HISTORY

OF

SCIENTIFIC IDEAS.

VOLUME I.

Cambridge:
PRINTED BY C. J. CLAY, M.A.
AT THE UNIVERSITY PRESS.

Vol 1.

HISTORY

OF

SCIENTIFIC IDEAS.

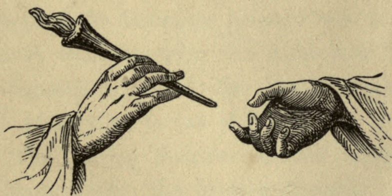
BY WILLIAM WHEWELL, D.D.,

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

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.

CONTENTS

OF

THE FIRST VOLUME.

BOOK II.

THE PHILOSOPHY OF THE PURE SCIENCES.

BOOK III.

THE PHILOSOPHY OF THE MECHANICAL SCIENCES.

BOOK IV.

THE PHILOSOPHY OF THE SECONDARY MECHANICAL SCIENCES.

BOOK V.

OF THE PHILOSOPHY OF THE MECHANICO-CHEMICAL SCIENCES.

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 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 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 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.

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 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 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 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 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

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

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.

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 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.

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 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 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 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. I. Aphor. xviii.

BOOK I.

OF IDEAS IN GENERAL.

CHAPTER I.

OF THE FUNDAMENTAL ANTITHESIS OF PHILOSOPHY.

Sect. I.—*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.

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.

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 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 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 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, *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 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 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 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 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 knowedge; 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 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.

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 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 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

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 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 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 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.

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,

---

[1] Ferrum exercebant vasto Cyclopes in Antro
Brontesque Steropesque et nudus membra Pyracmon;
His informatum manibus, jam parte polita
Fulmen erat.—*Æn.* viii. 424.

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 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 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 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 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 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 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?

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.

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.

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

---

[2] *Hist. Inductive Sciences*, b. vii. c. ii. sect. 5.

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.

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

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

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

---

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

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.

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 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

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 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

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 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 is insisted upon by almost all the best modern, as well as ancient, metaphysicians[1], 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.

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

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

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 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.

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. I HERE 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 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 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 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

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.

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 *conlcusion*.

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

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[1].' 'The only truth which such propositions can possess consists in conformity to the original principles.'

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 man-

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

ner 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

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

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.

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[1], 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.

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

---

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

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

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 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, 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[1]. 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.

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,

---

[1] *History of the Inductive Sciences.*

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 VI. 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

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 unparaleled 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,

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

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

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,

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.

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*

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, 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 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*.

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

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].

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 there-

[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.

fore 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 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.

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.

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[1].

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.

1. I had dissented from Stewart's assertion that mathematical truth is hypothetical, or depends upon arbitray definitions; since we understand by an hypo-

[1] 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.

thesis 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.

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

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 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

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

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 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

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 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.

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 mani-

festly 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

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[1]: '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,

---

[1] Phillips *On Painting*.

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.'

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.

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[2], and is a doctrine now generally assented to by metaphysical speculators.

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

---

[2] *Theory of Vision.*

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[3] 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.

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,

---

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

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[4], 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.

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

---

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

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

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[5].

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

---

[5] 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.

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[6] 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

---

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

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[7], is the means by which we become aware of the position of objects about us.

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

---

[7] *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.

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[8] says of Soracte, that it

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

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

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

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[9].

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

---

[9] 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.

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

K 2

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

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

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,

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 stíll,

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 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 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.

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

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

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[1].

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

---

[1] 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.

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 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 I. 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 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 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 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 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[1]. 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.

The ancients did not expressly introduce this conception of a Limit into their mathematical reasonings; although in the application of what is termed the

---

[1] 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.*

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

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 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

of Wheels, and Mr. Babbage's memoir[1] 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 sciences, 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.

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

1 *On a Method of expressing by Signs the action of Machinery. Phil. Trans.* 1826, p. 250.

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*[2], 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[3].

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.

---

[2] P. 528.

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

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

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[1] 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*[2], 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

---

[1] *Hist. Ind. Sc.* b. iii. c. ii. [2] *Epinomis*, p. 990.

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.'

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 establish-

ing 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[3]; and by employing also the Tangents of arcs, or, as they called them[4], *upright shadows.*

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

---

[3] Delambre, *Ast., M. A.*, p. 12. [4] *Ibid.* p. 17.

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[5]. 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[6], 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

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

[6] *Ibid.* p. 175.

are still labouring up to the present day. This course was not without its checks and perplexities. We have elsewhere quoted[7] 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.

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

---

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

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

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[8], 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.

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[9];—that Fresnel obtained one of the

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

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

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.

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.* II Th. I Abth. II 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 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:

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

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.

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.

---

[1] Book i. chap. xii.

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

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[1] 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[2].

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 asso-

---

1 *Essay on the Human Understanding*, b. ii. c. xxvi. 2 B. iv. c. x.

ciated 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[3]. 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.

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

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

maxim that "Every event must have a cause," to be an instinctive law of belief, or a fundamental principle of the human mind[4].

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[5], 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.

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[6], '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

---

[4] Stewart's *Active Powers*, vol. i. p. 347. Browne's *Lectures*, vol. i. p. 115.
[5] *Lectures*, vol. i. p. 114.
[6] *Lectures*, vol. i. p. 127.

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*.

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 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

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 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 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 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 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 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 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 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 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 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, 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 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 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 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 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 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 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, 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,* 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 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 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*, Ϝὶς, 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
> ἧκ' ἐπιδινήσας, ἐπέρεισε δὲ ϜΙ͂Ν' 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.

Homer[1] speaks of the *force* of the river, Ϝὶς ποταμοῖο; and Hesiod[2] of the *force* of the north wind, Ϝὶς ἀνέμου βορέαο.

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

---

1 *Il.* xxi.

2 *Op. et D.*

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 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 our muscular effort[3]. 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.

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

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

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[4], 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.

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

---

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

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.

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.

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 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 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 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.

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 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 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* *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.

7. *A Force may be supposed to act at any Point of its Direction.*—It has been stated in the history of Mechanics[1], 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.

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

---

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

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[2]. 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

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

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.

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

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[3], '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.

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

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

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[4]. 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.

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,

---

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

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

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[5], its motion must go on for ever, which is absurd. It may be asked how

[5] Stevin. *Statique*, livre i. prop. 19.

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.

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,

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*[6], 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.

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

[6] See *Hist. Ind. Sc.* b. vi. c. ii. sect. 4.

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 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*[7]; and by Marriotte in his *Traité de l'Équilibre des Liquides*, from whom it has generally been termed Marriotte'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[8], 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.

Now with regard to the principle that fluids press equally in all directions, the case is altogether different.

---

[7] Shaw's *Boyle*, Vol. ii. p. 671.

[8] 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.

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,

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 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

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 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[1], 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.

---

[1] *Dynamique.*

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[2]. 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.

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 frac-

---

[2] *Outlines of Natural Philosophy*, p. 26.

tion, is lost in each second[3]: and where is the absurdity of supposing such an expression really to represent the velocity?

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[4], "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."

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

[3] 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, $C\left(\frac{99}{100}\right)^t$.

[4] Poisson, *Dynamique*, ed. 2, art. 113.

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[5], as one of the earliest positive advances in the doctrine of motion. These laws were first rightly stated by Galileo, and esta-

---

[5] *Hist. Ind. Sc.* b. vi. c. ii. sect. 2.

blished 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.

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 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 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[6]. 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.

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

---

[6] *Dialogo*, iii. p. 95.

accelerated, consists, in the first place, in adducing the maxim that nature always employs the most simple means[7]. 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.'

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[8]. 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

---

[7] *Dialogo*, iii. p. 91. [8] *Dialogo*, iii. p. 36.

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.

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

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 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[9], 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.

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

[9] *Dialogo*, ii. p. 114.

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[10] And

[10] 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.)

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.

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

---

'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.

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 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[11] 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.

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[12], '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,

---

[11] *Dial. della Sc. Nuov.* iii. p. 96. See *Hist. Ind. Sci.* b. vi. c. ii. sect. 5.

[12] *Dialogo*, p. 104.

in the school of Galileo, to have been suggested and established at first by experiment, but afterwards confirmed and demonstrated by *à priori* considerations.

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 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[13], '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

[13] *Dynamique*, Pref. p. x.

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[14], '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[15], '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.'

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.

---

14 *Méc. Cél.* p. 15.

15 p. 18.

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 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 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

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, 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[16], 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.

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

---

[16] B. vi. c. v. sect. 2.

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 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[17], 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[18], 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 know-

---

[17] B. vi. c. v. sect. 2.

[18] 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.

ledge 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 battle-ground in the war of the *vis viva*.

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 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[1] 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.

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

[1] B. i. c. iv. Of Experience

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 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 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[2] 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

---

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

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.

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

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

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[3] 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

[3] Called the Bo-me-rang.

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.

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 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*[1], 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

[1] B. vii. c. i.

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.

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[2]. But a few years ago, doubts

[2] *Prin.* lib. iii. prop. 6.

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.

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):

'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

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[3]. 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

---

[3] See Vince, *Observations on the Hypothesis respecting Gravitation*, and the Critique of that work, *Edinb. Rev.* vol. xiii.

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.'

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[4]), 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.

Undoubtedly the existence of attraction is rendered more acceptable to common apprehension by supposing

---

[4] *Ancient Metaphysics*, vol. ii. p. 175.

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

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[1]. 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[2]. Their idea of mechanical action was not sufficiently distinct to enable them to see the absurdity of suppos-

---

1 *Hist. Ind. Sc.* b. iv. c. i. sect. 2.

2 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.

ing an intense pressure with no obstacle for it to exert itself against.

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[3] 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[4].

---

[3] *Hist. Ind. Sc.* b. v. c. iv. and b. vii. c. i.

[4] I have, in the History, applied to Descartes the character which Bacon gives to Aristotle, 'Audax simul et pavidus:' though he was bold enough

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*[5], 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

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.

[5] *Anc. Met.* vol. ii. b. v. c. vi. p. 413.

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.

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[6].' 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.

5. A similar absence of distinct mechanical thought appears in some of the most celebrated metaphysicians of Germany. I have elsewhere noted[7] the opinion expressed by Hegel, that the glory which belongs to

---

[6] *Anc. Met.* vol. i. b. ii. c. 19, p. 264.

[7] *Hist. Ind. Sc.* b. vii. c. ii. sect. 5.

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.'

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, 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*

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

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

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 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 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 χρώματος ἀδύνατον, λείπεται δὲ 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 ἀνάγκης· τὸ γὰρ διαφανὲς ὑπὸ τούτου γίνεται διαφανές. ὁ δ' αὐτὸς λόγος καὶ περὶ ψόφου καὶ ὀσμῆς ἐστίν· οὐθὲν γὰρ αὐτῶν ἁπτόμενον τοῦ αἰσθητηρίου ποιεῖ τὴν αἴσθησιν, 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 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.'

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.'

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.

---

[1] *Essay*, b. ii. ch. viii. s. 9, 10.

[2] *Essays*, b. ii. c. xvii.

[3] *Lectures*, ii. 12.

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 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 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

[4] Περὶ Ψυχῆς. ii. 7. See the motto to this Book.

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.

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 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.

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

---

[5] *De Rerum Naturâ*, Lib. iv. 529.

[6] Brown, vol. ii. p. 98.

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.'

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

[7] *Hist. Son. et Aud.* vol. ix. p. 87.

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 per-

ceiving 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

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 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

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 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 network 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 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 inverted? This cannot be, for we look at them as we do at other objects. Do we expect that things should appear to fall 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*[1].

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

[1] 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.

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.

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[2].

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,

[2] 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.

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[3], 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.

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[4] 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

---

[3] See Brown, vol. ii. p. 81.

[4] *Lectures*, vol. ii. p. 81.

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.

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 will 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 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[5] 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.

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

---

[5] *Phil. Trans.* 1839.

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 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

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[6] 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[7]: '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?

6 *Lectures*, vol. ii. p. 82.

7 *Ib.* vol. ii. p. 90.

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.

The whole of Brown's reasoning on this subject is so irreconcileable 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[8], 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.

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

[8] *Lectures*, vol. ii. p. 83.

again[9], 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.

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[10]. 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

[9] *Lectures*, vol. ii. p. 84.

[10] *Ib.* p. 87.

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[11].

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[12].' 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[13], '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 large one, it is perceived at once to be a sphere of

---

[11] 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?

[12] *Lectures*, vol. ii. p. 57.

[13] *Ib.* vol. ii. p. 89.

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?

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 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

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[1], 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.

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

---

[1] *Supr.* p. 297.

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 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 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 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, 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 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 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[2]. Other effects of heat, however, as for instance *latent heat*[3], and the change of *consistence* of bodies[4], were not satisfactorily brought into connexion with the hypothesis; while *con-*

---

[2] See the Account of the Theory of Exchanges, *Hist. Ind. Sc.* b. x. c. i. sect. 2.

[3] *Ib.* c. ii. sect. 3.

[4] *Ib.* c. ii. sect. 2.

duction[5], which at first did not appear to result from the fundamental assumption, was to a certain extent explained as internal radiation.

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.

---

[5] *Ib.* c. i. sect. 7.

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 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 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.

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

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 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[1]; 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 hyperbolœôn*.

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 vi-

[1] Burney's *History of Music*, vol. i. p. 28.

brate. 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[2], 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.

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 dis-

[2] *Harmonia*, lib. ii. prop. 19.

cernible 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 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

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.

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 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 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[3].

---

[3] 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,*

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,

---

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.

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[4], 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[5].

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

---

[4] *Phil. Trans.* 1829, p. 19.

[5] 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.

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

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*[6]. 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.*

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

[6] *Phil. Trans.* 1701.

of the researches which we have just mentioned, says[7], '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.

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.

---

[7] *Modif. de l'Atmosph.* 1782, p. 303.

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*[8] 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

---

[8] *Recherches sur la Chaleur*, 1791. *Hist. Ind. Sc.* b. x. c. i. sect. 2.

upon the excess of the radiation for a temperature $\theta + t$, above the radiation for a temperature θ.

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[9]. 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.

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[10], '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

---

[9] 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.

[10] *Annales de Chimie*, vii. 153.

Dalton and Gay Lussac[11],] 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.'

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[12], 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.

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

---

11 *Hist. Ind. Sc.* b. x. c. ii. sect. 1.

12 *Ib.* c. iv.

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.

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.	Nasal.
p	b	ph (*f*)	bh (*v*)	m
k	g (hard)	kh	gh	ng
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[13]. 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*.

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

---

[13] *Camb. Trans.* vol. iii. p. 239.

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 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.'

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

---

[1] *De Magn.* lib. i. c. iii.

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 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

[2] *Researches*, 516.

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.

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

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 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

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

[3] *Essay on the Theory of Chemical Properties*, 1820, p. 113.

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.

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 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.'

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.

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

[4] DUMAS, *Leçons sur la Philosophie Chimique*, p. 401.

[5] *Bridgewater Treatise*, p. 559.

[6] *Phil. Tr.* 1826, p. 415.

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.

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.

---

[7] *Researches*, p. 495, &c.

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*, crystalization 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 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 cur-

rent 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.

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[1]: '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.

---

1 *Researches*, 244.

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[2] of 1826, on the Relations of Electrical and

[2] *Phil. Trans.* 1826, p. 383.

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[3] 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[4], 'among other persons, as having imagined or anticipated the relation between electrical powers and chemical affinities before the discovery of the pile

[3] *Phil. Trans.* 1826, p. 389.

[4] *Ibid.* p. 384.

of Volta. But whoever will read with attention Ritter's "Evidence that Galvanic action exists in organised nature," and Winter'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.

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[5] 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.'

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[6] 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 essen-

---

[5] *Ann. Chim.* lxviii. 54.

[6] *Hist. Ind. Sc.* b. xiv. c. ix. sect. 1.

tially 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[7] 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[8] 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[9] 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[10], 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[11].

7. It will, perhaps, be said that this beautiful train of discovery was entirely due to experiment, and not to any *a priori* conviction that co-existent polarities

---

[7] See Faraday's Historical Sketch, *Researches*, 481—492.

[8] Art. 524.

[9] In 1834. Eleventh Series of *Researches*. Art. 662.

[10] *Hist. Ind. Sc.* b. xiv. c. ix. sect. 2.

[11] Arts. 915, 916, 917.

must be connected. I trust I have sufficiently stated that such an *a 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[12], '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.

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

---

[12] Art. 1658.

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[13] 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[14] 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.

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

---

[13] *Essay on Chemical Prop.* 113.

[14] *Researches.* Art. 1689.

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 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[15], 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

---

[15] *Ann. Chim.*, Tom. l. (1804), p. 191.

some years' labour, the author of one of the most remarkable and fertile physical discoveries of his time.

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[16], '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[17]: '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.'

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 electro-

---

16 P. 223.

17 P. 486.

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[18] 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.

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*[19]. 'Since,'

---

[18] Ueber Faraday's *Neueste Entdeckung. München.* 1832.

[19] Sec. 278.

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.

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

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.

END OF VOL. I.

Cambridge: Printed at the University Press.