which this science (Natural History) proceeds, and explain what peculiar and technical processes it employs in addition to those of common thought and common language.

CHAPTER II.

The Methods of Natural History, as regulated by the Idea of Likeness.

Sect. I.—*Natural History in general.*

1. *Idea of Likeness in Natural History.*—The various branches of Natural History, in so far as they are classificatory sciences merely, and do not depend upon physiological views, rest upon the same Idea of Likeness which is the ground of the application of the names, more or less general, of common language. But the nature of science requires that, for her purposes, this Idea should be applied in a more exact and rigourous manner than in its common and popular employment; just as occurs with regard to the other Ideas on which science is founded;—for instance, as

the idea of space gives rise, in popular use, to the relations implied in the prepositions and adjectives which refer to position and form, and in its scientific development gives rise to the more precise relations of geometry.

The way in which the Idea of Likeness has been applied, so as to lead to the construction of a science, is best seen in Botany: for, in the Classification of Animals, we are inevitably guided by a consideration of the *function* of parts; that is, by an idea of *purpose*, and not of likeness merely: and in Mineralogy, the attempts at classification on the principles of Natural History have been hitherto very imperfectly successful. But in Botany we have an example of a branch of knowledge in which systematic classification has been effected with great beauty and advantage; and in which the peculiarities and principles on which such 109 classification must depend have been carefully studied. Many of the principal botanists, as Linnæus, Adanson, Decandolle, have not only practically applied, but have

theoretically enunciated, what they held to be the sound maxims of classificatory science: and have thus enabled us to place before the reader with confidence the philosophy of this kind of science.

2. *Condition of its Use.*—We may begin by remarking that the Idea of Likeness, in its systematic employment, is governed by the same principle which we have already spoken of as regulating the distribution of things into kinds, and the assignment of names in unsystematic thought and speech; namely, the condition that *general propositions shall be possible*. But as in this case the propositions are to be of a scientific form and exactness, the likeness must be treated with a corresponding precision; and its consequences traced by steady and distinct processes. Naturalists must, for their purposes, employ the resemblances of objects in a technical manner. This technical process may be considered as consisting of three steps;—The fixation of the resemblances; The use of them in making a classification; The means of applying the

classification. These three steps may be spoken of as the *Terminology*, the *Plan of the System*, and the *Scheme of the Characters*.

SECT. II.—*Terminology*.[5]

[5] Decandolle and others use the term *Glossology* instead of Terminology, to avoid the blemish of a word compounded of two parts taken from different languages. The convenience of treating the termination *ology* (and a few other parts of compounds) as not restricted to Greek combinations, is so great, that I shall venture, in these cases, to disregard this philological scruple.

3. *Terminology* signifies the collection of *terms*, or technical words, which belong to the science. But in fixing the meaning of the terms, at least of the descriptive terms, we necessarily fix, at the same time, the perceptions and notions which the terms are to 110 convey; and thus the Terminology of a classificatory science exhibits the elements of its substance as well as of its language. A large but indispensable part of the study of botany (and of mineralogy and zoology also,) consists in the acquisition of the

peculiar vocabulary of the science.

The meaning of technical terms can be fixed in the first instance only by convention, and can be made intelligible only by presenting to the senses that which the terms are to signify. The knowledge of a colour by its name can only be taught through the eye. No description can convey to a hearer what we mean by *apple-green* or *French grey*. It might, perhaps, be supposed that, in the first example, the term *apple*, referring to so familiar an object, sufficiently suggests the colour intended. But it may easily be seen that this is not true; for apples are of many different hues of green, and it is only by a conventional selection that we can appropriate the term to one special shade. When this appropriation is once made, the term refers to the sensation, and not to the parts of this term; for these enter into the compound merely as a help to the memory, whether the suggestion be a natural connexion as in ‘apple-green,’ or a casual one as in ‘French grey.’ In order to derive due advantage from

technical terms of this kind, they must be associated *immediately* with the perception to which they belong; and not connected with it through the vague usages of common language. The memory must retain the sensation; and the technical word must be understood as directly as the most familiar word, and more distinctly. When we find such terms as *tin-white* or *pinchbeck-brown*, the metallic colour so denoted ought to start up in our memory without delay or search.

This, which it is most important to recollect with respect to the simpler properties of bodies, as colour and form, is no less true with respect to more compound notions. In all cases the term is fixed to a peculiar meaning by convention; and the student, in order to use the word, must be completely familiar with the convention, so that he has no need to frame 111 conjectures from the word itself. Such conjectures would always be insecure, and often erroneous. Thus the term *papilionaceous*, applied to a flower, is employed to indicate, not only a

resemblance to a butterfly, but a resemblance arising from five petals of a certain peculiar shape and arrangement; and even if the resemblance to a butterfly were much stronger than it is in such cases, yet if it were produced in a different way, as, for example, by one petal, or two only, instead of a ‘standard,’ two ‘wings,’ and a ‘keel’ consisting of two parts more or less united into one, we should no longer be justified in speaking of it as a ‘papilionaceous’ flower.

The formation of an exact and extensive descriptive language for botany has been executed with a degree of skill and felicity, which, before it was attained, could hardly have been dreamt of as attainable. Every part of a plant has been named; and the form of every part, even the most minute, has had a large assemblage of descriptive terms appropriated to it, by means of which the botanist can convey and receive knowledge of form and structure, as exactly as if each minute part were presented to him vastly magnified. This acquisition was

part of the Linnæan Reform, of which we have spoken in the *History*. 'Tournefort,' says Decandolle[6], 'appears to have been the first who really perceived the utility of fixing the sense of terms in such a way as always to employ the same word in the same sense, and always to express the same idea by the same word; but it was Linnæus who really created and fixed this botanical language, and this is his fairest claim to glory, for by this fixation of language he has shed clearness and precision over all parts of the science.'

[6] *Theor. Elem.* p. 327.

It is not necessary here to give any detailed account of the terms of botany. The fundamental ones have been gradually introduced, as the parts of plants were more carefully and minutely examined. Thus the flower was successively distinguished into the *calyx*, 112 the *corolla*, the *stamens*, and the *pistils*: the sections of the corolla were termed *petals* by Columna; those of the calyx were called *sepals* by Necker[7]. Sometimes terms of greater generality were

devised; as *perianth* to include the calyx and corolla, whether one or both of these were present[8]; *pericarp* for the part inclosing the grain, of whatever kind it be, fruit, nut, pod, &c. And it may easily be imagined that descriptive terms may, by definition and combination, become very numerous and distinct. Thus leaves may be called *pinnatifid*[9], *pinnatipartite*, *pinnatisect*, *pinnatilobate*, *palmatifid*, *palmatipartite*, &c., and each of these words designates different combinations of the modes and extent of the divisions of the leaf with the divisions of its outline. In some cases arbitrary numerical relations are introduced into the definition: thus a leaf is called *bilobate*[10] when it is divided into two parts by a notch; but if the notch go to the middle of its length, it is *bifid*; if it go near the base of the leaf, it is *bipartite*; if to the base, it is *bisect*. Thus, too, a pod of a cruciferous plant is a *silica*[11] if it be four times as long as it is broad, but if it be shorter than this it is a *silicula*. Such terms being established, the form of the very complex leaf or frond of a fern is exactly conveyed by the following phrase:

'fronds rigid pinnate, pinnæ recurved subunilateral pinnatifid, the segments linear undivided or bifid spinuloso-serrate[12].'

[7] Decandolle, 329

[8] For this Erhart and Decandolle use *Perigone*.

[9] Dec. 318.

[10] *Ib.* 493.

[11] *Ib.* 422.

[12] Hooker, *Brit. Flo.* p. 457. *Hymenophyllum Wilsoni*, Scottish filmy-fern, abundant in the highlands of Scotland and about Killarney.

Other characters, as well as form, are conveyed with the like precision: Colour by means of a classified scale of colours, as we have seen in speaking of the Measures of Secondary Qualities; to which, however, we must add, that the naturalist employs arbitrary names, (such as we have already quoted,) and not mere numerical exponents, to indicate a certain number of 113 selected colours. This was done with most precision by Werner, and his scale of colours is still

the most usual standard of naturalists. Werner also introduced a more exact terminology with regard to other characters which are important in mineralogy, as lustre, hardness. But Mohs improved upon this step by giving a numerical scale of hardness, in which *talc* is 1, *gypsum* 2, *calc spar* 3, and so on, as we have already explained in the History of Mineralogy. Some properties, as specific gravity, by their definition give at once a numerical measure; and others, as crystalline form, require a very considerable array of mathematical calculation and reasoning, to point out their relations and gradations. In all cases the features of likeness in the objects must be rightly apprehended, in order to their being expressed by a distinct terminology. Thus no terms could describe crystals for any purpose of natural history, till it was discovered that in a class of minerals the proportion of the faces might vary, while the angle remained the same. Nor could crystals be described so as to distinguish species, till it was found that the derived and primitive forms are connected by very

simple relations of space and number. The discovery of the mode in which characters must be apprehended so that they may be considered as *fixed* for a class, is an important step in the progress of each branch of Natural History; and hence we have had, in the History of Mineralogy and Botany, to distinguish as important and eminent persons those who made such discoveries, Romé de Lisle and Haüy, Cesalpinus and Gesner.

By the continued progress of that knowledge of minerals, plants, and other natural objects, in which such persons made the most distinct and marked steps, but which has been constantly advancing in a more gradual and imperceptible manner, the most important and essential features of similarity and dissimilarity in such objects have been selected, arranged, and fitted with names; and we have thus in such departments, systems of Terminology which fix our attention upon the resemblances which it is proper to consider, and 114 enable us to convey them in words. We

have now to speak of the mode in which such resemblances have been employed in the construction of a Systematic Classification.

SECT. III. *The Plan of the System.*

4. The collection of sound views and maxims by which the resemblances of natural objects are applied so as to form a scientific classification, is a department of the philosophy of natural history which has been termed by some writers (as Decandolle), *Taxonomy*, as containing the *Laws* of the *Taxis* (*arrangement*). By some Germans this has been denominated *Systematik*; if we could now form a new substantive after the analogy of the words *Logick*, *Rhetorick*, and the like, we might call it *Systematick*. But though our English writers commonly use the expression *Systematical Botany* for the Botany of Classification, they appear to prefer the term *Diataxis* for the method of constructing the classification. The rules of such a branch of science are curious and instructive.

In framing a Classification of objects we must attend to their resemblances and differences. But here the question occurs, to *what* resemblances and differences? for a different selection of the points of resemblance would give different results: a plant frequently agrees in leaves with one group of plants, in flowers with another. Which set of characters are we to take as our guide?

The view already given of the regulative principle of all classification, namely, that it must enable us to assert true and general propositions, will obviously occur as applicable here. The object of a scientific Classification is to enable us to enunciate scientific truths: we must therefore classify according to those resemblances of objects (plants or any others) which bring to light such truths.

But this reply to the inquiry, ‘On what characters of resemblance we are to found our system,’ is still too general and vague to be satisfactory. It carries us, 115 however, as

far as this;—that since the truths we are to attend to are scientific truths, governed by precise and homogeneous relations, we must not found our scientific Classification on casual, indefinite, and unconnected considerations. We must not, for instance, be satisfied with dividing plants, as Dioscorides does, into *aromatic*, *esculent*, *medicinal* and *vinous*; or even with the long prevalent distribution into *trees*, *shrubs*, and *herbs*; since in these subdivisions there is no consistent principle.

5. *Latent Reference to Natural Affinity.*—But there may be several kinds of truths, all exact and coherent, which may be discovered concerning plants or any other natural objects; and if this should be the case, our rule leaves us still at a loss in what manner our classification is to be constructed. And, historically speaking, a much more serious inconvenience has been this;—that the task of classification of plants was necessarily performed when the general laws of their form and nature were very little known; or rather, when the existence

of such laws was only just beginning to be discerned. Even up to the present day, the general propositions which botanists are able to assert concerning the structure and properties of plants, are extremely imperfect and obscure.

We are thus led to this conclusion:—that the Idea of Likeness could not be applied so as to give rise to a scientific Classification of plants, till considerable progress was made in studying the general relations of vegetable form and life; and that the selection of the resemblances which should be taken into account, must depend upon the nature of the relations which were then brought into view.

But this amounts to saying that, in the consideration of the Classification of vegetables, other Ideas must be called into action as well as the Idea of Likeness. The additional general views to which the more intimate study of plants leads, must depend, like all general truths, upon some regulating Idea which gives unity to scattered facts. No

progress could be made in botanical knowledge without the 116 operation of such principles: and such additional Ideas must be employed, besides those of mere likeness and unlikeness, in order to point out that Classification which has a real scientific value.

Accordingly, in the classificatory sciences, Ideas other than Likeness do make their appearance. Such Ideas in botany have influenced the progress of the science, even before they have been clearly brought into view. We have especially the Idea of Affinity, which is the basis of all Natural Systems of Classification, and which we shall consider in a succeeding chapter. The assumption that there *is* a Natural System, an assumption made by all philosophical botanists, implies a belief in the existence of Natural Affinity, and is carried into effect by means of principles which are involved in that Idea. But as the formation of all systems of classification must involve, in a great degree, the Idea of Resemblance and Difference, I shall first consider the effect of

that Idea, before I treat specially of Natural Affinity.

6. *Natural Classes.*—Many attempts were made to classify vegetables before the rules which govern a natural system were clearly apprehended. Botanists agree in esteeming some characters as of more value than others, before they had agreed upon any general rules or principles for estimating the relative importance of the characters. They were convinced of the necessity of adding other considerations to that of Resemblance, without seeing clearly what these others ought to be. They aimed at a Natural Classification, without knowing distinctly in what manner it was to be Natural.

The attempts to form *Natural Classes*, therefore, in the first part of their history, belong to the Idea of Likeness, though obscurely modified, even from an early period, by the Ideas of Affinity, and even of Function and of Development. Hence Natural Classes may, to a certain extent, be treated of in this place.

Natural Classes are opposed to Artificial Classes which are understood to be regulated by an *assumed* 117 character. Yet no classes can be so absolutely Artificial in this sense, as to be framed upon characters *arbitrarily* assumed; for instance, no one would speak of a class of shrubs defined by the circumstance of each having a hundred leaves: for of such a class no assertion could be made, and therefore the class could never come under our notice. In what sense then are Artificial Classes to be understood, as opposed to Natural?

7. *Artificial Classes.*—To this question, the following is the answer. When Natural Classes of a certain small extent have been formed, a system may be devised which shall be regulated by a few selected characters, and which shall not dissever these small Natural Classes, but conform to them as far as they go. If these selected characters be then made absolute and imperative, and if we abandon all attempt to obtain Natural Classes of any higher order and wider extent, we form an

Artificial System.

Thus in the Linnæan System of Botanical Classification, it is assumed that certain natural groups, namely, Species and Genera, are established; it is conceived, moreover, that the division of Classes according to the number of stamens and of pistils does not violate the natural connexions of Species and Genera. This arrangement, according to the number of stamens and pistils, (further modified in certain cases by other considerations,) is then made the ground of all the higher divisions of plants, and thus we have an Artificial System.

It has been objected to this view, that the Linnæan Artificial System does not in all cases respect the boundaries of genera, but would, if rigorously applied, distribute the species of the same genus into different artificial classes; it would divide, for instance, the genera *Valeriana*, *Geranium*[13], &c. To this we must reply, that so far as the Linnæan System does this, it is an imperfect Artificial System. Its great merit is in its

making such a disjunction in comparatively so 118 few cases; and in the artificial characters being, for the most part, obvious and easily applied.

[13] Decand. *Theor. Elem.* p. 45.

8. *Are Genera Natural*?—It has been objected also that Genera are not Natural groups. Linnæus asserts in the most positive manner that they are[14]. On which Adanson observes[15], 'I know not how any Botanist can maintain such a thesis: that which is certain is, that up to the present time no one has been able to prove it, nor to give an exact definition of a natural genus, but only of an artificial.' He then brings several arguments to confirm this view.

[14] *Phil. Bot.* Art. 165.

[15] *Famille de Ph.* Pref. cv.

But we are to observe, in answer to this, that Adanson improperly confounds the recognition of the existence of a natural group with the invention of a technical mark or definition of it. Genera are groups

of species associated in virtue of natural affinity, of general resemblance, of real propinquity: of such groups, certain selected characters, one or few, may usually be discovered, by which the species may be referred to their groups. These Artificial characters do not constitute, but indicate the genus: they are the *Diagnosis*, not the basis of the *Diataxis*: and they are always subject to be rejected, and to have others substituted for them, when they violate the natural connexion of species which a minute and enlarged study discovers.

It is, therefore, no proof that Genera are not Natural, to say that their artificial characters are different in different systems. Such characters are only different attempts to confine the variety of nature within the limits of definition. Nor is it sufficient to say that these groups themselves are different in different writers; that some botanists make genera what others make only species; as *Pedicularis*, *Rhinanthus*, *Euphrasia*, *Antirrhinum*[16]. This discrepancy shows only that the natural arrangement is not yet

completely known, even in the smaller groups; a conclusion to which we need not refuse our assent. But in 119 opposition to these negatives, the manner in which Genera have been established proves that they are regulated by the principle of being natural, and by that alone. For they are not formed according to any *à priori* rule. The Botanist does not take any selected or arbitrary part or parts of the plants, and marshal his genera according to the differences of this part. On the contrary, the divisions of genera are sometimes made by means of the flower; sometimes by means of the fruit: the anthers, the stamens, the seeds, the pericarp, and the most varied features of these parts, are used in the most miscellaneous and unsystematic manner. Linnæus has indeed laid down a maxim that the characteristic differences of genera must reside in the fructification[17]: but Adanson has justly remarked[18], that an arbitrary restriction like this makes the groups artificial: and that in some families other characters are more essential than those of the fructification; as the leaves in the

families of *Aparineæ* and *Leguminosæ*, and the disposition of the flowers in *Labiatæ*. And Naturalists are so far from thinking it sufficient to distribute species into genera by *arbitrary* marks, that we find them in many cases lamenting the absence of good *natural* marks: as in the families of *Umbelliferæ*, where Linnæus declared that any one who could find good characters of genera would deserve great admiration, and where it is only of late that good characters have been discovered and the arrangement settled[19] by means principally of the ribs of the fruit[20].

[16] Adanson, p. cvi.

[17] *Phil. Bot.* Art. 162.

[18] Adanson, Pref. p. cxx.

[19] Lindley, *Nat. Syst.* p. 5.

[20] In like manner we find Cuvier saying of Rondelet that he has 'un *sentiment* très vrai des genres.' *Hist. Ichth.* p. 39.

It is thus clear that Genera are not established on any assumed or preconceived basis. What, then, is the principle which

regulates botanists when they try to fix genera? What is the arrangement which they thus wish for, without being able to hit upon it? What is the tendency which thus drives them from the corolla to the anthers, from the flower to the fruit, 120 from the fructification to the leaves? It is plain that they seek something, not of their own devising and creating;—not anything merely conventional and systematic; but something which they conceive to exist in the relations of the plants themselves;—something which is without the mind, not within;—in nature, not in art;—in short, a Natural Order.

Thus the regulative principle of a Genus, or of any other natural group is, that it is, or is supposed to be, natural. And by reference to this principle as our guide, we shall be able to understand the meaning of that indefiniteness and indecision which we frequently find in the descriptions of such groups, and which must appear so strange and inconsistent to any one who does not suppose these descriptions to assume any deeper ground of connexion than an

arbitrary choice of the botanist. Thus in the family of the Rose-tree, we are told that the *ovules* are *very rarely* erect[21], the *stigmata* are *usually* simple. Of what use, it might be asked, can such loose accounts be? To which the answer is, that they are not inserted in order to distinguish the species, but in order to describe the family, and the total relations of the ovules and of the stigmata of the family are better known by this general statement. A similar observation may be made with regard to the Anomalies of each group, which occur so commonly, that Mr. Lindley, in his *Introduction to the Natural System of Botany*, makes the 'Anomalies' an article in each Family. Thus, part of the character of the Rosaceæ is that they have alternate *stipulate* leaves, and that the *albumen* is *obliterated*: but yet in *Lowea*, one of the genera of this family, the stipulæ are *absent*; and the albumen is *present* in another, *Neillia*. This implies, as we have already seen, that the artificial character (or *diagnosis* as Mr. Lindley calls it) is imperfect. It is, though very nearly, yet not exactly, commensurate

with the natural group: and hence, in certain cases, this character is made to yield to the general weight of natural affinities.

[21] Lindley, *Nat. Syst.* p. 81.

121 9. *Difference of Natural History and Mathematics.*—These views,—of classes determined by characters which cannot be expressed in words,—of propositions which state, not what happens in all cases, but only usually,—of particulars which are included in a class though they transgress the definition of it, may very probably surprise the reader. They are so contrary to many of the received opinions respecting the use of definitions and the nature of scientific propositions, that they will probably appear to many persons highly illogical and unphilosophical. But a disposition to such a judgment arises in a great measure from this;—that the mathematical and mathematico-physical sciences have, in a great degree, determined men's views of the general nature and form of scientific truth; while Natural History has not yet had time or opportunity to exert its

due influence upon the current habits of philosophizing. The apparent indefiniteness and inconsistency of the classifications and definitions of Natural History belongs, in a far higher degree, to all other except mathematical speculations: and the modes in which approximations to exact distinctions and general truths have been made in Natural History, may be worthy our attention, even for the light they throw upon the best modes of pursuing truth of all kinds.

10. *Natural Groups given by Type not by Definition.*—The further development of this suggestion must be considered hereafter. But we may here observe, that though in a Natural Group of objects a definition can no longer be of any use as a regulative principle, classes are not, therefore, left quite loose, without any certain standard or guide. The class is steadily fixed, though not precisely limited; it is given, though not circumscribed; it is determined, not by a boundary line without, but by a central point within; not by what it strictly

excludes, but by what it eminently includes; by an example, not by a precept; in short, instead of Definition we have a *Type* for our director.

A Type is an example of any class, for instance, a species of a genus, which is considered as eminently 122 possessing the characters of the class. All the species which have a greater affinity with this Type-species than with any others, form the genus, and are ranged about it, deviating from it in various directions and different degrees. Thus a genus may consist of several species, which approach very near the type, and of which the claim to a place with it is obvious; while there may be other species which straggle further from this central knot, and which yet are clearly more connected with it than with any other. And even if there should be some species of which the place is dubious, and which appear to be equally bound by two generic types, it is easily seen that this would not destroy the reality of the generic groups, any more than the scattered trees of the

intervening plain prevent our speaking intelligibly of the distinct forests of two separate hills.

The Type-species of every genus, the Type-genus of every family, is, then, one which possesses all the characters and properties of the genus in a marked and prominent manner. The Type of the Rose family has alternate stipulate leaves, wants the albumen, has the ovules not erect, has the stigmata simple, and besides these features, which distinguish it from the exceptions or varieties of its class, it has the features which make it prominent in its class. It is one of those which possess clearly several leading attributes; and thus, though we cannot say of any one genus that it *must* be the Type of the family, or of any one species that it *must* be the Type of the genus, we are still not wholly to seek: the Type must be connected by many affinities with most of the others of its group; it must be near the center of the crowd, and not one of the stragglers.

11. It has already been repeatedly stated, as the great rule of all classification, that the classification must serve to assert general propositions. It may be asked *what* propositions we are able to enunciate by means of such classifications as we are now treating of. And the answer is, that the collected knowledge of the characters, habits, properties, organization, and 123 functions of these groups and families, as it is found in the best botanical works, and as it exists in the minds of the best botanists, exhibits to us the propositions which constitute the science, and to the expression of which the classification is to serve. All that is not strictly definition, that is, all that is not artificial character, in the descriptions of such classes, is a statement of truths, more or less general, more or less precise, but making up, together, the positive knowledge which constitutes the science. As we have said, the consideration of the properties of plants in order to form a system of classification, has been termed Taxonomy, or the Systematick of Botany; all the parts of the descriptions, which, taking

the system for granted, convey additional information, are termed the *Physiography* of the science; and the same terms may be applied in the other branches of Natural History.

12. *Artificial and Natural Systems.*—If I have succeeded in making it apparent that an artificial system of characters necessarily implies natural classes which are not severed by the artificial marks, we shall now be able to compare the nature and objects of the Artificial and Natural Systems; points on which much has been written in recent times.

The Artificial System is one which is, or professes to be, entirely founded upon marks selected according to the condition which has been stated, of not violating certain narrow natural groups; namely in the Linnæan system, the natural genera of plants. The marks which form the basis of the system, being thus selected, are applied rigorously and universally without any further regard to any other characters or

indications of affinity. Thus in the Linnæan system, which depends mainly on the number of male organs or stamens, and on the number of female organs or styles, the largest divisions, or the Classes, are arranged according to the number of the stamens, and are *monandria*, *diandria*, *triandria*, *tetrandria*, *pentandria*, *hexandria*, and so on: the names being formed of the Greek numerical words, and of the word which implies *male*. And the Orders of each of these Classes are 124 distinguished by the number of styles, and are called *monogynia*, *digynia*, *trigynia*, and so on, the termination of these words meaning *female*. And so far as this numerical division and subdivision go on, the system is a rigorous system, and strictly artificial.

But the condition that the artificial system shall leave certain natural affinities untouched, makes it impossible to go through the vegetable kingdom by a method of mere numeration of stamens and styles. The distinction of flowers with twenty and with thirty stamens is not a fixed

distinction: flowers of one and the same kind, as roses, have, some fewer than the former, some more than the latter number. The Artificial System, therefore, must be modified. And there are various relations of connexion and proportion among the stamina which are more permanent and important than their mere number. Thus flowers with two longer and two shorter stamens are not placed in the class *tetrandria*, but are made a separate class *didynamia*; those with four longer and two shorter are in like manner *tetradynamia*, not *hexandria*; those in which the filaments are bound into two bundles are *diadelphia*. All these and other classes are deviations from the plan of the earlier Classes, and are so far defects of the artificial system; but they are deviations requisite in order that the system may leave a basis of natural groups, without which it would not be a System of *Vegetables*. And as the division is still founded on some properties of the stamens, it combines not ill with that part of the system which depends on the number of them. The Classes framed in virtue of these

various considerations make up an Artificial System which is tolerably coherent.

‘But since the Artificial System thus regards natural groups, in what does it differ from a Natural System?’ It differs in this:—That though it allows certain subordinate natural groups, it merely allows *these*, and does not endeavour to ascend to any wider natural groups. It takes all the *higher* divisions of its scheme from its artificial characters, its stamens and pistils, without looking to any natural affinities. It 125 accepts natural *Genera*, but it does not seek natural *Families*, or Orders, or Classes. It *assumes* natural groups, but does not *investigate* any; it forms wider and higher groups, but professes to frame them arbitrarily.

But then, on the other hand, the question occurs, ‘This being the case, what can be the use of the Artificial System?’ If its characters, in the higher stages of classification, be arbitrary, how can it lead us to the natural relations of plants? And

the answer is, that it does so in virtue of the original condition, that there shall be certain natural relations which the artificial system shall not transgress; and that its use arises from the facility with which we can follow the artificial arrangement as far as it goes. We can count the stamens and pistils, and thus we know the Class and Order of our plant; and we have then to discover its Genus and Species by means less symmetrical but more natural. The Artificial System, though arbitrary in a certain degree, brings us to a Class in which the whole of each Genus is contained, and there we can find the proper Genus by a suitable method of seeking. No Artificial System can conduct us into the extreme of detail, but it can place us in a situation where the detail is within our reach. We cannot find the house of a foreign friend by its latitude and longitude; but we may be enabled, by a knowledge of the latitude and longitude, to find the city in which he dwells, or at least the island; and we then can reach his abode by following the road or exploring the locality. The Artificial System is such a

method of travelling by latitude and longitude; the Natural System is that which is guided by a knowledge of the country.

The Natural System, then, is that which endeavours to arrange by the natural affinities of objects; and more especially, which attempts to ascend from the lower natural groups to the higher; as for example from genera to natural families, orders, and classes. But as we have already hinted, these expressions of natural affinities, natural groups, and the like, when 126 considered in reference to the idea of resemblance alone, without studying analogy or function, are very vague and obscure. We must notice some of the attempts which were made under the operation of this imperfect view of the subject.

SECT. IV.—*Modes of framing Natural Systems.*

13. Decandolle[22] distinguishes the attempts at Natural Classifications into three sorts: those of *blind trial* (*tâtonnement*), those of *general comparison*, and those of

subordination of characters. The two former do not depend distinctly upon any principle, except resemblance; the third refers us to other views, and must be considered in a future chapter.

[22] *Theor. Elem.* art 41.

Method of Blind Trial.—The notion of the existence of natural classes dependent on the general resemblance of plants,—of an affinity showing itself in different parts and various ways,—though necessarily somewhat vague and obscure, was acted upon at an early period, as we have seen in the formation of genera; and was enunciated in general terms soon after. Thus Magnolius[23] says that he discerns in plants an affinity, by means of which they may be arranged in families: 'Yet it is impossible to obtain from the fructification alone the Characters of these families; and I have therefore chosen those parts of plants in which the principal characteristic marks are found, as the root, the stem, the flower, the seed. In some plants there is even a certain resemblance; an affinity which does not

consist in the parts considered separately, but in their totality; an affinity which may be felt but not expressed; as we see in the families of agrimonies and cinquefoils, which every botanist will judge to be related, though they differ by their roots, their leaves, their flowers, and their seeds.'

[23] Dec. *Theor. Elem.* art. 42. Petri Magnoli, *Prodromus Hist. Gen. Plant.* 1689.

127 This obscure feeling of a resemblance on the whole, an affinity of an indefinite kind, appears fifty years later in Linnæus's attempts. 'In the Natural Classification,' he says[24], 'no *à priori* rule can be admitted, no part of the fructification can be taken exclusively into consideration; but only the simple symmetry of all its parts.' Hence though he proposed Natural Families, and even stated the formation of such Families to be the first and last object of all Methods, he never gave the Characters of those groups, or connected them by any method. He even declared it to be impossible to lay down such a system of characters. This persuasion was the result of his having

refused to admit into his mind any Idea more profound than that notion of Resemblance of which he had made so much and such successful use; he would not attempt to unravel the Ideas of Symmetry and of Function on which the clear establishment of natural relations must depend. He even despised the study of the inner organization of plants; and reckoned[25] the *Anatomici*, who studied the anatomy and physiology of plants and the laws of vegetation, among the *Botanophili*, the mere amateurs of his science.

[24] Dec. *Theor. Elem.* art 42.

[25] *Phil. Bot.* s. 44.

The same notion of general resemblance and affinity, accompanied with the same vagueness, is to be found in the writer who least participated in the general admiration of Linnæus, Buffon. Though it was in a great measure his love of higher views which made him dislike what he considered the pedantry of the Swedish school, he does not seem to have obtained a clearer sight of the

principle of the natural method than his rival, except that he did not restrict his Characters to the fructification. Things must be arranged by their resemblances and differences, (he says in 1750[26],) 'but the resemblances and differences must be taken not from one part but from the whole; and we must attend to the form, the size, the habit, the number and position of the parts, even the substance 128 of the part; and we must make use of these elements in greater or smaller number, as we have need.'

[26] Adanson, p. clvi. Buffon, *Hist. Nat.* t. i. p. 21.

14. *Method of General Comparison.*—A countryman of Buffon, who shared with him his depreciating estimate of the Linnæan system, and his wish to found a natural system upon a broader basis, was Adanson; and he invented an ingenious method of apparently avoiding the vagueness of the practice of following the general feeling of resemblance. This method consisted in making many Artificial Systems, in each of which plants were arranged by some one

part; and then collecting those plants which came near each other in the greatest number of those Artificial Systems, as plants naturally the most related. Adanson gives an account[27] of the manner in which this system arose in his mind. He had gone to Senegal, animated by an intense zeal for natural history; and there, amid the luxuriant vegetation of the torrid zone, he found that the methods of Linnæus and Tournefort failed him altogether as means of arranging his new botanical treasures. He was driven to seek a new system. 'For this purpose,' he says, 'I examined plants in all their parts, without omitting any, from the roots to the embryo, the folding of the leaves in the bud, their mode of sheathing[28], the situation and folding of the embryo and of its radicle in the seed, relatively to the fruit; in short, a number of particulars which few botanists notice. I made in the first place a complete description of each plant, putting each of its parts in separate articles, in all its details; when new species occurred I put down the points in which they differed, omitting those in which they

agreed. By means of the aggregate of these comparative descriptions, I perceived that plants arranged themselves into classes or families which could not be artificial or arbitrary, not being founded upon one or two parts, which might change at certain limits, but on all the parts; so that the disproportion of one of these parts was corrected and balanced by the introduction of another.' Thus the principle of Resemblance 129 was to suffice for the general arrangement, not by means of a new principle, as Symmetry or Organization, which should regulate its application, but by a numeration of the peculiarities in which the resemblance consisted.

[27] Pref. p. clvii.

[28] 'Leur manière de s'engainer.'

The labour which Adanson underwent in the execution of this thought was immense. By taking each Organ, and considering its situation, figure, number, &c., he framed sixty-five Artificial Systems; and collected his Natural Families by a numerical

combination of these. For example, his *sixty-fifth* Artificial System[29] is that which depends upon the situation of the Ovary with regard to the Flower; according to this system he frames *ten* Artificial Classes, including *ninety-three* Sections: and of these Sections the resulting Natural Arrangement retains *thirty-five*, above one-third: the same estimate is applied in other cases.

[29] Adanson, Pref. p. cccxii.

But this attempt to make Number supply the defects which the vague notion of Resemblance introduces, however ingenious, must end in failure. For, as Decandolle observes[30], it supposes that we know, not only all the Organs of plants, but all the points of view in which it is possible to consider them; and even if this assumption were true, which it is not, and must long be very far from being, the principle is altogether vicious; for it supposes that all these points of view, and all the resulting artificial systems are of equal importance:—a supposition manifestly erroneous. We are thus led back to the

consideration of the *Relative Importance* of Organs and their qualities, as a basis for the classification of plants, which no Artificial Method can supersede; and thus we find the necessity of attending to something besides mere external and detached Resemblance. The method of General Comparison cannot, any more than the method of Blind Trial, lead us, with any certainty or clearness, to the Natural Method. Adanson's Families are held by the best botanists to be, for the greater part, Natural; but his hypotheses are unfounded; and his success is 130 probably more due to the dim feeling of Affinity, by which he was unconsciously guided, than to the help he derived from his numerical processes.

[30] Dec. *Theor. Elem.* p. 67.

In a succeeding chapter I shall treat of that Natural Affinity on which a Natural System must really be founded. But before proceeding to this higher subject, we must say a few words on some of the other parts of the philosophy of Natural History,—the Gradation of Groups, the Nomenclature, the

Diagnosis, and the application of the methods to other subjects.

SECT. V.—*Gradation of Groups.*

15. It has been already noticed (last chapter,) that even that vague application of the idea of resemblance which gives rise to the terms of common language, introduces a subordination of classes, as *man*, *animal*, *body*, *substance*. Such a subordination appears in a more precise form when we employ this idea in a scientific manner as we do in Natural History. We have then a series of divisions, each inclusive of the lower ones, which are expressed by various metaphors in different writers. Thus some have gone as far as eight terms of the series[31], and have taken, for the most part, military names for them; as *Hosts*, *Legions*, *Phalanxes*, *Centuries*, *Cohorts*, *Sections*, *Genera*, *Species*. But the most received series is *Classes*, *Orders*, *Genera*, and *Species*; in which, however, we often have other terms interpolated, as *Sub-genera*, or Sections of genera. The expressions *Family* and *Tribe*,

are commonly appropriated to natural groups; and we speak of the Vegetable, Animal, Mineral *Kingdom*; but the other metaphors of Provinces, Districts, &c., which this suggests, have not been commonly used[32].

[31] Adanson, p. cvi.

[32] *Sub-Kingdom* has recently been employed by some naturalists.

It will of course be understood that each ascending step of classification is deduced by the same process from the one below. A Genus is a collection of Species which resemble each other more than they 131 resemble other species; an Order is a collection of Genera having, in like manner, the first degree of resemblance, and so on. How close or how wide the Degrees of Resemblance are, must depend upon the nature of the objects compared, and cannot possibly be prescribed beforehand. Hence the same term, *Class* and *Order* for instance, may imply, in different provinces of nature, very different degrees of resemblance. The Classes of Animals are Insects, Birds, Fish,

Beasts, &c. The Orders of Beasts are *Ruminants*, *Tardigrades*, *Plantigrades*, &c. The two Classes of Plants (according to the Natural Order[33]) are *Vascular* and *Cellular*, the latter having neither sexes, flowers, nor spiral vessels. The Vascular Plants are divided into Orders, as *Umbelliferæ*, *Ranunculaceæ*, &c.; but between this Class and its Orders are interposed two other steps:—two Sub-classes, *Dicotyledonous* and *Monocotyledonous*, and two Tribes of each: *Angiospermiæ*, *Gymnospermiæ* of the first; and *Petaloideæ*, *Glumaciæ* of the second. Such interpolations are modifications of the general formula of subordination, for the purpose of accommodating it to the most prominent natural affinities.

[33] Lindley.

16. *Species.*—As we have already seen in tracing the principles of the Natural Method, when by the intimate study of plants we seek to give fixity and definiteness to the notion of resemblance and affinity on which all these divisions depend, we are led to the study of

Organization and Analogy. But we make a reference to physiological conditions even from the first, with regard to the lowest step of our arrangement, the *Species*; for we consider it a proof of the impropriety of separating two Species, if it be shown that they can by any course of propagation, culture, and treatment, the one pass into the other. It is in this way, for example, that it has been supposed to be established that the common Primrose, Oxlip, Polyanthus, and Cowslip, are all the same species. Plants which thus, in virtue of external circumstances, as soil, 132 exposure, climate, exhibit differences which may disappear by changing the circumstances, are called *Varieties* of the species. And thus we cannot say that a Species is a collection of individuals which possess the First Degree of Resemblance; for it is clear that a primrose resembles another primrose more than it does a cowslip; but this resemblance only constitutes a Variety. And we find that we must necessarily include in our conception of Species, the notion of propagation from the same stock. And thus

a Species has been well defined[34]: 'The collection of the individuals descended from one another, or from common parents, and of those which resemble these as much as these resemble each other.' And thus the sexual doctrine of plants, or rather the consideration of them as things which propagate their kind, (whether by seed, shoot, or in any other way,) is at the basis of our classifications.

[34] Cuv. *Règne Animal*, p. 19.

17. The First permanent Degree of Resemblance among organized beings is thus that which depends on this relation of generation, and we might expect that the groups which are connected by this relation would derive their names from the notion of generation. It is curious that both in Greek and Latin languages and in our own, the words which have this origin (γένος, *genus*, *kind*,) do not, in the phraseology of science at least, denote the nearest degree of relationship, but have other terms subordinate to them, which appear etymologically to indicate a mere

resemblance of appearance (εἶδος, *species*, *sort*); and these latter terms are appropriated to the groups resulting from propagation. Probably the reason of this is, that the former terms (*genus*, &c.) had been applied so widely and loosely before the scientific fixation of terms, that to confine them to what we call *species* would have been to restrict them in a manner too unusual to be convenient.

18. *Varieties. Races.*—The Species, as we have said, is the collection of individuals which resemble each other as much as do the offspring of a common 133 stock. But within the limits of this boundary, there are often observable differences permanent enough to attract our notice, though capable of being obliterated by mixture in the course of generation. Such different groups are called *Varieties*. Thus the Primrose and Cowslip, as has been stated above, are found to be varieties of the same plant; the Poodle and the Greyhound are well marked varieties of the species *dog*. Such differences are hereditary, and it may be long doubtful

whether such hereditary differences are varieties only, or different species. In such cases the term *Race* has been applied.

SECT. VI.—*Nomenclature.*

19. The Nomenclature of any branch of Natural History is the collection of names of all its species; which, when they become extremely numerous, requires some artifice to make it possible to recollect or apply them. The known species of plants, for example, were 10,000 at the time of Linnæus, and are now probably 60,000. It would be useless to endeavour to frame and employ separate names for each of these species.

The division of the objects into a subordinated system of classification enables us to introduce a Nomenclature which does not require this enormous number of names. The artifice employed to avoid this inconvenience is to name a Species by means of two (or it might be more) steps of the successive division. Thus

in Botany, each of the genera has its name, and the species are marked by the addition of some epithet to the name of the genus. In this manner about 1,700 generic names, with a moderate number of specific names, were found by Linnæus sufficient to designate with precision all the species of vegetables known at his time. And this *Binary Method* of Nomenclature has been found so convenient that it has been universally adopted in every other department of the Natural History of organized beings.

Many other modes of Nomenclature have been tried, but no other has at all taken root. Linnæus himself 134 appears at first to have intended marking each species by the Generic Name accompanied by a characteristic Descriptive Phrase; and to have proposed the employment of a *trivial* Specific Name, as he termed it, only as a method of occasional convenience. The use of these *trivial names*, has, however, become universal, as we have said, and is by many persons considered the greatest

improvement introduced at the Linnæan reform.

Both Linnæus and other writers (as Adanson) have given many maxims with a view of regulating the selection of generic and specific names. The maxims of Linnæus were intended as much as possible to exclude barbarism and confusion, and have, upon the whole, been generally adopted; though many of them were objected to by his contemporaries (Adanson and others[35]), as capricious or unnecessary innovations. Many of the names, introduced by Linnæus, certainly appear fanciful enough: thus he gives the name of *Bauhinia* to a plant with leaves in pairs, because the Bauhins were a pair of brothers; *Banisteria* is the name of a climbing plant, in honour of Banister, who travelled among mountains. But such names, once established by adequate authority, lose all their inconvenience, and easily become permanent; and hence the reasonableness of the Linnæan rule[36], that as such a perpetuation of the names of persons by the names of plants is the only honour

botanists have to bestow, it ought to be used with care and caution.

[35] Pp. cxxix. clxxii.

[36] *Phil Bot.* s. 239.

The generic name must, as Linnæus says, be fixed[37] before we attempt to form a specific name; 'the latter without the former is like the clapper without the bell.' The name of the genus being established, the species may be marked by adding to it 'a single word taken at will from any quarter;' that is, not involving a description or any essential property of the plant, but a casual or arbitrary appellation[38]. Thus the 135 various species of *Hieracium*[39] are *Hieracium Alpinum*, *H. Halleri*, *H. Pilosella*, *H. dubium*, *H. murorum*, &c. where we see how different may be the kind of origin of the words.

[37] *Ib.* s. 222.

[38] *Ib.* s. 260.

[39] Hooker, *Fl. Scot.* 228.

Attempts have been made at various times to form the name of species from

those of genera in some more symmetrical manner. Thus some have numbered the species of genus, 1, 2, 3, &c.; but this method is liable to the inconveniences, first, that it offers nothing for the memory to take hold of; and second, that if a new species intermediate between 1 and 2, 2 and 3, &c., be discovered, it cannot be put in its place. It has also been proposed to mark the species by altering the termination of the genus. Thus Adanson[40], denoting a genus by the name *Fonna* (*Lychnidea*), conceived he might mark five of its species by altering the last vowel, *Fonna*, *Fonna-e*, *Fonna-i*, *Fonna-o*, *Fonna-u*; then others by *Fonna-ha*, *Fonna-ka*, and so on. This course would be liable to the same evils which have been noticed as belonging to the numerical method.

[40] Pref. clxxvi.

The names of plants (and the same is true of animals) have in common practice been binary only, consisting of a generic and a specific name. The Class and Order have not been admitted to form part of the appellation of the species. Indeed it is easy

to see that a name which must be identical in so many instances as that of an Order would be, would be felt as superfluous and burdensome. Accordingly, Linnæus makes it a precept[41], that the name of the Class and the Order must not be expressed but understood: and hence, he says, Royen, who took *Lilium* for the name of a Class, rightly rejected it as a generic name, and substituted *Lirium*, with the Greek termination.

[41] *Phil. Bot.* s. 215.

Yet we must not too peremptorily assume such maxims as these to be universal for all classificatory sciences. It is very possible that it may be found advisable to use *three* terms, that of order, genus and 136 species, in designating minerals, as is done in Mohs's nomenclature; for example, *Rhombohedral Calc Haloide*, *Paratomous Hal Baryte*.

It is possible also that it may be found useful in the same science to mark some of the steps of classification by the termination. Thus it has been proposed to

confine the termination *ite* to the Order *Silicides* of Naumann, as Apophyll*ite*, Stilb*ite*, Leuc*ite*, &c., and to use names of different form in other orders, as Talc *Spar* for Brennerite, Pyramidal Titanium *Oxide* for Octahedrite. Some such method appears to be the most likely to give us a tolerable mineralogical nomenclature.

SECT. VII.—*Diagnosis.*

20. German Naturalists speak of a part of the general method which they call the *Characteristik* of Natural History, and which is distinguished from the *Systematik* of the science. The *Systematick* arranges the objects by means of all their resemblances, the *Characteristick* enables us to detect their place in the arrangement by means of a few of their characters. What these characters are to be, must be discovered by observation of the groups and divisions of the system when they are formed. To construct a collection of such characters as shall be clear and fixed, is a useful, and generally a difficult task; for there is usually

no apparent connexion between the marks which are used in discriminating the groups, and the nature of the groups themselves. They are assumed only because the naturalist, extensively and exactly acquainted with the groups and the properties of the objects which compose them, sees, by a survey of the field, that these marks divide it properly.

The Characteristick has been termed by some English Botanists the *Diagnosis* of plants; a word which we may conveniently adopt. The Diagnosis of any genus or species is different according to the system we follow. Thus in the Linnæan System the Diagnosis of the Rose is in the first place given by its Class and Order: it is 137 Icosandrous, and Polygynous; and then the Generic Distinction is that the calyx is five-cleft, the tube urceolate, including many hairy achenia, the receptacle villous[42]. In the Natural System the Rose-Tribe are distinguished as being[43] 'Polypetalous dicotyledons, with lateral styles, superior simple ovaria, regular perigynous stamens,

exalbuminous definite seeds, and alternate stipulate leaves.' And the true Roses are further distinguished by having 'Nuts, numerous, hairy, terminated by the persistent lateral style and inclosed within the fleshy tube of the calyx,' &c.

[42] Lindley, *Nat. Syst.* p. 149.

[43] *Ib.* pp. 81, 3.

It will be observed that in a rigorous Artificial System the *Systematick* coincides with the *Characteristick*; the *Diataxis* with the *Diagnosis*; the reason why a plant is put in a division is identical with the mode by which it is known to be in the division. The Rose is in the class *icosandria*, because it has many stamens inserted in the calyx; and when we see such a set of stamens we immediately know the class. But this is not the case with the Diagnosis of Natural Families. Thus the genera *Lamium* and *Galeopsis* (Dead Nettle and Hemp Nettle) are each formed into a separate group in virtue of their general resemblances and differences, and not because the former has

one tooth on each side of the lower lip, and the latter a notch in its upper lip, though they are distinguished by these marks.

Thus so far as our Systems are natural, (which, as we have shown, all systems to a certain extent must be), the Characteristick is distinct both from a Natural and an Artificial System; and is, in fact, an Artificial Key to a Natural System. As being Artificial, it takes as few characters as possible; as being Natural, its characters are not selected by any general or prescribed rule, but follow the natural affinities. The Botanists who have made any steps in the formation of a natural method of plants since Linnæus, have all attempted to give a Diagnosis corresponding to the Diataxis of their method.

CHAPTER III.

Application of the Natural History Method to Mineralogy.

1. THE philosophy of the Sciences of Classification has had great light thrown upon it by discussions concerning the methods which are used in Botany: for that science is one of the most complete examples which can be conceived of the consistent and successful application of the principles and ideas of Classification; and this application has been made in general without giving rise to any very startling paradoxes, or disclosing any insurmountable difficulties. But the discussions concerning methods of Mineralogical Classification have been instructive for quite a different reason: they have brought into view the boundaries and the difficulties of the

process of Classification; and have presented examples in which every possible mode of classifying appeared to involve inextricable contradictions. I will notice some of the points of this kind which demand our attention, referring to the works published recently by several mineralogists.

In the History of Mineralogy we noticed the attempt made by Mohs and other Germans to apply to minerals a method of arrangement similar to that which has been so successfully employed for plants. The survey which we have now taken of the grounds of that method will point out some of the reasons of the very imperfect success of this attempt. We have already said that the *Terminology* of Mineralogy was materially reformed by Werner; and including in this branch of the subject (as we must do) the Crystallography of later writers, it may be considered as to a great extent complete. Of the attempts at a Natural arrangement, that of Mohs appears to proceed by the 139 method of *blind trial*, the undefinable perception of relationship,

by which the earliest attempts at a Natural Arrangement of plants were made. Breithaupt however, has made (though I do not know that he has published) an essay in a mode which corresponds very nearly to Adanson's process of *multiplied comparisons*. Having ascertained the specific gravity and hardness of all the species of minerals, he arranged them in a table, representing by two lines at right angles to each other these two numerical quantities. Thus all minerals were distributed according to two co-ordinates representing specific gravity and hardness. He conceived that the groups which were thus brought together were natural groups. On both these methods, and on all similar ones, we might observe, that in minerals as in plants, the mere general notion of Likeness cannot lead us to a real arrangement: this notion requires to have precision and aim given it by some other relation;—by the relation of Chemical Composition in minerals, as by the relation of Organic Function in vegetables. The physical and crystallographical properties of minerals must be studied with reference to

their constitution; and they must be arranged into Groups which have some common Chemical Character, before we can consider any advance as made towards a Natural Arrangement.

In reality, it happens in Mineralogy as it happened in Botany, that those speculators are regulated by an obscure perception of this ulterior relation, who do not profess to be regulated by it. Several of the Orders of Mohs have really great unity of chemical character, and thus have good evidence of their being really Natural Orders.

2. Supposing the Diataxis of minerals thus obtained, Mohs attempted the Diagnosis; and his *Characteristick of the Mineral Kingdom*, published in Dresden, in 1820, was the first public indication of his having constructed a system. From the nature of a Characteristick, it is necessarily brief, and without any ostensible principle; but its importance was duly appreciated by the author's countrymen. Since that 140 time, many attempts have been made at improved

arrangements of minerals, but none, I think, (except perhaps that of Breithaupt,) professing to proceed rigorously on the principles of Natural History;—to arrange by means of external characters, neglecting altogether, or rather postponing, the consideration of chemical properties. By relaxing from this rigour, however, and by combining physical and chemical considerations, arrangements have been obtained (for example, that of Naumann,) which appear more likely than the one of Mohs to be approximations to an ultimate really natural system. Naumann's Classes are *Hydrolytes*, *Haloides*, *Silicides*, *Metal Oxides*, *Metals*, *Sulphurides*, *Anthracides*, with subdivisions of Orders, as *Anhydrous unmetallic Silicides*. It may be remarked that the designations of these are mostly chemical. As we have observed already, Chemistry, and Mineralogy in its largest sense, are each the necessary supplement of the other. If Chemistry furnish the Nomenclature, Mineralogy must supply the Physiography: if the Arrangement be founded on External Characters and the

Names be independent of Chemistry, the chemical composition of each species is an important scientific Truth respecting it.

3. The inquiry may actually occur, whether any subordination of groups in the mineral kingdom has really been made out. The ancient chemical arrangements, for instance, that of Haüy, though professing to distribute minerals according to Classes, Orders, Genera, and Species, were not only arbitrary, but inapplicable; for the first postulate of any method, that the species should have constant characters of unity and difference, was not satisfied. It was not ascertained that carbonate of lime was really distinguishable in all cases from carbonate of magnesia, or of iron; yet these species were placed in remote parts of the system: and the above carbonates made just so many species; although, if they were distinct from one another at all, they were further distinguishable into additional species. Even now, we may, perhaps, say that the limits of mineralogical species, and their laws of fixity, are 141 not yet clearly

seen. For the discoveries of the isomorphous relations and of the optical properties of minerals have rather shown us in what direction the object lies, than led us to the goal. It is clear that, in the mineral kingdom, the Definition of Species, borrowed from the laws of the continuation of the kind, which holds throughout the organic world, fails us altogether, and must be replaced by some other condition: nor is it difficult to see that the definite atomic relations of the chemical constituents, and the definite crystalline angle, must supply the principles of the *Specific* Identity for minerals. Yet the exact limits of definiteness in both these cases (when we admit the effect of mechanical mixtures, &c.) have not yet been completely disentangled. Moreover, any *arbitrary* assumption (as the allowance of a certain per-centage of mixture, or a certain small deviation in the angle,) is altogether contrary to the philosophy of the Natural System, and can lead to no stable views. It is only by laborious, extensive, and minute research, that we can hope to attain to any solid basis

of arrangement.

4. Still, though there are many doubts respecting mineralogical species, a large number of such species are so far fixed that they may be supposed capable of being united under the higher divisions of a system with approximate truth. Of these higher divisions, those which have been termed *Orders* appear to tend to something like a fixed chemical character. Thus the *Haloids* of Naumann, and mostly those of Mohs, are combinations of an oxide with an acid, and thus resemble Salts, whence their name. The Silicides contain most of Mohs's *Spaths*: and the Orders *Pyrites*, *Glance*, and *Blende*, are common to Naumann and Mohs; being established by the latter on a difference of external character, which difference is, indeed, very manifest; and being included by the former in one chemical *Class*, *Sulphurides*. The distinctions of *Hydrous* and *Anhydrous*, *Metallic* and *Unmetallic*, are, of course, chemical distinctions, but occur as the differences of Orders in Naumann's mixed system. 142

We may observe that some French writers, following Haüy's last edition, use, instead of *metallic* and *unmetallic*, *autopside metallic* and *heteropside metallic*; meaning by this phraseology to acknowledge the discovery that earths, etc., *are* metallic, though they do not *appear* to be so, while metals both are and appear metallic. But this seems to be a refinement not only useless but absurd. For what is gained by adding the word *metallic*, which is common to all, and therefore makes no distinction? If certain metals are distinguished by their *appearing* to be metals, this appearance is a reason for giving them the peculiar name, *metals*. Nothing is gained by first bringing earths and metals together, and then immediately separating them again by new and inconvenient names. No proposition can be expressed better by calling *earths*, *heteropside metallic substances*, and therefore such nomenclature is to be rejected.

Granting, then, that the Orders of the best recent mineralogical systems approximate to natural groups, we are led to ask whether

the same can be said of the Genera of the Natural History systems, such as those of Mohs and Breithaupt. And here I must confess that I see no principle in these Genera; I have failed to apprehend the conceptions by the application of which they have been constructed: I shall therefore not pass any further judgment upon them. The subordination of Mineralogical Species to Orders is a manifest gain to science: in the interposition of Genera I see nothing but a source of confusion.

5. In Mineralogy, as in other branches of natural history, a reformed arrangement ought to give rise to a reformed Nomenclature; and for this, there is more occasion at present in Mineralogy than there was in Botany at the worst period, at least as far as the extent of the subject allows. The characters of minerals are much more dimly and unfrequently developed than those of plants; hence arbitrary chemical arrangements, which could not lead to any natural groups, and therefore not to any good names, prevailed till recently; and this

state of things produced an anarchy 143 in which every man did what seemed right in his own eyes,—proposed species without any ascertained distinction, and without a thought of subordination, and gave them arbitrary names; and thus with only about two or three hundred known species, we have thousands upon thousands of names, of anomalous form and uncertain application.

Mohs has attempted to reform the Nomenclature of the subject in a mode consistent with his attempt to reform the System. In doing this, he has fatally transgressed a rule always insisted upon by the legislators of Botany, of altering usual names as little as possible; and his names are both so novel and so cumbrous, that they appear to have little chance of permanent currency. They are, perhaps, more unwieldy than they need to be, by referring, as we have said, to three of the steps of his classification, the Species, Genus, and Order. We may, however, assert confidently, from the whole analogy of

natural history, that no good names can be found which do not refer to at least *two* terms of the arrangement. This rule has been practically adopted to a great extent by Naumann, who gives to most of his Haloids the name *Spar*, as Calc spar, Iron spar, &c.; to all his Oxides the terminal word *Erz* (*Ore*); and to the species of the orders *Kies* (*Pyrites*), *Glance*, and *Blende*, these names. It has also been theoretically assented to by Beudant, who proposes that we should say *silicate stilbite*, *silicate chabasie*; *carbonate calcaire*, *carbonate witherite*; *sulphate couperose*, &c. One great difficulty in this case would arise from the great number of *silicides*; it is not likely that any names would obtain a footing which tacked the term *silicide* to another word for each of these species. The artifice which I have proposed, in order to obviate this difficulty, is that we should make the names of the silicides, and those alone, end in *ite* or *lite*, which a large proportion of them do already.

By this and a few similar contrivances, we

might, I conceive, without any inconvenient change, introduce into Mineralogy a systematic nomenclature. 144

6. I shall now proceed to make a few remarks on a work on Mineralogy more recent than those which I have above noticed, and written with express reference to such difficulties as I have been discussing. I allude to the treatise of M. Necker, *Le Règne Mineral ramené aux Methods d'Histoire Naturelle*[44], which also contains various dissertations on the Philosophy of Classification in general, and its application to Mineralogy in particular.

[44] Paris, 1835.

M. Necker remarks very justly, that Mineralogy, as it has hitherto been treated, differs from all other branches of Natural History in this:—that while it is invested with all the forms of the sciences of classification,—Classes, Divisions, Genera, and the like,—the properties of those bodies to which the mineralogical student's attention is directed have no bearing

whatever on the classification. A person, he remarks[45], might be perfectly well acquainted with all the characters of minerals which Werner or Haüy examined so carefully, and might yet be quite unable to assign to any mineral its place in the divisions of their methods. There is[46] a complete separation between the study of mineralogical characters and the recognition of the name and systematic place of a mineral. Those who know *mineralogy* well, may know *minerals* ill, or hardly at all; the systematist may be in such knowledge vastly inferior to the mineral-dealer or the miner. In this respect there is a complete contrast between this science and other classificatory sciences.

[45] *Règne Mineral*, p. 3.

[46] *Ib.* p. 8.

Again, in the best-known systems of Mineralogy, (as those of Werner and Haüy,). the bodies which are grouped together as belonging to the same division, have not, as they have in other classificatory sciences,

any resemblance. The different members of the larger classes are united by the common possession of some abstract property,—as, that they all contain iron. This is a property to which no common circumstance in the bodies themselves corresponds. What is there common to the minerals named oxidulous iron, sulphuret 145 of iron, carbonate of iron, sulphate of iron, except that they all contain iron? And when we have classed these bodies together, what general assertion can we make concerning them, except that which is the ground of our classification, that they contain iron? They have nothing in common with iron or with each other in any other way.

Again, as these classes have no general properties, all the properties are particular to the species; and the descriptions of these necessarily become both tediously long, and inconveniently insulated.

7. These inconveniences arise from making Chemical Composition the basis of Mineralogical Classification without giving

Chemical Analysis the first place among Mineral Properties. Shall we, then, correct this omission, so far as it has affected mineralogical systems? Shall we teach the student the chemical analysis of minerals, and then direct him to classify them according to the results of his analysis[47]?

[47] *Règne Mineral*, p. 18.

But why should we do this? To what purpose, or on what ground, do we arrange the results of chemical analysis according to the forms and subordination of natural history? Is not Chemistry a science distinct from Natural History? Are not the sciences opposed? Is not natural history confined to organic bodies? Can mere chemical elements and their combinations be, with any propriety or consistency, arranged into Species, Genera, and Families? What is the principle on which genera and species depend? Do not Species imply Individuals? What is an Individual in the case of a chemical substance?

8. We thus find some of the widest and

deepest questions of the philosophy of classification brought under our consideration when we would provide a method for the classification of minerals. The answers to these questions are given by M. Necker; and I shall state some of his opinions; taking the liberty of adding such remarks as are suggested by referring the subject 146 to those principles which have already been established in this work.

M. Necker asserts[48] that the distinctions of different Sciences depend, not on the objects they consider, but on the different and independent points of view on which they proceed. Each science has its logic, that is, its mode of applying the general rules of human reason to its own special case. It has been said by some[49], that in minerals, natural history and chemistry contemplate common objects, and thus form a single science. But do chemistry and natural history consider minerals in the same point of view?

[48] *Règne Mineral*, p. 23.

[49] *Ib.* p. 27.

The answer is, that they do not. Physics and Chemistry consider the properties of bodies in an abstract manner; as, their composition, their elements, their mutual actions, with the laws of these; their forces, as attraction, affinity; all which objects are abstract ideas. In these cases we have nothing to do with bodies themselves, but as the vehicles of the powers and properties which we contemplate.

Natural History, on the other hand, has to do with natural bodies: their properties are not considered abstractedly, but only as characters. If the properties are abstracted, it is but for a moment. Natural history has to describe and class bodies as they are. All which cannot be perceived by the senses, belongs not to its domain, as molecules, atoms, elements.

Natural history[50] may have recourse to physics or chemistry in order to recognize those properties of bodies which serve as characters; but natural history is not, on

that account, physics or chemistry. Classification is the essential business of the natural historian[51], to which task chemistry and physics are only instrumental, and the further account of properties only complementary.

[50] *Ib.* p. 37.

[51] *Ib.* p. 41.

It has been said, in support of the doctrine that chemistry and mineralogy are identical, that chemistry does not neglect external characters. 'The chemist in 147 describing sulphur, mentions its colour, taste, odour, hardness, transparence, crystalline form, specific gravity; how does he then differ from the mineralogist?' But to this it is replied, that these notices of the external characters of this or any substance are introduced in chemistry merely as convenient marks of recognition; whereas they are essential in mineralogy. If we had taken the account given of several substances instead of one, we should have seen that the chemist and the naturalist

consider them in ways altogether different. The chemist will make it his business to discover the mutual action of the substances; he will combine them, form new products, determine the proportions of the elements. The mineralogist will divide the substances into groups according to their properties, and then subdivide these groups, till he refers each substance to its species. Exterior and physical characters are merely accessory and subordinate for the chemist; chemistry is merely instrumental for the mineralogist.

This view agrees with that to which we have been led by our previous reasonings; and may, according to our principles, be expressed briefly by saying, that the Idea which Chemistry has to apply is the Idea of Elementary Composition, while Natural History applies the Idea of Graduated Resemblances, and thus performs the task of classification.

9. The question occurs[52], whether Natural History can be applied to Inorganic

Substances? And the answer to this question is, that it can be applied, if there are such things as inorganic individuals, since the resemblances and differences with which natural history has to do are the resemblances and differences of individuals.

[52] *Règne Mineral*, p. 46.

What is an Individual? It certainly is not that which is so simple that it cannot be divided. Individual animals are composed of many parts. But if we examine, we shall find that our Idea of an Individual is, that it is a whole composed of parts, which 148 are not similar to the whole, and have not an independent existence, while the whole has an independent existence and a definite form[53].

[53] *Règne Mineral*, p. 52.

What then is the Mineralogical Individual? At first, while minerals were studied for their use, the most precious of the substances which they contained was looked upon as the characteristic of the mineral. The smallest trace of silver made a

mineral an *ore of silver*. Thus forms and properties were disregarded, and *substance* was considered as identical with *mineral*. And hence[54] Daubenton refused to recognize *species* in the mineral kingdom, because he recognized no individuals. He proposed to call *sorts* what we call species. In this way of considering minerals, there are no individuals.

[54] *Ib.* p. 54.

10. But still this is not satisfactory: for if we take a well-formed and distinct crystal, this clearly *is* an individual[55].

[55] *Ib.* p. 56.

It may be objected, that the crystal is divisible (according to the theory of crystallography) into smaller solids; that these small solids are really the simple objects; and that actual crystals are formed by combinations of these molecules according to certain laws.

But, as we have already said, an individual is such, not because it cannot be

divided, but because it cannot be divided into parts similar to the whole. As to the division of the form into its component *laws*, this is an abstract proceeding, foreign to natural history[56]. Therefore there is so far nothing to prevent a crystal from being an individual.

[56] *Ib.* p. 58.

11. We cannot (M. Necker goes on to remark) consider the *Integrant Molecules* as individuals. These are useful abstractions, but abstractions only, which we must not deal with as real objects. Haüy himself warns us[57] that his doctrine of increments is a purely abstract conception, and that nature, in fact, follows a different process. Accordingly, Weiss and Mohs express laws identical with those of Haüy, without even 149 speaking of molecules; and Wollaston and Davy have deemed it probable that the molecules are not polyhedrons, but spheres or spheroids. Such mere creations of the mind can never be treated as individuals. If the maxim of natural history,—that the Species is a collection of Individuals—be

applied so as to make those individuals mere abstractions; or if, instead of Individuals, we take such an abstraction as Substance or Matter, the course of natural history is altogether violated. And yet this errour has hitherto generally prevailed; and mineralogists have classified, not things, but abstract ideas[58].

[57] *Ib.* p. 61.

[58] *Règne Mineral*, p. 67.

12. But it may be said[59], will not the small solids obtained by Cleavage better answer the idea of individuals? To this it is replied, that these small solids have no independent existence. They are only the result of a mode of division. They are never found separate and independent. The secondary forms which they compose are determined by various circumstances (the nature of the solution, &c.); and the cleavage which produces these small solids is only one result among many, from the crystalline forces[60].

[59] *Ib.* p. 69.

[60] *Ib.* p. 71.

Thus neither Integrant Molecules, nor Solids obtained by Cleavage, can be such mineralogical Individuals as the spirit of natural history requires. Hence it appears that we must take the real Crystals for Individuals[61].

[61] *Ib.* p. 73.

13. We must, however, reject crystals (generally large ones) which are obviously formed of several smaller ones of a similar form (as occurs so often in quartz and calc spar). We must also distinguish cases in which a large regular form is composed of smaller but different regular forms (as octahedrons of fluor spar made up of cubes). Here the small component forms are the individuals. Also we must notice the cases[62] in which we have a natural crystal, similar to the primary form. Here the face will show whether 150 the body is a result obtained by cleavage or a natural individual.

[62] *Ib.* p. 75.

14. It will be objected[63], that the crystalline form ought not to be made the dominant character in mineralogy, since it rarely occurs perfect. To this it is replied, that even if the application of the principle be difficult, still it has been shown to be the only true principle, and therefore we have no alternative. But further[64], it is not true that amorphous substances are more numerous than crystals. In Leonhard's *Manual of Oryctognosy*, there are 377 mineral substances. Of these, 281 have a crystalline structure, and 96 only have not been found in a regular form.

[63] *Règne Mineral*, p. 79.

[64] *Ib.* p. 82.

Again, the 281 crystalline forms have each its varieties, some of which are crystalline, and some are not so. Now the crystalline varieties amount to 1453, and the uncrystalline to 186 only. Thus mineralogy, according to the view of it here presented, has a sufficiently wide field[65].

[65] *Ib.* p. 84.

15. It will be objected[66], that according to this mode of proceeding, we must reject from our system all non-crystalline minerals. But we reply, that if the mass be composed of crystals, the size of the crystals makes no difference. Now lamellar and other compact masses are very generally groups of crystals in various positions. Individuals mutilated and mixed together are not the less individuals; and therefore such masses may be treated as objects of natural history.

[66] *Ib.* p. 86.

If we cannot refer all rocks to crystalline species, those which elude our method may appear as an appendix, corresponding to those plants which botanists call *genera incertæ sedis*[67].

[67] *Ib.* p. 91.

But these genera and species will often be afterwards removed into the crystalline part of the system, by being identified with crystalline species. Thus *pyrope*, &c., have been referred to *garnet*, and *basalt*, 151

wacke, &c., to compound rocks. Thus veins of *Dolerite*, visibly composed of two or three elements, pass to an apparently simple state by becoming fine-grained[68].

[68] *Règne Mineral*, p. 93.

16. Finally[69], we have to ask, are artificial crystals to enter into our classification? M. Necker answers, No; because they are the result of art, like mules, mestizos, hybrids, and the like.

[69] *Ib.* p. 95.

17. Upon these opinions, we may observe, that they appear to be, in the main, consistent with the soundest philosophy. That each natural crystal is an individual, is a doctrine which is the only basis of Mineralogy as a Natural Historical Science; yet the imperfections and confused unions of crystals make this principle difficult to apply. Perhaps it may be expressed in a more precise manner by referring to the crystalline forces, and to the axes by which their operation is determined, rather than to the external form. *That* portion of a mineral

substance is a mineralogical *individual* which is determined by crystalline forces acting to the *same axes*. In this way we avoid the difficulty arising from the absence of faces, and enable ourselves to use either cleavage, or optical properties, or any others, as indications of the identity of the individual. The individual extends so far as the polar forces extend by which crystalline form is determined, whether or not those forces produce their full effect, namely, a perfectly circumscribed polyhedron.

18. There is only one material point on which our principles lead us to differ from M. Necker;—the propriety of including *artificial crystals* in our mineralogical classification. To exclude them, as he does, is a conclusion so entirely at variance with the whole course of his own reasonings, that it is difficult to conceive that he would persist in his conclusion, if his attention were drawn to the question more steadily. For, as he justly says[70], each science has its appropriate domain, determined by its peculiar point of view. Now artificial and

natural crystals are considered in the same point of view, (namely, with reference to 152 crystalline, physical, and optical properties, as subservient to classification,) and ought, therefore, to belong to the same science. Again, he says[71], that Chemistry would reject as useless all notice of the physical properties and external characters of substances, if a *special science* were to take charge of the description and classification of these products. But such a special science must be Mineralogy; for we cannot well make one science of the classification of natural, and another of that of artificial substances: or if we do, the two sciences will be identical in method and principles, and will extend over each other's boundaries, so that it will be neither useful nor possible to distinguish them. Again, M. Necker's own reasonings on the selection of the individual in mineralogy are supported by well chosen examples[72]; but these examples are taken from artificial salts; as, for instance, common salt crystallizing in different mixtures. Again, the analogy of mules and mestizos, as products of art, with

chemical compounds, is not just. Chemical compounds correspond rather to natural species, propagated by man under the most natural circumstances, in order that he may study the laws of their production[73].

[70] *Ib.* p. 23.

[71] *Règne Mineral*, p. 36.

[72] *Ib.* p. 71.

[73] We may remark that M. Necker, in his own arrangement of minerals, inserts among his species Iron and Lead, which do not occur Native.

19. But the decisive argument against the separation of natural and artificial crystals in our schemes of classification is, that we *cannot* make such a separation. Substances which were long known only as the products of the laboratory, are often discovered, after a time, in natural deposits. Are the crystals which are found in a forgotten retort or solution to be considered as belonging to a different science from those which occur in a deserted mine? And are the crystals which are produced where man has turned a stream of water or air out

of its course, to be separated from natural crystals, when the composition, growth, and properties, are exactly the same in both? And again: How many natural crystals can we already produce by 153 synthesis! How many more may we hope to imitate hereafter! M. Necker himself states[74], that Mitscherlich found, in the scoriæ of the mines of Sweden and Germany, artificial minerals having the same composition and the same crystalline form with natural minerals: as silicates of iron, lime, and magnesia, agreeing with Peridot; bisilicate of iron, lime, and magnesia, agreeing with Pyroxene; red oxide of copper; oxide of zinc; protoxide of iron (*fer oxydulé*); sulphurets of iron, zinc, lead; arseniuret of nickel; black mica. These were accidental results of fusion. But M. Berthier, by bringing together the elements in proper quantities, has succeeded in composing similar minerals, and has thus obtained artificial silicates, with the same forms and the same characters as natural silicates. Other chemists (M. Haldat, M. Becquerel) have, in like manner, obtained, by artificial

processes, other crystals, known previously as occurring naturally. How are these crystals, thus identical with natural minerals, to be removed out of the domain of mineralogy, and transferred to a science which shall classify artificial crystals only? If this be done, the mineralogist will not be able to classify any specimen till he has human testimony whether it was found naturally occurring or produced by chemical art. Or is the other alternative to be taken, and are these crystals to be given up to mineralogy because they occur naturally also? But what can be more unphilosophical than to refer to separate sciences the results of chemical processes closely allied, and all but identical? The chemist constructs bisilicates, and these are classified by the mineralogist: but if he constructs a trisilicate, it belongs to another science. All these intolerable incongruities are avoided by acknowledging that artificial, as well as natural, crystals belong to the domain of mineralogy. It is, in fact, the *name* only of *Mineralogy* which appears to discover any inconsistency in this mode of proceeding.

Mineralogy is the 154 representative of a science which has a wider office than mineralogists first contemplated; but which must exist, in order that the body of science may be complete. There must, as we have already said, be a Science, the object of which is to classify bodies by their physical characters, in order that we may have some means of asserting chemical truths concerning bodies; some language in which we may express the propositions which chemical analysis discovers. And this Science will have its object prescribed, not by any accidental or arbitrary difference of the story belonging to each specimen;—not by knowing whether the specimen was found in the mine or in the laboratory; produced by attempting to imitate nature, or to do violence to her:—but will have its course determined by its own character. The range and boundaries of this Science will be regulated by the Ideas with which it deals. Like all other sciences, it must extend to everything to which its principles apply. The limits of the province which it includes are fixed by the consideration that it must

be a connected whole. No previous definition, no historical accident, no casual phrase, can at all stand in the way of philosophical consistency;—can make this Science exclude what that includes, or oblige it to admit what that rejects. And thus, whatever we call our Science;—whether we term it External Chemistry, Mineralogy, the Natural History of Inorganic Bodies;—since it can be nothing but the Science of the Classification of Inorganic Bodies of definite forms and properties, it must classify all such bodies, whether or not they be minerals, and whether or not they be natural.

[74] *Règne Mineral*, p. 151.

20. In the application of the principles of classification to minerals, the question occurs, What are to be considered as mineral *Species*? By Species we are to understand, according to the usage of other parts of natural history, the lowest step of our subordinate divisions;—the most limited of the groups which have definite distinctions. What definite distinctions of

groups of objects of any kind really occur in nature, is to be learnt from an examination of nature: and the 155 result of our inquiries will be some general principle which connects the members of each group, and distinguishes the members of groups which, though contiguous, are different. In the classification of organized bodies, the rule which thus presides over the formation of Species is the principle of *reproduction*. Those animals and those plants are of the same Species which are produced from a common stock, or which resemble each other as much as the progeny of a common stock. Accordingly in practice, if any questions arise whether two varieties of form in organic things be of the same or different species, it is settled by reference to the fact of reproduction; and when it is ascertained that the two forms come within the habitual and regular limits of a common circle of reproduction, they are held to be of the same species. Now in crystals, this principle of reproduction disappears altogether, and the basis of the formation of species must be sought elsewhere. We must

have some other principle to replace the reproduction which belongs only to organic life. This principle will be, we may expect, one which secures the permanence and regularity of mineral forms, as the reproductive power does of animal and vegetable. Such a principle is the *Power of Crystallization*. The forces of which solidity, cohesion, and crystallization are the result, are those which give to minerals their permanent existence and their physical properties; and ever since the discovery of the distinctions of Crystalline Forms and Crystalline Systems, it is certain that this force distinguishes groups of crystals in the most precise and definite manner. The rhombohedral carbonates of lime and of iron, for instance, are distinguished exactly by the angles of their rhombohedrons. And if, in the case of any proposed crystal, we should doubt to which kind the specimen belongs, the measurement of the angles of cleavage would at once decide the question. The principle of Crystallization therefore appears, from analogy, to be exactly fitted to take the place of the principle of organic

Generation. The forces which make the individual permanent and its properties definite, here stand in the place of the forces 156 which preserve the race, while individuals are generated and die.

21. According to this view, the different Modifications of the *same* crystalline form would be *Varieties* only of the same Species. All the various solids, for example, which are produced by the different laws of derivation of rhombohedral carbonate of lime, would fall within the same Species. And this appears to be required by the general analogy of Natural History. For these differences of form, produced by the laws of crystalline derivation, are not *definite*. The faces which are added to one form in order to produce another, may be of any size, small or large, and thus the crystal which represents one modification passes by insensible degrees to another. The forms of calc spar, which we call *dog-tooth spar*, *cannon spar*, *nail-head spar*, and the like, appear at first, no doubt, distinct enough; but so do the races of dogs. And we find, in

the mineral as in the animal, that the distinction is obliterated by taking such intermediate steps as really occur. And if a *fragment* of any of these crystals is given us, we can determine that it is rhombohedral carbonate of lime; but it is not possible, in general, to determine to which of the kinds of crystals it has belonged.

22. Notwithstanding these considerations, M. Necker has taken for his basis of mineral species[75] the *Secondary* Modifications, and not the Primary Forms. Thus *cubical galena*, *octahedral galena*, and *triform galena*, are, with him, three *species* of crystals.

[75] *Règne Mineral*, p. 396.

On this I have to observe, as I have already done, that on this principle we have no *definite* distinction of species; for these forms may and do pass into each other: among cubo-octahedrons of galena occur cubes and octahedrons, as one face or another vanishes, and the transition is insensible. We shall, on this principle, find almost always three or four species in the

same tuft of crystals; for almost every individual in such assemblages may exhibit a different combination of 157 secondary faces. Again, in cases where the secondary laws are numerous, it would be impracticable to enumerate all their combinations, and impossible therefore to give a list of species. Accordingly M. Necker[76] gives seventy-one Species of *spath calcaire*, and then says, 'Nous n'avons pas énumeré la dixième partie des espèces connues de ce genre, qui se montent à plus de huit cents.' Again, in many substances, of which few crystals are found, every new specimen would be a new species; if indeed it were perfect enough to be referred to a species at all. But from a specimen without perfect external form, however perfect in crystalline character, although everything else might be known,—angles, optical properties, physical properties, and chemical constitution,—the species could not be determined. Thus M. Necker says[77] of the micas, 'Quant aux espèces propre à chaque genre, la lacune sera presque complète; car jusqu'ici les cristaux entiers

de Mica et de Talc n’ont pas été fort communs.’

[76] *Règne Mineral*, p. 364.

[77] *Ib.* ii. 414.

These inconveniences arise from neglecting the leading rule of natural history, that the *predominant principle* of the existence of an object must determine the Species; whether this principle be Reproduction operating for Development, or Crystallization operating for Permanence of form. We may add to the above statement of inconveniences this;—that if M. Necker’s view of mineralogical species be adopted, the distinction of Species is vague and indefinite, while that of Genera is perfectly precise and rigorous;—an aspect of the system entirely at variance with other parts of Natural History; for in all these the Species is a more definite group than the Genus.

This result follows, as has already been said, from M. Necker’s wish to have individuals marked by external form. If,

instead of this, we are contented to take for an individual that portion of a mass, of whatever form, which is connected by the continuous influence of the same crystalline forces, by whatever incidents these forces may be manifested, (as cleavage, 158 physical and optical properties, and the like,) our mode of proceeding avoids all the above inconveniences, applies alike to the most perfect and most imperfect specimens, and gives a result agreeable to the general analogy of natural history, and the rules of its methods[78].

[78] I will not again enter into the subject of Nomenclature; but I may remark that M. Necker has adopted (i. 415) the Nomenclature of Beudant, latinizing the names, and thus converting each into a single word. He has also introduced, besides the names of Genera, names of Families taken from the *typical* Genus. Thus the Family of *Carbonidiens* contains the following genera: *Calcispathum*, *Magnesispathum*, *Dolomispathum*, *Ferrispathum*, *&c.*, *Malachita*, *Azuria*, *Gaylusacia*.

I now quit the subject of mere Resemblance, and proceed to treat of that

natural affinity which Natural Systems of Classification for organic bodies must involve.

CHAPTER IV.

Of the Idea of Natural Affinity.

1. In the Second Chapter of this Book it was shown that although the Classificatory Sciences proceed ostensibly upon the Idea of Resemblance as their main foundation, they necessarily take for granted in the course of their progress a further Idea of Natural Affinity. This appeared[79] by a general consideration of the nature of Science, by the recognition of natural species and genera, even in Artificial Systems of Classification[80], and by the attempts of botanists to form a Natural System. It further appeared that among the processes by which endeavours have been made to frame a Natural System, some, as the method of *Blind Trial* and the method of *General Comparison*, have been altogether

unsuccessful, being founded only upon a collection of resemblances, casual in the one case and arbitrary in the other. In neither of these processes is there employed any general principle by which we may be definitely directed as to what resemblances we should employ, or by which the result at which we arrive may be verified and confirmed. Our object in the present chapter is to show that the Idea of Natural Affinity supplies us with a principle which may answer such purposes.

[79] Art. 5.

[80] Art. 7.

I shall first consider the Idea of Affinity as exemplified in organized beings. In doing this, we may appear to take for granted Ideas which have not yet come under our discussion, as the Ideas of Organization, and Vital Function; but it will be found that the principle to which we are led is independent of these additional Ideas. 160

2. We have already seen that the attempts to discover the divisions which result from

this Natural Affinity have led to the consideration of the *Subordination of Characters*. It is easy to see that some organs are more essential than others to the existence of an organized being; the organs of nutrition, for example, more essential than those of locomotion. But at the same time it is clear that any *arbitrary* assumption of a certain scale of relative values of different kinds of characters will lead only to an Artificial System. This will happen, if, for example, we begin by declaring the nutritive to be superior in importance to the reproductive functions. It is clear that this relation of importance of organs and functions must be collected by the study of the organized beings; and cannot be determined *à priori*, without depriving us of all right to expect a general accordance between our system and the arrangement of nature. We see, therefore, that our notion of Natural Affinity involves in it this consequence;—that it is not to be made out by an arbitrary subordination of characters.

3. The functions and actions of living

things which we separate from each other in our consideration, cannot be severed in nature. Each function is essential; Life implies a collection of movements, and ceases when any of these movements is stopped. A change in the organization subservient to one set of functions may lead necessarily to a change in the organization belonging to others. We can often see this necessary connexion; and from a comparison of the forms of organized beings,—from the way in which their structure changes in passing from one class to another, we are led to the conviction that there is some general principle which connects and graduates all such changes. When the circulatory system changes, the nervous system changes also: when the mode of locomotion changes, the respiration is also modified.

4. These corresponding changes may be considered as ways in which the living thing is fitted to its mode of life; as marks of *adaptation to a purpose*; or, as it has been otherwise expressed, as results of the 161

conditions of existence. But at the present moment, we put forward these correspondencies in a different light. We adduce them as illustrations of what we mean by Affinity, and what we consider as the tendency of a Natural Classification. It has sometimes been asserted that if we were to classify any of the departments of organized nature by means of one function, and then by means of another, the two classifications, if each strictly consistent with itself, would be consistent with each other. Such an assertion is perhaps more than we are entitled to make with confidence; but it shows very well what is meant by Affinity. The disposition to believe such a general identity of all partial natural classifications, shows how readily we fix upon the notion of Affinity, as a general result of the causes which determine the forms of living things. When these causes or principles, of whatever nature they are conceived to be, vary so as to modify one part of the organization of the being, they also modify another: and thus the groups which exhibit this variation of the

fundamental principles of form, are the same, whether the manifestation of the change be sought in one part or in another of the organized structure. The groups thus formed are related by Affinity; and in proportion as we find the evidence of more functions and more organs to the propriety of our groups, we are more and more satisfied that they are Natural Classes. It appears, then, that our Idea of Affinity involves the conviction of the *Coincidence of natural arrangements formed on different functions*; and this, rather than the principle of the Subordination of some characters to others, is the true ground of the natural method of Classification.

5. For example, Cuvier, after speaking of the Subordination of Characters as the guide which he intends to follow in his arrangement of animals, interprets this principle in such a manner[81] as to make it agree nearly with the one just stated: ‘In pursuance of what has been said on methods in general, we now require to 162 know what characters in animals are the

most influential, and therefore those which must be made the grounds of the primary divisions.' 'These,' he says, 'it is clear must be those which are taken from the animal functions;—sensation and motion:'—But how does he confirm this? Not by showing that the animal functions are independent of, or predominant over, the vegetative, but by observing that they follow the same gradations. 'Observation,' he continues, 'confirms this view, by showing that the degrees of development and complication of the animal functions agree with those of the vegetative. The heart and the organs of the circulation are a sort of center for the vegetative functions, as the brain and the trunk of the nervous system are for the animal functions. Now we see these two systems descend in the scale, and disappear the one with the other. In the lowest animals, when there are no longer any distinct nerves, there are also no longer distinct fibres, and the organs of digestion are simply hollowed out in the homogeneous mass of the body. The muscular system disappears even before the

nervous, in insects; but in general the distribution of the medullary masses corresponds to that of the muscular instruments; a spinal cord, on which knots or ganglions represent so many brains, corresponds to a body divided into numerous rings and supported on pairs of members placed at different points of the length, and so on.

[81] *Règne Animal*, p. 55.

'This *correspondence* of the general forms which result from the arrangement of the motive organs, from the distribution of the nervous masses, and from the energy of the circulatory system, must therefore form the ground of the first great sections by which we divide the animal kingdom.'

6. Decandolle takes the same view. There must be, he says, *an equilibrium* of the different functions[82]. And he exemplifies this by the case of the distinction of monocotyledonous and dicotyledonous plants, which being at first established by means of the organs of 163 reproduction,

was afterwards found to coincide with the distinction of endogenous and exogenous, which depends on the process of nutrition. 'Thus,' he adds, '*the natural classes founded on one of the great functions of the vegetable are necessarily the same as those which are founded upon the other function*; and I find here a very useful criterion to ascertain whether a class is natural: namely, in order to announce that it is so, it must be arrived at by the two roads which vegetable organization presents. Thus I affirm,' he says, 'that the division of monocotyledons from dicotyledons, and the distinction of Gramineæ from Cyperaceæ, are real, because in these cases, I arrive at the same result by the reproductive and the nutritive organs; while the distinction of monopetalous and polypetalous, of Rhodoraceæ and Ericineæ, appears to me artificial, because I can arrive at it only by the reproductive organs.'

[82] *Theor. Elem.* p. 79.

Thus the Correspondence of the indications of different functions is the

criterion of Natural Classes; and this correspondence may be considered as one of the best and most characteristic marks of the fundamental Idea of Affinity. And the Maxim by which all Systems professing to be natural must be tested is this:—that the *arrangement obtained from one set of characters coincides with the arrangement obtained from another set.*

This Idea of Affinity, as a natural connexion among various species, of which connexion all particular resemblances are indications, has principally influenced the attempts at classifying the animal kingdom. The reason why the classification in this branch of Natural History has been more easy and certain than that of the vegetable world is, as Decandolle says[83], that besides the functions of nutrition and reproduction, which animals have in common with plants, they have also in addition the function of sensation; and thus have a new means of verification and concordance. But we may add, as a further reason, that the functions of 164 animals are necessarily much more

obvious and intelligible to us than those of vegetables, from their clear resemblance to the operations which take place in our own bodies, to which our attention has necessarily been strongly directed.

[83] *Theor. Elem.* p. 80.

7. The question here offers itself, whether this Idea of Natural Affinity is applicable to inorganic as well as to organic bodies;—whether there be Natural Affinities among Minerals. And to this we are now enabled to reply by considering whether or not the principle just stated is applicable in such cases. And the conclusion to which our principle leads us is,—that there are such Natural Affinities among Minerals, since there are different sets of characters which may be taken, (and have by different writers been taken,) as the basis of classification. The hardness, specific gravity, colour, lustre, crystallization, and other *external* characters, as they are termed, form one body of properties according to which minerals may be classified; as has in fact been done by Mohs, Breithaupt, and others.

The *chemical* constitution of the substances, on the other hand, may be made the principle of their arrangement, as was done by Haüy, and more recently, and on a different scheme, by Berzelius. Which of these is the true and natural classification? To this we answer, that *each* of these arrangements is true and natural, then, and then only, when it coincides with the other. An arrangement by external characters which gives us classes possessing a common chemical character;—a chemical order which brings together like and separates unlike minerals;—such classifications have the evidence of truth in their agreement with one another. Every classification of minerals which does not aim at and tend to such a result, is so far merely arbitrary; and cannot be subservient to the expression of general chemical and mineralogical truths, which is the proper purpose of such a classification.

8. In the History of Mineralogy I have related the advances which have been made among mineralogists and chemists in

modern times towards a System 165 possessing this character of truth. I have there described the mixed systems of Werner and Haüy;—the attempt made by Mohs to form a pure Natural History system;—the first and second attempt of Berzelius to form a pure chemical system; and the failure of both these attempts. But the distinct separation of the two elements of which science requires the coincidence threw a very useful light upon the subject; and the succeeding mixed systems, such as that of Naumann, approached much nearer to the true conditions of the problem than any of the preceding ones had done. Thus, as I have stated, several of Naumann's groups have both a common chemical character and great external resemblances. Such are his *Anhydrous Unmetallic Haloids*—his *Anhydrous Metallic Haloids*—*Hydrous Metallic Haloids*—*Oxides* of metals—*Pyrites*—*Glances*—*Blendes*. The existence of such groups shows that we may hope ultimately to obtain a classification of minerals which shall be both chemically significant, and agreeable to the methods of Natural History:

although when we consider how very imperfect as yet our knowledge of the chemical composition of minerals is, we can hardly flatter ourselves that we shall arrive at such a result very soon.

We have thus seen that in Mineralogy, as well as in the sciences which treat of organized bodies, we may apply the Idea of Natural Affinity; of which the fundamental maxim is, that *arrangements obtained from different sets of characters must coincide*.

Since the notion of Affinity is thus applicable to inorganic as well as to organic bodies, it is plain that it is not a mere modification of the Idea of Organization or Function, although it may in some of its aspects appear to approach near to these other Ideas. But these Ideas, or others which are the foundation of them, necessarily enter in a very prominent and fundamental manner into all the other parts of Natural History. To the consideration of these, therefore, we shall now proceed.

BOOK IX.

THE

PHILOSOPHY

OF

BIOLOGY.

LA vie est donc un TOURBILLON plus ou moins rapide, plus ou moins compliqué, dont la direction est constante, et qui entraine toujours des molecules de mêmes sorts, mais où les molecules individuelles entrent et d'où elles sortent continuellement, de manière que la *Forme* du corps vivant lui est plus essentielle que sa *Matière*.

Tant que ce mouvement subsiste, le corps où il s'exerce est *vivant*; *il vit*. Lorsque le mouvement s'arrête sans retour, le corps *meurt*.

CUVIER, *Règne Animal*, s. 12.

I REMEMBER, upon asking our famous Harvey, what induced him to think of a circulation of the blood, he said, that observing the valves in the veins of many parts of the body, so placed as to give a free passage to the blood towards the heart, but to oppose the passage of the venal blood the contrary way, he imagined that so provident a cause as nature had not thus placed so many valves without design; and as no design seemed more probable than that the blood could not well, because of the interposing valves, be sent by the veins to the limbs, it should be sent through the arteries and return through the veins when valves did not oppose its course that way.

BOYLE *On the Final Causes of Natural Things*. On the Proposition: *'Tis often allowable for a naturalist, from the manifest and apposite uses of the parts of animal bodies, to collect some of the particular ends for which the Creator designed them: and in some cases we may, from the known nature and structure of the parts, draw particular conjectures about the particular offices of them.*

BOOK IX.

THE PHILOSOPHY OF BIOLOGY.

CHAPTER I.

Analogy of Biology With Other Sciences.

1. In the History of the Sciences, after treating of the Sciences of Classification, we proceeded to what are there termed the Organical Sciences, including in this term Physiology and Comparative Anatomy. A peculiar feature in this group of sciences is that they involve the notion of *living* things. The notion of *Life*, however vague and obscure it may be in men's minds, is

apprehended as a peculiar Idea, not resolvable into any other Ideas, such, for instance, as Matter and Motion. The separation between living creatures and inert matter, between organized and unorganized beings, is conceived as a positive and insurmountable barrier. The two classes of objects are considered as of a distinct kind, produced and preserved by different forces. Whether the Idea of Life is really thus original and fundamental, and whether, if so, it be one Idea only, or involve several, it must be the province of true philosophy to determine. What we shall here offer may be considered as an attempt to contribute something to the determination of these questions; but we shall perhaps be able to make it appear that science is at present only in the course of its progress towards a complete solution of such problems.

Since the main feature of those sciences of which we have now to examine the philosophy is, that they 170 involve the Idea of Life, it would be desirable to have them

designated by a name expressive of that circumstance. The word *Physiology*, by which they have most commonly been described, means *the Science of Nature*; and though it would be easy to explain, by reference to history, the train of thought by which the word was latterly restricted to *Living Nature*, it is plain that the name is, etymologically speaking, loose and improper. The term *Biology*, which means exactly what we wish to express, *the Science of Life*, has often been used, and has of late become not uncommon among good winters. I shall therefore venture to employ it, in most cases, rather than the word *Physiology*.

2. As I have already intimated, one main inquiry belonging to the Philosophy of Biology, is concerning the Fundamental Idea or Ideas which the science involves. If we look back at the course and the results of our disquisitions respecting other sciences in this work, and assume, as we may philosophically do, that there will be some general analogy between those sciences and

this, in their development and progress, we shall be enabled to anticipate in some measure the nature of the view which we shall now have to take. We have seen that in other subjects the Fundamental Ideas on which science depended, and the Conceptions derived from these, were at first vague, obscure, and confused;—that by gradual steps, by a constant union of thought and observation, these conceptions become more and more clear, more and more definite;—and that when they approached complete distinctness and precision, there were made great positive discoveries into which these conceptions entered; and thus the new precision of thought was fixed and perpetuated in some conspicuous and lasting truths. Thus we have seen how the first confused mechanical conceptions (Force, and the like,) were, from time to time, growing clearer, down to the epoch of Newton;—how true conceptions of Genera and of wider classes, gradually unfolded themselves among the botanists of the sixteenth and seventeenth centuries;—how

the idea of Substance became steady enough to govern the 171 theories of chemists only at the epoch of Lavoisier;—how the Idea of Polarity, although often used by physicists and chemists, is even now somewhat vague and indistinct in the minds of the greater part of speculators. In like manner we may expect to find that the Idea of Life, if indeed *that* be the governing Idea of the Science which treats of Living Things, will be found to have been gradually approaching towards a distinct and definite form among the physiologists of all ages up to the present day. And if this be the case, it may not be considered superfluous, with reference to so interesting a subject, if we employ some space in tracing historically the steps of this progress;—the changes by which the originally loose notion of Life, or of Vital Powers, became more nearly an Idea suited to the purposes of science.

3. But we may safely carry this analogy between Biology and other sciences somewhat further. We have seen, in other sciences, that while men in their

speculations were thus tending towards a certain peculiar Idea, but before they as yet saw clearly that it was peculiar and independent, they naturally and inevitably clothed their speculations in conceptions borrowed from some other extraneous idea. And the unsatisfactoriness of all such attempts, and the necessary consequence of this, a constant alteration and succession of such inappropriate hypotheses, were indications and aids of the progress which was going on towards a more genuine form of the science. For instance, we have seen that in chemistry, so long as men refused to recognize a peculiar and distinct kind of power in the *Affinity* which binds together the elements of bodies, they framed to themselves a series of hypotheses, each constructed according to the prevalent ideas of the time, by which they tried to represent the relation of the compound to the ingredients:—first, supposing that the elements bestowed upon the whole qualities *resembling* their own:—then giving up this supposition, and imagining that the properties of the body depended upon the

shape of the component particles;—then, as their view expanded, assuming that it was 172 not the shape, but the mechanical *forces* of the particles which gave the body its attributes;—and finally acquiescing in, or rather reluctantly admitting, the idea of *Affinity*, conceived as a peculiar power, different not only from material contact, but from any mechanical or dynamical attraction.

Now we cannot but think it very natural, if we find that the history of Biology offers a series of occurrences of the same nature. The notions of Life in general, or of any Vital Functions or Vital Forces in particular, are obviously very loose and vague as they exist in the minds of most men. The discrepancies and controversies respecting the definitions of all such terms, which are found in all works on physiology, afford us abundant evidence that these notions are not, at least not generally, apprehended with complete clearness and steadiness. We shall therefore find approaches and advances, intermediate steps, gradually

leading up to the greatest degree of distinctness which has yet been attained. And in those stages of imperfect apprehension in which the notions of Life and of Vital Powers are still too loose and unformed to be applied independently, we may expect to find them supported and embodied by means of hypotheses borrowed from other subjects, and thus, made so distinct and substantial as to supply at least a temporary possibility of scientific reasoning upon the laws of life.

4. For example, if we suppose that men begin to speculate upon the properties of living things, not acknowledging a peculiar Vital Power, but making use successively of the knowledge supplied by the study of other subjects, we may easily imagine a series of hypotheses along which they would pass.

They would probably, first, in this as in other sciences, have their thoughts occupied by vague and *mystical* notions in which material and spiritual agency, natural and

supernatural events, were mixed together without discrimination, and without any clear notion at all. But as they acquired a more genuine perception of the nature of knowledge, they would naturally try to explain vital motions and processes by means of 173 such forces as they had learnt the existence of from other sciences. They might first have a *mechanical* hypothesis, in which the mechanical *Forces* of the solids and fluids which compose organized bodies should be referred to, as the most important influences in the process of life. They might then attend to the actions which the fluids exercise in virtue of their *Affinity*, and might thus form a *chemical* theory. When they had proved the insufficience of these hypotheses, borrowed from the powers which matter exhibits in other cases, they might think themselves authorized to assume some peculiar power or agency, still material, and thus they would have the hypothesis of a *Vital Fluid*. And if they were driven to reject this, they might think that there was no resource but to assume an immaterial principle of life, and thus they

would arrive at the doctrine of an *Animal Soul*.

Now, through the cycle of hypotheses which we have thus supposed, physiology has actually passed. The conclusions to which the most philosophical minds have been led by a survey of this progress is, that by the failure of all these theories, men have exhausted this path of inquiry, and shown that scientific truth is to be sought in some other manner. But before I proceed further to illustrate this result, it will be proper, as I have already stated, to exhibit historically the various hypotheses which I have described. In doing this I shall principally follow the *History of Medicine* of Sprengel. It is only by taking for my guide a physiologist of acknowledged science and judgment, that I can hope, on such a subject, to avoid errours of detail. I proceed now to give in succession an account of the Mystical, the Iatrochemical, the Iatromathematical, and the Vital-Fluid Schools; and finally of the Psychical School, who hold the Vital Powers to be derived from the Soul (*Psyche*).

CHAPTER II.

Successive Biological Hypotheses.

Sect. I.—*The Mystical School.*

IN order to abbreviate as much as can conveniently be done the historical view which I have now to take, I shall altogether pass over the physiological speculations of the ancients, and begin my survey with the general revival of science in modern times.

We need not dwell long on the fantastical and unsubstantial doctrines concerning physiology which prevailed in the sixteenth century, and which flowed in a great measure from the fertile but ill-regulated imaginations of the cultivators of Alchemy and Magic. One of the prominent doctors of this school is the celebrated Paracelsus, whose doctrines contained a combination of

biblical interpretations, visionary religious notions, fanciful analogies, and bold experiments in practical medicine. The opinion of a close but mystical resemblance of parts between the universe and the human body,—the *Macrocosm* and the *Microcosm*,—as these two things, thus compared, were termed, had probably come down from the Neoplatonists; it was adopted by the Paracelsists[1], and connected with various astrological dreams and cabbalistic riddles. A succession of later Paracelsists[2], Rosicrucians, and other fanatics of the same kind, continued into the seventeenth century. Upon their notions was founded the pretension of curing wounds by a sympathetic powder, which Sir Kenelm Digby, among others, asserted; while animal magnetism, and the transfer of diseases from one person to another[3], were maintained by others of this 175 school. They held, too, the doctrines of *astral bodies* corresponding to each terrestrial body; and of the *signatures* of plants, that is, certain features in their external form by which their virtues might be known. How little

advantage or progress real physiology could derive from speculations of this kind may be seen from this, that their tendency was to obliterate the distinction between living and lifeless things: according to Paracelsus, all things are alive, eat, drink, and excrete; even minerals and fluids[4]. According to him and his school, besides material and immaterial beings, there are *elementary Spirits* which hold an intermediate place, *Sylvans*, *Nymphs*, *Gnomes*, *Salamanders*, &c. by whose agency various processes of enchantment may be achieved, and things apparently supernatural explained. Thus this spiritualist scheme dealt with a world of its own by means of fanciful inventions and mystical visions, instead of making any step in the study of nature.

[1] Spr. iii. 456.

[2] *Ib.* iv. 270.

[3] *Ib.* iv. 276.

[4] Spr. iii. 458. Parac. *De Vita Rerum Naturalium*, p. 889.

Perhaps, however, one of the most

fantastical of the inventions of Paracelsus may be considered as indicating a perception of a peculiar character in the vital powers. According to him, the business of digestion is performed by a certain demon whom he calls *Archæus*, who has his abode in the stomach, and who, by means of his alchemical processes, separates the nutritive from the harmful part of our food, and makes it capable of assimilation[5]. This fanciful notion was afterwards adopted and expanded by Van Helmont[6]. According to him the stomach and spleen are both under the direction of this Master-spirit, and these two organs form a sort of *Duumvirate* in the body.

[5] *Ib.* iii. 468.

[6] *Ib.* iv. 302.

But though we may see in such writers occasional gleams of physiological thought, the absence of definite physical relations in the speculations thus promulgated was necessarily intolerable to men of sound understanding and scientific tendencies.

Such men naturally took hold of that part of the phenomena of life which could be most distinctly conceived, and 176 which could be apparently explained by means of the sciences then cultivated; and this was the part which appeared to be reducible to chemical conceptions and doctrines. It will readily be supposed that the processes of chemistry have a considerable bearing upon physiological processes, and might, till their range was limited by a sound investigation, be supposed to have still more than they really had; and thus a Physiology was formed which depended mainly upon Chemistry, and the school which held this doctrine has been called the *Iatrochemical* School.

SECT. II.—*The Iatrochemical School.*

That all physical properties, and therefore chemical relations, have a material influence on physiological results, was already recognized, though dimly, in the Galenic doctrine of the ‘four elementary qualities.’ But at the time of Paracelsus,

chemical action was more distinctly than before separated from other kinds of physical action; and therefore a physiological doctrine, founded upon chemistry, and freed from the extravagance and mysticism of the Paracelsists, was a very promising path of speculation. Andrew Libavius[7] of Halle, in Saxony, Physician and Teacher in the Gymnasium at Koberg, is pointed out by Sprengel as the person who began to cultivate chemistry, as distinct from the theosophic fantasies of his predecessors; and Angelus Sala of Vienna[8], as his successor. The latter has the laudable distinction of having rejected the prevalent conceits about a potable gold, a universal medicine, and the like[9]. In Germany already at the beginning of the seventeenth century a peculiar chair of *Chymiatria* was already created at Marpurg: and many in various places pursued the same studies, till, in the middle of the seventeenth century, we come to Lemery[10], the principal reformer of pharmaceutical chemistry. But we are not here so much concerned 177 with the practical as with the theoretical parts of

Iatrochemistry; and hence we pass on to Sylvius[11] and his system.

[7] Spr. iii. 550.

[8] *Ib.* iv. 281.

[9] *Ib.* iv. 283.

[10] *Ib.* iv. 291.

[11] Spr. iv. 336.

The opinion that chemistry had an important bearing upon physiology did not, however, begin with Sylvius. Paracelsus, among his extravagant absurdities, did some service to medicine by drawing attention to this important truth. He used[12] chemical principles for the explanation of particular diseases: most or all diseases according to him, arise from the effervescence of salts, from the combustion of sulphur, or from the coagulation of mercury. His medicines were chemical preparations; and it was[13] an undeniable advantage of the Paracelsian doctrine that chemistry thus became indispensable to the physician. We still retain a remnant of the chemical

nomenclature of Paracelsus in the term *tartar*, denoting the stony concretion which forms on the teeth[14]. According to him there is a certain substance, the basis of all diseases which arise from a thickening of the juices and a collection of earthy matter; and this substance he calls *Tartarus*, because 'it burns like the fire of hell.' Helmont, the successor of Paracelsus in many absurdities, also followed him in the attempt to give a chemical account, however loose and wild, of the functions of the human body; and is by Sprengel considered, with all his extravagancies, as a meritorious and important discoverer. The notion of the fermentation of fluids[15], and of the aërial product thence resulting, to which he gave the name of *Gas*, forms an important part of his doctrines; and of the six digestions which he assumes, the *first* prepares an acid, which is neutralized by the gall when it reaches the duodenum, and this constitutes the *second* digestion.

[12] *Ib.* iii. 472.

[13] *Ib.* iii. 482.

[14] *Ib.* iii. 475.

[15] Vol. v. 315.

I have already, in the History of Chemistry[16], stated, that the doctrine of the opposition of acid and alkali, the great step which theoretical chemistry owes to Sylvius, was first brought into view as a physiological 178 tenet, although we had then to trace its consequences in another science. The explanation of all the functions of the animal system, both healthy and morbid, by means of this and other chemical doctrines, and the prescription of methods of cure founded upon such explanations, form the scheme of the *iatrochemical* school; a school which almost engrossed the favour of European physicians during the greater part of the seventeenth century.

[16] *Hist. Ind. Sc.* b. xiii. c. 2.

Sylvius taught medicine at Leyden, from the year 1658, with so much success, that Boerhaave alone surpassed him[17]. His notions, although he piqued himself on their

originality, were manifestly suggested in no small degree (as all such supposed novelties are) by the speculations of his predecessors, and the spirit of the times. Like Helmont[18], he considers digestion as consisting in a fermentation; but he states it more definitely as the effervescence of an acid, supplied by the saliva and the pancreatic juice, with the alkali of the gall. By various other hypothetical processes, all of a chemical nature, the blood becomes a collection of various juices, which are the subjects of the speculations of the iatrochemists, to the entire neglect of the solid parts of the body. Diseases were accounted for by a supposed prevalence of one or the other of the acrid principles, the acid or the alkaline: and Sylvius[19] was bold enough to found upon these hypotheses practical methods of cure, which were in the highest degree mischievous.

[17] Spr. iv. 336.

[18] *Ib.* 338.

[19] *Ib.* iv. 345.

The Sylvian doctrine was often combined with some of the notions of the Cartesian system of philosophy; but this mixture I shall not notice, since my present object is to trace the history of a mere chemical physiology as one of the unsuccessful attempts at a philosophy of life. With various modifications, this doctrine was diffused over Europe. It gave rise to several controversies, which turned upon the questions of the novelty of the doctrine, and the use of chemical remedies to which it pointed, as well as upon its 179 theoretical truth. We need not dwell long upon these controversies, although they were carried on with no small vehemence in their time. Thus the school of Paris opposed all innovation, remained true to the Galenic dogmatism, and declared itself earnestly against all combination of chemistry with medicine; and even against the chemical preparation of medicaments. Guy Patin, a celebrated and learned professor of that day, declares[20] that the chemists are no better than forgers, and ought to be punished as such. The use of antimonial

medicines was a main point of dispute between the iatrochemists and their opponents; Patin maintained that more men had been destroyed by antimony than by the thirty years' war of Germany; and endeavoured to substantiate this assertion by collecting all such cases in his *Martyrologium Antimonii*. It must have been a severe blow to Patin when[21] in 1666, the Doctors of the Faculty of Paris, assembled by command of the parliament, declared, by a majority of ninety-two voices, that the use of antimonial medicines was allowable and laudable, and when all attempts to set aside this decision failed.

[20] Spr. 349.

[21] *Ib.* iv. 350.

Florentius Schuyl of Leyden sought to recommend the iatrochemical doctrines, by maintaining that they were to be found in the Hippocratic writings; nor was it difficult to give a chemical interpretation of the humoral pathology of the ancients. The Italian[22] physicians also, for the most part,

took this line, and attempted to show the agreement of the principles of the ancient school of medicine with the new chemical notions. This, indeed, is the usual manner in which the diffusion of new theoretical ideas becomes universal.

[22] *Ib.* 368.

The progress of the chemical school of medicine in England[23] requires our more especial notice. Willis was the most celebrated champion of this sect. He assumed, but with modifications of his own, the three Paracelsian principles, Salt, Sulphur, and Mercury; considered digestion as the effect of an acid, and 180 explained other parts of the animal economy by distillation, fermentation, and the like. All diseases arise from the want of the requisite *ferment*; and the physician, he says[24], may be compared to a vintner, since both the one and the other have to take care that the necessary fermentations go on, that no foreign matter mixes itself with the wine of life, to interrupt or derange those operations. In the middle of the seventeenth

century, medicine had reached a point in which the life of the animal body was considered as merely a chemical process; the wish to explain everything on known principles left no recognized difference between organized and unorganized bodies, and diseases were treated according to this delusive notion. The condition of chemistry itself during this period, though not one of brilliant progress, was sufficiently stable and flourishing to give a plausibility to any speculation which was founded on chemical principles; and the real influence of these principles in the animal frame could not be denied.

[23] *Ib.* 353.

[24] Spr. 354.

The iatrochemists were at first resisted, as we have seen, by the adherents of the ancient schools; they were attacked on various grounds, and finally deposed from them ascendancy by another sect, which we have to speak of, as the iatromathematical, or mechanical school. This sect was no less

unsatisfactory and erroneous in its positive doctrines than the chemists had been; for the animal frame is no more a mere machine than a mere laboratory: but it promoted the cause of truth, by detecting and exposing the insufficient explanations and unproved assertions of the reigning theory.

Boyle was one of the persons who first raised doubts against the current chemical doctrines of his time, as we have elsewhere noted; but his objections had no peculiar physiological import. Herman Coming[25], the most learned physician of his time, a contemporary with Sylvius, took a view more pertinent to our present object; for he not only rejected the alchemical 181 and hermetical medicines, but taught expressly that chemistry, in its then existing condition, was better fitted to be of use in the practice of pharmacy, than in the theories of physiology and pathology. He made the important assertion, also, that chemical principles do not pre-exist *as such* in the animal body; and that there are

higher powers which operate in the organic world, and which do not depend on the form and mixture of matter.

[25] *Ib.* iv. 361.

Attempts were made to prove the acid and alkaline nature of the fluids of the human body by means of experiments, as by John Viridet of Geneva[26], and by Raimond Vieussens[27], the latter of whom maintained that he had extracted an acid from the blood, and detected a ferment in the stomach. In opposition to him, Hecquet, a disciple of the iatromathematical school, endeavoured to prove that digestion was performed, not by means of fermentation, but by trituration. Hecquet's own opinions cannot be defended; but his objections to the chemical doctrines, and his assertion of the difference of chemical and organical processes, are evidences of just thought[28].

[26] Spr. iv. 329.

[27] *Ib.* 350, (1715).

[28] *Ib.* 401.

The most important opponents of the iatrochemical school were Pitcairn in England, Bohn and Hoffman in Germany, and Boerhaave in Holland. These eminent physicians, about the end of the seventeenth century, argued on the same grounds of observation, that digestion is not fermentation, and that the Sylvian accounts of the origin of diseases by means of acid and alkali are false. The arguments and authority of these and other persons finally gained an ascendancy in the medical world, and soon after this period we may consider the reign of the chemical school of physiology as past. In fact, the attempts to prove its assertions experimentally were of the feeblest kind, and it had no solid basis on which it could rest, so as to resist the shock of the next hypothesis which the progress of the physical sciences might impel against it. We may, therefore, now consider the opinion of the mere 182 chemical nature of the vital processes as disproved, and we proceed next to notice the history of another unsuccessful essay to reduce vital actions to known actions of

another kind.

SECT. III.—*The Iatromathematical School.*

In the first Section of this chapter, we enumerated the biological hypotheses which at first present themselves, as the mystical, the mechanical, the chemical. We might have expected that they should occur to men's minds in the order thus stated: and in fact they did so; for the physiology of the ancient materialists, as Democritus and Lucretius, is mechanical so far as it is at all distinct in its views, and thus the mechanical preceded the chemical doctrine. But in modern times, the fluid or chemical physiology was developed before the solid or mechanical: of which the reason appears to have been this;—that Mechanics and Chemistry began to assume a scientific character about the same time; and that of the two, Chemistry not only appeared at first sight more applicable to the functions of the body, because all the more rapid changes appear to be connected with modifications of the fluids of the animal

system, but also, by its wider range of facts and more indefinite principles, afforded a better temporary refuge for the mind when perplexed by the difficulties and mysteries which spring out of the speculations concerning life. But if Chemistry was thus at first a more inviting field for the physiologist, Mechanics soon became more attractive in virtue of the splendid results obtained by the schools of Galileo and Newton. And when the insufficiency of chemical physiology was discovered by trial, as we have seen it was, the hope naturally arose, that the mechanical principles which had explained so many of the phenomena of the external universe might also be found, applicable to the smaller world of material life;—that the *microcosm* as well as the *macrocosm* might have its mechanical principles. From this hope sprung the 183 Iatromathematical School, or school of Mechanical Physiologists.

We may, however, divide this school into two parts, the Italian, and the Cartesio-

Newtonian sect. The former employed themselves in calculating and analysing a number of the properties of the animal frame which are undoubtedly mechanical; the latter, somewhat intoxicated by the supposed triumphs of the corpuscular philosophy, endeavoured to extend these to physiology, and for this purpose introduced into the subject many arbitrary and baseless hypotheses. I will very briefly mention some of the writers of both these sects.

The main points to which the Italian or genuine Mechanical Physiologists attended, were the application of mechanical calculations to the force of the muscles, and of hydraulical reasonings to the motion of the fluids of the animal system. The success with which Galileo and his disciples had pursued these branches of mechanical philosophy, and the ascendancy which they had obtained, first in Italy, and then in other lands, made such speculations highly interesting. Borelli may be considered as the first great name in his line, and his book, *De Motu Animalium*, (*Opus Posthumum*, Romæ,

1680,) is even now a very instructive treatise on the forces and action of the bones and muscles. This, certainly one of the most valuable portions of mechanical physiology, has not even yet been so fully developed as it deserves, although John Bernoulli[29] and his son Daniel[30] applied to it the resources of analysis, and Pemberton[31] in England, pursued the same subject. Other of these mechanico-physiological problems consisted in referring the pressure of the blood and of the breath to hydrostatical principles. In this manner Borelli was led to assert that the muscles of the heart exert a force of 180,000 pounds[32]. But a little later, Keill reduced this force 184 to a few ounces[33]. Keill and others attempted to determine, on similar principles, the velocity of the blood; we need not notice the controversies which thus arose, since there is not involved in them any peculiar physiological principle.

[29] *De Motu Musculorum.*

[30] *Act. Acad. Petrop.*

[31] *Course of Physiology*, 1773.

[32] Spr. iv. 110.

[33] Spr. iv. 443.

The peculiar character of the iatromathematical school, as an attempt at physiological theory, is more manifest in its other section, which we have called the Cartesio-Newtonian. The Cartesian system pretended to account for the appearances and changes of bodies by means of the size, figure, and motion of their minute particles. And though this system in its progress towards the intellectual empire of Europe was suddenly overturned by the rise of the Newtonian philosophy, these corpuscular doctrines rather gained than lost by the revolution; for the Newtonian philosophy enlarged the powers of the corpuscular hypothesis, by adding the effects of the attractive and repulsive forces of particles to those of their form and motion. By this means, although Newton's discoveries did not in fact augment the probability of the corpuscular hypothesis, they so far increased its plausibility, that this hypothesis found favour both with Newton

himself and his contemporaries, no less than it had done with the Cartesians.

The attempt to apply this corpuscular hypothesis to physiology was made by Des Cartes himself. The general character of such speculations may easily be guessed[34]. The secretions are effected by the organs operating after the manner of sieves. Bound particles pass through cylindrical tubes, pyramidal ones through triangular pores, cubical particles through square apertures, and thus different kinds of matter are separated. Similar speculations were pursued by other mathematicians: the various diameter of the vessels[35], their curvatures, folds, and angles, were made subjects of calculation. Bellini, Donzellini, Gulielmini, in Italy; Perrault, Dodart, in France; Cole, Keill, Jurin, in England, were the principal cultivators of such studies. 185 In the earlier part of the eighteenth century, physiological theorists considered it as almost self-evident that their science required them to reason concerning the size and shape of the particles of the fluids, the

diameter and form of the invisible vessels. Such was, for instance, the opinion of Cheyne[36], who held that acute fevers arise from the obstruction of the glands, which occasions a more vehement motion of the blood. Mead, the physician of the King, and the friend of Newton, in like manner explained the effects of poisons by hypotheses concerning the form of their particles[37], as we have already seen in speaking of chemistry.

[34] *Ib.* 329.

[35] *Ib.* 432.

[36] Spr. iv. 223.

[37] *Mechanical Account of Poisons*, 1702.

It is not necessary for us to dwell longer on this subject, or to point out the total insufficiency of the mere mechanical physiology. The iatrochemists had neglected the effect of the solids of the living frame; the iatromathematicians attended only to these[38]. And even these were considered only as canals, as cords, as levers, as lifeless machines. These reasoners never looked for

any powers of a higher order than the cohesion, the resistance, the gravity, the attraction, which operate in inert matter. If the chemical school assimilated the physician to a vintner or brewer, the mechanical physiologists made him an hydraulic engineer; and, in fact, several of the iatromathematicians were at the same time teachers of engineering and of medicine.

[38] Spr. iv. 419.

Several of the reasoners of this school combined chemical with their mechanical principles; but it would throw no additional light upon the subject to give any account of these, and I shall therefore go on to speak of the next form of the attempt to explain the processes of life.

SECT. IV.—*The Vital-Fluid School.*

I speak here, not of that opinion which assumes some kind of fluid or ether as the means of 186 communication along the *nerves* in particular, but of the hypothesis

that *all* the peculiar functions of *life* depend upon some subtile ethereal substance diffused through the frame;—not of a *Nervous* Fluid, but of a *Vital* Fluid. Again, I distinguish this opinion from the doctrine of an *immaterial* vital power or principle, an Animal Soul, which will be the subject of the next Section: nor is this distinction insignificant; for a material element, however subtile, however much spiritualized, must still act everywhere according to the same laws; whereas we do not conceive an immaterial spirit or soul to be subject to this necessity.

The iatromathematical school could explain to their own satisfaction how motions, once begun, were transferred and modified; but in many organs of the living frame there seemed to be a power of beginning motion, which is beyond all mere mechanical action. This led to the assumption of a Principle of a higher kind, though still material. Such a Principle was asserted by Frederick Hoffmann, who was born at Halle, in 1660[39], and became

Professor of Medicine at the newly established University there in 1694. According to him[40], the reason of the greater activity of organized bodies lies in the influence of a material substance of extreme subtilty, volatility, and energy. This is, he holds, no other than the Ether, which, diffused through all nature, produces in plants the bud, the secretion and motion of the juices, and is separated from the blood and lodged in the brain of animals[41]. From this, acting through the nerves, must be derived all the actions of the organs in the animal frame; for when the influence of the nerve upon the muscle ceases, muscular motion ceases also.

[39] Spr. v. 254.

[40] *Ib.* v. 257.

[41] *De Differentiâ Organismi et Mechanismi*, pp. 48, 67.

The mode of operation of this vital fluid was, however, by no means steadily apprehended by Hoffmann and his followers. Its operations are so far

mechanical[42] that all effects are reduced to motion, yet they 187 cannot be explained according to known mechanical laws. At one time the effects are said to take place according to laws of a Higher Mechanics which are still to be discovered[43]. At another time, in complete contradiction of the general spirit of the system, metaphysical conceptions are introduced: each particle of the vital fluid is said to have a determined *idea* of the whole mechanism and organism[44], and according to this, it forms the body and preserves it by its motion. By means of this fluid the soul operates upon the body, and the instincts and the passions have their source in this material sensitive soul. This attribution of ideas to the particles of the fluid is less unaccountable when we recollect that something of the same kind is admitted into Leibnitz's system, whose Monads have also ideas.

[42] Spr. v. 262, 3.

[43] Hoffmann, *Opp.* Vol. v. p. 123.

[44] *De Diff. Organ. et Mechan.* p. 81.

Notwithstanding its inconsistencies, Hoffmann's system was received with very general favour both in Germany and in the rest of Europe; the more so, inasmuch as it fell in very well with the philosophy both of Leibnitz and of Newton. The Newtonians were generally inclined to identify the Vital Fluid with the Ether, of which their master was so strongly disposed to assume the existence: and indeed he himself suggested this identification.

When the discoveries made respecting Electricity in the course of the eighteenth century had familiarized men with the notion of a pervading subtile agent, invisible, intangible, yet producing very powerful effects in every part of nature, physiologists also caught at the suggestion of such an agent, and tried, by borrowing or imitating it, to aid the imperfection of their notions of the vital powers. The Vital Principle[45] was imagined to be a substance of the same kind, by some to be the same substance, with the Electric Fluid. By its agency all these processes in organized

bodies were accounted for which cannot be 188 explained by mechanical or chemical laws, as the secretion of various matters (tears, milk, bile, &c.) from an homogeneous fluid, the blood; the production of animal heat, digestion, and the like. According to John Hunter, this attenuated substance pervaded the blood itself, as well as the solid organic frame; and the changes which take place in the blood which has flowed out of the veins into a basin are explained by saying that it is, for a time, till this vital fluid evaporates, truly alive.

[45] Prichard, *On the Doctrine of a Vital Principle*, p. 12.

The notion of a Vital Fluid appears also to be favourably looked upon by Cuvier; although with him this doctrine is mainly put forwards in the form of a Nervous Fluid. Yet in the following passage he extends the operation of such an agent to all the vital functions[46]: 'We have only to suppose that all the medullary and nervous parts produce the Nervous Agent, and that they alone

conduct it; that is, that it can only be transmitted by them, and that it is changed or consumed by their actions. Then everything appears simple. A detached portion of muscle preserves for some time its irritability, on account of the portion of nerve which always adheres to it. The sensibility and the irritability reciprocally exhaust each other by their exercise, because they change or consume the same agent. All the interior motions of digestion, secretion, excretion, participate in this exhaustion, or may produce it. All local excitation of the nerves brings thither more blood by augmenting the irritability of the arteries, and the afflux of blood augments the real sensibility by augmenting the production of the nervous agent. Hence the pleasures of titillations, the pains of inflammation. The particular sensations increase in the same manner and by the same causes; and the imagination exercises, (still by means of the nerves,) upon the internal fibres of the arteries or other parts, and through them on the sensations, an action analogous to that of the will upon the

voluntary motions. As each exterior sense is exclusively disposed 189 to admit the substances which it is to perceive, so each interior organ, secretory or other, is also more excitable by some one agent than by another: and hence arises what has been called the *proper sensibility* or *proper life of the organs*; and the influence of specifics which, introduced into the general circulation, affect only certain parts. In fine, if the nervous agent cannot become sensible to us, the reason is that all sensation requires that this agent should be altered in some way or other; and it cannot alter itself.

[46] *Hist. Sc. Nat. depuis* 1789, i. 214.

'Such is the summary idea which we may at present form of the mutual and general working of the vital powers in animals.'

Against the doctrine of a Vital Fluid as one uniform material agent pervading the organic frame, an argument has been stated which points out extremely well the philosophical objection to such an hypothesis[47]. If the Vital Principle be the

same in all parts of the body, how does it happen, it is asked, that the secretions are so *different*? How do the particles in the blood, separated from their old compounds and united into new ones, under the same influence, give origin to all the different fluids which are produced by the glands? The liver secretes bile, the lacrymal gland, tears, and so on. Is the Vital Principle different in all these organs? To assert this, is to multiply nominal principles without limit, and without any advance in the explanation of facts. Is the Vital Principle the same, but its operation modified by the structure of the organ? We have then two unknown causes, the Vital Principle and the Organic Structure, to account, for the effect. By such a multiplication of hypotheses nothing is gained. We may as well say at once, that the structure of the organ, acting by laws yet unknown, is the cause of the peculiar secretion. It is as easy to imagine this structure acting to produce the whole effect, as it is to imagine it modifying the activity of another agent. Thus the hypothesis of the Vital Fluid in this form

explains nothing, and does not in any 190 way help onwards the progress of real biological knowledge.

[47] Prichard, *On a Vital Principle*, p. 98.

The hypothesis of an *immaterial* vital principle must now be considered.

SECT. V.—*The Psychical School.*

The doctrine of an Animal Soul as the principle which makes the operations of organic different from those of inorganic matter, is quite distinct from, and we may say independent of, the doctrine of the soul as the intelligent, moral, responsible part of man's nature. It is the former doctrine alone of which we have here to speak, and those who thus hold the existence of an immaterial agent as the cause of the phenomena of life, I term the *Psychical School.*

Such a view of the constitution of living things is very ancient. For instance, Aristotle's Treatise '*On the Soul*,' goes

entirely upon the supposition that the Soul is the cause of motion, and he arrives at the conclusion that there are different *parts* in the Soul; the *nutritive* or *vegetative*, the *sensitive*, and the *rational*[48].

[48] Aristotle. Περὶ Ψυχῆς, ii. 2.

But this doctrine is more instructive to us, when it appears as the antagonist of other opinions concerning the nature of life. In this form it comes before us as promulgated by Stahl, whom we have already noticed as one of the great discoverers in chemistry. Born in the same year as Hoffmann, and appointed at his suggestion professor at the same time in the same new university of Halle, he soon published a rival physiological theory. In a letter to Lucas Schröck, the president of the Academy of Naturalists, he describes the manner in which he was led to form a system for himself[49]. Educated in the tenets of Sylvius and Willis, according to which all diseases are derived from the acidity of the fluids, Stahl, when a young student, often wondered how these fluids, so liable to be

polluted and corrupted, are so wonderfully preserved through innumerable external influences, and seem to 191 be far less affected by these than by age, constitution, passion. No material cause could, he thought, produce such effects. No attention to mechanism or chemistry alone could teach us the true nature and laws of organization.

[49] Spr. v. 303.

So far as Stahl recognized the influence, in living bodies, of something beyond the range of mechanics and chemistry, there can be no doubt of the sound philosophy of his views; but when he proceeds to found a positive system of physiology, his tenets become more precarious. The basis of his theory is this[50]: the body has, as body, no power to move itself, and must always be put in motion by immaterial substances. All motion is a spiritual act[51]. The source of all activity in the organic body, from which its preservation, the permanency of its composition, and all its other functions proceed, is an immaterial being, which

Stahl calls the *Soul*; because, as he says, when the effects are so similar, he will not multiply powers without necessity. Of this principle, he says, as the Hippocratians said of Nature, that ‘it does without teaching what it ought to do[52],’ and does it ‘without consideration[53].’ These ancient tenets Stahl interprets in such a manner that even the involuntary motions proceed from the soul, though without reflection or clear consciousness. It is indeed evident, that there are many customary motions and sensations which are perfectly rational, yet not the objects of distinct consciousness: and thus instinctive motions, and those of which we are quite unconscious, may still be connected with reason. The questions which in this view offer themselves, as, how the soul passes from the mother to the child, he dismisses as unprofitable[54]. He considers nutrition and secretion as the work of the soul. The corpuscular theory and the doctrine of animal spirits 192 are, he rightly observes, mere hypotheses, which are arbitrary in their character, and only shift the difficulty. For, if the animal spirits are

not matter, how can they explain the action of an immaterial substance on the body; and if they are matter, how are they themselves acted on?

[50] Spr. v. 308

[51] *Ib.* v. 314.

[52] Stahl, περὶ φύσεως ἀπαίδευτου.

[53] οὐκ ἐκ διανοίης.

[54] This was of course an obvious problem. Harvey, *On Generation* Exercise 27, p. 148, teaches, 'That the egg is not the production of the womb, but of the soul.'

This doctrine of the action of the soul on the body, was accepted by many persons, especially by the iatromathematicians, who could not but feel the insufficiency of their system without some such supplement: such were Cheyne and Mead. In Germany, Stahl's disciples in physiology were for the most part inconsiderable persons[55]. Several Englishmen who speculated concerning the metaphysics as well as the physiology of Sensation and Motion, inclined to this psychical view, as Porterfield and Whytt.

Among the French, Boissier de Sauvages was the most zealous defender of the Stahlian system. Actions, he says[56], which belong to the preservation of life are determined by a moral not a mechanical necessity. They proceed from the soul, but cannot be controlled by it, as the starting from fear, or the trembling at danger. Unzer, a physician at Altona[57], was also a philosophical Stahlian[58].

[55] Spr. v. 339, &c.

[56] *Ib.* 358.

[57] A.D. 1799

[58] Spr. v. 360.

We need not dwell on the opposition which was offered to this theory, first by Hoffmann, and afterwards by Haller. The former of these had promulgated, as we have seen, the rival theory of a Nervous Fluid, the latter was the principal assertor of the doctrine of Irritability, an important theory on which we may afterwards have to touch. Haller's animosity against the Stahlian hypothesis is a remarkable feature

in one who is in general so tolerant in his judgment of opinions. His arguments are taken from the absence of the control of the will over the vital actions, from the want of consciousness accompanying these actions, from the uniformity of them in different conditions of the mind, and from the small sensibility of 193 the heart which is the source of the vital actions. These objections, and the too decided distinction which Haller made between voluntary and involuntary muscles, were very satisfactorily answered by Whytt and Platner. In particular it was urged that the instinctive actions of brutes are inexplicable by means of mechanism, and may be compared with the necessary vital actions of the human body. Neither kind are accidental, neither kind are voluntary, both are performed without reflection.

Without tracing further the progress of the Psychical Doctrine, I shall borrow a few reflections upon it from Sprengel[59]:—

‘When the opponents of the Stahlian

system repeat incessantly that the assumption of a psychical cause in corporeal effects is a metaphysical speculation which does not belong to medicine, they talk to no purpose. The states of the soul are objects of our internal experience, and interest the physician too nearly to allow him to neglect them. The innumerable unconscious efforts of the soul, the powerful and daily effects of the passions upon the body, too often put to confusion those who would expel into the region of metaphysics the dispositions of the mind. The connexion of our knowledge of the soul, as gathered from experience, with our knowledge of the human body, is far closer than the mechanical and chemical physiologists suspect.

[59] Spr. v. 383.

'The strongest objection against the psychical system, and one which has never been sufficiently answered by any of its advocates, is the universality of organic effects in the *vegetable* kingdom. The comparison of the physiology of plants with the physiology of animals puts the latter in

its true light. Without absolutely trifling with the word *soul*, we cannot possibly derive from a soul the organic operations of vegetables. But just as little can we, as some Stahlians have done, draw a sharp line between plants and animals, and ascribe the processes of the former to mere mechanism, while 194 we derive the operations of the latter from an intellectual principle. Not to mention that such a line is not possible, the rise of the sap and the alteration of the fluids of plants cannot be derived entirely from material causes as their highest origin.'

Thus, I may add, this psychical theory, however difficult to defend in its detail, does in its generalities express some important truths respecting the vital powers. It not only, like the last theory, gives unity to the living body, but it marks, more clearly than any other theory, the wide interval which separates mechanical and chemical from vital action, and fixes our attention upon the new powers which the consideration of life compels us to assume. It not only reminds us that these

powers are elevated above the known laws of the material world, but also that they are closely connected with the world of thought and feeling, of will and reason; and thus it carries us, in a manner in which none of the preceding theories have done, to a true conception of a living, conscious, sentient, active individual.

At the same time we cannot but allow that the life of plants and of the lower orders of animals shows us very clearly that, in order to arrive at any sound and consistent knowledge respecting life, we must form some conception of it from which all the higher attributes which the term ‘soul’ involves, are utterly and carefully excluded; and therefore we cannot but come to the conclusion that the psychical school are right mainly in this; that in ascribing the functions of life to a *soul*, they mark strongly and justly the impossibility of ascribing them to any known attributes of *body*.

CHAPTER III.

Attempts to Analyse the Idea of Life.

1. *Definitions of Life.*—We have seen in the preceding chapter that all attempts to obtain a distinct conception of the nature of Life in general have ended in failure, and produced nothing beyond a negative result. And the conjecture may now naturally occur, that the cause of this failure resides in an erroneous mode of propounding to ourselves the problem. Instead of contemplating Life as a single Idea, it may perhaps be proper to separate it into several component notions: instead of seeking for one cause of all vital operations, it may be well to look at the separate vital functions, and to seek their causes. When the view of this possibility opens upon us, how shall we endeavour to verify it, and to take

advantage of it?

Let us, as one obvious course, take some of the attempts which have been made to *define* Life, and let us see whether they appear to offer to us any analysis of the idea into component parts. Such definitions, when they proceed from men of philosophical minds, are the ultimate result of a long course of thought and observation; and by no means deserve to be slighted as arbitrary selections of conditions, or empty forms of words.

2. Life has been defined by Stahl[60], 'The condition by which a body resists a natural tendency to chemical changes, such as putrefaction.' In like manner, M. von Humboldt[61] defines living bodies to be 'those which, notwithstanding the constant operation 196 of causes tending to change their form, are hindered by a certain inward power from undergoing such change.' The first of these definitions amounts only to the assertion, that vital processes are not chemical; a negative result, which we may

accept as true, but which is, as we have seen, a barren truth. The second appears to be, in its import, identical with the first. An *inward* principle can only be understood as distinguished from known external powers, such as mechanical and chemical agencies. Or if, by an internal principle, we mean such a principle as that of which we are *conscious* within ourselves, we ascribe a soul to all living things: an hypothesis which we have seen is not more effective than the former in promoting the progress of biological science. Nearly the same criticism applies to such definitions as that of Kant: that 'Life is an internal faculty producing change, motion, and action.'

[60] Treviranus, *Biologie*, p. 19. Stahlii, *Theor. Med.* p. 254.

[61] *Aphorismen aus d. Chem. Physiol. der Pflanzen*, s. 1.

Other definitions refer us, not to some property residing in the whole of an organized mass, but to the connexion and relation of its parts. Thus M. von Humboldt[62] has given another definition of

a living body: that ‘it is a whole whose parts, arbitrarily separated, no longer resist chemical changes.’ But this additional assertion concerning the parts, adds nothing of any value to the definition of the whole. And in some of the lower kinds of plants and animals it is hardly true as a fact.

[62] *Versuche über die gereitzte Muskel und Nervenfüser*, b. ii. p. 433.

3. Another definition[63] places the character of Life in ‘motions serviceable to the body moved.’ To this it has been objected[64], that, on this definition, the earth and the planets are living bodies. Perhaps it would be more philosophical to object to the introduction of so loose a notion as that of a property being *serviceable* to a body. We might also add, that if we speak of all vital functions as *motions*, we make an assumption quite unauthorized, and probably false.

[63] Erhard, Röschlaub’s *Magazin der Heilkunde*, b. i. st. 1. p. 69.

[64] Treviranus, *Biologie*, p. 41.

197 Other definitions refer the idea of Life to the idea of Organization. ‘Life is the activity of matter according to laws of organization[65].’ We are then naturally led to ask, What is Organization? In reply to this is given us the Kantian definition of Organization, which I have already quoted elsewhere[66], ‘An organized product of nature is that in which all the parts are mutually ends and means[67].’ That this definition involves exact fundamental ideas, and is capable of being made the basis of sound knowledge, I shall hereafter endeavour to show. But I may observe that such a definition leads us somewhat further. If the parts of organized bodies are known to be means to certain ends, this must be known because they fulfil these ends, and produce certain effects by the operation of a certain cause or causes. The question then recurs, what is *the cause* which produces such effects as take place in organized or living bodies? and this is identical with the problem of which in the last chapter we traced the history, and related the failure of physiologists in all attempts at its solution.

[65] Schmid, *Physiologie*, b. ii. p. 274.

[66] *Hist. Ind. Sc.* b. xvii. c. viii. s. 2.

[67] Kant, *Urtheilskraft*, p. 296.

4. But what has been just said suggests to us that it may be an improvement to put our problem in another shape:—not to take for granted that the cause of all vital processes is one, but to suppose that there may be several separate causes at work in a living body. If this be so, life is no longer one kind of activity, but several. We have a number of operations which are somehow bound together, and life is the totality of all these: in short, life is not one Function, but a System of Functions.

5. We are thus brought very near to the celebrated definition of life given by Bichat[68]: 'Life is the sum of the functions by which death is resisted.' But upon the definition thus stated, we may venture to observe;—first, that the introduction of the notion of 198 *death* in order to define the notion of *life* appears to be unphilosophical. We may more naturally define death with

reference to life, as the cessation of life; or at least we may consider life and death as correlative and interdependent notions. Again, the word 'sum,' used in the way in which it here occurs, appears to be likely to convey an erroneous conception, as if the functions here spoken of were simply added to each other, and connected by co-existence. It is plain that our idea of life involves more than this: the functions are all clearly connected, and mutually depend on each other; nutrition, circulation, locomotion, reproduction,—each has its influence upon all the others. These functions not merely co-exist, but exist with many mutual relations and connexions; they are continued so as to form, not merely a *sum*, but a *system*. And thus we are led to modify Bichat's definition, and to say that *Life is the system of vital functions*.

[68] *Physiological Researches on Life and Death.*

6. But it will be objected that by such a definition we explain nothing: the notion of *vital functions*, it may be said, involves the

idea of *life*, and thus brings us round again to our starting-point. Or if not, at least it is as necessary to define Vital Functions as to define Life itself, so that we have made little progress in our task.

To this we reply, that if any one seeks, upon such subjects, some ultimate and independent definition from which he can, by mere reasoning, deduce a series of conclusions, he seeks that which cannot be found. In the Inductive Sciences, a Definition does not form the basis of reasoning, *but points out the course of investigation*. The definition must include words; and the meaning of these words must be sought in the progress and results of observations, as I have elsewhere said[69]. 'The meaning of words is to be sought in the progress of thought; the history of science is our dictionary; the steps of scientific induction are our definitions.' It will appear, I think, that it is more easy for us to form an idea of a separate Function of the 199 animal frame, as Nutrition or Reproduction, than to comprehend Life in general under

any single idea. And when we say that Life is a system of Vital Functions, we are of course directed to study these functions separately, and (as in all other subjects of scientific research) to endeavour to form of them such clear and definite ideas as may enable us to discover their laws.

[69] *Hist. Ind. Sc.* b. xiii. c. ix.

7. The view to which we are thus led, of the most promising mode of conducting the researches of Biology, is one which the greatest and most philosophical physiologists of modern times have adopted. Thus Cuvier considers this as the true office of physiology at present. 'It belongs to modern times,' he says, 'to form a just classification of the vital phenomena; the task of the present time is to analyse the forces which belong to each organic element, and upon the zeal and activity which are given to this task, depends, according to my judgment, the fortune of physiology[70].' This classification of the phenomena of life involves, of course, a distinction and arrangement of the vital

functions; and the investigation of the powers by which these functions are carried on, is a natural sequel to such a classification.

[70] *Hist. Sc. Nat. dep.* 1789, i. 218.

8. *Classification of Functions.*—Attempts to classify the Vital Functions of man were made at an early period, and have been repeated in great number up to modern times. The task of classification is exposed to the same difficulties, and governed by the same conditions, in this as in other subjects. Here, as in the case of other things, there may be many classifications which are moderately good and natural, but there is only one which is the best and the true natural system. Here, as in other cases, one classification brings into view one set of relations; another, another; and each may be valuable for its special purpose. Here, as in other cases, the classes may be well constituted, though the boundary lines which divide them be somewhat indistinct, and the order doubtful. Here, 200 as in other cases, we may have approached to the

natural classification without having attained it; and here, as in other cases, to *define* our classes is the last and hardest of our problems.

9. The most ancient classification of the Functions of living things[71], is the division of them into *Vital*, *Natural*, and *Animal*. The Vital Functions are those which cannot be interrupted without loss of life, as *Circulation*, *Respiration*, and *Nervous Communication*. The *Natural Functions* are those which without the intervention of the will operate on their proper occasions to preserve the bodies of animals; they are *Digestion*, *Absorption*, *Nutrition*; to which was added *Generation*. The *Animal* Functions are those which involve perception and will, by which the animal is distinguished from the vegetable; they are *Sensibility*, *Locomotion*, and *Voice*.

[71] *Dict. des Sciences Nat.* art. *Fonctions*.

The two great grounds of this division, the distinction of functions which operate continually, and those which operate

occasionally; and again, the distinction of functions which involve sensation and voluntary motion from those which do not; are truly of fundamental importance, and gave a real value to this classification. It was, however, liable to obvious objections: namely, *First*, that the names of the classes were ill chosen; for all the functions are natural, all are vital: *Second*, that the lines of demarcation between the classes are indefinite and ambiguous; Respiration is a *vital* function, as being continually necessary to life; but it is also a *natural* function, since it occurs in the formation of the nutritive fluid, and an *animal* function, since it depends in part on the will. But these objections were not fatal, for a classification may be really sound and philosophical, though its boundary lines are vague, and its nomenclature ill selected. The division of the functions we have mentioned kept its ground long; or was employed with a subdivision of one class, so as to make them four; the *vital*, *natural*, *animal* and *sexual* functions. 201

10. I pass over many intermediate attempts to classify the functions, and proceed to that of Bichat as that which is, I believe, the one most generally assented to in modern times. The leading principle in the scheme of this celebrated physiologist is the distinction between *organic* and *animal* life. This separation is nearly identical with the one just noticed between the vital and animal functions; but Bichat, by the contrasts which he pointed out between these classes of functions, gave a decided prominence and permanence to the distinction. The Organic Life, which in animals is analogous to the life of vegetables, and the Animal Life, which implies sensation and voluntary motion, have each its system of organs. The center of the animal life is the brain, of the organic life, the heart. The former is carried on by a symmetrical, the latter, by an unsymmetrical system of organs: the former produces intermitting, the latter continuous actions: and, in addition to these, other differences are pointed out. This distinction of the two lives, being thus established,

each is subdivided into two orders of Functions. The Animal Functions are passive, as *Sensation*: or active, as *Locomotion* and *Voice*; again, the Organic Functions are those of Composition, which are concerned in taking matter into the system; *Digestion*, *Absorption*, *Respiration*, *Circulation*, *Assimilation*; and those of Decomposition, which reject the materials when they have discharged their office in the system; and these are again, *Absorption*, *Circulation*, and *Secretion*. To these are added *Calorification*, or the production of animal heat. It appears, from what has been said, that *Absorption* and *Circulation* (and we may add *Assimilation* and *Secretion*, which are difficult to separate,) belong alike to the processes of composition and decomposition; nor in truth, can we, with any rigour, separate the centripetal and centrifugal movements in that vortex which, as we shall see, is an apt image of organic life.

Several objections have been made to this classification: and in particular, to the terms

thus employed. It has been asserted to be a perversion of language to 202 ascribe to animals *two lives*, and to call the higher faculties in man, perception and volition, the *animal* functions. But, as we have already said, when a classification is really good, such objections, which bear only upon the mode in which it is presented, are by no means fatal: and it is generally acknowledged by all the most philosophical cultivators of biology, that this arrangement of the functions is better suited to the purposes of the science than those which preceded it.

11. But according to the principles which we have already laid down, the solidity of such a classification is to be verified by its serving as a useful guide in biological researches. If the arrangement which we have explained be really founded in natural relations, it will be found that in proportion as physiologists have studied the separate functions above enumerated, their ideas of these functions, and of the powers by which they are carried on, have become more and

more clear;—have tended more and more to the character of exact and rigorous science.

To examine how far this has been the case with regard to all the separate functions, would be to attempt to estimate the value of all the principal physiological speculations of modern times; a task far too vast and too arduous for any one to undertake who has not devoted his life to such studies. But it may properly come within the compass of our present plan to show how, with regard to the broader lines of the above classification, there has been such a progress as we have above described, from more loose and inaccurate notions of some of the vital functions to more definite and precise ideas. This I shall attempt to point out in one or two instances.

CHAPTER IV.

Attempts to form Ideas of separate Vital Forces, and first of Assimilation and Secretion.

Sect. I.—*Course of Biological Research.*

1. It is to be observed that at present I do not speak of the progress of our knowledge with regard to the detail of the processes which take place in the human body, but of the approach made to some distinct Idea of the specially vital part of each process. In the History of Physiology, it has been seen[72] that all the great discoveries made respecting the organs and motions of the animal frame have been followed by speculations and hypotheses connected with such discoveries. The discovery of the circulation of the blood led to theories of

animal heat; the discovery of the motion of the chyle led to theories of digestion; the close examination of the process of reproduction in plants and animals led to theories of generation. In all these cases, the discovery brought to light some portion of the process which was mechanical or chemical, but it also, in each instance, served to show that the process was something more than mechanical or chemical. The theory attempted to explain the process by the application of known causes; but there always remained some part of it which must unavoidably be referred to an unknown cause. But though unknown, such a cause was not a hopeless object of study. As the vital functions became better and better understood, it was seen more and more clearly at what precise points of the process it was necessary to assume a peculiar vital energy, and what sort of properties 204 this energy must be conceived to possess. It was perceived where, in what manner, in what degree, mechanical and chemical agencies were modified, over-ruled, or counteracted, by

agencies which must be hypermechanical and hyperchemical. And thus the discoveries made in anatomy by a laborious examination of facts, pointed out the necessity of introducing new ideas, in order that the facts might be intelligible. Observation taught much; and among other things, she taught that there was something which could not be observed, but which must, if possible, be conceived. I shall notice a few instances of this.

[72] *Hist. Ind. Sc.* b. xvii.

SECT. II.—*Attempts to form a distinct Conception of Assimilation and Secretion.*

2. *The Ancients.*—That plants and animals grow by taking into their substance matter previously extraneous, is obvious to all: but as soon as we attempt to conceive this process distinctly in detail, we find that it involves no inconsiderable mystery. How does the same food become blood and flesh, bone and hair? Perhaps the earliest attempt to explain this mystery, is that recorded by

Lucretius[73] as the opinion of Anaxagoras, that food contains some bony, some fleshy particles, some of blood, and so on. We might, on this supposition, conceive that the mechanism of the body appropriates each kind of particle to its suitable place.

[73] Lucr. i. 855. Nunc et Anaxagoræ scrutemur ὁμοιομέρειαν.

But it is easy to refute this essay at philosophizing (as Lucretius refutes it) by remarking that we do not find milk in grass, or blood in fruit, though such food gives such products in cattle and in men. In opposition to this 'Homoiomereia,' the opinion that is forced upon us by the facts is, that the process of nutrition is not a selection merely, but an *assimilation*; the organized system does not *find*, but *make*, the additions to its structure. 205

3. *Buffon.*—This notion of *assimilation* may be variously expressed and illustrated; and all that we can do here, in order to show the progress of thought, is to adduce the speculations of those writers who have

been most successful in seizing and marking its peculiar character. Buffon may be taken as an example of the philosophy of his time on this subject. 'The body of the animal,' says he[74], 'is a kind of *interior mould*, in which the matter subservient to its increase is modelled and assimilated to the whole, in such a way that, without occasioning any change in the order and proportion of the parts, there results an augmentation in each part taken separately. This increase, this development, if we would have a *clear idea* of it, how can we obtain it, except by considering the body of the animal, and each of the parts which is to be developed, as so many interior moulds which only receive the accessory matter in the order which results from the position of all their parts? This development cannot take place, as persons sometimes persuade themselves, by an addition to the outside; on the contrary, it goes on by an intimate susception which penetrates the mass; for, in the part thus developed, the size increases in all parts proportionally, so that the new matter must penetrate it in all its

dimensions: and it is quite necessary that this penetration of substance must take place in a certain order, and according to a certain measure; for if this were not so, some parts would develope themselves more than others. Now what can there be which shall prescribe such a rule to the accessory matter except the *interior mould*?'

[74] *Hist. Nat.* b. i. c. iii.

To speak of a *mould* simply, would convey a coarse mechanical notion, which could not be received as any useful contribution to physiological speculation. But this *interior* mould is, of course, to be understood figuratively, not as an assemblage of cavities, but as a collection of laws, shaping, directing, and modifying the new matter; giving it not only form, but motion 206 and activity, such as belong to the parts of an organic being.

4. It must be allowed, however, that even with this explanation, the comparison is very loose and insufficient. A *mould* may be permitted to mean a collection of laws, but

still it can convey no conception except that of laws regulated by relations of space; and such a conception is very plainly quite inadequate to the purpose. What can we conceive of the interior mould by which chyle is separated from the aliments at the pores of the lacteals, or tears secreted in the lacrymatory gland?

An additional objection to this mode of expression of Buffon is, that it suggests to us only a single marked change in the assimilated matter, not a continuous series of changes. Yet the animal fluids and other substances are, in fact, undergoing a constant series of changes. Food becomes chyme, and chyme becomes chyle; chyle is poured into the blood; from the blood secretions take place, as the bile; the bile is poured into the digestive canal, and a portion of the matter previously introduced is rejected out of the system. Here we must have a series of ‘interior moulds;’ and these must impress matter at its ejection from the organic system as well as at its reception. But, moreover, it is probable that none of

the above transformations are quite abrupt. Change is going on between the beginning and the end of each stage of the nutritive circulation. To express the laws of this continuous change, the image of an interior mould is quite unsuited. We must seek a better mode of conception.

5. Vegetable and animal nutrition is, as we have said, a constant circulation. The matter so assumed is not all retained: a perpetual subtraction accompanies a perpetual addition. There is an excretion as well as an intussusception. The matter which is assumed by the living creature is retained only for a while, and is then parted with. The individual is the same, but its parts are in a perpetual flux: they come and go. For a time the matter which belongs to the organic body is bound to it by certain laws: but before it is thus bound, and 207 after it is loose, this matter may circulate about the universe in any other form. Life consists in a permanent influence over a perpetually changing set of particles.

Cuvier.—This condition also has been happily expressed, by means of a comparison, by another great naturalist. 'If,' says Cuvier[75], 'if, in order to obtain a just idea of the essence of life, we consider it in the beings where its effects are most simple, we shall soon perceive that it consists in the faculty which belongs to certain bodily combinations to continue during a determinate time under a determinate form; constantly attracting into their composition a part of the surrounding substances, and giving up in return some part of their own substance.

[75] *Règne Animal*, i. 11.

'Life is thus a *vortex*, more or less rapid, more or less complex, which has a constant direction, and which always carries along its stream particles of the same kinds; but in which the individual particles are constantly entering in and departing out; so that the *form* of the living body is more essential to it than its matter.

'So long as this motion subsists, the body

in which it takes place is *alive*; it *lives*. When the motion stops finally, the body *dies*. After death, the elements which compose the body, given up to the ordinary chemical affinities, soon separate, and the body which was alive is dissolved.'

This notion of a vortex[76] which is permanent while the matter which composes it constantly changes,—of peculiar forces which act in this vortex so long as it exists, and which give place to chemical forces when 208 the circulatory motion ceases,—appears to express some of the leading conditions of the assimilative power of living things in a simple and general manner, and thus tends to give distinctness to the notion of this vital function.

[76] The definition of life given by M. de Blainville appears to me not to differ essentially from that of Cuvier: 'Un corps vivant est une sorte de foyer chimique où il-y-a à tous momens apport de nouvelles molecules et départ de molecules anciennes; où la composition n'est jamais fixe (si ce n'est d'un certain nombre de parties veritablement mortes ou en depôt),

> mais toujours pour ainsi dire *in nisu*, d'où mouvement plus ou moins lent et quelquefois chaleur.'—*Principes d'Anat.* 1822, t. i. p. 16.

6. But we may observe that this notion of a Vortex is still insufficient. Particles are not only taken into the system and circulated through it for a time, but, as we have seen, they are altered in character in a manner to us unintelligible, both at their first admission into the system and at every period of their progress through it. In the vortex each particle is constantly *transformed* while it whirls.

It may be said, perhaps, that this transformation of the kinds of matter may be conceived to be merely a new arrangement of their particles, and that thus all the changes which take place in the circulating substances are merely so many additional windings in the course of the whirling current. But to say this, is to take for granted the atomic hypothesis in its rudest form. What right have we to assume that blood and tears, bile and milk, consist

of like particles of matter differently arranged? What can arrangement, a mere relation of space, do towards explaining such differences? Is not the insufficiency, the absurdity of such an assumption proved by the whole course of science? Are not even chemical changes, according to the best views hitherto obtained, something more than a mere new arrangement of particles? And are not vital as much beyond chemical, as chemical are beyond geometrical modifications? It is not enough, then, to conceive life as a vortex. The particles which are taken into the organic frame do more than circulate there. They are, at every point of their circulation, acted upon by laws of an unknown kind, changing the nature of the substance which they compose. Life is a vortex in which vital forces act at every point of the stream: it is not only a current of whirling *matter*, but a cycle of recurring *powers*.

7. *Matter and Form.*—This image of a vortex is closely connected with the representation of life offered 209 us by

writers of a very different school. In Schelling's *Lectures on Academic Study*, he takes a survey of the various branches of human knowledge, determining according to his own principles the shape which each science must necessarily assume. The peculiar character of organization, according to him[77], is that the matter is only an accident of the thing itself, and the organization consists in Form alone. But this Form, by its very opposition to Matter, ceases to be independent of it, and is only ideally separable. In organization, therefore, substance and accident, matter and form, are completely identical[78]. This notion, that in organization the Form is essential and the Matter accidental, or, in other words, that the Form is permanent and the Matter fluctuating and transitory, agrees, if taken in the grossest sense of matter and form, with Cuvier's image of a Vortex. In a whirlpool, or in a waterfall, the form remains, the matter constantly passes away and is renewed. But we have already seen[79] that in metaphysical speculations in which matter and form are opposed, the word

form is used in a far more extensive sense than that which denotes a relation of space. It may indeed designate any change which matter can undergo; and we may very allowably say that food and blood are the same matter under different *forms*. Hence if we assert that *Life is a constant Form of a circulating Matter*, we express Cuvier's notion in a mode free from the false suggestion which 'Vortex' conveys.

[77] Lect. xiii. p. 288.

[78] I have not translated Schelling's words, but given their import as far as I could.

[79] Book i.

8. We may, however, still add something to this account of life. The circulating parts of the system not only circulate, but they form the non-circulating parts. Or rather, there are no non-circulating parts: all portions of the frame circulate more or less rapidly. The food which we take circulates rapidly in the fluids, more slowly in the flesh, still more slowly in the bones; but in all these parts it is taken into the system,

210 retained there for some time, and finally replaced by other matter. But while it remains in the body, it exercises upon the other circulating parts the powers by which their motion is produced. Nutriment forms and supports the organs, and the organs carry fresh nutriment to its destination. The peculiar forces of the living body, and its peculiar structure, are thus connected in an indescribable manner. The forces produce the structure; the structure, again, is requisite for the exertion of the forces. The Idea of an Organic or Living Being includes this peculiar condition—that its construction and powers are such, that it constantly appropriates to itself new portions of substance which, so appropriated, become indistinguishable parts of the whole, and serve to carry on subsequently the same functions by which they were assimilated. And thus *Organic Life is a constant Form of a circulating Matter, in which the Matter and the Form determine each other by peculiar laws* (*that is, by Vital Forces*).

SECT. III.—*Attempts to conceive the forces of Assimilation and Secretion.*

9. I have already stated that in our attempts to obtain clear and scientific Ideas of Vital Forces, we have, in the first place, to seek to understand the course of change and motion in each function, so as to see at what points of the process peculiar causes come into play; and next, to endeavour to obtain some insight into the peculiar character and attributes of these causes. Having spoken of the first part of this mode of investigation in regard to the general nutrition of organic bodies, I must now say a few words on the second part.

The Forces here spoken of are *Vital* Forces. From what has been said, we may see in some measure the distinction between forces of this kind and mechanical or chemical forces; the latter tend constantly to produce a final condition, after which there is no further cause of change: mechanical forces tend to produce equilibrium; chemical forces tend to produce 211

composition or decomposition; and this point once reached, the matter in which these forces reside is altogether inert. But an organic body tends to a constant motion, and the highest activity of organic forces shows itself in continuous change. Again, in mechanical and chemical forces, the force of any aggregate is the sum of the forces of all the parts: the sum of the forces corresponds to the sum of the matter. But in organic bodies, the amount of effect does not depend on the matter, but on the form: the particles lose their separate energy, in order to share in that of the system; they are not added, they are *assimilated*.

10. It is difficult to say whether anything has been gained to science by the various attempts to assign a fixed *name* to the vital force which is thus the immediate cause of Assimilation. It has been called *Organic Attraction* or *Vital Attraction*, *Organic Affinity* or *Vital Affinity*, being thus compared with mechanical Attraction or chemical Affinity. But, perhaps, as the process is certainly neither mechanical nor chemical, it is

desirable to appropriate to it a peculiar name; and the name *Assimilation*, or *Organic Assimilation*, by the usage of good biological writers, is generally employed for this purpose, and may be taken as the standard name of this Vital Force. To illustrate this, I will quote a passage from the excellent *Elements of Physiology* of Professor Müller. 'In the process of nutrition is exemplified the fundamental principle of *organic assimilation*. Each elementary particle of an organ attracts similar particles from the blood, and by the changes it produces in them, causes them to participate in the vital principle of the organ itself. Nerves take up nervous substance, muscles, muscular substance: even morbid structures have the assimilating power; warts in the skin grow with their own peculiar structure; in an ulcer, the base and border are nourished in a way conformable to the mode of action and secretion determined by the disease.'

11. The Force of Organic Assimilation spoken of in the last paragraph denotes peculiarly the force by which each organ

appropriates to itself a part of the 212 nutriment received into the system, and thus is maintained and augmented with the growth of the whole. But the growth of the solid parts is only one portion of the function of nutrition; besides this, we must consider the motion and changes of the fluids, and must ask what kind of forces may be conceived to produce these. What are the powers by which chyle is *absorbed* from the food, by which bile is *secreted* from the blood, by which the circulating *motion* of these and all other fluids of the body are constantly maintained? To the questions,—What are the forces by which *absorption*, *secretion*, and the *vital motions*, of fluids are produced?—no satisfactory answer has been returned. Yet still some steps have been made, which it may be instructive to point out.

12. In *Absorption* it would appear that a part of the agency is inorganic; for not only dead membranes, but inorganic substances, absorb fluids, and even absorb them with elective forces, according to the ingredients,

of the fluid. A force which is of this kind, and which has been termed *Endosmose*, has been found to produce very curious effects. When a membrane separates two fluids, holding in solution different ingredients, the fluids pass through the membrane in an imperceptible manner, and mix or exchange their elements. The force which produces these effects is capable of balancing a very considerable pressure. It appears, moreover, to depend, at least among other causes, upon attractions operating between the elements of the solids and the fluids, as well as between the different fluids; and this force, though thus apparently of a mechanical and chemical nature, probably has considerable influence in vital phenomena.

13. But still, though Endosmose may account in part for absorption in some cases, it is certain that there is some other vital force at work in this process. There must be, as Müller says[80], 'an organic attraction of a kind hitherto unknown.' 'If absorption,' he adds[81], is to be explained in

a manner analogous to 213 the laws of endosmose, it must be supposed that a chemical affinity, resulting from the vital process itself, is exerted between the chyme in the intestines and the chyle in the lacteals, by which the chyle is enabled to attract the chyme without being itself attracted by it. But such affinity or attraction would be of a vital nature, since it does not exist after death.'

[80] *Physiology*, p. 299.

[81] *Ib.* p. 301.

14. If the force of absorption be thus mysterious in its nature, the force of *Secretion* is still more so. In this case we have an organ filled with a fine net-work of blood-vessels, and in the cavities of some *gland*, or open part, we have a new fluid formed, of a kind altogether different from the blood itself. It is easily shown that this cannot be explained by any action of pores or capillary tubes. But what conception can we form of the forces by which such a change is produced? Here, again, I shall

borrow the expressions of Müller, as presenting the last result of modern physiology. He says[82], 'The more probable supposition is, that by virtue of imbibition, or the general organic porosity, the fluid portion of the blood becomes diffused through the tissue of the secreting organ; that the external surface of the glandular canals exerts a chemical attraction on the elements of the fluid, infusing into them at the same time a tendency to unite in new combinations; and then repels them in a manner which is certainly quite inexplicable, towards the inner surface of the secreting membrane, or glandular canals.' 'Although quite unsupported by facts,' he adds, 'this theory of attraction and repulsion is not without its analogy in physical phenomena; and it would appear that very similar powers effect the elimination of the fluid in secretion, and cause it to be taken up by the lymphatics in absorption.' He elsewhere says[83], 'Absorption seems to depend on an attraction the nature of which is unknown, but of which the very counterpart, as it

were, takes place in secretion; the fluids altered by the secreting action being repelled towards the free side or open surface only of the 214 secreting membranes, and then pressed forwards by the successive portions of the fluids secreted.'

[82] *Physiology*, p. 464.

[83] *Ib.* p. 301.

15. With regard to the forces which produce the *Motion* of absorbed or secreted fluids along their destined course, it may be seen, from the last quoted sentence, that the same vital force which changes the nature, also produces the movement of the substance. The fluids are pressed forwards by the successive portions absorbed or secreted. That this is the sole cause, or at least a very powerful cause, of the motion of the nutritive fluids in organic bodies, is easily shown by experience. It is found[84] that the organs which effect the ascent of the sap in trees during the spring are the terminal parts of the roots; that the whole

force by which the sap is impelled upwards is the *vis a tergo*, as it has been called, the force pushing from behind, exerted in the roots. And thus the force which produces this motion is exerted exactly at those points where the organic body selects from the contiguous mass those particles which it absorbs and appropriates. And the same may most probably be taken for the cause of the motion of the lymph and chyle; at least, Müller says[85] that no other motive power has been detected which impels those fluids in their course.

[84] Müller, p. 300.

[85] *Ib.* p. 254.

Thus, though we must confess the Vital Force concerned in Assimilation and Secretion to be unknown in its nature, we still obtain a view of some of the attributes which it involves. It has mechanical efficacy, producing motions, often such as would require great mechanical force. But it exerts at the same point both an attraction and a repulsion, attracting matter on one

side, and repelling it on the other; and in this circumstance it differs entirely from mechanical forces. Again, it is not only mechanical but chemical, producing a complete change in the nature of the substance on which it acts; to which we must add that the changes produced by the vital forces are such as, for the most part, our artificial chemistry 215 cannot imitate. But, again, by the action of the vital force at any point of an organ, not only are fluids made to pass, and changed as they pass, but the organ itself is maintained and strengthened, so as to continue or to increase its operation: and thus the vital energy supports its activity by its action, and is augmented by being exerted.

We have thus endeavoured to obtain a view of some of the peculiar characters which belong to the Force of Organic Assimilation;—the Force by which life is kept up, conceived in the most elementary form to which we can reduce it by observation and contemplation. It appears that it is a force which not only produces

motion and chemical change, but also *vitalizes* the matter on which it acts, giving to it the power of producing like changes on other matter, and so on indefinitely. It not only circulates the particles of matter, but puts them in a stream of which the flow is development as well as movement.

The force of Organic Assimilation being thus conceived, it becomes instructive to compare it with the force concerned in Generation, which we shall therefore endeavour to do.

SECT. IV.—*Attempts to conceive the Process of Generation.*

16. At first sight the function of Nutrition appears very different from the function of Generation. In the former case we have merely the existing organs maintained or enlarged, and their action continued; in the latter, we have a new individual produced and extricated from the parent. The term *Reproduction* has, no doubt, been applied, by different writers, to both these functions;—

to the processes by which an organ when mutilated, is restored by the forces of the living body, and to the process by which a new generation of individuals is produced which may be considered as taking the place of the old generation, as these are gradually removed by death. But these are obviously different senses of the word. In the latter case, the 216 term *Reproduction* is figuratively used; for the *same* individuals are not reproduced; but the species is kept up by the propagation of new individuals, as in nutrition the organ is kept up by the assimilation of new matter. To escape ambiguity, I shall avoid using the term *Reproduction* in the sense of *Propagation*.

17. In Nutrition, as we have seen, the matter, which from being at first extraneous, is appropriated by the living system, and directed to the sustentation of the organs, undergoes a series of changes of which the detail eludes our observation and apprehension. The nutriment which we receive contributes to the growth of flesh and bone, viscera and organs of sense. But

we cannot trace in its gradual changes a visible preparation for its final office. The portion of matter which is destined to repair the waste of the eye or the skin, is not found assuming a likeness to the parts of the eye or the structure of the skin, as it comes near the place where it is moulded into its ultimate form. The new parts are insinuated among the old ones, in an obscure and imperceptible matter. We can trace their progress only by their effects. The organs *are* nourished, and that is almost all we can learn: we cannot discover *how* this is done. We cannot follow nature through a series of manifest preparations and processes to this result.

18. In Generation the case is quite different. The young being is formed gradually and by a series of distinguishable processes. It is included within the parent before it is extruded, and approaches more or less to the likeness of the parent before it is detached. While it is still an embryo, it shares in the nutriment which circulates through the system of the mother; but its

destination is already clear. While the new and the old parts, in every other portion of the mother, are undistinguishably mixed together, this new part, the fœtus, is clearly distinct from the rest of the system, and becomes rapidly more and more so, as the time goes on. And thus there is formed, not a new part, but a new whole; it is not an organ which is kept up, but an offspring which is prepared. The progeny is 217 included in the parent, and is gradually fitted to be separated from it. The young is at first only the development of a part of the organization of the mother;—of a germ, an ovule. But it is not developed like other organs, retaining its general form. It does not become merely a larger bud, a larger ovule; it is entirely changed; it becomes—from a bud—a blossom, a flower, a fruit, a seed; from an ovule it becomes an egg, a chick, a bird; or it may be, a fœtus, a child. The original rudiment is not merely nourished, but unfolded and transformed through the most marked and remote changes, gradually tending to the form of the new individual.

19. But this is not all. The fœtus is, as we have said, a development of a portion of the mother's organization. But the fœtus (supposing it female) is a likeness of the mother. The mother, even before conception, contains within herself the germs of her progeny; the female fœtus, therefore, at a certain stage of development, will contain also the germs of possible progeny; and thus we may have the germs of future generations, pre-existing and included successively within one another. And this state of things, which thus suggests itself to us as possible, is found to be the case in facts which observation supplies. Anatomists have traced ovules in the unborn fœtus, and thus we have three generations included one within another.

20. Supposing we were to stop here, the process of propagation might appear to be altogether different from that of nutrition. The latter, as we have seen, may be in some measure illustrated by the image of a *vortex*; the former has been represented by the image of a series of germs, *sheathed* one

within another successively, and this without any limit. This view of the subject has been termed the doctrine of the *Pre-existence of germs*; and has been designated by German writers by a term 'Einschachtelungs-theorie' descriptive of the successive sheathing of which I have spoken. Imitating this term, we may call it *the Theory of successive inclusion*. It has always had many 218 adherents; and has been, perhaps, up to the present time, the most current opinion on the subject of generation. Cuvier inclines to this opinion[86]. 'Fixed forms perpetuating themselves by generation distinguish the species of living things. These forms do not produce themselves, do not change themselves. Life supposes them to exist already; its flame can be lighted only in organization previously prepared; and the most profound meditations and the most delicate researches terminate alike in the mystery of the *pre-existence of germs*.'

[86] *Règne Animal*, p. 20.

21. Yet this doctrine is full of difficulty. It

is, as Cuvier says, a mysterious view of the subject;—so mysterious, that it can hardly be accepted by us, who seek distinct conceptions as the basis of our philosophy. Can it be true, not only that the germ of the offspring is originally included in the parent, but also the germs of *its* progeny, and so on without limit:—so that each fruitful individual contains in itself an infinite collection of future possible individuals;—a reserve of infinite succeeding generations? This is hard to admit. Have we no alternative? What is the opposite doctrine?

22. The opposite doctrine deserves at least some notice. It extends, to the production of a new individual, the conception of growth by nutrition. According to this view, we suppose propagation to take place, not as in the view just spoken of, by inclusion and extrusion, but by assimilation and development;—not by the material pre-existence of germs, but by the communication of vital forces to new matter. This opinion appears to be

entertained by some of the most eminent physiologists of the present time. Thus, Müller says, ‘The organic force is also creative. The organic force which resides in the whole, and on which the existence of each part depends, has also the property of generating, from organic matter, the parts necessary to the whole.’ Life, he adds, is not merely a harmony of the 219 parts. On the contrary, the harmonious action of the parts subsists only by the influence of a force pervading all parts of the body. ‘This force exists before the harmonizing parts, which are in fact formed by it during the development of the embryo.’ And again; ‘The creative force exists in the germ, and creates in it the essential force of the future animal. The germ is *potentially* the whole animal: during the development of the germ the parts which constitute the actual whole are produced.’

23. In this view, we extend to the reproduction of an individual the same conception of organic assimilation which we have already arrived at, as the best notion

we can form of the force by which the reproduction and sustentation of parts takes place. And is not such an extension really very consistent? If a living thing can appropriate to itself extraneous matter, invest it with its own functions, and thus put it in the stream of constant development, may we not conceive the development of a new *whole* to take place in this way as well as of a *part*? If the organized being can infuse into new matter its vital forces, is there any contradiction in supposing this infusion to take place in the full measure which is requisite for the production of a new individual? The force of organic assimilation is transferred to the very matter on which it acts; it may be transferred so that the operation of the forces produces not only an organ, but a system of organs.

24. This identification of the forces which operate in Nutrition and Generation may at first seem forced and obscure, in consequence of the very strong apparent differences of the two processes which we

have already noticed. But this defect in the doctrine is remedied by the consideration of what may be considered as intermediate cases. It is not true that, in the nutrition of special organs, the matter is always conveyed to its ultimate destination without being on its way moulded into the form which it is finally to bear, as the embryo is moulded into the form of the 220 future individual. On the contrary, there are cases in which the waste of the organs is supplied by the growth of new ones, which are prepared and formed before they are used, just as the offspring is prepared and formed before it is separated from the parent. This is the case with the teeth of many animals, and especially with the teeth of animals of the crocodile kind. Young teeth grow near the root of the old ones, like buds on the stem of a plant; and as these become fully developed, they take the place of the parent tooth when that dies and is cast away. And these new teeth in their turn are succeeded by others which germinate from them. Several generations of such teeth, it is said as many as four, have been detected by

anatomists, visibly existing at the same time; just as several generations of germs of individuals have been, as we already stated, observed included in one another. But this case of the teeth appears to show very strikingly how insufficient such observations are to establish the doctrine of successive inclusion, or of the pre-existence of germs. Are we to suppose that every crocodile's tooth includes in itself the germs of an infinite number of possible teeth, as in the theory of pre-existing germs every individual includes an infinite number of individuals? If this be true of teeth, we must suppose that organ to follow laws entirely different from almost every other organ; for no one would apply to the other organs in general such a theory of reproduction. But if such a theory be not maintained respecting the teeth, how can we maintain the theory of the pre-existing germs of individuals, which has no recommendation except that of accounting for exactly the same phenomena?

It would seem, then, that we are, by the

closest consideration of the subject, led to conceive the forces by which generation is produced, as forces which vitalize certain portions of matter, and thus prepare them for development according to organic forms; and thus the conception of this Generative Force is identified with the conception of the Force of Organic Assimilation, to 221 which we were led by the consideration of the process of nutrition.

I shall not attempt to give further distinctness and fixity to this conception of one of the vital forces; but I shall proceed to exemplify the same analysis of life by some remarks upon another Vital Process, and the Forces of which it exhibits the operation.

CHAPTER V.

Attempts to form Ideas of separate Vital Forces, *continued*.—Voluntary Motion.

1. We formerly noticed the distinctions of *organic* and *animal* functions, organic and animal forces, as one of the most marked distinctions to which physiologists have been led in their analysis of the vital powers. I have now taken one of the former, the *organic* class of functions, namely, Nutrition; and have endeavoured to point out in some measure the peculiar nature of the vital forces by which this function is carried on. It may serve to show the extent and the difficulty of this subject, if, before quitting it, I offer a few remarks suggested by a function belonging to the other class, the *animal* functions. This I shall briefly do

with respect to *Voluntary Motion*.

2. In the History of Physiology, I have already related the progress of the researches by which the organs employed in voluntary motion became known to anatomists. It was ascertained to the satisfaction of all physiologists, that the immediate agents in such motion are the muscles; that the muscles are in some way contracted, when the nerves convey to them the agency of the will; and that thus the limbs are moved. It was ascertained, also, that the nerves convey sensations from the organs of sense inwards, so as to make these sensations the object of the animal's consciousness. In man and the higher animals, these impressions upon the nerves are all conveyed to one internal organ, the brain; and from this organ all impressions of the will appear to proceed; and thus the brain is 223 the center of animal life, towards which sensations converge, and from which volitions diverge.

But this being the process, we are led to

inquire how far we can obtain any knowledge, or form any conception, of the vital forces by means of which the process is carried on. And here I have further stated in the History[87], that the transfer of sensations and volitions along the nerves was often represented as consisting in the motion of a Nervous Fluid. I have related that the hypothesis of such a fluid, conveying its impressions either by motions of translation or of vibration, was countenanced by many great names, as Newton, Haller, and even Cuvier. But I have ventured to express my doubt whether this hypothesis can have much value: 'for,' I have said, 'this principle cannot be mechanical, chemical, or physical, and therefore cannot be better understood by embodying it in a fluid. The difficulty we have in conceiving what the force *is*, is not got rid of by explaining the machinery by which it is *transferred*.'

[87] *Hist. Ind. Sc.* b. xvii. c. v. s. 2.

3. I may add, that no succeeding biological researches appear to have diminished the force of these

considerations. In modern times, attempts have repeatedly been made to identify the nervous fluid with electricity or galvanism. But these attempts have not been satisfactory or conclusive of the truth of such an identity: and Professor Müller probably speaks the judgment of the most judicious physiologists, when he states it as his opinion, after examining the evidence[88], 'That the vital actions of the nerves are not attended with the development of any galvanic currents which our instruments can detect; and that the laws of action of the nervous principle are totally different from those of electricity.'

[88] *Elem. Phys.* p. 640.

That the powers by which the nerves are the instruments of sensation, and the muscles of motion, are vital endowments, incapable of being expressed or explained by any comparison with mechanical, chemical, and electrical forces, is the result which we should 224 expect to find, judging from the whole analogy of science; and which thus is confirmed by the history of

physiology up to the present time. We naturally, then, turn to inquire whether such peculiar vital powers have been brought into view with any distinctness and clearness.

4. The property by which muscles, under proper stimulation, contract and produce motion, has been termed *Irritability* or *Contractility*; the property by which nerves are susceptible of their appropriate impressions has been termed *Sensibility*. A very few words on each of these subjects must suffice.

Irritability.—I have, in the History of Physiology[89], noticed that Glisson, a Cambridge professor, distinguished the Irritation of muscles as a peculiar property, different from any merely mechanical or physical action. I have mentioned, also, that he divides Irritation into *natural*, *vital*, and *animal*; and points out, though briefly, the graduated differences of Irritability in different organs. Although these opinions did not at first attract much notice, about

seventy years afterwards attention was powerfully called to this vital force, *Irritability*, by Haller. I shall borrow Sprengel's reflections on this subject.

[89] *Hist. Ind. Sc.* b. xvii. c. v.

'Hitherto men had been led to see more and more clearly that the cause of the bodily functions, the fundamental power of the animal frame, is not to be sought in the mechanism, and still less in the mixture of the parts. In this conviction, they had had recourse partly to the quite supersensuous principle of the Soul, partly to the half-material principle of the Animal Spirits, in order to explain the bodily motions. Glisson alone saw the necessity of assuming an Original Power in the fibres, which, independent of the influence of the animal spirits, should produce contraction in them. And Gorter first held that this Original Power was not to be confined to the muscles, but to be extended to all parts of the living body. 225

'But as yet the laws of this Power were

not known, nor had men come to an understanding whether it were fully distinct from the elasticity of the parts, or by what causes it was put in action. They had neither instituted observations nor experiments which established its relation to other assumed forces of the body. There was still wanting a determination of the peculiar seat of this power, and experiments to trace its gradual differences in different parts of the body. In addition to other causes, the necessity of the assumption of such a power was felt the more, in consequence of the prevalence of Leibnitz's doctrine of the activity of matter; but it was an occult quality, and remained so till Haller, by numerous experiments and solid observations, placed in a clear light the peculiarities of the powers of the animal body.'

5. Perhaps, however, Haller did more in the way of determining experimentally the limits and details of the application of this idea of Irritability as a peculiar attribute, than in developing the Idea itself. In that

way his merits were great. As early as the year 1739, he published his opinion upon Irritability as the cause of muscular motion, which he promulgated again in 1743. But from the year 1747 he was more attentive to the peculiarities of Irritability, and its difference from the effect of the nerves. In the first edition of his *Physiology*, which appeared in 1747, he distinguished three kinds of Force in muscles,—the Dead Force, the Innate Force, and the Nervous Power. The first is identical with the elastic force of dead matter, and remains even after death. The *innate force* continues only a short time after death, and discloses itself especially by alternate oscillations; the motions which arise from this are much more lively than those which arise from mere elasticity: they are not excited by tension, nor by pressure, nor by any mechanical alteration, but only by *irritation*. The *nervous force* of the muscle is imparted to it from without by the nerves; it preserves the *irritability*, which cannot long subsist without the influence of the nervous force, but is not identical with it. 226

In the year 1752, Haller laid before the Society of Göttingen the result of one hundred and ninety experiments; from which it appears to what parts of the animal system Irritability and Nervous Power belong. These I need not enumerate. He also investigated with care its gradations in those parts which do possess it. Thus the heart possesses it in the highest degree, and other organs follow in their order.

6. Haller's doctrine was, that there resides in the muscles a peculiar vital power by which they contract, and that this power is distinct from the attributes of the nerves. And this doctrine has been accepted by the best physiologists of modern times. But this distinction of the *irritability* of the muscles from the *sensibility* of the nerves became somewhat clearer by giving to the former attribute the name of *Contractility*. This accordingly was done; it is, for example, the phraseology used by Bichat. By speaking of *animal sensibility* and *animal contractility*, the passive and the active element of the processes of animal life are clearly

separated and opposed to each other. The sensations which we feel, and the muscular action which we exert, may be closely and inseparably connected, yet still they are clearly distinguishable. We can easily in our apprehension separate the titillation felt in the nose on taking snuff, from the action of the muscles in sneezing; or the perception of an object falling towards the eye, from the exertion which shuts the eye-lid; although in these cases the passive and active part of the process are almost or quite inseparable in fact. And this clear separation of the active from the passive power is something, it would seem, peculiar to the Animal Vital Powers; it is a character by which they differ, not only from mechanical, chemical, and all other merely physical forces, but even from Organic Vital Powers.

7. But this difference between the Animal and the Organic Vital Powers requires to be further insisted upon, for it appears to have been overlooked or denied by very eminent physiologists. For instance, Bichat classifies the Vital Powers as Animal Sensibility, 227

Animal Contractility, Organic Sensibility, Organic Contractility.

Now the view which suggests itself to us, in agreement with what has been said, is this:—that though Animal Sensibility and Animal Contractility are clearly and certainly distinct, Organic Sensibility and Organic Contractility are neither separable in fact nor in our conception, but together make up a single Vital Power. That they are not separable in fact is, indeed, acknowledged by Bichat himself. 'The organic contractility,' he says[90], 'can never be separated from the sensibility of the same kind; the reaction of the excreting tubes is immediately connected with the action which the secreted fluids exercise upon them: the contraction of the heart must necessarily succeed the influx of the blood into it.' It is not wonderful, therefore, that it should have happened, as he complains, that 'authors have by no means separated these two things, either in their consideration or in language.' We cannot avoid asking, Are Organic Sensibility and

Organic Contractility really anything more than two different aspects of the same thing, like action and reaction in mechanics, which are only two ways of considering the action which takes place at a point; or like the positive and negative electricities, which, as we have seen, always co-exist and correspond to each other?

[90] *Life and Death*, p. 94.

8. But we may observe, moreover, that Bichat, by his use of the term Contractility, includes in it powers to which it cannot with any propriety be applied. Why should we suppose that the vital powers of absorption, secretion, assimilation, are of such a nature that the name *contractility* may be employed to describe them? We have seen, in the last chapter, that the most careful study of these powers leads us to conceive them in a manner altogether removed from any notion of contraction. Is it not then an abuse of language which cannot possibly lead to anything but 228 confusion, to write thus[91]: 'The insensible organic contractility is that, by virtue of

which the excreting tubes react upon their respective fluids, the secreting organs upon the blood which flows into them, the parts where nutrition is performed upon the nutritive juices, and the lymphatics upon the substances which excite their open extremities'? In the same manner he ascribes[92] to the peculiar sensibility of each organ the peculiarity of its products and operations. An increased absorption is produced by an increased susceptibility of the 'absorbent orifices.' And thus, in this view, each organic power may be contemplated either as sensibility or as contractility, and may be supposed to be rendered more intense by magnifying either of these its aspects; although, in fact, neither can be conceived to be increased without an exactly commensurate increase of the other.

[91] *Life and Death*, p. 95.

[92] *Ib.* p. 90.

9. This opinion, unfounded as it thus appears to be, that all the different organic

vital powers are merely different kinds of Contractility or Excitability, was connected with the doctrines of Brown and his followers, which were so celebrated in the last century, that all diseases arise from increase or from diminution of the Vital Force. The considerations which have already offered themselves would lead us to assent to the judgment which Cuvier has pronounced upon this system. 'The theory of excitation,' he says, 'so celebrated in these later times by its influence upon pathology and therapeutick, is at bottom only a modification of that, in which, including under a common name Sensibility and Irritability,' and we may add, applying this name to all the Vital Powers, 'the speculator takes refuge in an abstraction so wide, that if, by it, he simplifies medicine, he by it annihilates all positive physiology[93].'

[93] *Hist. des Sc. Nat. depuis* 1789, i. 219.

10. The separation of the nervous influence and the muscular irritability, although it has led to many highly instructive speculations, is not without its

229 difficulties, when viewed with reference to the Idea of Vital Power. If the irritability of each muscle reside in the muscle itself, how does it differ from a mere mechanical force, as elasticity? But, in point of fact, it is certain that the muscular irritability of the animal body is not an attribute of the muscle itself independent of its connexion with the system. No muscle, or other part, removed from the body, *long* preserves its irritability. This power cannot subsist permanently, except in connexion with an organic whole. This condition peculiarly constitutes irritability a *living* force: and this condition would be satisfied by considering the force as derived from the nervous system; but it appears that though the nervous system has the most important influence upon all vital actions, the muscular irritability must needs be considered as something distinct. And thus the Irritability or Contractility of the muscle is a peculiar endowment of the texture, but it is at the same time an endowment which can only co-exist with life; it is, in short, a peculiar Vital Power.

11. This necessity of the union of the muscle with the whole nervous system, in order that it may possess irritability, was the meaning of the true part of Stahl's psychical doctrine; and the reason why he and his adherents persisted in asserting the power of the soul even over involuntary motions. This doctrine was the source of much controversy in later times.

'But,' says Cuvier[94], 'this opposition of opinion may be reconciled by the intimate union of the nervous substance with the fibre and the other contractile organic elements, and by their reciprocal action;—doctrines which had been presented with so much probability by physiologists of the Scotch school, but which were elevated above the rank of hypotheses only by the observations of more recent times.

[94] *Hist. des Sc. Nat. depuis* 1789, i. 213.

'The fibre does not contract by itself, but by the influence of the nervous filaments, which are always united with it. The change which produces the 230 contraction cannot

take place without the concurrence of both these substances; and it is further necessary that it should be occasioned each time by an exterior cause, by a stimulant.

'The Will is one of these stimulants; but it only excites the Irritability, it does not constitute it; for in the case of persons paralytic from apoplexy, the Irritability remains, though the power of the Will over it is gone. Thus *irritability* depends in part on the *nerve*, but not on the *sensibility*: this last is another property, still more admirable and occult than the irritability; but it is only one among several functions of the nervous system. It would be an abuse of words to extend this denomination to functions unaccompanied by perception.'

12. Supposing, then, that Contractility is established as a peculiar Vital Power residing in the muscles, we may ask whether we can trace with any further exactness the seat and nature of this power. It would be unsuitable to the nature of the present work to dwell upon the anatomical

discussions bearing upon this point. I will only remark that some anatomists maintain[95] that muscles are contracted by those fibres assuming a zigzag form, which at first were straight. Others (Professor Owen and Dr. A. Thompson) doubt the accuracy of this observation; and conceive that the muscular fibre becomes shorter and thicker, but does not deviate from a right line. We may remark that the latter kind of action appears to be more elementary in its nature. We can, as a matter of geometry, conceive a straight line thrown into a zigzag shape by muscular contractions taking place between remote parts of it; but it is difficult to conceive by what *elementary* mode of action a straight fibre could bend itself at certain points, and at certain points only; since the elementary force must act at every point of the fibre, and not at certain selected points.

[95] Müller, *Elem. Phys.* p. 887.

13. A circumstance which remarkably marks the difference between the vital force of Contractility, 231 inherent in muscles,

and any merely dead or mechanical force, is this; that in assuming their contractile state, muscles exert a tension which they could not themselves support or convey if not strengthened by their vital irritability. They are capable of raising weights by their exertion, which will tear them asunder when the power of contraction is lost by death. This has induced Cuvier and other physiologists[96] to believe 'that in the moment of action, the particles that compose a fibre, not only approach towards each other longitudinally, but that their cohesive attraction becomes instantaneously much greater than it was before: for without such an increase of cohesive force, the tendency to shorten could not, as it would appear, prevent the fibre from being torn.' We see here the difficulty, or rather the impossibility, of conceiving muscular contractility as a mere mechanical force; and perhaps there is little hope of any advantage by calling in the aid of chemical hypothesis to solve the mechanical difficulty. Cuvier conjectures that a sudden change in the chemical composition may

thus so quickly and powerfully augment the cohesion. But we may ask, are not a chemical synthesis and analysis, suddenly performed by a mere act of the will, as difficult to conceive as a sudden increase and decrease of mechanical power directly produced by the same cause?

[96] Prichard, *Vital Prin.* p. 126.

14. *Sensibility.* The nerves are the organs and channels of Sensibility. By means of them we receive our sensations, whether of mere pleasure and pain, or of qualities which we ascribe to external objects, as a bitter taste, a sweet odour, a shrill sound, a red colour, a hard or a hot feeling of touch. Some of these sensations are but obscurely the objects of our consciousness; as for example the feeling which our feet have of the ground, or the sight which our eyes have of neighbouring objects, when we walk in a reverie. In these cases the sensations, though obscure, exist; for they 232 serve to balance and guide us as we walk. In other cases, our sensations are distinctly and directly the objects of our

attention.

But our Sensations, as we have already said, we ascribe as Qualities to external objects. By our senses we perceive objects, and thus our *sensations* become *perceptions*. We have not only the sensation of *round*, *purple*, and *green*, repeated and varied, but the perception of a *bunch of grapes* partly ripe and partly unripe. We have not only sensations of noise and of variously-coloured specks rapidly changing their places, but we have perceptions, by sound and sight, of a stone rolling down the hill and crushing the shrubs in its path. We scarcely ever dwell upon our Sensations; our thoughts are employed upon Objects. We regard the impressions upon our nerves, not for what they *are*, but for what they *tell* us.

But in what Language do the impressions upon the nerves thus speak to us of an external world,—of the forms and qualities and actions of objects? How is it that by the aid of our nervous system we become acquainted not only with impressions but

with *things*; that we learn not only the relation of objects to us, but to one another?

15. It has been shown at some length in the previous Books, that the mode in which Sensations are connected in our minds so as to convey to us the knowledge of Objects and their Relations, is by being contemplated with reference to *Ideas*. Our Sensations, connected by the Idea of Space, become Figures; connected by the Idea of Time, they become Causes and Effects; connected by the Idea of Resemblance, they become Individuals and Kinds; connected by the Idea of Organization, they become Living Things. It has been shown that without these Ideas there can be no connexion among our sensations, and therefore no perception of Figure, Action, Kind, or in short, of bodies under any aspect whatever. Sensations are the rude *Matter* of our perceptions; and are nothing, except so far as they have *Form* given them by Ideas. 233 But thus moulded by our Ideas, Sensation becomes the source of an endless store of important Knowledge of every

possible kind.

16. But one of the most obvious uses of our perceptions and our knowledge is to direct our Actions. It is suitable to the condition of our being that when we perceive a bunch of grapes, we should be able to pluck and eat the ripe ones; that when we perceive a stone rushing down the side of a hill, we should be able to move so as to avoid it. And this must be done by moving our limbs; in short, by the use of our muscles. And thus Sensation leads, not directly, but through the medium of Ideas, to muscular Contraction. I say that sensation and Muscular action are in such cases connected through the medium of Ideas. For when we proceed to pluck the grape which we see, the *sensation* does not determine the motion of the hand by any necessary geometrical or mechanical conditions, as an impression made upon a machine determines its motions; but the *perception* leads us to stretch forth the hand to that part of space, wherever it is, where we *know* that the grape is; and this, not in any

determinate path, but, it may be, avoiding or removing intervening obstacles, which we also *perceive*. There is in every such case a connexion between the sensation and the resulting action, not of a material but of a mental kind. The cause and the effect are bound together, not by physical but by intellectual ties.

17. And thus in such cases, between the two *vital* operations, Sensation and Muscular Action, there intervenes, as an intermediate step, Perception or Knowledge, which is not merely vital but *ideal*. But this is not all; there is still another mental part of the process which may be readily distinguished from that which we have described. An act of the *Will*, a Volition, is that, in the Mind, which immediately determines the action of the Muscles of the Body. And thus Will intervenes between Knowledge and Action; and the cycle of operations which take place when animals act with reference to external objects is 234 this:—Sensation, Perception, Volition, Muscular Contraction.

18. To attempt further to analyse the mental part of this cycle does not belong to the present part of our work. But we may remark here, as we have already remarked in the History[97], how irresistibly we are led by physiological researches into the domain of thought and mind. We pass from the body to the soul, from physics to metaphysics; from biology to psychology; from things to persons; from nouns to pronouns. I have there noticed the manner in which Cuvier expresses this transition by the introduction of the pronoun: 'The impression of external objects upon the ME, the production of a sensation, of an image, is a mystery impenetrable to our thoughts.'

[97] *Hist. Ind. Sc.* b. xvii. c. v. s. 2.

19. But to return to the merely biological part of our speculations. We have arrived, it will be perceived, at this result: that in animal actions there intervenes between the two terms of Sensation and Muscular Contraction, an intermediate process; which may be described as a communication to and from a Center. The Center is the seat of

the sentient and volent faculties, and is of a *hyperphysical* nature. But the existence of such a Center as a necessary element in the functions of the *animal* life is a truth which is important in biology. This indeed may be taken as the peculiar character of animal, as distinguished from merely *organic* powers. Accordingly, it is so stated by Bichat. For although he superfluously, as I have tried to show, introduces into his list of vital powers an organic sensibility, he still draws the distinction of which I have spoken; 'in the animal life, Sensibility is the faculty of receiving an Impression *plus* that of referring it to a common Center[98].'

[98] *Life and Death*, p. 84.

20. But since Sensibility and Contractility are thus connected by reference to a common Center, we may ask, before quitting the subject, what are the different forms which this reference assumes? Is the connexion 235 always attended by the distinct steps of Knowledge and Will,—by a clear act of consciousness, as in the case which we have taken, of plucking a grape;

or may these steps become obscure, or vanish altogether?

We need not further illustrate the *conscious* connexion. Such actions as we have described are called *voluntary* actions. In extreme cases, the mental part of the process is obvious enough. But we may gradually pass from these to cases in which the mental operation is more and more obscure.

In walking, in speaking, in eating, in breathing, our muscular exertions are directed by our sensations and perceptions: yet in such processes, how dimly are we conscious of perceptive and directive power! How the mind should be able to exercise such a power, and yet should be scarcely or not at all conscious of its exercise, is a very curious problem. But in all or in most of the instances just mentioned, the solution of this problem appears to depend upon psychological rather than biological principles, and therefore does not belong to this place.

21. But in cases at the other extreme (unconscious actions) the mental part of the operation vanishes altogether. In many animals, even after decapitation, the limbs shrink when irritated. The motions of the iris are determined by the influence of light on our eyes, without our being aware of the motions. Here Sensations produce Motions, but with no trace of intervening Perception or Will. The Sensation appears to be *reflected* back from the central element of animal life, in the form of a Muscular Contraction; but in this case the Sensation is not modified or regulated by any *Idea*. These reflected motions have no reference to relations of space or force among surrounding objects. They are blind and involuntary, like the movements of convulsion, depending for direction and amount only on the position and circumstances of the limb itself with its muscles. Here the Centre from which the reflection takes place is merely *animal*, not intellectual.

In this case some physiologists have

doubted whether the reflection of the sensation in the form of a muscular 236 contraction does really take place from the Center; and have conceived that sensorial impressions might affect motor nerves without any communication with the nervous Center. But on this subject we may, I conceive, with safety adopt the decision of Professor Müller, deliberately given after a careful examination of the subject: 'When impressions made by the action of external stimuli on sensitive nerves give rise to motions in other parts, these motions are never the result of the *direct* reaction of the sensitive and motor fibres of the nerves on each other; the irritation is conveyed by the sensitive fibres to the brain and spinal cord, and is by these communicated to the motor fibres.'

22. Thus we have two extreme cases of the connexion of sensation with muscular action; in one of which the connexion clearly *is*, and in the other it as clearly *is not*, determined by relations of Ideas, in its transit through the nervous Center. There is

another highly curious case standing intermediate between these two, and extremely difficult to refer to either. I speak of the case of *Instinct*.

Instinct leads to actions which are *such as if they were determined by Ideas*. The lamb follows its mother by instinct; but the motions by which it does this, the special muscular exertions, depend entirely upon the geometrical and mechanical relations of external bodies, as the form of the ground, and the force of the wind. The contractions of the muscles which are requisite in order that the creature may obey its instinct, vary with every variation of these external conditions;—are not determined by any rule or necessity, but by properties of Space and Force. Thus the action is not governed by Sensations directly, but by sensations moulded by Ideas. And the same is the case with other cases of instinct. The dog hunts by instinct; but he hunts certain kinds of animals merely, thus showing that his instinct acts according to Resemblances and Differences; he crosses the field repeatedly

to find the track of his prey by scent; thus recognizing the relations of Space with reference to the track; he leaps, adjusting his Force to 237 the distance and height of the leap with mechanical precision; and thus he practically recognizes the Ideas of Resemblance, Space, and Force.

But have animals such Ideas? In any proper sense in which we can speak of possessing Ideas, it appears plain that they have not. Animals cannot, at any time, be said properly to possess ideas, for ideas imply the possibility of *speculative* knowledge.

23. But even if we allow to animals only the *practical* possession of Ideas, we have still a great difficulty remaining. In the case of man, his ideas are unfolded gradually by his intercourse with the external world. The child learns to distinguish forms and positions by a repeated and incessant use of his hands and eyes; he learns to walk, to run, to leap, by slow and laborious degrees; he distinguishes one man from another, and

one animal from another, only after repeated mistakes. Nor can we conceive this to be otherwise. How should the child know at once what muscles he is to exert in order to touch with his hand a certain visible object? How should he know what muscles to exert that he may stand and not fall, till he has tried often? How should he learn to direct his attention to the differences of different faces and persons, till he is roused by some memory, or hope which implies memory? It seems to us as if the sensations could not, without considerable practice, be rightly referred to Ideas of Space, Force, Resemblance, and the like.

Yet that which thus appears impossible, is in fact done by animals. The lamb almost immediately after its birth follows its mother, accommodating the actions of its muscles to the form of the ground. The chick, just escaped from the shell, picks up a minute insect, directing its beak with the greatest accuracy. Even the human infant seeks the breast and exerts its muscles in sucking, almost as soon as it is born. Hence,

then, we see that Instinct produces at once actions regulated by Ideas, or, at least, which take place *as if* they were regulated by Ideas; although the Ideas cannot have been developed by exercise, and only appear to exist so far as such actions are concerned. 238

24. The term *Instinct* may properly be opposed to *Insight*. The former implies an inward principle of action, implanted within a creature and practically impelling it, but not capable of being developed into a subject of contemplation. While the instinctive actions of animals are directed by such a principle, the deliberate actions of man are governed by insight: he can contemplate the ideal relations on which the result of his action depends. He can in his mind map the path he will follow, and estimate the force he will exert, and class the objects he has to deal with, and determine his actions by the relations which he thus has present to his mind. He thus possesses Ideas not only practically, but speculatively. And knowing that the Ideas

by which he commonly directs his actions, Space, Cause, Resemblance, and the like, have been developed to that degree of clearness in which he possesses them by the assiduous exercise of the senses and the mind from the earliest stage of infancy, and that these Ideas are capable of being still further unfolded into long trains of speculative truth, he is unable to conceive the manner in which animals possess such Ideas as their instinctive actions disclose:—Ideas which neither require to be unfolded nor admit of unfolding; which are adequate for practical purposes without any previous exercise, and inadequate for speculative purposes with whatever labour cultivated.

I have ventured to make these few remarks on Instinct since it may, perhaps, justly be considered as the last province of Biology, where we reach the boundary line of Psychology. I have now, before quitting this subject, only one other principle to speak of.

CHAPTER VI.

Of the Idea of Final Causes.

1. By an examination of those notions which enter into all our reasonings and judgments on living things, it appeared that we conceive animal life as a vortex or cycle of moving matter in which the form of the vortex determines the motions, and these motions again support the form of the vortex: the stationary parts circulate the fluids, and the fluids nourish the permanent parts. Each portion ministers to the others, each depends upon the other. The parts make up the whole, but the existence of the whole is essential to the preservation of the parts. But parts existing under such conditions are *organs*, and the whole is *organized*. This is the fundamental conception of organization. ‘Organized

beings,' says the physiologist[99], 'are composed of a number of essential and mutually dependent parts.' 'An organized product of nature,' says the great metaphysician[100], 'is that in which all the parts are mutually ends and means.'

[99] Müller, *Elem.* p. 18.

[100] Kant, *Urtheilskraft*, p. 296.

2. It will be observed that we do not content ourselves with saying that in such a whole, all the parts are *mutually dependent*. This might be true even of a mechanical structure; it would be easy to imagine a framework in which each part should be necessary to the support of each of the others; for example, an arch of several stones. But in such a structure, the parts have no properties which they derive from the whole. They are beams or stones when separate; they are no more when joined. But the same is not the case in an organized whole. The limb of an animal separated 240 from the body, loses the properties of a limb, and soon ceases to retain even its

form.

3. Nor do we content ourselves with saying that the parts are *mutually causes and effects*. This is the case in machinery. In a clock, the pendulum by means of the escapement causes the descent of the weight, the weight by the same escapement keeps up the motion of the pendulum. But things of this kind may happen by accident. Stones slide from a rock down the side of a hill and cause it to be smooth; the smoothness of the slope causes stones still to slide. Yet no one would call such a slide an organized system. The system is organized, when the effects which take place among the parts are *essential to our conception of the whole*; when the whole would not *be* a whole, nor the parts, parts, except these effects were produced; when the effects not only happen in fact, but are included in the idea of the object; when they are not only seen, but foreseen; not only expected, but intended: in short when, instead of being causes and effects, they are *ends* and *means*, as they are termed in the above definition.

Thus we necessarily include, in our Idea of Organization, the notion of an End, a Purpose, a Design; or, to use another phrase which has been peculiarly appropriated in this case, a *Final Cause*. This idea of a Final Cause is an essential condition in order to the pursuing our researches respecting organized bodies.

4. This Idea of Final Cause is not *deduced* from the phenomena by reasoning, but is *assumed* as the only condition under which we can reason on such subjects at all. We do not deduce the Idea of Space, or Time, or efficient Cause from the phenomena about us, but necessarily look at phenomena as subordinate to these Ideas from the beginning of our reasoning. It is true, our ideas of relations of Space, and Time, and Force, may become much more clear by our familiarizing ourselves with particular phenomena: but still, the Fundamental Ideas are not generated, but unfolded; not extracted from the external world, but evolved from the world within. In like manner, in the contemplation of organic

structures, we consider 241 each part as subservient to some use, and we cannot study the structure as organic without such a conception. This notion of adaptation,—this Idea of an End,—may become much more clear and impressive by seeing it exemplified in particular cases. But still, though suggested and evoked by special cases, it is not furnished by them. If it be not supplied by the mind itself, it can never be logically deduced from the phenomena. It is not a portion of the facts which we study, but it is a principle which connects, includes, and renders them intelligible; as our other Fundamental Ideas do the classes of facts to which they respectively apply.

5. This has already been confirmed by reference to fact; in the History of Physiology, I have shown that those who studied the structure of animals were irresistibly led to the conviction that the parts of this structure have each its end or purpose;—that each member and organ not merely produces a certain effect or answers a certain use, but is so framed as to impress

us with the persuasion that it was constructed *for* that use:—that it was *intended* to produce the effect. It was there seen that this persuasion was repeatedly expressed in the most emphatic manner by Galen;—that it directed the researches and led to the discoveries of Harvey;—that it has always been dwelt upon as a favourite contemplation, and followed as a certain guide, by the best anatomists;—and that it is inculcated by the physiologists of the profoundest views and most extensive knowledge of our own time. All these persons have deemed it a most certain and important principle of physiology, that in every organized structure, plant or animal, each intelligible part has its allotted office:—each organ is designed for its appropriate function:—that nature, in these cases, produces nothing in vain: that, in short, each portion of the whole arrangement has its *final cause*; an End to which it is adapted, and in this End, the reason that it is where and what it is.

6. This Notion of Design in organized

bodies must, I say, be supplied by the student of organization out of his own mind: a truth which will become clearer if 242 we attend to the most conspicuous and acknowledged instances of *design*. The structure of the Eye, in which the parts are curiously adjusted so as to produce a distinct image on the retina, as in an optical instrument;—the Trochlear Muscle of the eye, in which the tendon passes round a support and turns back, like a rope round a pulley;—the prospective contrivances for the preservation of animals, provided long before they are wanted, as the Milk of the mother, the Teeth of the child, the Eyes and Lungs of the fœtus:—these arrangements, and innumerable others, call up in us a persuasion that Design has entered into the plan of animal form and progress. And if we bring in our minds this conception of Design, nothing can more fully square with and fit it, than such instances as these. But if we did not already possess the Idea of Design;—if we had not had our notion of mechanical contrivance awakened by inspection of optical instruments, or pulleys,

or in some other way:—if we had never been conscious ourselves of providing for the future;—if this were the case, we could not recognize contrivance and prospectiveness in such instances as we have referred to. The facts are, indeed, admirably in accordance with these conceptions, when the two are brought together: but the facts and the conceptions come together from different quarters—from without and from within.

7. We may further illustrate this point by referring to the relations of travellers who tell us that when consummate examples of human mechanical contrivance have been set before savages, they have appeared incapable of apprehending them as proofs of design. This shows that in such cases the Idea of Design had not been developed in the minds of the people who were thus unintelligent: but it no more proves that such an idea does not naturally and necessarily arise, in the progress of men's minds, than the confused manner in which the same savages apprehend the relations of

space, or number, or cause, proves that these ideas do not naturally belong to their intellects. All men have these ideas; and it is because they 243 cannot help referring their sensations to such ideas, that they apprehend the world as existing in time and space, and as a series of causes and effects. It would be very erroneous to say that the belief of such truths is obtained by logical reasoning from facts. And in like manner we cannot logically deduce design from the contemplation of organic structures; although it is impossible for us, when the facts are clearly before us, not to find a reference to design operating in our minds.

8. Again; the evidence of the doctrine of Final Causes as a fundamental principle of Biology may be obscured and weakened in some minds by the constant habit of viewing this doctrine with suspicion as unphilosophical and at variance with Morphology. By cherishing such views, it is probable that many persons, physiologists and others, have gradually brought themselves to suppose that many or most of

the arrangements which are familiarly adduced as instances of design may be accounted for, or explained away;—that there is a certain degree of prejudice and narrowness of comprehension in that lively admiration of the adaptation of means to ends which common minds derive from the spectacle of organic arrangements. And yet, even in persons accustomed to these views, the strong and natural influence of the Idea of a Final Cause, the spontaneous recognition of the relation of Means to an End as the assumption which makes organic arrangements intelligible, breaks forth when we bring before them a new case, with regard to which their genuine convictions have not yet been modified by their intellectual habits. I will offer, as an example which may serve to illustrate this, the discoveries recently made with regard to the process of Suckling in the Kangaroo. In the case of this, as of other pouched animals, the young animal is removed, while very small and imperfectly formed, from the womb to the pouch, in which the teats are, and is there placed with its lips

against one of the nipples. But the young animal taken altogether is not so large as the nipple, and is therefore incapable of sucking after the manner of common mammals. Here is a difficulty: 244 how is it overcome?—By an appropriate *contrivance*: the nipple, which in common mammals is not furnished with any muscle, is in the kangaroo provided with a powerful extrusory muscle by which the mother can inject the milk into the mouth of her offspring. And again; in order to give attachment to this muscle there is a bone which is not found in animals of other kinds. But this mode of solving the problem of suckling so small a creature introduces another difficulty. If the milk is injected into the mouth of the young one, without any action of its own muscles, what is to prevent the fluid entering the windpipe and producing suffocation? How is this danger avoided?—By another appropriate *contrivance*: there is a funnel in the back of the throat by which the air passage is completely separated from the passage for nutriment, and the injected milk passes in a

divided stream on each side of the larynx to the œsophagus[101]. And as if to show that this apparatus is really formed with a view to the wants of the young one, the structure alters in the course of the animal's growth; and the funnel, no longer needed, is modified and disappears.

[101] Mr. Owen, in *Phil. Trans.* 1834, p. 348.

9. With regard to this and similar examples, the remark which I would urge is this:—that no one, however prejudiced or unphilosophical he may in general deem the reference to Final Causes, can, at the first impression, help regarding this curious system of arrangement as the Means to an End. So contemplated, it becomes significant, intelligible, admirable: without such a principle, it is an unmeaning complexity, a collection of contradictions, producing an almost impossible result by a portentous conflict of chances. The parts of this apparatus cannot have produced one another: one part is in the mother; another part in the young one: without their harmony they could not be effective; but

nothing except design can operate to make them harmonious. They are *intended* to work together; and we cannot resist the conviction of this intention when the facts first come before us. Perhaps 245 there may hereafter be physiologists who, tracing the gradual development of the parts of which we have spoken, and the analogies which connect them with the structures of other animals, may think that this development, these analogies, account for the conformation we have described; and may hence think lightly of the explanation derived from the reference to Final Causes. Yet surely it is clear, on a calm consideration of the subject, that the latter explanation is not disturbed by the former; and that the observer's first impression, that this is 'an irrefragable evidence of creative foresight[102],' can never be obliterated; however much it may be obscured in the minds of those who confuse this view by mixing it with others which are utterly heterogeneous to it, and therefore cannot be contradictory.

[102] Mr. Owen, in *Phil. Trans.* 1834, p. 349.

10. I have elsewhere[103] remarked how physiologists, who thus look with suspicion and dislike upon the introduction of Final Causes into physiology, have still been unable to exclude from their speculations causes of this kind. Thus Cabanis says[104], 'I regard with the great Bacon, the philosophy of Final Causes as sterile; but I have elsewhere acknowledged that it was very difficult for the most cautious man never to have recourse to them in his explanations.' Accordingly, he says, 'The partisans of Final Causes nowhere find arguments so strong in favour of their way of looking at nature as in the laws which preside and the circumstances of all kinds which concur in the reproduction of living races. In no case do the means employed appear so clearly relative to the end.' And it would be easy to find similar acknowledgments, express or virtual, in other writers of the same kind. Thus Bichat, after noting the difference between the organic sensibility by which the organs are made to perform their

offices, and the animal sensibility of which the 246 nervous center is the seat, says[105], 'No doubt it will be asked, *why*'—that is, as we shall see, for what *end*—'the organs of internal life have received from nature an inferior degree of sensibility only, and why they do not transmit to the brain the impressions which they receive, while all the acts of the animal life imply this transmission? The reason is simply this, that all the phenomena which establish our connexions with surrounding objects *ought to be*, and are in fact, under the influence of the Will; while all those which serve for the purpose of assimilation only, escape, and *ought* indeed to escape, such influence.' The *reason* here assigned is the Final Cause; which, as Bichat justly says, we cannot help asking for.

[103] *Bridgewater Treatise*, p. 352.

[104] *Rapports du Physique et du Moral*, i. 299.

[105] *Life and Death*, (trans.) p. 32.

11. Again; I may quote from the writer last mentioned another remark, which

shows that in the organical sciences, and in them alone, the Idea of forces as Means acting to an End, is inevitably assumed and acknowledged as of supreme authority. In Biology alone, observes Bichat[106], have we to contemplate the state of *Disease*. 'Physiology is to the movements of living bodies, what astronomy, dynamics, hydraulics, &e., are to those of inert matter: but these latter sciences have no branches which correspond to them as Pathology corresponds to Physiology. For the same reason all notion of a Medicament is repugnant to the physical sciences. A Medicament has for its object to bring the properties of the system back to their Natural Type; but the physical properties never depart from this Type, and have no need to be brought back to it: and thus there is nothing in the physical sciences which holds the place of Therapeutick in Physiology.' Or, as we might express it otherwise, of inert forces we have no conception of what they *ought to do*, except what they *do*. The forces of gravity, elasticity, affinity, never act in a *diseased*

manner; we never conceive them as failing in their purpose; for we do not conceive them as having any purpose which is answered by one mode of their action rather than 247 another. But with *organical* forces the case is different; they are necessarily conceived as acting for the preservation and development of the system in which they reside. If they do not do this, they fail, they are deranged, diseased. They have for their object to conform the living being to a certain type; and if they cause or allow it to deviate from this type, their action is distorted, morbid, contrary to the ends of nature. And thus this conception of organized beings as susceptible of disease, implies the recognition of a state of health, and of the organs and the vital forces as means for preserving this normal condition. The state of health, and of perpetual development, is necessarily contemplated as the Final Cause of the processes and powers with which the different parts of plants and animals are endowed.

[106] *Anatomie Générale*, i. liii.

12. This Idea of a Final Cause is applicable as a fundamental and regulative idea to our speculations concerning organized creatures only. That there is a purpose in many other parts of the creation, we find abundant reason to believe, from the arrangements and laws which prevail around us. But this persuasion is not to be allowed to regulate and direct our reasonings with regard to inorganic matter, of which conception the relation of means and end forms no essential part. In mere Physics, Final Causes, as Bacon has observed, are not to be admitted as a principle of reasoning. But in the organical sciences, the assumption of design and purpose in every part of every whole, that is, the pervading idea of Final Cause, is the basis of sound reasoning and the source of true doctrine.

13. The Idea of Final Cause, of end, purpose, design, intention, is altogether different from the Idea of Cause, as Efficient Cause, which we formerly had to consider; and on this account the use of the word

Cause in this phrase has been objected to. If the idea be clearly entertained and steadily applied, the word is a question of subordinate importance. The term Final Cause has been long familiarly used, and appears not likely to lead to confusion. 248

14. The consideration of Final Causes, both in physiology and in other subjects, has at all times attracted much attention, in consequence of its bearing upon the belief of an Intelligent Author of the Universe. I do not intend, in this place, to pursue the subject far in this view: but there is one antithesis of opinion, already noticed in the History of Physiology, on which I will again make a few remarks[107].

[107] *Hist. Ind. Sc.* b. xvii. c. viii. On the Doctrine of Final Causes in Physiology.

It has appeared to some persons that the mere aspect of order and symmetry in the works of nature—the contemplation of comprehensive and consistent law—is sufficient to lead us to the conception of a design and intelligence producing the order

and carrying into effect the law. Without here attempting to decide whether this is true, we may discern, after what has been said, that the conception of Design, arrived at in this manner, is altogether different from that Idea of Design which is suggested to us by organized bodies, and which we describe as the doctrine of Final Causes. The regular form of a crystal, whatever beautiful symmetry it may exhibit, whatever general laws it may exemplify, does not prove design in the same manner in which design is proved by the provisions for the preservation and growth of the seeds of plants, and of the young of animals. The law of universal gravitation, however wide and simple, does not impress us with the belief of a purpose, as does that propensity by which the two sexes of each animal are brought together. If it could be shown that the symmetrical structure of a flower results from laws of the same kind as those which determine the regular forms of crystals, or the motions of the planets, the discovery might be very striking and important, but it would not at all come under our idea of

Final Cause.

15. Accordingly, there have been, in modern times, two different schools of physiologists, the one proceeding upon the idea of Final Causes, the other school 249 seeking in the realm of organized bodies wide laws and analogies from which that idea is excluded. All the great biologists of preceding times, and some of the greatest of modern times, have belonged to the former school; and especially Cuvier, who may be considered as the head of it. It was solely by the assiduous application of this principle of Final Cause, as he himself constantly declared, that he was enabled to make the discoveries which have rendered his name so illustrious, and which contain a far larger portion of important anatomical and biological truth than it ever before fell to the lot of one man to contribute to the science.

The opinions which have been put in opposition to the principle of Final Causes have, for the most part, been stated vaguely

and ambiguously. Among the most definite of such principles, is that which, in the History of the subject, I have termed the Principle of Metamorphosed and Developed Symmetry, upon which has been founded the science of Morphology.

The reality and importance of this principle are not to be denied by us: we have shown how they are proved by its application in various sciences, and especially in botany. But those advocates of this principle who have placed it in antithesis to the doctrine of Final Causes, have, by this means, done far more injustice to their own favourite doctrine than damage to the one which they opposed. The adaptation of the bones of the skeleton to the muscles, the provision of fulcrums, projecting processes, channels, so that the motions and forces shall be such as the needs of life require, cannot possibly become less striking and convincing, from any discovery of general analogies of one animal frame with another, or of laws connecting the development of different

parts. Whenever such laws are discovered, we can only consider them as the means of producing that adaptation which we so much admire. Our conviction that the Artist works intelligently, is not destroyed, though it may be modified and transferred, when we obtain a sight of his tools. Our discovery of laws cannot contradict our persuasion of ends; our Morphology cannot prejudice our Teleology. 250

16. The irresistible and constant apprehension of a purpose in the forms and functions of animals has introduced into the writings of speculators on these subjects various forms of expression, more or less precise, more or less figurative; as, that ‘animals are framed with a view to the part which they have to play;’—that ‘nature does nothing in vain;’ that ‘she employs the best means for her ends;’ and the like. However metaphorical or inexact any of these phrases may be in particular, yet taken altogether, they convey, clearly and definitely enough to preclude any serious errour, a principle of the most profound reality and of the

highest importance in the organical sciences. But some adherents of the morphological school of which 1 have spoken reject, and even ridicule, all such modes of expression. ‘I know nothing,’ says M. Geoffroy Saint Hilaire, ‘of animals which have to play a part in nature. I cannot make of nature an intelligent being who does nothing in vain; who acts by the shortest mode; who does all for the best.’ The philosophers of this school, therefore, do not, it would seem, feel any of the admiration which is irresistibly excited in all the rest of mankind at the contemplation of the various and wonderful adaptations for the preservation, the enjoyment, the continuation of the creatures which people the globe;—at the survey of the mechanical contrivances, the chemical agencies, the prospective arrangements, the compensations, the minute adaptations, the comprehensive interdependencies, which zoology and physiology have brought into view, more and more, the further their researches have been carried. Yet the clear and deep-seated conviction of the reality of

these provisions, which the study of anatomy produces in its most profound and accurate cultivators, cannot be shaken by any objections to the metaphors or terms in which this conviction is clothed. In regard to the Idea of a Purpose in organization, as in regard to any other idea, we cannot fully express our meaning by phrases borrowed from any extraneous source; but that impossibility arises precisely from the circumstance of its being a Fundamental Idea which is inevitably assumed in our 251 representation of each special fact. The same objection has been made to the idea of mechanical *force*, on account of its being often expressed in metaphorical language; for writers have spoken of an *energy*, *effort*, or *solicitation* to motion; and bodies have been said to be *animated* by a force. Such language, it has been urged, implies volition, and the act of animated beings. But the idea of Force as distinct from mere motion,—as the Cause of motion, or of tendency to motion,—is not on that account less real. We endeavour in vain to conduct our mechanical reasonings without the aid

of this idea, and must express it as we can. Just as little can we reason concerning organized beings without assuming that each part has its function, each function its purpose; and so far as our phrases imply this, they will not mislead us, however inexact, or however figurative they be.

17. The doctrine of a purpose in Organization has been sometimes called the doctrine of *the Conditions of Existence*; and has been stated as teaching that each animal must be so framed as to contain in its structure the Conditions which its existence requires. When expressed in this manner, it has given rise to the objection, that it merely offers an identical proposition; since no animal can exist without such conditions. But in reality, such expressions as those just quoted give an inadequate statement of the Principle of a Final Cause. For we discover in innumerable cases, arrangements in an animal, of which we see, indeed, that they are subservient to its well being; but the nature of which we never should have been able at all to

conjecture, from considering what was necessary to its existence, and which strike us, no less by their unexpectedness than by their adaptation: so far are they from being presented by any perceptible necessity. Who would venture to say that the trochlear muscle, or the power of articulate speech, must occur in man, because they are the necessary conditions of his existence? When, indeed, the general scheme and mode of being of an animal are known, the expert and profound anatomist can reason concerning the proportions and 252 form of its various parts and organs, and prove in some measure what their relations must be. We can assert, with Cuvier, that certain forms of the viscera require certain forms of the teeth, certain forms of the limbs, certain powers of the senses. But in all this, the functions of self-nutrition and digestion are supposed already existing as ends: and it being taken for granted, as the only conceivable basis of reasoning, that the organs are means to these ends, we may discover what modifications of these organs are necessarily related to and connected

with each other. Instead of terming this rule of speculation merely ‘the Principle of the Conditions of Existence,’ we might term it ‘the Principle of the conditions of organs as *Means* adapted to animal existence as their *End*.’ And how far this principle is from being a mere barren truism, the extraordinary discoveries made by the great assertor of the principle, and universally assented to by naturalists, abundantly prove. The vast extinct creation which is recalled to life in Cuvier’s great work, the *Ossemens Fossiles*, cannot be the consequence of a mere identical proposition.

18. It has been objected, also, that the doctrine of Final Causes supposes us to be acquainted with the intentions of the Creator; which, it is insinuated, is a most presumptuous and irrational basis for our reasonings. But there can be nothing presumptuous or irrational in reasoning on that basis, which if we reject, we cannot reason at all. If men really can discern, and cannot help discerning, a design in certain

portions of the works of creation, this perception is the soundest and most satisfactory ground for the convictions to which it leads. The Ideas which we necessarily employ in the contemplation of the world around us, afford us the only natural means of forming any conception of the Creator and Governor of the Universe; and if we are by such means enabled to elevate our thoughts, however inadequately, towards Him, where is the presumption of doing so? or rather, where is the wisdom of refusing to open our minds to contemplations so animating and elevating, and yet 253 so entirely convincing? We possess the ideas of Time and Space, under which all the objects of the universe present themselves to us; and in virtue of these ideas thus possessed, we believe the Creator to be eternal and omnipotent. When we find that we, in like manner, possess the idea of a Design in Creation, and that with regard to ourselves, and creatures more or less resembling ourselves, we cannot but contemplate their constitution under this idea, we cannot abstain from ascribing to

the Creator the infinite profundity and extent of design to which all these special instances belong as parts of a whole.

19. I have here considered Design as manifest in organization only: for in that field of speculation it is forced upon us as contained in all the phenomena, and as the only mode of our understanding them. The existence of Final Causes has often been pointed out in other portions of the creation;—for instance, in the apparent adaptations of the various parts of the earth and of the solar system to each other and to organized beings. In these provinces of speculation, however, the principle of Final Causes is no longer the basis and guide, but the sequel and result of our physical reasonings. If in looking at the universe, we follow the widest analogies of which we obtain a view, we see, however dimly, reason to believe that all its laws are adapted to each other, and intended to work together for the benefit of its organic population, and for the general welfare of its rational tenants. On this subject,

however, not immediately included in the principle of Final Causes as here stated, I shall not dwell. I will only make this remark; that the assertion appears to be quite unfounded, that as science advances from point to point, Final Causes recede before it, and disappear one after the other. The principle of design changes its mode of application indeed, but it loses none of its force. We no longer consider particular facts as produced by special interpositions, but we consider design as exhibited in the establishment and adjustment of the laws by which particular facts are produced. We do not look upon each particular 254 cloud as brought near us that it may drop fatness on our fields; but the general adaptation of the laws of heat, and air, and moisture, to the promotion of vegetation, does not become doubtful. We do not consider the sun as less intended to warm and vivify the tribes of plants and animals, because we find that, instead of revolving round the earth as an attendant, the earth along with other planets revolves round him. We are rather, by the discovery of the general laws of

nature, led into a scene of wider design, of deeper contrivance, of more comprehensive adjustments. Final causes, if they appear driven further from us by such an extension of our views, embrace us only with a vaster and more majestic circuit: instead of a few threads' connecting some detached objects, they become a stupendous net-work, which is wound round and round the universal frame of things.

20. I now quit the subject of Biology, and with it the circle of sciences depending upon separate original Ideas and permanent relations. If from the general relations which permanently prevail and constantly recur among the objects around us, we turn to the inquiry of what has actually happened,—if from Science we turn to History,—we find ourselves in a new field. In this region of speculation we can rarely obtain a complete and scientific view of the connexion between objects and events. The past History of Man, of the Arts, of Languages, of the Earth, of the Solar System, offers a vast series of problems, of

which perhaps not one has been rigorously solved. Still, man, as his speculative powers unfold themselves, cannot but feel prompted and invited to employ his thoughts even on these problems. He cannot but wish and endeavour to understand the connexion between the successive links of such chains of events. He attempts to form a Science which shall be applicable to each of these Histories; and thus he begins to construct the class of sciences to which I now, in the last place, proceed.

BOOK X.

THE

PHILOSOPHY

OF

PALÆTIOLOGY.

Τὴν μὲν οὖν τοιαύτην Αἰτιολογίαν ἧττον ἄν τις ἀποδέξαιτο· μᾶλλον δ᾽ ἀπὸ τῶν φανερωτέρων καὶ τῶν καθ᾽ ἡμέραν τρόπον τινὰ ὁρωμένων ἀναπτέον τὸν λόγον. Καὶ γὰρ κατακλυσμοὶ, καὶ σεισμοὶ, καὶ ἀναφυσήματα, καὶ ἀνοιδήσεις τῆς ὑφάλου γῆς, μετεωρίζουσι καὶ τὴν θάλατταν· αἱ δὲ συνιζήσεις ταπεινοῦσιν αὐτήν.

STRABO, *Geogr.* 1. p. 54.

IT is therefore, not so much what these forms of the earth actually are, as what they are

> continually becoming, that we have to observe; nor is it possible thus to observe them without an instinctive reference to the first state out of which they have been brought.... Yet to such questions continually suggesting themselves, it is never possible to give a complete answer. For a certain distance, the past work of existing forces can be traced; but then gradually the mist gathers, and the footsteps of more gigantic agencies are traceable in the darkness; and still as we endeavour to penetrate further and further into departed time, the thunder of the Almighty power sounds louder and louder, and the clouds gather broader and more fearfully, until at last the Sinai of the world is seen altogether upon a smoke, and the fence of its foot is reached, where none can break through.

RUSKIN, *Modern Painters*, Vol. IV. p. 143.

BOOK X.

THE PHILOSOPHY OF PALÆTIOLOGY.

CHAPTER I.

Of Palætiological Sciences in General.

1. I HAVE already stated in the *History of the Sciences*[1], that the class of Sciences which I designate as *Palætiological* are those in which the object is to ascend from the present state of things to a more ancient condition, from which the present is derived by intelligible causes. As conspicuous examples of this class we may take Geology,

Glossology or Comparative Philology, and Comparative Archæology. These provinces of knowledge might perhaps be intelligibly described as *Histories*; the History of the Earth,—the History of Languages,—the History of Arts. But these phrases would not fully describe the sciences we have in view; for the object to which we now suppose their investigations to be directed is, not merely to ascertain what the series of events has been, as in the common forms of History, but also how it has been brought about. These sciences are to treat of causes as well as of effects. Such researches might be termed *Philosophical History*; or, in order to mark more distinctly that the *causes* of events are the leading object of attention, *Ætiological History*. But since 258 it will be more convenient to describe this class of sciences by a single appellation, I have taken the liberty of proposing to call them[2] the *Palætiological* Sciences.

[1] B. xviii. Introd.

[2] A philological writer, in a very interesting work (Mr. Donaldson, in his *New Cratylus*, p. 12), expresses his dislike

> of this word, and suggests that I must mean *palæ-ætiological*. I think the word is more likely to obtain currency in the more compact and euphonious form in which I have used it. It has been adopted by Mr. Winning, in his *Manual of Comparative Philology*, and more recently, by other writers.

While Palæontology describes the beings which have lived in former ages without investigating their causes, and *Ætiology* treats of causes without distinguishing historical from mechanical causation; *Palætiology* is a combination of the two sciences; exploring, by means of the second, the phenomena presented by the first. The portions of knowledge which I include in this term are palæontological ætiological sciences.

2. All these sciences are connected by this bond;—that they all endeavour to ascend to a past state, by considering what is the present state of things, and what are the causes of change. Geology examines the existing appearance of the materials which form the earth, infers from them previous conditions, and speculates concerning the

forces by which one condition has been made to succeed another. Another science, cultivated with great zeal and success in modern times, compares the languages of different countries and nations, and by an examination of their materials and structure, endeavours to determine their descent from one another: this science has been termed *Comparative Philology*, or *Ethnography*; and by the French, *Linguistique*, a word which we might imitate in order to have a single name for the science, but the Greek derivative *Glossology* appears to be more convenient in its form. The progress of the Arts (Architecture and the like);—how one stage of the culture produced another; and how far we can trace their maturest and most complete condition to their earliest form in various nations;—are problems of great interest belonging to another subject, which we may for the present term 259 *Comparative Archæology*. I have already noticed, in the History[3] how the researches into the origin of natural objects, and those relating to works of art, pass by slight gradations into each other; how the

examination of the changes which have affected an ancient temple or fortress, harbour or river, may concern alike the geologist and the antiquary. Cuvier's assertion that the geologist is an antiquary of a new order, is perfectly correct, for both are palætiologists.

[3] B. xviii. Introd.

3. We are very far from having exhausted, by this enumeration, the class of sciences which are thus connected. We may easily point out many other subjects of speculation of the same kind. As we may look back towards the first condition of our planet, we may in like manner turn our thoughts towards the first condition of the solar system, and try whether we can discern any traces of an order of things antecedent to that which is now established; and if we find, as some great mathematicians have conceived, indications of an earlier state in which the planets were not yet gathered into their present forms, we have, in the pursuit of this train of research, a palætiological portion of Astronomy. Again,

as we may inquire how languages, and how man, have been diffused over the earth's surface from place to place, we may make the like inquiry with regard to the races of plants and animals, founding our inferences upon the existing geographical distribution of the animal and vegetable kingdoms: and thus the Geography of Plants and of Animals also becomes a portion of Palætiology. Again, as we can in some measure trace the progress of Arts from nation to nation and from age to age, we can also pursue a similar investigation with respect to the progress of Mythology, of Poetry, of Government, of Law. Thus the philosophical history of the human race, viewed with reference to these subjects, if it can give rise to knowledge so exact as to be properly called Science, will supply Sciences belonging to the class I am now to consider. 260

4. It is not an arbitrary and useless proceeding to construct such a Class of Sciences. For wide and various as their subjects are, it will be found that they have

all certain principles, maxims, and rules of procedure in common; and thus may reflect light upon each other by being treated of together. Indeed it will, I trust, appear, that we may by such a juxtaposition of different speculations, obtain most salutary lessons. And questions, which, when viewed as they first present themselves under the aspect of a special science, disturb and alarm men's minds, may perhaps be contemplated more calmly, as well as more clearly, when they are considered as general problems of palætiology.

5. It will at once occur to the reader that, if we include in the circuit of our classification such subjects as have been mentioned,—politics and law, mythology and poetry,—we are travelling very far beyond the material sciences within whose limits we at the outset proposed to confine our discussion of principles. But we shall remain faithful to our original plan; and for that purpose shall confine ourselves, in this work, to those palætiological sciences which deal with material things. It is true, that the

general principles and maxims which regulate these sciences apply also to investigations of a parallel kind respecting the products which result from man's imaginative and social endowments. But although there may be a similarity in the general form of such portions of knowledge, their materials are so different from those with which we have been hitherto dealing, that we cannot hope to take them into our present account with any profit. Language, Government, Law, Poetry, Art, embrace a number of peculiar Fundamental Ideas, hitherto not touched upon in the disquisitions in which we have been engaged; and most of them involved in far greater perplexity and ambiguity, the subject of controversies far more vehement, than the Ideas we have hitherto been examining. We must therefore avoid resting any part of our philosophy upon sciences, or supposed sciences, which treat of such subjects. To attend to this caution, 261 is the only way in which we can secure the advantage we proposed to ourselves at the outset, of taking, as the basis of our

speculations, none but systems of undisputed truths, clearly understood and expressed[4]. We have already said that we must, knowingly and voluntarily, resign that livelier and warmer interest which doctrines on subjects of Polity or Art possess, and content ourselves with the cold truths of the material sciences, in order that we may avoid having the very foundations of our philosophy involved in controversy, doubt, and obscurity.

[4] See Introd. p. 9.

6. We may remark, however, that the necessity of rejecting from our survey a large portion of the researches which the general notion of Palætiology includes, suggests one consideration which adds to the interest of our task. We began our inquiry with the trust that any sound views which we should be able to obtain respecting the nature of Truth in the physical sciences, and the mode of discovering it, must also tend to throw light upon the nature and prospects of knowledge of all other kinds;—must be useful to us in

moral, political, and philological researches. We stated this as a confident anticipation; and the evidence of the justice of our belief already begins to appear. We have seen, in the last Book, that biology leads us to psychology, if we choose to follow the path; and thus the passage from the material to the immaterial has already unfolded itself at one point; and we now perceive that there are several large provinces of speculation which concern subjects belonging to man's immaterial nature, and which are governed by the same laws as sciences altogether physical. It is not our business here to dwell on the prospects which our philosophy thus opens to our contemplation; but we may allow ourselves, in this last stage of our pilgrimage among the foundations of the physical sciences, to be cheered and animated by the ray 262 that thus beams upon us, however dimly, from a higher and brighter region.

But in our reasonings and examples we shall mainly confine ourselves to the physical sciences; and for the most part to

Geology, which in the *History* I have put forwards as the best representative of the Palætiological Sciences.

CHAPTER II.

Of the Three Members of a Palætiological Science.

1. *Divisions of such Sciences.*—In each of the Sciences of this class we consider some particular order of phenomena now existing:—from our knowledge of the causes of change among such phenomena, we endeavour to infer the causes which have made this order of things what it is:—we ascend in this manner to some previous stage of such phenomena;—and from that, by a similar course of inference, to a still earlier stage, and to its causes. Hence it will be seen that each such science will consist of two parts,—the knowledge of the Phenomena, and the knowledge of their Causes. And such a division is, in fact, generally recognized in such sciences: thus

we have History, and the Philosophy of History; we have Comparison of Languages, and the Theories of the Origin and Progress of Language; we have Descriptive Geology, and Theoretical or Physical Geology. In all these cases, the relation between the two parts in these several provinces of knowledge is nearly the same; and it may, on some occasions at least, be useful to express the distinction in a uniform or general manner. The investigation of Causes has been termed *Ætiology* by philosophical writers, and this term we may use, in contradistinction to the mere *Phenomenology* of each such department of knowledge. And thus we should have *Phenomenal Geology* and *Ætiological Geology*, for the two divisions of the science which we have above termed *Descriptive* and *Theoretical Geology*.

2. *The Study of Causes.*—But our knowledge respecting the causes which actually *have* produced any 264 order of phenomena must be arrived at by ascertaining what the causes of change in

such matters *can* do. In order to learn, for example, what share earthquakes, and volcanoes, and the beating of the ocean against its shores, ought to have in our Theory of Geology, we must make out what effects these agents of change are able to produce. And this must be done, not hastily, or unsystematically, but in a careful and connected manner; in short, this study of the causes of change in each order of phenomena must become a distinct body of Science, which must include a large amount of knowledge, both comprehensive and precise, before it can be applied to the construction of a theory. We must have an Ætiology corresponding to each order of phenomena.

3. *Ætiology.*—In the History of Geology, I have spoken of the necessity for such an Ætiology with regard to geological phenomena: this necessity I have compared with that which, at the time of Kepler, required the formation of a separate science of Dynamics (the doctrine of the Causes of Motion), before Physical Astronomy could

grow out of Phenomenal Astronomy. In pursuance of this analogy, I have there given the name of *Geological Dynamics* to the science which treats of the causes of geological change in general. But, as I have there intimated, in a large portion of the subject the changes are so utterly different in their nature from any modification of motion, that the term *Dynamics*, so applied, sounds harsh and strange. For in this science we have to treat, not only of the subterraneous forces by which parts of the earth's crust are shaken, elevated, or ruptured, but also of the causes which may change the climate of a portion of the earth's surface, making a country hotter or colder than in former ages; again, we have to treat of the causes which modify the forms and habits of animals and vegetables, and of the extent to which the effects of such causes can proceed; whether, for instance, they can extinguish old species and produce new. These and other similar investigations would not be naturally included in the notion of *Dynamics*; and therefore it 265 might perhaps be better to

use the term *Ætiology* when we wish to group together all those researches which have it for their object to determine the laws of such changes. In the same manner the Comparison and History of Languages, if it is to lead to any stable and exact knowledge, must have appended to it an Ætiology, which aims at determining the nature and the amount of the causes which really do produce changes in language; as colonization, conquest, the mixture of races, civilization, literature, and the like. And the same rule applies to all sciences of this class. We shall now make a few remarks on the characteristics of such branches of science as those to which we are led by the above considerations.

4. *Phenomenology requires Classification. Phenomenal Geology.*—The Phenomenal portions of each science imply Classification, for no description of a large and varied mass of phenomena can be useful or intelligible without classification. A representation of phenomena, in order to answer the purposes of science, must be

systematic. Accordingly, in giving the History of Descriptive or Phenomenal Geology, I have called it *Systematic Geology*, just as Classificatory Botany is termed *Systematic Botany*. Moreover, as we have already seen, Classification can never be an arbitrary process, but always implies some natural connexion among the objects of the same Class; for if this connexion did not exist, the Classes could not be made the subjects of any true assertion. Yet though the classes of phenomena which our system acknowledges must be such as already exist in nature, the discovery of these classes is, for the most part, very far from obvious or easy. To detect the true principles of Natural Classes, and to select marks by which these may be recognized, are steps which require genius and good fortune, and which fall to the lot only of the most eminent persons in each science. In the History, I have pointed out Werner, William Smith, and Cuvier, as the three great authors of Systematic Geology of Europe. The mode of classifying the materials of the earth's surface which was found, by these

philosophers, fitted to 266 enunciate such general facts as came under their notice, was to consider the rocks and other materials as divided into successive layers or strata, superimposed one on another, and variously inclined and broken. The German geologist distinguished his strata for the most part by their mineralogical character; the other two, by the remains of animals and plants which the rocks contained. After a beginning had thus been made in giving a genuine scientific form to phenomenal geology, other steps followed in rapid succession, as has already been related in the History[5]. The Classification of the Strata was fixed by a suitable Nomenclature. Attempts were made to apply to other countries the order of strata which had been found to prevail in that first studied: and in this manner it was ascertained what rocks in distant regions are the synonyms, or *Equivalents*[6],—of each other. The knowledge thus collected and systematized was exhibited in the form of Geological Maps.

[5] *Hist. Ind. Sc.* b. xviii. c. iii.

[6] *Ib.* sect. 4.

Moreover, among the phenomena of geology we have Laws of Nature as well as Classes. The general form of mountain-chains; the relations of the direction and inclination of different chains to each other; the general features of mineral veins, faults, and fissures; the prevalent characters of slaty cleavage;—were the subjects of laws established, or supposed to be established, by extensive observation of facts. In like manner the organic fossils discovered in the strata were found to follow certain laws with reference to the climate which they appeared to have lived in; and the evidence which they gave of a regular zoological development. And thus, by the assiduous labours of many accomplished and active philosophers, Descriptive or Phenomenal Geology was carried towards a state of completeness.

5. *Phenomenal Uranography.*—In like manner in other palætiological researches, as soon as they approach to an exact and scientific form, we find the necessity of

constructing in the first place a science of 267 classification and exact description, by means of which the phenomena may be correctly represented and compared; and of obtaining by this step a solid basis for an inquiry into the causes which have produced them. Thus the Palætiology of the Solar System has, in recent times, drawn the attention of speculators; and a hypothesis has been started, that our sun and his attendant planets have been produced by the condensation of a mass of diffused matter, such as that which constitutes the nebulous patches which we observe in the starry heavens. But the sagest and most enlightened astronomers have not failed to acknowledge, that to verify or to disprove this conjecture, must be the work of many ages of observation and thought. They have perceived also that the first step of the labour requisite for the advancement of this portion of science must be to obtain and to record the most exact knowledge at present within our reach, respecting the phenomena of these nebulæ, with which we thus compare our own system; and, as a

necessary element of such knowledge, they have seen the importance of a classification of these objects, and of others, such as Double Stars, of the same kind. Sir William Herschel, who first perceived the bearing of the phenomena of nebulæ upon the history of the solar system, made the observation of such objects his business, with truly admirable zeal and skill; and in the account of the results of his labours, gave a classification of Nebulæ; separating them into, first, *Clusters of Stars*; second, *Resolvable Nebulæ*; third, *Proper Nebulæ*; fourth, *Planetary Nebulæ*; fifth, *Stellar Nebulæ*; sixth, *Nebulous Stars*[7]. And since, in order to obtain from these remote appearances, any probable knowledge respecting our own system, we must discover whether they undergo any changes in the course of ages, he devoted himself to the task of forming a record of their number and appearance in his own time, that thus the astronomers of succeeding generations might have a 268 definite and exact standard with which to compare their observations. Still, this task would have

been executed only for that part of the heavens which is visible in this country, if this Hipparchus of the Nebulæ and Double Stars had not left behind him a son who inherited all his father's zeal and more than his father's knowledge. Sir John Herschel in 1833 went to the Cape of Good Hope to complete what Sir William Herschel left wanting; and in the course of five years observed with care all the nebulæ and double stars of the Southern hemisphere. This great *Herschelian Survey of the Heavens*, the completion of which is the noblest monument ever erected by a son to a father, must necessarily be, to all ages, the basis of all speculations concerning the history and origin of the solar system; and has completed, so far as at present it can be completed, the phenomenal portion of Astronomical Palætiology.

[7] *Phil. Trans.* 1786 and 1789, and Sir J. Herschel's *Astronomy*, Art. 616.

6. *Phenomenal Geography of Plants and Animals.*—Again, there is another Palætiological Science, closely connected

with the speculations forced upon the geologist by the organic fossils which he discovers imbedded in the strata of the earth;—namely, the Science which has for its object the Causes of the Diffusion and Distribution of the various kinds of Plants and Animals. And the science also has for its first portion and indispensable foundation a description and classification of the existing phenomena. Such portions of science have recently been cultivated with great zeal and success, under the titles of the *Geography of Plants*, and the *Geography of Animals*. And the results of the inquiries thus undertaken have assumed a definite and scientific form by leading to a division of the earth's surface into a certain number of botanical and zoological *Provinces*, each province occupied by its own peculiar vegetable and animal population. We find, too, in the course of these investigations, various general laws of the phenomena offered to our notice; such, for instance, as this:—that the difference of the animals originally occupying each province, which is clear and entire for the higher orders of 269 animals

and plants, becomes more doubtful and indistinct when we descend to the lower kinds of organizations; as Infusoria and Zoophytes[8] in the animal kingdom, Grasses and Mosses among vegetables. Again, other laws discovered by those who have studied the geography of plants are these:—that countries separated from each other by wide tracts of sea, as the opposite shores of the Mediterranean, the islands of the Indian and Pacific Oceans, have usually much that is common in their vegetation:—and again, that in parallel climates, analogous tribes replace each other. It would be easy to adduce other laws, but those already stated may serve to show the great extent of the portions of knowledge which have just been mentioned, even considered as merely Sciences of Phenomena.

[8] Prichard, *Researches into the Physical History of Mankind*, i. 55, 28.

7. *Phenomenal Glossology.*—It is not my purpose in the present work to borrow my leading illustrations from any portions of knowledge but those which are concerned

with the study of material nature; and I shall, therefore, not dwell upon a branch of research, singularly interesting, and closely connected with the one just mentioned, but dealing with relations of thought rather than of things;—I mean the Palætiology of Language;—the theory, so far as the facts enable us to form a theory, of the causes which have led to the resemblances and differences of human speech in various regions and various ages. This, indeed, would be only a portion of the study of the history and origin of the diffusion of animals, if we were to include man among the animals whose dispersion we thus investigate; for language is one of the most clear and imperishable records of the early events in the career of the human race. But the peculiar nature of the faculty of speech, and the ideas which the use of it involves, make it proper to treat *Glossology* as a distinct science. And of this science, the first part must necessarily be, as in the other sciences of this order, a 270 classification and comparison of languages governed in many respects by the same rules, and

presenting the same difficulties, as other sciences of classification. Such, accordingly, has been the procedure of the most philosophical glossologists. They have been led to throw the languages of the earth into certain large classes or *Families*, according to various kinds of resemblance; as the *Semitic* Family, to which belong Hebrew, Arabic, Chaldean, Syrian, Phoenician, Ethiopian, and the like; the *Indo-European*, which includes Sanskrit, Persian, Greek, Latin, and German; the *Monosyllabic* languages, Chinese, Tibetan, Birman, Siamese; the *Polysynthetic* languages, a class including most of the North-American Indian dialects; and others. And this work of classification has been the result of the labour and study of many very profound linguists, and has advanced gradually from step to step. Thus the Indo-European Family was first formed on an observation of the coincidences between Sanskrit, Greek, and Latin; but it was soon found to include the Teutonic languages, and more recently Dr. Prichard[9] has shown beyond doubt that the Celtic must be included in the same Family.

Other general resemblances and differences of languages have been marked by appropriate terms: thus August von Schlegel has denominated them *synthetical* and *analytical*, according as they form their conjugations and declensions by auxiliary verbs and prepositions, or by changes in the word itself: and the *polysynthetic* languages are so named by M. Duponceau, in consequence of their still more complex mode of inflexion. Nor are there wanting, in this science also, general laws of phenomena; such, for instance, is the curious rule of the interchange of consonants in the cognate words of Greek, Gothic, and German, which has been discovered by James Grimm. All these remarkable portions of knowledge, and the great works which have appeared on Glossology, such, for example, as the *Mithridates* of Adelung and Vater, contain, for their largest, and 271 hitherto probably their most valuable part, the phenomenal portion of the science, the comparison of languages as they now are. And beyond all doubt, until we have brought this

Comparative Philology to a considerable degree of completeness, all our speculations respecting the causes which have operated to produce the languages of the earth must be idle and unsubstantial dreams.

[9] Dr Prichard, *On the Eastern Origin of the Celtic Nations*. 1831.

Thus in all Palætiological Sciences, in all attempts to trace back the history and discover the origin of the present state of things, the portion of the science which must first be formed is that which classifies the phenomena, and discovers general laws prevailing among them. When this work is performed, and not till then, we may begin to speculate successfully concerning causes, and to make some progress in our attempts to go back to an origin. We must have a *Phenomenal* science preparatory to each *Ætiological* one.

8. *The Study of Phenomena leads to Theory.*—As we have just said, we cannot, in any subject, speculate successfully concerning the causes of the present state of things, till

we have obtained a tolerably complete and systematic view of the phenomena. Yet in reality men have not in any instance waited for this completeness and system in their knowledge of facts before they have begun to form theories. Nor was it natural, considering the speculative propensities of the human mind, and how incessantly it is endeavouring to apply the Idea of Cause, that it should thus restrain itself. I have already noticed this in the History of Geology. 'While we have been giving an account,' it is there said, 'of the objects with which Descriptive Geology is occupied, it must have been felt how difficult it is, in contemplating such facts, to confine ourselves to description and classification. Conjectures and reasonings respecting the causes of the phenomena force themselves upon us at every step; and even influence our classification and nomenclature. Our Descriptive Geology impels us to construct a Physical Geology.' And the same is the case with regard to the other subjects which I have mentioned. The mere 272 consideration of the different degrees of

condensation of different Nebulæ led Herschel and Laplace to contemplate the hypothesis that our solar system is a condensed Nebula. Immediately upon the division of the earth's surface into botanical and zoological provinces, and even at an earlier period, the opposite hypotheses of the Origin of all the animals of each kind from a single pair, and of their original diffusion all over the earth, were under discussion. And the consideration of the families of languages irresistibly led to speculations concerning the Families of the earliest human inhabitants of the earth. In all cases the contemplation of a very few phenomena, the discovery of a very few steps in the history, made men wish for and attempt to form a theory of the history from the very beginning of things.

9. *No sound Theory without Ætiology.*—But though man is thus impelled by the natural propensities of his intellect to trace each order of things to its causes, he does not at first discern the only sure way of obtaining such knowledge: he does not suspect how

much labour and how much method are requisite for success in this undertaking: he is not aware that for each order of phenomena he must construct, by the accumulated results of multiplied observation and distinct thought, a separate Æiology. Thus, as I have elsewhere remarked[10], when men had for the first time become acquainted with some of the leading phenomena of Geology, and had proceeded to speculate concerning the past changes and revolutions by which such results had been produced, they forthwith supposed themselves able to judge what would be the effects of any of the obvious agents of change, as Water or Volcanic Fire. It did not at first occur to them to suspect that their common and extemporaneous judgment on such points was by no means sufficient for sound knowledge. They did not foresee that, before they could determine what share these or any other causes had had in producing the present condition of the earth, they must create 273 a special science whose object should be to estimate the general laws and effects of such

assumed causes;—that before they could obtain any sound Geological Theory, they must carefully cultivate Geological Ætiology.

[10] *Hist. Ind. Sc.* b. xviii. c. v. sect. 1.

The same disposition to proceed immediately from the facts to the theory, without constructing, as an intermediate step, a Science of Causes, might be pointed out in the other sciences of this order. But in all of them this errour has been corrected by the failures to which it led. It soon appeared, for instance, that a more careful inquiry into the effects which climate, food, habit and circumstances can produce in animals, was requisite in order to determine how the diversities of animals in different countries have originated. The Ætiology of Animal Life (if we may be allowed to give this name to that study of such causes of change which is at present so zealously cultivated, and which yet has no distinctive designation, except so far as it coincides with the *Organic Geological Dynamics* of our History) is now perceived to be a necessary

portion of all attempts to construct a history of the earth and its inhabitants.

10. *Cause, in Palætiology.*—We are thus led to contemplate a class of Sciences which are commenced with the study of Causes. We have already considered sciences which depended mainly upon the Idea of Cause, namely, the Mechanical Sciences. But it is obvious that the Idea of Cause in the researches now under our consideration must be employed in a very different way from that in which we applied it formerly. Force is the *Cause* of motion, because force at all times and under all circumstances, if not counteracted, produces motion; but the Cause of the present condition and elevation of the Alps, whatever it was, was manifested in a series of events of which each happened but once, and occupied its proper place in the series of time. The former is *mechanical*, the latter *historical*, *cause*. In our present investigations, we consider the events which we contemplate, of whatever order they be, as forming a chain which is extended 274 from the beginning of things down to the

present time; and the causes of which we now speak are those which connect the successive links of this chain. Every occurrence which has taken place in the history of the solar system, or the earth, or its vegetable and animal creation, or man, has been at the same time effect and cause; —the effect of what preceded, the cause of what succeeded. By being effect and cause, it has occupied some certain portion of time; and the times which have thus been occupied by effects and causes, summed up and taken altogether, make up the total of Past Time. The Past has been a series of events connected by this historical causation, and the Present is the last term of this series. The problem in the Palætiological Sciences, with which we are here concerned, is, to determine the manner in which each term is derived from the preceding, and thus, if possible, to calculate backwards to the origin of the series.

11. *Various kinds of Cause.*—Those modes by which one term in the natural series of events is derived from another,—the forms

of historical causation,—the kinds of connexion between the links of the infinite chain of time,—are very various; nor need we attempt to enumerate them. But these kinds of causation being distinguished from each other, and separately studied, each becomes the subject of a separate Ætiology. Thus the causes of change in the earth's surface, residing in the elements, fire and water, form the main subject of Geological Ætiology. The Ætiology of the vegetable and animal kingdoms investigates the causes by which the forms and distribution of species of plants and animals are affected. The study of causes in Glossology leads to an Ætiology of Language, which shall distinguish, analyse, and estimate the causes by which certain changes are produced in the languages of nations; in like manner we may expect to have an Ætiology of Art, which shall scrutinise the influences by which the various forms of art have each given birth to its successor: by which, for example, there have been brought into being those various forms of architecture which we term Egyptian, 275 Doric, Ionic,

Roman, Byzantine, Romanesque, Gothic, Italian, Elizabethan. It is easily seen by this slight survey how manifold and diverse are the kinds of cause which the Palætiological Sciences bring under our consideration. But in each of those sciences we shall obtain solid and complete systems of knowledge, only so far as we study, with steady thought and careful observation, that peculiar kind of cause which is appropriate to the phenomena under our consideration.

12. *Hypothetical Order of Palætiological Causes.*—The various kinds of historical cause are not only connected with each other by their common bearing upon the historical sciences, but they form a kind of progression which we may represent to ourselves as having acted in succession in the hypothetical history of the earth and its inhabitants. Thus assuming, merely as a momentary hypothesis, the origin of the Solar System by the condensation of a Nebula, we have to contemplate, first, the causes by which the luminous incandescent diffused mass of which a nebula is supposed

to be constituted, is gradually condensed, cooled, collected into definite masses, solidified, and each portion made to revolve about its axis, and the whole to travel about another body. We have no difficulty in ascribing the globular form of each mass to the mutual attraction of its particles: but when this form was once assumed, and covered with a solid crust, are there, we may ask, in the constitution of such a body, any causes at work by which the crust might be again broken up and portions of it displaced, and covered with other matter? Again, if we can thus explain the origin of the Earth, can we with like success account for the presence of the Atmosphere and the Waters of earth and ocean? Supposing this done, we have then to consider by what causes such a body could become stocked with vegetable and animal Life; for there have not been wanting persons, extravagant speculators, no doubt, who have conceived that even this event in the history of the world might be the work of natural causes. Supposing an origin given to life 276 upon our earth, we have then, brought before us

by geological observations, a series of different forms of vegetable and animal existence; occurring in different strata, and, as the phenomena appear irresistibly to prove, existing at successive periods: and we are compelled to inquire what can have been the causes by which the forms of each period have passed into those of the next. We find, too, that strata, which must have been at first horizontal and continuous, have undergone enormous dislocations and ruptures, and we have to consider the possible effect of aqueous and volcanic causes to produce such changes in the earth's crust. We are thus led to the causes which have produced the present state of things on the earth; and these are causes to which we may hypothetically ascribe, not only the form and position of the inert materials of the earth, but also the nature and distribution of its animal and vegetable population. Man too, no less than other animals, is affected by the operation of such causes as we have referred to, and must, therefore, be included in such speculations. But man's history only begins, where that of

other animals ends, with his mere existence. They are stationary, he is progressive. Other species of animals, once brought into being, continue the same through all ages; man is changing, from age to age, his language, his thoughts, his works. Yet even these changes are bound together by laws of causation; and these causes too may become objects of scientific study. And such causes, though not to be dwelt upon now, since we permit ourselves to found our philosophy upon the material sciences only, must still, when treated scientifically, fall within the principles of our philosophy, and must be governed by the same general rules to which all science is subject. And thus we are led by a close and natural connexion, through a series of causes, extending from those which regulate the imperceptible changes of the remotest nebulæ in the heavens, to those which determine the diversities of language, the mutations of art, and even the progress of civilization, polity, and literature. 277

While I have been speaking of this

supposed series of events, including in its course the formation of the earth, the introduction of animal and vegetable life, and the revolutions by which one collection of species has succeeded another, it must not be forgotten, that though I have thus hypothetically spoken of these events as occurring by force of natural causes, this has been done only that the true efficacy of such causes might be brought under our consideration and made the subject of scientific examination. It may be found, that such occurrences as these are quite inexplicable by the aid of any natural causes with which we are acquainted; and thus, the result of our investigations, conducted with strict regard to scientific principles, may be, that we must either contemplate supernatural influences as part of the past series of events, or declare ourselves altogether unable to form this series into a connected chain.

13. *Mode of Cultivating Ætiology:—In Geology.*—In what manner, it may be asked, is Ætiology, with regard to each subject

such as we have enumerated, to be cultivated? In order to answer this question, we must, according to our method of proceeding, take the most successful and complete examples which we possess of such portions of science. But in truth, we can as yet refer to few examples of this kind. In Geology, it is only very recently, and principally through the example and influence of Sir Charles Lyell, that the Ætiology has been detached from the descriptive portion of the science; and cultivated with direct attention: in other sciences the separation has hardly yet been made. But if we examine what has already been done in Geological Ætiology, or as in the History it is termed, *Geological Dynamics*, we shall find a number of different kinds of investigation which, by the aid of our general principles respecting the formation of sciences, may suffice to supply very useful suggestions for Ætiology in general.

In Geological Ætiology, causes have been studied, in many instances, by attending to

their action in the phenomena of the present state of things, and by inferring 278 from this the nature and extent of the action which they may have exercised in former times. This has been done, for example, by Von Hoff, Sir Charles Lyell, and others, with regard to the operations of rivers, seas, springs, glaciers, and other aqueous causes of change, Again, the same course has been followed by the same philosophers with respect to volcanoes, earthquakes, and other violent agents. Sir Charles Lyell has attempted to show, too, that there take place, in our own time, not only violent agitations, but slow motions of parts of the earth's crust, of the same kind and order with those which have assisted in producing all anterior changes.

But while we thus seek instruction in the phenomena of the present state of things, we are led to the question, What are the limits of this 'present' period? For instance, among the currents of lava which we trace as part of the shores of Italy and Sicily, *which* shall we select as belonging to the

existing order of things? In going backwards in time, where shall we draw the line? and why at such particular point? These questions are important, for our estimate of the efficacy of known causes will vary with the extent of the effects which we ascribe to them. Hence the mode in which we group together rocks is not only a step in geological classification, but is also important to Ætiology. Thus, when the vast masses of trap rocks in the Western Isles of Scotland and in other countries, which had been maintained by the Wernerians to be of aqueous origin, were, principally by the sagacity and industry of Macculloch, identified as to their nature with the products of recent volcanoes, the amount of effect which might justifiably be ascribed to volcanic agency was materially extended.

In other cases, instead of observing the current effects of our geological causes, we have to estimate the results from what we know of the causes themselves; as when, with Herschel, we calculate the alterations in the temperature of the earth which

astronomical changes may possibly produce; or when, with Fourier, we try to calculate the rate of cooling of the earth's 279 surface, on the hypothesis of an incandescent central mass. In other cases, again, we are not able to calculate the effects of our causes rigorously, but estimate them as well as we can; partly by physical reasonings, and partly by comparison with such analogous cases as we can find in the present state of things. Thus Sir Charles Lyell infers the change of climate which would result if land were transferred from the neighbourhood of the poles to that of the equator, by reasonings on the power of land and water to contain and communicate heat, supported by a reference to the different actual climates of places, lying under the same latitude, but under different conditions as to the distribution of land and water.

Thus our Ætiology is constructed partly from calculation and reasoning, partly from phenomena. But we may observe that when we reason from phenomena to causes, we

usually do so by various steps; often ascending from phenomena to mere laws of phenomena, before we can venture to connect the phenomenon confidently with its cause. Thus the law of subterranean heat, that it increases in descending below the surface, is now well established, although the doctrine which ascribes this effect to a central heat is not universally assented to.

14. *In the Geography of Plants and Animals.* —We may find in other subjects also, considerable contributions towards Ætiology, though not as yet a complete System of Science. The Ætiology of Vegetables and Animals, indeed, has been studied with great zeal in modern times, as an essential preparative to geological theory; for how can we decide whether any assumed causes have produced the succession of species which we find in the earth's strata, except we know what effect of this kind given causes can produce? Accordingly, we find in Sir Charles Lyell's *Treatise on Geology* the most complete discussion of such questions as belong to

these subjects:—for example, the question whether species can be transmuted into other species by the long-continued influence of external causes, as climate, food, domestication, combined with internal 280 causes, as habits, appetencies, progressive tendencies. We may observe, too, that as we have brought before us, the inquiry what change difference of climate can produce in any species, we have also the inverse problem, how far a different development of the species, or a different collection of species, proves a difference of climate. In the same way, the geologist of the present day considers the question, whether, in virtue of causes now in action, species are from time to time extinguished; and in like manner, the geologists of an earlier period discussed the question, now long completely decided, whether fossil species in general are really extinct species.

15. *In Languages.*—Even with reference to the Ætiology of Language, although this branch of science has hardly been considered separately from the glossological

investigations in which it is employed or assumed to be employed, it might perhaps be possible to point out causes or conditions of change which, being general in their nature, must operate upon all languages alike. Changes made for the sake of euphony when words are modified and combined, occur in all dialects. Who can doubt that such changes of consonants as those by which the Greek roots become Gothic, and the Gothic, German, have for their cause some general principle in the pronunciation of each language? Again, we might attempt to decide other questions of no small interest. Have the terminations of verbs arisen from the accretion of pronouns; or, on the other hand, does the modification of a verb imply a simpler mental process than the insulation of a pronoun, as Adam Smith has maintained? Again, when the language of a nation is changed by the invasion and permanent mixture of an enemy of different speech, is it generally true that it is changed from a synthetic to an analytical structure? I will mention only one more of these wide and general

glossological inquiries. Is it true, as Dr. Prichard has suggested[11], that languages have become more permanent as we come down 281 towards later times? May we justifiably suppose, with him, that in the very earliest times, nations, when they had separated from one stock, might lose all traces of this common origin out of their languages, though retaining strong evidences of it in their mythology, social forms, and arts, as appears to be the case with the ancient Egyptians and the Indians[12].

[11] *Researches*, ii. 221.

[12] *Researches*, ii. 192.

Large questions of this nature cannot be treated profitably in any other way than by an assiduous study of the most varied forms of living and dead languages. But on the other hand, the study of languages should be prosecuted not only by a direct comparison of one with another, but also with a view to the formation of a science of causes and general principles, embracing

such discussions as I have pointed out. It is only when such a science has been formed, that we can hope to obtain any solid and certain results in the Palætiology of Language;—to determine, with any degree of substantial proof, what is the real evidence which the wonderful faculty of speech, under its present developments and forms, bears to the events which have taken place in its own history, and in the history of man since his first origin.

16. *Construction of Theories.*—When we have thus obtained, with reference to any such subject as those we have here spoken of, these two portions of science, a Systematic Description of the Facts, and a rigorous Analysis of the Causes,—the *Phenomenology* and the *Ætiology* of the subject,—we are prepared for the third member which completes the science, the *Theory* of the actual facts. We can then take a view of the events which really have happened, discerning their connexion, interpreting their evidence, supplying from the context the parts which are unapparent.

We can account for known facts by intelligible causes; we can infer latent facts from manifest effects, so as to obtain a distinct insight into the whole history of events up to the present time, and to see the last result of the whole in the present condition of things. 282 The term *Theory*, when rigorously employed in such sciences as those which we here consider, bears nearly the sense which I have adopted: it implies a consistent and systematic view of the actual facts, combined with a true apprehension of their connexion and causes. Thus if we speak of ‘a Theory of Mount Etna,’ or ‘a Theory of the Paris Basin,’ we mean a connected and intelligible view of the events by which the rocks in these localities have come into their present condition. Undoubtedly the term *Theory* has often been used in a looser sense; and men have put forth ‘*Theories of the Earth*,’ which, instead of including the whole mass of actual geological facts and their causes, only assigned, in a vague manner, some causes by which some few phenomena might, it was conceived, be accounted for. Perhaps

the portion of our Palætiological Sciences which we now wish to designate, would be more generally understood if we were to describe it as *Theoretical* or *Philosophical History*; as when we talk of 'the Theoretical History of Architecture,' or 'the Philosophical History of Language.' And in the same manner we might speak of the Theoretical History of the Animal and Vegetable Kingdoms; meaning, a distinct account of the events which have produced the present distribution of species and families. But by whatever phrase we describe this portion of science, it is plain that such a Theory, such a Theoretical History, must result from the application of causes well understood to facts well ascertained. And if the term *Theory* be here employed, we must recollect that it is to be understood, not in its narrower sense as opposed to facts, but in its wider signification, as including all known facts and differing from them only in introducing among them principles of intelligible connexion. The Theories of which we now speak are true *Theories*, precisely because

they are identical with the total system of the *Facts*.

17. *No sound Palætiological Theory yet extant.*—It is not to disparage unjustly the present state of science, to say that as yet no such theory exists on any subject. 'Theories of the Earth' have been 283 repeatedly published; but when we consider that even the facts of geology have been observed only on a small portion of the earth's surface, and even within those narrow bounds very imperfectly studied, we shall be able to judge how impossible it is that geologists should have yet obtained a well-established Theoretical History of the changes which have taken place in the crust of the terrestrial globe from its first origin. Accordingly, I have ventured in my History to designate the most prominent of the Theories which have hitherto prevailed as *premature* geological theories[13]: and we shall soon see that geological theory has not advanced beyond a few conjectures, and that its cultivators are at present mainly occupied with a controversy in which the

two extreme hypotheses which first offer themselves to men's minds are opposed to each other. And if we have no theoretical History of the Earth which merits any confidence, still less have we any theoretical History of Language, or of the Arts, which we can consider as satisfactory. The Theoretical History of the Vegetable and Animal Kingdoms is closely connected with that of the Earth on which they subsist, and must follow the fortunes of Geology. And thus we may venture to say that no Palætiological Science, as yet, possesses all its three members. Indeed most of them are very far from having completed and systematized their Phenomenology: in all, the cultivation of Ætiology is but just begun, or is not begun; in all, the Theory must reward the exertions of future, probably of distant, generations.

[13] *Hist. Ind. Sc.* b. xviii. c. vii. sect. 3.

But in the mean time we may derive some instruction from the comparison of the two antagonist hypotheses of which I have spoken.

CHAPTER III.

Of the Doctrine of Catastrophes and the Doctrine of Uniformity.

1. *Doctrine of Catastrophes.*—I have already shown, in the History of Geology, that the attempts to frame a theory of the earth have brought into view two completely opposite opinions:—one, which represents the course of nature as *uniform* through all ages, the causes which produce change having had the same intensity in former times which they have at the present day;—the other opinion, which sees, in the present condition of things, evidences of *catastrophes*;—changes of a more sweeping kind, and produced by more powerful agencies than those which occur in recent times. Geologists who held the latter opinion, maintained that the forces which

have elevated the Alps or the Andes to their present height could not have been any forces which are now in action: they pointed to vast masses of strata hundreds of miles long, thousands of feet thick, thrown into highly-inclined positions, fractured, dislocated, crushed: they remarked that upon the shattered edges of such strata they found enormous accumulations of fragments and rubbish, rounded by the action of water, so as to denote ages of violent aqueous action: they conceived that they saw instances in which whole mountains of rock in a state of igneous fusion, must have burst the earth's crust from below: they found that in the course of the revolutions by which one stratum of rock was placed upon another, the whole collection of animal species which tenanted the earth and the seas had been removed, and a new set of living things introduced in its place: finally, they found, above all the strata, 285 vast masses of sand and gravel containing bones of animals, and apparently the work of a mighty deluge. With all these proofs before their eyes, they thought it impossible not to

judge that the agents of change by which the world was urged from one condition to another till it reached its present state must have been more violent, more powerful, than any which we see at work around us. They conceived that the evidence of ‘catastrophes’ was irresistible.

2. *Doctrine of Uniformity*.—I need not here repeat the narrative (given in the History[14]) of the process by which this formidable array of proofs was, in the minds of some eminent geologists, weakened, and at last overcome. This was done by showing that the sudden breaks in the succession of strata were apparent only, the discontinuity of the series which occurred in one country being removed by terms interposed in another locality:—by urging that the total effect produced by existing causes, taking into account the accumulated result of long periods, is far greater than a casual speculator would think possible:—by making it appear that there are in many parts of the world evidences of a slow and imperceptible rising of the land since it was

the habitation of now existing species:—by proving that it is not universally true that the strata separated in time by supposed catastrophes contain distinct species of animals:—by pointing out the limited fields of the supposed diluvial action:—and finally, by remarking that though the *creation* of species is a mystery, the *extinction* of species is going on in our own day. Hypotheses were suggested, too, by which it was conceived that the change of climate might be explained, which, as the consideration of the fossil remains seemed to show, must have taken place between the ancient and the modern times. In this manner the whole evidence of catastrophes was explained away: the notion of a series of paroxysms of violence in the causes of change was represented as a delusion arising from our 286 contemplating short periods only, in the action of present causes: length of time was called in to take the place of intensity of force: and it was declared that Geology need not despair of accounting for the revolutions of the earth, as Astronomy accounts for the revolutions

of the heavens, by the universal action of causes which are close at hand to us, operating through time and space without variation or decay.

[14] *Hist. Ind. Sc.* b. xviii. c. viii. sect. 2.

An antagonism of opinions, somewhat of the same kind as this, will be found to manifest itself in the other Palætiological Sciences as well as in Geology; and it will be instructive to endeavour to balance these opposite doctrines. I will mention some of the considerations which bear upon the subject in its general form.

3. *Is Uniformity probable à priori?*—The doctrine of Uniformity in the course of nature has sometimes been represented by its adherents as possessing a great degree of *à priori* probability. It is highly unphilosophical, it has been urged, to assume that the causes of the geological events of former times were of a different kind from causes now in action, if causes of this latter kind can in any way be made to explain the facts. The analogy of all other

sciences compels us, it was said, to explain phenomena by known, not by unknown, causes. And on these grounds the geological teacher recommended[15] ‘an earnest and patient endeavour to reconcile the indications of former change with the evidence of gradual mutations now in progress.’

[15] Lyell, 4th ed. b. iv. c. i. p. 328.

But on this we may remark, that if by *known* causes we mean causes acting with the same intensity which they have had during historical times, the restriction is altogether arbitrary and groundless. Let it be granted, for instance, that many parts of the earth’s surface are now undergoing an imperceptible rise. It is not pretended that the rate of this elevation is rigorously uniform; what, then, are the limits of its velocity? Why may it not increase so as to assume that character of violence which we may term a 287 *catastrophe* with reference to all changes hitherto recorded? Why may not the rate of elevation be such that we may conceive the strata to assume *suddenly* a

position nearly vertical? And is it, in fact, easy to conceive a position of strata nearly vertical, a position which occurs so frequently, to be *gradually* assumed? In cases where the strata are nearly vertical, as in the Isle of Wight, and hundreds of other places, or where they are actually inverted, as sometimes occurs, are not the causes which have produced the effect as truly known causes, as those which have raised the coasts where we trace the former beach in an elevated terrace? If the latter case proves *slow* elevation, does not the former case prove *rapid* elevation? In neither case have we any measure of the time employed in the change; but does not the very nature of the results enable us to discern, that if one was gradual, the other was comparatively sudden?

The causes which are now elevating a portion of Scandinavia can be called known *causes*, only because we know the *effect*. Are not the causes which have elevated the Alps and the Andes known causes in the same sense? We know nothing in either case

which confines the intensity of the force within any limit, or prescribes to it any law of uniformity. Why, then, should we make a merit of cramping our speculations by such assumptions? Whether the causes of change do act uniformly;—whether they oscillate only within narrow limits;—whether their intensity in former times was nearly the same as it now is;—these are precisely the questions which we wish Science to answer to us impartially and truly: where is then the wisdom of 'an earnest and patient endeavour' to secure an *affirmative* reply?

Thus I conceive that the assertion of an *à priori* claim to probability and philosophical spirit in favour of the doctrine of uniformity, is quite untenable. We must learn from an examination of all the facts, and not from any assumption of our own, whether the course of nature be uniform. The limit of intensity being really unknown, catastrophes are just as probable 288 as uniformity. If a volcano may repose for a thousand years, and then break out and destroy a city; why may not another

volcano repose for ten thousand years, and then destroy a continent; or if a continent, why not the whole habitable surface of the earth?

4. *Cycle of Uniformity indefinite.*—But this argument may be put in another form. When it is said that the course of nature is uniform, the assertion is not intended to exclude certain smaller variations of violence and rest, such as we have just spoken of;—alternations of activity and repose in volcanoes; or earthquakes, deluges, and storms, interposed in a more tranquil state of things. With regard to such occurrences, terrible as they appear at the time, they may not much affect the average rate of change; there may be a *cycle*, though an irregular one, of rapid and slow change; and if such cycles go on succeeding each other, we may still call the order of nature uniform, notwithstanding the periods of violence which it involves. The maximum and minimum intensities of the forces of mutation alternate with one another; and we may estimate the average course of

nature as that which corresponds to something between the two extremes.

But if we thus attempt to maintain the uniformity of nature by representing it as a series of *cycles*, we find that we cannot discover, in this conception, any solid ground for excluding catastrophes. What is the length of that cycle, the repetition of which constitutes uniformity? What interval from the maximum to the minimum does it admit of? We may take for our cycle a hundred or a thousand years, but evidently such a proceeding is altogether arbitrary. We may mark our cycles by the greatest known paroxysms of volcanic and terremotive agency, but this procedure is no less indefinite and inconclusive than the other.

But further; since the cycle in which violence and repose alternate is thus indefinite in its length and in its range of activity, what ground have we for assuming more than one such cycle, extending from the origin of things to the present time?

Why may we not suppose the maximum force of the causes of change 289 to have taken place at the earliest period, and the tendency towards the minimum to have gone on ever since? Or instead of only one cycle, there may have been several, but of such length that our historical period forms a portion only of the last;—the feeblest portion of the latest cycle. And thus violence and repose may alternate upon a scale of time and intensity so large, that man's experience supplies no evidence enabling him to estimate the amount. The course of things is *uniform*, to an Intelligence which can embrace the succession of several cycles, but it is *catastrophic* to the contemplation of man, whose survey can grasp a part only of one cycle. And thus the hypothesis of uniformity, since it cannot exclude degrees of change, nor limit the range of these degrees, nor define the interval of their recurrence, cannot possess any essential simplicity which, previous to inquiry, gives it a claim upon our assent superior to that of the opposite catastrophic hypothesis.

5. *Uniformitarian Arguments are Negative only.*—There is an opposite tendency in the mode of maintaining the catastrophist and the uniformitarian opinions, which depends upon their fundamental principles, and shows itself in all the controversies between them. The Catastrophist is affirmative, the Uniformitarian is negative in his assertions: the former is constantly attempting to construct a theory; the latter delights in demolishing all theories. The one is constantly bringing fresh evidence of some great past event, or series of events, of a striking and definite kind; his antagonist is at every step explaining away the evidence, and showing that it proves nothing. One geologist adduces his proofs of a vast universal deluge; but another endeavours to show that the proofs do not establish either the universality or the vastness of such an event. The inclined broken edges of a certain formation, covered with their own fragments, beneath superjacent horizontal deposits, are at one time supposed to prove a catastrophic breaking up of the earlier strata; but this opinion is controverted by

showing that the same formations, when pursued into other countries, 290 exhibit a uniform gradation from the lower to the upper, with no trace of violence. Extensive and lofty elevations of the coast, continents of igneous rock, at first appear to indicate operations far more gigantic than those which now occur; but attempts are soon made to show that time only is wanting to enable the present age to rival the past in the production of such changes. Each new fact adduced by the catastrophist is at first striking and apparently convincing; but as it becomes familiar, it strikes the imagination less powerfully; and the uniformitarian, constantly labouring to produce some imitation of it by the machinery which he has so well studied, at last in every case seems to himself to succeed, so far as to destroy the effect of his opponent's evidence.

This is so with regard to more remote, as well as with regard to immediate evidences of change. When it is ascertained that in every part of the earth's crust the

temperature increases as we descend below the surface, at first this fact seems to indicate a central heat: and a central heat naturally suggests an earlier state of the mass, in which it was incandescent, and from which it is now cooling. But this original incandescence of the globe of the earth is manifestly an entire violation of the present course of things; it belongs to the catastrophist view, and the advocates of uniformity have to explain it away. Accordingly, one of them holds that this increase of heat in descending below the surface may very possibly not go on all the way to the center. The heat which increases at first as we descend, may, he conceives, afterwards decrease; and he suggests causes which may have produced such a succession of hotter and colder shells within the mass of the earth. I have mentioned this suggestion in the History of Geology; and have given my reasons for believing it altogether untenable[16]. Other persons also, desirous of reconciling this subterraneous heat with the tenet of uniformity, have 291 offered another suggestion:—that the

warmth or incandescence of the interior parts of the earth does not arise out of an originally hot condition from which it is gradually cooling, but results from chemical action constantly going on among the materials of the earth's substance. And thus new attempts are perpetually making, to escape from the cogency of the reasonings which send us towards an original state of things different from the present. Those who theorize concerning an origin go on building up the fabric of their speculations, while those who think such theories unphilosophical, ever and anon dig away the foundation of this structure. As we have already said, the uniformitarian's doctrines are a collection of negatives.

[16] *Hist. Ind. Sc.* b. xviii. c. v. sect. 5, and note.

This is so entirely the case, that the uniformitarian would for the most part shrink from maintaining as positive tenets the explanations which he so willingly uses as instruments of controversy. He puts forward his suggestions as difficulties, but

he will not stand by them as doctrines. And this is in accordance with his general tendency; for any of his hypotheses, if insisted upon as positive theories, would be found inconsistent with the assertion of uniformity. For example, the nebular hypothesis appears to give to the history of the heavens an aspect which obliterates all special acts of creation, for, according to that hypothesis, new planetary systems are constantly forming; but when asserted as the origin of our own solar system, it brings with it an original incandescence, and an origin of the organic world. And if, instead of using the chemical theory of subterraneous heat to neutralize the evidence of original incandescence, we assert it as a positive tenet, we can no longer maintain the infinite past duration of the earth; for chemical forces, as well as mechanical, tend to equilibrium; and that condition once attained, their efficacy ceases. Chemical affinities tend to form new compounds; and though, when many and various elements are mingled together, the play of synthesis and analysis may go on for

a long time, it must at last end. If, for instance, a large portion of the earth's mass were originally pure potassium, we 292 can imagine violent igneous action to go on so long as any part remained unoxidized; but when the oxidation of the whole has once taken place, this action must be at an end; for there is in the hypothesis no agency which can reproduce the deoxidized metal. Thus a perpetual motion is impossible in chemistry, as it is in mechanics; and a theory of constant change continued through infinite time, is untenable when asserted upon chemical, no less than upon mechanical principles. And thus the Skepticism of the uniformitarian is of force only so long as it is employed against the Dogmatism of the catastrophist. When the Doubts are erected into Dogmas, they are no longer consistent with the tenet of Uniformity. When the Negations become Affirmations, the Negation of an Origin vanishes also.

6. *Uniformity in the Organic World.*—In speaking of the violent and sudden changes

which constitute catastrophes, our thoughts naturally turn at first to great *mechanical* and *physical* effects;—ruptures and displacements of strata; extensive submersions and emersions of land; rapid changes of temperature. But the catastrophes which we have to consider in geology affect the *organic* as well as the inorganic world. The sudden extinction of one collection of species, and the introduction of another in their place, is a Catastrophe, even if unaccompanied by mechanical violence. Accordingly, the antagonism of the catastrophist and uniformitarian schools has shown itself in this department of the subject, as well as in the other. When geologists had first discovered that the successive strata are each distinguished by appropriate organic fossils, they assumed at once that each of these collections of living things belonged to a separate creation. But this conclusion, as I have already said, Sir C. Lyell has attempted to invalidate, by proving that in the existing order of things, some species become extinct; and by suggesting it as possible,

that in the same order, it may be true that new species are from time to time produced, even in the present course of nature. And in this, as in the other part of the subject, he calls in 293 the aid of vast periods of time, in order that the violence of the changes may be softened down: and he appears disposed to believe that the actual extinction and creation of species may be so slow as to excite no more notice than it has hitherto obtained; and yet may be rapid enough, considering the immensity of geological periods, to produce such a succession of different collections of species as we find in the strata of the earth's surface.

7. *Origin of the present Organic World.*— The last great event in the history of the vegetable and animal kingdoms was that by which their various tribes were placed in their present seats. And we may form various hypotheses with regard to the sudden or gradual manner in which we may suppose this distribution to have taken place. We may assume that at the beginning

of the present order of things, a stock of each species was placed in the vegetable or animal *province* to which it belongs, by some cause out of the common order of nature; or we may take a uniformitarian view of the subject, and suppose that the provinces of the organic world derived their population from some anterior state of things by the operation of natural causes.

Nothing has been pointed out in the existing order of things which has any analogy or resemblance, of any valid kind, to that creative energy which must be exerted in the production of a new species. And to assume the introduction of new species as ‘a part of the order of nature,’ without pointing out any natural fact with which such an event can be classed, would be to reject creation by an arbitrary act. Hence, even on natural grounds, the most intelligible view of the history of the animal and vegetable kingdoms seems to be, that each period which is marked by a distinct collection of species forms a cycle; and that at the beginning of each such cycle a

creative power was exerted, of a kind to which there was nothing at all analogous in the succeeding part of the same cycle. If it be urged that in some cases the same species, or the same genus, runs through two geological formations, 294 which must, on other grounds, be referred to different cycles of creative energy, we may reply that the creation of many new species does not imply the extinction of all the old ones.

Thus we are led by our reasonings to this view, that the present order of things was commenced by an act of creative power entirely different to any agency which has been exerted since. None of the influences which have modified the present races of animals and plants since they were placed in their habitations on the earth's surface can have had any efficacy in producing them at first. We are necessarily driven to assume, as the beginning of the present cycle of organic nature, an event not included in the course of nature. And we may remark that this necessity is the more cogent, precisely because other cycles have

preceded the present.

8. *Nebular Origin of the Solar System.*—If we attempt to apply the same antithesis of opinion (the doctrines of Catastrophe and Uniformity) to the other subjects of palætiological sciences, we shall be led to similar conclusions. Thus, if we turn our attention to Astronomical Palætiology, we perceive that the Nebular Hypothesis has a uniformitarian tendency. According to this hypothesis the formation of this our system of sun, planets, and satellites, was a process of the same kind as those which are still going on in the heavens. One after another, nebulæ condense into separate masses, which begin to revolve about each other by mechanical necessity, and form systems of which our solar system is a finished example. But we may remark, that the uniformitarian doctrine on this subject rests on most unstable foundations. We have as yet only very vague and imperfect reasonings to show that by such condensation a *material* system such as ours could result; and the introduction of

organized beings into such a material system is utterly out of the reach of our philosophy. Here again, therefore, we are led to regard the present order of the world as pointing towards an origin altogether of a different kind from anything which our material science can grasp. 295

9. *Origin of Languages.*—We may venture to say that we should be led to the same conclusion once more, if we were to take into our consideration those palætiological sciences which are beyond the domain of matter; for instance, the History of Languages. We may explain many of the differences and changes which we become acquainted with, by referring to the action of causes of change which still operate. But what glossologist will venture to declare that the efficacy of such causes has been uniform;—that the influences which mould a language, or make one language differ from others of the same stock, operated formerly with no more efficacy than they exercise now. 'Where,' as has elsewhere been asked, 'do we now find a language in

the process of formation, unfolding itself in inflexions, terminations, changes of vowels by grammatical relations, such as characterise the oldest known languages?' Again, as another proof how little the history of languages suggests to the philosophical glossologist the persuasion of a uniform action of the causes of change, I may refer to the conjecture of Dr. Prichard, that the varieties of language produced by the separation of one stock into several, have been greater and greater as we go backwards in history:—that[17] the formation of sister dialects from a common language (as the Scandinavian, German, and Saxon dialects from the Teutonic, or the Gaelic, Erse and Welsh from the Celtic) belongs to the first millennium before the Christian era; while the formation of cognate languages of the same family, as the Sanskrit, Latin, Greek and Gothic, must be placed at least two thousand years before that era; and at a still earlier period took place the separation of the great families themselves, the Indo-European, Semitic, and others, in which it is now difficult to trace

the features of a common origin. No hypothesis except one of this kind will explain the existence of the families, groups, and dialects of languages, which we find in existence. Yet this is an entirely different view from that which 296 the hypothesis of the uniform progress of change would give. And thus, in the earliest stages of man's career, the revolutions of language must have been, even by the evidence of the theoretical history of language itself, of an order altogether different from any which have taken place within the recent history of man. And we may add, that as the early stages of the progress of language must have been widely different from those later ones of which we can in some measure trace the natural causes, we cannot place the origin of language in any point of view in which it comes under the jurisdiction of natural causation at all.

[17] *Researches*, ii. 224.

10. *No Natural Origin discoverable.*—We are thus led by a survey of several of the palætiological sciences to a confirmation of

the principle formerly asserted[18], That in no palætiological science has man been able to arrive at a beginning which is homogeneous with the known course of events. We can in such sciences often go very far back;—determine many of the remote circumstances of the past series of events;—ascend to a point which seems to be near the origin;—and limit the hypotheses respecting the origin itself: but philosophers never have demonstrated, and, so far as we can judge, probably never will be able to demonstrate, what was that primitive state of things from which the progressive course of the world took its first departure. In all these paths of research, when we travel far backwards, the aspect of the earlier portions becomes very different from that of the advanced part on which we now stand; but in all cases the path is lost in obscurity as it is traced backwards towards its starting-point: it becomes not only invisible, but unimaginable; it is not only an interruption, but an abyss, which interposes itself between us and any intelligible beginning of things.

[18] *Hist. Ind. Sc.* b. xviii. c. vi. sect 5.

CHAPTER IV.

Of the Relation of Tradition to Palætiology.

1. *Importance of Tradition.*—Since the Palætiological Sciences have it for their business to study the train of past events produced by natural causes down to the present time, the knowledge concerning such events which is supplied by the remembrance and records of man, in whatever form, must have an important bearing upon these sciences. All changes in the condition and extent of land and sea, which have taken place within man's observation, all effects of deluges, sea-waves, rivers, springs, volcanoes, earthquakes, and the like, which come within the reach of human history, have a strong interest for the palætiologist. Nor is

he less concerned in all recorded instances of the modification of the forms and habits of plants and animals, by the operations of man, or by transfer from one land to another. And when we come to the Palætiology of Language, of Art, of Civilization, we find our subject still more closely connected with history; for in truth these are historical, no less than palætiological investigations. But, confining ourselves at present to the material sciences, we may observe that though the importance of the information which tradition gives us, in the sciences now under our consideration, as, for instance, geology, has long been tacitly recognised; yet it is only recently that geologists have employed themselves in collecting their historical facts upon such a scale and with such comprehensive views as are required by the interest and use of collections of this kind. The Essay of Von 298 Hoff[19], *On the Natural Alterations in the Surface of the Earth which are proved by Tradition*, was the work which first opened the eyes of geologists to the extent and importance of this kind of

investigation. Since that time the same path of research has been pursued with great perseverance by others, especially by Sir C. Lyell; and is now justly considered as an essential portion of Geology.

[19] Vol. i. 1822; vol. ii. 1824.

2. *Connexion of Tradition and Science.*—Events which we might naturally expect to have some bearing on geology, are narrated in the historical writings which, even on mere human grounds, have the strongest claim to our respect as records of the early history of the world, and are confirmed by the traditions of various nations all over the globe; namely, the formation of the earth and of its population, and a subsequent deluge. It has been made a matter of controversy how the narrative of these events is to be understood, so as to make it agree with the facts which an examination of the earth's surface and of its vegetable and animal population discloses to us. Such controversies, when they are considered as merely archæological, may occur in any of the palætiological sciences. We may have to

compare and to reconcile the evidence of existing phenomena with that of historical tradition. But under some circumstances this process of conciliation may assume an interest of another kind, on which we will make a few remarks.

3. *Natural and Providential History of the World.*—We may contemplate the existence of man upon the earth, his origin and his progress, in the same manner as we contemplate the existence of any other race of animals; namely, in a purely palætiological view. We may consider how far our knowledge of laws of causation enables us to explain his diffusion and migration, his differences and resemblances, his actions and works. And this is the view of man as a member of the *Natural* Course of Things. 299

But man, at the same time the contemplator and the subject of his own contemplation, endowed with faculties and powers which make him a being of a different nature from other animals, cannot

help regarding his own actions and enjoyments, his recollections and his hopes, under an aspect quite different from any that we have yet had presented to us. We have been endeavouring to place in a clear light the Fundamental Ideas, such as that of Cause, on which depends our knowledge of the natural course of things. But there are other Ideas to which man necessarily refers his actions; he is led by his nature, not only to consider his own actions, and those of his fellow-men, as springing out of this or that cause, leading to this or that material result; but also as *good* or *bad*, as what they *ought* or *ought not* to be. He has Ideas of moral relations as well as those Ideas of material relations with which we have hitherto been occupied. He is a moral as well as a natural agent.

Contemplating himself and the world around him by the light of his Moral Ideas, man is led to the conviction that his moral faculties were bestowed upon him by design and for a purpose; that he is the subject of a Moral Government; that the course of the

world is directed by the Power which governs it, to the unfolding and perfecting of man's moral nature; that this guidance may be traced in the career of individuals and of the world; that there is a *Providential* as well as a Natural Course of Things.

Yet this view is beset by no small difficulties. The full development of man's moral faculties;—the perfection of his nature up to the measure of his own ideas;—the adaptation of his moral being to an ultimate destination, by its transit through a world full of moral evil, in which evil each person has his share;—are effects for which the economy of the world appears to contain no adequate provision. Man, though aware of his moral nature, and ready to believe in an ultimate destination of purity and blessedness, is too feeble to resist the temptation of evil, and too helpless to restore his purity when once lost. He cannot but look for 300 some confirmation of that providential order which he has begun to believe; some provision for those deficiencies in his moral condition which he

has begun to feel.

He looks at the history of the world, and he finds that at a certain period it offers to him the promise of what he seeks. When the natural powers of man had been developed to their full extent, and were beginning to exhibit symptoms of decay;—when the intellectual progress of the world appeared to have reached its limit, without supplying man's moral needs;—we find the great Epoch in the Providential History of the world. We find the announcement of a Dispensation by which man's deficiencies shall be supplied and his aspirations fulfilled: we find a provision for the purification, the support, and the ultimate beatification of those who use the provided means. And thus the providential course of the world becomes consistent and intelligible.

4. *The Sacred Narrative.*—But with the new Dispensation, we receive, not only an account of its own scheme and history, but also a written narrative of the providential

course of the world from the earliest times, and even from its first creation. This narrative is recognized and authorized by the new dispensation, and accredited by some of the same evidences as the dispensation itself. That the existence of such a sacred narrative should be a part of the providential order of things, cannot but appear natural; but, naturally also, the study of it leads to some difficulties.

The Sacred Narrative in some of its earliest portions speaks of natural objects and occurrences respecting them. In the very beginning of the course of the world, we may readily believe (indeed, as we have seen in the last chapter, our scientific researches lead us to believe) that such occurrences were very different from anything which now takes place;—different to an extent and in a manner which we cannot estimate. Now the narrative must speak of objects and occurrences in the words and phrases which have derived their meaning from their application to the existing natural state of things. When

applied to an initial 301 supernatural state therefore, these words and phrases cannot help being to us obscure and mysterious, perhaps ambiguous and seemingly contradictory.

5. *Difficulties in interpreting the Sacred Narrative.*—The moral and providential relations of man's condition are so much more important to him than mere natural relations, that at first we may well suppose he will accept the Sacred Narrative, as not only unquestionable in its true import, but also as a guide in his views even of mere natural things. He will try to modify the conceptions which he entertains of objects and their properties, so that the Sacred Narrative of the supernatural condition shall retain the first meaning which he had put upon it in virtue of his own habits in the usage of language.

But man is so constituted that he cannot persist in this procedure. The powers and tendencies of his intellect are such that he cannot help trying to attain true conceptions

of objects and their properties by the study of things themselves. For instance, when he at first read of a firmament dividing the waters above from the waters below, he perhaps conceived a transparent floor in the skies, on which the superior waters rested, which descend in rain; but as his observations and his reasonings satisfied him that such a floor could not exist, he became willing to allow (as St. Augustine allowed) that the waters above the firmament are in a state of vapour. And in like manner in other subjects, men, as their views of nature became more distinct and precise, modified, so far as it was necessary for consistency's sake, their first rude interpretations of the Sacred Narrative; so that, without in any degree losing its import as a view of the providential course of the world, it should be so conceived as not to contradict what they knew of the natural order of things.

But this accommodation was not always made without painful struggles and angry controversies. When men had conceived the

occurrences of the Sacred Narrative in a particular manner, they could not readily and willingly adopt a new mode of conception; and all attempts to recommend to them such novelties, they 302 resisted as attacks upon the sacredness of the Narrative. They had clothed their belief of the workings of Providence in certain images; and they clung to those images with the persuasion that, without them, their belief could not subsist. Thus they imagined to themselves that the earth was a flat floor, solidly and broadly laid for the convenience of man; and they felt as if the kindness of Providence was disparaged, when it was maintained that the earth was a globe held together only by the mutual attraction of its parts.

The most memorable instance of a struggle of this kind is to be found in the circumstances which attended the introduction of the Heliocentric Theory of Copernicus to general acceptance. On this controversy I have already made some remarks in the *History of Science*[20], and have

attempted to draw from it some lessons which may be useful to us when any similar conflict of opinions may occur. I will here add a few reflections with a similar view.

[20] B. v. c. iii. sect. 4.

6. *Such difficulties inevitable.*—In the first place, I remark that such modifications of the current interpretation of the words of Scripture appear to be an inevitable consequence of the progressive character of Natural Science. Science is constantly teaching us to describe known facts in new language; but the language of Scripture is always the same. And not only so, but the language of Scripture is necessarily adapted to the common state of man's intellectual development, in which he is supposed not to be possessed of science. Hence the phrases used by Scripture are precisely those which science soon teaches man to consider as inaccurate. Yet they are not, on that account, the less fitted for their proper purpose: for if any terms had been used, adapted to a more advanced state of knowledge, they must have been

unintelligible among those to whom the Scripture was first addressed. If the Jews had been told that water existed in the clouds in small drops, they would have marvelled that it did 303 not constantly descend; and to have explained the reason of this, would have been to teach Atmology in the sacred writings. If they had read in their Scripture that the earth was a sphere, when it appeared to be a plain, they would only have been disturbed in their thoughts or driven to some wild and baseless imaginations, by a declaration to them so strange. If the Divine Speaker, instead of saying that he would set his bow in the clouds, had been made to declare that he would give to water the property of refracting different colours at different angles, how utterly unmeaning to the hearers would the words have been! And in these cases, the expressions, being unintelligible, startling, and bewildering, would have been such as tended to unfit the Sacred Narrative for its place in the providential dispensation of the world.

Accordingly, in the great controversy which took place in Galileo's time between the defenders of the then customary interpretations of Scripture, and the assertors of the Copernican system of the universe, when the innovators were upbraided with maintaining opinions contrary to Scripture, they replied that Scripture was not intended to teach men astronomy, and that it expressed the acts of divine power in images which were suited to the ideas of unscientific men. To speak of the rising and setting and travelling of the sun, of the fixity and of the foundations of the earth, was to use the only language which would have made the Sacred Narrative intelligible. To extract from these and the like expressions doctrines of science, was, they declared, in the highest degree unjustifiable; and such a course could lead, they held, to no result but a weakening of the authority of Scripture in proportion as its credit was identified with that of these modes of applying it. And this judgment has since been generally assented to by those who most reverence and value

the study of the designs of Providence as well as that of the works of nature.

7. *Science tells us nothing concerning Creation.*—Other apparent difficulties arise from the accounts given in the Scripture of the first origin of the world 304 in which we live: for example, Light is represented as created before the Sun. With regard to difficulties of this kind, it appears that we may derive some instruction from the result to which we were led in the last chapter;—namely, that in the sciences which trace the progress of natural occurrences, we can in no case go back to an origin, but in every instance appear to find ourselves separated from it by a state of things, and an order of events, of a kind altogether different from those which come under our experience. The thread of induction respecting the natural course of the world snaps in our fingers, when we try to ascertain where its beginning is. Since, then, science can teach us nothing positive respecting the beginning of things, she can neither contradict nor confirm what is taught by Scripture on that

subject; and thus, as it is unworthy timidity in the lover of Scripture to fear contradiction, so is it ungrounded presumption to look for confirmation, in such cases. The providential history of the world has its own beginning, and its own evidence; and we can only render the system insecure, by making it lean on our material sciences. If any one were to suggest that the nebular hypothesis countenances the Scripture history of the formation of this system, by showing how the luminous matter of the sun might exist previous to the sun itself, we should act wisely in rejecting such an attempt to weave together these two heterogeneous threads;—the one a part of a providential scheme, the other a fragment of a physical speculation.

We shall best learn those lessons of the true philosophy of science which it is our object to collect, by attending to portions of science which have gone through such crises as we are now considering; nor is it requisite, for this purpose, to bring forwards any subjects which are still under

discussion. It may, however, be mentioned that such maxims as we are now endeavouring to establish, and the one before us in particular, bear with a peculiar force upon those Palætiological Sciences of which we have been treating in the present Book. 305

8. *Scientific views, when familiar, do not disturb the authority of Scripture.*—There is another reflection which may serve to console and encourage us in the painful struggles which thus take place, between those who maintain interpretations of Scripture already prevalent and those who contend for such new ones as the new discoveries of science require. It is this;—that though the new opinion is resisted by one party as something destructive of the credit of Scripture and the reverence which is its due, yet, in fact, when the new interpretation has been generally established and incorporated with men's current thoughts, it ceases to disturb their views of the authority of the Scripture or of the truth of its teaching. When the language

of Scripture, invested with its new meaning, has become familiar to men, it is found that the ideas which it calls up are quite as reconcileable as the former ones were, with the most entire acceptance of the providential dispensation. And when this has been found to be the case, all cultivated persons look back with surprise at the mistake of those who thought that the essence of the revelation was involved in their own arbitrary version of some collateral circumstance in the revealed narrative. At the present day, we can hardly conceive how reasonable men could ever have imagined that religious reflections on the stability of the earth, and the beauty and use of the luminaries which revolve round it, would be interfered with by an acknowledgment that this rest and motion are apparent only[21]. And thus the authority of revelation is not shaken by any changes introduced by the progress of science in the mode of interpreting expressions which describe physical objects and occurrences; provided the new interpretation is admitted at a proper season, and in a proper spirit; so

as to soften, as much as possible, both the public controversies and the private scruples which almost inevitably accompany such an alteration.

[21] I have here borrowed a sentence or two from my own *History*.

9. *When should old Interpretations be given up?*—But the question then occurs, What is the proper 306 season for a religious and enlightened commentator to make such a change in the current interpretation of sacred Scripture? At what period ought the established exposition of a passage to be given up, and a new mode of understanding the passage, such as is, or seems to be, required by new discoveries respecting the laws of nature, accepted in its place? It is plain, that to introduce such an alteration lightly and hastily would be a procedure fraught with inconvenience; for if the change were made in such a manner, it might be afterwards discovered that it had been adopted without sufficient reason, and that it was necessary to reinstate the old exposition. And the minds of the readers of

Scripture, always to a certain extent and for a time disturbed by the subversion of their long-established notions, would be distressed without any need, and might be seriously unsettled. While, on the other hand, a too protracted and obstinate resistance to the innovation, on the part of the scriptural expositors, would tend to identify, at least in the minds of many, the authority of the Scripture with the truth of the exposition; and therefore would bring discredit upon the revealed word, when the established interpretation was finally proved to be untenable.

A rule on this subject, propounded by some of the most enlightened dignitaries of the Roman Catholic church, on the occasion of the great Copernican controversy begun by Galileo, seems well worthy of our attention. The following was the opinion given by Cardinal Bellarmine at the time: —'When a *demonstration* shall be found to establish the earth's motion, it will be proper to interpret the sacred Scriptures otherwise than they have hitherto been

interpreted in those passages where mention is made of the stability of the earth and movement of the heavens.' This appears to be a judicious and reasonable maxim for such cases in general. So long as the supposed scientific discovery is doubtful, the exposition of the meaning of Scripture given by commentators of established credit is not wantonly to be disturbed: but when a scientific theory, irreconcileable with this ancient 307 interpretation, is clearly proved, we must give up the interpretation, and seek some new mode of understanding the passage in question, by means of which it may be consistent with what we know; for if it be not, our conception of the things so described is no longer consistent with itself.

It may be said that this rule is indefinite, for who shall decide when a new theory is completely demonstrated, and the old interpretation become untenable? But to this we may reply, that if the rule be assented to, its application will not be very difficult. For when men have admitted as a general rule, that the current interpretations

of scriptural expressions respecting natural objects and events may possibly require, and in some cases certainly will require, to be abandoned, and new ones admitted, they will hardly allow themselves to contend for such interpretations as if they were essential parts of revelation; and will look upon the change of exposition, whether it come sooner or later, without alarm or anger. And when men lend themselves to the progress of truth in this spirit, it is not of any material importance at what period a new and satisfactory interpretation of the scriptural difficulty is found; since a scientific exactness in our apprehension of the meaning of such passages as are now referred to is very far from being essential to our full acceptance of revelation.

10. *In what Spirit should the Change be accepted?*—Still these revolutions in scriptural interpretation must always have in them something which distresses and disturbs religious communities. And such uneasy feelings will take a different shape, according as the community acknowledges

or rejects a paramount interpretative authority in its religious leaders. In the case in which the interpretation of the Church is binding upon all its members, the more placid minds rest in peace upon the ancient exposition, till the spiritual authorities announce that the time for the adoption of a new view has arrived; but in these circumstances, the more stirring and inquisitive minds, which cannot refrain from the pursuit of new truths 308 and exact conceptions, are led to opinions which, being contrary to those of the Church, are held to be sinful. On the other hand, if the religious constitution of the community allow and encourage each man to study and interpret for himself the Sacred Writings, we are met by evils of another kind. In this case, although, by the unforced influence of admired commentators, there may prevail a general agreement in the usual interpretation of difficult passages, yet as each reader of the Scripture looks upon the sense which he has adopted as being his own interpretation, he maintains it, not with the tranquil acquiescence of one who

has deposited his judgment in the hands of his Church, but with the keenness and strenuousness of self-love. In such a state of things, though no judicial severities can be employed against the innovators, there may arise more angry controversies than in the other case.

It is impossible to overlook the lesson which here offers itself, that it is in the highest degree unwise in the friends of religion, whether individuals or communities, unnecessarily to embark their credit in expositions of Scripture on matters which appertain to natural Science. By delivering physical doctrines as the teaching of revelation, religion may lose much, but cannot gain anything. This maxim of practical wisdom has often been urged by Christian writers. Thus St. Augustine says[22]: ‘In obscure matters and things far removed from our senses, if we read anything, even in the divine Scripture, which may produce diverse opinions without damaging the faith which we cherish, let us not rush headlong by positive assertion to either the one

opinion or the other; lest, when a more thorough discussion has shown the opinion which we had adopted to be false, our faith may fall with it: and we should be found contending, not for the doctrine of the sacred Scriptures, but for our own; endeavouring to make our doctrine to be that of the Scriptures, instead of taking the doctrine of the Scriptures to be ours.' And in nearly the same spirit, at the 309 time of the Copernican controversy, it was thought proper to append to the work of Copernicus a postil, to say that the work was written to account for the phenomena, and that people must not run on blindly and condemn either of the opposite opinions. Even when the Inquisition, in 1616, thought itself compelled to pronounce a decision upon this subject, the verdict was delivered in very moderate language;—that 'the doctrine of the earth's motion appeared to be contrary to Scripture:' and yet, moderate as this expression is, it has been blamed by judicious members of the Roman church as deciding a point such as religious authorities ought not to pretend to decide;

and has brought upon that church no ordinary weight of general condemnation. Kepler pointed out, in his lively manner, the imprudence of employing the force of religious authorities on such subjects: *Acies dolabræ in ferrum illisa, postea nec in lignum valet amplius. Capiat hoc cujus interest.* 'If you *will* try to chop iron, the axe becomes unable to cut even wood. I warn those whom it concerns.'

[22] Lib. i. *de Genesi*, cap. xviii.

11. *In what Spirit should the Change be urged?*—But while we thus endeavour to show in what manner the interpreters of Scripture may most safely and most properly accept the discoveries of science, we must not forget that there may be errours committed on the other side also; and that men of science, in bringing forward views which may for a time disturb the minds of lovers of Scripture, should consider themselves as bound by strict rules of candour, moderation, and prudence. Intentionally to make their supposed discoveries a means of discrediting,

contradicting, or slighting the sacred Scriptures, or the authority of religion, is in them unpardonable. As men who make the science of Truth the business of their lives, and are persuaded of her genuine superiority, and certain of her ultimate triumph, they are peculiarly bound to urge her claims in a calm and temperate spirit; not forgetting that there are other kinds of truth besides that which they peculiarly study. They may properly reject authority in matters of science; but they are to leave 310 it its proper office in matters of religion. I may here again quote Kepler's expressions: 'In Theology we balance authorities, in Philosophy we weigh reasons. A holy man was Lactantius who denied that the earth was round; a holy man was Augustine, who granted the rotundity, but denied the antipodes; a holy thing to me is the Inquisition, which allows the smallness of the earth, but denies its motion; but more holy to me is Truth; and hence I prove, from philosophy, that the earth is round, and inhabited on every side, of small size, and in motion among the stars,—and this I do with

no disrespect to the Doctors.' I the more willingly quote such a passage from Kepler, because the entire ingenuousness and sincere piety of his character does not allow us to suspect him in anything of hypocrisy or latent irony. That similar professions of respect may be made ironically, we have a noted example in the celebrated Introduction to *Galileo's Dialogue on the Copernican System*; probably the part which was most offensive to the authorities. 'Some years ago,' he begins, 'a wholesome edict was promulgated at Rome, which, in order to check the perilous scandals of the present age, imposed silence upon the Pythagorean opinion of the mobility of the earth. There were not wanting,' he proceeds, 'persons who rashly asserted that this decree was the result, not of a judicious inquiry, but of passion ill-informed; and complaints were heard that councillors, utterly unacquainted with astronomical observation, ought not to be allowed, with their sudden prohibitions, to clip the wings of speculative intellects. *At the hearing of rash lamentations like these, my zeal could not keep silence.*' And he then goes

on to say, that he wishes, in his *Dialogue*, to show that the subject had been fully examined at Rome. Here the irony is quite transparent, and the sarcasm glaringly obvious. I think we may venture to say that this is not the temper in which scientific questions should be treated; although by some, perhaps, the prohibition of public discussion may be considered as justifying any evasion which is likely to pass unpunished. 311

12. *Duty of Mutual Forbearance.*—We may add, as a further reason for mutual forbearance in such cases, that the true interests of both parties are the same. The man of science is concerned, no less than any other person, in the truth and import of the divine dispensation; the religious man, no less than the man of science, is, by the nature of his intellect, incapable of believing two contradictory declarations. Hence they have both alike a need for understanding the Scripture in some way in which it shall be consistent with their understanding of nature. It is for their

common advantage to conciliate, as Kepler says, the finger and the tongue of God, his works and his word. And they may find abundant reason to bear with each other, even if they should adopt for this purpose different interpretations, each finding one satisfactory to himself; or if any one should decline employing his thoughts on such subjects at all. I have elsewhere[23] quoted a passage from Kepler[24] which appears to me written in a most suitable spirit: 'I beseech my reader that, not unmindful of the divine goodness bestowed upon man, he do with me praise and celebrate the wisdom of the Creator, which I open to him from a more inward explication of the form of the world, from a searching of causes, from a detection of the errours of vision; and that thus not only in the firmness and stability of the earth may we perceive with gratitude the preservation of all living things in nature as the gift of God: but also that in its motion, so recondite, so admirable, we may acknowledge the wisdom of the Creator. But whoever is too dull to receive this science, or too weak to believe the Copernican

system without harm to his piety, him, I say, I advise that, leaving the school of astronomy, and condemning, if so he please, any doctrines of the philosophers, he follow his own path, and desist from this wandering through the universe; and that, lifting up his natural eyes, with which alone he can see, 312 he pour himself out from his own heart in worship of God the Creator, being certain that he gives no less worship to God than the astronomer, to whom God has given to see more clearly with his inward eyes, and who, from what he has himself discovered, both can and will glorify God.'

[23] *Bridgewater Tr.* p. 314.

[24] *Com. Stell. Mart.* Introd.

13. *Case of Galileo.*—I may perhaps venture here to make a remark or two upon this subject with reference to a charge brought against a certain portion of the *History of the Inductive Sciences*. Complaint has been made[25] that the character of the Roman church, as shown in its behaviour

towards Galileo, is misrepresented in the account given of it in the History of Astronomy. It is asserted that Galileo provoked the condemnation he incurred; first, by pertinaciously demanding the assent of the ecclesiastical authorities to his opinion of the consistency of the Copernican doctrine with Scripture; and afterwards by contumaciously, and, as we have seen, contumeliously violating the silence which the Church had enjoined upon him. It is further declared that the statement which represents it as the habit of the Roman church to dogmatize on points of natural science is unfounded; as well as the opinion that in consequence of this habit, new scientific truths were promulgated less boldly in Italy than in other countries. I shall reply very briefly on these subjects; for the decision of them is by no means requisite in order to establish the doctrines to which I have been led in the present chapter, nor, I hope, to satisfy my reader that my views have been collected from an impartial consideration of scientific history.

[25] *Dublin Review*, No. ix. July, 1838, p. 72.

With regard to Galileo, I do not think it can be denied that he obtruded his opinions upon the ecclesiastical authorities in an unnecessary and imprudent manner. He was of an ardent character, strongly convinced himself, and urged on still more by the conviction which he produced among his disciples, and 313 thus he became impatient for the triumph of truth. This judgment of him has recently been delivered by various independent authorities, and has undoubtedly considerable foundation[26]. As to the question whether authority in matters of natural science were habitually claimed by the authorities of the Church of Rome, I have to allow that I cannot produce instances which establish such a habit. We, who have been accustomed to have daily before our eyes the Monition which the Romish editors of Newton thought it necessary to prefix—*Cæterum latis a summo Pontifice contra telluris motum Decretis, nos obsequi profitemur*—were not likely to conjecture that this was a solitary instance

of the interposition of the Papal authority on such subjects. But although it would be easy to find declarations of heresy delivered by Romish Universities, and writers of great authority, against tenets belonging to the natural sciences, I am not aware that any other case can be adduced in which the Church or the Pope can be shown to have pronounced such a sentence. I am well contented to acknowledge this; for I should be far more gratified by finding myself compelled to hold up the seventeenth century as a model for the nineteenth in this respect, than by having to sow enmity between the admirers of the past and the present through any disparaging contrast[27].

[26] Besides the *Dublin Review*, I may quote the *Edinburgh Review*, which I suppose will not be thought likely to have a bias in favour of the exercise of ecclesiastical authority in matters of science; though certainly there is a puerility in the critic's phraseology which does not add to the weight of his judgment. 'Galileo contrived to surround the truth with every variety of obstruction. The tide of knowledge, which had hitherto advanced in peace, he crested with angry breakers, and he involved in its surf both his friends and his

foes.'—*Ed. Rev.* No. cxxiii. p. 126.

[27] I may add that the most candid of the adherents of the Church of Rome condemn the assumption of authority in matters of science, made, in this one instance at least, by the ecclesiastical tribunals. The author of the *Ages of Faith* (book viii. p. 248), says, 'A Congregation, it is to be lamented, declared the new system to be opposed to Scripture, and therefore heretical.'

314 With respect to the attempt made in my History to characterize the intellectual habits of Italy as produced by her religious condition,—certainly it would ill become any student of the history of science to speak slightingly of that country, always the mother of sciences, always ready to catch the dawn and hail the rising of any new light of knowledge. But I think our admiration of this activity and acuteness of mind is by no means inconsistent with the opinion, that new truths were promulgated more boldly beyond the Alps, and that the subtilty of the Italian intellect loved to insinuate what the rough German bluntly asserted. Of the decent duplicity with which forbidden opinions were handled, the

reviewer himself gives us instances, when he boasts of the liberality with which Copernican professors were placed in important stations by the ecclesiastical authorities, soon after the doctrine of the motion of the earth had been declared by the same authorities to be contrary to Scripture. And in the same spirit is the process of demanding from Galileo a public and official recantation of opinions which he had repeatedly been told by his ecclesiastical superiors he might hold as much as he pleased. I think it is easy to believe that among persons so little careful to reconcile public profession with private conviction, official decorum was all that was demanded. When Galileo had made his renunciation of the earth's motion on his knees, he rose and said, as we are told, *E pur si muove*—'and yet it *does* move.' This is sometimes represented as the heroic soliloquy of a mind cherishing its conviction of the truth, in spite of persecution; I think we may more naturally conceive it uttered as a playful epigram in the ear of a cardinal's secretary, with a full knowledge

that it would be immediately repeated to his master[28].

[28] I have somewhat further discussed the case of Galileo in the later editions of the *History*, book v. chap. iii. sect. 4.

Besides the Ideas involved in the material sciences, 315 of which we have already examined the principal ones, there is one Idea or Conception which our Sciences do not indeed include, but to which they not obscurely point; and the importance of this Idea will make it proper to speak of it, though this must be done very briefly.

CHAPTER V.

Of the Conception of a First Cause.

1. At the end of the last chapter but one, we were led to this result,—that we cannot, in any of the Palætiological Sciences, ascend to a beginning which is of the same nature as the existing cause of events, and which depends upon causes that are still in operation. Philosophers never have demonstrated, and probably never will be able to demonstrate, what was the original condition of the solar system, of the earth, of the vegetable and animal worlds, of languages, of arts. On all these subjects the course of investigation, followed backwards as far as our materials allow us to pursue it, ends at last in an impenetrable gloom. We strain our eyes in vain when we try, by our natural faculties, to discern an origin.

2. Yet speculative men have been constantly employed in attempts to arrive at that which thus seems to be placed out of their reach. The Origin of Languages, the Origin of the present Distribution of Plants and Animals, the Origin of the Earth, have been common subjects of diligent and persevering inquiry. Indeed inquiries respecting such subjects have been, at least till lately, the usual form which Palætiological researches have assumed. *Cosmogony*, the Origin of the World, of which, in such speculations, the earth was considered as a principal part, has been a favourite study both of ancient and of modern times: and most of the attempts at Geology previous to the present period have been *Cosmogonies* or *Geogonies*, rather than that more genuine science which we have endeavoured to delineate. Again: Glossology, though now an extensive body of solid knowledge, was 317 mainly brought into being by inquiries concerning the Original Language spoken by men; and the nature of the first separation and diffusion of languages, the first peopling of the earth

by man and by animals, were long sought after with ardent curiosity, although of course with reference to the authority of the Scriptures, as well as the evidence of natural phenomena. Indeed the interest of such inquiries even yet is far from being extinguished. The disposition to explore the past in the hope of finding, by the light of natural reasoning as well as by the aid of revelation, the origin of the present course of things, appears to be unconquerable. ‘What was the beginning?’ is a question which the human race cannot desist from perpetually asking. And no failure in obtaining a satisfactory answer can prevent inquisitive spirits from again and again repeating the inquiry, although the blank abyss into which it is uttered does not even return an echo.

3. What, then, is the reason of an attempt so pertinacious yet so fruitless? By what motive are we impelled thus constantly to seek what we can never find? Why are the errour of our conjectures, the futility of our reasonings, the precariousness of our

interpretations, over and over again proved to us in vain? Why is it impossible for us to acquiesce in our ignorance and to relinquish the inquiry? Why cannot we content ourselves with examining those links of the chain of causes which are nearest to us,—those in which the connexion is intelligible and clear; instead of fixing our attention upon those remote portions where we can no longer estimate its coherence? In short, why did not men from the first take for the subject of their speculations the Course of Nature rather than the Origin of Things?

To this we reply, that in doing what they have thus done, in seeking what they have sought, men are impelled by an intellectual necessity. They cannot conceive a Series of connected occurrences without a Commencement; they cannot help supposing a cause for the Whole, as well as a cause for each part; they cannot be satisfied with a succession of causes without 318 assuming a First Cause. Such an assumption is necessarily impressed upon our minds by our contemplation of a series

of causes and effects; that *there must be a First Cause*, is accepted by all intelligent reasoners as an Axiom: and like other Axioms, its truth is necessarily implied in the Idea which it involves.

4. The evidence of this axiom may be illustrated in several ways. In the first place, the axiom is assumed in the argument usually offered to prove the existence of the Deity. Since, it is said, the world now exists, and since nothing cannot produce something, something must have existed from eternity. This Something is the First Cause: it is God.

Now what I have to remark here is this:—the conclusiveness of this argument, as a proof of the existence of one independent, immutable Deity, depends entirely upon the assumption of the axiom above stated. The World, a series of causes and effects, exists: therefore there must be, not only this series of causes and effects, but also a First Cause. It will be easily seen, that without the axiom, that in every series of causes and

effects there must be a First Cause, the reasoning is altogether inconclusive.

5. Or to put the matter otherwise: The argument for the existence of the Deity was stated thus: Something exists, therefore something must have existed from eternity. ‘Granted,’ the opponent might say; ‘but this something which has existed from eternity, why may it not be this very series of causes and effects which is now going on, and which appears to contain in itself no indication of beginning or end?’ And thus, without the assumption of the necessity of a First Cause, the force of the argument may be resisted.

6. But, it may be asked, how do those who have written to prove the existence of the Deity reply to such an objection as the one just stated? It is natural to suppose that, on a subject so interesting and so long discussed, all the obvious arguments with their replies, have been fully brought into view. What is the result in this case? 319

The principal modes of replying to the

above objection, that the series of causes and effects which now exists, may have existed from eternity, appear to be these.

In the first place, our minds cannot be satisfied with a series of successive, dependent, causes and effects, without something first and independent. We pass from effect to cause, and from that to a higher cause, in search of something on which the mind can rest; but if we can do nothing but repeat this process, there is no use in it. We move our limbs, but make no advance. Our question is not answered, but evaded. The mind cannot acquiesce in the destiny thus presented to it, of being referred from event to event, from object to object, along an interminable vista of causation and time. Now this mode of stating the reply,—to say that the mind *cannot thus be satisfied*, appears to be equivalent to saying that the mind is conscious of a Principle, in virtue of which such a view as this must be rejected;—the mind takes refuge in the assumption of a First Cause, from an employment

inconsistent with its own nature.

7. Or again, we may avoid the objection, by putting the argument for the existence of a Deity in this form: The series of causes and effects which we call the *world*, or the *course of nature*, may be considered as a *whole*, and this whole must have a cause of its existence. The whole collection of objects and events may be comprehended as a single effect, and of this effect there must be a cause. This Cause of the Universe must be superior to, and independent of the special events, which, happening in time, make up the universe of which He is the cause. He must exist and exercise causation, before these events can begin: He must be the First Cause.

Although the argument is here somewhat modified in form, the substance is the same as before. For the assumption that we may consider the whole series of causes and effects as a *single effect*, is equivalent to the assumption that besides partial causes we must have a First Cause. And thus the Idea

of a First Cause, and 320 the axiom which asserts its necessity, are recognized in the usual argumentation on this subject.

8. This Idea of a First Cause, and the principle involved in the Idea, have been the subject of discussion in another manner. As we have already said, we assume as an axiom that a First Cause must exist; and we assert that God, the First Cause, exists eternal and immutable, by the necessity which the axiom implies. Hence God is said to exist necessarily;—to be a necessarily existing being. And when this *necessary existence* of God had been spoken of, it soon began to be contemplated as a sufficient reason, and as an absolute demonstration of His existence; without any need of referring to the world as an effect, in order to arrive at God as the cause. And thus men conceived that they had obtained a proof of the existence of the Deity, *à priori*, from Ideas, as well as *à posteriori*, from Effects.

9. Thus, Thomas Aquinas employs this reasoning to prove the *eternity* of God[29]:

‘Oportet ponere aliquod primum necessarium quod est per se ipsum necessarium; et hoc est Deus, cum sit prima causa ut dictum est: igitur Deus æternus est, cum omne necessarium per se sit æternum.’ It is true that the schoolmen never professed to be able to prove the *existence* of the Deity *à priori*: but they made use of this conception of necessary existence in a manner which approached very near to such an attempt. Thus Suarez[30] discusses the question, ‘Utrum aliquo modo possit *à priori* demonstrari Deum esse.’ And resolves the question in this manner: ‘Ad hunc ergo modum dicendum est: Demonstrato *à posteriori* Deum esse ens necessarium et a se, ex hoc attributo posse *à priori* demonstrari præter illud non posse esse aliud ens necessarium et a se, et consequenter demonstrari Deum esse.’

[29] Aquin. *Cont. Gentil.* lib. i. c. xiv. p. 21.

[30] *Metaphys.* tom. ii. disp. xxix. sect. 3, p. 28.

But in modern times attempts were made by Descartes and Samuel Clarke, to prove

the Divine 321 existence at once *à priori*, from the conception of necessary existence; which, it was argued, could not subsist without actual existence. This argumentation was acutely and severely criticised by Dr. Waterland.

10. Without dwelling upon a subject, the discussion of which does not enter into the design of the present work, I may remark that the question whether an *à priori* proof of the existence of a First Cause be possible, is a question concerning the nature of our Ideas, and the evidence of the axioms which they involve, of the same kind as many questions which we have already had to discuss. Is our Conception or Idea of a First Cause gathered from the effects we see around us? It is plain that we must answer, here as in other cases, that the Idea is not extracted from the phenomena, but assumed in order that the phenomena may become intelligible to the mind;—that the Idea is a necessary one, inasmuch as it does not depend upon observation for its evidence; but that it depends upon observation for its

development, since without some observation, we cannot conceive the mind to be cognizant of the relation of causation at all. In this respect, however, the Idea of a First Cause is no less necessary than the ideas of Space, or Time, or Cause in general. And whether we call the reasoning derived from such a necessity an argument *à priori* or *à posteriori*, in either case it possesses the genuine character of demonstration, being founded upon axioms which command universal assent.

11. I have, however, spoken of our *Conception* rather than of our *Idea* of a First Cause; for the notion of a First Cause appears to be rather a modification of the Fundamental Idea of Cause, which was formerly discussed, than a separate and peculiar Idea. And the Axiom, *that there must be a First Cause*, is recognised by most persons as an application of the general Axiom of Causation, *that every effect must have a Cause*; this latter Axiom being applied to the World, considered in its totality, as a single Effect. This distinction,

however, between an Idea and a Conception, is of no material consequence to our argument; provided we 322 allow the maxim, that there must be a First Cause, to be necessarily and evidently true; whether it be thought better to speak of it as an independent Axiom, or to consider it as derived from the general Axiom of Causation.

12. Thus we necessarily infer a First Cause, although the Palætiological Sciences only point *towards* it, and do not lead us *to* it. But I must observe further; that in each of the series of events which form the subject of Palætiological research, the First Cause is the *same*. Without here resting upon reasoning founded upon our Conception of a First Cause, I may remark that this identity is proved by the close connexion of all the branches of natural science, and the way in which the causes and the events of each are interwoven with those which belong to the others. We must needs believe that the First Cause which produced the earth and its atmosphere is

also the Cause of the plants which clothe its surface; that the First Cause of the vegetable and of the animal world are the same; that the First Cause which produced light produced also eyes; that the First Cause which produced air and organs of articulation produced also language and the faculties by which language is rendered possible: and if *those* faculties, then also all man's other faculties;—the powers by which, as we have said, he discerns right and wrong, and recognises a providential as well as a natural course of things. Nor can we think otherwise than that the Being who gave these faculties, bestowed them for some purpose;—bestowed them for that purpose which alone is compatible with their nature:—the purpose, namely, of guiding and elevating man in his present career, and of preparing him for another state of being to which they irresistibly direct his hopes. And thus, although, as we have said, no one of the Palætiological Sciences can be traced continuously to an Origin, yet they not only each point to an Origin, but all to the same Origin. Their

lines are broken indeed, as they run backwards into the early periods of the world, but yet they all appear to converge to the same invisible point. And 323 this point, thus indicated by the natural course of things, can be no other than that which is disclosed to us as the starting-point of the providential course of the world; for we are persuaded by such reasons as have just been hinted, that the Creator of the natural world can be no other than the Author and Governor and Judge of the moral and spiritual world.

13. Thus we are led, by our material Sciences, and especially by the Palætiological class of them, to the borders of a higher region, and to a point of view from which we have a prospect of other provinces of knowledge;—to contemplations in which other faculties of man are concerned besides his intellectual, other interests involved besides those of speculation. On these it does not belong to our present plan to dwell: but even such a brief glance as we have taken of the

connexion of material with moral speculations may not be useless, since it may serve to show that the principles of truth which we are now laboriously collecting among the results of the physical sciences, may possibly find some application in those parts of knowledge towards which men most naturally look with deeper interest and more serious reverence.

We have been employed hitherto in examining the materials of knowledge, Facts and Ideas;—Facts in our former History, and Ideas in the present History. We have dwelt at length on this latter element; inasmuch as the consideration of it is, on various accounts, and especially at the present time, by far the most important, having hitherto been least distinctly attended to as a special element of scientific knowledge.

There still remains an important task, with a view to which we have undertaken this survey of the past course of human

thought and discovery:—namely, the task of determining the processes by which these materials may actually be made to constitute knowledge. 324 We have surveyed the stones which lie before us, partly built and partly ready for building: we have found them exactly squared, and often curiously covered with significant imagery and important inscriptions. We have now to discover how they may best be fitted into their places, and cemented together, so that rising stage above stage, they may grow at last into that fair and lofty temple of Truth, for which we cannot doubt that they were intended by the Great Architect.

This task, the description of the processes by which Scientific Truth is discovered and established, we shall, as has already been said, entitle, in reference to previous attempts of the same kind, *Novum Organum Renovatum*.

END OF VOL. II.

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Transcriber's Notes

Whewell published the first edition of the *Philosophy of the Inductive Sciences* in 1840, as a companion to the 1837 *History of the Inductive Sciences*. Revised second editions of both works appeared in 1847. The third editions saw a major reshaping of the *Philosophy*: a two volume *History of Scientific Ideas* (1858 - the present text, relying upon resources kindly provided by the Internet Archive), *Novum Organon Renovatum* (1858), and *On the Philosophy of Discovery: chapters historical and critical* (1860 - already in Project Gutenberg's collection: #5155).

The present text has combined the two volumes into one continuous text, and has moved the Table of Contents of volume 2 to follow the first volume's Contents. Footnotes are numbered by Book; in the original, notes were numbered by chapter. Page numbers appear in colour; where a word was hyphenated across pages the number has been placed before the word.

There is one significant emendation to report. For Book IX chapter VI, the Table of Contents lists 20 articles, but the actual text has only 19

numbered paragraphs. The text version leaves this inconsistency untouched; in the htm version, a correction has been made by numbering the paragraph beginning on p. 244 as #9, and renumbering those that follow, thereby matching the descriptions in the Table of Contents. A few other emendations are indicated by dotted red underline, the change appearing on mouse-over.

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