NOVUM ORGANON RENOVATUM.

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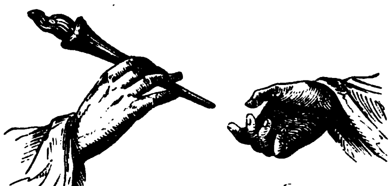
BEING THE SECOND PART OF THE PHILOSOPHY
OF THE INDUCTIVE SCIENCES.

THE THIRD EDITION, WITH LARGE ADDITIONS.



ΛΑΜΠΑΔΙΑ ΕΧΟΝΤΕΣ ΔΙΑΔΩΣΟΥΣΙΝ ΑΛΛΗΛΟΙΣ.

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It is to our immortal countryman, Bacon, that we owe the broad announcement of this grand and fertile principle; and the developement of the idea, that the whole of natural philosophy consists entirely of a series of inductive generalizations, commencing with the most circumstantially stated particulars, and carried up to universal laws, or axioms, which comprehend in their statements every subordinate degree of generality; and of a corresponding series of inverted reasoning from generals to particulars, by which these axioms are traced back into their remotest consequences, and all particular propositions deduced from them; as well those by whose immediate considerations we rose to their discovery, as those of which we had no previous knowledge.

HERSCHEL, *Discourse on Natural Philosophy*, Art. 96.

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PREFACE.

Even if Bacon's *Novum Organon* had possessed the character to which it aspired as completely as was possible in its own day, it would at present need renovation: and even if no such book had ever been written, it would be a worthy undertaking to determine the machinery, intellectual, social and material, by which human knowledge can best be augmented. Bacon could only divine how sciences might be constructed; we can trace, in their history, how their construction has taken place. However sagacious were his conjectures, the facts which have really occurred must give additional instruction: however large were his anticipations, the actual progress of science since his time has illustrated them in all their extent. And as to the structure and operation of the *Organ* by which truth is to be collected from nature,—that is, the Methods by which science is to be promoted—we know that, though Bacon's general maxims are sagacious and animating, his particular precepts failed in his hands, and are now practically useless. This, perhaps, was not wonderful, seeing that they were, as I have said, mainly derived from conjectures respecting knowledge and the progress of knowledge; but at the present day, when, in several provinces of knowledge, we have a large actual progress of solid truth to look back upon, we may make the like attempt with the prospect of better success, at least on that ground. It may be a task, not hopeless, to extract from the past progress of science the elements of an effectual and substantial method of Scientific Discovery. The advances which have, during the last three centuries, been made in the physical sciences;—in Astronomy, in Physics, in Chemistry, in Natural History, in Physiology;—these are allowed by all to be real, to be great, to be striking; may it not be that the steps of progress in these different cases have in them something alike? May it not be that in each advancing movement of such knowledge there is some common principle, some common process? May it not be that discoveries are made by an *Organ* which has something uniform in its working? If we can shew that this is so, we shall have the *New Organ*, which Bacon aspired to construct, *renovated* according to our advanced intellectual position and office.

It was with the view of opening the way to such an attempt that I undertook that survey of the past progress of physical knowledge, of which I have given the results in the *History of the Sciences*, and the *History of Scientific Ideas*[1]; the former containing the history of the sciences, so far as it depends on

[1] Published in two former editions as part of the *Philosophy of the Inductive Sciences* (b. i.—x.).

observed *Facts;* the latter containing the history of those *Ideas* by which such Facts are bound into Theories.

It can hardly happen that a work which treats of Methods of Scientific Discovery, shall not seem to fail in the positive results which it offers. For an Art of Discovery is not possible. At each step of the investigation are needed Invention, Sagacity, Genius, —elements which no art can give. We may hope in vain, as Bacon hoped, for an Organ which shall enable all men to construct Scientific Truths, as a pair of compasses enables all men to construct exact circles[a]. This cannot be. The practical results of the Philosophy of Science must be rather classification and analysis of what has been done, than precept and method for future doing. Yet I think that the methods of discovery which I have to recommend, though gathered from a wider survey of scientific history, both as to subjects and as to time, than (so far as I am aware) has been elsewhere attempted, are quite as definite and practical as any others which have been proposed; with the great additional advantage of being the methods by which all great discoveries in science have really been made. This may be said, for instance, of *the Method of Gradation* and *the Method of Natural Classification*, spoken of b. iii. c. viii; and in a narrower sense, of *the Method of Curves, the Method of*

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[a] *Nov. Org.* lib. i. aph. 61.

Means, the Method of Least Squares and *the Method of Residues*, spoken of in chap. vii. of the same Book. Also the Remarks on the *Use of Hypotheses* and on the *Tests of Hypotheses* (b. ii. c. v) point out features which mark the usual course of discovery.

But one of the principal lessons resulting from our views is undoubtedly this:—that different sciences may be expected to advance by different modes of procedure, according to their present condition; and that in many of these sciences, an Induction performed by any of the methods which have just been referred to is not the next step which we may expect to see made. Several of the sciences may not be in a condition which fits them for such a *Colligation of Facts;* (to use the phraseology to which the succeeding analysis has led me). The Facts may, at the present time, require to be more fully observed, or the Idea by which they are to be colligated may require to be more fully unfolded.

But in this point also, our speculations are far from being barren of practical results. The examination to which we have subjected each science, gives us the means of discerning whether what is needed for the further progress of the science, has its place in the Observations, or in the Ideas, or in the union of the two. If observations be wanted, the Methods of Observation, given in b. iii. c. ii. may be referred to. If those who are to make the next discoveries need, for that purpose, a developement of their Ideas, the modes in which such a developement has usually taken place are treated of in Chapters iii. and iv. of that Book.

No one who has well studied the history of science can fail to see how important a part of that history is the explication, or as I might call it, the *clarification* of men's Ideas. This, the metaphysical aspect of each of the physical sciences, is very far from being, as some have tried to teach, an aspect which it passes through at an early period of progress, and previously to the stage of positive knowledge. On the contrary, the metaphysical movement is a necessary part of the inductive movement. This, which is evidently so by the nature of the case, was proved by a copious collection of historical evidences, in the *History of Scientific Ideas*. The ten Books of that History contain an account of the principal philosophical controversies which have taken place in all the physical sciences, from Mathematics to Physiology. These controversies, which must be called *metaphysical* if anything be so called, have been conducted by the greatest discoverers in each science, and have been an essential part of the discoveries made. Physical discoverers have differed from barren speculators, not by having *no* metaphysics in their heads, but by having *good* metaphysics in their heads while their adversaries had bad; and by binding their metaphysics to their physics, instead of keeping the two asunder. I trust that the *History of Scientific Ideas* is of some value, even as a record of a number of remarkable controversies; but I conceive that it also contains an indisputable proof that there is, in progressive science, a metaphysical as well as a physical element;—ideas as well as facts;—thoughts as well as things. Metaphysics is the process of ascertaining that thought is consistent with itself: and if it be not so, our supposed knowledge is not knowledge.

In Chapter vi. of the Second Book, I have spoken of *the Logic of Induction*. Several writers[3] have quoted very emphatically my assertion that the Logic of Induction does not exist in previous writers: using it as an introduction to Logical Schemes of their own. They seem to have overlooked the fact that at the same time that I noted the deficiency, I offered a scheme which I think fitted to supply this want. And I am obliged to say that I do not regard the schemes proposed by any of those gentlemen as at all satisfactory for the purpose. But I must defer to a future occasion any criticism of authors who have written on the subjects here treated. A critical notice of such authors formed the Twelfth Book of the former edition of the *Philosophy of the Sciences*. I have there examined the opinions concerning the Nature of Real Knowledge and the mode of acquiring it, which have been promulgated in all ages, from Plato and Aristotle, to Roger Bacon, to Francis Bacon, to Newton, to Herschel. Such a survey, with the additions which I should now have to make to it, may hereafter be put forth as a separate book: but I

[3] Apelt *Die Theorie der Induction:* Gratry *Logique.*

have endeavoured to confine the present volume to such positive teaching regarding Knowledge and Science as results from the investigations pursued in the other works of this series. But with regard to this matter, of the *Logic of Induction*, I may venture to say, that we shall not find anything deserving the name explained in the common writers on Logic, or exhibited under the ordinary Logical Forms. *That* in previous writers which comes the nearest to the notice of such a Logic as the history of science has suggested and verified, is the striking declaration of Bacon in two of his Aphorisms (b. i. aph. civ. cv.).

"There will be good hopes for the Sciences then, and not till then, when by a true SCALE or Ladder, and by successive steps, following continuously without gaps or breaks, men shall ascend from particulars to the narrower Propositions, from those to intermediate ones, rising in order one above another, and at last to the most general.

"But in establishing such propositions, we must devise some other FORM OF INDUCTION than has hitherto been in use; and this must be one which serves not only to prove and discover *Principles*, (as very general Propositions are called,) but also the narrower and the intermediate, and in short, all true Propositions."

And he elsewhere speaks of successive FLOORS of Induction.

All the truths of an extensive science form a Series of such Floors, connected by such Scales or Ladders; and a part of the Logic of Induction consists, as I conceive, in the construction of a *Scheme* of such Floors. Converging from a wide basis of various classes of particulars, at last to one or a few general truths, these schemes necessarily take the shape of a Pyramid. I have constructed such Pyramids for Astronomy and for Optics[4]; and the illustrious Von Humboldt in speaking of the former subject, does me the honour to say that my attempt in that department is perfectly successful[5]. The Logic of Induction contains other portions, which may be seen in the following work, b. ii. c. vi.

I have made large additions to the present edition, especially in what regards the Application of Science, (b. iii. c. ix.) and the Language of Science. The former subject I am aware that I have treated very imperfectly. It would indeed, of itself, furnish material for a large work; and would require an acquaintance with practical arts and manufactures of the most exact and extensive kind. But even a general observer may see how much more close the union of Art with Science is now than it ever was before; and what large and animating hopes this union inspires, both for the progress of Art and of Science. On another subject also I might have dilated to a great extent, —what I may call (as I have just now called it) the *social* machinery for the advancement of science. There can be no doubt that at certain stages of sciences,

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[4] See the Tables at the end of book ii.

[5] *Cosmos*, vol. ii. n. 35.

Societies and Associations may do much to promote their further progress; by combining their observations, comparing their views, contributing to provide material means of observation and calculation, and dividing the offices of observer and generalizer. We have had in Europe in general, and especially in this country, very encouraging examples of what may be done by such Associations. For the present I have only ventured to propound one Aphorism on the subject, namely this; (Aph. LV.) That it is worth considering whether a continued and connected system of observation and calculation, like that of Astronomy, might not be employed in improving our knowledge of other subjects; as Tides, Currents, Winds, Clouds, Rain, Terrestrial Magnetism, Aurora Borealis, composition of crystals, and the like. In saying this, I have mentioned those subjects which are, as appears to me, most likely to profit by continued and connected observations.

I have thrown the substance of my results into Aphorisms, as Bacon had done in his *Novum Organum.* This I have done, not in the way of delivering dogmatic assertions or oracular sentences; for the Aphorisms are all supported by reasoning, and were, in fact, written after the reasoning, and extracted from it. I have adopted this mode of gathering results into compact sentences, because it seems to convey lessons with additional clearness and emphasis.

I have only to repeat what I have already said; that this task of adapting the *Novum Organum* to the present state of Physical Science, and of constructing a *Newer Organ* which may answer the purposes at which Bacon aimed, seems to belong to the present generation; and being here founded upon a survey of the past history and present condition of the Physical Sciences, will I hope, not be deemed presumptuous.

TRINITY LODGE,
1 *November*, 1858.

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NOVUM ORGANON RENOVATUM.

De Scientiis tum demum bene sperandum est, quando per Scalam veram et per gradus continuos, et non intermissos aut hiulcos, a particularibus ascendetur ad Axiomata minora, et deinde ad media, alia aliis superiora, et postremo demum ad generalissima.

In constituendo autem Axiomate, Forma Inductionis alia quam adhuc in usu fuit, excogitanda est; et quæ non ad Principia tantum (quæ vocant) probanda et invenienda, sed etiam ad Axiomata minora, et media, denique omnia.

Bacon, *Nov. Org.*, Aph. civ. cv.

NOVUM ORGANON RENOVATUM.

THE name *Organon* was applied to the works of Aristotle which treated of Logic, that is, of the method of establishing and proving knowledge, and of refuting errour, by means of Syllogisms. Francis Bacon, holding that this method was insufficient and futile for the augmentation of real and useful knowledge, published his *Novum Organon*, in which he proposed for that purpose methods from which he promised a better success. Since his time real and useful knowledge has made great progress, and many Sciences have been greatly extended or newly constructed; so that even if Bacon's method had been the right one, and had been complete as far as the progress of Science up to his time could direct it, there would be room for the revision and improvement of the methods of arriving at scientific knowledge.

Inasmuch as we have gone through the *Histories* of the principal *Sciences*, from the earliest up to the present time, in a previous work, and have also traced the *History of Scientific Ideas* in another work, it may perhaps be regarded as not too presumptuous if we attempt this revision and improvement of the methods by which Sciences must rise and grow. This is our task in the present volume; and to mark the reference of this undertaking to the work of Bacon, we name our book *Novum Organon Renovatum.*

Bacon has delivered his precepts in Aphorisms, some of them stated nakedly, others expanded into dissertations. The general results at which we have arrived by tracing the history of Scientific Ideas are the groundwork of such Precepts as we have to give: and I shall therefore begin by summing up these results in Aphorisms, referring to the former work for the historical proof that these Aphorisms are true.

NOVUM ORGANON RENOVATUM.

BOOK I.

APHORISMS CONCERNING IDEAS DERIVED FROM THE HISTORY OF IDEAS.

I.

MAN is the Interpreter of Nature, Science the right interpretation. (*History of Scientific Ideas:* Book I. Chapter 1.)

II.

The Senses *place before us the* Characters *of the Book of Nature; but these convey no knowledge to us, till we have discovered the Alphabet by which they are to be read.* (Ibid. I. 2.)

III.

The Alphabet, *by means of which we interpret Phenomena, consists of the* Ideas *existing in our own minds; for these give to the phenomena that coherence and significance which is not an object of sense.* (I. 2.)

IV.

The antithesis of Sense *and* Ideas *is the foundation of the Philosophy of Science. No knowledge can exist without the union, no philosophy without the separation, of these two elements.* (I. 2.)

V.

Fact *and* Theory *correspond to Sense on the one hand, and to Ideas on the other, so far as we are* conscious *of our Ideas: but all facts involve ideas* unconsciously; *and thus the distinction of Facts and Theories is not tenable, as that of Sense and Ideas is.* (I. 2.)

VI.

Sensations and Ideas in our knowledge are like Matter and Form in bodies. Matter cannot exist without Form, nor Form without Matter: yet the two are altogether distinct and opposite. There is no possibility either of separating, or of confounding them. The same is the case with Sensations and Ideas. (I. 2.)

VII.

Ideas are not trans*formed, but* in*formed Sensations; for without ideas, sensations have no form.* (I. 2.)

VIII.

The Sensations are the Objective, *the Ideas the* Subjective *part of every act of perception or knowledge.* (I. 2.)

IX.

General Terms denote Ideal Conceptions, *as a* circle, *an* orbit, *a* rose. *These are not* Images *of real things, as was held by the Realists, but Conceptions: yet they are conceptions, not bound together by mere* Name, *as the Nominalists held, but by an Idea.* (I. 2.)

X.

It has been said by some, that all Conceptions are merely states *or* feelings of the mind, *but this assertion only tends to confound what it is our business to distinguish.* (I. 2.)

XI.

Observed Facts are connected so as to produce new truths, by superinducing upon them an Idea: and such truths are obtained by Induction. (I. 2.)

XII.

Truths once obtained by legitimate Induction are Facts: these Facts may be again connected, so as to produce higher truths: and thus we advance to Successive Generalizations. (I. 2.)

XIII.

Truths obtained by Induction are made compact and permanent by being expressed in Technical Terms. (I. 3.)

XIV.

Experience cannot conduct us to universal and necessary truths:—Not to universal, because she has not tried all cases:—Not to necessary, because necessity is not a matter to which experience can testify. (I. 5.)

XV.

Necessary truths derive their necessity from the Ideas *which they involve; and the existence of necessary truths proves the existence of Ideas not generated by experience.* (I. 5.)

XVI.

In Deductive Reasoning, we cannot have any truth in the conclusion which is not virtually contained in the premises. (I. 6.)

XVII.

In order to acquire any exact and solid knowledge, the student must possess with perfect precision the ideas appropriate to that part of knowledge: and this precision is tested by the student's perceiving *the axiomatic evidence of the* axioms *belonging to each* Fundamental Idea. (I. 6.)

XVIII.

The Fundamental Ideas which it is most important to consider, as being the Bases of the Material Sciences, are the Ideas of Space, Time (*including Number*), Cause (*including Force and Matter*), Outness *of Objects, and* Media *of Perception of Secondary Qualities,* Polarity (*Contrariety*), *Chemical* Composition *and* Affinity, Substance, Likeness *and Natural* Affinity, Means and Ends (*whence the Notion of Organization*), Symmetry, *and the Ideas of* Vital Powers. (I. 8.)

XIX.

The Sciences which depend upon the Ideas of Space and Number are Pure *Sciences, not* Inductive *Sciences: they do not infer special Theories from Facts, but deduce the conditions of all theory from Ideas. The Elementary Pure Sciences, or Elementary Mathematics, are Geometry, Theoretical Arithmetic and Algebra.* (II. 1.)

XX.

The Ideas on which the Pure Sciences depend, are those of Space *and* Number; *but Number is a modification of the conception of Repetition, which belongs to the Idea of* Time. (II. 1.)

XXI.

The Idea of Space *is not derived from experience, for experience of external objects* presupposes *bodies to exist in Space. Space is a condition under which the mind receives the impressions of sense, and therefore the relations of space are necessarily and universally true of all perceived objects. Space is a* form *of our perceptions, and regulates them, whatever the* matter *of them may be.* (II. 2.)

XXII.

Space is not a General Notion collected by abstraction from particular cases; for we do not speak of Spaces *in general, but of universal or absolute* Space. *Absolute Space is infinite. All special spaces are* in *absolute space, and are parts of it.* (II. 3.)

XXIII.

Space is not a real object or thing, distinct from the objects which exist in it; but it is a real condition of the existence of external objects. (II. 3.)

XXIV.

We have an Intuition *of objects in space; that is, we contemplate objects as* made up *of spatial parts, and apprehend their spatial relations by the same act by which we apprehend the objects themselves.* (II. 3.)

XXV.

Form *or Figure is space limited by boundaries. Space has necessarily* three *dimensions, length, breadth, depth; and no others which cannot be resolved into these.* (II. 3.)

XXVI.

The Idea of Space is exhibited for scientific purposes, by the Definitions *and* Axioms *of Geometry; such, for instance, as these:—the* Definition of a Right Angle, *and* of a Circle; —*the* Definition of Parallel Lines, *and the* Axiom *concerning them;—the* Axiom *that* two straight lines cannot inclose a space. *These Definitions are necessary, not arbitrary; and the Axioms are needed as well as the Definitions, in order to express the necessary conditions which the Idea of Space imposes.* (II. 4.)

XXVII.

The Definitions and Axioms of Elementary Geometry do not completely *exhibit the Idea of Space. In proceeding to the Higher Geometry, we may introduce other additional and independent Axioms; such as that of Archimedes, that* a curve line which joins two points is less than any broken line joining the same points and including the curve line. (II. 4.)

XXVIII.

The perception of a solid object *by sight requires that* act *of mind by which, from figure and shade, we infer distance and position in space. The perception of* figure *by sight requires that act of mind by which we give an outline to each object.* (II. 6.)

XXIX.

The perception of Form by touch is not an impression on the passive sense, but requires an act *of our muscular frame by which we become aware of the position of our own limbs. The perceptive faculty involved in this act has been called* the muscular sense. (II. 6.)

XXX.

The Idea of Time *is not derived from experience, for experience of changes* presupposes *occurrences to take place in Time. Time is a condition under which the mind receives the impressions of sense, and therefore the relations of time are necessarily and universally true of all perceived occurrences. Time is a* form *of our perceptions, and regulates them, whatever the* matter *of them may be.* (II. 7.)

XXXI.

Time is not a General Notion collected by abstraction from particular cases. For we do not speak of particular Times *as examples of time in general, but as parts of a single and infinite* Time. (II. 8.)

XXXII.

Time, like Space, is a form, not only of perception, but of Intuition. *We consider the whole of any time as* equal *to the* sum *of the parts; and an occurrence as* coinciding *with the portion of time which it occupies.* (II. 8.)

XXXIII.

Time is analogous to Space of one dimension: *portions of both have a beginning and an end, are long or short. There is nothing in Time which is analogous to Space of two, or of three, dimensions, and thus nothing which corresponds to Figure.* (II. 8.)

XXXIV.

The Repetition of a set of occurrences, as, for example, strong and weak, or long and short sounds, according to a *steadfast order, produces* Rhythm, *which is a conception peculiar to Time, as Figure is to Space.* (II. 8.)

XXXV.

The simplest form of Repetition is that in which there is no variety, and thus gives rise to the conception of Number. (II. 8.)

XXXVI.

The simplest numerical truths are seen by Intuition; when we endeavour to deduce the more complex from these simplest, we employ such maxims as these:—If equals be added to equals the wholes are equal:—If equals be subtracted from equals the remainders are equal:—The whole is equal to the sum of all its parts. (II. 9.)

XXXVII.

The Perception of Time involves a constant and latent kind of memory, which may be termed a Sense of Succession. *The Perception of Number also involves this Sense of Succession, although in small numbers we appear to apprehend the units simultaneously and not successively.* (II. 10.)

XXXVIII.

The Perception of Rhythm is not an impression on the passive sense, but requires an act *of thought by which we connect and group the strokes which form the Rhythm.* (II. 10.)

XXXIX.

Intuitive *is opposed to* Discursive *reason. In intuition, we obtain our conclusions by dwelling upon* one *aspect of the fundamental Idea; in discursive reasoning, we combine* several *aspects of the Idea, (that is, several axioms,) and reason from the combination.* (II. 11.)

XL.

Geometrical deduction (and deduction in general) is called Synthesis, *because we introduce, at successive steps, the* *results of new principles. But in reasoning on the relations of space, we sometimes go on* separating *truths into their component truths, and these into other component truths; and so on: and this is geometrical* Analysis. (II. 11.)

XLI.

Among the foundations of the Higher Mathematics, is the Idea of Symbols *considered as general* Signs *of Quantity. This idea of a Sign is distinct from, and independent of other ideas. The Axiom to which we refer in reasoning by means of Symbols of quantity is this:*—The interpretation of such symbols must be perfectly general. *This Idea of Axiom are the bases of Algebra in its most general form.* (II. 12.)

XLII.

Among the foundations of the Higher Mathematics is also the Idea of a Limit. *The Idea of a Limit cannot be superseded by any other definitions or Hypotheses. The Axiom which we employ in introducing this Idea into our reasoning is this:*—What is true up to the Limit is true at the Limit. *This Idea and Axiom are the bases of all Methods of Limits, Fluxions, Differentials, Variations, and the like.* (II. 12.)

XLIII.

There is a pure *Science of Motion, which does not depend upon observed facts, but upon the Idea of motion. It may also be termed* Pure Mechanism, *in opposition to Mechanics Proper, or* Machinery, *which involves the mechanical conceptions of force and matter. It has been proposed to name this Pure Science of Motion,* Kinematics. (II. 13.)

XLIV.

The pure Mathematical Sciences must be successfully cultivated, in order that the progress of the principal Inductive Sciences may take place. This appears in the case of Astronomy, in which Science, both in ancient and in modern times, each advance of the theory has depended upon the pre-

vious solution of problems in pure mathematics. It appears also inversely in the Science of the Tides, in which, at present, we cannot advance in the theory, because we cannot solve the requisite problems in the Integral Calculus. (II. 14.)

XLV.

The Idea of Cause, *modified into the conceptions of mechanical cause, or Force, and resistance to force, or Matter, is the foundation of the Mechanical Sciences; that is, Mechanics, (including Statics and Dynamics,) Hydrostatics, and Physical Astronomy.* (III. 1.)

XLVI.

The Idea of Cause is not derived from experience; for in judging of occurrences which we contemplate, we consider them as being, universally and necessarily, Causes and Effects, which a finite experience could not authorize us to do. The Axiom, that every event must have a cause, is true independently of experience, and beyond the limits of experience. (III. 2.)

XLVII.

The Idea of Cause is expressed for purposes of science by these three Axioms:—Every Event must have a Cause:—Causes are measured by their Effects:—Reaction is equal and opposite to Action. (III. 4.)

XLVIII.

The Conception of Force involves the Idea of Cause, as applied to the motion and rest of bodies. The conception of force *is suggested by muscular action exerted: the conception of* matter *arises from muscular action resisted. We necessarily ascribe to all bodies solidity and inertia, since we conceive Matter as that which cannot be compressed or moved without resistance.* (III. 5.)

XLIX.

Mechanical Science depends on the Conception of Force; and is divided into Statics, *the doctrine of Force preventing* *motion, and* Dynamics, *the doctrine of Force producing motion.* (III. 6.)

L.

The Science of Statics depends upon the Axiom, that Action and Reaction are equal, which in Statics assumes this form:—When two equal weights are supported on the middle point between them, the pressure on the fulcrum is equal to the sum of the weights. (III. 6.)

LI.

The Science of Hydrostatics depends upon the Fundamental Principle that fluids press equally in all directions. *This principle necessarily results from the conception of a Fluid, as a body of which the parts are perfectly moveable in all directions. For since the Fluid is a body, it can transmit pressure; and the transmitted pressure is equal to the original pressure, in virtue of the Axiom that Reaction is equal to Action. That the Fundamental Principle is not derived from experience, is plain both from its evidence and from its history.* (III. 6.)

LII.

The Science of Dynamics depends upon the three Axioms above stated respecting Cause. The First Axiom,—that every change must have a Cause,—gives rise to the First Law of Motion,—that a body not acted upon by a force will move with a uniform velocity in a straight line. *The Second Axiom,—that Causes are measured by their Effects,—gives rise to the Second Law of Motion,—that* when a force acts upon a body in motion, the effect of the force is compounded with the previously existing motion. *The Third Axiom,—that* Reaction is equal and opposite to Action,—*gives rise to the Third Law of Motion, which is expressed in the same terms as the Axiom; Action and Reaction being understood to signify momentum gained and lost.* (III. 7.)

LIII.

The above Laws of Motion, historically speaking, were established by means of experiment: but since they have been discovered and reduced to their simplest form, they have been considered by many philosophers as self-evident. This result is principally due to the introduction and establishment of terms and definitions, which enable us to express the Laws in a very simple manner. (III. 7.)

LIV.

In the establishment of the Laws of Motion, it happened, in several instances, that Principles were assumed as self-evident which do not now appear evident, but which have since been demonstrated from the simplest and most evident principles. Thus it was assumed that a perpetual motion is impossible;—*that* the velocities of bodies acquired by falling down planes or curves of the same vertical height are equal;—*that* the actual descent of the center of gravity is equal to its potential ascent. *But we are not hence to suppose that these assumptions were made without ground: for since they really follow from the laws of motion, they were probably, in the minds of the discoverers, the results of undeveloped demonstrations which their sagacity led them to divine.* (III. 7.)

LV.

It is a Paradox *that Experience should lead us to truths confessedly universal, and apparently necessary, such as the Laws of Motion are. The* Solution *of this paradox is, that these laws are interpretations of the Axioms of Causation. The axioms are universally and necessarily true, but the right interpretation of the terms which they involve, is learnt by experience. Our Idea of Cause supplies the* Form, *Experience, the* Matter, *of these Laws.* (III. 8.)

LVI.

Primary *Qualities of Bodies are those which we can conceive as directly perceived;* Secondary *Qualities are those* *which we conceive as perceived by means of a Medium.* (IV. 1.)

LVII.

We necessarily perceive bodies as without *us; the* Idea *of* Externality *is one of the conditions of perception.* (IV. 1.)

LVIII.

We necessarily assume a Medium *for the perceptions of Light, Colour, Sound, Heat, Odours, Tastes; and this Medium* must *convey impressions by means of its mechanical attributes.* (IV. 1.)

LIX.

Secondary Qualities are not extended *but* intensive: *their effects are not augmented by addition of parts, but by increased operation of the medium. Hence they are not measured directly, but by* scales; *not by* units, *but by* degrees. (IV. 4.)

LX.

In the Scales of Secondary Qualities, it is a condition (in order that the scale may be complete,) that every example of the quality must either agree *with one of the degrees of the Scale, or lie between two* contiguous *degrees.* (IV. 4.)

LXI.

We perceive by means of *a medium and* by means of *impressions on the nerves: but we do not (by our senses) perceive either the medium or the impressions on the nerves.* (IV. 1.)

LXII.

The Prerogatives of the Sight *are, that by this sense we necessarily and immediately apprehend the* position *of its objects: and that from visible circumstances, we* infer *the* distance *of objects from us, so readily that we seem to perceive and not to infer.* (IV. 2.)

LXIII.

The Prerogatives of the Hearing *are, that by this sense we perceive relations perfectly precise and definite between two notes, namely,* Musical Intervals (*as an* Octave, *a* Fifth); *and that when two notes are perceived together, they are apprehended as distinct,* (*a* Chord,) *and as having a certain relation,* (Concord *or* Discord.) (IV. 2.)

LXIV.

The Sight cannot decompose a compound colour into simple colours, or distinguish a compound from a simple colour. The Hearing cannot directly perceive the place, still less the distance, of its objects: we infer these obscurely and vaguely from audible circumstances. (IV. 2.)

LXV.

The First Paradox of Vision *is, that we see objects* upright, *though the images on the retina are* inverted. *The solution is, that we do not see the image on the retina at all, we only see by means of it.* (IV. 2.)

LXVI.

The Second Paradox of Vision *is, that we see objects* single, *though there are two images on the retinas, one in each eye. The explanation is, that it is a Law of Vision that we see (small or distant) objects single, when their images fall on* corresponding points *of the two retinas.* (IV. 2.)

LXVII.

The law of single vision for near *objects is this:—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 and solid if the two objects are such as would be produced by a single solid object seen by the eyes separately.* (IV. 2.)

LXVIII.

The ultimate object of each of the Secondary Mechanical Sciences is, to determine the nature and laws of the processes *by which the impression of the Secondary Quality treated of is conveyed: but before we discover the* cause, *it may be necessary to determine the* laws *of the phenomena; and for this purpose a* Measure *or* Scale *of each quality is necessary.* (IV. 4.)

LXIX.

Secondary qualities are measured by means of such effects as can be estimated in number or space. (IV. 4.)

LXX.

The Measure of Sounds, as high or low, is the Musical Scale, *or* Harmonic Canon. (IV. 4.)

LXXI.

The Measures of Pure Colours are the Prismatic Scale; *the same, including* Fraunhofer's Lines; *and* Newton's Scale *of Colours. The principal Scales of Impure Colours are* Werner's Nomenclature *of Colours, and* Merimée's Nomenclature *of Colours.* (IV. 4.)

LXXII.

The Idea of Polarity *involves the conception of contrary properties in contrary directions:—the properties being, for example, attraction and repulsion, darkness and light, synthesis and analysis; and the contrary directions being those which are directly opposite, or, in some cases, those which are at right angles.* (V. 1.)

LXXIII. (Doubtful.)

Coexistent polarities are fundamentally identical. (V. 2.)

LXXIV.

The Idea of Chemical Affinity, *as implied in Elementary Composition, involves peculiar conceptions. It is not properly expressed by assuming the qualities of bodies to* resemble *those of the elements, or to depend on the* figure *of the elements, or on their* attractions. (VI. 1.)

LXXV.

Attractions take place between bodies, Affinities between the particles of a body. The former may be compared to the alliances of states, the latter to the ties of family. (VI. 2.)

LXXVI.

The governing principles of Chemical Affinity are, that it is elective; *that it is* definite; *that it* determines the properties *of the compound; and that* analysis is possible. (VI. 2.)

LXXVII.

We have an idea of Substance: *and an axiom involved in this Idea is, that* the weight of a body is the sum of the weights of all its elements. (VI. 3.)

LXXVIII.

Hence Imponderable Fluids are not to be admitted as chemical elements. (VI. 4.)

LXXIX.

The Doctrine of Atoms is admissible as a mode of expressing and calculating laws of nature; but is not proved by any fact, chemical or physical, as a philosophical truth. (VI. 5.)

LXXX.

We have an Idea of Symmetry; *and an axiom involved in this Idea is, that in a symmetrical natural body, if there be a tendency to modify any member in any manner, there is a tendency to modify all the corresponding members in the same manner.* (VII. 1.)

LXXXI.

All hypotheses respecting the manner in which the elements of inorganic bodies are arranged in space, must be constructed with regard to the general facts of crystallization. (VII. 3.)

LXXXII.

When we consider any object as One, *we give unity to it by an act of thought. The condition which determines what this unity shall include, and what it shall exclude, is this;—that assertions concerning the one thing shall be possible.* (VIII. 1.)

LXXXIII.

We collect individuals into Kinds *by applying to them the Idea of Likeness. Kinds of things are not determined by definitions, but by this condition:—that general assertions concerning such kinds of things shall be possible.* (VIII. 1.)

LXXXIV.

The Names *of kinds of things are governed by their use; and that may be a right name in one use which is not so in another. A whale is not a* fish *in natural history, but it is a* fish *in commerce and law.* (VIII. 1.)

LXXXV.

We take for granted that each kind of things has a special character *which may be expressed by a Definition. The ground of our assumption is this;—that reasoning must be possible.* (VIII. 1.)

LXXXVI.

The "Five Words," Genus, Species, Difference, Property, Accident, *were used by the Aristotelians, in order to express the subordination of Kinds, and to describe the nature of Definitions and Propositions. In modern times, these technical expressions have been more referred to by Natural Historians than by Metaphysicians.* (VIII. 1.)

LXXXVII.

The construction of a Classificatory Science includes Terminology, *the formation of a descriptive language;—* Diataxis, *the Plan of the System of Classification, called* *also the* Systematick;—Diagnosis, *the Scheme of the Characters by which the different Classes are known, called also the* Characteristick. Physiography *is the knowledge which the System is employed to convey. Diataxis includes* Nomenclature. (VIII. 2.)

LXXXVIII.

Terminology *must be conventional, precise, constant; copious in words, and minute in distinctions, according to the needs of the science. The student must understand the terms,* directly *according to the convention, not through the medium of explanation or comparison.* (VIII. 2.)

LXXXIX.

The Diataxis, *or Plan of the System, may aim at a Natural or at an Artificial System. But no classes can be absolutely artificial, for if they were, no assertions could be made concerning them.* (VIII. 2.)

XC.

An Artificial System *is one in which the* smaller *groups (the Genera) are* natural; *and in which the* wider *divisions (Classes, Orders) are constructed by the* peremptory *application of selected Characters; (selected, however, so as not to break up the smaller groups.)* (VIII. 2.)

XCI.

A Natural System *is one which attempts to make* all *the divisions* natural, *the widest as well as the narrowest; and therefore applies* no *characters* peremptorily. (VIII. 2.)

XCII.

Natural Groups are best described, not by any Definition which marks their boundaries, but by a Type *which marks their center. The Type of any natural group is an example which possesses in a marked degree all the leading characters of the class.* (VIII. 2.)

XCIII.

A Natural Group is steadily fixed, though not precisely limited; it is given in position, though not circumscribed; it is determined, not by a boundary without, but by a central point within;—not by what it strictly excludes, but by what it eminently includes;—by a Type, not by a Definition. (VIII. 2.)

XCIV.

The prevalence of Mathematics as an element of education has made us think Definition the philosophical mode of fixing the meaning of a word: if (Scientific) Natural History were introduced into education, men might become familiar with the fixation of the signification of words by Types; *and this process agrees more nearly with the common processes by which words acquire their significations.* (VIII. 2.)

XCV.

The attempts at Natural Classification are of three sorts; according as they are made by the process of blind trial, *of* general comparison, *or of* subordination of characters. *The process of Blind Trial professes to make its classes by attention to* all *the characters, but without proceeding methodically. The process of General Comparison professes to enumerate all the characters, and forms its classes by the* majority. *Neither of these methods can really be carried into effect. The method of Subordination of Characters considers some characters as* more important *than others; and this method gives more consistent results than the others. This method, however, does not depend upon the Idea of Likeness only, but introduces the Idea of Organization or Function.* (VIII. 2.)

XCVI.

A Species *is a collection of individuals which are descended from a common stock, or which resemble such a collection as much as these resemble each other: the resemblance being opposed to a* definite *difference.* (VIII. 2.)

XCVII.

A Genus *is a collection of species which resemble each other more than they resemble other species: the resemblance being opposed to a* definite *difference.* (VIII. 2.)

XCVIII.

The Nomenclature *of a Classificatory Science is the collection of the names of the Species, Genera, and other divisions. The* binary *nomenclature, which denotes a species by the* generic *and* specific *name, is now commonly adopted in Natural History.* (VIII. 2.)

XCIX.

The Diagnosis, *or Scheme of the Characters, comes, in the order of philosophy, after the Classification. The characters do not* make *the classes, they only enable us to* recognize *them. The Diagnosis is an Artificial Key to a Natural System.* (VIII. 2.)

C.

The basis of all Natural Systems of Classification is the Idea of Natural Affinity. The Principle which this Idea involves is this:—Natural arrangements, obtained from different *sets of characters, must* coincide *with each other.* (VIII. 4.)

CI.

In order to obtain a Science of Biology, we must analyse the Idea of Life. It has been proved by the biological speculations of past time, that Organic Life cannot rightly be solved into Mechanical or Chemical Forces, or the operation of a Vital Fluid, or of a Soul. (IX. 2.)

CII.

Life is a System of Vital Forces; and the conception of such Forces involves a peculiar Fundamental Idea. (IX. 3.)

CIII.

Mechanical, chemical, and vital Forces form an ascending progression, each including the preceding. Chemical Affinity includes in its nature Mechanical Force, and may often be practically resolved into Mechanical Force. (Thus the ingredients of gunpowder, liberated from their chemical union, exert great mechanical Force: a galvanic battery acting by chemical process does the like.) Vital Forces include in their nature both chemical Affinities and mechanical Forces: for Vital Powers produce both chemical changes, (as digestion,) and motions which imply considerable mechanical force, (as the motion of the sap and of the blood.) (IX. 4.)

CIV.

In voluntary *motions, Sensations produce Actions, and the connexion is made by means of Ideas: in* reflected *motions, the connexion neither seems to be nor is made by means of Ideas: in* instinctive *motions, the connexion is such as requires Ideas, but we cannot believe the Ideas to exist.* (IX. 5.)

CV.

The Assumption of a Final Cause in the structure of each part of animals and plants is as inevitable as the assumption of an Efficient Cause for every event. The maxim that in organized bodies nothing is in vain, *is as necessarily true as the maxim that nothing happens* by chance. (IX. 6.)

CVI.

The Idea of living beings as subject to disease *includes a recognition of a Final Cause in organization; for disease is a state in which the vital forces do not attain their* proper ends. (IX. 7.)

CVII.

The Palætiological Sciences depend upon the Idea of Cause; but the leading conception which they involve is that of historical cause, *not mechanical cause.* (X. 1.)

CVIII.

Each Palætiological Science, when complete, must possess three members: the Phenomenology, *the* Ætiology, *and the* Theory. (x. 2.)

CIX.

There are, in the Palætiological Sciences, two antagonist doctrines: Catastrophes *and* Uniformity. *The doctrine of a* uniform course of nature *is tenable only when we extend the notion of Uniformity so far that it shall include Catastrophes.* (x. 3.)

CX.

The Catastrophist constructs Theories, the Uniformitarian demolishes them. The former adduces evidence of an Origin, the latter explains the evidence away. The Catastrophist's dogmatism is undermined by the Uniformitarian's skeptical hypotheses. But when these hypotheses are asserted dogmatically, they cease to be consistent with the doctrine of Uniformity. (x. 3.)

CXI.

In each of the Palætiological Sciences, we can ascend to remote periods by a chain of causes, but in none can we ascend to a beginning *of the chain.* (x. 3.)

CXII.

Since the Palætiological sciences deal with the conceptions of historical cause, History, *including* Tradition, *is an important source of materials for such sciences.* (x. 4.)

CXIII.

The history and tradition which present to us the providential course of the world form a Sacred Narrative; *and in reconciling the Sacred Narrative with the results of science, arise inevitable difficulties which disturb the minds of those who reverence the Sacred Narrative.* (x. 4.)

CXIV.

The disturbance of reverent minds, arising from scientific views, ceases when such views become familiar, the Sacred Narrative being then interpreted anew in accordance with such views. (x. 4.)

CXV.

A new interpretation of the Sacred Narrative, made for the purpose of reconciling it with doctrines of science, should not be insisted on till such doctrines are clearly proved; and when they are so proved, should be frankly accepted, in the confidence that a reverence for the Sacred Narrative is consistent with a reverence for the Truth. (x. 4.)

CXVI.

In contemplating the series of causes and effects which constitutes the world, we necessarily assume a First Cause *of the whole series.* (x. 5.)

CXVII.

The Palætiological Sciences point backwards with lines which are broken, but which all converge to the same *invisible point: and this point is the Origin of the Moral and Spiritual, as well as of the Natural World.* (x. 5.)

NOVUM ORGANON RENOVATUM.

BOOK II.

OF THE CONSTRUCTION OF SCIENCE.

CHAPTER I.

OF TWO PRINCIPAL PROCESSES BY WHICH SCIENCE IS CONSTRUCTED.

APHORISM I.

THE two processes by which Science is constructed are the Explication of Conceptions, *and the* Colligation of Facts.

TO the subject of the present and next Book all that has preceded is subordinate and preparatory. In former works we have treated of the History of Scientific Discoveries and of the History of Scientific Ideas. We have now to attempt to describe the manner in which discoveries are made, and in which Ideas give rise to knowledge. It has already been stated that Knowledge requires us to possess both Facts and Ideas;—that every step in our knowledge consists in applying the Ideas and Conceptions furnished by our minds to the Facts which observation and experiment offer to us. When our Conceptions are clear and distinct, when our Facts are certain and sufficiently numerous, and when the Conceptions, being suited to the nature of the

Facts, are applied to them so as to produce an exact and universal accordance, we attain knowledge of a precise and comprehensive kind, which we may term *Science*. And we apply this term to our knowledge still more decidedly when, Facts being thus included in exact and general Propositions, such Propositions are, in the same manner, included with equal rigour in Propositions of a higher degree of Generality; and these again in others of a still wider nature, so as to form a large and systematic whole.

But after thus stating, in a general way, the nature of science, and the elements of which it consists, we have been examining with a more close and extensive scrutiny, some of those elements; and we must now return to our main subject, and apply to it the results of our long investigation. We have been exploring the realm of Ideas; we have been passing in review the difficulties in which the workings of our own minds involve us when we would make our conceptions consistent with themselves: and we have endeavoured to get a sight of the true solutions of these difficulties. We have now to inquire how the results of these long and laborious efforts of thought find their due place in the formation of our Knowledge. What do we gain by these attempts to make our notions distinct and consistent; and in what manner is the gain of which we thus become possessed, carried to the general treasure-house of our permanent and indestructible knowledge? After all this battling in the world of ideas, all this struggling with the shadowy and changing forms of intellectual perplexity, how do we secure to ourselves the fruits of our warfare, and assure ourselves that we have really pushed forwards the frontier of the empire of Science? It is by such an appropriation, that the task which we have had in our hands during the two previous works, (the *History of the Inductive Sciences* and the *History of Scientific Ideas*,) must acquire its real value and true place in our design.

In order to do this, we must reconsider, in a more definite and precise shape, the doctrine which has already been laid down;—that our Knowledge consists in applying Ideas to Facts; and that the conditions of real knowledge are that the ideas be distinct and appropriate, and exactly applied to clear and certain facts. The steps by which our knowledge is advanced are those by which one or the other of these two processes is rendered more complete;—by which *Conceptions* are *made more clear* in themselves, or by which the Conceptions more strictly *bind together the Facts.* These two processes may be considered as together constituting the whole formation of our knowledge; and the principles which have been established in the History of Scientific Ideas bear principally upon the former of these two operations;—upon the business of elevating our conceptions to the highest possible point of precision and generality. But these two portions of the progress of knowledge are so clearly connected with each other, that we shall deal with them in immediate succession. And having now to consider these operations in a more exact and formal manner than it was before possible to do, we shall designate them by certain constant and technical phrases. We shall speak of the two processes by which we arrive at science, as *the Explication of Conceptions* and *the Colligation of Facts:* we shall show how the discussions in which we have been engaged have been necessary in order to promote the former of these offices; and we shall endeavour to point out modes, maxims, and principles by which the second of the two tasks may also be furthered.

CHAPTER II.

OF THE EXPLICATION OF CONCEPTIONS.

APHORISM II.

The Explication of Conceptions, as requisite for the progress of scientce, has been effected by means of discussions and controversies among scientists; often by debates concerning definitions; these controversies have frequently led to the establishment of a Definition; but along with the Definition, a corresponding Proposition has always been expressed or implied. The essential requisite for the advance of science is the clearness of the Conception, not the establishment of a Definition. The construction of an exact Definition is often very difficult. The requisite conditions of clear Conceptions may often be expressed by Axioms as well as by Definitions.

APHORISM III.

Conceptions, for purposes of science, must be appropriate *as well as clear: that is, they must be modifications of* that *Fundamental Idea, by which the phenomena can really be interpreted. This maxim may warn us from errour, though it may not lead to discovery. Discovery depends upon the previous cultivation or natural clearness of the appropriate Idea, and therefore* no discovery is the work of accident.

SECT. I.—*Historical Progress of the Explication of Conceptions.*

1. WE have given the appellation of *Ideas* to certain comprehensive forms of thought,—as *space, number, cause, composition, resemblance,*—which we apply to the phenomena which we contemplate. But the special modifications of these ideas which are

exemplified in particular facts, we have termed *Conceptions;* as *a circle*, *a square number*, *an accelerating force*, *a neutral combination* of elements, a *genus*. Such Conceptions involve in themselves certain necessary and universal relations derived from the Ideas just enumerated; and these relations are an indispensable portion of the texture of our knowledge. But to determine the contents and limits of this portion of our knowledge, requires an examination of the Ideas and Conceptions from which it proceeds. The Conceptions must be, as it were, carefully *unfolded*, so as to bring into clear view the elements of truth with which they are marked from their ideal origin. This is one of the processes by which our knowledge is extended and made more exact; and this I shall describe as the *Explication of Conceptions*.

In the several Books of the History of Ideas we have discussed a great many of the Fundamental Ideas of the most important existing sciences. We have, in those Books, abundant exemplifications of the process now under our consideration. We shall here add a few general remarks, suggested by the survey which we have thus made.

2. Such discussions as those in which we have been engaged concerning our fundamental Ideas, have been the course by which, historically speaking, those Conceptions which the existing sciences involve have been rendered so clear as to be fit elements of exact knowledge. Thus, the disputes concerning the various kinds and measures of *Force* were an important part of the progress of the science of Mechanics. The struggles by which philosophers attained a right general conception of *plane*, of *circular*, of *elliptical Polarization*, were some of the most difficult steps in the modern discoveries of Optics. A Conception of the *Atomic Constitution* of bodies, such as shall include what we know, and assume nothing more, is even now a matter of conflict among Chemists. The debates by which, in recent times, the Conceptions of *Species* and *Genera* have been rendered more exact, have improved the science of Botany: the imperfection of the science of

Mineralogy arises in a great measure from the circumstance, that in that subject, the Conception of a *Species* is not yet fixed. In Physiology, what a vast advance would that philosopher make, who should establish a precise, tenable, and consistent Conception of *Life!*

Thus discussions and speculations concerning the import of very abstract and general terms and notions, may be, and in reality have been, far from useless and barren. Such discussions arose from the desire of men to impress their opinions on others, but they had the effect of making the opinions much more clear and distinct. In trying to make others understand them, they learnt to understand themselves. Their speculations were begun in twilight, and ended in the full brilliance of day. It was not easily and at once, without expenditure of labour or time, that men arrived at those notions which now form the elements of our knowledge; on the contrary, we have, in the history of science, seen how hard, discoverers, and the forerunners of discoverers, have had to struggle with the indistinctness and obscurity of the intellect, before they could advance to the critical point at which truth became clearly visible. And so long as, in this advance, some speculators were more forward than others, there was a natural and inevitable ground of difference of opinion, of argumentation, of wrangling. But the tendency of all such controversy is to diffuse truth and to dispel errour. Truth is consistent, and can bear the tug of war; Errour is incoherent, and falls to pieces in the struggle. True Conceptions can endure the sun, and become clearer as a fuller light is obtained; confused and inconsistent notions vanish like visionary spectres at the break of a brighter day. And thus all the controversies concerning such Conceptions as science involves, have ever ended in the establishment of the side on which the truth was found.

3. Indeed, so complete has been the victory of truth in most of these instances, that at present we can hardly imagine the struggle to have been necessary. The very essence of these triumphs is that they lead us to regard the views we reject as not only false,

but inconceivable. And hence we are led rather to look back upon the vanquished with contempt than upon the victors with gratitude. We now despise those who, in the Copernican controversy, could not conceive the apparent motion of the sun on the heliocentric hypothesis;—or those who, in opposition to Galileo, thought that a uniform force might be that which generated a velocity proportional to the space;—or those who held there was something absurd in Newton's doctrine of the different refrangibility of differently coloured rays;—or those who imagined that when elements combine, their sensible qualities must be manifest in the compound;—or those who were reluctant to give up the distinction of vegetables into herbs, shrubs, and trees. We cannot help thinking that men must have been singularly dull of comprehension, to find a difficulty in admitting what is to us so plain and simple. We have a latent persuasion that we in their place should have been wiser and more clear-sighted;—that we should have taken the right side, and given our assent at once to the truth.

4. Yet in reality, such a persuasion is a mere delusion. The persons who, in such instances as the above, were on the losing side, were very far, in most cases, from being persons more prejudiced, or stupid, or narrow-minded, than the greater part of mankind now are; and the cause for which they fought was far from being a manifestly bad one, till it had been so decided by the result of the war. It is the peculiar character of scientific contests, that what is only an epigram with regard to other warfare is a truth in this;—They who are defeated are really in the wrong. But they may, nevertheless, be men of great subtilty, sagacity, and genius; and we nourish a very foolish self-complacency when we suppose that we are their superiors. That this is so, is proved by recollecting that many of those who have made very great discoveries have laboured under the imperfection of thought which was the obstacle to the next step in knowledge. Though Kepler detected with great acuteness the Numerical Laws of the solar system, he laboured in

vain to conceive the very simplest of the Laws of Motion by which the paths of the planets are governed. Though Priestley made some important steps in chemistry, he could not bring his mind to admit the doctrine of a general Principle of Oxidation. How many ingenious men in the last century rejected the Newtonian Attraction as an impossible chimera! How many more, equally intelligent, have, in the same manner, in our own time, rejected, I do not now mean as false, but as inconceivable, the doctrine of Luminiferous Undulations! To err in this way is the lot, not only of men in general, but of men of great endowments, and very sincere love of truth.

5. And those who liberate themselves from such perplexities, and who thus go on in advance of their age in such matters, owe their superiority in no small degree to such discussions and controversies as those to which we now refer. In such controversies, the Conceptions in question are turned in all directions, examined on all sides; the strength and the weakness of the maxims which men apply to them are fully tested; the light of the brightest minds is diffused to other minds. Inconsistency is unfolded into self-contradiction; axioms are built up into a system of necessary truths; and ready exemplifications are accumulated of that which is to be proved or disproved, concerning the ideas which are the basis of the controversy.

The History of Mechanics from the time of Kepler to that of Lagrange, is perhaps the best exemplification of the mode in which the progress of a science depends upon such disputes and speculations as give clearness and generality to its elementary conceptions. This, it is to be recollected, is the kind of progress of which we are now speaking; and this is the principal feature in the portion of scientific history which we have mentioned. For almost all that was to be done by reference to observation, was executed by Galileo and his disciples. What remained was the task of generalization and simplification. And this was promoted in no small degree by the various controversies which took place within that period concerning mechanical conceptions:—as, for example, the question concerning the measure of the Force of Percussion;—the war of the *Vis Viva*;—the controversy of the Center of Oscillation;—of the independence of Statics and Dynamics;—of the principle of Least Action;—of the evidence of the Laws of Motion;—and of the number of Laws really distinct. None of these discussions was without its influence in giving generality and clearness to the mechanical ideas of mathematicians: and therefore, though remote from general apprehension, and dealing with very abstract notions, they were of eminent use in the perfecting the science of Mechanics. Similar controversies concerning fundamental notions, those, for example, which Galileo himself had to maintain, were no less useful in the formation of the science of Hydrostatics. And the like struggles and conflicts, whether they take the form of controversies between several persons, or only operate in the efforts and fluctuations of the discoverer's mind, are always requisite, before the conceptions acquire that clearness which makes them fit to appear in the enunciation of scientific truth. This, then, was one object of the History of Ideas;—to bring under the reader's notice the main elements of the controversies which have thus had so important a share in the formation of the existing body of science, and the decisions on the controverted points to which the mature examination of the subject has led; and thus to give an abundant exhibition of that step which we term the Explication of Conceptions.

SECT. II.—*Use of Definitions.*

6. The result of such controversies as we have been speaking of, often appears to be summed up in a *Definition;* and the controversy itself has often assumed the form of a battle of definitions. For example, the inquiry concerning the Laws of Falling Bodies led to the question whether the proper Definition of a *uniform force* is, that it generates a velocity proportional to the *space* from rest, or to the *time.* The controversy of the *Vis Viva* was, what was the

proper Definition of the *measure of force*. A principal question in the classification of minerals is, what is the Definition of a *mineral species*. Physiologists have endeavoured to throw light on their subject, by Defining *organization*, or some similar term.

7. It is very important for us to observe, that these controversies have never been questions of insulated and *arbitrary* Definitions, as men seem often tempted to suppose them to have been. In all cases there is a tacit assumption of some Proposition which is to be expressed by means of the Definition, and which gives it its importance. The dispute concerning the Definition thus acquires a real value, and becomes a question concerning true and false. Thus in the discussion of the question, What is a Uniform Force? it was taken for granted that 'gravity is a uniform force:'—in the debate of the *Vis Viva*, it was assumed that 'in the mutual action of bodies the whole effect of the force is unchanged:'—in the zoological definition of Species, (that it consists of individuals which have, or may have, sprung from the same parents,) it is presumed that 'individuals so related resemble each other more than those which are excluded by such a definition;' or perhaps, that 'species so defined have permanent and definite differences.' A definition of Organization, or of any other term, which was not employed to express some principle, would be of no value.

The establishment, therefore, of a right Definition of a Term may be a useful step in the Explication of our Conceptions; but this will be the case *then* only when we have under our consideration some Proposition in which the Term is employed. For then the question really is, how the Conception shall be understood and defined in order that the Proposition may be true.

8. The establishment of a Proposition requires an attention to observed Facts, and can never be rightly derived from our Conceptions alone. We must hereafter consider the necessity which exists that the Facts should be rightly bound together, as well as that our Conceptions should be clearly employed, in order to

lead us to real knowledge. But we may observe here that, in such cases at least as we are now considering, the two processes are co-ordinate. To unfold our Conceptions by the means of Definitions, has never been serviceable to science, except when it has been associated with an immediate *use* of the Definitions. The endeavour to define a Uniform Force was combined with the assertion that 'gravity is a uniform force:' the attempt to define Accelerating Force was immediately followed by the doctrine that 'accelerating forces may be compounded:' the process of defining Momentum was connected with the principle that 'momenta gained and lost are equal:' naturalists would have given in vain the Definition of Species which we have quoted, if they had not also given the 'characters' of species so separated. Definition and Proposition are the two handles of the instrument by which we apprehend truth; the former is of no use without the latter. Definition may be the best mode of explaining our Conception, but that which alone makes it worth while to explain it in any mode, is the opportunity of using it in the expression of Truth. When a Definition is propounded to us as a useful step in knowledge, we are always entitled to ask what Principle it serves to enunciate. If there be no answer to this inquiry, we define and give clearness to our conceptions in vain. While we labour at such a task, we do but light up a vacant room;—we sharpen a knife with which we have nothing to cut;—we take exact aim, while we load our artillery with blank cartridge;—we apply strict rules of grammar to sentences which have no meaning.

If, on the other hand, we have under our consideration a proposition probably established, every step which we can make in giving distinctness and exactness to the Terms which this proposition involves, is an important step towards scientific truth. In such cases, any improvement in our Definition is a real advance in the explication of our Conception. The clearness of our Expressions casts a light upon the Ideas which we contemplate and convey to others.

9. But though *Definition* may be subservient to a right explication of our conceptions, it is *not essential* to that process. It is absolutely necessary to every advance in our knowledge, that those by whom such advances are made should possess clearly the conceptions which they employ: but it is by no means necessary that they should unfold these conceptions in the words of a formal Definition. It is easily seen, by examining the course of Galileo's discoveries, that he had a distinct conception of the *Moving Force* which urges bodies downwards upon an inclined plane, while he still hesitated whether to call it *Momentum*, *Energy*, *Impetus*, or *Force*, and did not venture to offer a Definition of the thing which was the subject of his thoughts. The Conception of *Polarization* was clear in the minds of many optical speculators, from the time of Huyghens and Newton to that of Young and Fresnel. This Conception we have defined to be 'Opposite properties depending upon opposite positions;' but this notion was, by the discoverers, though constantly assumed and expressed by means of superfluous hypotheses, never clothed in definite language. And in the mean time, it was the custom, among subordinate writers on the same subjects, to say, that the term *Polarization* had no definite meaning, and was merely an expression of our ignorance. The Definition which was offered by Haüy and others of a *Mineralogical Species*;—'The same elements combined in the same proportions, with the same fundamental form;'—was false, inasmuch as it was incapable of being rigorously applied to any one case; but this defect did not prevent the philosophers who propounded such a Definition from making many valuable additions to mineralogical knowledge, in the way of identifying some species and distinguishing others. The right Conception which they possessed in their minds prevented their being misled by their own very erroneous Definition. The want of any precise Definitions of *Strata*, and *Formations*, and *Epochs*, among geologists, has not prevented the discussions which they have carried on upon such subjects from being highly serviceable

in the promotion of geological knowledge. For however much the apparent vagueness of these terms might leave their arguments open to cavil, there was a general understanding prevalent among the most intelligent cultivators of the science, as to what was meant in such expressions; and this common understanding sufficed to determine what evidence should be considered conclusive and what inconclusive, in these inquiries. And thus the distinctness of Conception, which is a real requisite of scientific progress, existed in the minds of the inquirers, although Definitions, which are a partial and accidental evidence of this distinctness, had not yet been hit upon. The Idea had been developed in men's minds, although a clothing of words had not been contrived for it, nor, perhaps, the necessity of such a vehicle felt: and thus that essential condition of the progress of knowledge, of which we are here speaking, existed; while it was left to the succeeding speculators to put this unwritten Rule in the form of a verbal Statute.

10. Men are often prone to consider it as a thoughtless *omission* of an essential circumstance, and as a *neglect* which involves some blame, when knowledge thus assumes a form in which Definitions, or rather Conceptions, are implied but are not expressed. But in such a judgment, they assume *that* to be a matter of choice requiring attention only, which is in fact as difficult and precarious as any other portion of the task of discovery. To *define*, so that our Definition shall have any scientific value, requires no small portion of that sagacity by which truth is detected. As we have already said, Definitions and Propositions are co-ordinate in their use and in their origin. In many cases, perhaps in most, the Proposition which contains a scientific truth, is apprehended with confidence, but with some vagueness and vacillation, before it is put in a positive, distinct, and definite form. It is thus known to be true, before it can be enunciated in terms each of which is rigorously defined. The business of Definition is part of the business of discovery. When it has been clearly seen what ought to be our Definition, it

must be pretty well known what truth we have to state. The Definition, as well as the discovery, supposes a decided step in our knowledge to have been made. The writers on Logic in the middle ages, made *Definition* the last stage in the progress of knowledge; and in this arrangement at least, the history of science, and the philosophy derived from the history, confirm their speculative views. If the Explication of our Conceptions ever assume the form of a Definition, this will come to pass, not as an arbitrary process, or as a matter of course, but as the mark of one of those happy efforts of sagacity to which all the successive advances of our knowledge are owing.

SECT. III.—*Use of Axioms.*

11. Our Conceptions, then, even when they become so clear as the progress of knowledge requires, are not adequately expressed, or necessarily expressed at all, by means of Definitions. We may ask, then, whether there is any *other mode* of expression in which we may look for the evidence and exposition of that peculiar exactness of thought which the formation of Science demands. And in answer to this inquiry, we may refer to the discussions respecting many of the Fundamental Ideas of the sciences contained in our *History* of such Ideas. It has there been seen that these Ideas involve many elementary truths which enter into the texture of our knowledge, introducing into it connexions and relations of the most important kind, although these elementary truths cannot be deduced from any verbal definition of the idea. It has been seen that these elementary truths may often be enunciated by means of *Axioms,* stated in addition to, or in preference to, Definitions. For example, the Idea of Cause, which forms the basis of the science of Mechanics, makes its appearance in our elementary mechanical reasonings, not as a Definition, but by means of the Axioms that 'Causes are measured by their effects,' and that 'Reaction is equal and opposite to action.' Such axioms, tacitly assumed or occa-

sionally stated, as maxims of acknowledged validity, belong to all the Ideas which form the foundations of the sciences, and are constantly employed in the reasoning and speculations of those who think clearly on such subjects. It may often be a task of some difficulty to detect and enunciate in words the Principles which are thus, perhaps silently and unconsciously, taken for granted by those who have a share in the establishment of scientific truth: but inasmuch as these Principles are an essential element in our knowledge, it is very important to our present purpose to separate them from the associated materials, and to trace them to their origin. This accordingly I attempted to do, with regard to a considerable number of the most prominent of such Ideas, in the *History*. The reader will there find many of these Ideas resolved into Axioms and Principles by means of which their effect upon the elementary reasonings of the various sciences may be expressed. That Work is intended to form, in some measure, a representation of the Ideal Side of our physical knowledge;—a Table of those contents of our Conceptions which are not received directly from facts;—an exhibition of Rules to which we know that truth must conform.

SECT. IV.—*Clear and appropriate Ideas.*

12. In order, however, that we may see the necessary cogency of these rules, we must possess, clearly and steadily, the Ideas from which the rules flow. In order to perceive the necessary relations of the Circles of the Sphere, we must possess clearly the Idea of Solid Space:—in order that we may see the demonstration of the composition of forces, we must have the Idea of Cause moulded into a distinct Conception of Statical Force. This is that *Clearness of Ideas* which we stipulate for in any one's mind, as the first essential condition of his making any new step in the discovery of truth. And we now see what answer we are able to give, if we are asked for a Criterion of this Clearness of

Idea. The Criterion is, that the person shall *see* the necessity of the Axioms belonging to each Idea;—shall accept them in such a manner as to perceive the cogency of the reasonings founded upon them. Thus, a person has a clear Idea of Space who follows the reasonings of geometry and fully apprehends their conclusiveness. The Explication of Conceptions, which we are speaking of as an essential part of real knowledge, is the process by which we bring the Clearness of our Ideas to bear upon the Formation of our knowledge. And this is done, as we have now seen, not always, nor generally, nor principally, by laying down a Definition of the Conception; but by acquiring such a possession of it in our minds as enables, indeed compels us, to admit, along with the Conception, all the Axioms and Principles which it necessarily implies, and by which it produces its effect upon our reasonings.

13. But in order that we may make any real advance in the discovery of truth, our Ideas must not only be clear, they must also be *appropriate*. Each science has for its basis a different class of Ideas; and the steps which constitute the progress of one science can never be made by employing the Ideas of another kind of science. No genuine advance could ever be obtained in Mechanics by applying to the subject the Ideas of Space and Time merely:—no advance in Chemistry, by the use of mere Mechanical Conceptions:—no discovery in Physiology, by referring facts to mere Chemical and Mechanical Principles. Mechanics must involve the Conception of *Force;*—Chemistry, the Conception of *Elementary Composition;*—Physiology, the Conception of *Vital Powers*. Each science must advance by means of its *appropriate* Conceptions. Each has its own field, which extends as far as its principles can be applied. I have already noted the separation of several of these fields by the divisions of the Books of the *History* of Ideas. The Mechanical, the Secondary Mechanical, the Chemical, the Classificatory, the Biological Sciences form so many great Provinces in the Kingdom of knowledge, each in a great measure possessing its own peculiar fundamental principles. Every attempt to build up a

new science by the application of principles which belong to an old one, will lead to frivolous and barren speculations.

This truth has been exemplified in all the instances in which subtle speculative men have failed in their attempts to frame new sciences, and especially in the essays of the ancient schools of philosophy in Greece, as has already been stated in the History of Science. Aristotle and his followers endeavoured in vain to account for the mechanical relation of forces in the lever by applying the *inappropriate* geometrical conceptions of the properties of the circle:—they speculated to no purpose about the elementary composition of bodies, because they assumed the *inappropriate* conception of *likeness* between the elements and the compound, instead of the genuine notion of elements merely *determining* the qualities of the compound. And in like manner, in modern times, we have seen, in the history of the fundamental ideas of the physiological sciences, how all the *inappropriate* mechanical and chemical and other ideas which were applied in succession to the subject failed in bringing into view any genuine physiological truth.

14. That the real cause of the failure in the instances above mentioned lay in the *Conceptions*, is plain. It was not ignorance of the facts which in these cases prevented the discovery of the truth. Aristotle was as well acquainted with the fact of the proportion of the weights which balance on a Lever as Archimedes was, although Archimedes alone gave the true mechanical reason for the proportion.

With regard to the doctrine of the Four Elements indeed, the inapplicability of the conception of composition of qualities, required, perhaps, to be proved by some reference to facts. But this conception was devised at first, and accepted by succeeding times, in a blind and gratuitous manner, which could hardly have happened if men had been awake to the necessary condition of our knowledge;—that the conceptions which we introduce into our doctrines are not arbitrary or accidental notions, but certain peculiar modes of apprehension strictly determined by the subject of our speculations.

15. It may, however, be said that this injunction that we are to employ *appropriate* Conceptions only in the formation of our knowledge, cannot be of practical use, because we can only determine what Ideas *are* appropriate, by finding that they truly combine the facts. And this is to a certain extent true. Scientific discovery must ever depend upon some happy thought, of which we cannot trace the origin;—some fortunate cast of intellect, rising above all rules. No maxims can be given which inevitably lead to discovery. No precepts will elevate a man of ordinary endowments to the level of a man of genius: nor will an inquirer of truly inventive mind need to come to the teacher of inductive philosophy to learn how to exercise the faculties which nature has given him. Such persons as Kepler or Fresnel, or Brewster, will have their powers of discovering truth little augmented by any injunctions respecting Distinct and Appropriate Ideas; and such men may very naturally question the utility of rules altogether.

16. But yet the opinions which such persons may entertain, will not lead us to doubt concerning the value of the attempts to analyse and methodize the process of discovery. Who would attend to Kepler if he had maintained that the speculations of Francis Bacon were worthless? Notwithstanding what has been said, we may venture to assert that the Maxim which points out the necessity of Ideas appropriate as well as clear, for the purpose of discovering truth, is not without its use. It may, at least, have a value as a caution or prohibition, and may thus turn us away from labours certain to be fruitless. We have already seen, in the *History* of Ideas, that this maxim, if duly attended to, would have at once condemned, as wrongly directed, the speculations of physiologists of the mathematical, mechanical, chemical, and vital-fluid schools; since the Ideas which the teachers of these schools introduce, cannot suffice for the purposes of physiology, which seeks truths respecting the vital powers. Again,

it is clear from similar considerations that no definition of a mineralogical species by chemical characters alone can answer the end of science, since we seek to make mineralogy, not an analytical but a classificatory science[1]. Even before the appropriate conception is matured in men's minds so that they see clearly what it is, they may still have light enough to see what it is not.

17. Another result of this view of the necessity of appropriate Ideas, combined with a survey of the history of science is, that though for the most part, as we shall see, the progress of science consists in accumulating and combining Facts rather than in debating concerning Definitions; there are still certain periods when the *discussion* of Definitions may be the most useful mode of cultivating some special branch of science. This discussion is of course always to be conducted by the light of facts; and, as has already been said, along with the settlement of every good Definition will occur the corresponding establishment of some Proposition. But still at particular periods, the want of a Definition, or of the clear conceptions which Definition supposes, may be peculiarly felt. A good and tenable Definition of *Species* in Mineralogy would at present be perhaps the most important step which the science could make. A just conception of the nature of *Life*, (and if expressed by means of a Definition, so much the better,) can hardly fail to give its possessor an immense advantage in the speculations which now come under the considerations of physiologists. And controversies respecting Definitions, in these cases, and such as these, may be very far from idle and unprofitable.

Thus the knowledge that Clear and Appropriate Ideas are requisite for discovery, although it does not lead to any very precise precepts, or supersede the value of natural sagacity and inventiveness, may still

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1 This agrees with what M. Necker has well observed in his *Règne Minéral*, that those who have treated mineralogy as a merely chemical science, have substituted the analysis of substances for the classification of individuals. See *History of Ideas*, b. viii. chap. iii.

be of use to us in our pursuit after truth. It may show us what course of research is, in each stage of science, recommended by the general analogy of the history of knowledge; and it may both save us from hopeless and barren paths of speculation, and make us advance with more courage and confidence, to know that we are looking for discoveries in the manner in which they have always hitherto been made.

SECT. V.—*Accidental Discoveries.*

18. Another consequence follows from the views presented in this Chapter, and it is the last I shall at present mention. *No scientific discovery* can, with any justice, be considered *due to accident.* In whatever manner facts may be presented to the notice of a discoverer, they can never become the materials of exact knowledge, except they find his mind already provided with precise and suitable conceptions by which they may be analysed and connected. Indeed, as we have already seen, facts cannot be observed as Facts, except in virtue of the Conceptions which the observer[2] himself unconsciously supplies; and they are not Facts of Observation for any purpose of Discovery, except these familiar and unconscious acts of thought be themselves of a just and precise kind. But supposing the Facts to be adequately observed, they can never be combined into any new Truth, except by means of some new Conceptions, clear and appropriate, such as I have endeavoured to characterize. When the observer's mind is prepared with such instruments, a very few facts, or it may be a single one, may bring the process of discovery into action. But in such cases, this previous condition of the intellect, and not the single fact, is really the main and peculiar cause of the success. The fact is merely the occasion by which the engine of discovery is brought into play sooner or later. It is, as I have elsewhere said, only the spark which discharges a gun already loaded and pointed; and there

[2] B. i. of this vol. Aphorism III.

is little propriety in speaking of such an accident as the cause why the bullet hits the mark. If it were true that the fall of an apple was the occasion of Newton's pursuing the train of thought which led to the doctrine of universal gravitation, the habits and constitution of Newton's intellect, and not the apple, were the real source of this great event in the progress of knowledge. The common love of the marvellous, and the vulgar desire to bring down the greatest achievements of genius to our own level, may lead men to ascribe such results to any casual circumstances which accompany them; but no one who fairly considers the real nature of great discoveries, and the intellectual processes which they involve, can seriously hold the opinion of their being the effect of accident.

19. Such accidents never happen to common men. Thousands of men, even of the most inquiring and speculative men, had seen bodies fall; but who, except Newton, ever followed the accident to such consequences? And in fact, how little of his train of thought was contained in, or even directly suggested by, the fall of the apple! If the apple fall, said the discoverer, 'why should not the moon, the planets, the satellites, fall?' But how much previous thought,—what a steady conception of the universality of the laws of motion gathered from other sources,—were requisite, that the inquirer should see any connexion in these cases! Was it by accident that he saw in the apple an image of the moon, and of every body in the solar system?

20. The same observations may be made with regard to the other cases which are sometimes adduced as examples of accidental discovery. It has been said, 'By the accidental placing of a rhomb of calcareous spar upon a book or line Bartholinus discovered the property of the *Double Refraction* of light.' But Bartholinus could have seen no such consequence in the accident if he had not previously had a clear conception of *single refraction*. A lady, in describing an optical experiment which had been shown her, said of her teacher, 'He told me to *increase and diminish*

the angle of refraction, and at last I found that he only meant me to move my head up and down.' At any rate, till the lady had acquired the notions which the technical terms convey, she could not have made Bartholinus's discovery by means of his accident. 'By accidentally combining two rhombs in different positions,' it is added, 'Huyghens discovered the *Polarization* of Light.' Supposing that this experiment had been made without design, what Huyghens really observed was, that the images appeared and disappeared alternately as he turned one of the rhombs round. But was it an easy or an obvious business to analyze this curious alternation into the circumstances of the rays of light having *sides*, as Newton expressed it, and into the additional hypotheses which are implied in the term 'polarization'? Those will be able to answer this question, who have found how far from easy it is to understand clearly what is meant by 'polarization' in this case, now that the property is fully established. Huyghens's success depended on his clearness of thought, for this enabled him to perform the intellectual analysis, which never would have occurred to most men, however often they had 'accidentally combined two rhombs in different positions.' 'By accidentally looking through a prism of the same substance, and turning it round, Malus discovered the polarization of light by reflection.' Malus saw that, in some positions of the prism, the light reflected from the windows of the Louvre thus seen through the prism, became dim. A common man would have supposed this dimness the result of accident; but Malus's mind was differently constituted and disciplined. He considered the position of the window, and of the prism; repeated the experiment over and over; and in virtue of the eminently distinct conceptions of space which he possessed, resolved the phenomena into its geometrical conditions. A believer in accident would not have sought them; a person of less clear ideas would not have found them. A person must have a strange confidence in the virtue of chance, and the worthlessness of intellect, who can say that 'in all these fundamental discoveries appropriate ideas had no share,' and that the discoveries 'might have been made by the most ordinary observers.'

21. I have now, I trust, shown in various ways, how the *Explication of Conceptions*, including in this term their clear development from Fundamental Ideas in the discoverer's mind, as well as their precise expression in the form of Definitions or Axioms, when that can be done, is an essential part in the establishment of all exact and general physical truths. In doing this, I have endeavoured to explain in what sense the possession of clear and appropriate ideas is a main requisite for every step in scientific discovery. That it is far from being the only step, I shall soon have to show; and if any obscurity remain on the subject treated of in the present chapter, it will, I hope, be removed when we have examined the other elements which enter into the constitution of our knowledge.

CHAPTER III.

Of Facts as the Materials of Science.

Aphorism IV.

Facts are the materials of science, but all Facts involve Ideas. Since, in observing Facts, we cannot exclude Ideas, we must, for the purposes of science, take care that the Ideas are clear and rigorously applied.

Aphorism V.

The last Aphorism leads to such Rules as the following:— That Facts, for the purposes of material science, must involve Conceptions of the Intellect only, and not Emotions:—That Facts must be observed with reference to our most exact conceptions, Number, Place, Figure, Motion:—That they must also be observed with reference to any other exact conceptions which the phenomena suggest, as Force, in mechanical phenomena, Concord, in musical.

Aphorism VI.

The resolution of complex Facts into precise and measured partial Facts, we call the Decomposition of Facts. *This process is requisite for the progress of science, but does not necessarily lead to progress.*

1. WE have now to examine how Science is built up by the combination of Facts. In doing this, we suppose that we have already attained a supply of definite and certain Facts, free from obscurity and doubt. We must, therefore, first consider under what conditions Facts can assume this character.

When we inquire what Facts are to be made the materials of Science, perhaps the answer which we should most commonly receive would be, that they must be *True Facts*, as distinguished from any mere inferences or opinions of our own. We should probably be told that we must be careful in such a case to consider as Facts, only what we really observe;—that we must assert only what we see; and believe nothing except upon the testimony of our senses.

But such maxims are far from being easy to apply, as a little examination will convince us.

2. It has been explained, in preceding works, that all perception of external objects and occurrences involves an active as well as a passive process of the mind;—includes not only Sensations, but also Ideas by which Sensations are bound together, and have a unity given to them. From this it follows, that there is a difficulty in separating in our perceptions what we receive from without, and what we ourselves contribute from within;—what we perceive, and what we infer. In many cases, this difficulty is obvious to all: as, for example, when we witness the performances of a juggler or a ventriloquist. In these instances, we imagine ourselves to see and to hear what certainly we do not see and hear. The performer takes advantage of the habits by which our minds supply interruptions and infer connexions; and by giving us fallacious indications, he leads us to perceive as an actual fact, what does not happen at all. In these cases, it is evident that we ourselves assist in making the fact; for we make one which does not really exist. In other cases, though the fact which we perceive be true, we can easily see that a large portion of the perception is our own act; as when, from the sight of a bird of prey we infer a carcase, or when we read a half-obliterated inscription. In the latter case, the mind supplies the meaning, and perhaps half the letters; yet we do not hesitate to say that we actually *read* the inscription. Thus, in many cases, our own inferences and interpretations enter into our facts. But this happens in many instances in which it is at first sight less obvious. When any one has seen an oak-tree blown down by a strong gust of wind, he does not think of the occurrence

any otherwise than as a *Fact* of which he is assured by his senses. Yet by what sense does he perceive the Force which he thus supposes the wind to exert? By what sense does he distinguish an Oak-tree from all other trees? It is clear upon reflexion, that in such a case, his own mind supplies the conception of extraneous impulse and pressure, by which he thus interprets the motions observed, and the distinction of different kinds of trees, according to which he thus names the one under his notice. The Idea of Force, and the idea of definite Resemblances and Differences, are thus combined with the impressions on our senses, and form an undistinguished portion of that which we consider as the Fact. And it is evident that we can in no other way perceive Force, than by seeing motion; and cannot give a Name to any object, without not only seeing a difference of single objects, but supposing a difference of classes of objects. When we speak as if we saw impulse and attraction, things and classes, we really see only objects of various forms and colours, more or less numerous, variously combined. But do we really perceive so much as this? When we see the form, the size, the number, the motion of objects, are these really mere impressions on our senses, unmodified by any contribution or operation of the mind itself? A very little attention will suffice to convince us that this is not the case. When we see a windmill turning, it may happen, as we have elsewhere noticed[1], that we mistake the direction in which the sails turn: when we look at certain diagrams, they may appear either convex or concave: when we see the moon first in the horizon and afterwards high up in the sky, we judge her to be much larger in the former than in the latter position, although to the eye she subtends the same angle. And in these cases and the like, it has been seen that the errour and confusion which we thus incur arise from the mixture of acts of the mind itself with impressions on the senses. But such acts are, as we have also seen, *inseparable* portions of the process

[1] *History of Ideas*, B. ii. c. vi. s. 6.

of perception. A certain activity of the mind is involved, not only in seeing objects erroneously, but in seeing them at all. With regard to solid objects, this is generally acknowledged. When we seem to see an edifice occupying space in all dimensions, we really see only a representation of it as it appears referred by perspective to a surface. The inference of the solid form is an operation of our own, alike when we look at a reality and when we look at a picture. But we may go further. Is plane Figure really a mere Sensation? If we look at a decagon, do we see at once that it has ten sides, or is it not necessary for us to count them: and is not counting an act of the mind? All objects are seen in space; all objects are seen as one or many: but are not the Idea of Space and the Idea of Number requisite in order that we may thus apprehend what we see? That these Ideas of Space and Number involve a connexion derived from the mind, and not from the senses, appears, as we have already seen, from this, that those Ideas afford us the materials of universal and necessary truths:—such truths as the senses cannot possibly supply. And thus, even the perception of such facts as the size, shape, and number of objects, cannot be said to be impressions of sense, distinct from all acts of mind, and cannot be expected to be free from errour on the ground of their being mere observed Facts.

Thus the difficulty which we have been illustrating, of distinguishing Facts from inferences and from interpretations of facts, is not only great, but amounts to an impossibility. The separation at which we aimed in the outset of this discussion, and which was supposed to be necessary in order to obtain a firm groundwork for science, is found to be unattainable. We cannot obtain a sure basis of Facts, by rejecting all inferences and judgments of our own, for such inferences and judgments form an unavoidable element in all Facts. We cannot exclude our Ideas from our Perceptions, for our Perceptions involve our Ideas.

3. But still, it cannot be doubted that in selecting the Facts which are to form the foundation of Science,

we must reduce them to their most simple and certain form; and must reject everything from which doubt or errour may arise. Now since this, it appears, cannot be done, by rejecting the Ideas which all Facts involve, in what manner are we to conform to the obvious maxim, that the Facts which form the basis of Science must be perfectly definite and certain?

The analysis of facts into Ideas and Sensations, which we have so often referred to, suggests the answer to this inquiry. We are not able, nor need we endeavour, to exclude Ideas from our Facts; but we may be able to discern, with perfect distinctness, the Ideas which we include. We cannot observe any phenomena without applying to them such Ideas as Space and Number, Cause and Resemblance, and usually, several others; but we may avoid applying these Ideas in a wavering or obscure manner, and confounding Ideas with one another. We cannot read any of the inscriptions which nature presents to us, without interpreting them by means of some language which we ourselves are accustomed to speak; but we may make it our business to acquaint ourselves perfectly with the language which we thus employ, and to interpret it according to the rigorous rules of grammar and analogy.

This maxim, that when Facts are employed as the basis of Science, we must distinguish clearly the Ideas which they involve, and must apply these in a distinct and rigorous manner, will be found to be a more precise guide than we might perhaps at first expect. We may notice one or two Rules which flow from it.

4. In the first place, Facts, when used as the materials of physical Science, must be *referred to Conceptions of the Intellect only*, all emotions of fear, admiration, and the like, being rejected or subdued. Thus, the observations of phenomena which are related as portents and prodigies, striking terrour and boding evil, are of no value for purposes of science. The tales of armies seen warring in the sky, the sound of arms heard from the clouds, fiery dragons, chariots, swords seen in the air, may refer to meteorological phenomena; but the records of phenomena observed in the

state of mind which these descriptions imply can be of no scientific value. We cannot make the poets our observers.

> Armorum sonitum toto Germania cœlo
> Audiit; insolitis tremuerunt motibus Alpes.
> Vox quoque per lucos vulgo exaudita silentes
> Ingens, et simulacra modis pallentia miris
> Visa sub obscurum noctis: pecudesque locutæ.

The mixture of fancy and emotion with the observation of facts has often disfigured them to an extent which is too familiar to all to need illustration. We have an example of this result, in the manner in which Comets are described in the treatises of the middle ages. In such works, these bodies are regularly distributed into several classes, accordingly as they assume the form of a sword, of a spear, of a cross, and so on. When such resemblances had become matters of interest, the impressions of the senses were governed, not by the rigorous conceptions of form and colour, but by these assumed images; and under these circumstances, we can attach little value to the statement of what was seen.

In all such phenomena, the reference of the objects to the exact Ideas of Space, Number, Position, Motion, and the like, is the first step of Science: and accordingly, this reference was established at an early period in those sciences which made an early progress, as, for instance, Astronomy. Yet even in astronomy there appears to have been a period when the predominant conceptions of men in regarding the heavens and the stars pointed to mythical story and supernatural influence, rather than to mere relations of space, time, and motion: and of this primeval condition of those who gazed at the stars, we seem to have remnants in the Constellations, in the mythological Names of the Planets, and in the early prevalence of Astrology. It was only at a later period, when men had begun to measure the places, or at least to count the revolutions of the stars, that Astronomy had its birth.

5. And thus we are led to another Rule:—that in collecting Facts which are to be made the basis of

Science, the Facts are to be observed, as far as possible, *with reference to place, figure, number, motion*, and the like Conceptions; which, depending upon the Ideas of Space and Time, are the most universal, exact, and simple of our conceptions. It was by early attention to these relations in the case of the heavenly bodies, that the ancients formed the science of Astronomy: it was by not making precise observations of this kind in the case of terrestrial bodies, that they failed in framing a science of the Mechanics of Motion. They succeeded in Optics as far as they made observations of this nature; but when they ceased to trace the geometrical paths of rays in the actual experiment, they ceased to go forwards in the knowledge of this subject.

6. But we may state a further Rule:—that though these relations of Time and Space are highly important in almost all Facts, we are not to confine ourselves to these: but are to consider the phenomena *with reference to other Conceptions also*: it being always understood that these conceptions are to be made as exact and rigorous as those of geometry and number. Thus the science of Harmonics arose from considering sounds with reference to *Concords* and *Discords*; the science of Mechanics arose from not only observing motions as they take place in Time and Space, but further, referring them to *Force* as their *Cause*. And in like manner, other sciences depend upon other Ideas, which, as I have endeavoured to show, are not less fundamental than those of Time and Space; and like them, capable of leading to rigorous consequences.

7. Thus the Facts which we assume as the basis of Science are to be freed from all the mists which imagination and passion throw round them; and to be separated into those elementary Facts which exhibit simple and evident relations of Time, or Space, or Cause, or some other Ideas equally clear. We resolve the complex appearances which nature offers to us, and the mixed and manifold modes of looking at these appearances which rise in our thoughts, into limited, definite, and clearly-understood portions. This process we may term the *Decomposition of Facts*. It is the

beginning of exact knowledge,—the first step in the formation of all Science. This Decomposition of Facts into Elementary Facts, clearly understood and surely ascertained, must precede all discovery of the laws of nature.

8. But though this step is necessary, it is not infallibly sufficient. It by no means follows that when we have thus decomposed Facts into Elementary Truths of observation, we shall soon be able to combine these, so as to obtain Truths of a higher and more speculative kind. We have examples which show us how far this is from being a necessary consequence of the former step. Observations of the weather, made and recorded for many years, have not led to any general truths, forming a science of Meteorology: and although great numerical precision has been given to such observations by means of barometers, thermometers, and other instruments, still, no general laws regulating the cycles of change of such phenomena have yet been discovered. In like manner the faces of crystals, and the sides of the polygons which these crystals form, were counted, and thus numerical facts were obtained, perfectly true and definite, but still of no value for purposes of science. And when it was discovered what Element of the form of crystals it was important to observe and measure, namely, the Angle made by two faces with each other, this discovery was a step of a higher order, and did not belong to that department, of mere exact observation of manifest Facts, with which we are here concerned.

9. When the Complex Facts which nature offers to us are thus decomposed into Simple Facts, the decomposition, in general, leads to the introduction of *Terms* and Phrases, more or less technical, by which these Simple Facts are described. When Astronomy was thus made a science of measurement, the things measured were soon described as *Hours*, and *Days*, and *Cycles*, *Altitude* and *Declination*, *Phases* and *Aspects*. In the same manner, in Music, the concords had names assigned them, as *Diapente*, *Diatessaron*, *Diapason;* in studying Optics, the *Rays* of light were spoken of as

having their course altered by *Reflexion* and *Refraction;* and when useful observations began to be made in Mechanics, the observers spoke of *Force*, *Pressure*, *Momentum*, *Inertia*, and the like.

10. When we take phenomena in which the leading Idea is Resemblance, and resolve them into precise component Facts, we obtain some kind of Classification; as, for instance, when we lay down certain Rules by which particular trees, or particular animals are to be known. This is the earliest form of Natural History; and the Classification which it involves is that which corresponds, nearly or exactly, with the usual Names of the objects thus classified.

11. Thus the first attempts to render observation certain and exact, lead to a decomposition of the obvious facts into Elementary Facts, connected by the Ideas of Space, Time, Number, Cause, Likeness, and others: and into a Classification of the Simple Facts; a classification more or less just, and marked by Names either common or technical. Elementary Facts, and Individual Objects, thus observed and classified, form the materials of Science; and any improvement in Classification or Nomenclature, or any discovery of a Connexion among the materials thus accumulated, leads us fairly within the precincts of Science. We must now, therefore, consider the manner in which Science is built up of such materials;—the process by which they are brought into their places, and the texture of the bond which unites and cements them.

CHAPTER IV.

OF THE COLLIGATION OF FACTS.

APHORISM VII.

Science begins with common *observation of facts; but even at this stage, requires that the observations be precise. Hence the sciences which depend upon space and number were the earliest formed. After common observation, come Scientific* Observation *and* Experiment.

APHORISM VIII.

The Conceptions by which Facts are bound together, are suggested by the sagacity of discoverers. This sagacity cannot be taught. It commonly succeeds by guessing; and this success seems to consist in framing several tentative hypotheses *and selecting the right one. But a supply of appropriate hypotheses cannot be constructed by rule, nor without inventive talent.*

APHORISM IX.

The truth of tentative hypotheses must be tested by their application to facts. The discoverer must be ready, carefully to try his hypotheses in this manner, and to reject them if they will not bear the test, in spite of indolence and vanity.

1. FACTS such as the last Chapter speaks of are, by means of such Conceptions as are described in the preceding Chapter, bound together so as to give rise to those general Propositions of which Science consists. Thus the Facts that the planets revolve

about the sun in certain periodic times and at certain distances, are included and connected in Kepler's Law, by means of such Conceptions as the *squares of numbers*, the *cubes of distances*, and the *proportionality* of these quantities. Again the existence of this proportion in the motions of any two planets, forms a set of Facts which may all be combined by means of the Conception of a certain *central accelerating force*, as was proved by Newton. The whole of our physical knowledge consists in the establishment of such propositions; and in all such cases, Facts are bound together by the aid of suitable Conceptions. This part of the formation of our knowledge I have called the *Colligation of Facts:* and we may apply this term to every case in which, by an act of the intellect, we establish a precise connexion among the phenomena which are presented to our senses. The knowledge of such connexions, accumulated and systematized, is Science. On the steps by which science is thus collected from phenomena we shall proceed now to make a few remarks.

2. Science begins with *Common* Observation of facts, in which we are not conscious of any peculiar discipline or habit of thought exercised in observing. Thus the common perceptions of the appearances and recurrences of the celestial luminaries, were the first steps of Astronomy: the obvious cases in which bodies fall or are supported, were the beginning of Mechanics; the familiar aspects of visible things, were the origin of Optics; the usual distinctions of well-known plants, first gave rise to Botany. Facts belonging to such parts of our knowledge are noticed by us, and accumulated in our memories, in the common course of our habits, almost without our being aware that we are observing and collecting facts. Yet such facts may lead to many scientific truths; for instance, in the first stages of Astronomy (as we have shown in the *History*) such facts led to Methods of Intercalation and Rules of the Recurrence of Eclipses. In succeeding stages of science, more especial attention and preparation on the part of the observer, and a selection of certain

kinds of facts, becomes necessary; but there is an early period in the progress of knowledge at which man is a physical philosopher, without seeking to be so, or being aware that he is so.

3. But in all stages of the progress, even in that early one of which we have just spoken, it is necessary, in order that the facts may be fit materials of any knowledge, that they should be decomposed into Elementary Facts, and that these should be observed with precision. Thus, in the first infancy of astronomy, the recurrence of phases of the moon, of places of the sun's rising and setting, of planets, of eclipses, was observed to take place at intervals of certain definite numbers of days, and in a certain exact order; and thus it was, that the observations became portions of astronomical science. In other cases, although the facts were equally numerous, and their general aspect equally familiar, they led to no science, because their exact circumstances were not apprehended. A vague and loose mode of looking at facts very easily observable, left men for a long time under the belief that a body, ten times as heavy as another, falls ten times as fast;—that objects immersed in water are always magnified, without regard to the form of the surface;—that the magnet exerts an irresistible force;—that crystal is always found associated with ice;—and the like. These and many others are examples how blind and careless men can be, even in observation of the plainest and commonest appearances; and they show us that the mere faculties of perception, although constantly exercised upon innumerable objects, may long fail in leading to any exact knowledge.

4. If we further inquire what was the favourable condition through which some special classes of facts were, from the first, fitted to become portions of science, we shall find it to have been principally this;—that these facts were considered with reference to the Ideas of Time, Number, and Space, which are Ideas possessing peculiar definiteness and precision; so that with regard to them, confusion and indistinctness are hardly possible. The interval from new moon to new

moon was always a particular number of days: the sun in his yearly course rose and set near to a known succession of distant objects: the moon's path passed among the stars in a certain order:—these are observations in which mistake and obscurity are not likely to occur, if the smallest degree of attention is bestowed upon the task. To count a number is, from the first opening of man's mental faculties, an operation which no science can render more precise. The relations of space are nearest to those of number in obvious and universal evidence. Sciences depending upon these Ideas arise with the first dawn of intellectual civilization. But few of the other Ideas which man employs in the acquisition of knowledge possess this clearness in their common use. The Idea of *Resemblance* may be noticed, as coming next to those of Space and Number in original precision; and the Idea of *Cause*, in a certain vague and general mode of application, sufficient for the purposes of common life, but not for the ends of science, exercises a very extensive influence over men's thoughts. But the other Ideas on which science depends, with the Conceptions which arise out of them, are not unfolded till a much later period of intellectual progress; and therefore, except in such limited cases as I have noticed, the observations of common spectators and uncultivated nations, however numerous or varied, are of little or no effect in giving rise to Science.

5. Let us now suppose that, besides common everyday perception of facts, we turn our attention to some other occurrences and appearances, with a design of obtaining from them speculative knowledge. This process is more peculiarly called *Observation*, or, when we ourselves occasion the facts, *Experiment*. But the same remark which we have already made, still holds good here. These facts can be of no value, except they are resolved into those exact Conceptions which contain the essential circumstances of the case. They must be determined, not indeed necessarily, as has sometimes been said, 'according to Number, Weight, and Measure;' for, as we have endeavoured to show

in the preceding Books[1], there are many other Conceptions to which phenomena may be subordinated, quite different from these, and yet not at all less definite and precise. But in order that the facts obtained by observation and experiment may be capable of being used in furtherance of our exact and solid knowledge, they must be apprehended and analysed according to some Conceptions which, applied for this purpose, give distinct and definite results, such as can be steadily taken hold of and reasoned from; that is, the facts must be referred to Clear and Appropriate Ideas, according to the manner in which we have already explained this condition of the derivation of our knowledge. The phenomena of light, when they are such as to indicate sides in the ray, must be referred to the Conception of *polarization;* the phenomena of mixture, when there is an alteration of qualities as well as quantities, must be combined by a Conception of *elementary composition.* And thus, when mere position, and number, and resemblance, will no longer answer the purpose of enabling us to connect the facts, we call in other Ideas, in such cases more efficacious, though less obvious.

6. But how are we, in these cases, to discover such Ideas, and to judge which will be efficacious, in leading to a scientific combination of our experimental data? To this question, we must in the first place answer, that the first and great instrument by which facts, so observed with a view to the formation of exact knowledge, are combined into important and permanent truths, is that peculiar Sagacity which belongs to the genius of a Discoverer; and which, while it supplies those distinct and appropriate Conceptions which lead to its success, cannot be limited by rules, or expressed in definitions. It would be difficult or impossible to describe in words the habits of thought which led Archimedes to refer the conditions of equilibrium on the Lever to the Conception of *pressure,* while Aristotle could not see in them anything more than the results

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[1] *Hist. of Sci. Id.* Bs. v. vi. vii. viii. ix. x.

of the strangeness of the properties of the circle;—or which impelled Pascal to explain by means of the Conception of the *weight of air*, the facts which his predecessors had connected by the notion of nature's horrour of a vacuum;—or which caused Vitello and Roger Bacon to refer the magnifying power of a convex lens to the bending of the rays of light towards the perpendicular by *refraction*, while others conceived the effect to result from the matter of medium, with no consideration of its form. These are what are commonly spoken of as felicitous and inexplicable strokes of inventive talent; and such, no doubt, they are. No rules can ensure to us similar success in new cases; or can enable men who do not possess similar endowments, to make like advances in knowledge.

7. Yet still, we may do something in tracing the process by which such discoveries are made; and this it is here our business to do. We may observe that these, and the like discoveries, are not improperly described as happy *Guesses;* and that Guesses, in these as in other instances, imply various suppositions made, of which some one turns out to be the right one. We may, in such cases, conceive the discoverer as inventing and trying many conjectures, till he finds one which answers the purpose of combining the scattered facts into a single rule. The discovery of general truths from special facts is performed, commonly at least, and more commonly than at first appears, by the use of a series of Suppositions, or *Hypotheses*, which are looked at in quick succession, and of which the one which really leads to truth is rapidly detected, and when caught sight of, firmly held, verified, and followed to its consequences. In the minds of most discoverers, this process of invention, trial, and acceptance or rejection of the hypothesis, goes on so rapidly that we cannot trace it in its successive steps. But in some instances, we can do so; and we can also see that the other examples of discovery do not differ essentially from these. The same intellectual operations take place in other cases, although this often happens so instantaneously that we lose the trace of the pro-

gression. In the discoveries made by Kepler, we have a curious and memorable exhibition of this process in its details. Thanks to his communicative disposition, we know that he made nineteen hypotheses with regard to the motion of Mars, and calculated the results of each, before he established the true doctrine, that the planet's path is an ellipse. We know, in like manner, that Galileo made wrong suppositions respecting the laws of falling bodies, and Mariotte, concerning the motion of water in a siphon, before they hit upon the correct view of these cases.

8. But it has very often happened in the history of science, that the erroneous hypotheses which preceded the discovery of the truth have been made, not by the discoverer himself, but by his precursors; to whom he thus owed the service, often an important one in such cases, of exhausting the most tempting forms of errour. Thus the various fruitless suppositions by which Kepler endeavoured to discover the law of refraction, led the way to its real detection by Snell; Kepler's numerous imaginations concerning the forces by which the celestial motions are produced,—his 'physical reasonings' as he termed them,—were a natural prelude to the truer physical reasonings of Newton. The various hypotheses by which the suspension of vapour in air had been explained, and their failure, left the field open for Dalton with his doctrine of the mechanical mixture of gases. In most cases, if we could truly analyze the operation of the thoughts of those who make, or who endeavour to make discoveries in science, we should find that many more suppositions pass through their minds than those which are expressed in words; many a possible combination of conceptions is formed and soon rejected. There is a constant invention and activity, a perpetual creating and selecting power at work, of which the last results only are exhibited to us. Trains of hypotheses are called up and pass rapidly in review; and the judgment makes its choice from the varied group.

9. It would, however, be a great mistake to suppose that the hypotheses, among which our choice thus

lies, are constructed by an enumeration of obvious cases, or by a wanton alteration of relations which occur in some first hypothesis. It may, indeed, sometimes happen that the proposition which is finally established is such as may be formed, by some slight alteration, from those which are justly rejected. Thus Kepler's elliptical theory of Mars's motions, involved relations of lines and angles much of the same nature as his previous false suppositions: and the true law of refraction so much resembles those erroneous ones which Kepler tried, that we cannot help wondering how he chanced to miss it. But it more frequently happens that new truths are brought into view by the application of new Ideas, not by new modifications of old ones. The cause of the properties of the Lever was learnt, not by introducing any new *geometrical* combination of lines and circles, but by referring the properties to genuine *mechanical* Conceptions. When the Motions of the Planets were to be explained, this was done, not by merely improving the previous notions, of cycles of time, but by introducing the new conception of *epicycles* in space. The doctrine of the Four Simple Elements was expelled, not by forming any new scheme of elements which should impart, according to new rules, their sensible qualities to their compounds, but by considering the elements of bodies as *neutralizing* each other. The Fringes of Shadows could not be explained by ascribing new properties to the single rays of light, but were reduced to law by referring them to the *interference* of several rays.

Since the true supposition is thus very frequently something altogether diverse from all the obvious conjectures and combinations, we see here how far we are from being able to reduce discovery to rule, or to give any precepts by which the want of real invention and sagacity shall be supplied. We may warn and encourage these faculties when they exist, but we cannot create them, or make great discoveries when they are absent.

10. The Conceptions which a true theory requires are very often clothed in a *Hypothesis* which connects

with them several superfluous and irrelevant circumstances. Thus the Conception of the Polarization of Light was originally represented under the image of *particles* of light having their poles all turned in the same direction. The Laws of Heat may be made out perhaps most conveniently by conceiving Heat to be a *Fluid.* The Attraction of Gravitation might have been successfully applied to the explanation of facts, if Newton had throughout treated Attraction as the result of an *Ether* diffused through space; a supposition which he has noticed as a possibility. The doctrine of Definite and Multiple Proportions may be conveniently expressed by the hypothesis of *Atoms.* In such cases, the Hypothesis may serve at first to facilitate the introduction of a new Conception. Thus a pervading Ether might for a time remove a difficulty, which some persons find considerable, of imagining a body to exert force at a distance. A Particle with Poles is more easily conceived than Polarization in the abstract. And if hypotheses thus employed will really explain the facts by means of a few simple assumptions, the laws so obtained may afterwards be reduced to a simpler form than that in which they were first suggested. The general laws of Heat, of Attraction, of Polarization, of Multiple Proportions, are now certain, whatever image we may form to ourselves of their ultimate causes.

11. In order, then, to discover scientific truths, suppositions consisting either of new Conceptions, or of new Combinations of old ones, are to be made, till we find one supposition which succeeds in binding together the Facts. But how are we to find this? How is the trial to be made? What is meant by 'success' in these cases? To this we reply, that our inquiry must be, whether the Facts have the same relation in the Hypothesis which they have in reality;—whether the results of our suppositions agree with the phenomena which nature presents to us. For this purpose, we must both carefully observe the phenomena, and steadily trace the consequences of our assumptions, till we can

bring the two into comparison. The Conceptions which our hypotheses involve, being derived from certain Fundamental Ideas, afford a basis of rigorous reasoning, as we have shown in the Books of the *History* of those Ideas. And the results to which this reasoning leads, will be susceptible of being verified or contradicted by observation of the facts. Thus the Epicyclical Theory of the Moon, once assumed, determined what the moon's place among the stars ought to be at any given time, and could therefore be tested by actually observing the moon's places. The doctrine that musical strings of the same length, stretched with weights of 1, 4, 9, 16, would give the musical intervals of an octave, a fifth, a fourth, in succession, could be put to the trial by any one whose ear was capable of appreciating those intervals: and the inference which follows from this doctrine by numerical reasoning,—that there must be certain imperfections in the concords of every musical scale,—could in like manner be confirmed by trying various modes of *Temperament*. In like manner all received theories in science, up to the present time, have been established by taking up some supposition, and comparing it, directly or by means of its remoter consequences, with the facts it was intended to embrace. Its agreement, under certain cautions and conditions, of which we may hereafter speak, is held to be the evidence of its truth. It answers its genuine purpose, the Colligation of Facts.

12. When we have, in any subject, succeeded in one attempt of this kind, and obtained some true Bond of Unity by which the phenomena are held together, the subject is open to further prosecution; which ulterior process may, for the most part, be conducted in a more formal and technical manner. The first great outline of the subject is drawn; and the finishing of the resemblance of nature demands a more minute pencilling, but perhaps requires less of genius in the master. In the pursuance of this task, rules and precepts may be given, and features and leading circumstances pointed out, of which it may often be useful to the inquirer to be aware.

Before proceeding further, I shall speak of some characteristic marks which belong to such scientific processes as are now the subject of our consideration, and which may sometimes aid us in determining when the task has been rightly executed.

CHAPTER V.

OF CERTAIN CHARACTERISTICS OF SCIENTIFIC INDUCTION.

APHORISM X.

The process of scientific discovery is cautious and rigorous, not by abstaining from hypotheses, but by rigorously comparing hypotheses with facts, and by resolutely rejecting all which the comparison does not confirm.

APHORISM XI.

Hypotheses may be useful, though involving much that is superfluous, and even erroneous: for they may supply the true bond of connexion of the facts; and the superfluity and errour may afterwards be pared away.

APHORISM XII.

It is a test of true theories not only to account for, but to predict phenomena.

APHORISM XIII.

Induction *is a term applied to describe the* process *of a true Colligation of Facts by means of an exact and appropriate Conception.* An Induction *is also employed to denote the* proposition *which results from this process.*

APHORISM XIV.

The Consilience of Inductions *takes place when an Induction, obtained from one class of facts, coincides with an Induction, obtained from another different class. This*

Consilience is a test of the truth of the Theory in which it occurs.

APHORISM XV.

An Induction is not the mere sum *of the Facts which are colligated. The Facts are not only brought together, but seen in a new point of view. A new mental Element is* super-induced; *and a peculiar constitution and discipline of mind are requisite in order to make this Induction.*

APHORISM XVI.

Although in Every Induction a new conception is super-induced upon the Facts; yet this once effectually done, the novelty of the conception is overlooked, and the conception is considered as a part of the fact.

SECT. I.—*Invention a part of Induction.*

1. THE two operations spoken of in the preceding chapters,—the Explication of the Conceptions of our own minds, and the Colligation of observed Facts by the aid of such Conceptions,—are, as we have just said, inseparably connected with each other. When united, and employed in collecting knowledge from the phenomena which the world presents to us, they constitute the mental process of *Induction;* which is usually and justly spoken of as the genuine source of all our *real general knowledge* respecting the external world. And we see, from the preceding analysis of this process into its two constituents, from what origin it derives each of its characters. It is *real,* because it arises from the combination of Real Facts, but it is *general,* because it implies the possession of General Ideas. Without the former, it would not be knowledge of the External World; without the latter, it would not be Knowledge at all. When Ideas and Facts are separated from each other, the neglect of Facts gives rise to empty speculations, idle subtleties, visionary inventions, false opinions concerning the laws of phenomena, disregard of the true aspect of nature:

while the want of Ideas leaves the mind overwhelmed, bewildered, and stupified by particular sensations, with no means of connecting the past with the future, the absent with the present, the example with the rule; open to the impression of all appearances, but capable of appropriating none. Ideas are the *Form*, facts the *Material*, of our structure. Knowledge does not consist in the empty mould, or in the brute mass of matter, but in the rightly-moulded substance. Induction gathers general truths from particular facts;—and in her harvest, the corn and the reaper, the solid ears and the binding band, are alike requisite. All our knowledge of nature is obtained by Induction; the term being understood according to the explanation we have now given. And our knowledge is then most complete, then most truly deserves the name of Science, when both its elements are most perfect;—when the Ideas which have been concerned in its formation have, at every step, been clear and consistent; and when they have, at every step also, been employed in binding together real and certain Facts. Of such Induction, I have already given so many examples and illustrations in the two preceding chapters, that I need not now dwell further upon the subject.

2. Induction is familiarly spoken of as the process by which we collect a *General Proposition* from a number of *Particular Cases:* and it appears to be frequently imagined that the general proposition results from a mere juxta-position of the cases, or at most, from merely conjoining and extending them. But if we consider the process more closely, as exhibited in the cases lately spoken of, we shall perceive that this is an inadequate account of the matter. The particular facts are not merely brought together, but there is a New Element added to the combination by the very act of thought by which they are combined. There is a Conception of the mind introduced in the general proposition, which did not exist in any of the observed facts. When the Greeks, after long observing the motions of the planets, saw that these motions might be rightly considered as produced by the motion of one

wheel revolving in the inside of another wheel, these Wheels were Creations of their minds, added to the Facts which they perceived by sense. And even if the wheels were no longer supposed to be material, but were reduced to mere geometrical spheres or circles, they were not the less products of the mind alone,—something additional to the facts observed. The same is the case in all other discoveries. The facts are known, but they are insulated and unconnected, till the discoverer supplies from his own stores a Principle of Connexion. The pearls are there, but they will not hang together till some one provides the String. The distances and periods of the planets were all so many separate facts; by Kepler's Third Law they are connected into a single truth: but the Conceptions which this law involves were supplied by Kepler's mind, and without these, the facts were of no avail. The planets described ellipses round the sun, in the contemplation of others as well as of Newton; but Newton conceived the deflection from the tangent in these elliptical motions in a new light,—as the effect of a Central Force following a certain law; and then it was, that such a force was discovered truly to exist.

Thus[1] in each inference made by Induction, there is introduced some General Conception, which is given, not by the phenomena, but by the mind. The conclusion is not contained in the premises, but includes them by the introduction of a New Generality. In order to obtain our inference, we travel beyond the cases which we have before us; we consider them as mere exemplifications of some Ideal Case in which the relations are complete and intelligible. We take a Standard, and measure the facts by it; and this Standard is constructed by us, not offered by Nature. We assert, for example, that a body left to itself will move on with unaltered velocity; not because our senses ever disclosed to us a body doing this, but because (taking this as our Ideal Case) we find that all

[1] I repeat here remarks made at the end of the *Mechanical Euclid*, p. 178.

actual cases are intelligible and explicable by means of the Conception of *Forces*, causing change and motion, and exerted by surrounding bodies. In like manner, we see bodies striking each other, and thus moving and stopping, accelerating and retarding each other: but in all this, we do not perceive by our senses that abstract quantity, *Momentum*, which is always lost by one body as it is gained by another. This Momentum is a creation of the mind, brought in among the facts, in order to convert their apparent confusion into order, their seeming chance into certainty, their perplexing variety into simplicity. This the Conception of *Momentum gained and lost* does: and in like manner, in any other case in which a truth is established by Induction, some Conception is introduced, some Idea is applied, as the means of binding together the facts, and thus producing the truth.

3. Hence in every inference by Induction, there is some Conception *superinduced* upon the Facts: and we may henceforth conceive this to be the peculiar import of the term *Induction*. I am not to be understood as asserting that the term was originally or anciently employed with this notion of its meaning; for the peculiar feature just pointed out in Induction has generally been overlooked. This appears by the accounts generally given of Induction. 'Induction,' says Aristotle[2], 'is when by means of one extreme term[3] we infer the other extreme term to be true of the middle term.' Thus, (to take such exemplifications as belong to our subject,) from knowing that Mercury, Venus, Mars, describe ellipses about the Sun, we infer that all Planets describe ellipses about the Sun. In making this inference syllogistically, we assume that the evident proposition, 'Mercury, Venus, Mars, do what all Planets do,' may be taken *conversely*, 'All

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[2] *Analyt. Prior.* lib. ii. c. xxiii. Περὶ τῆς ἐπαγωγῆς.

[3] The syllogism here alluded to would be this:—

Mercury, Venus, Mars, describe ellipses about the Sun;
All Planets do what Mercury, Venus, Mars, do;
Therefore all Planets describe ellipses about the Sun.

Planets do what Mercury, Venus, Mars, do.' But we may remark that, in this passage, Aristotle (as was natural in his line of discussion) turns his attention entirely to the *evidence* of the inference; and overlooks a step which is of far more importance to our knowledge, namely, the *invention* of the second extreme term. In the above instance, the particular luminaries, Mercury, Venus, Mars, are one logical *Extreme;* the general designation Planets is the *Middle Term;* but having these before us, how do we come to think of *description of ellipses,* which is the other Extreme of the syllogism? When we have once invented this 'second Extreme Term,' we may, or may not, be satisfied with the evidence of the syllogism; we may, or may not, be convinced that, so far as this property goes, the extremes are co-extensive with the middle term[4]; but the *statement* of the syllogism is the important step in science. We know how long Kepler laboured, how hard he fought, how many devices he tried, before he hit upon this *Term,* the Elliptical Motion. He rejected, as we know, many other 'second extreme Terms,' for example, various combinations of epicyclical constructions, because they did not represent with sufficient accuracy the special facts of observation. When he had established his premiss, that 'Mars does describe an Ellipse about the Sun,' he does not hesitate to *guess* at least that, in this respect, he might *convert* the other premiss, and assert that 'All the Planets do what Mars does.' But the main business was, the inventing and verifying the proposition respecting the Ellipse. The Invention of the Conception was the great step in the *discovery;* the Verification of the Proposition was the great step in the *proof* of the discovery. If Logic consists in pointing out the conditions of proof, the Logic of Induction must consist in showing what are the conditions of proof, in such inferences as this: but this subject must be pursued in the next chapter; I now speak principally of the act of

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[4] Εἰ οὖν ἀντιστρέφει τὸ Γ τῷ Β καὶ μὴ ὑπερτείνει τὸ μέσον.—Aristot. *Ibid.*

Invention, which is requisite in every inductive inference.

4. Although in every inductive inference, an act of invention is requisite, the act soon slips out of notice. Although we bind together facts by superinducing upon them a new Conception, this Conception, once introduced and applied, is looked upon as inseparably connected with the facts, and necessarily implied in them. Having once had the phenomena bound together in their minds in virtue of the Conception, men can no longer easily restore them back to the detached and incoherent condition in which they were before they were thus combined. The pearls once strung, they seem to form a chain by their nature. Induction has given them a unity which it is so far from costing us an effort to preserve, that it requires an effort to imagine it dissolved. For instance, we usually represent to ourselves the Earth as *round*, the Earth and the Planets as *revolving* about the Sun, and as *drawn* to the Sun by a Central Force; we can hardly understand how it could cost the Greeks, and Copernicus, and Newton, so much pains and trouble to arrive at a view which to us is so familiar. These are no longer to us Conceptions caught hold of and kept hold of by a severe struggle; they are the simplest modes of conceiving the facts: they are really Facts. We are willing to *own* our obligation to those discoverers, but we hardly *feel* it: for in what other manner (we ask in our thoughts) could we represent the facts to ourselves?

Thus we see why it is that this step of which we now speak, the Invention of a new Conception in every inductive inference, is so generally overlooked that it has hardly been noticed by preceding philosophers. When once performed by the discoverer, it takes a fixed and permanent place in the understanding of every one. It is a thought which, once breathed forth, permeates all men's minds. All fancy they nearly or quite knew it before. It oft was thought, or almost thought, though never till now expressed. Men accept it and retain it, and know it cannot be taken from them, and look upon it as their own. They will not and cannot part with it, even though they may deem it trivial and obvious. It is a secret, which once uttered, cannot be recalled, even though it be despised by those to whom it is imparted. As soon as the leading term of a new theory has been pronounced and understood, all the phenomena change their aspect. There is a standard to which we cannot help referring them. We cannot fall back into the helpless and bewildered state in which we gazed at them when we possessed no principle which gave them unity. Eclipses arrive in mysterious confusion: the notion of a *Cycle* dispels the mystery. The Planets perform a tangled and mazy dance; but *Epicycles* reduce the maze to order. The Epicycles themselves run into confusion; the conception of an *Ellipse* makes all clear and simple. And thus from stage to stage, new elements of intelligible order are introduced. But this intelligible order is so completely adopted by the human understanding, as to seem part of its texture. Men ask Whether Eclipses follow a Cycle; Whether the Planets describe Ellipses; and they imagine that so long as they do not *answer* such questions rashly, they take nothing for granted. They do not recollect how much they assume in *asking* the question:—how far the conceptions of Cycles and of Ellipses are beyond the visible surface of the celestial phenomena:—how many ages elapsed, how much thought, how much observation, were needed, before men's thoughts were fashioned into the words which they now so familiarly use. And thus they treat the subject, as we have seen Aristotle treating it; as if it were a question, not of invention, but of proof; not of substance, but of form: as if the main thing were not *what* we assert, but *how* we assert it. But for our purpose, it is requisite to bear in mind the feature which we have thus attempted to mark; and to recollect that, in every inference by induction, there is a Conception supplied by the mind and superinduced upon the Facts.

5. In collecting scientific truths by Induction, we often find (as has already been observed) a Definition

and a Proposition established at the same time,—introduced together, and mutually dependent on each other. The combination of the two constitutes the Inductive act; and we may consider the Definition as representing the superinduced Conception, and the Proposition as exhibiting the Colligation of Facts.

SECT. II.—*Use of Hypotheses.*

6. To discover a Conception of the mind which will justly represent a train of observed facts is, in some measure, a process of conjecture, as I have stated already; and as I then observed, the business of conjecture is commonly conducted by calling up before our minds several suppositions, and selecting that one which most agrees with what we know of the observed facts. Hence he who has to discover the laws of nature may have to invent many suppositions before he hits upon the right one; and among the endowments which lead to his success, we must reckon that fertility of invention which ministers to him such imaginary schemes, till at last he finds the one which conforms to the true order of nature. A facility in devising hypotheses, therefore, is so far from being a fault in the intellectual character of a discoverer, that it is, in truth, a faculty indispensable to his task. It is, for his purposes, much better that he should be too ready in contriving, too eager in pursuing systems which promise to introduce law and order among a mass of unarranged facts, than that he should be barren of such inventions and hopeless of such success. Accordingly, as we have already noticed, great discoverers have often invented hypotheses which would not answer to all the facts, as well as those which would; and have fancied themselves to have discovered laws, which a more careful examination of the facts overturned.

The tendencies of our speculative nature[5], carrying

[5] I here take the liberty of characterizing inventive minds in general in the same phraseology which, in the History of Science, I have employed in reference to particular examples. These expressions are what

us onwards in pursuit of symmetry and rule, and thus producing all true theories, perpetually show their vigour by overshooting the mark. They obtain something, by aiming at much more. They detect the order and connexion which exist, by conceiving imaginary relations of order and connexion which have no existence. Real discoveries are thus mixed with baseless assumptions; profound sagacity is combined with fanciful conjecture; not rarely, or in peculiar instances, but commonly, and in most cases; probably in all, if we could read the thoughts of discoverers as we read the books of Kepler. To try wrong guesses is, with most persons, the only way to hit upon right ones. The character of the true philosopher is, not that he never conjectures hazardously, but that his conjectures are clearly conceived, and brought into rigid contact with facts. He sees and compares distinctly the Ideas and the Things;—the relations of his notions to each other and to phenomena. Under these conditions, it is not only excusable, but necessary for him, to snatch at every semblance of general rule,—to try all promising forms of simplicity and symmetry.

Hence advances in knowledge[6] are not commonly made without the previous exercise of some boldness and license in guessing. The discovery of new truths requires, undoubtedly, minds careful and scrupulous in examining what is suggested; but it requires, no less, such as are quick and fertile in suggesting. What is Invention, except the talent of rapidly calling before us the many possibilities, and selecting the appropriate one? It is true, that when we have rejected all the inadmissible suppositions, they are often quickly forgotten; and few think it necessary to dwell on these discarded hypotheses, and on the process by which they were condemned. But all who discover truths, must have reasoned upon many errours to obtain each truth;

I have used in speaking of the discoveries of Copernicus.—*Hist. Ind. Sc.* b. v. c. ii.

[6] These observations are made on occasion of Kepler's speculations, and are illustrated by reference to his discoveries.—*Hist. Ind. Sc.* b. v. c. iv. sect. 1.

every accepted doctrine must have been one chosen out of many candidates. If many of the guesses of philosophers of bygone times now appear fanciful and absurd, because time and observation have refuted them, others, which were at the time equally gratuitous, have been confirmed in a manner which makes them appear marvellously sagacious. To form hypotheses, and then to employ much labour and skill in refuting them, if they do not succeed in establishing them, is a part of the usual process of inventive minds. Such a proceeding belongs to the *rule* of the genius of discovery, rather than (as has often been taught in modern times) to the *exception*.

7. But if it be an advantage for the discoverer of truth that he be ingenious and fertile in inventing hypotheses which may connect the phenomena of nature, it is indispensably requisite that he be diligent and careful in comparing his hypotheses with the facts, and ready to abandon his invention as soon as it appears that it does not agree with the course of actual occurrences. This constant comparison of his own conceptions and supposition with observed facts under all aspects, forms the leading employment of the discoverer: this candid and simple love of truth, which makes him willing to suppress the most favourite production of his own ingenuity as soon as it appears to be at variance with realities, constitutes the first characteristic of his temper. He must have neither the blindness which cannot, nor the obstinacy which will not, perceive the discrepancy of his fancies and his facts. He must allow no indolence, or partial views, or self-complacency, or delight in seeming demonstration, to make him tenacious of the schemes which he devises, any further than they are confirmed by their accordance with nature. The framing of hypotheses is, for the inquirer after truth, not the end, but the beginning of his work. Each of his systems is invented, not that he may admire it and follow it into all its consistent consequences, but that he may make it the occasion of a course of active experiment and observation. And if the results of this process contra-

dict his fundamental assumptions, however ingenious, however symmetrical, however elegant his system may be, he rejects it without hesitation. He allows no natural yearning for the offspring of his own mind to draw him aside from the higher duty of loyalty to his sovereign, Truth: to her he not only gives his affections and his wishes, but strenuous labour and scrupulous minuteness of attention.

We may refer to what we have said of Kepler, Newton, and other eminent philosophers, for illustrations of this character. In Kepler we have remarked[7] the courage and perseverance with which he undertook and executed the task of computing his own hypotheses: and, as a still more admirable characteristic, that he never allowed the labour he had spent upon any conjecture to produce any reluctance in abandoning the hypothesis, as soon as he had evidence of its inaccuracy. And in the history of Newton's discovery that the moon is retained in her orbit by the force of gravity, we have noticed the same moderation in maintaining the hypothesis, after it had once occurred to the author's mind. The hypothesis required that the moon should fall from the tangent of her orbit every second through a space of sixteen feet; but according to his first calculations it appeared that in fact she only fell through a space of thirteen feet in that time. The difference seems small, the approximation encouraging, the theory plausible; a man in love with his own fancies would readily have discovered or invented some probable cause of the difference. But Newton acquiesced in it as a disproof of his conjecture, and 'laid aside at that time any further thoughts of this matter[8].'

8. It has often happened that those who have undertaken to instruct mankind have not possessed this pure love of truth and comparative indifference to the maintenance of their own inventions. Men have frequently adhered with great tenacity and vehemence to the hypotheses which they have once framed; and in their

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[7] *Hist. Ind. Sc.* b. v. c. iv. sect. 1.

[8] *Hist. Ind. Sc.* b. vii. c. ii. sect. 3.

affection for these, have been prone to overlook, to distort, and to misinterpret facts. In this manner, *Hypotheses* have so often been prejudicial to the genuine pursuit of truth, that they have fallen into a kind of obloquy; and have been considered as dangerous temptations and fallacious guides. Many warnings have been uttered against the fabrication of hypotheses, by those who profess to teach philosophy; many disclaimers of such a course by those who cultivate science.

Thus we shall find Bacon frequently discommending this habit, under the name of 'anticipation of the mind,' and Newton thinks it necessary to say emphatically 'hypotheses non fingo.' It has been constantly urged that the inductions by which sciences are formed must be *cautious* and *rigorous;* and the various imaginations which passed through Kepler's brain, and to which he has given utterance, have been blamed or pitied, as lamentable instances of an unphilosophical frame of mind. Yet it has appeared in the preceding remarks that hypotheses rightly used are among the helps, far more than the dangers, of science;—that scientific induction is not a 'cautious' or a 'rigorous' process in the sense of *abstaining from* such suppositions, but in *not adhering to* them till they are confirmed by fact, and in carefully seeking from facts confirmation or refutation. Kepler's distinctive character was, not that he was peculiarly given to the construction of hypotheses, but that he narrated with extraordinary copiousness and candour the course of his thoughts, his labours, and his feelings. In the minds of most persons, as we have said, the inadmissible suppositions, when rejected, are soon forgotten: and thus the trace of them vanishes from the thoughts, and the successful hypothesis alone holds its place in our memory. But in reality, many other transient suppositions must have been made by all discoverers;—hypotheses which are not afterwards asserted as true systems, but entertained for an instant;—'tentative hypotheses,' as they have been called. Each of these hypotheses is followed by its corresponding train of observations, from which it derives its power of leading to truth. The hypothesis is like the captain, and the observations like the soldiers of an army: while he appears to command them, and in this way to work his own will, he does in fact derive all his power of conquest from their obedience, and becomes helpless and useless if they mutiny.

Since the discoverer has thus constantly to work his way onwards by means of hypotheses, false and true, it is highly important for him to possess talents and means for rapidly *testing* each supposition as it offers itself. In this as in other parts of the work of discovery, success has in general been mainly owing to the native ingenuity and sagacity of the discoverer's mind. Yet some Rules tending to further this object have been delivered by eminent philosophers, and some others may perhaps be suggested. Of these we shall here notice only some of the most general, leaving for a future chapter the consideration of some more limited and detailed processes by which, in certain cases, the discovery of the laws of nature may be materially assisted.

SECT. III.—*Tests of Hypotheses.*

9. A maxim which it may be useful to recollect is this;—that *hypotheses may often be of service to science, when they involve a certain portion of incompleteness, and even of errour.* The object of such inventions is to bind together facts which without them are loose and detached; and if they do this, they may lead the way to a perception of the true rule by which the phenomena are associated together, even if they themselves somewhat misstate the matter. The imagined arrangement enables us to contemplate, as a whole, a collection of special cases which perplex and overload our minds when they are considered in succession; and if our scheme has so much of truth in it as to conjoin what is really connected, we may afterwards duly correct or limit the mechanism of this connexion. If our hypothesis renders a reason for the agreement of cases really similar, we may afterwards find this reason to be

false, but we shall be able to translate it into the language of truth.

A conspicuous example of such an hypothesis,—one which was of the highest value to science, though very incomplete, and as a representation of nature altogether false,—is seen in the *Doctrine of epicycles* by which the ancient astronomers explained the motions of the sun, moon, and planets. This doctrine connected the places and velocities of these bodies at particular times in a manner which was, in its general features, agreeable to nature. Yet this doctrine was erroneous in its assertion of the *circular* nature of all the celestial motions, and in making the heavenly bodies revolve *round the earth*. It was, however, of immense value to the progress of astronomical science; for it enabled men to express and reason upon many important truths which they discovered respecting the motion of the stars, up to the time of Kepler. Indeed we can hardly imagine that astronomy could, in its outset, have made so great a progress under any other form, as it did in consequence of being cultivated in this shape of the incomplete and false *epicyclical hypothesis*.

We may notice another instance of an exploded hypothesis, which is generally mentioned only to be ridiculed, and which undoubtedly is both false in the extent of its assertion, and unphilosophical in its expression; but which still, in its day, was not without merit. I mean the doctrine of *Nature's horrour of a vacuum* (*fuga vacui*), by which the action of siphons and pumps and many other phenomena were explained, till Mersenne and Pascal taught a truer doctrine. This hypothesis was of real service; for it brought together many facts which really belong to the same class, although they are very different in their first aspect. A scientific writer of modern times[9] appears to wonder that men did not at once divine the weight of the air, from which the phenomena formerly ascribed to the *fuga vacui* really result. 'Loaded, com-

[9] Deluc, *Modifications de l'Atmosphère*, Partie I.

pressed by the atmosphere,' he says, 'they did not recognize its action. In vain all nature testified that air was elastic and heavy; they shut their eyes to her testimony. The water rose in pumps and flowed in siphons at that time, as it does at this day. They could not separate the boards of a pair of bellows of which the holes were stopped; and they could not bring together the same boards without difficulty, if they were at first separated. Infants sucked the milk of their mothers; air entered rapidly into the lungs of animals at every inspiration; cupping-glasses produced tumours on the skin; and in spite of all these striking proofs of the weight and elasticity of the air, the ancient philosophers maintained resolutely that air was light, and explained all these phenomena by the horrour which they said nature had for a vacuum.' It is curious that it should not have occurred to the author while writing this, that if these facts, so numerous and various, can all be accounted for by *one* principle, there is a strong presumption that the principle is not altogether baseless. And in reality is it not true that nature *does* abhor a vacuum, and does all she can to avoid it? No doubt this power is not unlimited; and moreover we can trace it to a mechanical cause, the pressure of the circumambient air. But the tendency, arising from this pressure, which the bodies surrounding a space void of air have to rush into it, may be expressed, in no extravagant or unintelligible manner, by saying that nature has a repugnance to a vacuum.

That imperfect and false hypotheses, though they may thus explain *some* phenomena, and may be useful in the progress of science, cannot explain *all* phenomena;—and that we are never to rest in our labours or acquiesce in our results, till we have found some view of the subject which *is* consistent with *all* the observed facts;—will of course be understood. We shall afterwards have to speak of the other steps of such a progress.

10. Thus the hypotheses which we accept ought to explain phenomena which we have observed. But they

ought to do more than this: our hypotheses ought to *foretel* phenomena which have not yet been observed; at least all phenomena of the same kind as those which the hypothesis was invented to explain. For our assent to the hypothesis implies that it is held to be true of all particular instances. That these cases belong to past or to future times, that they have or have not already occurred, makes no difference in the applicability of the rule to them. Because the rule prevails, it includes all cases; and will determine them all, if we can only calculate its real consequences. Hence it will predict the results of new combinations, as well as explain the appearances which have occurred in old ones. And that it does this with certainty and correctness, is one mode in which the hypothesis is to be verified as right and useful.

The scientific doctrines which have at various periods been established have been verified in this manner. For example, the *Epicyclical Theory* of the heavens was confirmed by its *predicting* truly eclipses of the sun and moon, configurations of the planets, and other celestial phenomena; and by its leading to the construction of Tables by which the places of the heavenly bodies were given at every moment of time. The truth and accuracy of these predictions were a proof that the hypothesis was valuable, and, at least to a great extent, true; although, as was afterwards found, it involved a false representation of the structure of the heavens. In like manner, the discovery of the *Laws of Refraction* enabled mathematicians to *predict*, by calculation, what would be the effect of any new form or combination of transparent lenses. Newton's hypothesis of *Fits of Easy Transmission and Easy Reflection* in the particles of light, although not confirmed by other kinds of facts, involved a true statement of the law of the phenomena which it was framed to include, and served to *predict* the forms and colours of thin plates for a wide range of given cases. The hypothesis that Light operates by *Undulations* and *Interferences*, afforded the means of *predicting* results under a still larger extent of conditions. In like manner in the progress of chemical knowledge, the doctrine of *Phlogiston* supplied the means of *foreseeing* the consequence of many combinations of elements, even before they were tried; but the *Oxygen Theory*, besides affording predictions, at least equally exact, with regard to the general results of chemical operations, included all the facts concerning the relations of weight of the elements and their compounds, and enabled chemists to *foresee* such facts in untried cases. And the Theory of *Electro-magnetic Forces*, as soon as it was rightly understood, enabled those who had mastered it to *predict* motions such as had not been before observed, which were accordingly found to take place.

Men cannot help believing that the laws laid down by discoverers must be in a great measure identical with the real laws of nature, when the discoverers thus determine effects beforehand in the same manner in which nature herself determines them when the occasion occurs. Those who can do this, must, to a considerable extent, have detected nature's secret;—must have fixed upon the conditions to which she attends, and must have seized the rules by which she applies them. Such a coincidence of untried facts with speculative assertions cannot be the work of chance, but implies some large portion of truth in the principles on which the reasoning is founded. To trace order and law in that which has been observed, may be considered as interpreting what nature has written down for us, and will commonly prove that we understand her alphabet. But to predict what has not been observed, is to attempt ourselves to use the legislative phrases of nature; and when she responds plainly and precisely to that which we thus utter, we cannot but suppose that we have in a great measure made ourselves masters of the meaning and structure of her language. The prediction of results, even of the same kind as those which have been observed, in new cases, is a proof of real success in our inductive processes.

11. We have here spoken of the prediction of facts *of the same kind* as those from which our rule was collected. But the evidence in favour of our

induction is of a much higher and more forcible character when it enables us to explain and determine cases of a *kind different* from those which were contemplated in the formation of our hypothesis. The instances in which this has occurred, indeed, impress us with a conviction that the truth of our hypothesis is certain. No accident could give rise to such an extraordinary coincidence. No false supposition could, after being adjusted to one class of phenomena, exactly represent a different class, where the agreement was unforeseen and uncontemplated. That rules springing from remote and unconnected quarters should thus leap to the same point, can only arise from *that* being the point where truth resides.

Accordingly the cases in which inductions from classes of facts altogether different have thus *jumped together*, belong only to the best established theories which the history of science contains. And as I shall have occasion to refer to this peculiar feature in their evidence, I will take the liberty of describing it by a particular phrase; and will term it the *Consilience of Inductions.*

It is exemplified principally in some of the greatest discoveries. Thus it was found by Newton that the doctrine of the Attraction of the Sun varying according to the Inverse Square of this distance, which explained Kepler's *Third Law*, of the proportionality of the cubes of the distances to the squares of the periodic times of the planets, explained also his *First* and *Second Laws*, of the elliptical motion of each planet; although no connexion of these laws had been visible before. Again, it appeared that the force of Universal Gravitation, which had been inferred from the *Perturbations* of the moon and planets by the sun and by each other, also accounted for the fact, apparently altogether dissimilar and remote, of the *Precession of the equinoxes.* Here was a most striking and surprising coincidence, which gave to the theory a stamp of truth beyond the power of ingenuity to counterfeit. In like manner in Optics; the hypothesis of alternate Fits of easy Transmission and Reflection would explain the colours of thin plates, and indeed was devised and adjusted for that very purpose; but it could give no account of the phenomena of the fringes of shadows. But the doctrine of Interferences, constructed at first with reference to phenomena of the nature of the *Fringes*, explained also the *Colours of thin plates* better than the supposition of the Fits invented for that very purpose. And we have in Physical Optics another example of the same kind, which is quite as striking as the explanation of Precession by inferences from the facts of Perturbation. The doctrine of Undulations propagated in a Spheroidal Form was contrived at first by Huyghens, with a view to explain the laws of *Double Refraction* in calc-spar; and was pursued with the same view by Fresnel. But in the course of the investigation it appeared, in a most unexpected and wonderful manner, that this same doctrine of spheroidal undulations, when it was so modified as to account for the *directions* of the two refracted rays, accounted also for the positions of their *Planes of Polarization*[10]; a phenomenon which, taken by itself, it had perplexed previous mathematicians, even to represent.

The Theory of Universal Gravitation, and of the Undulatory Theory of Light, are, indeed, full of examples of this Consilience of Inductions. With regard to the latter, it has been justly asserted by Herschel, that the history of the undulatory theory was a succession of *felicities*[11]. And it is precisely the unexpected coincidences of results drawn from distant parts of the subject which are properly thus described. Thus the Laws of the *Modification of polarization* to which Fresnel was led by his general views, accounted for the Rule respecting the *Angle at which light is polarized*, discovered by Sir D. Brewster[12]. The conceptions of the theory pointed out peculiar *Modifications* of the phenomena when *Newton's rings* were produced by polarised light, which modifications were ascer-

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10 *Hist. Ind. Sc.* b. ix. c. xi. sect. 4.

11 See *Hist. Ind. Sc.* b. ix. c. xii.

12 *Ib.* c. xi. sect. 4.

tained to take place in fact, by Arago and Airy[13]. When the beautiful phenomena of *Dipolarized light* were discovered by Arago and Biot, Young was able to declare that they were reducible to the general laws of *Interference* which he had already established[14]. And what was no less striking a confirmation of the truth of the theory, *Measures* of the same element deduced from various classes of facts were found to coincide. Thus the *Length* of a luminiferous undulation, calculated by Young from the measurement of *Fringes* of shadows, was found to agree very nearly with the previous calculation from the colours of *Thin plates*[15].

No example can be pointed out, in the whole history of science, so far as I am aware, in which this Consilience of Inductions has given testimony in favour of an hypothesis afterwards discovered to be false. If we take one class of facts only, knowing the law which they follow, we may construct an hypothesis, or perhaps several, which may represent them: and as new circumstances are discovered, we may often adjust the hypothesis so as to correspond to these also. But when the hypothesis, of itself and without adjustment for the purpose, gives us the rule and reason of a class of facts not contemplated in its construction, we have a criterion of its reality, which has never yet been produced in favour of falsehood.

12. In the preceding Article I have spoken of the hypothesis with which we compare our facts as being framed *all at once*, each of its parts being included in the original scheme. In reality, however, it often happens that the various suppositions which our system contains are *added* upon occasion of different researches. Thus in the Ptolemaic doctrine of the heavens, new epicycles and eccentrics were added as new inequalities of the motions of the heavenly bodies were discovered; and in the Newtonian doctrine of material rays of light, the supposition that these rays had

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[13] See *Hist. Ind. Sc.* b. ix. c. xiii. sect. 6.

[14] *Ib.* c. xi. sect. 5.

[15] *Ib.* c. xi. sect. 2.

'fits,' was added to explain the colours of thin plates; and the supposition that they had 'sides' was introduced on occasion of the phenomena of polarization. In like manner other theories have been built up of parts devised at different times.

This being the mode in which theories are often framed, we have to notice a distinction which is found to prevail in the progress of true and false theories. In the former class all the additional suppositions *tend to simplicity* and harmony; the new suppositions resolve themselves into the old ones, or at least require only some easy modification of the hypothesis first assumed: the system becomes more coherent as it is further extended. The elements which we require for explaining a new class of facts are already contained in our system. Different members of the theory run together, and we have thus a constant convergence to unity. In false theories, the contrary is the case. The new suppositions are something altogether additional;—not suggested by the original scheme; perhaps difficult to reconcile with it. Every such addition adds to the complexity of the hypothetical system, which at last becomes unmanageable, and is compelled to surrender its place to some simpler explanation.

Such a false theory, for example, was the ancient doctrine of eccentrics and epicycles. It explained the general succession of the Places of the Sun, Moon, and Planets; it would not have explained the proportion of their Magnitudes at different times, if these could have been accurately observed; but this the ancient astronomers were unable to do. When, however, Tycho and other astronomers came to be able to observe the planets accurately in all positions, it was found that *no* combination of *equable* circular motions would exactly represent all the observations. We may see, in Kepler's works, the many new modifications of the epicyclical hypothesis which offered themselves to him; some of which would have agreed with the phenomena with a certain degree of accuracy, but not with so great a degree as Kepler, fortunately for the progress of science, insisted upon obtaining. After these epicycles had been thus accumulated, they all disappeared and gave way to the simpler conception of an *elliptical* motion. In like manner, the discovery of new inequalities in the Moon's motions encumbered her system more and more with new machinery, which was at last rejected all at once in favour of the *elliptical* theory. Astronomers could not but suppose themselves in a wrong path, when the prospect grew darker and more entangled at every step.

Again; the Cartesian system of Vortices might be said to explain the primary phenomena of the revolutions of planets about the sun, and satellites about planets. But the elliptical form of the orbits required new suppositions. Bernoulli ascribed this curve to the shape of the planet, operating on the stream of the vortex in a manner similar to the rudder of a boat. But then the motions of the aphelia, and of the nodes,—the perturbations,—even the action of gravity towards the earth,—could not be accounted for without new and independent suppositions. Here was none of the simplicity of truth. The theory of Gravitation, on the other hand, became more simple as the facts to be explained became more numerous. The attraction of the sun accounted for the motions of the planets; the attraction of the planets was the cause of the motion of the satellites. But this being assumed, the perturbations, and the motions of the nodes and aphelia, only made it requisite to extend the attraction of the sun to the satellites, and that of the planets to each other:—the tides, the spheroidal form of the earth, the precession, still required nothing more than that the moon and sun should attract the parts of the earth, and that these should attract each other;—so that all the suppositions resolved themselves into the single one, of the universal gravitation of all matter. It is difficult to imagine a more convincing manifestation of simplicity and unity.

Again, to take an example from another science;—the doctrine of Phlogiston brought together many facts in a very plausible manner,—combustion, acidification, and others,—and very naturally prevailed for a while.

But the balance came to be used in chemical operations, and the facts of weight as well as of combination were to be accounted for. On the phlogistic theory, it appeared that this could not be done without a new supposition, and *that*, a very strange one;—that phlogiston was an element not only not heavy, but absolutely light, so that it diminished the weight of the compounds into which it entered. Some chemists for a time adopted this extravagant view; but the wiser of them saw, in the necessity of such a supposition to the defence of the theory, an evidence that the hypothesis of an element *phlogiston* was erroneous. And the opposite hypothesis, which taught that oxygen was subtracted, and not phlogiston added, was accepted because it required no such novel and inadmissible assumption.

Again, we find the same evidence of truth in the progress of the Undulatory Theory of light, in the course of its application from one class of facts to another. Thus we explain Reflection and Refraction by undulations; when we come to Thin Plates, the requisite 'fits' are already involved in our fundamental hypothesis, for they are the length of an undulation: the phenomena of Diffraction also require such intervals; and the intervals thus required agree exactly with the others in magnitude, so that no new property is needed. Polarization for a moment appears to require some new hypothesis; yet this is hardly the case; for the direction of our vibrations is hitherto arbitrary: —we allow polarization to decide it, and we suppose the undulations to be transverse. Having done this for the sake of Polarization, we turn to the phenomena of Double Refraction, and inquire what new hypothesis they require. But the answer is, that they require none: the supposition of transverse vibrations, which we have made in order to explain Polarization, gives us also the law of Double Refraction. Truth may give rise to such a coincidence; falsehood cannot. Again, the facts of Dipolarization come into view. But they hardly require any new assumption; for the difference of optical elasticity of crystals in different directions, which is already assumed in uniaxal crystals[16], is extended to biaxal exactly according to the law of symmetry; and this being done, the laws of the phenomena, curious and complex as they are, are fully explained. The phenomena of Circular Polarization by internal reflection, instead of requiring a new hypothesis, are found to be given by an interpretation of an apparently inexplicable result of an old hypothesis. The Circular Polarization of Quartz and its Double Refraction does indeed appear to require a new assumption, but still not one which at all disturbs the form of the theory; and in short, the whole history of this theory is a progress, constant and steady, often striking and startling, from one degree of evidence and consistence to another of a higher order.

In the Emission Theory, on the other hand, as in the theory of solid epicycles, we see what we may consider as the natural course of things in the career of a false theory. Such a theory may, to a certain extent, explain the phenomena which it was at first contrived to meet; but every new class of facts requires a new supposition—an addition to the machinery: and as observation goes on, these incoherent appendages accumulate, till they overwhelm and upset the original frame-work. Such has been the hypothesis of the Material Emission of light. In its original form, it explained Reflection and Refraction: but the colours of Thin Plates added to it the Fits of easy Transmission and Reflection; the phenomena of Diffraction further invested the emitted particles with complex laws of Attraction and Repulsion; Polarization gave them Sides: Double Refraction subjected them to peculiar Forces emanating from the axes of the crystal: finally, Dipolarization loaded them with the complex and unconnected contrivance of Moveable Polarization: and even when all this had been done, additional mechanism was wanting. There is here no unexpected success, no happy coincidence, no convergence of principles from remote quarters. The philosopher builds

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16 *Hist. Ind. Sc.* b. ix. c. xi. sect. 5.

the machine, but its parts do not fit. They hold together only while he presses them. This is not the character of truth.

As another example of the application of the Maxim now under consideration, I may perhaps be allowed to refer to the judgment which, in the History of Thermotics, I have ventured to give respecting Laplace's Theory of Gases. I have stated[17], that we cannot help forming an unfavourable judgment of this theory, by looking for that great characteristic of true theory; namely, that the hypotheses which were assumed to account for *one class* of facts are found to explain *another class* of a different nature. Thus Laplace's first suppositions explain the connexion of Compression with Density, (the law of Boyle and Mariotte,) and the connexion of Elasticity with Heat, (the law of Dalton and Gay Lussac). But the theory requires other assumptions when we come to Latent Heat; and yet these new assumptions produce no effect upon the calculations in any application of the theory. When the hypothesis, constructed with reference to the Elasticity and Temperature, is applied to another class of facts, those of Latent Heat, we have no Simplification of the Hypothesis, and therefore no evidence of the truth of the theory.

13. The last two sections of this chapter direct our attention to two circumstances, which tend to prove, in a manner which we may term irresistible, the truth of the theories which they characterize:—the *Consilience of Inductions* from different and separate classes of facts;—and the progressive *Simplification of the Theory* as it is extended to new cases. These two Characters are, in fact, hardly different; they are exemplified by the same cases. For if these Inductions, collected from one class of facts, supply an unexpected explanation of a new class, which is the case first spoken of, there will be no need for new machinery in the hypothesis to apply it to the newly-contemplated facts; and thus, we have a case in which the system does not become

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[17] *Hist. Ind. Sc.* b. x. c. iv.

more complex when its application is extended to a wider field, which was the character of true theory in its second aspect. The Consiliences of our Inductions give rise to a constant Convergence of our Theory towards Simplicity and Unity.

But, moreover, both these cases of the extension of the theory, without difficulty or new suppositions, to a wider range and to new classes of phenomena, may be conveniently considered in yet another point of view; namely, as successive steps by which we gradually ascend in our speculative views to a higher and higher point of generality. For when the theory, either by the concurrence of two indications, or by an extension without complication, has included a new range of phenomena, we have, in fact, a new induction of a more general kind, to which the inductions formerly obtained are subordinate, as particular cases to a general proposition. We have in such examples, in short, an instance of *successive generalization.* This is a subject of great importance, and deserving of being well illustrated; it will come under our notice in the next chapter.

CHAPTER VI.

Of the Logic of Induction.

Aphorism XVII.

The Logic of Induction *consists in stating the Facts and the Inference in such a manner, that the Evidence of the Inference is manifest; just as the Logic of Deduction consists in stating the Premises and the Conclusion in such a manner that the Evidence of the Conclusion is manifest.*

Aphorism XVIII.

The Logic of Deduction is exhibited by means of a certain Formula; namely, a Syllogism; and every train of deductive reasoning, to be demonstrative, must be capable of resolution into a series of such Formulæ legitimately constructed. In like manner, the Logic of Induction may be exhibited by means of certain Formulæ; *and every train of inductive inference, to be sound, must be capable of resolution into a scheme of such Formulæ, legitimately constructed.*

Aphorism XIX.

The inductive act of thought *by which several Facts are colligated into one Proposition, may be expressed by saying:* The several Facts are exactly expressed as one Fact, if, and only if, we adopt the Conceptions and the Assertion *of the Proposition.*

Aphorism XX.

The One Fact, thus inductively obtained from several Facts, may be combined with other Facts, and colligated with them by a new act of Induction. This process may be *indefinitely repeated: and these successive processes are the* Steps *of Induction, or of* Generalization, *from the lowest to the highest.*

Aphorism XXI.

The relation of the successive Steps of Induction may be exhibited by means of an Inductive Table, *in which the several Facts are indicated, and tied together by a Bracket, and the Inductive Inference placed on the other side of the Bracket; and this arrangement repeated, so as to form a genealogical Table of each Induction, from the lowest to the highest.*

Aphorism XXII.

The Logic of Induction is the Criterion of Truth *inferred from Facts, as the Logic of Deduction is the Criterion of Truth deduced from necessary Principles. The Inductive Table enables us to apply such a Criterion; for we can determine whether each Induction is verified and justified by the Facts which its Bracket includes; and if each induction in particular be sound, the highest, which merely combines them all, must necessarily be sound also.*

Aphorism XXIII.

The distinction of Fact *and* Theory *is only relative. Events and phenomena, considered as Particulars which may be colligated by Induction, are* Facts; *considered as Generalities already obtained by colligation of other Facts, they are* Theories. *The same event or phenomenon is a Fact or a Theory, according as it is considered as standing on one side or the other of the Inductive Bracket.*

1. THE subject to which the present chapter refers is described by phrases which are at the present day familiarly used in speaking of the progress of knowledge. We hear very frequent mention of *ascending from particular to general* propositions, and from these to propositions still more general;—of

truths *included* in other truths of a higher degree of generality;—of different *stages of generalization;*—and of the *highest step* of the process of discovery, to which all others are subordinate and preparatory. As these expressions, so familiar to our ears, especially since the time of Francis Bacon, denote, very significantly, processes and relations which are of great importance in the formation of science, it is necessary for us to give a clear account of them, illustrated with general exemplifications; and this we shall endeavour to do.

We have, indeed, already explained that science consists of Propositions which include the Facts from which they were collected; and other wider Propositions, collected in like manner from the former, and including them. Thus, that the stars, the moon, the sun, rise, culminate, and set, are facts *included* in the proposition that the heavens, carrying with them all the celestial bodies, have a diurnal revolution about the axis of the earth. Again, the observed monthly motions of the moon, and the annual motions of the sun, are *included* in certain propositions concerning the movements of those luminaries with respect to the stars. But all these propositions are really *included* in the doctrine that the earth, revolving on its axis, moves round the sun, and the moon round the earth. These movements, again, considered as facts, are explained and *included* in the statement of the forces which the earth exerts upon the moon, and the sun upon the earth. Again, this doctrine of the forces of these three bodies is *included* in the assertion, that all the bodies of the solar system, and all parts of matter, exert forces, each upon each. And we might easily show that all the leading facts in astronomy are comprehended in the same generalization. In like manner with regard to any other science, so far as its truths have been well established and fully developed, we might show that it consists of a gradation of propositions, proceeding from the most special facts to the most general theoretical assertions. We shall exhibit this gradation in some of the principal branches of science.

2. This gradation of truths, successively included in other truths, may be conveniently represented by *Tables* resembling the genealogical tables by which the derivation of descendants from a common ancestor is exhibited; except that it is proper in this case to invert the form of the Table, and to make it converge to unity downwards instead of upwards, since it has for its purpose to express, not the derivation of many from one, but the collection of one truth from many things. Two or more co-ordinate facts or propositions may be ranged side by side, and joined by some mark of connexion, (a bracket, as ⏟ or ⎵,) beneath which may be placed the more general proposition which is collected by induction from the former. Again, propositions co-ordinate with this more general one may be placed on a level with it; and the combination of these, and the result of the combination, may be indicated by brackets in the same manner; and so on, through any number of gradations. By this means the streams of knowledge from various classes of facts will constantly run together into a smaller and smaller number of channels; like the confluent rivulets of a great river, coming together from many sources, uniting their ramifications so as to form larger branches, these again uniting in a single trunk. The *genealogical tree* of each great portion of science, thus formed, will contain all the leading truths of the science arranged in their due co-ordination and subordination. Such Tables, constructed for the sciences of Astronomy and of Optics, will be given at the end of this chapter.

3. The union of co-ordinate propositions into a proposition of a higher order, which occurs in this Tree of Science wherever two twigs unite in one branch, is, in each case, an example of *Induction.* The single proposition is collected by the process of induction from its several members. But here we may observe, that the image of a mere *union* of the parts at each of these points, which the figure of a tree or a river presents, is very inadequate to convey the true state of the case; for in Induction, as we have seen, besides mere collection of particulars, there is always a *new conception*, a

principle of connexion and unity, supplied by the mind, and superinduced upon the particulars. There is not merely a juxta-position of materials, by which the new proposition contains all that its component parts contained; but also a formative act exerted by the understanding, so that these materials are contained in a new shape. We must remember, therefore, that our Inductive Tables, although they represent the elements and the order of these inductive steps, do not fully represent the whole signification of the process in each case.

4. The principal features of the progress of science spoken of in the last chapter are clearly exhibited in these Tables; namely, the *Consilience of Inductions*, and the constant Tendency to Simplicity observable in true theories. Indeed in all cases in which, from propositions of considerable generality, propositions of a still higher degree are obtained, there is a convergence of inductions; and if in one of the lines which thus converge, the steps be rapidly and suddenly made in order to meet the other line, we may consider that we have an example of Consilience. Thus when Newton had collected, from Kepler's Laws, the Central Force of the sun, and from these, combined with other facts, the Universal Force of all the heavenly bodies, he suddenly turned round to include in his generalization the Precession of the Equinoxes, which he declared to arise from the attraction of the sun and moon upon the protuberant part of the terrestrial spheroid. The apparent remoteness of this fact, in its nature, from the other facts with which he thus associated it, causes this part of his reasoning to strike us as a remarkable example of *Consilience*. Accordingly, in the Table of Astronomy we find that the columns which contain the facts and theories relative to the *sun* and *planets*, after exhibiting several stages of induction within themselves, are at length suddenly connected with a column till then quite distinct, containing the *precession of the equinoxes*. In like manner, in the Table of Optics, the columns which contain the facts and theories relative to *double refraction*, and those which

include *polarization by crystals*, each go separately through several stages of induction; and then these two sets of columns are suddenly connected by Fresnel's mathematical induction, that double refraction and polarization arise from the same cause: thus exhibiting a remarkable *Consilience*.

5. The constant *Tendency to Simplicity* in the sciences of which the progress is thus represented, appears from the form of the Table itself; for the single trunk into which all the branches converge, contains in itself the substance of all the propositions by means of which this last generalization was arrived at. It is true, that this ultimate result is sometimes not so simple as in the Table it appears: for instance, the ultimate generalization of the Table exhibiting the progress of Physical Optics,—namely, that Light consists in Undulations,—must be understood as including some other hypotheses; as, that the undulations are transverse, that the ether through which they are propagated has its elasticity in crystals and other transparent bodies regulated by certain laws; and the like. Yet still, even acknowledging all the complication thus implied, the Table in question evidences clearly enough the constant advance towards unity, consistency, and simplicity, which have marked the progress of this Theory. The same is the case in the Inductive Table of Astronomy in a still greater degree.

6. These Tables naturally afford the opportunity of assigning to each of the distinct steps of which the progress of science consists, the name of the *Discoverer* to whom it is due. Every one of the inductive processes which the brackets of our Tables mark, directs our attention to some person by whom the induction was first distinctly made. These names I have endeavoured to put in their due places in the Tables; and the Inductive Tree of our knowledge in each science becomes, in this way, an exhibition of the claims of each discoverer to distinction, and, as it were, a Genealogical Tree of scientific nobility. It is by no means pretended that such a tree includes the

names of all the meritorious labourers in each department of science. Many persons are most usefully employed in collecting and verifying truths, who do not advance to any new truths. The labours of a number of such are included in each stage of our ascent. But such Tables as we have now before us will present to us the names of all the most eminent discoverers: for the main steps of which the progress of science consists, are transitions from more particular to more general truths, and must therefore be rightly given by these Tables; and those must be the greatest names in science to whom the principal events of its advance are thus due.

7. The Tables, as we have presented them, exhibit the course by which we pass from Particular to General through various gradations, and so to the most general. They display the order of *discovery*. But by reading them in an inverted manner, beginning at the single comprehensive truths with which the Tables end, and tracing these back into the more partial truths, and these again into special facts, they answer another purpose;—they exhibit the process of *verification* of discoveries once made. For each of our general propositions is true in virtue of the truth of the narrower propositions which it involves; and we cannot satisfy ourselves of its truth in any other way than by ascertaining that these its constituent elements are true. To assure ourselves that the sun attracts the planets with forces varying inversely as the square of the distance, we must analyse by geometry the motion of a body in an ellipse about the focus, so as to see that such a motion does imply such a force. We must also verify those calculations by which the observed places of each planet are stated to be included in an ellipse. These calculations involve assumptions respecting the path which the earth describes about the sun, which assumptions must again be verified by reference to observation. And thus, proceeding from step to step, we resolve the most general truths into their constituent parts; and these again into their parts; and by testing, at each step, both the reality of the asserted ingredients and the propriety

of the conjunction, we establish the whole system of truths, however wide and various it may be.

8. It is a very great advantage, in such a mode of exhibiting scientific truths, that it resolves the verification of the most complex and comprehensive theories, into a number of small steps, of which almost any one falls within the reach of common talents and industry. That *if* the particulars of any one step be true, the generalization also is true, any person with a mind properly disciplined may satisfy himself by a little study. That each of these particular propositions *is* true, may be ascertained, by the same kind of attention, when this proposition is resolved into *its* constituent and more special propositions. And thus we may proceed, till the most general truth is broken up into small and manageable portions. Of these portions, each may appear by itself narrow and easy; and yet they are so woven together, by hypothesis and conjunction, that the truth of the parts necessarily assures us of the truth of the whole. The verification is of the same nature as the verification of a large and complex statement of great sums received by a mercantile office on various accounts from many quarters. The statement is separated into certain comprehensive heads, and these into others less extensive; and these again into smaller collections of separate articles, each of which can be inquired into and reported on by separate persons. And thus at last, the mere addition of numbers performed by these various persons, and the summation of the results which they obtain, executed by other accountants, is a complete and entire security that there is no errour in the whole of the process.

9. This comparison of the process by which we verify scientific truth to the process of Book-keeping in a large commercial establishment, may appear to some persons not sufficiently dignified for the subject. But, in fact, the possibility of giving this formal and business-like aspect to the evidence of science, as involved in the process of successive generalization, is an inestimable advantage. For if no one could pronounce concerning a wide and profound theory except he who

could at once embrace in his mind the whole range of inference, extending from the special facts up to the most general principles, none but the greatest geniuses would be entitled to judge concerning the truth or errour of scientific discoveries. But, in reality, we seldom need to verify more than one or two steps of such discoveries at one time; and this may commonly be done (when the discoveries have been fully established and developed,) by any one who brings to the task clear conceptions and steady attention. The progress of science is gradual: the discoveries which are successively made, are also verified successively. We have never any very large collections of them on our hands at once. The doubts and uncertainties of any one who has studied science with care and perseverance are generally confined to a few points. If he can satisfy himself upon these, he has no misgivings respecting the rest of the structure; which has indeed been repeatedly verified by other persons in like manner. The fact that science is capable of being resolved into separate processes of verification, is that which renders it possible to form a great body of scientific truth, by adding together a vast number of truths, of which many men, at various times and by multiplied efforts, have satisfied themselves. The treasury of Science is constantly rich and abundant, because it accumulates the wealth which is thus gathered by so many, and reckoned over by so many more: and the dignity of Knowledge is no more lowered by the multiplicity of the tasks on which her servants are employed, and the narrow field of labour to which some confine themselves, than the rich merchant is degraded by the number of offices which it is necessary for him to maintain, and the minute articles of which he requires an exact statement from his accountants.

10. The analysis of doctrines inductively obtained, into their constituent facts, and the arrangement of them in such a form that the conclusiveness of the induction may be distinctly seen, may be termed *the Logic of Induction.* By *Logic* has generally been meant a system which teaches us so to arrange our

reasonings that their truth or falsehood shall be evident in their form. In *deductive* reasonings, in which the general principles are assumed, and the question is concerning their application and combination in particular cases, the device which thus enables us to judge whether our reasonings are conclusive is the *Syllogism;* and this *form*, along with the rules which belong to it, does in fact supply us with a criterion of deductive or demonstrative reasoning. The *Inductive Table,* such as it is presented in the present chapter, in like manner supplies the means of ascertaining the truth of our *inductive* inferences, so far as the *form* in which our reasoning may be stated can afford such a criterion. Of course some care is requisite in order to reduce a train of demonstration into the form of a series of syllogisms; and certainly not less thought and attention are required for resolving all the main doctrines of any great department of science into a graduated table of coordinate and subordinate inductions. But in each case, when this task is once executed, the evidence or want of evidence of our conclusions appears immediately in a most luminous manner. In each step of induction, our Table enumerates the particular facts, and states the general theoretical truth which includes these and which these constitute. The special act of attention by which we satisfy ourselves that the facts *are* so included,—that the general truth *is* so constituted,—then affords little room for errour, with moderate attention and clearness of thought.

11. We may find an example of this *act of attention* thus required, at any one of the steps of induction in our Tables; for instance, at the step in the early progress of astronomy at which it was inferred, that the earth is a globe, and that the sphere of the heavens (relatively) performs a diurnal revolution round this globe of the earth. How was this established in the belief of the Greeks, and how is it fixed in our conviction? As to the globular form, we find that as we travel to the north, the apparent pole of the heavenly motions, and the constellations which are near it, seem to mount higher, and as we proceed southwards they descend.

Again, if we proceed from two different points considerably to the east and west of each other, and travel directly northwards from each, as from the south of Spain to the north of Scotland, and from Greece to Scandinavia, these two north and south lines will be much nearer to each other in their northern than in their southern parts. These and similar facts, as soon as they are clearly estimated and connected in the mind, are *seen to be consistent* with a convex surface of the earth, and with no other: and this notion is further confirmed by observing that the boundary of the earth's shadow upon the moon is always circular; it being supposed to be already established that the moon receives her light from the sun, and that lunar eclipses are caused by the interposition of the earth. As for the assertion of the (relative) diurnal revolution of the starry sphere, it is merely putting the visible phenomena in an exact geometrical form: and thus we establish and verify the doctrine of the revolution of the sphere of the heavens about the globe of the earth, by contemplating it so as to *see* that it does really and exactly include the particular facts from which it is collected.

We may, in like manner, illustrate this mode of verification by any of the other steps of the same Table. Thus if we take the great Induction of Copernicus, the heliocentric scheme of the solar system, we find it in the Table exhibited as including and explaining, *first*, the diurnal revolution just spoken of; *second*, the motions of the moon among the fixed stars; *third*, the motions of the planets with reference to the fixed stars and the sun; *fourth*, the motion of the sun in the ecliptic. And the scheme being clearly conceived, we *see* that all the particular facts *are* faithfully represented by it; and this agreement, along with the simplicity of the scheme, in which respect it is so far superior to any other conception of the solar system, persuade us that it is really the plan of nature.

In exactly the same way, if we attend to any of the several remarkable discoveries of Newton, which form the principal steps in the latter part of the Table, as for instance, the proposition that the sun attracts all the planets with a force which varies inversely as the square of the distance, we find it proved by its including three other propositions previously established;—*first*, that the sun's mean force on different planets follows the specified variation (which is proved from Kepler's third law); *second*, that the force by which each planet is acted upon in different parts of its orbit tends to the sun (which is proved by the equable description of areas); *third*, that this force in different parts of the same orbit is also inversely as the square of the distance (which is proved from the elliptical form of the orbit). And the Newtonian generalization, when its consequences are mathematically traced, is *seen* to agree with each of these particular propositions, and thus is fully established.

12. But when we say that the more general proposition *includes* the several more particular ones, we must recollect what has before been said, that these particulars form the general truth, not by being merely enumerated and added together, but by being seen *in a new light*. No mere verbal recitation of the particulars can decide whether the general proposition is true; a special act of thought is requisite in order to determine how truly each is included in the supposed induction. In this respect the Inductive Table is not like a mere schedule of accounts, where the rightness of each part of the reckoning is tested by mere addition of the particulars. On the contrary, the Inductive truth is never the mere *sum* of the facts. It is made into something more by the introduction of a new mental element; and the mind, in order to be able to supply this element, must have peculiar endowments and discipline. Thus looking back at the instances noticed in the last article, how are we to see that a convex surface of the earth is necessarily implied by the convergence of meridians towards the north, or by the visible descent of the north pole of the heavens as we travel south? Manifestly the student, in order to see this, must have clear conceptions of the relations of space, either naturally inherent in his mind, or established there by geometrical cultivation,—by study-

ing the properties of circles and spheres. When he is so prepared, he will feel the force of the expressions we have used, that the facts just mentioned are *seen to be consistent* with a globular form of the earth; but without such aptitude he will not see this consistency: and if this be so, the mere assertion of it in words will not avail him in satisfying himself of the truth of the proposition.

In like manner, in order to perceive the force of the Copernican induction, the student must have his mind so disciplined by geometrical studies, or otherwise, that he sees clearly how absolute motion and relative motion would alike produce apparent motion. He must have learnt to cast away all prejudices arising from the seeming fixity of the earth; and then he will see that there is nothing which stands in the way of the induction, while there is much which is on its side. And in the same manner the Newtonian induction of the law of the sun's force from the elliptical form of the orbit, will be evidently satisfactory to him only who has such an insight into Mechanics as to see that a curvilinear path must arise from a constantly deflecting force; and who is able to follow the steps of geometrical reasoning by which, from the properties of the ellipse, Newton proves this deflection to be in the proportion in which he asserts the force to be. And thus in all cases the inductive truth must indeed be verified by comparing it with the particular facts; but then this comparison is possible for him only whose mind is properly disciplined and prepared in the use of those conceptions, which, in addition to the facts, the act of induction requires.

13. In the Tables some indication is given, at several of the steps, of the act which the mind must thus perform, besides the mere conjunction of facts, in order to attain to the inductive truth. Thus in the cases of the Newtonian inductions just spoken of, the inferences are stated to be made 'By Mechanics;' and in the case of the Copernican induction, it is said that, 'By the nature of motion, the apparent motion is the same, whether the heavens or the earth have a

diurnal motion; and the latter is more simple.' But these verbal statements are to be understood as mere hints[1]: they cannot supersede the necessity of the student's contemplating for himself the mechanical principles and the nature of motion thus referred to.

14. In the common or Syllogistic Logic, a certain *Formula* of language is used in stating the reasoning, and is useful in enabling us more readily to apply the Criterion of Form to alleged demonstrations. This formula is the usual Syllogism; with its members, Major Premiss, Minor Premiss, and Conclusion. It may naturally be asked whether in Inductive Logic there is any such Formula? whether there is any standard form of words in which we may most properly express the inference of a general truth from particular facts?

At first it might be supposed that the formula of Inductive Logic need only be of this kind: 'These particulars, and all known particulars of the same kind, are exactly included in the following general proposition.' But a moment's reflection on what has just been said will show us that this is not sufficient: for the particulars are not merely *included* in the general proposition. It is not enough that they appertain to it by enumeration. It is, for instance, no adequate example of Induction to say, 'Mercury describes an elliptical path, so does Venus, so do the Earth, Mars, Jupiter, Saturn, Uranus; therefore all the Planets describe elliptical paths.' This is, as we have seen, the mode of stating the *evidence* when the proposition is once suggested; but the Inductive step consists in the *suggestion* of a conception not before apparent. When Kepler, after trying to connect the observed places of the planet Mars in many other ways, found at last that the conception of an *ellipse* would include them all, he obtained a truth by induction: for this conclusion was not obviously included in the phenomena, and had not been applied to these

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1 In the Inductive Tables they are marked by an asterisk.

facts previously. Thus in our Formula, besides stating that the particulars are included in the general proposition, we must also imply that the generality is constituted by a new Conception,—new at least in its application.

Hence our Inductive Formula might be something like the following: 'These particulars, and all known particulars of the same kind, are exactly expressed by adopting the Conceptions and Statement of the following Proposition.' It is of course requisite that the Conceptions should be perfectly clear, and should precisely embrace the facts, according to the explanation we have already given of those conditions.

15. It may happen, as we have already stated, that the Explication of a Conception, by which it acquires its due distinctness, leads to a Definition, which Definition may be taken as the summary and total result of the intellectual efforts to which this distinctness is due. In such cases, the Formula of Induction may be modified according to this condition; and we may state the inference by saying, after an enumeration and analysis of the appropriate facts, 'These facts are completely and distinctly expressed by adopting the following Definition and Proposition.'

This Formula has been adopted in stating the Inductive Propositions which constitute the basis of the science of Mechanics, in a work intitled *The Mechanical Euclid.* The fundamental truths of the subject are expressed in *Inductive Pairs* of Assertions, consisting each of a Definition and a Proposition, such as the following:

Def.—A *Uniform Force* is that which acting in the direction of the body's motion, adds or subtracts equal velocities in equal times.

Prop.—Gravity is a Uniform Force.

Again,

Def.—Two *Motions* are *compounded* when each produces its separate effect in a direction parallel to itself.

Prop.—When any Force acts upon a body in motion, the motion which the Force would produce in the body at rest is compounded with the previous motion of the body.

And in like manner in other cases.

In these cases the proposition is, of course, established, and the definition realized, by an enumeration of the facts. And in the case of inferences made in such a form, the Definition of the Conception and the Assertion of the Truth are both requisite and are correlative to one another. Each of the two steps contains the verification and justification of the other. The Proposition derives its meaning from the Definition; the Definition derives its reality from the Proposition. If they are separated, the Definition is arbitrary or empty, the Proposition vague or ambiguous.

16. But it must be observed that neither of the preceding Formulæ expresses the full cogency of the inductive proof. They declare only that the results *can* be clearly explained and rigorously deduced by the employment of a certain Definition and a certain Proposition. But in order to make the conclusion demonstrative, which in perfect examples of Induction it is, we ought to be able to declare that the results can be clearly explained and rigorously declared *only* by the Definition and Proposition which we adopt. And in reality, the conviction of the sound inductive reasoner does reach to this point. The Mathematician asserts the Laws of Motion, seeing clearly that they (or laws equivalent to them) afford the only means of clearly expressing and deducing the actual facts. But this conviction, that the inductive inference is not only consistent with the facts, but necessary, finds its place in the mind gradually, as the contemplation of the consequences of the proposition, and the various relations of the facts, becomes steady and familiar. It is scarcely possible for the student at once to satisfy himself that the inference is thus inevitable. And when he arrives at this conviction, he sees also, in many cases at least, that there may be other ways of expressing the substance of the truth established, besides that special Proposition which he has under his notice.

We may, therefore, without impropriety, renounce the undertaking of conveying in our formula this final conviction of the necessary truth of our inference. We may leave it to be thought, without insisting upon saying it, that in such cases what *can* be true, *is* true. But if we wish to express the ultimate significance of the Inductive Act of thought, we may take as our Formula for the Colligation of Facts by Induction, this:—'The several Facts are exactly expressed as one Fact if, *and only if*, we adopt the Conception and the Assertion' of the inductive inference.

17. I have said that the mind must be properly disciplined in order that it may see the necessary connexion between the facts and the general proposition in which they are included. And the perception of this connexion, though treated as *one step* in our inductive inference, may imply *many steps* of demonstrative proof. The connexion is this, that the particular case is included in the general one, that is, may be *deduced* from it: but this deduction may often require many links of reasoning. Thus in the case of the inference of the law of the force from the elliptical form of the orbit by Newton, the proof that in the ellipse the deflection from the tangent is inversely as the square of the distance from the focus of the ellipse, is a ratiocination consisting of several steps, and involving several properties of Conic Sections; these properties being supposed to be previously established by a geometrical system of demonstration on the special subject of the Conic Sections. In this and similar cases the Induction involves many steps of Deduction. And in such cases, although the Inductive Step, the Invention of the Conception, is really the most important, yet since, when once made, it occupies a familiar place in men's minds; and since the Deductive Demonstration is of considerable length and requires intellectual effort to follow it at every step; men often admire the deductive part of the proposition, the geometrical or algebraical demonstration, far more than that part in which the philosophical merit really resides.

18. Deductive reasoning is virtually a collection of syllogisms, as has already been stated; and in such reasoning, the general principles, the Definitions and Axioms, necessarily stand at the *beginning* of the demonstration. In an inductive inference, the Definitions and Principles are the *final result* of the reasoning, the ultimate effect of the proof. Hence when an Inductive Proposition is to be established by a proof involving several steps of demonstrative reasoning, the enunciation of the Proposition will contain, explicitly or implicitly, principles which the demonstration proceeds upon as axioms, but which are really inductive inferences. Thus in order to prove that the force which retains a planet in an ellipse varies inversely as the square of the distance, it is taken for granted that the Laws of Motion are true, and that they apply to the planets. Yet the doctrine that this is so, as well as the law of the force, were established only by this and the like demonstrations. The doctrine which is the *hypothesis* of the deductive reasoning, is the *inference* of the inductive process. The special facts which are the basis of the inductive inference, are the conclusion of the train of deduction. And in this manner the deduction establishes the induction. The principle which we gather from the facts is true, because the facts can be derived from it by rigorous demonstration. Induction moves upwards, and deduction downwards, on the same stair.

But still there is a great difference in the character of their movements. Deduction descends steadily and methodically, step by step: Induction mounts by a leap which is out of the reach of method. She bounds to the top of the stair at once; and then it is the business of Deduction, by trying each step in order, to establish the solidity of her companion's footing. Yet these must be processes of the same mind. The Inductive Intellect makes an assertion which is subsequently justified by demonstration; and it shows its sagacity, its peculiar character, by enunciating the proposition when as yet the demonstration does not exist: but then it shows that it *is* sagacity, by also producing the demonstration.

It has been said that inductive and deductive reasoning are contrary in their scheme; that in Deduction we infer particular from general truths; while in Induction we infer general from particular: that Deduction consists of many steps, in each of which we apply known general propositions in particular cases; while in Induction we have a single step, in which we pass from many particular truths to one general proposition. And this is truly said; but though contrary in their motions, the two are the operation of the same mind travelling over the same ground. Deduction is a necessary part of Induction. Deduction justifies by calculation what Induction had happily guessed. Induction recognizes the ore of truth by its weight; Deduction confirms the recognition by chemical analysis. Every step of Induction must be confirmed by rigorous deductive reasoning, followed into such detail as the nature and complexity of the relations (whether of quantity or any other) render requisite. If not so justified by the supposed discoverer, it is *not* Induction.

19. Such Tabular arrangements of propositions as we have constructed may be considered as the *Criterion of Truth* for the doctrines which they include. They are the Criterion of Inductive Truth, in the same sense in which Syllogistic Demonstration is the Criterion of Necessary Truth,—of the certainty of conclusions, depending upon evident First Principles. And that such Tables are really a Criterion of the truth of the propositions which they contain, will be plain by examining their structure. For if the connexion which the inductive process assumes be ascertained to be in each case real and true, the assertion of the general proposition merely collects together ascertained truths; and in like manner each of those more particular propositions is true, because it merely expresses collectively more special facts: so that the most general theory is only the assertion of a great body of facts, duly classified and subordinated. When we

assert the truth of the Copernican theory of the motions of the solar system, or of the Newtonian theory of the forces by which they are caused, we merely assert the groups of propositions which, in the Table of Astronomical Induction, are included in these doctrines; and ultimately, we may consider ourselves as merely asserting at once so many Facts, and therefore, of course, expressing an indisputable truth.

20. At any one of these steps of Induction in the Table, the inductive proposition is a *Theory* with regard to the Facts which it includes, while it is to be looked upon as a *Fact* with respect to the higher generalizations in which it is included. In any other sense, as was formerly shown, the opposition of *Fact* and *Theory* is untenable, and leads to endless perplexity and debate. Is it a Fact or a Theory that the planet Mars revolves in an Ellipse about the Sun? To Kepler, employed in endeavouring to combine the separate observations by the Conception of an Ellipse, it is a Theory; to Newton, engaged in inferring the law of force from a knowledge of the elliptical motion, it is a Fact. There are, as we have already seen, no special attributes of Theory and Fact which distinguish them from one another. Facts are phenomena apprehended by the aid of conceptions and mental acts, as Theories also are. We commonly call our observations *Facts*, when we apply, without effort or consciousness, conceptions perfectly familiar to us: while we speak of Theories, when we have previously contemplated the Facts and the connecting Conception separately, and have made the connexion by a conscious mental act. The real difference is a difference of relation; as the same proposition in a demonstration is the *premiss* of one syllogism and the *conclusion* in another;—as the same person is a father and a son. Propositions are Facts and Theories, according as they stand above or below the Inductive Brackets of our Tables.

21. To obviate mistakes I may remark that the terms *higher* and *lower*, when used of generalizations, are unavoidably represented by their opposites in our Inductive Tables. The highest generalization is that

which includes all others; and this stands the lowest on our page, because, reading downwards, that is the place which we last reach.

There is a distinction of the knowledge acquired by Scientific Induction into two kinds, which is so important that we shall consider it in the succeeding chapter.

CHAPTER VII.

OF LAWS OF PHENOMENA AND OF CAUSES.

APHORISM XXIV.

Inductive truths are of two kinds, Laws of Phenomena, *and* Theories of Causes. *It is necessary to begin in every science with the Laws of Phenomena; but it is impossible that we should be satisfied to stop short of a Theory of Causes. In Physical Astronomy, Physical Optics, Geology, and other sciences, we have instances showing that we can make a great advance in inquiries after true Theories of Causes.*

1. IN the first attempts at acquiring an exact and connected knowledge of the appearances and operations which nature presents, men went no further than to learn *what* takes place, not *why* it occurs. They discovered an Order which the phenomena follow, Rules which they obey; but they did not come in sight of the Powers by which these rules are determined, the Causes of which this order is the effect. Thus, for example, they found that many of the celestial motions took place as if the sun and stars were carried round by the revolutions of certain celestial spheres; but what causes kept these spheres in constant motion, they were never able to explain. In like manner in modern times, Kepler discovered that the planets describe ellipses, before Newton explained why they select this particular curve, and describe it in a particular manner. The laws of reflection, refraction, dispersion, and other properties of light have long been known; the causes of these laws are at present under discussion. And the same might be

said of many other sciences. The discovery of *the Laws of Phenomena* is, in all cases, the first step in exact knowledge; these Laws may often for a long period constitute the whole of our science; and it is always a matter requiring great talents and great efforts, to advance to a knowledge of the *Causes* of the phenomena.

Hence the larger part of our knowledge of nature, at least of the certain portion of it, consists of the knowledge of the Laws of Phenomena. In Astronomy indeed, besides knowing the rules which guide the appearances, and resolving them into the real motions from which they arise, we can refer these motions to the forces which produce them. In Optics, we have become acquainted with a vast number of laws by which varied and beautiful phenomena are governed; and perhaps we may assume, since the evidence of the Undulatory Theory has been so fully developed, that we know also the Causes of the Phenomena. But in a large class of sciences, while we have learnt many Laws of Phenomena, the causes by which these are produced are still unknown or disputed. Are we to ascribe to the operation of a fluid or fluids, and if so, in what manner, the facts of heat, magnetism, electricity, galvanism? What are the forces by which the elements of chemical compounds are held together? What are the forces, of a higher order, as we cannot help believing, by which the course of vital action in organized bodies is kept up? In these and other cases, we have extensive departments of science; but we are as yet unable to trace the effects to their causes; and our science, so far as it is positive and certain, consists entirely of the laws of phenomena.

2. In those cases in which we have a division of the science which teaches us the doctrine of the causes, as well as one which states the rules which the effects follow, I have, in the *History*, distinguished the two portions of the science by certain terms. I have thus spoken of *Formal* Astronomy and *Physical* Astronomy. The latter phrase has long been commonly employed to describe that department of Astronomy which deals with

those forces by which the heavenly bodies are guided in their motions; the former adjective appears well suited to describe a collection of rules depending on those ideas of space, time, position, number, which are, as we have already said, the *forms* of our apprehension of phenomena. The laws of phenomena may be considered as *formulæ*, expressing results in terms of those ideas. In like manner, I have spoken of Formal Optics and Physical Optics; the latter division including all speculations concerning the machinery by which the effects are produced. Formal Acoustics and Physical Acoustics may be distinguished in like manner, although these two portions of science have been a good deal mixed together by most of those who have treated of them. Formal Thermotics, the knowledge of the laws of the phenomena of heat, ought in like manner to lead to Physical Thermotics, or the Theory of Heat with reference to the cause by which its effects are produced;—a branch of science which as yet can hardly be said to exist.

3. What *kinds of cause* are we to admit in science? This is an important, and by no means an easy question. In order to answer it, we must consider in what manner our progress in the knowledge of causes has hitherto been made. By far the most conspicuous instance of success in such researches, is the discovery of the causes of the motions of the heavenly bodies. In this case, after the formal laws of the motions,—their conditions as to space and time,—had become known, men were enabled to go a step further; to reduce them to the familiar and general cause of motion—mechanical force; and to determine the laws which this force follows. That this was a step in addition to the knowledge previously possessed, and that it was a real and peculiar truth, will not be contested. And a step in any other subject which should be analogous to this in astronomy;—a discovery of causes and forces as certain and clear as the discovery of universal gravitation;—would undoubtedly be a vast advance upon a body of science consisting only of the laws of phenomena.

4. But although physical astronomy may well be taken as a standard in estimating the value and magnitude of the advance from the knowledge of phenomena to the knowledge of causes; the peculiar features of the transition from formal to physical science in that subject must not be allowed to limit too narrowly our views of the nature of this transition in other cases. We are not, for example, to consider that the step which leads us to the knowledge of causes in any province of nature must necessarily consist in the discovery of centers of forces, and collections of such centers, by which the effects are produced. The discovery of the causes of phenomena may imply the detection of a fluid by whose undulations, or other operations, the results are occasioned. The phenomena of acoustics are, we know, produced in this manner by the air; and in the cases of light, heat, magnetism, and others, even if we reject all the theories of such fluids which have hitherto been proposed, we still cannot deny that such theories are intelligible and possible, as the discussions concerning them have shown. Nor can it be doubted that if the assumption of such a fluid, in any case, were as well evidenced as the doctrine of universal gravitation is, it must be considered as a highly valuable theory.

5. But again; not only must we, in aiming at the formation of a Causal Section in each Science of Phenomena, consider Fluids and their various modes of operation admissible, as well as centers of mechanical force; but we must be prepared, if it be necessary, to consider the forces, or powers to which we refer the phenomena, under still more general aspects, and invested with characters different from mere mechanical force. For example; the forces by which the chemical elements of bodies are bound together, and from which arise, both their sensible texture, their crystalline form, and their chemical composition, are certainly forces of a very different nature from the mere attraction of matter according to its mass. The powers of assimilation and reproduction in plants and animals are obviously still more removed from mere mechanism; yet

these powers are not on that account less real, nor a less fit and worthy subject of scientific inquiry.

6. In fact, these forces—mechanical, chemical and vital,—as we advance from one to the other, each bring into our consideration new characters; and what these characters are, has appeared in the historical survey which we made of the Fundamental Ideas of the various sciences. It was then shown that the forces by which chemical effects are produced necessarily involve the Idea of Polarity,—they are polar forces; the particles tend together in virtue of opposite properties which in the combination neutralize each other. Hence, in attempting to advance to a theory of Causes in chemistry, our task is by no means to invent laws of *mechanical* force, and collections of forces, by which the effects may be produced. We know beforehand that no such attempt can succeed. Our aim must be to conceive such new kinds of force, including Polarity among their characters, as may best render the results intelligible.

7. Thus in advancing to a Science of Cause in any subject, the labour and the struggle is, not to analyse the phenomena according to any preconceived and already familiar ideas, but to form distinctly new conceptions, such as do really carry us to a more intimate view of the processes of nature. Thus in the case of astronomy, the obstacle which deferred the discovery of the true causes from the time of Kepler to that of Newton, was the difficulty of taking hold of mechanical conceptions and axioms with sufficient clearness and steadiness; which, during the whole of that interval, mathematicians were learning to do. In the question of causation which now lies most immediately in the path of science, that of the causes of electrical and chemical phenomena, the business of rightly fixing and limiting the conception of polarity, is the proper object of the efforts of discoverers. Accordingly a large portion of Mr Faraday's recent labours[1] is directed, not to

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[1] Eleventh, Twelfth, and Thirteenth Series of Researches, *Phil. Trans.* 1837 and 8.

the attempt at discovering new laws of phenomena, but to the task of throwing light upon the conception of polarity, and of showing how it must be understood, so that it shall include electrical induction and other phenomena, which have commonly been ascribed to forces acting mechanically at a distance. He is by no means content, nor would it answer the ends of science that he should be, with stating the results of his experiments; he is constantly, in every page, pointing out the interpretation of his experiments, and showing how the conception of Polar Forces enters into this interpretation. 'I shall,' he says[a], 'use every opportunity which presents itself of returning to that strong test of truth, experiment; but,' he adds, 'I shall necessarily have occasion to speak theoretically, and even hypothetically.' His hypothesis that electrical inductive action always takes place by means of a continuous line of polarized particles, and not by attraction and repulsion at a distance, if established, cannot fail to be a great step on our way towards a knowledge of causes, as well as phenomena, in the subjects under his consideration.

8. The process of obtaining new conceptions is, to most minds, far more unwelcome than any labour in employing old ideas. The effort is indeed painful and oppressive; it is feeling in the dark for an object which we cannot find. Hence it is not surprising that we should far more willingly proceed to seek for new causes by applying conceptions borrowed from old ones. Men were familiar with solid frames, and with whirlpools of fluid, when they had not learnt to form any clear conception of attraction at a distance. Hence they at first imagined the heavenly motions to be caused by Crystalline Spheres, and by Vortices. At length they were taught to conceive Central Forces, and then they reduced the solar system to these. But having done this, they fancied that all the rest of the machinery of nature must be central forces. We find Newton

[a] Art. 1318.

expressing this conviction[3], and the mathematicians of the last century acted upon it very extensively. We may especially remark Laplace's labours in this field. Having explained, by such forces, the phenomena of capillary attraction, he attempted to apply the same kind of explanation to the reflection, refraction, and double refraction of light;—to the constitution of gases;—to the operation of heat. It was soon seen that the explanation of refraction was arbitrary, and that of double refraction illusory; while polarization entirely eluded the grasp of this machinery. Centers of force would no longer represent the modes of causation which belonged to the phenomena. Polarization required some other contrivance, such as the undulatory theory supplied. No theory of light can be of any avail in which the fundamental idea of Polarity is not clearly exhibited.

9. The sciences of magnetism and electricity have given rise to theories in which this relation of polarity is exhibited by means of two opposite fluids[4];—a positive and a negative fluid, or a vitreous and a resinous, for electricity, and a boreal and an austral fluid for magnetism. The hypothesis of such fluids gives results agreeing in a remarkable manner with the facts and their measures, as Coulomb and others have shown. It may be asked how far we may, in such a case, suppose that we have discovered the true cause of the phenomena, and whether it is sufficiently proved that these fluids really exist. The right answer seems to be, that the hypothesis certainly represents the truth so far as regards the polar relation of the two energies, and the laws of the attractive and repulsive forces of the particles in which these energies reside; but that we are not entitled to assume that the vehicles of these energies possess other attributes of material fluids, or that the forces thus ascribed to the particles are the primary elementary forces from which

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[3] Multa me movent, &c.,—Pref. to the *Principia*, already quoted in the *History*.

[4] *Hist. Ind. Sc.* b. xi. c. ii.

the action originates. We are the more bound to place this cautious limit to our acceptance of the Coulombian theory, since in electricity Faraday has in vain endeavoured to bring into view one of the polar fluids without the other: whereas such a result ought to be possible if there were two separable fluids. The impossibility of this separate exhibition of one fluid appears to show that the fluids are *real* only so far as they are *polar*. And Faraday's view above mentioned, according to which the attractions at a distance are resolved into the action of lines of polarized particles of air, appears still further to show that the conceptions hitherto entertained of electrical forces, according to the Coulombian theory, do not penetrate to the real and intimate nature of the causation belonging to this case.

10. Since it is thus difficult to know when we have seized the true cause of the phenomena in any department of science, it may appear to some persons that physical inquirers are imprudent and unphilosophical in undertaking this Research of Causes; and that it would be safer and wiser to confine ourselves to the investigation of the laws of phenomena, in which field the knowledge which we obtain is definite and certain. Hence there have not been wanting those who have laid it down as a maxim that 'science must study only the laws of phenomena, and never the mode of production[5].' But it is easy to see that such a maxim would confine the breadth and depth of scientific inquiries to a most scanty and miserable limit. Indeed, such a rule would defeat its own object; for the laws of phenomena, in many cases, cannot be even expressed or understood without some hypothesis respecting their mode of production. How could the phenomena of polarization have been conceived or reasoned upon, except by imagining a polar arrangement of particles, or transverse vibrations, or some equivalent hypothesis? The doctrines of fits of easy transmission, the doctrine of moveable polarization, and the like, even when

[5] Comte, *Philosophie Positive.*

erroneous as representing the whole of the phenomena, were still useful in combining some of them into laws; and without some such hypotheses the facts could not have been followed out. The doctrine of a fluid caloric may be false; but without imagining such a fluid, how could the movement of heat from one part of a body to another be conceived? It may be replied that Fourier, Laplace, Poisson, who have principally cultivated the Theory of Heat, have not conceived it as a fluid, but have referred conduction to the radiation of the molecules of bodies, which they suppose to be separate points. But this molecular constitution of bodies is itself an assumption of the mode in which the phenomena are produced; and the radiation of heat suggests inquiries concerning a fluid emanation, no less than its conduction does. In like manner, the attempts to connect the laws of phenomena of heat and of gases, have led to hypotheses respecting the constitution of gases, and the combination of their particles with those of caloric, which hypotheses may be false, but are probably the best means of discovering the truth.

To debar science from inquiries like these, on the ground that it is her business to inquire into facts, and not to speculate about causes, is a curious example of that barren caution which hopes for truth without daring to venture upon the quest of it. This temper would have stopped with Kepler's discoveries, and would have refused to go on with Newton to inquire into the mode in which the phenomena are produced. It would have stopped with Newton's optical facts, and would have refused to go on with him and his successors to inquire into the mode in which these phenomena are produced. And, as we have abundantly shown, it would, on that very account, have failed in seeing what the phenomena really are.

In many subjects the attempt to study the laws of phenomena, independently of any speculations respecting the causes which have produced them, is neither possible for human intelligence nor for human temper. Men cannot contemplate the phenomena without clothing them in terms of some hypothesis, and will not be schooled to suppress the questionings which at every moment rise up within them concerning the causes of the phenomena. Who can attend to the appearances which come under the notice of the geologist;—strata regularly bedded, full of the remains of animals such as now live in the depths of the ocean, raised to the tops of mountains, broken, contorted, mixed with rocks such as still flow from the mouths of volcanos;—who can see phenomena like these, and imagine that he best promotes the progress of our knowledge of the earth's history, by noting down the facts, and abstaining from all inquiry whether these are really proofs of past states of the earth and of subterraneous forces, or merely an accidental imitation of the effects of such causes? In this and similar cases, to proscribe the inquiry into causes would be to annihilate the science.

Finally, this caution does not even gain its own single end, the escape from hypotheses. For, as we have said, those who will not seek for new and appropriate causes of newly-studied phenomena, are almost inevitably led to ascribe the facts to modifications of causes already familiar. They may declare that they will not hear of such causes as vital powers, elective affinities, electric, or calorific, or luminiferous ethers or fluids; but they will not the less on that account assume hypotheses equally unauthorized;—for instance—universal mechanical forces; a molecular constitution of bodies; solid, hard, inert matter;—and will apply these hypotheses in a manner which is arbitrary in itself as well as quite insufficient for its purpose.

11. It appears, then, to be required, both by the analogy of the most successful efforts of science in past times and by the irrepressible speculative powers of the human mind, that we should attempt to discover both the *laws of phenomena*, and their *causes*. In every department of science, when prosecuted far enough, these two great steps of investigation must succeed each other. The laws of phenomena must be known before we can speculate concerning causes; the causes must be inquired into when the phenomena have been

reduced to rule. In both these speculations the suppositions and conceptions which occur must be constantly tested by reference to observation and experiment. In both we must, as far as possible, devise hypotheses which, when we thus test them, display those characters of truth of which we have already spoken;—an agreement with facts such as will stand the most patient and rigid inquiry; a provision for predicting truly the results of untried cases; a consilience of inductions from various classes of facts; and a progressive tendency of the scheme to simplicity and unity.

We shall attempt hereafter to give several rules of a more precise and detailed kind for the discovery of the causes, and still more, of the laws of phenomena. But it will be useful in the first place to point out the Classification of the Sciences which results from the principles already established in this word. And for this purpose we must previously decide the question, whether the practical Arts, as Medicine and Engineering, must be included in our list of Sciences.

CHAPTER VIII.

OF ART AND SCIENCE.

APHORISM XXV.

Art and Science differ. The object of Science is Knowledge; the objects of Art, are Works. In Art, truth is a means to an end; in Science, it is the only end. Hence the Practical Arts are not to be classed among the Sciences.

APHORISM XXVI.

Practical Knowledge, such as Art implies, is not Knowledge such as Science includes. Brute animals have a practical knowledge of relations of space and force; but they have no knowledge of Geometry or Mechanics.

1. THE distinction of Arts and Sciences very materially affects all classifications of the departments of Human Knowledge. It is often maintained, expressly or tacitly, that the Arts are a part of our knowledge, in the same sense in which the Sciences are so; and that Art is the application of Science to the purposes of practical life. It will be found that these views require some correction, when we understand *Science* in the exact sense in which we have throughout endeavoured to contemplate it, and in which alone our examination of its nature can instruct us in the true foundations of our knowledge.

When we cast our eyes upon the early stages of the histories of nations, we cannot fail to be struck with the consideration, that in many countries the Arts of life already appear, at least in some rude form or other, when, as yet, nothing of science exists. A practical knowledge of Astronomy, such as enables them to reckon months and years, is found among all nations except the mere savages. A practical knowledge of Mechanics must have existed in those nations which have left us the gigantic monuments of early architecture. The pyramids and temples of Egypt and Nubia, the Cyclopean walls of Italy and Greece, the temples of Magna Græcia and Sicily, the obelisks and edifices of India, the cromlechs and Druidical circles of countries formerly Celtic,—must have demanded no small practical mechanical skill and power. Yet those modes of reckoning time must have preceded the rise of speculative Astronomy; these structures must have been erected before the theory of Mechanics was known. To suppose, as some have done, a great body of science, now lost, to have existed in the remote ages to which these remains belong, is not only quite gratuitous and contrary to all analogy, but is a supposition which cannot be extended so far as to explain all such cases. For it is impossible to imagine that *every* art has been preceded by the science which renders a reason for its processes. Certainly men formed wine from the grape, before they possessed a Science of Fermentation; the first instructor of every artificer in brass and iron can hardly be supposed to have taught the Chemistry of metals as a Science; the inventor of the square and the compasses had probably no more knowledge of demonstrated Geometry than have the artisans who now use those implements; and finally, the use of speech, the employment of the inflections and combinations of words, must needs be assumed as having been prior to any general view of the nature and analogy of Language. Even at this moment, the greater part of the arts which exist in the world are not accompanied by the sciences on which they theoretically depend. Who shall state to us the general chemical truths to which the manufactures of glass, and porcelain, and iron, and brass, owe their existence? Do not almost all artisans practise many successful artifices long before science explains the ground of the process? Do not arts at this day exist, in a high state of perfection, in countries in which there is no science, as China and India? These countries and many others have no theories of mechanics, of optics, of chemistry, of physiology; yet they construct and use mechanical and optical instruments, make chemical combinations, take advantage of physiological laws. It is too evident to need further illustration that Art may exist without Science;—that the former has usually been anterior to the latter, and even now commonly advances independently, leaving science to follow as it can.

2. We here mean by *Science*, that exact, general, speculative knowledge, of which we have, throughout this work, been endeavouring to exhibit the nature and rules. Between such Science and the *practical Arts* of life, the points of difference are sufficiently manifest. The object of Science is *Knowledge;* the object of Art are *Works*. The latter is satisfied with producing its material results; to the former, the operations of matter, whether natural or artificial, are interesting only so far as they can be embraced by intelligible principles. The End of Art is the Beginning of Science; for when it is seen *what* is done, then comes the question *why* it is done. Art may have fixed general rules, stated in words; but she has these merely as means to an end: to Science, the propositions which she obtains are each, in itself, a sufficient end of the effort by which it is acquired. When Art has brought forth her product, her task is finished; Science is constantly led by one step of her path to another: each proposition which she obtains impels her to go onwards to other propositions more general, more profound, more simple. Art puts elements together, without caring to know what they are, or why they coalesce. Science analyses the compound, and at every such step strives not only to perform, but to understand the analysis. Art advances in proportion as she becomes able to bring forth products more multiplied, more complex, more various; but Science, straining her eyes to penetrate more and more deeply into the nature of things, reckons her success in proportion as she sees, in all the phenomena, however

multiplied, complex, and varied, the results of one or two simple and general laws.

3. There are many acts which man, as well as animals, performs by the guidance of nature, without seeing or seeking the reason why he does so; as, the acts by which he balances himself in standing or moving, and those by which he judges of the form and position of the objects around him. These actions have their reason in the principles of geometry and mechanics; but of such reasons he who thus acts is unaware: he works blindly, under the impulse of an unknown principle which we call *Instinct.* When man's speculative nature seeks and finds the reasons why he should act thus or thus;—why he should stretch out his arm to prevent his falling, or assign a certain position to an object in consequence of the angles under which it is seen;—he may perform the same actions as before, but they are then done by the aid of a different faculty, which, for the sake of distinction, we may call *Insight.* Instinct is a purely active principle; it is seen in deeds alone; it has no power of looking inwards; it asks no questions; it has no tendency to discover reasons or rules; it is the opposite of Insight.

4. Art is not identical with Instinct: on the contrary, there are broad differences. Instinct is stationary; Art is progressive. Instinct is mute; it acts, but gives no rules for acting: Art can speak; she can lay down rules. But though Art is thus separate from Instinct, she is not essentially combined with Insight. She can see what to do, but she needs not to see why it is done. She may lay down Rules, but it is not her business to give Reasons. When man makes *that* his employment, he enters upon the domain of Science. Art takes the phenomena and laws of nature as she finds them: that they are multiplied, complex, capricious, incoherent, disturbs her not. She is content that the rules of nature's operations should be perfectly arbitrary and unintelligible, provided they are constant, so that she can depend upon their effects. But Science is impatient of all appearance of caprice,

inconsistency, irregularity, in nature. She will not believe in the existence of such characters. She resolves one apparent anomaly after another; her task is not ended till every thing is so plain and simple, that she is tempted to believe that she sees that it could by no possibility have been otherwise than it is.

5. It may be said that, after all, Art does really involve the knowledge which Science delivers;—that the artisan who raises large weights, practically *knows* the properties of the mechanical powers;—that he who manufactures chemical compounds is virtually acquainted with the laws of chemical combination. To this we reply, that it might on the same grounds be asserted, that he who acts upon the principle that two sides of a triangle are greater than the third is really acquainted with geometry; and that he who balances himself on one foot knows the properties of the center of gravity. But this is an acquaintance with geometry and mechanics which even brute animals possess. It is evident that it is not of such knowledge as this that we have here to treat. It is plain that this mode of possessing principles is altogether different from that contemplation of them on which science is founded. We neglect the most essential and manifest differences, if we confound our unconscious assumptions with our demonstrative reasonings.

6. The real state of the case is, that the principles which Art *involves*, Science alone *evolves*. The truths on which the success of Art depends, lurk in the artist's mind in an undeveloped state; guiding his hand, stimulating his invention, balancing his judgment, but not appearing in the form of enunciated Propositions. Principles are not to him direct objects of meditation: they are secret Powers of Nature, to which the forms which tenant the world owe their constancy, their movements, their changes, their luxuriant and varied growth, but which he can nowhere directly contemplate. That the creative and directive Principles which have their lodgment in the artist's mind, when *unfolded* by our speculative powers into

systematic shape, become Science, is true; but it is precisely this process of *development* which gives to them their character of Science. In practical Art, principles are unseen guides, leading us by invisible strings through paths where the end alone is looked at: it is for Science to direct and purge our vision so that these airy ties, these principles and laws, generalizations and theories, become distinct objects of vision. Many may feel the intellectual monitor, but it is only to her favourite heroes that the Goddess of Wisdom visibly reveals herself.

7. Thus Art, in its earlier stages at least, is widely different from Science, is independent of it, and is anterior to it. At a later period, no doubt, Art may borrow aid from Science; and the discoveries of the philosopher may be of great value to the manufacturer and the artist. But even then, this application forms no essential part of the science: the interest which belongs to it is not an intellectual interest. The augmentation of human power and convenience may impel or reward the physical philosopher; but the processes by which man's repasts are rendered more delicious, his journeys more rapid, his weapons more terrible, are not, therefore, Science. They may involve principles which are of the highest interest to science; but as the advantage is not practically more precious because it results from a beautiful theory, so the theoretical principle has no more conspicuous place in science because it leads to convenient practical consequences. The nature of Science is purely intellectual; Knowledge alone,—exact general Truth,—is her object; and we cannot mix with such materials, as matters of the same kind, the merely empirical maxims of Art, without introducing endless confusion into the subject, and making it impossible to attain any solid footing in our philosophy.

8. I shall therefore not place, in our Classification of the Sciences, the Arts, as has generally been done; nor shall I notice the applications of sciences to art, as forming any separate portion of each science. The sciences, considered as bodies of general speculative

truths, are what we are here concerned with; and applications of such truths, whether useful or useless, are important to us only as illustrations and examples. Whatever place in human knowledge the Practical Arts may hold, they are not Sciences. And it is only by this rigorous separation of the Practical from the Theoretical, that we can arrive at any solid conclusions respecting the nature of Truth, and the mode of arriving at it, such as it is our object to attain.

CHAPTER IX.

OF THE CLASSIFICATION OF SCIENCES.

1. THE Classification of Sciences has its chief use in pointing out to us the extent of our powers of arriving at truth, and the analogies which may obtain between those certain and lucid portions of knowledge with which we are here concerned, and those other portions, of a very different interest and evidence, which we here purposely abstain to touch upon. The classification of human knowledge will, therefore, have a more peculiar importance when we can include in it the moral, political, and metaphysical, as well as the physical portions of our knowledge. But such a survey does not belong to our present undertaking: and a general view of the connexion and order of the branches of sciences which our review has hitherto included, will even now possess some interest; and may serve hereafter as an introduction to a more complete scheme of the general body of human knowledge.

2. In this, as in any other case, a sound classification must be the result, not of any assumed principles imperatively applied to the subject, but of an examination of the objects to be classified;—of an analysis of them into the principles in which they agree and differ. The Classification of Sciences must result from the consideration of their nature and contents. Accordingly, that review of the Sciences in which the *History* of the Sciences engaged us, led to a Classification, of which the main features are indicated in that work. The Classification thus obtained, depends neither upon the faculties of the mind to which the separate parts of our knowledge owe their origin, nor upon the objects which each science contemplates; but upon a more

natural and fundamental element;—namely, the *Ideas* which each science involves. The Ideas regulate and connect the facts, and are the foundations of the reasoning, in each science: and having in another work more fully examined these *Ideas*, we are now prepared to state here the classification to which they lead. If we have rightly traced each science to the Conceptions which are really fundamental *with regard to it*, and which give rise to the first principles on which it depends, it is not necessary for our purpose that we should decide whether these Conceptions are absolutely ultimate principles of thought, or whether, on the contrary, they can be further resolved into other Fundamental Ideas. We need not now suppose it determined whether or not *Number* is a mere modification of the Idea of Time, and *Force* a mere modification of the Idea of Cause: for however this may be, our Conception of Number is the foundation of Arithmetic, and our Conception of Force is the foundation of Mechanics. It is to be observed also that in our classification, each Science may involve, not only the Ideas or Conceptions which are placed opposite to it in the list, but also all which *precede* it. Thus Formal Astronomy involves not only the Conception of Motion, but also those which are the foundation of Arithmetic and Geometry. In like manner, Physical Astronomy employs the Sciences of Statics and Dynamics, and thus, rests on their foundations; and they, in turn, depend upon the Ideas of Space and of Time, as well as of Cause.

3. We may further observe, that this arrangement of Sciences according to the Fundamental Ideas which they involve, points out the transition from those parts of human knowledge which have been included in our History and Philosophy, to other regions of speculation into which we have not entered. We have repeatedly found ourselves upon the borders of inquiries of a psychological, or moral, or theological nature. Thus the History of Physiology[1] led us to the consideration

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

of Life, Sensation, and Volition; and at these Ideas we stopped, that we might not transgress the boundaries of our subject as then predetermined. It is plain that the pursuit of such conceptions and their consequences, would lead us to the sciences (if we are allowed to call them sciences) which contemplate not only animal, but human principles of action, to Anthropology, and Psychology. In other ways, too, the Ideas which we have examined, although manifestly the foundations of sciences such as we have here treated of, also plainly pointed to speculations of a different order; thus the Idea of a Final Cause is an indispensable guide in Biology, as we have seen; but the conception of Design as directing the order of nature, once admitted, soon carries us to higher contemplations. Again, the Class of Palætiological Sciences which we were in the *History* led to construct, although we there admitted only one example of the Class, namely Geology, does in reality include many vast lines of research; as the history and causes of the diffusion of plants and animals, the history of languages, arts, and consequently of civilization. Along with these researches, comes the question how far these histories point backwards to a natural or a supernatural origin; and the Idea of a First Cause is thus brought under our donsideration. Finally, it is not difficult to see that as the Physical Sciences have their peculiar governing Ideas, which support and shape them, so the Moral and Political Sciences also must similarly have their fundamental and formative Ideas, the source of universal and certain truths, each of their proper kind. But to follow out the traces of this analogy, and to verify the existence of those Fundamental Ideas in Morals and Politics, is a task quite out of the sphere of the work in which we are here engaged.

4. We may now place before the reader our Classification of the Sciences. I have added to the list of Sciences, a few not belonging to our present subject, that the nature of the transition by which we are to extend our philosophy into a wider and higher region may be in some measure perceived.

The Classification of the Sciences is given over leaf.

A few remarks upon it offer themselves.

The *Pure* Mathematical Sciences can hardly be called *Inductive* Sciences. Their principles are not obtained by Induction from Facts, but are necessarily assumed in reasoning upon the subject matter which those sciences involve.

The Astronomy of the Ancients aimed only at explaining the motions of the heavenly bodies, as a *mechanism*. Modern Astronomy explains these motions on the principles of Mechanics.

The term *Physics*, when confined to a peculiar class of Sciences, is usually understood to exclude the Mechanical Sciences on the one side, and Chemistry on the other; and thus embraces the Secondary Mechanical and Analytico-Mechanical Sciences. But the adjective *Physical* applied to any science and opposed to *Formal*, as in Astronomy and Optics, implies those speculations in which we consider not only the Laws of Phenomena but their Causes; and generally, as in those cases, their Mechanical Causes.

The term *Metaphysics* is applied to subjects in which the Facts examined are emotions, thoughts and mental conditions; subjects not included in our present survey.

Fundamental Ideas or Conceptions.	Sciences.	Classification.
Space	Geometry	Pure Mathematical Sciences.
Time		
Number	Arithmetic	
Sign	Algebra	
Limit	Differentials	
Motion	Pure Mechanism	Pure Motional Sciences.
	Formal Astronomy	
Cause		
Force	Statics	Mechanical Sciences.
Matter	Dynamics	
Inertia	Hydrostatics	
Fluid Pressure	Hydrodynamics	
	Physical Astronomy	
Outness		
Medium *of Sensation*	Acoustics	Secondary Mechanical Sciences. (*Physics.*)
Intensity *of Qualities*	Formal Optics	
Scales of Qualities	Physical Optics	
	Thermotics	
	Atmology	
Polarity	Electricity	Analytico-Mechanical Sciences. (*Physics.*)
	Magnetism	
	Galvanism	
Element (*Composition*)		
Chemical Affinity		
Substance (*Atoms*)	Chemistry	Analytical Science.
Symmetry	Crystallography	Analytico-Classificatory Sciences.
Likeness	Systematic Mineralogy	
Degrees of Likeness	Systematic Botany	Classificatory Sciences.
	Systematic Zoology	
Natural Affinity	Comparative Anatomy	
(*Vital Powers*)		
Assimilation		
Irritability		
(*Organization*)	Biology	Organical Sciences.
Final Cause		
Instinct		
Emotion	Psychology	(*Metaphysics.*)
Thought		
Historical Causation	Geology	Palætiological Sciences.
	Distribution of Plants and Animals	
	Glossology	
	Ethnography	
First Cause	Natural Theology.	

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BOOK III.

OF METHODS EMPLOYED IN THE FORMATION OF SCIENCE.

CHAPTER I.

Introduction.

Aphorism XXVII.

The Methods by which the construction of Science is promoted are, Methods of Observation, Methods of obtaining clear Ideas, *and* Methods of Induction.

1. IN the preceding Book, we pointed out certain general Characters of scientific knowledge which may often serve to distinguish it from opinions of a looser or vaguer kind. In the course of the progress of knowledge from the earliest to the present time, men have been led to a perception, more or less clear, of these characteristics. Various philosophers, from Plato and Aristotle in the ancient world, to Richard de Saint Victor and Roger Bacon in the middle ages, Galileo and Gilbert, Francis Bacon and Isaac Newton, in modern times, were led to offer precepts and maxims, as fitted to guide us to a real and fundamental knowledge of nature. It may on another occasion be our business to estimate the value of these precepts and maxims. And other contributions of the same kind to the philosophy of science might be noticed, and some which

contain still more valuable suggestions, and indicate a more practical acquaintance with the subject. Among these, I must especially distinguish Sir John Herschel's *Discourse on the Study of Natural Philosophy*. But my object at present is not to relate the history, but to present the really valuable results of preceding labours: and I shall endeavour to collect, both from them and from my own researches and reflections, such views and such rules as seem best adapted to assist us in the discovery and recognition of scientific truth; or, at least, such as may enable us to understand the process by which this truth is obtained. I would present to the reader the Philosophy and, if possible, the Art of Discovery.

2. But, in truth, we must acknowledge, before we proceed with this subject, that, speaking with strictness, an *Art of Discovery* is not possible;—that we can give no Rules for the pursuit of truth which shall be universally and peremptorily applicable;—and that the helps which we can offer to the inquirer in such cases are limited and precarious. Still, we trust it will be found that aids may be pointed out which are neither worthless nor uninstructive. The mere classification of examples of successful inquiry, to which our rules give occasion, is full of interest for the philosophical speculator. And if our maxims direct the discoverer to no operations which might not have occurred to his mind of themselves, they may still concentrate our attention on that which is most important and characteristic in these operations, and may direct us to the best mode of insuring their success. I shall, therefore, attempt to resolve the Process of Discovery into its parts, and to give an account as distinct as may be of Rules and Methods which belong to each portion of the process.

3. In Book II. we considered the three main parts of the process by which science is constructed: namely, the Decomposition and Observation of Complex Facts; the Explication of our Ideal Conceptions; and the Colligation of Elementary Facts by means of those Conceptions. The first and last of

these three steps are capable of receiving additional accuracy by peculiar processes. They may further the advance of science in a more effectual manner, when directed by special technical *Methods*, of which in the present Book we must give a brief view. In this more technical form, the observation of facts involves the *Measurement of Phenomena;* and the Colligation of Facts includes all arts and rules by which the process of Induction can be assisted. Hence we shall have here to consider *Methods of Observation*, and *Methods of Induction*, using these phrases in the widest sense. The second of the three steps above mentioned, the Explication of our Conceptions, does not admit of being much assisted by methods, although something may be done by Education and Discussion.

4. The Methods of Induction, of which we have to speak, apply only to the first step in our ascent from phenomena to laws of nature;—the discovery of *Laws of Phenomena*. A higher and ulterior step remains behind, and follows in natural order the discovery of Laws of Phenomena; namely, the *Discovery of Causes;* and this must be stated as a distinct and essential process in a complete view of the course of science. Again, when we have thus ascended to the causes of phenomena and of their laws, we can often reason downwards from the cause so discovered; and we are thus led to suggestions of new phenomena, or to new explanations of phenomena already known. Such proceedings may be termed *Applications* of our Discoveries; including in the phrase, *Verifications* of our Doctrines by such an application of them to observed facts. Hence we have the following series of processes concerned in the formation of science.

(1.) Decomposition of Facts;
(2.) Measurement of Phenomena;
(3.) Explication of Conceptions;
(4.) Induction of Laws of Phenomena;
(5.) Induction of Causes;
(6.) Application of Inductive Discoveries.

5. Of these six processes, the methods by which the second and fourth may be assisted are here our

peculiar object of attention. The treatment of these subjects in the present work must necessarily be scanty and imperfect, although we may perhaps be able to add something to what has hitherto been systematically taught on these heads. Methods of Observation and of Induction might of themselves form an abundant subject for a treatise, and hereafter probably will do so, in the hands of future writers. A few remarks, offered as contributions to this subject, may serve to show how extensive it is, and how much more ready it now is than it ever before was, for a systematic discussion.

Of the above steps of the formation of science, the first, the Decomposition of Facts, has already been sufficiently explained in the last Book: for if we pursue it into further detail and exactitude, we find that we gradually trench upon some of the succeeding parts. I, therefore, proceed to treat of the second step, the Measurement of Phenomena;—of *Methods* by which this work, in its widest sense, is executed, and these I shall term Methods of Observation.

CHAPTER II.

Of Methods of Observation.

Aphorism XXVIII.

The Methods of Observation of Quantity in general are, Numeration, *which is precise by the nature of Number; the* Measurement of Space *and* of Time, *which are easily made precise; the* Conversion cf Space and Time, *by which each aids the measurement of the other; the* Method of Repetition; *the* Method of Coincidences *or* Interferences. *The measurement of Weight is made precise by the* Method of Double-weighing. *Secondary Qualities are measured by means of* Scales of Degrees; *but in order to apply these Scales, the student requires the* Education of the Senses. *The Education of the Senses is forwarded by the practical study of* Descriptive Natural History, Chemical Manipulation, *and* Astronomical Observation.

1. I SHALL speak, in this chapter, of Methods of exact and systematic observation, by which such facts are collected as form the materials of precise scientific propositions. These Methods are very various, according to the nature of the subject inquired into, and other circumstances: but a great portion of them agree in being processes of measurement. These I shall peculiarly consider: and in the first place those referring to Number, Space, and Time, which are at the same time objects and instruments of measurement.

2. But though we have to explain how observations may be made as perfect as possible, we must not forget that in most cases complete perfection is unattainable. *Observations are never perfect.* For we

observe phenomena by our senses, and measure their relations in time and space; but our senses and our measures are all, from various causes, inaccurate. If we have to observe the exact place of the moon among the stars, how much of instrumental apparatus is necessary! This apparatus has been improved by many successive generations of astronomers, yet it is still far from being perfect. And the senses of man, as well as his implements, are limited in their exactness. Two different observers do not obtain precisely the same measures of the time and place of a phenomenon; as, for instance, of the moment at which the moon occults a star, and the point of her *limb* at which the occultation takes place. Here, then, is a source of inaccuracy and errour, even in astronomy, where the means of exact observation are incomparably more complete than they are in any other department of human research. In other cases, the task of obtaining accurate measures is far more difficult. If we have to observe the tides of the ocean when rippled with waves, we can see the average level of the water first rise and then fall; but how hard is it to select the exact moment when it is at its greatest height, or the exact highest point which it reaches! It is very easy, in such a case, to err by many minutes in time, and by several inches in space.

Still, in many cases, good Methods can remove very much of this inaccuracy, and to these we now proceed.

3. (I.) *Number.*—Number is the first step of measurement, since it measures itself, and does not, like space and time, require an arbitrary standard. Hence the first exact observations, and the first advances of rigorous knowledge, appear to have been made by means of number; as for example,—the number of days in a month and in a year;—the cycles according to which eclipses occur;—the number of days in the revolutions of the planets; and the like. All these discoveries, as we have seen in the History of Astronomy, go back to the earliest period of the science, anterior to any distinct tradition; and these discoveries presuppose a series, probably a very long series, of observations, made prin-

cipally by means of number. Nations so rude as to have no other means of exact measurement, have still systems of numeration by which they can reckon to a considerable extent. Very often, such nations have very complex systems, which are capable of expressing numbers of great magnitude. Number supplies the means of measuring other quantities, by the assumption of a *unit* of measure of the appropriate kind: but where nature supplies the unit, number is applicable directly and immediately. Number is an important element in the Classificatory as well as in the Mathematical Sciences. The History of those Sciences shows how the formation of botanical systems was effected by the adoption of number as a leading element, by Cæsalpinus; and how afterwards the Reform of Linnæus in classification depended in a great degree on his finding, in the pistils and stamens, a better numerical basis than those before employed. In like manner, the number of rays in the membrane of the gills[1], and the number of rays in the fins of fish, were found to be important elements in ichthyological classification by Artedi and Linnæus. There are innumerable instances, in all parts of Natural History, of the importance of the observation of number. And in this observation, no instrument, scale or standard is needed, or can be applied; except the scale of natural numbers, expressed either in words or in figures, can be considered as an instrument.

4. (II.) *Measurement of Space.*—Of quantities admitting of *continuous* increase and decrease, (for number is discontinuous,) space is the most simple in its mode of measurement, and requires most frequently to be measured. The obvious mode of measuring space is by the repeated application of a material measure, as when we take a foot-rule and measure the length of a room. And in this case the foot-rule is the *unit* of space, and the length of the room is expressed by the number of such units which it contains: or, as it may not contain an exact number, by a number with a *fraction*. But besides this measurement of linear space,

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[1] *Hist. Ind. Sc.* b. xvi. c. vii.

there is another kind of space which, for purposes of science, it is still more important to measure, namely, angular space. The visible heavens being considered as a sphere, the portions and paths of the heavenly bodies are determined by drawing circles on the surface of this sphere, and are expressed by means of the parts of these circles thus intercepted: by such measures the doctrines of astronomy were obtained in the very beginning of the science. The arcs of circles thus measured, are not like linear spaces, reckoned by means of an *arbitrary* unit; for there is a *natural unit*, the total circumference, to which all arcs may be referred. For the sake of convenience, the whole circumference is divided into 360 parts or *degrees;* and by means of these degrees and their parts, all arcs are expressed. The *arcs* are the measures of the *angles at the center*, and the degrees may be considered indifferently as measuring the one or the other of these quantities.

5. In the History of Astronomy[2], I have described the method of observation of celestial angles employed by the Greeks. They determined the lines in which the heavenly bodies were seen, by means either of Shadows, or of Sights; and measured the angles between such lines by arcs or rules properly applied to them. The Armill, Astrolabe, Dioptra, and Parallactic Instrument of the ancients, were some of the instruments thus constructed. Tycho Brahe greatly improved the methods of astronomical observation by giving steadiness to the frame of his instruments, (which were large *quadrants*,) and accuracy to the divisions of the *limb*[3]. But the application of the *telescope* to the astronomical quadrant and the fixation of the center of the field by a *cross* of fine wires placed in the focus, was an immense improvement of the instrument, since it substituted a precise visual ray, pointing to the star, instead of the coarse coincidence of Sights. The accuracy of observation was still further increased

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[2] *Hist. Ind. Sc.* b. iii. c. iv. sect. 3.

[3] *Ib.* b. vii. c. vi. sect. 1.

by applying to the telescope a *micrometer* which might subdivide the smaller divisions of the arc.

6. By this means, the precision of astronomical observation was made so great, that very minute angular spaces could be measured: and it then became a question whether discrepancies which appeared at first as defects in the theory, might not arise sometimes from a bending or shaking of the instrument, and from the degrees marked on the limb being really somewhat unequal, instead of being rigorously equal. Accordingly, the framing and balancing of the instrument, so as to avoid all possible tremor or flexure, and the exact division of an arc into equal parts, became great objects of those who wished to improve astronomical observations. The observer no longer gazed at the stars from a lofty tower, but placed his telescope on the solid ground, and braced and balanced it with various contrivances. Instead of a quadrant, an entire circle was introduced (by Ramsden;) and various processes were invented for the dividing of instruments. Among these we may notice Troughton's method of dividing; in which the visual ray of a microscope was substituted for the points of a pair of compasses, and, by *stepping* round the circle, the partial arcs were made to bear their exact relation to the whole circumference.

7. Astronomy is not the only science which depends on the measurement of angles. Crystallography also requires exact measures of this kind; and the *goniometer*, especially that devised by Wollaston, supplies the means of obtaining such measures. The science of Optics also, in many cases, requires the measurement of angles.

8. In the measurement of linear space, there is no natural standard which offers itself. Most of the common measures appear to be taken from some part of the human body; as a *foot*, a *cubit*, a *fathom;* but such measures cannot possess any precision, and are altered by convention: thus there were in ancient times many kinds of cubits; and in modern Europe, there are a great number of different standards of the foot, as the Rhenish foot, the Paris foot, the English foot. It is

very desirable that, if possible, some permanent standard, founded in nature, should be adopted; for the conventional measures are lost in the course of ages; and thus, dimensions expressed by means of them become unintelligible. Two different natural standards have been employed in modern times: the French have referred their measures of length to the total circumference of a meridian of the earth; a quadrant of this meridian consists of ten million units or *metres*. The English have fixed their linear measure by reference to the length of a pendulum which employs an exact second of time in its small oscillation. Both these methods occasion considerable difficulties in carrying them into effect; and are to be considered mainly as means of recovering the standard if it should ever be lost. For common purposes, some material standard is adopted as authority for the time: for example, the standard which in England possessed legal authority up to the year 1835 was preserved in the House of Parliament; and was lost in the conflagration which destroyed that edifice. The standard of length now generally referred to by men of science in England is that which is in the possession of the Astronomical Society of London.

9. A standard of length being established, the artifices for applying it, and for subdividing it in the most accurate manner, are nearly the same as in the case of measures of arcs: as for instance, the employment of the visual rays of microscopes instead of the legs of compasses and the edges of rules; the use of micrometers for minute measurements; and the like. Many different modes of avoiding errour in such measurements have been devised by various observers, according to the nature of the cases with which they had to deal[4].

10. (III.) *Measurement of Time.*—The methods of measuring Time are not so obvious as the methods of

[4] On the precautions employed in astronomical instruments for the measure of space, see Sir J. Herschel's *Astronomy*, (in the *Cabinet Cyclopædia*,) Arts. 103—110.

measuring space; for we cannot apply one portion of time to another, so as to test their equality. We are obliged to begin by assuming some change as the measure of time. Thus the motion of the sun in the sky, or the length and position of the shadows of objects, were the first modes of measuring the parts of the day. But what assurance had men, or what assurance could they have, that the motion of the sun or of the shadow was uniform? They could have no such assurance, till they had adopted some measure of smaller times; which smaller times, making up larger times by repetition, they took as the standard of uniformity;—for example, an hour-glass, or a clepsydra which answered the same purpose among the ancients. There is no apparent reason why the successive periods measured by the emptying of the hour-glass should be unequal; they are implicitly accepted as equal; and by reference to these, the uniformity of the sun's motion may be verified. But the great improvement in the measurement of time was the use of a pendulum for the purpose by Galileo, and the application of this device to clocks by Huyghens in 1656. For the successive oscillations of a pendulum are rigorously equal, and a clock is only a train of machinery employed for the purpose of counting these oscillations. By means of this invention, the measure of time in astronomical observations became as accurate as the measure of space.

11. What is the *natural unit* of time? It was assumed from the first by the Greek astronomers, that the sidereal days, measured by the revolution of a star from any meridian to the same meridian again, are exactly equal; and all improvements in the measure of time tended to confirm this assumption. The sidereal day is therefore the natural standard of time. But the solar day, determined by the diurnal revolution of the sun, although not rigorously invariable, as the sidereal day is, undergoes scarcely any perceptible variation; and since the course of daily occurrences is regulated by the sun, it is far more convenient to seek the basis of our unit of time in *his* motions. Accordingly the solar day (the *mean* solar day) is divided into 24 hours,

and these, into minutes and seconds; and this is our scale of time. Of such time, the sidereal day has 23 hours 56 minutes 4·09 seconds. And it is plain that by such a statement the length of the hour is fixed, with reference to a sidereal day. The *standard* of time (and the standard of space in like manner) equally answers its purpose, whether or not it coincides with any *whole number* of units.

12. Since the sidereal day is thus the standard of our measures of time, it becomes desirable to refer to it, constantly and exactly, the instruments by which time is measured, in order that we may secure ourselves against errour. For this purpose, in astronomical observatories, observations are constantly made of the transit of stars across the meridian; the *transit instrument* with which this is done being adjusted with all imaginable regard to accuracy[5].

13. When exact measures of time are required in other than astronomical observations, the same instruments are still used, namely, clocks and chronometers. In chronometers, the regulating part is an oscillating body; not, as in clocks, a pendulum oscillating by the force of gravity, but a wheel swinging to and fro on its center, in consequence of the vibrations of a slender coil of elastic wire. To divide time into still smaller portions than these vibrations, other artifices are used; some of which will be mentioned under the next head.

14. (IV.) *Conversion of Space and Time.*—Space and time agree in being extended quantities, which are made up and measured by the repetition of homogeneous parts. If a body move uniformly, whether in the way of revolving or otherwise, the *space* which any point describes, is *proportional* to the *time* of its motion; and the space and the time may each be taken as a measure of the other. Hence in such cases, by taking space instead of time, or time instead of

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[5] On the precautions employed in the measure of time by astronomers, see Herschel's *Astronomy*, Art. 115—127.

space, we may often obtain more convenient and precise measures, than we can by measuring directly the element with which we are concerned.

The most prominent example of such a conversion, is the measurement of the Right Ascension of stars, (that is, their angular distance from a standard meridian[6] on the celestial sphere,) by means of the time employed in their coming to the meridian of the place of observation. Since, as we have already stated, the visible celestial sphere, carrying the fixed stars, revolves with perfect uniformity about the pole; if we observe the stars as they come in succession to a fixed circle passing through the poles, the intervals of time between these observations will be proportional to the angles which the meridian circles passing through these stars make at the poles where they meet; and hence, if we have the means of measuring time with great accuracy, we can, by watching the *times* of the transits of successive stars across some visible mark in our own meridian, determine the *angular distances* of the meridian circles of all the stars from one another.

Accordingly, now that the pendulum clock affords astronomers the means of determining time exactly, a measurement of the Right Ascensions of heavenly bodies by means of a clock and a transit instrument, is a part of the regular business of an observatory. If the sidereal clock be so adjusted that it marks the beginning of its scale of time when the first point of Right Ascension is upon the visible meridian of our observatory, the point of the scale at which the clock points when any other star is in our meridian, will truly represent the Right Ascension of the star.

Thus as the motion of the stars is our measure of time, we employ time, conversely, as our measure of the places of the stars. The celestial machine and our terrestrial machines correspond to each other in their movements; and the star steals silently and steadily

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[6] A *meridian* is a circle passing through the poles about which the celestial sphere revolves. The meridian *of any place* on the earth is that meridian which is exactly over the place.

across our meridian line, just as the pointer of the clock steals past the mark of the hour. We may judge of the scale of this motion by considering that the full moon employs about two minutes of time in sailing across any fixed line seen against the sky, transverse to her path: and all the celestial bodies, carried along by the revolving sphere, travel at the same rate.

15. In this case, up to a certain degree, we render our measures of astronomical angles more exact and convenient by substituting time for space; but when, in the very same kind of observation, we wish to proceed to a greater degree of accuracy, we find that it is best done by substituting space for time. In observing the transit of a star across the meridian, if we have the clock within hearing, we can count the beats of the pendulum by the noise which they make, and tell exactly at which second of time the passage of the star across the visible thread takes place; and thus we measure Right Ascension by means of time. But our perception of time does not allow us to divide a second into ten parts, and to pronounce whether the transit takes place three-tenths, six-tenths, or seven-tenths of a second after the preceding beat of the clock. This, however, can be done by the usual mode of observing the transit of a star. The observer, listening to the beat of his clock, fastens his attention upon the star at each beat, and especially at the one immediately before and the one immediately after the passage of the thread: and by this means he has these two positions and the position of the thread so far present to his intuition at once, that he can judge in what proportion the thread is nearer to one position than the other, and can thus divide the intervening second in its due proportion. Thus if he observe that at the beginning of the second the star is on one side of the thread, and at the end of the second on the other side; and that the two distances from the thread are as two to three, he knows that the transit took place at two-fifths (or four-tenths) of a second after the former beat. In this way a second of time in astronomical observations may, by a skilful observer, be divided into ten equal

parts; although when time is observed as time, a tenth of a second appears almost to escape our senses. From the above explanation, it will be seen that the reason why the subdivision is possible in the way thus described, is this:—that the moment of time thus to be divided is so small, that the eye and the mind can retain, to the end of this moment, the impression of position which it received at the beginning. Though the two positions of the star, and the intermediate thread, are seen successively, they can be contemplated by the mind as if they were seen simultaneously: and thus it is precisely the smallness of this portion of time which enables us to subdivide it by means of space.

16. There is another case, of somewhat a different kind, in which time is employed in measuring space; namely, when space, or the standard of space, is defined by the length of a pendulum oscillating in a given time. We might in this way define any space by the time which a pendulum of such a length would take in oscillating; and thus we might speak, as was observed by those who suggested this device, of five minutes of cloth, or a rope half an hour long. We may observe, however, that in this case, the space is *not proportional* to the time. And we may add, that though we thus appear to avoid the arbitrary standard of space (for as we have seen, the standard of measures of time is a natural one,) we do not do so in fact: for we assume the invariableness of gravity, which really varies (though very slightly,) from place to place.

17. (V.) *The Method of Repetition in Measurement.*—In many cases we can give great additional accuracy to our measurements by repeatedly adding to itself the quantity which we wish to measure. Thus if we wished to ascertain the exact breadth of a thread, it might not be easy to determine whether it was one-ninetieth, or one-ninety-fifth, or one-hundredth part of an inch; but if we find that ninety-six such threads placed side by side occupy exactly an inch, we have the precise measure of the breadth of the thread. In

the same manner, if two clocks are going nearly at the same rate, we may not be able to distinguish the excess of an oscillation of one of the pendulums over an oscillation of the other: but when the two clocks have gone for an hour, one of them may have gained ten seconds upon the other; thus showing that the proportion of their times of oscillation is 3610 to 3600.

In the latter of these instances, we have the principle of repetition truly exemplified, because (as has been justly observed by Sir J. Herschel[7],) there is then 'a juxtaposition of units without errour,'—'one vibration commences exactly where the last terminates, no part of time being lost or gained in the addition of the units so counted.' In space, this juxtaposition of units without errour cannot be rigorously accomplished, since the units must be added together by material contact (as in the above case of the threads,) or in some equivalent manner. Yet the principle of repetition has been applied to angular measurement with considerable success in Borda's Repeating Circle. In this instrument, the angle between two objects which we have to observe, is repeated along the graduated limb of the circle by turning the telescope from one object to the other, alternately fastened to the circle (by its *clamp*) and loose from it (by unclamping). In this manner the errours of graduation may (theoretically) be entirely got rid of: for if an angle repeated *nine* times be found to go twice round the circle, it must be *exactly* eighty degrees: and where the repetition does not give an exact number of circumferences, it may still be made to subdivide the errour to any required extent.

18. Connected with the principle of repetition, is the *Method of coincidences* or *interferences*. If we have two Scales, on one of which an inch is divided into 10, and on the other into 11 equal parts; and if, these Scales being placed side by side, it appear that the beginning of the latter Scale is between the 2nd and 3rd division of the former, it may not be apparent

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7 *Disc. Nat. Phil.* art. 121.

what fraction added to 2 determines the place of beginning of the second Scale as measured on the first. But if it appear also that the 3rd division of the second Scale *coincides* with a certain division of the first, (the 5th,) it is certain that 2 and *three-tenths* is the *exact* place of the beginning of the second Scale, measured on the first Scale. The 3rd division of the 11 Scale will coincide (or interfere with) a division of the 10 Scale, when the beginning or *zero* of the 11 divisions is three-tenths of a division beyond the preceding line of the 10 Scale; as will be plain on a little consideration. And if we have two Scales of equal units, in which each unit is divided into nearly, but not quite, the same number of equal parts (as 10 and 11, 19 and 20, 29 and 30,) and one sliding on the other, it will always happen that some one or other of the division lines will coincide, or very nearly coincide; and thus the exact position of the beginning of one unit, measured on the other scale, is determined. A sliding scale, thus divided for the purpose of subdividing the units of that on which it slides, is called a *Vernier*, from the name of its inventor.

19. The same Principle of Coincidence or Interference is applied to the exact measurement of the length of time occupied in the oscillation of a pendulum. If a detached pendulum, of such a length as to swing in little less than a second, be placed before the seconds' pendulum of a clock, and if the two pendulums begin to move together, the former will gain upon the latter, and in a little while their motions will be quite discordant. But if we go on watching, we shall find them, after a time, to agree again exactly; namely, when the detached pendulum has gained one complete oscillation (back and forwards,) upon the clock pendulum, and again coincides with it in its motion. If this happen after 5 minutes, we know that the times of oscillation of the two pendulums are in the proportion of 300 to 302, and therefore the detached pendulum oscillates in $\frac{150}{151}$ of a second. The accuracy which can be obtained in the measure of an oscillation by this means is great; for the clock can be compared (by

observing transits of the stars or otherwise) with the natural standard of time, the sidereal day. And the moment of coincidence of the two pendulums may, by proper arrangements, be very exactly determined.

We have hitherto spoken of methods of measuring time and space, but other elements also may be very precisely measured by various means.

20. (VI.) *Measurement of Weight.*—Weight, like space and time, is a quantity made up by addition of parts, and may be measured by similar methods. The principle of repetition is applicable to the measurement of weight; for if two bodies be simultaneously put in the same pan of a balance, and if they balance pieces in the other pan, their weights are exactly added.

There may be difficulties of practical workmanship in carrying into effect the mathematical conditions of a perfect balance; for example, in securing an exact equality of the effective arms of the beam in all positions. These difficulties are evaded by the *Method of double weighing;* according to which the standard weights, and the body which is to be weighed, are successively put in the *same* pan, and made to balance by a third body in the opposite scale. By this means the different lengths of the arms of the beam, and other imperfections of the balance, become of no consequence[8].

21. There is no natural *Standard* of weight. The conventional weight taken as the standard, is the weight of a given bulk of some known substance; for instance, a *cubic foot of water.* But in order that this may be definite, the water must not contain any portion of heterogeneous substance: hence it is required that the water be *distilled* water.

22. (VII.) *Measurement of Secondary Qualities.*—We have already seen[9] that secondary qualities are estimated by means of conventional Scales, which refer

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[8] For other methods of measuring weights accurately, see Faraday's *Chemical Manipulation*, p. 25.

[9] B. iii. c. ii. Of the Measure of Secondary Qualities.

them to space, number, or some other definite expression. Thus the Thermometer measures heat; the Musical Scale, with or without the aid of number, expresses the pitch of a note; and we may have an exact and complete Scale of Colours, pure and impure. We may remark, however, that with regard to sound and colour, the estimates of the ear and the eye are not superseded, but only assisted: for if we determine what a note is, by comparing it with an instrument known to be in tune, we still leave the ear to decide when the note is *in unison* with one of the notes of the instrument. And when we compare a colour with our chromatometer, we judge by the eye which division of the chromatometer it *matches*. Colour and sound have their Natural Scales, which the eye and ear habitually apply; what science requires is, that those scales should be systematized. We have seen that several conditions are requisite in such scales of qualities: the observer's skill and ingenuity are mainly shown in devising such scales and methods of applying them.

23. The Method of Coincidences is employed in harmonics: for if two notes are nearly, but not quite, in unison, the coincidences of the vibrations produce an audible undulation in the note, which is called the *howl;* and the exactness of the unison is known by this howl vanishing.

24. (VIII.) *Manipulation.*—The process of applying practically methods of experiment and observation, is termed Manipulation; and the value of observations depends much upon the proficiency of the observer in this art. This skill appears, as we have said, not only in devising means and modes in measuring results, but also in inventing and executing arrangements by which elements are subjected to such conditions as the investigation requires: in finding and using some material combination by which nature shall be asked the question which we have in our minds. To do this in any subject may be considered as a peculiar Art, but especially in Chemistry; where 'many experiments, and even whole trains of research, are

essentially dependent for success on mere manipulation[10].' The changes which the chemist has to study,—compositions, decompositions, and mutual actions, affecting the internal structure rather than the external form and motion of bodies,—are not familiarly recognized by common observers, as those actions are which operate upon the total mass of a body: and hence it is only when the chemist has become, to a certain degree, familiar with his science, that he has the power of observing. He must learn to interpret the effects of mixture, heat, and other Chemical agencies, so as to see in them those facts which chemistry makes the basis of her doctrines. And in learning to interpret this language, he must also learn to call it forth;—to place bodies under the requisite conditions, by the apparatus of his own laboratory and the operations of his own fingers. To do this with readiness and precision, is, as we have said, an Art, both of the mind and of the hand, in no small degree recondite and difficult. A person may be well acquainted with all the doctrines of chemistry, and may yet fail in the simplest experiment. How many precautions and observances, what resource and invention, what delicacy and vigilance, are requisite in *Chemical Manipulation*, may be seen by reference to Dr. Faraday's work on that subject.

25. The same qualities in the observer are requisite in some other departments of science; for example, in the researches of Optics: for in these, after the first broad facts have been noticed, the remaining features of the phenomena are both very complex and very minute; and require both ingenuity in the invention of experiments, and a keen scrutiny of their results. We have instances of the application of these qualities in most of the optical experimenters of recent times, and certainly in no one more than Sir David Brewster. Omitting here all notice of his succeeding labours, his *Treatise on New Philosophical Instruments*, published in 1813, is an excellent model of the kind of resource

10 Faraday's *Chemical Manipulation*, p. 3.

and skill of which we now speak. I may mention as an example of this skill, his mode of determining the refractive power of an *irregular* fragment of any transparent substance. At first this might appear an impossible problem; for it would seem that a regular and smooth surface are requisite, in order that we may have any measurable refraction. But Sir David Brewster overcame the difficulty by immersing the fragment in a combination of fluids, so mixed, that they had the same refractive power as the specimen. The question, *when* they had this power, was answered by noticing when the fragment became so transparent that its surface could hardly be seen; for this happened when, the refractive power within and without the fragment being the same, there was no refraction at the surface. And this condition being obtained, the refractive power of the fluid, and therefore of the fragment, was easily ascertained.

26. (IX.) *The Education of the Senses.*—Colour and Musical Tone are, as we have seen, determined by means of the Senses, whether or not Systematical Scales are used in expressing the observed fact. Systematical Scales of sensible qualities, however, not only give precision to the record, but to the observation. But for this purpose such an Education of the Senses is requisite as may enable us to apply the scale immediately. The memory must retain the sensation or perception to which the technical term or degree of the scale refers. Thus with regard to colour, as we have said already[11], when we find such terms as *tin-white* or *pinchbeck-brown*, the metallic colour so denoted ought to occur at once to our recollection without delay or search. The observer's senses, therefore, must be educated, at first by an actual exhibition of the standard, and afterwards by a familiar use of it, to understand readily and clearly each phrase and degree of the scales which in his observations he has to apply. This is not only the best, but in many cases the only way in which the observation can be expressed. Thus *glassy lustre, fatty lustre, adamantine lustre,* denote certain kinds of

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11 B. viii. c. iii. Terminology.

shining in minerals, which appearances we should endeavour in vain to describe by periphrasis; and which the terms, if considered as terms in common language, would by no means clearly discriminate: for who, in common language, would say that coal has a fatty lustre? But these terms, in their conventional sense, are perfectly definite; and when the eye is once familiarized with this application of them, are easily and clearly intelligible.

27. The education of the senses, which is thus requisite in order to understand well the terminology of any science, must be acquired by an inspection of the objects which the science deals with; and is, perhaps, best promoted by the practical study of Natural History. In the different departments of Natural History, the descriptions of species are given by means of an extensive technical *terminology*: and that education of which we now speak, ought to produce the effect of making the observer as familiar with each of the terms of this terminology as we are with the words of our common language. The technical terms have a much more precise meaning than other terms, since they are defined by express convention, and not learnt by common usage merely. Yet though they are thus defined, not the definition, but the perception itself, is that which the term suggests to the proficient.

In order to use the terminology to any good purpose, the student must possess it, not as a dictionary, but as a language. The terminology of his sciences must be the natural historian's most familiar tongue. He must learn to think in such language. And when this is achieved, the terminology, as I have elsewhere said, though to an uneducated eye cumbrous and pedantical, is felt to be a useful implement, not an oppressive burden[12]. The impatient schoolboy looks upon his grammar and vocabulary as irksome and burdensome; but the accomplished student who has learnt the language by means of them, knows that they have given him the means of expressing what he thinks, and

[12] *Hist. Ind. Sc.* b. xvi. c. iv. sect. 2.

even of thinking more precisely. And as the study of language thus gives precision to the thoughts, the study of Natural History, and especially of the descriptive part of it, gives precision to the senses.

The Education of the Senses is also greatly promoted by the practical pursuit of any science of experiment and observation, as chemistry or astronomy. The methods of manipulating, of which we have just spoken, in chemistry, and the methods of measuring extremely minute portions of space and time which are employed in astronomy, and which are described in the former part of this chapter, are among the best modes of educating the senses for purposes of scientific observation.

28. By the various Methods of precise observation which we have thus very briefly described, facts are collected, of an exact and definite kind; they are then bound together in general laws, by the aid of general ideas and of such methods as we have now to consider. It is true, that the ideas which enable us to combine facts into general propositions, do commonly operate in our minds while we are still engaged in the office of observing. Ideas of one kind or other are requisite to connect our phenomena into facts, and to give meaning to the terms of our descriptions: and it frequently happens, that long before we have collected all the facts which induction requires, the mind catches the suggestion which some of these ideas offer, and leaps forwards to a conjectural law while the labour of observation is yet unfinished. But though this actually occurs, it is easy to see that the process of combining and generalizing facts is, in the order of nature, posterior to, and distinct from, the process of observing facts. Not only is this so, but there is an intermediate step which, though inseparable from all successful generalization, may be distinguished from it in our survey; and may, in some degree, be assisted by peculiar methods. To the consideration of such methods we now proceed.

CHAPTER III.

OF METHODS OF ACQUIRING CLEAR SCIENTIFIC IDEAS; *and first* OF INTELLECTUAL EDUCATION.

APHORISM XXIX.

The Methods by which the acquisition of clear Scientific Ideas is promoted, are mainly two; Intellectual Education *and* Discussion of Ideas.

APHORISM XXX.

The Idea of Space becomes more clear by studying Geometry; *the Idea of Force, by studying* Mechanics; *the Ideas of Likeness, of Kind, of Subordination of Classes, by studying* Natural History.

APHORISM XXXI.

Elementary Mechanics *should now form a part of intellectual education, in order that the student may understand the Theory of Universal Gravitation: for an intellectual education should cultivate such ideas as enable the student to understand the most complete and admirable portions of the knowledge which the human race has attained to.*

APHORISM XXXII.

Natural History *ought to form a part of intellectual education, in order to correct certain prejudices which arise from cultivating the intellect by means of mathematics alone; and in order to lead the student to see that the division of things into Kinds, and the attribution and use of Names, are processes susceptible of great precision.*

THE ways in which men become masters of those clear and yet comprehensive conceptions which the formation and reception of science require, are mainly two; which, although we cannot reduce them to any exact scheme, we may still, in a loose use of the term, call *Methods* of acquiring clear Ideas. These two ways are Education and Discussion.

1. (I.) *Idea of Space.*—It is easily seen that Education may do at least something to render our ideas distinct and precise. To learn Geometry in youth, tends, manifestly, to render our idea of space clear and exact. By such an education, all the relations, and all the consequences of this idea, come to be readily and steadily apprehended; and thus it becomes easy for us to understand portions of science which otherwise we should by no means be able to comprehend. The conception of *similar triangles* was to be mastered, before the disciples of Thales could see the validity of his method of determining the height of lofty objects by the length of their shadows. The conception of *the sphere with its circles* had to become familiar, before the annual motion of the sun and its influence upon the lengths of days could be rightly traced. The properties of circles, combined with the *pure*[1] *doctrine of motion,* were required as an introduction to the theory of Epicycles: the properties of *conic sections* were needed, as a preparation for the discoveries of Kepler. And not only was it necessary that men should possess a *knowledge* of certain figures and their properties; but it was equally necessary that they should have the *habit of reasoning* with perfect steadiness, precision, and conclusiveness concerning the relations of space. No small discipline of the mind is requisite, in most cases, to accustom it to go, with complete insight and security, through the demonstrations respecting intersecting planes and lines, dihedral and trihedral angles, which occur in solid geometry. Yet how absolutely necessary is a perfect mastery of such reasonings, to him who is to explain the motions of the moon in

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1 See *Hist. Sc. Ideas,* b. ii. c. xiii.

latitude and longitude! How necessary, again, is the same faculty to the student of crystallography! Without mathematical habits of conception and of thinking, these portions of science are perfectly inaccessible. But the early study of plane and solid geometry gives to all tolerably gifted persons, the habits which are thus needed. The discipline of following the reasonings of didactic works on this subject, till we are quite familiar with them, and of devising for ourselves reasonings of the same kind, (as, for instance, the solutions of problems proposed,) soon gives the mind the power of *discoursing* with perfect facility concerning the most complex and multiplied relations of space, and enables us to refer to the properties of all plane and solid figures as surely as to the visible forms of objects. Thus we have here a signal instance of the efficacy of education in giving to our Conceptions that clearness, which the formation and existence of science indispensably require.

2. It is not my intention here to enter into the details of the form which should be given to education, in order that it may answer the purposes now contemplated. But I may make a remark, which the above examples naturally suggest, that in a mathematical education, considered as a preparation for furthering or understanding physical science, Geometry is to be cultivated, far rather than Algebra:—the properties of space are to be studied and reasoned upon as they are in themselves, not as they are replaced and disguised by symbolical representations. It is true, that when the student is become quite familiar with elementary geometry, he may often enable himself to deal in a more rapid and comprehensive manner with the relations of space, by using the language of symbols and the principles of symbolical calculation: but this is an ulterior step, which may be added to, but can never be substituted for, the direct cultivation of geometry. The method of symbolical reasoning employed upon subjects of geometry and mechanics, has certainly achieved some remarkable triumphs in the treatment of the theory of the universe. These successful appli-

cations of symbols in the highest problems of physical astronomy appear to have made some teachers of mathematics imagine that it is best to *begin* the pupil's course with such symbolical generalities. But this mode of proceeding will be so far from giving the student clear ideas of mathematical relations, that it will involve him in utter confusion, and probably prevent his ever obtaining a firm footing in geometry. To commence mathematics in such a way, would be much as if we should begin the study of a language by reading the highest strains of its lyrical poetry.

3. (II.) *Idea of Number, &c.*—The study of mathematics, as I need hardly observe, developes and renders exact, our conceptions of the relations of number, as well as of space. And although, as we have already noticed, even in their original form the conceptions of number are for the most part very distinct, they may be still further improved by such discipline. In complex cases, a methodical cultivation of the mind in such subjects is needed: for instance, questions concerning Cycles, and Intercalations, and Epacts, and the like, require very great steadiness of arithmetical apprehension in order that the reasoner may deal with them rightly. In the same manner, a mastery of problems belonging to the science of Pure Motion, or, as I have termed it, *Mechanism,* requires either great natural aptitude in the student, or a mind properly disciplined by suitable branches of mathematical study.

4. Arithmetic and Geometry have long been standard portions of the education of cultured persons throughout the civilized world; and hence all such persons have been able to accept and comprehend those portions of science which depend upon the idea of space: for instance, the doctrine of the globular form of the earth, with its consequences, such as the measures of latitude and longitude;—the heliocentric system of the universe in modern, or the geocentric in ancient times;—the explanation of the rainbow; and the like. In nations where there is no such education, these portions of science cannot exist as a part of the general stock of the knowledge of society, however intelligently they

may be pursued by single philosophers dispersed here and there in the commnity.

5. (III.) *Idea of Force.*—As the idea of Space is brought out in its full evidence by the study of Geometry, so the idea of Force is called up and developed by the study of the science of Mechanics. It has already been shown, in our scrutiny of the Ideas of the Mechanical Sciences, that Force, the Cause of motion or of equilibrium, involves an independent Fundamental Idea, and is quite incapable of being resolved into any mere modification of our conceptions of space, time, and motion. And in order that the student may possess this idea in a precise and manifest shape, he must pursue the science of Mechanics in the mode which this view of its nature demands;—that is, he must study it as an independent science, resting on solid elementary principles of its own, and not built upon some other unmechanical science as its substructure. He must trace the truths of Mechanics from their own axioms and definitions; these axioms and definitions being considered as merely means of bringing into play the Idea on which the science depends. The conceptions of force and matter, of action and reaction, of momentum and inertia, with the reasonings in which they are involved, cannot be evaded by any substitution of lines or symbols for the conceptions. Any attempts at such substitution would render the study of Mechanics useless as a preparation of the mind for physical science; and would, indeed, except counteracted by great natural clearness of thought on such subjects, fill the mind with confused and vague notions, quite unavailing for any purposes of sound reasoning. But, on the other hand, the study of Mechanics, in its genuine form, as a branch of education, is fitted to give a most useful and valuable precision of thought on such subjects; and is the more to be recommended, since, in the general habits of most men's minds, the mechanical conceptions are tainted with far greater obscurity and perplexity than belongs to the conceptions of number, space, and motion.

6. As habitually distinct conceptions of *space* and

motion were requisite for the reception of the doctrines of formal astronomy, (the Ptolemaic and Copernican system,) so a clear and steady conception of *force* is indispensably necessary for understanding the Newtonian system of physical astronomy. It may be objected that the study of Mechanics as a science has not commonly formed part of a liberal education in Europe, and yet that educated persons have commonly accepted the Newtonian system. But to this we reply, that although most persons of good intellectual culture have professed to assent to the Newtonian system of the universe, yet they have, in fact, entertained it in so vague and perplexed a manner as to show very clearly that a better mental preparation than the usual one is necessary, in order that such persons may really understand the doctrine of universal attraction. I have elsewhere spoken of the prevalent indistinctness of mechanical conceptions[2]; and need not here dwell upon the indications, constantly occurring in conversation and in literature, of the utter inaccuracy of thought on such subjects which may often be detected; for instance, in the mode in which many men speak of centrifugal and centripetal forces;—of projectile and central forces;—of the effect of the moon upon the waters of the ocean; and the like. The incoherence of ideas which we frequently witness on such points, shows us clearly that, in the minds of a great number of men, well educated according to the present standard, the acceptance of the doctrine of Universal Gravitation is a result of traditional prejudice, not of rational conviction. And those who are Newtonians on such grounds, are not at all more intellectually advanced by being Newtonians in the nineteenth century, than they would have been by being Ptolemaics in the fifteenth.

7. It is undoubtedly in the highest degree desirable that all great advances in science should become the common property of all cultivated men. And this can only be done by introducing into the course of a liberal education such studies as unfold and fix in men's minds

[2] *Hist. Sc. Ideas*, b. iii. c. x.

the fundamental ideas upon which the new-discovered truths rest. The progress made by the ancients in geography, astronomy, and other sciences, led them to assign, wisely and well, a place to arithmetic and geometry among the steps of an ingenuous education. The discoveries of modern times have rendered these steps still more indispensable; for we cannot consider a man as cultivated up to the standard of his times, if he is not only ignorant of, but incapable of comprehending, the greatest achievements of the human intellect. And as innumerable discoveries of all ages have thus secured to Geometry her place as a part of good education, so the great discoveries of Newton make it proper to introduce Elementary Mechanics as a part of the same course. If the education deserve to be called *good*, the pupil will not remain ignorant of those discoveries, the most remarkable extensions of the field of human knowledge which have ever occurred. Yet he cannot by possibility comprehend them, except his mind be previously disciplined by mechanical studies. The period appears now to be arrived when we may venture, or rather when we are bound to endeavour, to include a new class of Fundamental Ideas in the elementary discipline of the human intellect. This is indispensable, if we wish to educe the powers which we know that it possesses, and to enrich it with the wealth which lies within its reach[3].

8. By the view which is thus presented to us of the nature and objects of intellectual education, we are led to consider the mind of man as undergoing a progress from age to age. By the discoveries which are made, and by the clearness and evidence which, after a time, (not suddenly nor soon,) the truths thus discovered acquire, one portion of knowledge after another becomes *elementary;* and if we would really secure this progress, and make men share in it, these new portious must be treated as elementary in the constitution of a

[3] The University of Cambridge has, by a recent law, made an examination in Elementary Mechanics requisite for the Degree of B.A.

liberal education. Even in the rudest forms of intelligence, man is immeasurably elevated above the unprogressive brute, for the idea of number is so far developed that he can count his flock or his arrows. But when number is contemplated in a speculative form, he has made a vast additional progress; when he steadily apprehends the relations of space, he has again advanced; when in thought he carries these relations into the vault of the sky, into the expanse of the universe, he reaches a higher intellectual position. And when he carries into these wide regions, not only the relations of space and time, but of cause and effect, of force and reaction, he has again made an intellectual advance; which, wide as it is at first, is accessible to all; and with which all should acquaint themselves, if they really desire to prosecute with energy the ascending path of truth and knowledge which lies before them. This should be an object of exertion to all ingenuous and hopeful minds. For, that exertion is necessary,—that after all possible facilities have been afforded, it is still a matter of toil and struggle to appropriate to ourselves the acquisitions of great discoverers, is not to be denied. Elementary mechanics, like elementary geometry, is a study accessible to all: but like that too, or perhaps more than that, it is a study which requires effort and contention of mind,—a forced steadiness of thought. It is long since one complained of this labour in geometry; and was answered that in that region there is no *Royal Road*. The same is true of Mechanics, and must be true of all branches of solid education. But we should express the truth more appropriately in our days by saying that there is no *Popular Road* to these sciences. In the mind, as in the body, strenuous exercise alone can give strength and activity. The art of exact thought can be acquired only by the labour of close thinking.

9. (IV.) *Chemical Ideas.*—We appear then to have arrived at a point of human progress in which a liberal education of the scientific intellect should include, besides arithmetic, elementary geometry and mechanics.

The question then occurs to us, whether there are any other Fundamental Ideas, among those belonging to other sciences, which ought also to be made part of such an education;—whether, for example, we should strive to develope in the minds of all cultured men the ideas of *polarity*, mechanical and chemical, of which we spoke in a former part of this work.

The views to which we have been conducted by the previous inquiry lead us to reply that it would not be well at present to make *chemical* Polarities, at any rate, a subject of elementary instruction. For even the most profound and acute philosophers who have speculated upon this subject,—they who are leading the van in the march of discovery,—do not seem yet to have reduced their thoughts on this subject to a consistency, or to have taken hold of this idea of Polarity in a manner quite satisfactory to their own minds. This part of the subject is, therefore, by no means ready to be introduced into a course of general elementary education; for, with a view to such a purpose, nothing less than the most thoroughly luminous and transparent condition of the idea will suffice. Its whole efficacy, as a means and object of disciplinal study, depends upon there being no obscurity, perplexity, or indefiniteness with regard to it, beyond that transient deficiency which at first exists in the learner's mind, and is to be removed by his studies. The idea of chemical Polarity is not yet in this condition; and therefore is not yet fit for a place in education. Yet since this idea of Polarity is the most general idea which enters into chemistry, and appears to be that which includes almost all the others, it would be unphilosophical, and inconsistent with all sound views of science, to introduce into education some chemical conceptions, and to omit those which depend upon this idea: indeed such a partial adoption of the science could hardly take place without not only omitting, but misrepresenting, a great part of our chemical knowledge. The conclusion to which we are necessarily led, therefore, is this:—that at present chemistry cannot with any advantage, form a portion of the general intellectual education[4].

10. (V.) *Natural-History Ideas.*—But there remains still another class of Ideas, with regard to which we may very properly ask whether they may not advantageously form a portion of a liberal education: I mean the Ideas of definite Resemblance and Difference, and of one set of resemblances subordinate to another, which form the bases of the classificatory sciences. These Ideas are developed by the study of the various branches of Natural History, as Botany, and Zoology; and beyond all doubt, those pursuits, if assiduously followed, very materially affect the mental habits. There is this obvious advantage to be looked for from the study of Natural History, considered as a means of intellectual discipline:—that it gives us, in a precise and scientific form, examples of the classing and naming of objects; which operations the use of common language leads us constantly to perform in a loose and inexact way. In the usual habits of our minds and tongues, things are distinguished or brought together, and names are applied, in a manner very indefinite, vacillating, and seemingly capricious: and we may naturally be led to doubt whether such defects can be avoided;—whether exact distinctions of things, and rigorous use of words be possible. Now upon this point we may receive the instruction of Natural History; which proves to us, by the actual performance of the task, that a precise classification and nomenclature are attainable, at least for a mass of objects all of the same kind. Further, we also learn from this study, that there may exist, not only an exact distinction of kinds of things, but a series of distinctions, one set subordinate to another, and the more general including

[4] I do not here stop to prove that an education (if it be so called) in which the memory only retains the verbal expression of results, while the mind does not apprehend the principles of the subject, and therefore cannot even understand the words in which its doctrines are expressed, is of no value whatever to the intellect, but rather, is highly hurtful to the habits of thinking and reasoning.

the more special, so as to form a system of classification. All these are valuable lessons. If by the study of Natural History we evolve, in a clear and well defined form, the conceptions of *genus*, *species*, and of *higher* and *lower steps* of classification, we communicate precision, clearness, and method to the intellect, through a great range of its operations.

11. It must be observed, that in order to attain the disciplinal benefit which the study of Natural History is fitted to bestow, we must teach the *natural* not the artificial *classifications;* or at least the natural as well as the artificial. For it is important for the student to perceive that there are classifications, not merely arbitrary, founded upon some *assumed* character, but natural, recognized by some *discovered* character: he ought to see that our classes being collected according to one mark, are confirmed by many marks not originally stated in our scheme; and are thus found to be grouped together, not by a single resemblance, but by a mass of resemblances, indicating a natural affinity. That objects may be collected into such groups, is a highly important lesson, which Natural History alone, pursued as the science of *natural classes*, can teach.

12. Natural History has not unfrequently been made a portion of education: and has in some degree produced such effects as we have pointed out. It would appear, however, that its lessons have, for the most part, been very imperfectly learnt or understood by persons of ordinary education: and that there are perverse intellectual habits very commonly prevalent in the cultivated classes, which ought ere now to have been corrected by the general teaching of Natural History. We may detect among speculative men many prejudices respecting the nature and rules of reasoning, which arise from pure mathematics having been so long and so universally the instrument of intellectual cultivation. Pure Mathematics reasons from definitions: whatever term is introduced into her pages, as a *circle*, or a *square*, its definition comes along with it; and this definition is supposed to supply all that the reasoner needs to know, respecting the term.

If there be any doubt concerning the validity of the conclusion, the doubt is resolved by recurring to the definitions. Hence it has come to pass that in other subjects also, men seek for and demand definitions as the most secure foundation of reasoning. The definition and the term defined are conceived to be so far identical, that in all cases the one may be substituted for the other; and such a substitution is held to be the best mode of detecting fallacies.

13. It has already been shown that even geometry is not founded upon definitions alone: and we shall not here again analyse the fallacy of this belief in the supreme value of definitions. But we may remark that the study of Natural History appears to be the proper remedy for this erroneous habit of thought. For in every department of Natural History the object of our study is *kinds* of things, not one of which kinds can be rigorously defined, yet all of them are sufficiently definite. In these cases we may indeed give a specific description of one of the kinds, and may call it a definition; but it is clear that such a definition does not contain the essence of the thing. We say[5] that the Rose Tribe are 'Polypetalous dicotyledons, with lateral styles, superior simple ovaria, regular perigynous stamens, exalbuminous definite seeds, and alternate stipulate leaves.' But no one would say that this was our essential conception of a rose, to be substituted for it in all cases of doubt or obscurity, by way of making our reasonings perfectly clear. Not only so; but as we have already seen[6], the definition does not even apply to all the tribe. For the stipulæ are absent in Lowea: the albumen is present in Neillia: the fruit of Spiræa sorbifolia is capsular. If, then, we can possess any certain knowledge in Natural History, (which no cultivator of the subject will doubt,) it is evident that our knowledge cannot depend on the possibility of laying down exact definitions and reasoning from them.

14. But it may be asked, if we cannot define a

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[5] Lindley's *Nat. Syst. Bot.* p. 81.

[6] *Hist. Sc. Ideas*, b. viii. c. ii. sect. 3.

word, or a class of things which a word denotes, how can we distinguish what it does mean from what it does not mean? How can we say that it signifies one thing rather than another, except we declare what is its signification?

The answer to this question involves the general principle of a natural method of classification, which has already been stated[7] and need not here be again dwelt on. It has been shown that names of *kinds* of things (*genera*) associate them according to total resemblances, not partial characters. The principle which connects a group of objects in natural history is not a *definition*, but a *type*. Thus we take as the type of the Rose family, it may be, the common *wild rose;* all species which resemble this flower more than they resemble any other group of species are also *roses*, and form one *genus*. All genera which resemble Roses more than they resemble any other group of genera are of the same *family*. And thus the Rose family is collected about some one species, which is the type or central point of the group.

In such an arrangement, it may readily be conceived that though the nucleus of each group may cohere firmly together, the outskirts of contiguous groups may approach, and may even be intermingled, so that some species may doubtfully adhere to one group or another. Yet this uncertainty does not at all affect the truths which we find ourselves enabled to assert with regard to the general mass of each group. And thus we are taught that there may be very important differences between two groups of objects, although we are unable to tell where the one group ends and where the other begins; and that there may be propositions of indisputable truth, in which it is impossible to give unexceptionable definitions of the terms employed.

15. These lessons are of the highest value with regard to all employments of the human mind; for the mode in which words in common use acquire their meaning, approaches far more nearly to the *Method of*

[7] *Hist. Sc. Ideas*, b. viii. c. ii. sect. 3.

Type than to the method of definition. The terms which belong to our practical concerns, or to our spontaneous and unscientific speculations, are rarely capable of exact definition. They have been devised in order to express assertions, often very important, yet very vaguely conceived: and the signification of the word is extended, as far as the assertion conveyed by it can be extended, by apparent connexion or by analogy. And thus, in all the attempts of man to grasp at knowledge, we have an exemplification of that which we have stated as the rule of induction, that Definition and Proposition are mutually dependent, each adjusted so as to give value and meaning to the other: and this is so, even when both the elements of truth are defective in precision: the Definition being replaced by an incomplete description or a loose reference to a Type; and the Proposition being in a corresponding degree insecure.

16. Thus the study of Natural History, as a corrective of the belief that definitions are essential to substantial truth, might be of great use; and the advantage which might thus be obtained is such as well entitles this study to a place in a liberal education. We may further observe, that in order that Natural History may produce such an effect, it must be studied by inspection of the *objects* themselves, and not by the reading of books only. Its lesson is, that we must in all cases of doubt or obscurity refer, not to words or definitions, but to things. The Book of Nature is its dictionary: it is there that the natural historian looks, to find the meaning of the words which he uses[8]. So

[8] It is a curious example of the influence of the belief in definitions, that elementary books have been written in which Natural History is taught in the way of question and answer, and consequently by means of words alone. In such a scheme, of course all objects are *defined:* and we may easily anticipate the value of the knowledge thus conveyed. Thus, 'Iron is a well-known hard metal, of a darkish gray colour, and very elastic:' 'Copper is an orange-coloured metal, more sonorous than any other, and the most elastic of any except iron.' This is to pervert the meaning of education, and to make it a business of mere words.

long as a plant, in its most essential parts, is more *like* a rose than any thing else, it *is* a rose. He knows no other definition.

17. (VI.) *Well-established Ideas alone to be used.*—We may assert in general what we have elsewhere, as above, stated specially with reference to the fundamental principles of chemistry:—no Ideas are suited to become the elements of elementary education, till they have not only become perfectly distinct and fixed in the minds of the leading cultivators of the science to which they belong; but till they have been so for some considerable period. The entire clearness and steadiness of view which is essential to sound science, must have time to extend itself to a wide circle of disciples. The views and principles which are detected by the most profound and acute philosophers, are soon appropriated by all the most intelligent and active minds of their own and of the following generations; and when this has taken place, (and not till then,) it is right, by a proper constitution of our liberal education, to extend a general knowledge of such principles to all cultivated persons. And it follows, from this view of the matter, that we are by no means to be in haste to adopt, into our course of education, all new discoveries as soon as they are made. They require some time, in order to settle into their proper place and position in men's minds, and to show themselves under their true aspects; and till this is done, we confuse and disturb, rather than enlighten and unfold, the ideas of learners, by introducing the discoveries into our elementary instruction. Hence it was perhaps reasonable that a century should elapse from the time of Galileo, before the rigorous teaching of Mechanics became a general element of intellectual training; and the doctrine of Universal Gravitation was hardly ripe for such an employment till the end of the last century. We must not direct the unformed youthful mind to launch its little bark upon the waters of speculation, till all the agitation of discovery, with its consequent fluctuation and controversy, has well subsided.

18. But it may be asked, How is it that time ope-

rates to give distinctness and evidence to scientific ideas? In what way does it happen that views and principles, obscure and wavering at first, after a while become luminous and steady? Can we point out any process, any intermediate steps, by which this result is produced? If we can, this process must be an important portion of the subject now under our consideration.

To this we reply, that the transition from the hesitation and contradiction with which true ideas are first received, to the general assent and clear apprehension which they afterwards obtain, takes place through the circulation of various arguments for and against them, and various modes of presenting and testing them, all which we may include under the term *Discussion*, which we have already mentioned as the second of the two ways by which scientific views are developed into full maturity.

CHAPTER IV.

Of Methods of acquiring clear Scientific Ideas, *continued.*—Of the Discussion of Ideas.

Aphorism XXXIII.

The conceptions involved in scientific truths have attained the requisite degree of clearness by means of the Discussions *respecting ideas which have taken place among discoverers and their followers. Such discussions are very far from being unprofitable to science. They are* metaphysical, *and must be so: the difference between discoverers and barren reasoners is, that the former employ good, and the latter bad metaphysics.*

1. IT is easily seen that in every part of science, the establishment of a new set of ideas has been accompanied with much of doubt and dissent. And by means of discussions so occasioned, the new conceptions, and the opinions which involve them, have gradually become definite and clear. The authors and asserters of the new opinions, in order to make them defensible, have been compelled to make them consistent: in order to recommend them to others, they have been obliged to make them more entirely intelligible to themselves. And thus the Terms which formed the main points of the controversy, although applied in a loose and vacillating manner at first, have in the end become perfectly definite and exact. The opinions discussed have been, in their main features, the same throughout the debate; but they have at first been dimly, and at last clearly apprehended: like the objects of a landscape, at which we look through a telescope ill adjusted, till, by sliding the tube backwards and

forwards, we at last bring it into focus, and perceive every feature of the prospect sharp and bright.

2. We have in the last Book[1] fully exemplified this gradual progress of conceptions from obscurity to clearness by means of Discussion. We have seen, too, that this mode of treating the subject has never been successful, except when it has been associated with an appeal to facts as well as to reasonings. A combination of experiment with argument, of observation with demonstration, has always been found requisite in order that men should arrive at those distinct conceptions which give them substantial truths. The arguments used led to the rejection of undefined, ambiguous, self-contradictory notions; but the reference to facts led to the selection, or at least to the retention, of the conceptions which were both true and useful. The two correlative processes, definition and true assertion, the formation of clear ideas and the induction of laws, went on together.

Thus those discussions by which scientific conceptions are rendered ultimately quite distinct and fixed, include both reasonings from Principles and illustrations from Facts. At present we turn our attention more peculiarly to the former part of the process; according to the distinction already drawn, between the Explication of Conceptions and the Colligation of Facts. The Discussions of which we here speak, are the Method (if they may be called a *method*) by which the Explication of Conceptions is carried to the requisite point among philosophers.

3. In the *History* of the Fundamental Ideas of the Sciences which forms the Prelude to this work, and in the *History of the Inductive Sciences*, I have, in several instances, traced the steps by which, historically speaking, these Ideas have obtained their ultimate and permanent place in the minds of speculative men. I have thus exemplified the reasonings and controversies which constitute such Discussion as we now speak of. I have stated, at considerable length, the

[1] B. i. c. ii. Of the Explication of Conceptions.

various attempts, failures, and advances, by which the ideas which enter into the science of Mechanics were evolved into their present evidence. In like manner we have seen the conception of *refracted rays* of light, obscure and confused in Seneca, growing clearer in Roger Bacon, more definite in Descartes, perfectly distinct in Newton. The *polarity* of light, at first contemplated with some perplexity, became very distinct to Malus, Young, and Fresnel; yet the phenomena of *circular polarization*, and still more, the *circular polarization of fluids*, leave us, even at present, some difficulty in fully mastering this conception. The *related polarities* of electricity and magnetism are not yet fully comprehended, even by our greatest philosophers. One of Mr. Faraday's late papers (the Fourteenth Series of his Researches) is employed in an experimental discussion of this subject, which leads to no satisfactory result. The controversy between MM. Biot and Ampère[2], on the nature of the Elementary Forces in electro-dynamic action, is another evidence that the discussion of this subject has not yet reached its termination. With regard to *chemical polarity*, I have already stated that this idea is as yet very far from being brought to an ultimate condition of definiteness; and the subject of Chemical Forces, (for that whole subject must be included in this idea of polarity,) which has already occasioned much perplexity and controversy, may easily occasion much more, before it is settled to the satisfaction of the philosophical world. The ideas of the *classificatory* sciences also have of late been undergoing much, and very instructive discussion, in the controversies respecting the relations and offices of the natural and artificial methods. And with regard to *physiological* ideas, it would hardly be too much to say, that the whole history of physiology up to the present time has consisted of the discussion of the fundamental ideas of the science, such as Vital Forces, Nutrition, Reproduction, and the like. We had before us at some length, in the *History of Scientific Ideas*, a review

[2] *Hist. Ind. Sc.* b. xiii. c. 6.

of the opposite opinions which have been advanced on this subject; and we attempted in some degree to estimate the direction in which these ideas are permanently settling. But without attaching any importance to this attempt, the account there given may at least serve to show, how important a share in the past progress of this subject the *discussion* of its Fundamental Ideas has hitherto had.

4. There is one reflexion which is very pointedly suggested by what has been said. The manner in which our scientific ideas acquire their distinct and ultimate form being such as has been described,—always involving much abstract reasoning and analysis of our conceptions, often much opposite argumentation and debate;—how unphilosophical is it to speak of abstraction and analysis, of dispute and controversy, as frivolous and unprofitable processes, by which true science can never be benefitted; and how erroneous to put such employments in antithesis with the study of facts!

Yet some writers are accustomed to talk with contempt of all past controversies, and to wonder at the blindness of those who did not *at first* take the view which was established *at last.* Such persons forget that it was precisely the controversy, which established among speculative men that final doctrine which they themselves have quietly accepted. It is true, they have had no difficulty in thoroughly adopting the truth; but that has occurred because all dissentient doctrines have been suppressed and forgotten; and because systems, and books, and language itself, have been accommodated peculiarly to the expression of the accepted truth. To despise those who have, by their mental struggles and conflicts, brought the subject into a condition in which errour is almost out of our reach, is to be ungrateful exactly in proportion to the amount of the benefit received. It is as if a child, when its teacher had with many trials and much trouble prepared a telescope so that the vision through it was distinct, should wonder at his stupidity in pushing the tube of the eye-glass out and in so often.

5. Again, some persons condemn all that we have here spoken of as the discussion of ideas, terming it *metaphysical:* and in this spirit, one writer[3] has spoken of the 'metaphysical period' of each science, as preceding the period of 'positive knowledge.' But as we have seen, that process which is here termed 'metaphysical,'—the analysis of our conceptions and the exposure of their inconsistencies,—(accompanied with the study of facts,)—has always gone on most actively in the most prosperous periods of each science. There is, in Galileo, Kepler, Gassendi, and the other fathers of mechanical philosophy, as much of *metaphysics* as in their adversaries. The main difference is, that the metaphysics is of a better kind; it is more conformable to metaphysical truth. And the same is the case in other sciences. Nor can it be otherwise. For all truth, before it can be consistent with *facts,* must be consistent with *itself:* and although this rule is of undeniable authority, its application is often far from easy. The perplexities and ambiguities which arise from our having the same idea presented to us under different aspects, are often difficult to disentangle: and no common acuteness and steadiness of thought must be expended on the task. It would be easy to adduce, from the works of all great discoverers, passages more profoundly metaphysical than any which are to be found in the pages of barren *à priori* reasoners.

6. As we have said, these metaphysical discussions are not to be put in opposition to the study of facts; but are to be stimulated, nourished and directed by a constant recourse to experiment and observation. The cultivation of ideas is to be conducted as having for its object the connexion of facts; never to be pursued as a mere exercise of the subtilty of the mind, striving to build up a world of its own, and neglecting that which exists about us. For although man may in this way please himself, and admire the creations of his own brain, he can never, by this course, hit upon the

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[3] M. Auguste Comte, *Cours de Philosophie Positive.*

real scheme of nature. With his ideas unfolded by education, sharpened by controversy, rectified by metaphysics, he may *understand* the natural world, but he cannot *invent* it. At every step, he must try the value of the advances he has made in thought, by applying his thoughts to things. The Explication of Conceptions must be carried on with a perpetual reference to the Colligation of Facts.

Having here treated of Education and Discussion as the methods by which the former of these two processes is to be promoted, we have now to explain the methods which science employs in order most successfully to execute the latter. But the Colligation of Facts, as already stated, may offer to us two steps of a very different kind,—the laws of Phenomena, and their Causes. We shall first describe some of the methods employed in obtaining truths of the former of these two kinds.

CHAPTER V.

ANALYSIS OF THE PROCESS OF INDUCTION.

APHORISM XXXIV.

The Process of Induction may be resolved into three steps; the Selection of the Idea, *the* Construction of the Conception, *and the* Determination of the Magnitudes.

APHORISM XXXV.

These three steps correspond to the determination of the Independent Variable, *the* Formula, *and the* Coefficients, *in mathematical investigations; or to the* Argument, *the* Law, *and the* Numerical Data, *in a Table of an astronomical or other* Inequality.

APHORISM XXXVI.

The Selection of the Idea depends mainly upon inventive sagacity: which operates by suggesting and trying various hypotheses. Some inquirers try erroneous hypotheses; and thus, exhausting the forms of errour, form the Prelude to Discovery.

APHORISM XXXVII.

The following Rules may be given, in order to the selection of the Idea for purposes of Induction:—the Idea and the Facts must be homogeneous; *and the Rule must be* tested by the Facts.

SECT. I.—*The Three Steps of Induction.*

1. WHEN facts have been decomposed and phenomena measured, the philosopher endeavours to combine them into general laws, by the aid of

Ideas and Conceptions; these being illustrated and regulated by such means as we have spoken of in the last two chapters. In this task, of gathering laws of nature from observed facts, as we have already said[1], the natural sagacity of gifted minds is the power by which the greater part of the successful results have been obtained; and this power will probably always be more efficacious than any Method can be. Still there are certain methods of procedure which may, in such investigations, give us no inconsiderable aid, and these I shall endeavour to expound.

2. For this purpose, I remark that the Colligation of ascertained Facts into general Propositions may be considered as containing three steps, which I shall term *the Selection of the Idea*, *the Construction of the Conception*, and *the Determination of the Magnitudes.* It will be recollected that by the word *Idea*, (or Fundamental Idea,) used in a peculiar sense, I mean certain wide and general fields of intelligible relation, such as Space, Number, Cause, Likeness; while by *Conception* I denote more special modifications of these ideas, as a *circle*, a *square number*, a *uniform force*, a *like form* of flower. Now in order to establish any law by reference to facts, we must select the *true Idea* and the *true Conception.* For example; when Hipparchus found[2] that the distance of the bright star Spica Virginis from the equinoxial point had increased by two degrees in about two hundred years, and desired to reduce this change to a law, he had first to assign, if possible, the *idea* on which it depended;—whether it was regulated for instance, by *space*, or by *time;* whether it was determined by the positions of other stars at each moment, or went on progressively with the lapse of ages. And when there was found reason to select *time* as the regulative *idea* of this change, it was then to be determined how the change went on with the time;—whether uniformly, or in some other manner: the *conception*, or the rule of the progression, was to be

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1 B. ii. c. vi.

2 *Hist. Ind. Sc.* b. iii. c. iv. sect. 3.

rightly constructed. Finally, it being ascertained that the change did go on uniformly, the question then occurred what was its *amount*:—whether exactly a degree in a century, or more, or less, and how much: and thus the determination of the *magnitude* completed the discovery of the law of phenomena respecting this star.

3. Steps similar to these three may be discerned in all other discoveries of laws of nature. Thus, in investigating the laws of the motions of the sun, moon or planets, we find that these motions may be resolved, besides a uniform motion, into a series of partial motions, or Inequalities; and for each of these Inequalities, we have to learn upon what it directly depends, whether upon the progress of time only, or upon some configuration of the heavenly bodies in space; then, we have to ascertain its law; and finally, we have to determine what is its amount. In the case of such Inequalities, the fundamental element on which the Inequality depends, is called by mathematicians the *Argument*. And when the Inequality has been fully reduced to known rules, and expressed in the form of a Table, the Argument is the fundamental Series of Numbers which stands in the margin of the Table, and by means of which we refer to the other Numbers which express the Inequality. Thus, in order to obtain from a Solar Table the Inequality of the sun's annual motion, the Argument is the Number which expresses the day of the year; the Inequalities for each day being (in the Table) ranged in a line corresponding to the days. Moreover, the Argument of an Inequality being assumed to be known, we must, in order to calculate the Table, that is, in order to exhibit the law of nature, know also the *Law* of the Inequality, and its *Amount*. And the investigation of these three things, the Argument, the Law, and the Amount of the Inequality, represents the three steps above described, the Selection of the Idea, the Construction of the Conception, and the Determination of the Magnitude.

4. In a great body of cases, *mathematical* language and calculation are used to express the connexion be-

tween the general law and the special facts. And when this is done, the three steps above described may be spoken of as the Selection of the *Independent Variable*, the Construction of the *Formula*, and the Determination of the *Coefficients*. It may be worth our while to attend to an exemplification of this. Suppose then, that, in such observations as we have just spoken of, namely, the shifting of a star from its place in the heavens by an unknown law, astronomers had, at the end of three successive years, found that the star had removed by 3, by 8, and by 15 minutes from its original place. Suppose it to be ascertained also, by methods of which we shall hereafter treat, that this change depends upon the time; we must then take the *time*, (which we may denote by the symbol t,) for the *independent variable*. But though the star changes its place *with* the time, the change is not *proportional* to the time; for its motion which is only 3 minutes in the first year, is 5 minutes in the second year, and 7 in the third. But it is not difficult for a person a little versed in mathematics to perceive that the series 3, 8, 15, may be obtained by means of two terms, one of which is proportional to the time, and the other to the square of the time; that is, it is expressed by the *formula* $at + btt$. The question then occurs, what are the values of the *coefficients* a and b; and a little examination of the case shows us that a must be 2, and b, 1: so that the formula is $2t + tt$. Indeed if we add together the series 2, 4, 6, which expresses a change proportional to the time, and 1, 4, 9, which is proportional to the square of the time, we obtain the series 3, 8, 15, which is the series of numbers given by observation. And thus the three steps which give us the Idea, the Conception, and the Magnitudes; or the Argument, the Law, and the Amount, of the change; give us the Independent Variable, the Formula, and the Coefficients, respectively.

We now proceed to offer some suggestions of methods by which each of these steps may be in some degree promoted.

Sect. II.—*Of the Selection of the Fundamental Idea.*

5. When we turn our thoughts upon any assemblage of facts, with a view of collecting from them some connexion or law, the most important step, and at the same time that in which rules can least aid us, is the Selection of the Idea by which they are to be collected. So long as this idea has not been detected, all seems to be hopeless confusion or insulated facts; when the connecting idea has been caught sight of, we constantly regard the facts with reference to their connexion, and wonder that it should be possible for any one to consider them in any other point of view.

Thus the different seasons, and the various aspects of the heavenly bodies, might at first appear to be direct manifestations from some superior power, which man could not even understand: but it was soon found that the ideas of time and space, of motion and recurrence, would give coherency to many of the phenomena. Yet this took place by successive steps. Eclipses, for a long period, seemed to follow no law; and being very remarkable events, continued to be deemed the indications of a supernatural will, after the common motions of the heavens were seen to be governed by relations of time and space. At length, however, the Chaldeans discovered that, after a period of eighteen years, similar sets of eclipses recur; and, thus selecting the idea of *time*, simply, as that to which these events were to be referred, they were able to reduce them to rule; and from that time, eclipses were recognized as parts of a regular order of things. We may, in the same manner, consider any other course of events, and may enquire by what idea they are bound together. For example, if we take the weather, years peculiarly wet or dry, hot and cold, productive and unproductive, follow each other in a manner which, at first sight at least, seems utterly lawless and irregular. Now can we in any way discover some rule and order in these occurrences? Is there, for example, in these events, as in eclipses, a certain cycle of years, after which like seasons come round again? or does the weather depend upon the force of some extraneous body—for instance, the moon—and follow in some way her aspects? or would the most proper way of investigating this subject be to consider the effect of the moisture and heat of various tracts of the earth's surface upon the ambient air? It is at our choice to *try* these and other modes of obtaining a science of the weather: that is, we may refer the phenomena to the idea of *time*, introducing the conception of a cycle;—or to the idea of external *force*, by the conception of the moon's action;—or to the idea of *mutual action*, introducing the conceptions of thermotical and atmological agencies, operating between different regions of earth, water, and air.

6. It may be asked, How are we to decide in such alternatives? How are we to select the one right idea out of several conceivable ones? To which we can only reply, that this must be done by *trying* which will succeed. If there really exist a cycle of the weather, as well as of eclipses, this must be established by comparing the asserted cycle with a good register of the seasons, of sufficient extent. Or if the moon really influence the meteorological conditions of the air, the asserted influence must be compared with the observed facts, and so accepted or rejected. When Hipparchus had observed the increase of longitude of the stars, the idea of a motion of the celestial sphere suggested itself as the explanation of the change; but this thought was *verified* only by observing several stars. It was conceivable that each star should have an independent motion, governed by time only, or by other circumstances, instead of being regulated by its place in the sphere; and this possibility could be rejected by trial alone. In like manner, the original opinion of the composition of bodies supposed the compounds to derive their properties from the elements according to the law of *likeness;* but this opinion was overturned by a thousand facts; and thus the really applicable Idea of Chemical Composition was introduced in modern times. In what has already been said on the History of Ideas, we have seen how each science was in a state

of confusion and darkness till the right idea was introduced.

7. No general method of evolving such ideas can be given. Such events appear to result from a peculiar sagacity and felicity of mind;—never without labour, never without preparation;—yet with no constant dependence upon preparation, or upon labour, or even entirely upon personal endowments. Newton explained the colours which refraction produces, by referring each colour to a peculiar *angle of refraction*, thus introducing the right idea. But when the same philosopher tried to explain the colours produced by diffraction, he erred, by attempting to apply the same idea, (*the course of a single ray*,) instead of applying the truer idea, of the *interference of two rays*. Newton gave a wrong rule for the double refraction of Iceland spar, by making the refraction depend on the *edges* of the rhombohedron: Huyghens, more happy, introduced the idea of the *axis of symmetry* of the solid, and thus was able to give the true law of the phenomena.

8. Although the selected idea is proved to be the right one, only when the true law of nature is established by means of it, yet it often happens that there prevails a settled conviction respecting the relation which must afford the key to the phenomena, before the selection has been confirmed by the laws to which it leads. Even before the empirical laws of the tides were made out, it was not doubtful that these laws depended upon the places and motions of the sun and moon. We know that the crystalline form of a body must depend upon its chemical composition, though we are as yet unable to assign the law of this dependence.

Indeed in most cases of great discoveries, the right idea to which the facts were to be referred, was selected by many philosophers, before the decisive demonstration that it *was* the right idea, was given by the discoverer. Thus Newton showed that the motions of the planets might be explained by means of a central force in the sun: but though he established, he did not first select the idea involved in the conception of a central force. The idea had already been sufficiently pointed out, dimly by Kepler, more clearly by Borelli, Huyghens, Wren, and Hooke. Indeed this anticipation of the true idea is always a principal part of that which, in the *History of the Sciences*, we have termed the *Prelude* of a Discovery. The two steps of *proposing* a philosophical problem, and of *solving* it, are, as we have elsewhere said, both important, and are often performed by different persons. The former step is, in fact, the Selection of the Idea. In explaining any change, we have to discover first the *Argument*, and then the *Law* of the change. The selection of the Argument is the step of which we here speak; and is that in which inventiveness of mind and justness of thought are mainly shown.

9. Although, as we have said, we can give few precise directions for this cardinal process, the Selection of the Idea, in speculating on phenomena, yet there is one Rule which may have its use: it is this:—*The idea and the facts must be homogeneous:* the elementary Conceptions, into which the facts have been decomposed, must be of the same nature as the Idea by which we attempt to collect them into laws. Thus, if facts have been observed and measured by reference to space, they must be bound together by the idea of space: if we would obtain a knowledge of mechanical forces in the solar system, we must observe mechanical phenomena. Kepler erred against this rule in his attempts at obtaining physical laws of the system; for the facts which he took were the *velocities*, not the *changes of velocity*, which are really the mechanical facts. Again, there has been a transgression of this Rule committed by all chemical philosophers who have attempted to assign the relative position of the elementary particles of bodies in their component molecules. For their purpose has been to discover the *relations* of the particles *in space;* and yet they have neglected the only facts in the constitution of bodies which have a reference to space—namely, *crystalline form*, and *optical properties*. No progress can be made in the theory of the elementary structure of bodies,

without making these classes of facts the main basis of our speculations.

10. The only other Rule which I have to offer on this subject, is that which I have already given:—*the Idea must be tested by the facts.* It must be tried by applying to the facts the conceptions which are derived from the idea, and not accepted till some of these succeed in giving the law of the phenomena. The justice of the suggestion cannot be known otherwise than by making the trial. If we can discover a *true law* by employing any conceptions, the idea from which these conceptions are derived is the *right* one; nor can there be any proof of its rightness so complete and satisfactory, as that we are by it led to a solid and permanent truth.

This, however, can hardly be termed a Rule; for when we would know, to conjecture and to try the truth of our conjecture by a comparison with the facts, is the natural and obvious dictate of common sense.

Supposing the Idea which we adopt, or which we would try, to be now fixed upon, we still have before us the range of many Conceptions derived from it; many Formulæ may be devised depending on the same Independent Variable, and we must now consider how our selection among these is to be made.

CHAPTER VI.

General Rules for the Construction of the Conception.

Aphorism XXXVIII.

The Construction of the Conception very often includes, in a great measure, the Determination of the Magnitudes.

Aphorism XXXIX.

When a series of progressive *numbers is given as the result of observation, it may generally be reduced to law by combinations of arithmetical and geometrical progressions.*

Aphorism XL.

A true formula for a progressive series of numbers cannot commonly be obtained from a narrow range *of observations.*

Aphorism XLI.

Recurrent *series of numbers must, in most cases, be expressed by circular formulæ.*

Aphorism XLII.

The true construction of the conception is frequently suggested by some hypothesis; and in these cases, the hypothesis may be useful, though containing superfluous parts.

1. IN speaking of the discovery of laws of nature, those which depend upon *quantity*, as number, space, and the like, are most prominent and most easily conceived, and therefore in speaking of such researches, we shall often use language which applies peculiarly to

the cases in which quantities numerically measurable are concerned, leaving it for a subsequent task to extend our principles to ideas of other kinds.

Hence we may at present consider the Construction of a Conception which shall include and connect the facts, as being the construction of a Mathematical Formula, coinciding with the numerical expression of the facts; and we have to consider how this process can be facilitated, it being supposed that we have already before us the numerical measures given by observation.

2. We may remark, however, that the construction of the right Formula for any such case, and the determination of the Coefficients of such formula, which we have spoken of as two separate steps, are in practice almost necessarily simultaneous; for the near coincidence of the results of the theoretical rule with the observed facts confirms at the same time the Formula and its Coefficients. In this case also, the mode of arriving at truth is to try various hypotheses;—to modify the hypotheses so as to approximate to the facts, and to multiply the facts so as to test the hypotheses.

The Independent Variable, and the Formula which we would try, being once selected, mathematicians have devised certain special and technical processes by which the value of the coefficients may be determined. These we shall treat of in the next Chapter; but in the mean time we may note, in a more general manner, the mode in which, in physical researches, the proper formula may be obtained.

3. A person somewhat versed in mathematics, having before him a series of numbers, will generally be able to devise a formula which approaches near to those numbers. If, for instance, the series is constantly progressive, he will be able to see whether it more nearly resembles an arithmetical or a geometrical progression. For example, MM. Dulong and Petit, in their investigation of the law of cooling of bodies, obtained the following series of measures. A thermometer, made hot, was placed in an enclosure of which the temperature was 0 degrees, and the rapidity of

cooling of the thermometer was noted for many temperatures. It was found that

For the temperature	240	the rapidity of cooling	was	10·69
"	220	"		8·81
"	200	"		7·40
"	180	"		6·10
"	160	"		4·89
"	140	"		3·88

and so on. Now this series of numbers manifestly increases with greater rapidity as we proceed from the lower to the higher parts of the scale. The numbers do not, however, form a geometrical series, as we may easily ascertain. But if we were to take the differences of the successive terms we should find them to be—

1·88, 1·41, 1·30, 1·21, 1·01, &c.

and these numbers are very nearly the terms of a geometric series. For if we divide each term by the succeeding one, we find these numbers,

1·33, 1·09, 1·07, 1·20, 1·27,

in which there does not appear to be any constant tendency to diminish or increase. And we shall find that a geometrical series in which the ratio is 1·165, may be made to approach very near to this series, the deviations from it being only such as may be accounted for by conceiving them as errours of observation. In this manner a certain formula[1] is obtained, giving results

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[1] The formula is $v = 2{,}037\ (a^t - 1)$ where v is the velocity of cooling, t the temperature of the thermometer expressed in degrees, and a is the quantity 1,0077.

The degree of coincidence is as follows:—

Excess of temperature of the thermometer, or values of t.	Observed values of v.	Calculated values of v.
240	10·69	10·68
220	8·81	8·89
200	7·40	7·34
180	6·10	6·03
160	4·89	4·87
140	3·88	3·89
120	3·02	3·05
100	2·30	2·33
80	1·74	1·72

which very nearly coincide with the observed facts, as may be seen in the margin.

The physical law expressed by the formula just spoken of is this:—that when a body is cooling in an empty inclosure which is kept at a constant temperature, the quickness of the cooling, for excesses of temperature in arithmetical progression, increases as the terms of a geometrical progression, diminished by a constant number.

4. In the actual investigation of Dulong and Petit, however, the formula was not obtained in precisely the manner just described. For the quickness of cooling depends upon two elements, the temperature of the hot body and the temperature of the inclosure; not merely upon the *excess* of one of these over the other. And it was found most convenient, first, to make such experiments as should exhibit the dependence of the velocity of cooling upon the temperature of the enclosure; which dependence is contained in the following law:—The quickness of cooling of a thermometer in vacuo for a constant excess of temperature, increases in geometric progression, when the temperature of the inclosure increases in arithmetic progression. From this law the preceding one follows by necessary consequence[2].

This example may serve to show the nature of the artifices which may be used for the construction of formulæ, when we have a constantly progressive series of numbers to represent. We must not only endeavour by trial to contrive a formula which will answer the conditions, but we must vary our experiments so as to determine, first one factor or portion of the formula, and then the other; and we must use the most pro-

[2] For if θ be the temperature of the inclosure, and t the excess of temperature of the hot body, it appears, by this law, that the radiation of heat is as a^θ. And hence the quickness of cooling, which is as the excess of radiation, is as $a^{\theta+t} - a^\theta$; that is, as $a^\theta(a^t - 1)$ which agrees with the formula given in the last note.

The whole of this series of researches of Dulong and Petit is full of the most beautiful and instructive artifices for the construction of the proper formulæ in physical research.

bable hypothesis as means of suggestion for our formulæ.

5. In a *progressive* series of numbers, unless the formula which we adopt be really that which expresses the law of nature, the deviations of the formula from the facts will generally become enormous, when the experiments are extended into new parts of the scale. True formulæ for a progressive series of results can hardly ever be obtained from a very limited range of experiments: just as the attempt to guess the general course of a road or a river, by knowing two or three points of it in the neighbourhood of one another, would generally fail. In the investigation respecting the laws of the cooling of bodies just noticed, one great advantage of the course pursued by the experimenters was, that their experiments included so great a range of temperatures. The attempts to assign the law of elasticity of steam deduced from experiments made with moderate temperatures, were found to be enormously wrong, when very high temperatures were made the subject of experiment. It is easy to see that this must be so: an arithmetical and a geometrical series may nearly coincide for a few terms moderately near each other: but if we take remote corresponding terms in the two series, one of these will be very many times the other. And hence, from a narrow range of experiments, we may infer one of these series when we ought to infer the other; and thus obtain a law which is widely erroneous.

6. In Astronomy, the serieses of observations which we have to study are, for the most part, not progressive, but *recurrent*. The numbers observed do not go on constantly increasing; but after increasing up to a certain amount they diminish; then, after a certain space, increase again; and so on, changing constantly through certain *cycles*. In cases in which the observed numbers are of this kind, the formula which expresses them must be a *circular function*, of some sort or other; involving, for instance, sines, tangents, and other forms of calculation, which have recurring values when the angle on which they depend goes on constantly

increasing. The main business of formal astronomy consists in resolving the celestial phenomena into a series of *terms* of this kind, in detecting their *arguments*, and in determining their *coefficients*.

7. In constructing the formulæ by which laws of nature are expressed, although the first object is to assign the Law of the Phenomena, philosophers have, in almost all cases, not proceeded in a purely empirical manner, to connect the observed numbers by some expression of calculation, but have been guided, in the selection of their formula, by some *Hypothesis* respecting the mode of connexion of the facts. Thus the formula of Dulong and Petit above given was suggested by the Theory of Exchanges; the first attempts at the resolution of the heavenly motions into circular functions were clothed in the hypothesis of Epicycles. And this was almost inevitable. 'We must confess,' says Copernicus[3], 'that the celestial motions are circular, or compounded of several circles, since their inequalities observe a fixed law, and recur in value at certain intervals, which could not be except they were circular: for a circle alone can make that quantity which has occurred recur again.' In like manner the first publication of the *Law of the Sines*, the true formula of optical refraction, was accompanied by Descartes with an hypothesis, in which an explanation of the law was pretended. In such cases, the mere comparison of observations may long fail in suggesting the true formulæ. The fringes of shadows and other diffracted colours were studied in vain by Newton, Gramaldi, Comparetti, the elder Herschel, and Mr. Brougham, so long as these inquirers attempted merely to trace the laws of the facts as they appeared in themselves; while Young, Fresnel, Fraunhofer, Schwerdt, and others, determined these laws in the most rigorous manner, when they applied to the observations the Hypothesis of Interferences.

8. But with all the aid that Hypotheses and Calculation can afford, the construction of true formulæ, in

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[3] *De Rev.* l. i. c. iv.

those cardinal discoveries by which the progress of science has mainly been caused, has been a matter of great labour and difficulty, and of good fortune added to sagacity. In the *History of Science*, we have seen how long and how hard Kepler laboured, before he converted the formula for the planetary motions, from an *epicyclical* combination, to a simple *ellipse*. The same philosopher, labouring with equal zeal and perseverance to discover the formula of optical refraction, which now appears to us so simple, was utterly foiled. Malus sought in vain the formula determining the Angle at which a transparent surface polarizes light: Sir D. Brewster[4], with a happy sagacity, discovered the formula to be simply this, that the *index* of refraction is the *tangent* of the angle of polarization.

Though we cannot give rules which will be of much service when we have thus to divine the general form of the relation by which phenomena are connected, there are certain methods by which, in a narrower field, our investigations may be materially promoted;—certain special methods of obtaining Laws from Observations. Of these we shall now proceed to treat.

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[4] *Hist. Ind. Sc.* b. ix. c. vi.

CHAPTER VII.

SPECIAL METHODS OF INDUCTION APPLICABLE TO QUANTITY.

APHORISM XLIII.

There are special Methods of Induction applicable to Quantity; of which the principal are, the Method of Curves, *the* Method of Means, *the* Method of Least Squares, *and the* Method of Residues.

APHORISM XLIV.

The Method of Curves *consists in drawing a curve, of which the observed quantities are the Ordinates, the quantity on which the change of these quantities depends being the Abscissa. The efficacy of this Method depends upon the faculty which the eye possesses, of readily detecting regularity and irregularity in forms. The Method may be used to detect the Laws which the observed quantities follow; and also, when the Observations are inexact, it may be used to correct these Observations, so as to obtain data more true than the observed facts themselves.*

APHORISM XLV.

The Method of Means *gets rid of irregularities by taking the arithmetical mean of a great number of observed quantities. Its efficacy depends upon this; that in cases in which observed quantities are affected by other inequalities, besides that of which we wish to determine the law, the excesses* above *and defects* below *the quantities which the law in question would produce, will, in a collection of* many *observations,* balance *each other.*

APHORISM XLVI.

The Method of Least Squares *is a Method of Means, in which the mean is taken according to the condition, that the sum of the squares of the errours of observation shall be the least possible which the law of the facts allows. It appears, by the Doctrine of Chances, that this is the* most probable *mean.*

APHORISM XLVII.

The Method of Residues *consists in subtracting, from the quantities given by Observation, the quantity given by any Law already discovered; and then examining the remainder, or* Residue, *in order to discover the leading Law which it follows. When this second Law has been discovered, the quantity given by it may be subtracted from the first Residue; thus giving a* Second Residue, *which may be examined in the same manner; and so on. The efficacy of this method depends principally upon the circumstance of the Laws of variation being successively smaller and smaller in amount (or at least in their mean effect); so that the ulterior undiscovered Laws do not prevent the Law in question from being* prominent *in the observations.*

APHORISM XLVIII.

The Method of Means and the Method of Least Squares cannot be applied without our knowing the Arguments *of the Inequalities which we seek. The Method of Curves and the Method of Residues, when the Arguments of the principal Inequalities are known, often make it easy to find the others.*

IN cases where the phenomena admit of numerical measurement and expression, certain mathematical methods may be employed to facilitate and give accuracy to the determination of the formula by which the observations are connected into laws. Among the most usual and important of these Methods are the following:—

I. The Method of Curves.
II. The Method of Means.
III. The Method of Least Squares.
IV. The Method of Residues.

SECT. I.—*The Method of Curves.*

1. THE Method of Curves proceeds upon this basis; that when one quantity undergoes a series of changes depending on the progress of another quantity, (as, for instance, the Deviation of the Moon from her equable place depends upon the progress of Time,) this dependence may be expressed by means of a *curve.* In the language of mathematicians, the variable quantity, whose changes we would consider, is made the *ordinate* of the curve, and the quantity on which the changes depend is made the *abscissa.* In this manner, the curve will exhibit in its form a series of undulations, rising and falling so as to correspond with the alternate Increase and Diminution of the quantity represented, at intervals of Space which correspond to the intervals of Time, or other quantity by which the changes are regulated. Thus, to take another example, if we set up, at equal intervals, a series of ordinates representing the Height of all the successive High Waters brought by the tides at a given place, for a year, the curve which connects the summits of all these ordinates will exhibit a series of undulations, ascending and descending once in about each Fortnight; since, in that interval, we have, in succession, the high spring tides and the low neap tides. The curve thus drawn offers to the eye a picture of the order and magnitude of the changes to which the quantity under contemplation, (the height of high water,) is subject.

2. Now the peculiar facility and efficacy of the Method of Curves depends upon this circumstance;—that order and regularity are more readily and clearly recognized, when thus exhibited to the eye in a picture, than they are when presented to the mind in any other manner. To detect the relations of Number considered directly as Number, is not easy: and we might

contemplate for a long time a Table of recorded Numbers without perceiving the order of their increase and diminution, even if the law were moderately simple; as any one may satisfy himself by looking at a Tide Table. But if these Numbers are expressed by the magnitude of *Lines*, and if these Lines are arranged in regular order, the eye readily discovers the rule of their changes: it follows the curve which runs along their extremities, and takes note of the order in which its convexities and concavities succeed each other, if any order be readily discoverable. The separate observations are in this manner compared and generalized and reduced to rule by the eye alone. And the eye, so employed, detects relations of order and succession with a peculiar celerity and evidence. If, for example, we thus arrange as ordinates the prices of corn in each year for a series of years, we shall see the order, rapidity, and amount of the increase and decrease of price, far more clearly than in any other manner. And if there were any recurrence of increase and decrease at stated intervals of years, we should in this manner perceive it. The eye, constantly active and busy, and employed in making into shapes the hints and traces of form which it contemplates, runs along the curve thus offered to it; and as it travels backwards and forwards, is ever on the watch to detect some resemblance or contrast between one part and another. And these resemblances and contrasts, when discovered, are the images of Laws of Phenomena; which are made manifest at once by this artifice, although the mind could not easily catch the indications of their existence, if they were not thus reflected to her in the clear mirror of Space.

Thus when we have a series of good Observations, and know the argument upon which their change of magnitude depends, the Method of Curves enables us to ascertain, almost at a glance, the law of the change; and by further attention, may be made to give us a formula with great accuracy. The Method enables us to perceive, among our observations, an order, which without the method, is concealed in obscurity and perplexity.

3. But the Method of Curves not only enables us to obtain laws of nature from *good* Observations, but also, in a great degree, from observations which are very *imperfect*. For the imperfection of observations may in part be corrected by this consideration;—that though they may appear irregular, the correct facts which they imperfectly represent, are really regular. And the Method of Curves enables us to remedy this apparent irregularity, at least in part. For when Observations thus imperfect are laid down as Ordinates, and their extremities connected by a line, we obtain, not a smooth and flowing curve, such as we should have if the observations contained only the rigorous results of regular laws; but a broken and irregular line, full of sudden and capricious twistings, and bearing on its face marks of irregularities dependent, not upon law, but upon chance. Yet these irregular and abrupt deviations in the curve are, in most cases, but small in extent, when compared with those bendings which denote the effects of regular law. And this circumstance is one of the great grounds of advantage in the Method of Curves. For when the observations thus laid down present to the eye such a broken and irregular line, we can still see, often with great ease and certainty, what twistings of the line are probably due to the irregular errours of observation; and can at once reject these, by drawing a more regular curve, cutting off all such small and irregular sinuosities, leaving some to the right and some to the left; and then proceeding as if this regular curve, and not the irregular one, expressed the observations. In this manner, we suppose the errours of observation to balance each other; some of our corrected measures being too great and others too small, but with no great preponderance either way. We draw our main regular curve, not *through* the points given by our observations, but *among* them: drawing it, as has been said by one of the philosophers[1] who first systematically used this method, 'with a bold but careful hand.'

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[1] Sir J. Herschel, *Ast. Soc. Trans.* vol. v. p. 1.

The regular curve which we thus obtain, thus freed from the casual errours of observation, is that in which we endeavour to discover the laws of change and succession.

4. By this method, thus getting rid at once, in a great measure, of errours of observation, we obtain data which are *more true than the* individual *facts themselves.* The philosopher's business is to compare his hypotheses with facts, as we have often said. But if we make the comparison with separate special facts, we are liable to be perplexed or misled, to an unknown amount, by the errours of observation; which may cause the hypothetical and the observed result to agree, or to disagree, when otherwise they would not do so. If, however, we thus take the *whole mass of the facts,* and remove the errours of actual observation[2], by making the curve which expresses the supposed observation regular and smooth, we have the separate facts corrected by their general tendency. We are put in possession, as we have said, of something more true than any fact by itself is.

One of the most admirable examples of the use of this Method of Curves is found in Sir John Herschel's *Investigation of the Orbits of Double Stars*[3]. The author there shows how far inferior the direct observations of the angle of position are, to the observations corrected by a curve in the manner above stated. 'This curve once drawn,' he says, 'must represent, it is evident, the law of variation of the angle of position, with the time, not only for instants intermediate between the dates of observations, but even at the moments of observation themselves, much better than the individual *raw* observations can possibly (on an average) do. It is only requisite to try a case or two, to be satisfied that by substituting the curve for the points, we have made a nearer approach to nature, and in a great measure eliminated errours of observation.' 'In following the graphical process,' he adds, 'we have a conviction almost approaching to moral certainty that

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[2] *Ib.* vol. v. p. 4. [3] *Ib*

we cannot be greatly misled.' Again, having thus corrected the raw observations, he makes another use of the graphical method, by trying whether an ellipse can be drawn 'if not *through*, at least *among* the points, so as to approach tolerably near them all; and thus approaching to the orbit which is the subject of investigation.'

5. The *Obstacles* which principally impede the application of the Method of Curves are (I.) our *ignorance of the argument* of the changes, and (II.) the *complication of several laws* with one another.

(I.) If we do not know on what quantity those changes depend which we are studying, we may fail entirely in detecting the law of the changes, although we throw the observations into curves. For the true *argument* of the change should, in fact, be made the *abscissa* of the curve. If we were to express, by a series of ordinates, the *hour* of high water on successive days, we should not obtain, or should obtain very imperfectly, the law which these times follow; for the real argument of this change is not the *solar hour*, but the *hour* at which the *moon* passes the meridian. But if we are supposed to be aware that *this* is the *argument*, (which theory suggests and trial instantly confirms) we then do immediately obtain the primary Rules of the Time of High Water, by throwing a series of observations into a Curve, with the Hour of the Moon's Transit for the abscissa.

In like manner, when we have obtained the first great or Semi-mensual Inequality of the tides, if we endeavour to discover the laws of other Inequalities by means of curves, we must take from theory the suggestion that the Arguments of such inequalities will probably be the *parallax* and the *declination* of the moon. This suggestion again is confirmed by trial; but if we were supposed to be entirely ignorant of the dependence of the changes of the tide on the Distance and Declination of the moon, the curves would exhibit unintelligible and seemingly capricious changes. For by the effect of the Inequality arising from the Parallax, the convexities of the curves which belong to the spring tides, are in some years made alternately greater and less all the year through; while in other years they are made all nearly equal. This difference does not betray its origin, till we refer it to the Parallax; and the same difficulty in proceeding would arise if we were ignorant that the moon's Declination is one of the Arguments of tidal changes.

In like manner, if we try to reduce to law any meteorological changes, those of the Height of the Barometer for instance, we find that we can make little progress in the investigation, precisely because we do not know the Argument on which these changes depend. That there is a certain regular *diurnal* change of small amount, we know; but when we have abstracted this Inequality, (of which the Argument is the *time of day*,) we find far greater Changes left behind, from day to day and from hour to hour; and we express these in curves, but we cannot reduce them to Rule, because we cannot discover on what numerical quantity they depend. The assiduous study of barometrical observations, thrown into curves, may perhaps hereafter point out to us what are the relations of time and space by which these variations are determined; but in the mean time, this subject exemplifies to us our remark, that the method of curves is of comparatively small use, so long as we are in ignorance of the real Arguments of the Inequalities.

6. (II.) In the next place, I remark that a difficulty is thrown in the way of the Method of Curves by *the Combination of several laws* one with another. It will readily be seen that such a cause will produce a complexity in the curves which exhibit the succession of facts. If, for example, we take the case of the Tides, the Height of high water increases and diminishes with the Approach of the sun to, and its Recess from, the syzygies of the moon. Again, this Height increases and diminishes as the moon's Parallax increases and diminishes; and again, the Height diminishes when the Declination increases, and *vice versa;* and all these Arguments of change, the Distance from Syzygy, the Parallax, the Declination, complete their circuit and

return into themselves in different periods. Hence the curve which represents the Height of high water has not any periodical interval in which it completes its changes and commences a new cycle. The sinuosity which would arise from each Inequality separately considered, interferes with, disguises, and conceals the others; and when we first cast our eyes on the curve of observation, it is very far from offering any obvious regularity in its form. And it is to be observed that we have not yet enumerated *all* the elements of this complexity: for there are changes of the tide depending upon the Parallax and Declination of the Sun as well as of the Moon. Again; besides these changes, of which the Arguments are obvious, there are others, as those depending upon the Barometer and the Wind, which follow no known regular law, and which constantly affect and disturb the results produced by other laws.

In the Tides, and in like manner in the motions of the Moon, we have very eminent examples of the way in which the discovery of laws may be rendered difficult by the number of laws which operate to affect the same quantity. In such cases, the Inequalities are generally picked out in succession, nearly in the order of their magnitudes. In this way there were successively collected, from the study of the Moon's motions by a series of astronomers, those Inequalities which we term the *Equation of the Center*, the *Evection*, the *Variation*, and the *Annual Equation*. These Inequalities were not, in fact, obtained by the application of the Method of Curves; but the Method of Curves might have been applied to such a case with great advantage. The Method has been applied with great industry and with remarkable success to the investigation of the laws of the Tides; and by the use of it, a series of Inequalities both of the Times and of the Heights of high water has been detected, which explain all the main features of the observed facts.

SECT. II.—*The Method of Means.*

7. The Method of Curves, as we have endeavoured to explain above, frees us from the casual and extraneous irregularities which arise from the imperfection of observation; and thus lays bare the results of the laws which really operate, and enables us to proceed in search of those laws. But the Method of Curves is not the only one which effects such a purpose. The errours arising from detached observations may be got rid of, and the additional accuracy which multiplied observations give may be obtained, by operations upon the observed numbers, without expressing them by spaces. The process of curves assumes that the errours of observation balance each other;—that the accidental excesses and defects are nearly equal in amount;—that the true quantities which would have been observed if all accidental causes of irregularity were removed, are obtained, exactly or nearly, by selecting quantities, upon the whole, equally distant from the extremes of great and small, which our imperfect observations offer to us. But when, among a number of unequal quantities, we take a quantity equally distant from the greater and the smaller, this quantity is termed the *Mean* of the unequal quantities. Hence the correction of our observations by the method of curves consists in taking the Mean of the observations.

8. Now without employing curves, we may proceed arithmetically to take the Mean of all the observed numbers of each class. Thus, if we wished to know the Height of the spring tide at a given place, and if we found that four different spring tides were measured as being of the height of ten, thirteen, eleven, and fourteen feet, we should conclude that the true height of the tide was the *Mean* of these numbers,—namely, twelve feet; and we should suppose that the deviation from this height, in the individual cases, arose from the accidents of weather, the imperfections of observation, or the operation of other laws, besides the alternation of spring and neap tides.

This process of finding the Mean of an assemblage of observed numbers is much practised in discovering, and still more in confirming and correcting, laws of phenomena. We shall notice a few of its peculiarities.

9. The Method of Means requires a knowledge of the *Argument* of the changes which we would study; for the numbers must be arranged in certain Classes, before we find the Mean of each Class; and the principle on which this arrangement depends is the Argument. This knowledge of the Argument is more indispensably necessary in the Method of Means than in the Method of Curves; for when Curves are drawn, the eye often spontaneously detects the law of recurrence in their sinuosities; but when we have collections of Numbers, we must divide them into classes by a selection of our own. Thus, in order to discover the law which the heights of the tide follow, in the progress from spring to neap, we arrange the observed tides according to the *day of the moon's age;* and we then take the mean of all those which thus happen at the *same period* of the Moon's Revolution. In this manner we obtain the law which we seek; and the process is very nearly the same in all other applications of this Method of Means. In all cases, we begin by assuming the Classes of measures which we wish to compare, the Law which we could confirm or correct, the Formula of which we would determine the coefficients.

10. The Argument being thus assumed, the Method of Means is very efficacious in ridding our inquiry of errours and irregularities which would impede and perplex it. Irregularities which are altogether accidental, or at least accidental with reference to some law which we have under consideration, compensate each other in a very remarkable way, when we take the Means of *many* observations. If we have before us a collection of observed tides, some of them may be elevated, some depressed by the wind, some noted too high and some too low by the observer, some augmented and some diminished by uncontemplated changes in the moon's distance or motion: but in the course of a year or two at the longest, all these causes of irregularity balance

each other; and the law of succession, which runs through the observations, comes out as precisely as if those disturbing influences did not exist. In any particular case, there appears to be no possible reason why the deviation should be in one way, or of one moderate amount, rather than another. But taking the mass of observations together, the deviations in opposite ways will be of equal amount, with a degree of exactness very striking. This is found to be the case in all inquiries where we have to deal with observed numbers upon a large scale. In the progress of the population of a country, for instance, what can appear more inconstant, in detail, than the causes which produce births and deaths? yet in each country, and even in each province of a country, the proportions of the whole numbers of births and deaths remain nearly constant. What can be more seemingly beyond the reach of rule than the occasions which produce letters that cannot find their destination? yet it appears that the number of 'dead letters' is nearly the same from year to year. And the same is the result when the deviations arise, not from mere accident, but from laws perfectly regular, though not contemplated in our investigation[4]. Thus the effects of the Moon's Parallax upon the Tides, sometimes operating one way and sometimes another, according to certain rules, are quite eliminated by taking the Means of a long series of observations; the excesses and defects neutralizing each other, so far as concerns the effect upon any law of the tides which we would investigate.

11. In order to obtain very great accuracy, very large masses of observations are often employed by philosophers, and the accuracy of the result increases with the multitude of observations. The immense collections of astronomical observations which have in this manner been employed in order to form and correct the Tables of the celestial motions are perhaps the most signal instances of the attempts to obtain

[4] Provided the argument of the law which we neglect have no coincidence with the argument of the law which we would determine.

accuracy by this accumulation of observations. Delambre's Tables of the Sun are founded upon nearly 3000 observations; Burg's Tables of the Moon upon above 4000.

But there are other instances hardly less remarkable. Mr. Lubbock's first investigations of the laws of the tides of London[5], included above 13,000 observations, extending through nineteen years; it being considered that this large number was necessary to remove the effects of accidental causes[6]. And the attempts to discover the laws of change in the barometer have led to the performance of labours of equal amount: Laplace and Bouvard examined this question by means of observations made at the Observatory of Paris, four times every day for eight years.

12. We may remark one striking evidence of the accuracy thus obtained by employing large masses of observations. In this way we may often detect inequalities much smaller than the errours by which they are encumbered and concealed. Thus the Diurnal Oscillations of the Barometer were discovered by the comparison of observations of many days, classified according to the hours of the day; and the result was a clear and incontestable proof of the existence of such oscillations, although the differences which these oscillations produce at different hours of the day are far smaller than the casual changes, hitherto reduced to no law, which go on from hour to hour and from day to day. The effect of law, operating incessantly and steadily, makes itself more and more felt as we give it a longer range; while the effect of accident, followed out in the

[5] *Phil. Trans.* 1831.

[6] This period of nineteen years was also selected for a reason which is alluded to in a former note. It was thought that this period secured the inquirer from the errours which might be produced by the partial coincidence of the Arguments of different irregularities; for example, those due to the moon's Parallax and to the moon's Declination. It has since been found (*Phil. Tr.* 1838. *On the Determination of the Laws of the Tides from Short Series of Observations*), that with regard to Parallax at least, the Means of one year give sufficient accuracy.

same manner, is to annihilate itself, and to disappear altogether from the result.

SECT. III.—*The Method of Least Squares.*

13. The Method of Least Squares is in fact a method of means, but with some peculiar characters. Its object is to determine the *best Mean* of a number of observed quantities; or the *most probable Law* derived from a number of observations, of which some, or all, are allowed to be more or less imperfect. And the method proceeds upon this supposition;—that all errours are not *equally* probable, but that small errours are more probable than large ones. By reasoning mathematically upon this ground, we find that the best result is obtained (since we cannot obtain a result in which the errours vanish) by making, not the *Errours* themselves, but the *Sum of their Squares* of the *smallest* possible amount.

14. An example may illustrate this. Let a quantity which is known to increase uniformly, (as the distance of a star from the meridian at successive instants,) be measured at equal intervals of time, and be found to be successively 4, 12, 14. It is plain, upon the face of these observations, that they are erroneous; for they ought to form an arithmetical progression, but they deviate widely from such a progression. But the question then occurs, what arithmetical progression do they *most probably* represent: for we may assume several arithmetical progressions which more or less approach the observed series; as for instance, these three; 4, 9, 14; 6, 10, 14; 5, 10, 15. Now in order to see the claims of each of these to the truth, we may tabulate them thus.

Observation	4, 12, 14	Errours.	Sums of Errours.	Sums of Squares of Errours.
Series (1)	4, 9, 14	0, 3, 0	3	9
„ (2)	6, 10, 14	2, 2, 0	4	8
„ (3)	5, 10, 15	1, 2, 1	4	6

Here, although the first series gives the sum of the errours less than the others, the third series gives the sum of the squares of the errours least; and is therefore, by the proposition on which this Method depends, the *most probable* series of the three.

This Method, in more extensive and complex cases, is a great aid to the calculator in his inferences from facts, and removes much that is arbitrary in the Method of Means.

SECT. IV.—*The Method of Residues.*

15. By either of the preceding Methods we obtain, from observed facts, such Laws as readily offer themselves; and by the Laws thus discovered, the most prominent changes of the observed quantities are accounted for. But in many cases we have, as we have noticed already, *several* Laws of nature operating at the same time, and combining their influences to modify those quantities which are the subjects of observation. In these cases we may, by successive applications of the Methods already pointed out, detect such Laws one after another: but this successive process, though only a repetition of what we have already described, offers some peculiar features which make it convenient to consider it in a separate Section, as the Method of Residues.

16. When we have, in a series of changes of a variable quantity, discovered *one* Law which the changes follow, detected its Argument, and determined its Magnitude, so as to explain most clearly the course of observed facts, we may still find that the observed changes are not fully accounted for. When we compare the results of our Law with the observations, there may be a difference, or as we may term it, a *Residue,* still unexplained. But this Residue being thus detached from the rest, may be examined and scrutinized in the same manner as the whole observed quantity was treated at first: and we may in this way detect in *it* also a Law of change. If we can do this, we must accommodate this new found Law as nearly as possible to the Residue to which it belongs; and

this being done, the difference of our Rule and of the Residue itself, forms a *Second Residue.* This Second Residue we may again bring under our consideration; and may perhaps in *it* also discover some Law of change by which its alterations may be in some measure accounted for. If this can be done, so as to account for a large portion of this Residue, the remaining unexplained part forms a *Third Residue;* and so on.

17. This course has really been followed in various inquiries, especially in those of Astronomy and Tidology. The *Equation of the Center*, for the Moon, was obtained out of the *Residue* of the Longitude, which remained when the *Mean Anomaly* was taken away. This Equation being applied and disposed of, the *Second Residue* thus obtained, gave to Ptolemy the *Evection.* The *Third Residue*, left by the Equation of the Center and the Evection, supplied to Tycho the *Variation* and the *Annual Equation.* And the Residue, remaining from these, has been exhausted by other Equations, of various arguments, suggested by theory or by observation. In this case, the successive generations of astronomers have gone on, each in its turn executing some step in this Method of Residues. In the examination of the Tides, on the other hand, this method has been applied systematically and at once. The observations readily gave the *Semimensual Inequality;* the *Residue* of this supplied the corrections due to the Moon's *Parallax* and *Declination;* and when these were determined, the *remaining Residue* was explored for the law of the Solar Correction.

18. In a certain degree, the Method of Residues and the Method of Means are *opposite* to each other. For the Method of Residues extricates Laws from their combination, *bringing them into view in succession;* while the Method of Means discovers each Law, not by bringing the others into view, but by *destroying their effect* through an accumulation of observations. By the Method of Residues we should *first* extract the Law of the Parallax Correction of the Tides, and *then*, from the Residue left by this, obtain the Declination Correction. But we might at once employ the Method

of Means, and put together all the cases in which the Declination was the same; not allowing for the Parallax in each case, but taking for granted that the Parallaxes belonging to the same Declination would neutralize each other; as many falling above as below the mean Parallax. In cases like this, where the Method of Means is not impeded by a partial coincidence of the Arguments of different unknown Inequalities, it may be employed with almost as much success as the Method of Residues. But still, when the Arguments of the Laws are clearly known, as in this instance, the Method of Residues is more clear and direct, and is the rather to be recommended.

19. If for example, we wish to learn whether the Height of the Barometer exerts any sensible influence on the Height of the Sea's Surface, it would appear that the most satisfactory mode of proceeding, must be to subtract, in the first place, what we know to be the effects of the Moon's Age, Parallax and Declination, and other ascertained causes of change; and to search in the *unexplained Residue* for the effects of barometrical pressure. The contrary course has, however, been adopted, and the effect of the Barometer on the ocean has been investigated by the direct application of the Method of Means, classing the observed heights of the water according to the corresponding heights of the Barometer without any previous reduction. In this manner, the suspicion that the tide of the sea is affected by the pressure of the atmosphere, has been confirmed. This investigation must be looked upon as a remarkable instance of the efficacy of the Method of Means, since the amount of the barometrical effect is much smaller than the other changes from among which it was by this process extricated. But an application of the Method of Residues would still be desirable on a subject of such extent and difficulty.

20. Sir John Herschel, in his *Discourse on the Study of Natural Philosophy* (Articles 158—161), has pointed out the mode of making discoveries by studying Residual Phenomena; and has given several illustrations of the process. In some of these, he has also

considered this method in a wider sense than we have done; treating it as not applicable to quantity only, but to properties and relations of different kinds.

We likewise shall proceed to offer a few remarks on Methods of Induction applicable to other relations than those of quantity.

CHAPTER VIII.

METHODS OF INDUCTION DEPENDING ON RESEMBLANCE.

APHORISM XLIX.

The Law of Continuity *is this:—that a quantity cannot pass from one amount to another by any change of conditions, without passing through all intermediate magnitudes according to the intermediate conditions. This Law may often be employed to disprove distinctions which have no real foundation.*

APHORISM L.

The Method of Gradation *consists in taking a number of stages of a property in question, intermediate between two extreme cases which appear to be different. This Method is employed to determine whether the extreme cases are really distinct or not.*

APHORISM LI.

The Method of Gradation, applied to decide the question, whether the existing geological *phenomena arise from existing causes, leads to this result:—That the phenomena* do *appear to arise from Existing Causes, but that the action of existing causes may, in past times, have transgressed, to any extent, their* recorded *limits of intensity.*

APHORISM LII.

The Method of Natural Classification *consists in classing cases, not according to any* assumed *Definition, but according to the connexion of the facts themselves, so as to make them the means of asserting general truths.*

SECT. I.—*The Law of Continuity.*

1. THE Law of Continuity is applicable to quantity primarily, and therefore might be associated with the methods treated of in the last chapter: but inasmuch as its inferences are made by a transition from one degree to another among contiguous cases, it will be found to belong more properly to the Methods of Induction of which we have now to speak.

The *Law of Continuity* consists in this proposition, —That a quantity cannot pass from one amount to another by any change of conditions, without passing through all intermediate degrees of magnitude according to the intermediate conditions. And this law may often be employed to correct inaccurate inductions, and to reject distinctions which have no real foundation in nature. For example, the Aristotelians made a distinction between motions according to nature, (as that of a body falling vertically downwards,) and motions contrary to nature, (as that of a body moving along a horizontal plane:) the former, they held, became naturally quicker and quicker, the latter naturally slower and slower. But to this it might be replied, that a horizontal line may pass, by gradual motion, through various inclined positions, to a vertical position: and thus the retarded motion may pass into the accelerated; and hence there must be some inclined plane on which the motion downwards is naturally uniform: which is false, and therefore the distinction of such kinds of motion is unfounded. Again, the proof of the First Law of Motion depends upon the Law of Continuity: for since, by diminishing the resistance to a body moving on a horizontal plane, we diminish the retardation, and this without limit, the law of continuity will bring us at the same time to the case of no resistance and to the case of no retardation.

2. The Law of Continuity is asserted by Galileo in a particular application; and the assertion which it

suggests is by him referred to Plato;—namely[1], that a moveable body cannot pass from rest to a determinate degree of velocity without passing through all smaller degrees of velocity. This law, however, was first asserted in a more general and abstract form by Leibnitz[2]: and was employed by him to show that the laws of motion propounded by Descartes must be false. The Third Cartesian Law of Motion was this[3]: that when one moving body meets another, if the first body have a less momentum than the second, it will be reflected with its whole motion: but if the first have a greater momentum than the second, it will lose a part of its motion, which it will transfer to the second. Now each of these cases leads, by the Law of Continuity, to the case in which the two bodies have *equal* momentums: but in this case, by the first part of the law the body would *retain all* its motion; and by the second part of the law it would *lose* a portion of it: hence the Cartesian Law is false.

3. I shall take another example of the application of this Law from Professor Playfair's Dissertation on the History of Mathematical and Physical Science[4]. 'The Academy of Sciences at Paris having (in 1724) proposed, as a Prize Question, the Investigation of the Laws of the Communication of Motion, John Bernoulli presented an Essay on the subject very ingenious and profound; in which, however, he denied the existence of hard bodies, because in the collision of such bodies, a finite change of motion must take place in an instant: an event which, on the principle just explained, he maintained to be impossible.' And this reasoning was justifiable: for we can form a *continuous* transition from cases in which the impact manifestly occupies a finite time, (as when we strike a large soft body) to cases in which it is apparently instantaneous. Maclaurin and others are disposed, in order to avoid the conclusion of Bernoulli, to reject the Law of Con-

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1 *Dialog.* iii. 150. iv. 32.

2 *Opera*, i. 366.

3 Cartes, *Prin.* p. 35.

4 In the *Encyc. Brit.* p. 537.

tinuity. This, however, would not only be, as Playfair says, to deprive ourselves of an auxiliary, commonly useful though sometimes deceptive; but what is much worse, to acquiesce in false propositions, from the want of clear and patient thinking. For the Law of Continuity, when rightly interpreted, is *never* violated in actual fact. There are not really any such bodies as have been termed *perfectly hard:* and if we approach towards such cases, we must learn the laws of motion which rule them by attending to the Law of Continuity, not by rejecting it.

4. Newton used the Law of Continuity to suggest, but not to prove, the doctrine of universal gravitation. Let, he said, a terrestrial body be carried as high as the moon: will it not still fall to the earth? and does not the moon fall by the same force[5]? Again: if any one says that there is a material ether which does not gravitate[6], this kind of matter, by condensation, may be gradually transmuted to the density of the most intensely gravitating bodies: and these gravitating bodies, by taking the internal texture of the condensed ether, may cease to gravitate; and thus the weight of bodies depends, not on their quantity of matter, but on their texture; which doctrine Newton conceived he had disproved by experiment.

5. The evidence of the Law of Continuity resides in the universality of those Ideas, which enter into our apprehension of Laws of Nature. When, of two quantities, one depends upon the other, the Law of Continuity necessarily governs this dependence. Every philosopher has the power of applying this law, in proportion as he has the faculty of apprehending the Ideas which he employs in his induction, with the same clearness and steadiness which belong to the fundamental ideas of Quantity, Space and Number. To those who possess this faculty, the Law is a Rule of very wide and decisive application. Its use, as has appeared in the above examples, is seen rather in the disproof of erroneous views, and in the correction of false propositions,

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[5] *Principia*, lib. iii. prop. 6. [6] *Ib.* cor. 2.

than in the invention of new truths. It is a test of truth, rather than an instrument of discovery.

Methods, however, approaching very near to the Law of Continuity may be employed as positive means of obtaining new truths; and these I shall now describe.

SECT. II.—*The Method of Gradation.*

6. To gather together the cases which resemble each other, and to separate those which are essentially distinct, has often been described as the main business of science; and may, in a certain loose and vague manner of speaking, pass for a description of some of the leading procedures in the acquirement of knowledge. The selection of instances which agree, and of instances which differ, in some prominent point or property, are important steps in the formation of science. But when classes of things and properties have been established in virtue of such comparisons, it may still be doubtful whether these classes are separated by distinctions of opposites, or by differences of degree. And to settle such questions, the *Method of Gradation* is employed; which consists in taking intermediate stages of the properties in question, so as to ascertain by experiment whether, in the transition from one class to another, we have to leap over a manifest gap, or to follow a continuous road.

7. Thus for instance, one of the early *Divisions* established by electrical philosophers was that of *Electrics* and *Conductors.* But this division Dr. Faraday has overturned as an essential opposition. He takes[7] a *Gradation* which carries him from Conductors to Non-conductors. Sulphur, or Lac, he says, are held to be non-conductors, but are not rigorously so. Spermaceti is a bad conductor: ice or water better than spermaceti: metals so much better that they are put in a different class. But even in metals the transit of the electricity is not instantaneous: we have in them proof of a retardation of the electric current: 'and what

[7] *Researches*, 12th series, art. 1328.

reason," Mr. Faraday asks, "why this retardation should not be of the same kind as that in spermaceti, or in lac, or sulphur? But as, in them, retardation is insulation, [and insulation is induction[8]] why should we refuse the same relation to the same exhibitions of force in the metals?"

The process employed by the same sagacious philosopher to show the *identity* of Voltaic and Franklinic electricity, is another example of the same kind[9]. Machine [Franklinic] electricity was made to exhibit the same phenomena as Voltaic electricity, by causing the discharge to pass through a bad conductor, into a very extensive discharging train: and thus it was clearly shown that Franklinic electricity, not so conducted, differs from the other kinds, only in being in a state of successive tension and explosion instead of a state of continued current.

Again; to show that the decomposition of bodies in the Voltaic circuit was not due to the *Attraction* of the Poles[10], Mr. Faraday devised a beautiful series of experiments, in which these supposed *Poles* were made to assume all possible electrical conditions:—in some cases the decomposition took place against air, which according to common language is not a conductor, nor is decomposed;—in others, against the metallic poles, which are excellent conductors but undecomposable;—and so on: and hence he infers that the decomposition cannot justly be considered as due to the Attraction, or Attractive Powers, of the Poles.

8. The reader of the *Novum Organon* may perhaps, in looking at such examples of the Rule, be reminded of some of Bacon's Classes of Instances, as his *instantiæ absentiæ in proximo*, and his *instantiæ migrantes*. But we may remark that Instances classed and treated as Bacon recommends in those parts of his work, could hardly lead to scientific truth. His

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[8] These words refer to another proposition, also established by the Method of Gradation.

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

[10] *Ibid. Researches*, art. 497.

processes are vitiated by his proposing to himself the *form* or *cause* of the property before him, as the object of his inquiry; instead of being content to obtain, in the first place, the *law of phenomena*. Thus his example[11] of a Migrating Instance is thus given. "Let the *Nature inquired into* be that of Whiteness; an Instance Migrating to the production of this property is glass, first whole, and then pulverized; or plain water, and water agitated into a foam; for glass and water are transparent, and not white; but glass powder and foam are white, and not transparent. Hence we must inquire what has happened to the glass or water in that Migration. For it is plain that the *Form of Whiteness* is conveyed and induced by the crushing of the glass and shaking of the water." No real knowledge has resulted from this line of reasoning:—from taking the Natures and Forms of things and of their qualities for the primary subject of our researches.

9. We may easily give examples from other subjects in which the Method of Gradation has been used to establish, or to endeavour to establish, very extensive propositions. Thus Laplace's Nebular Hypothesis,—that systems like our solar system are formed by gradual condensation from diffused masses, such as the nebulæ among the stars,—is founded by him upon an application of this Method of Gradation. We see, he conceives, among these nebulæ, instances of all degrees of condensation, from the most loosely diffused fluid, to that separation and solidification of parts by which suns, and satellites, and planets are formed: and thus we have before us instances of systems in all their stages; as in a forest we see trees in every period of growth. How far the examples in this case satisfy the demands of the Method of Gradation, it remains for astronomers and philosophers to examine.

Again; this method was used with great success by Macculloch and others to refute the opinion, put in currency by the Wernerian school of geologists, that

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11 *Nov. Org.* lib. ii. Aph. 28.

the rocks called *trap rocks* must be classed with those to which a *sedimentary* origin is ascribed. For it was shown that a gradual *transition* might be traced from those examples in which trap rocks most resembled stratified rocks, to the lavas which have been recently ejected from volcanoes: and that it was impossible to assign a different origin to one portion, and to the other, of this kind of mineral masses; and as the volcanic rocks were certainly not sedimentary, it followed, that the trap rocks were not of that nature.

Again; we have an attempt of a still larger kind made by Sir C. Lyell, to apply this Method of Gradation so as to disprove all distinction between the causes by which geological phenomena have been produced, and the causes which are now acting at the earth's surface. He has collected a very remarkable series of changes which have taken place, and are still taking place, by the action of water, volcanoes, earthquakes, and other terrestrial operations; and he conceives he has shown in these a *gradation* which leads, with no wide chasm or violent leap, to the state of things of which geological researches have supplied the evidence.

10. Of the value of this Method in geological speculations, no doubt can be entertained. Yet it must still require a grave and profound consideration, in so vast an application of the Method as that attempted by Sir C. Lyell, to determine what extent we may allow to the steps of our *gradation;* and to decide how far the changes which have taken place in distant parts of the series may exceed those of which we have historical knowledge, without ceasing to be of the *same kind.* Those who, dwelling in a city, see, from time to time, one house built and another pulled down, may say that such *existing causes*, operating through past time, sufficiently explain the existing condition of the city. Yet we arrive at important political and historical truths, by considering the *origin* of a city as an event of a *different order* from those daily changes. The causes which are now working to produce geological results, may be supposed to have been, at some former epoch, so far exaggerated in their operation, that the changes

should be paroxysms, not degrees;—that they should violate, not continue, the gradual series. And we have no kind of evidence whether the duration of our historical times is sufficient to give us a just measure of the limits of such degrees;—whether the terms which we have under our notice enable us to ascertain the average rate of progression.

11. The result of such considerations seems to be this:—that we may apply the Method of Gradation in the investigation of geological causes, provided we leave the Limits of the Gradation undefined. But, then, this is equivalent to the admission of the opposite hypothesis: for a continuity of which the successive intervals are not limited, is not distinguishable from discontinuity. The geological sects of recent times have been distinguished as *uniformitarians* and *catastrophists:* the Method of Gradation seems to prove the doctrine of the uniformitarians; but then, at the same time that it does this, it breaks down the distinction between them and the catastrophists.

There are other exemplifications of the use of gradations in Science which well deserve notice: but some of them are of a kind somewhat different, and may be considered under a separate head.

Sect. III. *The Method of Natural Classification.*

12. The Method of Natural Classification consists, as we have seen, in grouping together objects, not according to any selected properties, but according to their most important resemblances; and in combining such grouping with the assignation of certain marks of the classes thus formed. The examples of the successful application of this method are to be found in the Classificatory Sciences through their whole extent; as, for example, in framing the Genera of plants and animals. The same method, however, may often be extended to other sciences. Thus the classification of Crystalline Forms, according to their Degree of Symmetry, (which is really an important distinction,) as introduced by Mohs and Weiss, was a great improvement

upon Haüy's arbitrary division according to certain assumed primary forms. Sir David Brewster was led to the same distinction of crystals by the study of their optical properties; and the scientific value of the classification was thus strongly exhibited. Mr. Howard's classification of Clouds appears to be founded in their real nature, since it enables him to express the laws of their changes and successions. As we have elsewhere said, the criterion of a true classification is, that it makes general propositions possible. One of the most prominent examples of the beneficial influence of a right classification, is to be seen in the impulse given to geology by the distinction of strata according to the organic fossils which they contain[12]: which, ever since its general adoption, has been a leading principle in the speculations of geologists.

13. The mode in which, in this and in other cases, the Method of Natural Classification directs the researches of the philosopher, is this:—his arrangement being adopted, at least as an instrument of inquiry and trial, he follows the course of the different members of the classification, according to the guidance which Nature herself offers; not prescribing beforehand the marks of each part, but distributing the facts according to the total resemblances, or according to those resemblances which he finds to be most important. Thus, in tracing the course of a series of strata from place to place, we identify each stratum, not by any single character, but by all taken together;—texture, colour, fossils, position, and any other circumstances which offer themselves. And if, by this means, we come to ambiguous cases, where different indications appear to point different ways, we decide so as best to preserve undamaged those general relations and truths which constitute the value of our system. Thus although we consider the organic fossils in each stratum as its most important characteristic, we are not prevented, by the disappearance of some fossils, or the addition of others, or by the total absence of fossils,

[12] *Hist. Ind. Sc.* b. xviii. c. ii. sect. 3.

from identifying strata in distant countries, if the position and other circumstances authorize us to do so. And by this Method of Classification, the doctrine of *Geological Equivalents*[13] has been applied to a great part of Europe.

14. We may further observe, that the same method of natural classification which thus enables us to identify strata in remote situations, notwithstanding that there may be great differences in their material and contents, also forbids us to assume the identity of the series of rocks which occur in different countries, when this identity has not been verified by such a continuous exploration of the component members of the series. It would be in the highest degree unphilosophical to apply the special names of the English or German strata to the rocks of India, or America, or even of southern Europe, till it has appeared that in those countries the geological series of northern Europe really exists. In each separate country, the divisions of the formations which compose the crust of the earth must be made out, by applying the Method of Natural Arrangement *to that particular case*, and not by arbitrarily extending to it the nomenclature belonging to another case. It is only by such precautions, that we can ever succeed in obtaining geological propositions, at the same time true and comprehensive; or can obtain any sound general views respecting the physical history of the earth.

15. The Method of Natural Classification, which we thus recommend, falls in with those mental habits which we formerly described as resulting from the study of Natural History. The method was then termed the *Method of Type*, and was put in opposition to the *Method of Definition*.

The Method of Natural Classification is directly opposed to the process in which we assume and apply *arbitrary* definitions; for in the former Method, we find our classes in nature, and do not make them by marks of our own imposition. Nor can any advantage

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[13] *Hist. Ind. Sc.* b. xviii. c. iii. sect. 4.

to the progress of knowledge be procured, by laying down our characters when our arrangements are as yet quite loose and unformed. Nothing was gained by the attempts to *define* Metals by their weight, their hardness, their ductility, their colour; for to all these marks, as fast as they were proposed, exceptions were found, among bodies which still could not be excluded from the list of Metals. It was only when elementary substances were divided into *Natural Classes*, of which classes Metals were one, that a true view of their distinctive characters was obtained. Definitions in the outset of our examination of nature are almost always, not only useless, but prejudicial.

16. When we obtain a Law of Nature by induction from phenomena, it commonly happens, as we have already seen, that we introduce, at the same time, a Proposition and a Definition. In this case, the two are correlative, each giving a real value to the other. In such cases, also, the Definition, as well as the Proposition, may become the basis of rigorous reasoning, and may lead to a series of deductive truths. We have examples of such Definitions and Propositions in the Laws of Motion, and in many other cases.

17. When we have established Natural Classes of objects, we seek for Characters of our classes; and these Characters may, to a certain extent, be called the *Definitions* of our classes. This is to be understood, however, only in a limited sense: for these Definitions are not absolute and permanent. They are liable to be modified and superseded. If we find a case which manifestly belongs to our Natural Class, though violating our Definition, we do not shut out the case, but alter our definition. Thus, when we have made it part of our Definition of the *Rose* family, that they have *alternate stipulate leaves*, we do not, therefore, exclude from the family the genus *Lowœa*, which has *no stipulœ*. In Natural Classifications, our Definitions are to be considered as temporary and provisional only. When Sir C. Lyell established the distinctions of the tertiary strata, which he termed *Eocene*, *Miocene*, and *Pliocene*, he took a numerical criterion

(the proportion of recent species of shells contained in those strata) as the basis of his division. But now that those kinds of strata have become, by their application to a great variety of cases, a series of Natural Classes, we must, in our researches, keep in view the natural connexion of the formations themselves in different places; and must by no means allow ourselves to be governed by the numerical proportions which were originally contemplated; or even by any amended numerical criterion equally arbitrary; for however amended, Definitions in natural history are never immortal. The etymologies of *Pliocene* and *Miocene* may, hereafter, come to have merely an historical interest; and such a state of things will be no more inconvenient, provided the natural connexions of each class are retained, than it is to call a rock *oolite* or *porphyry* when it has no roelike structure and no fiery spots.

The Methods of Induction which are treated of in this and the preceding chapter, and which are specially applicable to causes governed by relations of Quantity or of Resemblance, commonly lead us to *Laws of Phenomena* only. Inductions founded upon other ideas, those of Substance and Cause for example, appear to conduct us somewhat further into a knowledge of the essential nature and real connexions of things. But before we speak of these, we shall say a few words respecting the way in which inductive propositions, once obtained, may be verified and carried into effect by their application.

CHAPTER IX.

OF THE APPLICATION OF INDUCTIVE TRUTHS.

APHORISM LIII.

When the theory of any subject is established, the observations and experiments which are made in applying the science to use and to instruction, supply a perpetual verification *of the theory.*

APHORISM LIV.

Such observations and experiments, when numerous and accurate, supply also corrections *of the* constants *involved in the theory; and sometimes, (by the Method of Residues,)* additions *to the theory.*

APHORISM LV.

It is worth considering, whether a continued and connected system of observation and calculation, like that of astronomy, might not be employed with advantage in improving our knowledge of other subjects; as Tides, Currents, Winds, Clouds, Rain, Terrestrial Magnetism, Aurora Borealis, Composition of Crystals, and many other subjects.

APHORISM LVI.

An extension *of a well-established theory to the explanation of new facts excites admiration as a discovery; but it is a discovery of a lower order than the theory itself.*

APHORISM LVII.

The practical inventions which are most important in Art may be either unimportant parts of Science, or results not explained by Science.

APHORISM LVIII.

In modern times, in many departments, Art is constantly guided, governed and advanced by Science.

APHORISM LIX.

Recently several New Arts have been invented, which may be regarded as notable verifications of the anticipations of material benefits to be derived to man from the progress of Science.

1. BY the application of inductive truths, we here mean, according to the arrangement given in chap. I. of this book, those steps, which in the natural order of science, follow the discovery of each truth. These steps are, the *verification* of the discovery by additional experiments and reasonings, and its *extension* to new cases, not contemplated by the original discoverer. These processes occupy that period, which, in the history of each great discovery, we have termed the *Sequel* of the epoch; as the collection of facts, and the elucidation of conceptions, form its Prelude.

2. It is not necessary to dwell at length on the processes of the Verification of Discoveries. When the Law of Nature is once stated, it is far easier to devise and execute experiments which prove it, than it was to discern the evidence before. The truth becomes one of the standard doctrines of the science to which it belongs, and is verified by all who study or who teach the science experimentally. The leading doctrines of Chemistry are constantly exemplified by each chemist in his *Laboratory;* and an amount of verification is thus obtained of which books give no adequate conception. In Astronomy, we have a still stronger example of the process of verifying discoveries. Ever since the science assumed a systematic form, there have been *Observatories,* in which the consequences of the theory were habitually compared with the results of observation. And to facilitate this comparison, *Tables* of great extent have been calculated, with immense labour, from each theory, showing the place which the

theory assigned to the heavenly bodies at successive times; and thus, as it were, challenging nature to deny the truth of the discovery. In this way, as I have elsewhere stated, the continued prevalence of an errour in the systematic parts of astronomy is impossible[1]. An errour, if it arise, makes its way into the tables, into the ephemeris, into the observer's nightly list, or his sheet of reductions; the evidence of sense flies in its face in a thousand Observatories; the discrepancy is traced to its source, and soon disappears for ever.

3. In these last expressions, we suppose the theory, not only to be tested, but also to be *corrected* when it is found to be imperfect. And this also is part of the business of the observing astronomer. From his accumulated observations, he deduces more exact values than had previously been obtained, of the *Constants* or *Coefficients* of these Inequalities of which the *Argument* is already known. This he is enabled to do by the methods explained in the fifth chapter of this book; the Method of Means, and especially the Method of Least Squares. In other cases, he finds, by the Method of Residues, some new Inequality; for if no change of the Coefficients will bring the Tables and the observation to a coincidence, he knows that a new Term is wanting in his formula. He obtains, as far as he can, the law of this unknown Term; and when its existence and its law have been fully established, there remains the task of tracing it to its cause.

4. The condition of the science of Astronomy, with regard to its security and prospect of progress, is one of singular felicity. It is a question well worth our consideration, as regarding the interests of science, whether, in other branches of knowledge also, *a continued and connected system of observation and calculation*, imitating the system employed by astronomers, might not be adopted. But the discussion of this question would involve us in a digression too wide for the present occasion.

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[1] *Hist. Ind. Sc.* b. vii. c. vi. sect. 6.

5. There is another mode of application of true theories after their discovery, of which we must also speak; I mean the process of showing that facts, not included in the original induction, and apparently of a different kind, are explained by reasonings founded upon the theory:—*extensions* of the theory as we may call them. The history of physical astronomy is full of such events. Thus after Bradley and Wargentin had observed a certain cycle among the perturbations of Jupiter's satellites, Laplace explained this cycle by the doctrine of universal gravitation[2]. The long inequality of Jupiter and Saturn, the diminution of the obliquity of the ecliptic, the acceleration of the moon's mean motion, were in like manner accounted for by Laplace. The coincidence of the nodes of the moon's equator with those of her orbit was proved to result from mechanical principles by Lagrange. The motions of the recently-discovered planets, and of comets, shown by various mathematicians to be in exact accordance with the theory, are Verifications and Extensions still more obvious.

6. In many of the cases just noticed, the consistency between the theory, and the consequences thus proved to result from it, is so far from being evident, that the most consummate command of all the powers and aids of mathematical reasoning is needed, to enable the philosopher to arrive at the result. In consequence of this circumstance, the labours just referred to, of Laplace, Lagrange, and others, have been the object of very great and very just admiration. Moreover, the necessary connexion of new facts, at first deemed inexplicable, with principles already known to be true;—a connexion utterly invisible at the outset, and yet at last established with the certainty of demonstration;—strikes us with the delight of a new discovery; and at first sight appears no less admirable than an original induction. Accordingly, men sometimes appear tempted to consider Laplace and other great mathematicians as persons of a kindred genius to Newton. We must not

[2] *Hist. Ind. Sc.* b. vii. c. iv. sect. 3.

forget, however, that there is a great and essential difference between inductive and deductive processes of the mind. The discovery of a *new* theory, which is true, is a step widely distinct from any mere development of the consequences of a theory already invented and established.

7. In the other sciences also, which have been framed by a study of natural phenomena, we may find examples of the explanation of new phenomena by applying the principles of the science when once established. Thus, when the laws of the reflection and refraction of light had been established, a new and poignant exemplification of them was found in the explanation of the Rainbow by the reflection and refraction of light in the spherical drops of a shower; and again, another, no less striking, when the intersecting Luminous Circles and Mock Suns, which are seen in cold seasons, were completely explained by the hexagonal crystals of ice which float in the upper regions of the atmosphere. The Darkness of the space between the primary and secondary rainbow is another appearance which optical theory completely explains. And when we further include in our optical theory the doctrine of interferences, we find the explanation of other phenomena; for instance, the Supernumerary Rainbows which accompany the primary rainbow on its inner side, and the small Halos which often surround the sun and moon. And when we come to optical experiments, we find many instances in which the doctrine of interferences and of undulations have been applied to explain the phenomena by calculations almost as complex as those which we have mentioned in speaking of astronomy: with results as little foreseen at first and as entirely satisfactory in the end. Such are Schwerdt's explanation of the diffracted images of a triangular aperture by the doctrine of interferences, and the explanation of the coloured Lemniscates seen by polarized light in biaxal crystals, given by Young and by Herschel: and still more marked is another case, in which the curves are unsymmetrical, namely, the curves seen by passing polarized

light through plates of quartz, which agree in a wonderful manner with the calculations of Airy. To these we may add the curious phenomena, and equally curious mathematical explanation, of Conical Refraction, as brought to view by Professor Lloyd and Sir W. Hamilton. Indeed, the whole history both of Physical Optics and of Physical Astronomy is a series of *felicities* of this kind, as we have elsewhere observed. Such applications of theory, and unforeseen explanations of new facts by complicated trains of reasoning necessarily flowing from the theory, are strong proofs of the truth of the theory, while it is in the course of being established; but we are here rather speaking of them as applications of the theory after it has been established.

Those who thus apply principles already discovered are not to be ranked in their intellectual achievements with those who discover new principles; but still, when such applications are masked by the complex relations of space and number, it is impossible not to regard with admiration the clearness and activity of intellect which thus discerns in a remote region the rays of a central truth already unveiled by some great discoverer.

8. As examples in other fields of the application of a scientific discovery to the explanation of natural phenomena, we may take the identification of Lightning with electricity by Franklin, and the explanation of Dew by Wells. For Wells's *Inquiry into the Cause of Dew*, though it has sometimes been praised as an original discovery, was, in fact, only resolving the phenomenon into principles already discovered. The atmologists of the last century were aware[3] that the vapour which exists in air in an invisible state may be condensed into water by cold; and they had noticed that there is always a certain temperature, lower than that of the atmosphere, to which if we depress bodies, water forms upon them in fine drops. This temperature is the limit of that which is neces-

[3] *Hist. Ind. Sc.* b. x. c. iii. sect. 5.

sary to constitute vapour, and is hence called the *constituent temperature.* But these principles were not generally familiar in England till Dr. Wells introduced them into his *Essay on Dew*, published in 1814; having indeed been in a great measure led to them by his own experiments and reasonings. His explanation of Dew,—that it arises from the coldness of the bodies on which it settles,—was established with great ingenuity; and is a very elegant confirmation of the Theory of Constituent Temperature.

9. As other examples of such explanations of new phenomena by a theory, we may point out Ampère's Theory that Magnetism is transverse voltaic currents, applied to explain the rotation of a voltaic wire round a magnet, and of a magnet round a voltaic wire. And again, in the same subject, when it had been proved that electricity might be converted into magnetism, it seemed certain that magnetism might be converted into electricity; and accordingly Faraday found under what conditions this may be done; though indeed here, the theory rather suggested the experiment than explained it when it had been independently observed. The production of an electric spark by a magnet was a very striking exemplification of the theory of the identity of these different polar agencies.

10. In Chemistry such applications of the principles of the science are very frequent; for it is the chemist's business to account for the innumerable changes which take place in material substances by the effects of mixture, heat, and the like. As a marked instance of such an application of the science, we may take the explanation of the explosive force of gunpowder[4], from the conversion of its materials into gases. In Mineralogy also we have to apply the

[4] The explanation is, that the force is due to the sudden development of a large volume of nitrogen and carbonic acid gases, which at the ordinary temperature of the air would occupy a space equal to about 300 times the bulk of the powder used, but from the intense heat developed at the moment of the explosion, the dilatation amounts to at least 1500 times the volume of the gunpowder employed.

principles of Chemistry to the analysis of bodies: and I may mention, as a case which at the time excited much notice, the analysis of a mineral called Heavy Spar. It was found that different specimens of this mineral differed in their crystalline angles about three degrees and a half; a difference which was at variance with the mineralogical discovery then recently made, of the constancy of the angle of the same substance. Vauquelin solved this difficulty by discovering that the crystals with the different angles were really minerals chemically different; the one kind being sulphate of barytes, and the other, sulphate of strontian.

11. In this way a scientific theory, when once established, is perpetually finding new applications in the phenomena of nature; and those who make such applications, though, as we have said, they care not to be ranked with the great discoverers who establish theories new and true, often receive a more prompt and general applause than great discoverers do; because they have not to struggle with the perplexity and averseness which often encounter the promulgation of new truths.

12. Along with the verification and extension of scientific truths, we are naturally led to consider the useful application of them. The example of all the best writers who have previously treated of the philosophy of sciences, from Bacon to Herschel, draws our attention to those instances of the application of scientific truths, which are subservient to the uses of practical life; to the support, the safety, the pleasure of man. It is well known in how large a degree the furtherance of these objects constituted the merit of the *Novum Organon* in the eyes of its author; and the enthusiasm with which men regard these visible and tangible manifestations of the power and advantage which knowledge may bring, has gone on increasing up to our own day. And undoubtedly such applications of the discoveries of science to promote the preservation, comfort, power and dignity of man, must always be objects of great philosophical as well as practical interest. Yet we may observe that those

practical inventions which are of most importance in the Arts, have not commonly, in the past ages of the world, been the results of theoretical knowledge, nor have they tended very greatly to the promotion of such knowledge. The use of bread and of wine has existed from the first beginning of man's social history; yet men have not had—we may question whether they yet have—a satisfactory theory of the constitution and fabrication of bread and of wine. From a very early period there have been workers in metal: yet who could tell upon what principles depended the purifying of gold and silver by the fire, or the difference between iron and steel? In some cases, as in the story of the brass produced by the Corinthian conflagration, some particular step in art is ascribed to a special accident; but hardly ever to the thoughtful activity of a scientific speculator. The Dyeing of cloths, the fabrication and colouring of earthenware and glass vessels was carried to a very high degree of completeness; yet who had any sound theoretical knowledge respecting these processes? Are not all these arts still practised with a degree of skill which we can hardly or not at all surpass, by nations which have, properly speaking, no science? Till lately, at least, if even now the case be different, the operations by which man's comforts, luxuries, and instruments were produced, were either mere practical processes, which the artist practises, but which the scientist cannot account for; or, as in astronomy and optics, they depended upon a small portion only of the theoretical sciences, and did not tend to illustrate, or lead to, any larger truths. Bacon mentions as recent discoveries, which gave him courage and hope with regard to the future progress of human knowledge, the invention of gunpowder, glass, and printing, the introduction of silk, and the discovery of America. Yet which of these can be said to have been the results of a theoretical enlargement of human knowledge? except perhaps the discovery of the New World, which was in some degree the result of Columbus's conviction of the globular form of the earth. This, however, was not a recent, but a very ancient doctrine of all sound astronomers. And which of these discoveries has been the cause of a great enlargement of our theoretical knowledge?—except any one claims such a merit for the discovery of printing; in which sense the result is brought about in a very indirect manner, in the same way in which the progress of freedom and of religion may be ascribed as consequences to the same discovery. However great or striking, then, such discoveries have been, they have not, generally speaking, produced any marked advance of the Inductive Sciences in the sense in which we here speak of them. They have increased man's power, it may be: that is, his power of adding to his comforts and communicating with his fellow-men. But they have not necessarily or generally increased his theoretical knowledge. And, therefore, with whatever admiration we may look upon such discoveries as these, we are not to admire them as steps in Inductive Science.

And on the other hand, we are not to ask of Inductive Science, as a necessary result of her progress, such additions as these to man's means of enjoyment and action. It is said, with a feeling of triumph, that Knowledge is Power: but in whatever sense this may truly be said, we value Knowledge, not because it is Power but because it is Knowledge; and we estimate wrongly both the nature and the dignity of that kind of science with which we are here concerned, if we expect that every new advance in theory will forthwith have a market value:—that science will mark the birth of a new Truth with some new birthday present, such as a softer stuff to wrap our limbs, a brighter vessel to grace our table, a new mode of communication with our friends and the world, a new instrument for the destruction of our enemies, or a new region which may be the source of wealth and interest.

13. Yet though, as we have said, many of the most remarkable processes which we reckon as the triumphs of Art did not result from a previous progress of Science, we have, at many points of the history of Science, applications of new views, to enable man to *do* as well

as to *see*. When Archimedes had obtained clear views of the theory of machines, he forthwith expressed them in his bold practical boast; 'Give me whereon to stand, and I will move the earth.' And his machines with which he is said to have handled the Roman ships like toys, and his burning mirrors with which he is reported to have set them on fire, are at least possible applications of theoretical principles. When he saw the waters rising in the bath as his body descended, and rushed out crying, 'I have found the way;' what he had found was the solution of the practical question of the quantity of silver mixed with the gold of Hiero's crown. But the mechanical inventions of Hero of Alexandria, which moved by the force of air or of steam, probably involved no exact theoretical notions of the properties of air or of steam. He devised a toy which revolved by the action of steam; but by the force of steam exerted in issuing from an orifice, not by its pressure or condensation. And the Romans had no arts derived from science in addition to those which they inherited from the Greeks. They built aqueducts, not indeed through ignorance of the principles of hydrostatics, as has sometimes been said; for we, who know our hydrostatics, build aqueducts still; but their practice exemplified only Archimedean hydrostatics. Their clepsydras or water-clocks were adjusted by trial only. They used arches and vaults more copiously than the Greeks had done, but the principle of the arch appears, by the most recent researches, to have been known to the Greeks. Domes and groined arches, such as we have in the Pantheon and in the Baths of Caracalla, perhaps they invented; certainly they practised them on a noble scale. Yet this was rather practical skill than theoretical knowledge; and it was pursued by their successors in the middle ages in the same manner, as practical skill rather than theoretical knowledge. Thus were produced flying buttresses, intersecting pointed vaults, and the other wonders of mediæval architecture. The engineers of the fifteenth century, as Leonardo da Vinci, began to convert their practical into theoretical knowledge of Mechanics; but still clocks and watches, flying machines and printing presses involved no new mechanical principle.

14. But from this time the advances in Science generally produced, as their result, new inventions of a practical kind. Thus the doctrine of the weight of air led to such inventions as the barometer used as a Weather-glass, the Air-pump with its train of curious experiments, the Diving-Bell, the Balloon. The telescope was perhaps in some degree a discovery due to accident, but its principles had been taught by Roger Bacon, and still more clearly by Descartes. Newton invented a steady thermometer by attending to steady laws of nature. And in the case of the improvements of the steam engine made by Watt, we have an admirable example how superior the method of improving Art by Science is, to the blind gropings of mere practical habit.

Of this truth, the history of most of the useful arts in our time offers abundant proofs and illustrations. All improvements and applications of the forces and agencies which man employs for his purposes are now commonly made, not by blind trial, but with the clearest theoretical as well as practical insight which he can obtain, into the properties of the agents which he employs. In this way he has constructed, (using theory and calculation at every step of his construction,) steam engines, steam boats, screw-propellers, locomotive engines, railroads and bridges and structures of all kinds. Lightning-conductors have been improved and applied to the preservation of buildings, and especially of ships, with admirable effect, by Sir Wm. Snow Harris, an experimenter who has studied with great care the theory of electricity. The measurement of the quantity of oxygen, that is, of vital power, in air, has been taught by Cavendish, and by Dr Ure a skilful chemist of our time. Methods for measuring the bleaching power of a substance have been devised by eminent chemical philosophers, Gay Lussac and Mr Graham. Davy used his discoveries concerning the laws of flame in order to construct his Safety Lamp:—his discoveries concerning the galvanic battery in order to protect ships' bottoms from corrosion. The skilled geologist has repeatedly given to those who were about to dig for coal where it could have no geological place, advice which has saved them from ruinous expence. Sir Roderick Murchison, from geological evidence, declared the likelihood of gold being found abundantly in Australia, many years before the diggings began.

Even the subtle properties of light as shewn in the recent discoveries of its interference and polarization, have been applied to useful purposes. Young invented an *Eriometer*, an instrument which should measure the fineness of the threads of wool by the coloured fringes which they produce; and substances which it is important to distinguish in the manufacture of sugar, are discriminated by their effect in rotating the plane of polarization of light. One substance has been termed *Dextrin*, from its impressing a right-handed rotation on the plane of polarization.

And in a great number of Arts and Manufactures, the necessity of a knowledge of theory to the right conduct of practice is familiarly acknowledged and assumed. In the testing and smelting of metals, in the fabrication of soap, of candles, of sugar; in the dyeing and printing of woollen, linen, cotton and silken stuffs; the master manufacturer has always the scientific chemist at his elbow;—either a 'consulting chemist' to whom he may apply on a special occasion, (for such is now a regular profession;) or a chemist who day by day superintends, controls, and improves the processes which his workmen daily carry on. In these cases, though Art long preceded Science, Science now guides, governs and advances Art.

15. Other Arts and manufactures which have arisen in modern times have been new creations produced by Science, and requiring a complete acquaintance with scientific processes to conduct them effectually and securely. Such are the photographic Arts, now so various in their form; beginning with those which, from their authors, are called Daguerrotype and Talbotype. Such are the Arts of Electrotype modelling

and Electrotype plating. Such are the Arts of preparing fulminating substances; gun-cotton; fulminate of silver, and of mercury; and the application of those Arts to use, in the fabrication of percussion-caps for guns. Such is the Art of Electric Telegraphy, from its first beginning to its last great attempt, the electric cord which connects England and America. Such is the Art of imitating by the chemistry of the laboratory the vegetable chemistry of nature, and thus producing the flavour of the pear, the apple, the pine-apple, the melon, the quince. Such is the Art of producing in man a temporary insensibility to pain, which was effected first through the means of sulphuric ether by Dr Jackson of America, and afterwards through the use of chloroform by Dr Simpson of Edinburgh. In these cases and many others Science has endowed man with New Arts. And though even in these Arts, which are thus the last results of Science, there is much which Science cannot fully understand and explain; still, such cases cannot but be looked upon as notable verifications of the anticipations of those who in former times expected from the progress of Science a harvest of material advantages to man.

We must now conclude our task by a few words on the subject of inductions involving Ideas ulterior to those already considered.

CHAPTER X.

OF THE INDUCTION OF CAUSES.

APHORISM LX.

In the Induction of Causes *the principal Maxim is, that we must be careful to possess, and to apply, with perfect clearness, the Fundamental Idea on which the Induction depends.*

APHORISM LXI.

The Induction of Substance, of Force, of Polarity, go beyond mere laws of phenomena, and may be considered as the Induction of Causes.

APHORISM LXII.

The Cause of certain phenomena being inferred, we are led to inquire into the Cause of this Cause, which inquiry must be conducted in the same manner as the previous one; and thus we have the Induction of Ulterior Causes.

APHORISM LXIII.

In contemplating the series of Causes which are themselves the effects of other causes, we are necessarily led to assume a Supreme Cause in the Order of Causation, as we assume a First Cause in Order of Succession.

1. WE formerly[1] stated the objects of the researches of Science to be Laws of Phenomena and Causes; and showed the propriety and the necessity of not resting in the former object, but extending our

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[1] B. ii. c. vii.

inquiries to the latter also. Inductions, in which phenomena are connected by relations of Space, Time, Number and Resemblance, belong to the former class; and of the Methods applicable to such Inductions we have treated already. In proceeding to Inductions governed by any ulterior Ideas, we can no longer lay down any Special Methods by which our procedure may be directed. A few general remarks are all that we shall offer.

The principal Maxim in such cases of Induction is the obvious one:—that we must be careful to possess and to apply, with perfect clearness and precision, the Fundamental Idea on which the Induction depends.

We may illustrate this in a few cases.

2. *Induction of Substance.*—The Idea of Substance[2] involves this axiom, that the weight of the whole compound must be equal to the weights of the separate elements, whatever changes the composition or separation of the elements may have occasioned. The application of this Maxim we may term the *Method of the Balance.* We have seen[3] elsewhere how the memorable revolution in Chemistry, the overthrow of Phlogiston, and the establishment of the Oxygen Theory, was produced by the application of this Method. We have seen too[4] that the same Idea leads us to this Maxim;—that *Imponderable Fluids* are not to be admitted as *chemical* elements of bodies.

Whether those which have been termed *Imponderable Fluids,*—the supposed fluids which produce the phenomena of Light, Heat, Electricity, Galvanism, Magnetism,—really exist or no, is a question, not merely of the *Laws*, but of the *Causes* of Phenomena. It is, as has already been shown, a question which we cannot help discussing, but which is at present involved in great obscurity. Nor does it appear at all likely that we shall obtain a true view of the cause of Light, Heat, and Electricity, till we have discovered precise and general laws connecting optical, thermotical, and

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[2] *Hist. Sc. Ideas*, Book vi. c. iii. [3] *Ibid.* b. vi. c. iv. [4] *Ibid.*

electrical *phenomena* with those chemical doctrines to which the Idea of Substance is necessarily applied.

3. *Induction of Force.*—The inference of *Mechanical Forces* from phenomena has been so abundantly practised, that it is perfectly familiar among scientific inquirers. From the time of Newton, it has been the most common aim of mathematicians; and a persuasion has grown up among them, that mechanical forces, —attraction and repulsion,—are the only modes of action of the particles of bodies which we shall ultimately have to consider. I have attempted to show that this mode of conception is inadequate to the purposes of sound philosophy;—that the Particles of crystals, and the Elements of chemical compounds, must be supposed to be combined in some other way than by mere mechanical attraction and repulsion. Dr. Faraday has gone further in shaking the usual conceptions of the force exerted, in well-known cases. Among the most noted and conspicuous instances of attraction and repulsion exerted at a distance, were those which take place between electrized bodies. But the eminent electrician just mentioned has endeavoured to establish, by experiments of which it is very difficult to elude the weight, that the action in these cases does not take place at a distance, but is the result of a chain of intermediate particles connected at every point by forces of another kind.

4. *Induction of Polarity.*—The forces to which Dr. Faraday ascribes the action in these cases are *Polar Forces*[5]. We have already endeavoured to explain the Idea of Polar Forces; which implies[6] that at every point forces exactly equal act in opposite directions; and thus, in the greater part of their course, neutralize and conceal each other; while at the extremities of the line, being by some cause liberated, they are manifested, still equal and opposite. And the criterion by which this polar character of forces is recognized, is implied in the reasoning of Faraday, on the question of one or two electricities, of which we

[5] *Researches*, 12th series.

[6] B. v. c. i.

formerly spoke[7]. The maxim is this:—that in the action of polar forces, along with every manifestation of force or property, there exists a corresponding and simultaneous manifestation of an equal and opposite force or property.

5. As it was the habit of the last age to reduce all action to mechanical forces, the present race of physical speculators appears inclined to reduce all forces to polar forces. Mosotti has endeavoured to show that the positive and negative electricities pervade all bodies, and that gravity is only an apparent excess of one of the kinds over the other. As we have seen, Faraday has given strong experimental grounds for believing that the supposed remote actions of electrized bodies are really the effects of polar forces among contiguous particles. If this doctrine were established with regard to all electrical, magnetical, and chemical forces, we might ask, whether, while all other forces are polar, gravity really affords a single exception to the universal rule? Is not the universe pervaded by an omnipresent antagonism, a fundamental conjunction of contraries, everywhere opposite, nowhere independent? We are, as yet, far from the position in which Inductive Science can enable us to answer such inquiries.

6. *Induction of Ulterior Causes.*—The first Induction of a Cause does not close the business of scientific inquiry. Behind proximate causes, there are ulterior causes, perhaps a succession of such. Gravity is the cause of the motions of the planets; but what is the cause of gravity? This is a question which has occupied men's minds from the time of Newton to the present day. Earthquakes and volcanoes are the causes of many geological phenomena; but what is the cause of those subterraneous operations? This inquiry after ulterior causes is an inevitable result from the intellectual constitution of man. He discovers mechanical causes, but he cannot rest in them. He must needs ask, whence it is that matter has its universal power of attracting matter. He discovers polar forces: but even

[7] Book v. c. i.

if these be universal, he still desires a further insight into the cause of this polarity. He sees, in organic structures, convincing marks of adaptation to an end: whence, he asks, is this adaptation? He traces in the history of the earth a chain of causes and effects operating through time: but what, he inquires, is the power which holds the end of this chain?

Thus we are referred back from step to step, in the order of causation, in the same manner as, in the palætiological sciences, we were referred back in the order of time. We make discovery after discovery in the various regions of science; each, it may be, satisfactory, and in itself complete, but none final. Something always remains undone. The last question answered, the answer suggests still another question. The strain of music from the lyre of Science flows on, rich and sweet, full and harmonious, but never reaches a close: no cadence is heard with which the intellectual ear can feel satisfied.

Of the Supreme Cause.—In the utterance of Science, no cadence is heard with which the human mind can feel satisfied. Yet we cannot but go on listening for and expecting a satisfactory close. The notion of a cadence appears to be essential to our relish of the music. The idea of some closing strain seems to lurk among our own thoughts, waiting to be articulated in the notes which flow from the knowledge of external nature. The idea of something ultimate in our philosophical researches, something in which the mind can acquiesce, and which will leave us no further questions to ask, of *whence*, and *why*, and *by what power*, seems as if it belonged to us;—as if we could not have it withheld from us by any imperfection or incompleteness in the actual performances of science. What is the meaning of this conviction? What is the reality thus anticipated? Whither does the developement of this Idea conduct us?

We have already seen that a difficulty of the same kind, which arises in the contemplation of causes and effects considered as forming an historical series, drives us to the assumption of a First Cause, as an Axiom to which our Idea of Causation in time necessarily leads. And as we were thus guided to a First Cause, in order of Succession, the same kind of necessity directs us to a Supreme Cause in order of Causation.

On this most weighty subject it is difficult to speak fitly; and the present is not the proper occasion, even for most of that which may be said. But there are one or two remarks which flow from the general train of the contemplations we have been engaged in, and with which this Work must conclude.

We have seen how different are the kinds of cause to which we are led by scientific researches. *Mechanical Forces* are insufficient without *Chemical Affinities;* Chemical Agencies fail us, and we are compelled to have recourse to *Vital Powers;* Vital Powers cannot be merely physical, and we must believe in something hyperphysical, something of the nature of a *Soul.* Not only do biological inquiries lead us to assume an animal soul, but they drive us much further; they bring before us *Perception,* and *Will* evoked by Perception. Still more, these inquiries disclose to us *Ideas* as the necessary forms of Perception, in the actions of which we ourselves are conscious. We are aware, we cannot help being aware, of our Ideas and our Volitions as belonging to *us,* and thus we pass from *things* to *persons;* we have the idea of *Personality* awakened. And the idea of Design and *Purpose,* of which we are conscious in our own minds, we find reflected back to us, with a distinctness which we cannot overlook, in all the arrangements which constitute the frame of organized beings.

We cannot but reflect how widely diverse are the kinds of principles thus set before us;—by what vast strides we mount from the lower to the higher, as we proceed through that series of causes which the range of the sciences thus brings under our notice. Yet we know how narrow is the range of these sciences when compared with the whole extent of human knowledge. We cannot doubt that on many other subjects, besides those included in physical speculation, man has made out solid and satisfactory trains of connexion;—

has discovered clear and indisputable evidence of causation. It is manifest, therefore, that, if we are to attempt to ascend to the Supreme Cause—if we are to try to frame an idea of the Cause of all these subordinate causes;—we must conceive it as more different from any of them, than the most diverse are from each other;—more elevated above the highest, than the highest is above the lowest.

But further;—though the Supreme Cause must thus be inconceivably different from all subordinate causes, and immeasurably elevated above them all, it must still include in itself all that is essential to each of them, by virtue of that very circumstance that it is the Cause of their Causality. Time and Space,—Infinite Time and Infinite Space,—must be among its attributes; for we cannot but conceive Infinite Time and Space as attributes of the Infinite Cause of the Universe. Force and Matter must depend upon it for their efficacy; for we cannot conceive the activity of Force, or the resistance of Matter, to be independent powers. But these are its lower attributes. The Vital Powers, the Animal Soul, which are the Causes of the actions of living things, are only the Effects of the Supreme Cause of Life. And this Cause, even in the lowest forms of organized bodies, and still more in those which stand higher in the scale, involves a reference to Ends and Purposes, in short, to manifest Final Causes. Since this is so, and since, even when we contemplate ourselves in a view studiously narrowed, we still find that we have Ideas, and Will and Personality, it would render our philosophy utterly incoherent and inconsistent with itself, to suppose that Personality, and Ideas, and Will, and Purpose, do not belong to the Supreme Cause from which we derive all that we have and all that we are.

But we may go a step further;—though, in our present field of speculation, we confine ourselves to knowledge founded on the facts which the external world presents to us, we cannot forget, in speaking of such a theme as that to which we have thus been led, that these are but a small, and the least significant portion of the facts which bear upon it. We cannot fail to recollect that there are facts belonging to the world within us, which more readily and strongly direct our thoughts to the Supreme Cause of all things. We can plainly discern that we have Ideas elevated above the region of mechanical causation, of animal existence, even of mere choice and will, which still have a clear and definite significance, a permanent and indestructible validity. We perceive as a fact, that we have a Conscience, judging of Right and Wrong; that we have Ideas of Moral Good and Evil; that we are compelled to conceive the organization of the moral world, as well as of the vital frame, to be directed to an end and governed by a purpose. And since the Supreme Cause is the cause of these facts, the Origin of these Ideas, we cannot refuse to recognize Him as not only the Maker, but the Governor of the World; as not only a Creative, but a Providential Power; as not only a Universal Father, but an Ultimate Judge.

We have already passed beyond the boundary of those speculations which we proposed to ourselves as the basis of our conclusions. Yet we may be allowed to add one other reflection. If we find in ourselves Ideas of Good and Evil, manifestly bestowed upon us to be the guides of our conduct, which guides we yet find it impossible consistently to obey;—if we find ourselves directed, even by our natural light, to aim at a perfection of our moral nature from which we are constantly deviating through weakness and perverseness; if, when we thus lapse and err, we can find, in the region of human philosophy, no power which can efface our aberrations, or reconcile our actual with our ideal being, or give us any steady hope and trust with regard to our actions, after we have thus discovered their incongruity with their genuine standard;—if we discern that this is our condition, how can we fail to see that it is in the highest degree consistent with all the indications supplied by such a philosophy as that of which we have been attempting to lay the foundations, that the Supreme Cause, through whom man exists as a moral being of vast capacities and infinite hopes, should have Himself provided a teaching for our ignorance, a propitiation for our sin, a support for our weakness, a purification and sanctification of our nature?

And thus, in concluding our long survey of the grounds and structure of science, and of the lessons which the study of it teaches us, we find ourselves brought to a point of view in which we can cordially sympathize, and more than sympathize, with all the loftiest expressions of admiration and reverence and hope and trust, which have been uttered by those who in former times have spoken of the elevated thoughts to which the contemplation of the nature and progress of human knowledge gives rise. We can not only hold with Galen, and Harvey, and all the great physiologists, that the organs of animals give evidence of a purpose;—not only assert with Cuvier that this conviction of a purpose can alone enable us to understand every part of every living thing;—not only say with Newton that 'every true step made in philosophy brings us nearer to the First Cause, and is on that account highly to be valued;'—and that 'the business of natural philosophy is to deduce causes from effects, till we come to the very First Cause, which certainly is not mechanical:'—but we can go much further, and declare, still with Newton, that 'this beautiful system could have its origin no other way than by the purpose and command of an intelligent and powerful Being, who governs all things, not as the soul of the world, but as the Lord of the Universe; who is not only God, but Lord and Governor.'

When we have advanced so far, there yet remains one step. We may recollect the prayer of one, the master in this school of the philosophy of science: 'This also we humbly and earnestly beg;—that human things may not prejudice such as are divine;—neither that from the unlocking of the gates of sense, and the kindling of a greater natural light, anything may arise of incredulity or intellectual night towards divine mysteries; but rather that by our minds thoroughly purged and cleansed from fancy and vanity, and yet subject and perfectly given up to the divine oracles, there may be given unto faith the things that are faith's.' When we are thus prepared for a higher teaching, we may be ready to listen to a greater than Bacon, when he says to those who have sought their God in the material universe, 'Whom ye ignorantly worship, him declare I unto you.' And when we recollect how utterly inadequate all human language has been shown to be, to express the nature of that Supreme Cause of the Natural, and Rational, and Moral, and Spiritual world, to which our Philosophy points with trembling finger and shaded eyes, we may receive, with the less wonder but with the more reverence, the declaration which has been vouchsafed to us:

ΕΝ ΑΡΧΗ ΗΝ 'Ο ΛΟΓΟΣ, ΚΑΙ 'Ο ΛΟΓΟΣ ΗΝ ΠΡΟΣ ΤΟΝ ΘΕΟΝ, ΚΑΙ ΘΕΟΣ ΗΝ 'Ο ΛΟΓΟΣ.

NOVUM ORGANON RENOVATUM.

BOOK IV.

OF THE LANGUAGE OF SCIENCE.

INTRODUCTION.

IT has been shown in the *History of the Sciences*, and has further appeared in the course of the *History of Ideas*, that almost every step in the progress of science is marked by the formation or appropriation of a technical term. Common language has, in most cases, a certain degree of looseness and ambiguity; as common knowledge has usually something of vagueness and indistinctness. In common cases too, knowledge usually does not occupy the intellect alone, but more or less interests some affection, or puts in action the fancy; and common language, accommodating itself to the office of expressing such knowledge, contains, in every sentence, a tinge of emotion or of imagination. But when our knowledge becomes perfectly exact and purely intellectual, we require a language which shall also be exact and intellectual;—which shall exclude alike vagueness and fancy, imperfection and superfluity;—in which each term shall convey a meaning steadily fixed and rigorously limited. Such a language that of science becomes, through the use of Technical Terms. And we must now endeavour to lay down some maxims and suggestions, by attention to which Technical Terms may be better fitted to answer their purpose. In order to do this, we shall in the first place take a rapid survey of the manner in which Technical Terms have been employed from the earliest periods of scientific history.

The progress of the use of technical scientific language offers to our notice two different and successive periods; in the first of which, technical terms were formed casually, as convenience in each case prompted; while in the second period, technical language was constructed intentionally, with set purpose, with a regard to its connexion, and with a view of constructing a system. Though the casual and the systematic formation of technical terms cannot be separated by any precise date of time, (for at all periods some terms in some sciences have been framed unsystematically,) we may, as a general description, call the former the *Ancient* and the latter the *Modern* Period. In illustrating the two following Aphorisms, I will give examples of the course followed in each of these periods.

Aphorism I.

In the Ancient Period of Sciences, Technical Terms were formed in three different ways:—by appropriating common words and fixing their meaning;—by constructing terms containing a description;—by constructing terms containing reference to a theory.

The earliest sciences offer the earliest examples of technical terms. These are Geometry, Arithmetic, and Astronomy; to which we have soon after to add Harmonics, Mechanics, and Optics. In these sciences, we may notice the above-mentioned three different modes in which technical terms were formed.

I. The simplest and first mode of acquiring technical terms, is to take words current in common usage, and by rigorously defining or otherwise fixing their meaning, to fit them for the expression of scientific truths. In this manner almost all the fundamental technical terms of Geometry were formed. A *sphere*, a *cone*, a *cylinder*, had among the Greeks, at first, meanings less precise than those which geometers gave to these words, and besides the mere designation of form, implied some use or application. A *sphere* (σφαῖρα) was a hand-ball used in games; a *cone* (κῶνος) was a boy's spinning-top, or the crest of a helmet; a *cylinder* (κύλινδρος) was a roller; a *cube* (κύβος) was a die: till these words were adopted by the geometers, and made to signify among them pure modifications of space. So an *angle* (γωνία) was only a corner; a *point* (σημεῖον) was a signal; a *line* (γραμμή) was a mark; a *straight* line (εὐθεῖα) was marked by an adjective which at first meant only *direct*. A *plane* (ἐπίπεδον) is the neuter form of an adjective, which by its derivation means *on the ground*, and hence *flat*. In all these cases, the word adopted as a term of science has its sense rigorously fixed; and where the common use of the term is in any degree vague, its meaning may be modified at the same time that it is thus limited. Thus a *rhombus* (ῥόμβος) by its derivation, might mean any figure which is *twisted* out of a regular form; but it is confined by geometers to that figure which has four equal sides, its angles being oblique. In like manner, a *trapezium* (τραπέζιον) originally signifies a *table*, and thus might denote any form; but as the tables of the Greeks had one side shorter than the opposite one, such a figure was at first called a *trapezium*. Afterwards the term was made to signify any figure with four unequal sides; a name being more needful in geometry for this kind of figure than for the original form.

This class of technical terms, namely, words adopted from common language, but rendered precise and determinate for purposes of science, may also be exemplified in other sciences. Thus, as was observed in the early portion of the history of astronomy[1], a *day*, a *month*, a *year*, described at first portions of time marked by familiar changes, but afterwards portions determined by rigorous mathematical definitions. The conception of the heavens as a revolving sphere, is so obvious,

[1] *Hist. Ind. Sc.* b. iii. c. i.

that we may consider the terms which involve this conception as parts of common language; as the *pole* (πόλος); the *arctic circle*, which includes the stars that never set[2]; the *horizon* (ὁρίζων) a boundary, applied technically to the circle bounding the visible earth and sky. The *turnings of the sun* (τροπαὶ ἠελίοιο), which are mentioned by Hesiod, gave occasion to the term *tropics*, the circles at which the sun in his annual motion turns back from his northward or southward advance. The *zones* of the earth, (the *torrid*, *temperate*, and *frigid*;) the *gnomon* of a dial; the *limb* (or border) of the moon, or of a circular instrument, are terms of the same class. An *eclipse* (ἔκλειψις) is originally a deficiency or disappearance, and joined with the name of the luminary, an *eclipse of the sun* or *of the moon*, described the phenomenon; but when the term became technical, it sufficed, without addition, to designate the phenomenon.

In Mechanics, the Greeks gave a scientific precision to very few words: we may mention *weights* (βάρεα), the *arms of a lever* (μήχεα), its *fulcrum* (ὑπομόχλιον), and the verb *to balance* (ἰσοῤῥοπεῖν). Other terms which they used, as *momentum* (ῥοπή) and *force* (δύναμις), did not acquire a distinct and definite meaning till the time of Galileo, or later. We may observe that all abstract terms, though in their scientific application expressing mere conceptions, were probably at first derived from some word describing external objects. Thus the Latin word for force, *vis*, seems to be connected with a Greek word, ἶς, or Ϝὶς, which often has nearly the same meaning; but originally, as it would seem, signified a sinew or muscle, the obvious seat of animal strength.

In later times, the limitation imposed upon a word by its appropriation to scientific purposes, is often more marked than in the cases above described. Thus the *variation* is made to mean, in astronomy, the second inequality of the moon's motion; in magnetism, the *variation* signifies the angular deviation of the

[2] *Hist. Ast.* b. iii. c. i. sect. 8.

compass-needle from the north; in pure mathematics, the *variation* of a quantity is the formula which expresses the result of any small change of the most general kind. In like manner, *parallax* (παράλλαξις) denotes a *change* in general, but is used by astronomers to signify the change produced by the spectator's being removed from the center of the earth, his theoretical place, to the surface. *Alkali* at first denoted the ashes of a particular plant, but afterwards, all bodies having a certain class of chemical properties; and, in like manner, *acid*, the class opposed to alkali, was modified in signification by chemists, so as to refer no longer to the taste.

Words thus borrowed from common language, and converted by scientific writers into technical terms, have some advantages and some disadvantages. They possess this great convenience, that they are understood after a very short explanation, and retained in the memory without effort. On the other hand, they lead to some inconvenience; for since they have a meaning in common language, a careless reader is prone to disregard the technical limitation of this meaning, and to attempt to collect their import in scientific books, in the same vague and conjectural manner in which he collects the purpose of words in common cases. Hence the language of science, when thus resembling common language, is liable to be employed with an absence of that scientific precision which alone gives it value. Popular writers and talkers, when they speak of *force*, *momentum*, *action and reaction*, and the like, often afford examples of the inaccuracy thus arising from the scientific appropriation of common terms.

II. Another class of technical terms, which we find occurring as soon as speculative science assumes a distinct shape, consists of those which are intentionally constructed by speculators, and which contain some description or indication distinctive of the conception to which they are applied. Such are a *parallelogram* (παραλληλόγραμμον), which denotes a plane figure bounded by two pairs of parallel lines; a *parallelopiped* (παραλληλοπίπεδον), which signifies a solid figure bounded by three pairs of parallel planes. A *triangle* (τρίγωνος, *trigon*) and a *quadrangle* (τετράγωνος, *tetragon*) were perhaps words invented independently of the mathematicians: but such words extended to other cases, *pentagon*, *decagon*, *heccædecagon*, *polygon*, are inventions of scientific men. Such also are *tetrahedron*, *hexahedron*, *dodecahedron*, *tesseracontaoctohedron*, *polyhedron*, and the like. These words being constructed by speculative writers, explain themselves, or at least require only some conventional limitation, easily adopted. Thus *parallelogram* might mean a figure bounded by any number of sets of parallel lines, but it is conventionally restricted to a figure of *four* sides. So a *great circle* in a sphere means one which passes through the center of the sphere; and a *small circle* is any other. So in trigonometry, we have the *hypotenuse* (ὑποτεινοῦσα), or *subtending* line, to designate the line subtending an angle, and especially a right angle. In this branch of mathematics we have many invented technical terms; as *complement*, *supplement*, *cosine*, *cotangent*, a *spherical angle*, the *pole of a circle*, or of a sphere. The word *sine* itself appears to belong to the class of terms already described as scientific appropriations of common terms, although its origin is somewhat obscure.

Mathematicians were naturally led to construct these and many other terms by the progress of their speculations. In like manner, when astronomy took the form of a speculative science, words were invented to denote distinctly the conceptions thus introduced. Thus the sun's annual path among the stars, in which not only solar, but also all lunar eclipses occur, was termed the *ecliptic*. The circle which the sun describes in his diurnal motion, when the days and nights are equal, the Greeks called the *equidiurnal* (ἰσημερινὸς,) the Latin astronomers the *equinoctial*, and the corresponding circle on the earth was the *equator*. The ecliptic intersected the equinoctial in the *equinoctial points*. The *solstices* (in Greek, τροπαὶ) were the times when the sun arrested his motion northwards or southwards; and the *solstitial points* (τὰ τροπικὰ σημεῖα) were the places in the ecliptic where he then was. The name of *meridians* was given to circles passing through the poles of the equator; the *solstitial colure* (κόλουρος, curtailed), was one of these circles, which passes through the solstitial points, and is intercepted by the horizon.

We have borrowed from the Arabians various astronomical terms, as *Zenith, Nadir, Azimuth, Almacantar.* And these words, which among the Arabians probably belonged to the first class, of appropriated scientific terms, are for us examples of the second class, invented scientific terms; although they differ from most that we have mentioned, in not containing an etymology corresponding to their meaning in any language with which European cultivators of science are generally familiar. Indeed, the distinction of our two classes, though convenient, is in a great measure, casual. Thus most of the words we formerly mentioned, as *parallax, horizon, eclipse,* though appropriated technical terms among the Greeks, are to us invented technical terms.

In the construction of such terms as we are now considering, those languages have a great advantage which possess a power of forming words by composition. This was eminently the case with the Greek language; and hence most of the ancient terms of science in that language, when their origin is once explained, are clearly understood and easily retained. Of modern European languages, the German possesses the greatest facility of composition; and hence scientific authors in that language are able to invent terms which it is impossible to imitate in the other languages of Europe. Thus Weiss distinguishes his various systems of crystals as *zwei-und-zwei-gliedrig, ein-und-zwei-gliedrig, drey-und-drey-gliedrig, &c.,* (two-and-two-membered, one-and-two-membered, &c.) And Hessel, also a writer on crystallography, speaks of *doubly-one-membered edges, four-and-three spaced rays,* and the like.

How far the composition of words, in such cases, may be practised in the English language, and the general question, what are the best rules and artifices in such cases, I shall afterwards consider. In the mean time, I may observe that this list of invented technical terms might easily be much enlarged. Thus in harmonics we have the various intervals, as a *Fourth*, a *Fifth*, an *Octave*, (*Diatessaron*, *Diapente*, *Diapason*,) a *Comma*, which is the difference of a *Major* and *Minor Tone;* we have the various *Moods* or *Keys*, and the notes of various lengths, as *Minims*, *Breves*, *Semibreves*, *Quavers*. In chemistry, *Gas* was at first a technical term invented by Van Helmont, though it has now been almost adopted into common language. I omit many words which will perhaps suggest themselves to the reader, because they belong rather to the next class, which I now proceed to notice.

III. The third class of technical terms consists of such as are constructed by men of science, and involve some theoretical idea in the meaning which their derivation implies. They do not merely describe, like the class last spoken of, but describe with reference to some doctrine or hypothesis which is accepted as a portion of science. Thus *latitude* and *longitude*, according to their origin, signify breadth and length; they are used, however, to denote measures of the distance of a place on the earth's surface from the equator, and from the first meridian, of which distances, one cannot be called *length* more properly than the other. But this appropriation of these words may be explained by recollecting that the earth, as known to the ancient geographers, was much further extended from east to west than from north to south. The *Precession* of the equinoxes is a term which implies that the stars are fixed, while the point which is the origin of the measure of celestial longitude moves backward. The *Right Ascension* of a star is a measure of its position corresponding to terrestrial longitude; this quantity is identical with the angular ascent of the equinoctial point, when the star is in the horizon in a *right* sphere; that is, a sphere which supposes the spectator to be at the equator. The *Oblique Ascension* (a term now little used), is derived in like manner from an oblique sphere. The motion of a planet is *direct* or *retrograde*, *in con-*

sequentia (*signa*), or *in antecedentia*, in reference to a certain assumed standard direction for celestial motions, namely, the direction opposite to that of the sun's daily motion, and agreeing with his annual motion among the stars; or with what is much more evident, the moon's monthly motion. The *equation of time* is the quantity which must be added to or subtracted from the time marked by the sun, in order to reduce it to a theoretical condition of equable progress. In like manner the *equation of the center* of the sun or of the moon is the angle which must be added to, or subtracted from, the actual advance of the luminary in the heavens, in order to make its motion equable. Besides the equation of the center of the moon, which represents the first and greatest of her deviations from equable motion, there are many other *equations*, by the application of which her motion is brought nearer and nearer to perfect uniformity. The second of these equations is called the *evection*, the third the *variation*, the fourth the *annual equation*. The motion of the sun as affected by its inequalities is called his *anomaly*, which term denotes inequality. In the History of Astronomy, we find that the inequable motions of the sun, moon, and planets were, in a great measure, reduced to rule and system by the Greeks, by the aid of an hypothesis of circles, revolving, and carrying in their motion other circles which also revolved. This hypothesis introduced many technical terms, as *deferent*, *epicycle*, *eccentric*. In like manner, the theories which have more recently taken the place of the theory of epicycles have introduced other technical terms, as the *elliptical orbit*, the *radius vector*, and the *equable description of areas* by this radius, which phrases express the true laws of the planetary motions.

There is no subject on which theoretical views have been so long and so extensively prevalent as astronomy, and therefore no other science in which there are so many technical terms of the kind we are now considering. But in other subjects also, so far as theories have been established, they have been accompanied by the introduction or fixation of technical terms. Thus, as we have seen in the examination of the foundations of mechanics, the terms *force* and *inertia* derive their precise meaning from a recognition of the first law of motion; *accelerating force* and *composition of motion* involve the second law; *moving force*, *momentum*, *action* and *reaction*, are expressions which imply the third law. The term *vis viva* was introduced to express a general property of moving bodies; and other terms have been introduced for like purposes, as *impetus* by Smeatón, and *work done*, by other engineers. In the recent writings of several French engineers, the term *travail* is much employed, to express the work done and the force which does it: this term has been rendered by *labouring force*. The proposition which was termed the *hydrostatic paradox* had this name in reference to its violating a supposed law of the action of forces. The verb to *gravitate*, and the abstract term *gravitation*, sealed the establishment of Newton's theory of the solar system.

In some of the sciences, opinions, either false, or disguised in very fantastical imagery, have prevailed; and the terms which have been introduced during the reign of such opinions, bear the impress of the time. Thus in the days of alchemy, the substances with which the operator dealt were personified; and a metal when exhibited pure and free from all admixture was considered as a little king, and was hence called a *regulus*, a term not yet quite obsolete. In like manner, a substance from which nothing more of any value could be extracted, was dead, and was called a *caput mortuum*. Quick silver, that is, live silver (*argentum vivum*), was killed by certain admixtures, and was *revived* when restored to its pure state.

We find a great number of medical terms which bear the mark of opinions formerly prevalent among physicians; and though these opinions hardly form a part of the progress of science, and were not presented in our History, we may notice some of these terms as examples of the mode in which words involve in their derivation obsolete opinions. Such words as *hysterics*, *hypochondriac*, *melancholy*, *cholera*, *colic*, *quinsey* (*squi-*

nantia, *συνάγχη*, a suffocation), *megrim*, *migraine* (*hemicranium*, the middle of the skull), *rickets*, (*rachitis*, from ῥάχις, the backbone), *palsy*, (*paralysis*, *παράλυσις*,) *apoplexy* (*ἀποπληξία*, a stroke), *emrods*, (*αἱμοῤῥοΐδες*, *hemorrhoids*, a flux of blood), *imposthume*, (corrupted from *aposteme*, *ἀπόστημα*, an abscess), *phthisic* (*φθίσις*, consumption), *tympany* (*τυμπανία*, swelling), *dropsy* (*hydropsy*, *ὕδρωψ*,) *sciatica*, isciatica (*ἰσχιαδική*, from *ἰσχίον*, the hip), *catarrh* (*κατάῤῥους*, a flowing down), *diarrhœa* (*διαῤῥοία*, a flowing through), *diabetes* (*διαβήτης*, a passing through), *dysentery* (*δυσεντερία*, a disorder of the entrails), *arthritic* pains (from *ἄρθρα*, the joints), are names derived from the supposed or real seat and circumstances of the diseases. The word from which the first of the above names is derived (*ὑστέρα*, the last place,) signifies the womb, according to its order in a certain systematic enumeration of parts. The second word, *hypochondriac*, means something affecting the viscera below the cartilage of the breastbone, which cartilage is called *χόνδρος*; *melancholy* and *cholera* derive their names from supposed affections of *χολή*, the bile. *Colic* is that which affects the *colon* (*κῶλον*), the largest member of the bowels. A disorder of the eye is called *gutta serena* (the 'drop serene' of Milton), in contradistinction to *gutta turbida*, in which the impediment to vision is perceptibly opake. Other terms also record the opinions of the ancient anatomists, as *duodenum*, a certain portion of the intestines, which they estimated as twelve inches long. We might add other allusions, as the *tendon of Achilles*.

Astrology also supplied a number of words founded upon fanciful opinions; but this study having been expelled from the list of sciences, such words now survive only so far as they have found a place in common language. Thus men were termed *mercurial*, *martial*, *jovial*, or *saturnine*, accordingly as their characters were supposed to be determined by the influence of the planets, Mercury, Mars, Jupiter, or Saturn. Other expressions, such as *disastrous*, *ill-starred*, *exorbitant*, *lord of the ascendant*, and hence *ascendancy*, *influence*, a *sphere of action*, and the like, may serve to show how extensively astrological opinions have affected language, though the doctrine is no longer a recognized science.

The preceding examples will make it manifest that opinions, even of a recondite and complex kind, are often implied in the derivation of words; and thus will show how scientific terms, framed by the cultivators of science, may involve received hypotheses and theories. When terms are thus constructed, they serve not only to convey with ease, but to preserve steadily and to diffuse widely, the opinions which they thus assume. Moreover, they enable the speculator to employ these complex conceptions, the creations of science, and the results of much labour and thought, as readily and familiarly as if they were convictions borrowed at once from the senses. They are thus powerful instruments in enabling philosophers to ascend from one step of induction and generalization to another; and hereby contribute powerfully to the advance of knowledge and truth.

It should be noticed, before we proceed, that the names of natural objects, when they come to be considered as the objects of a science, are selected according to the processes already enumerated. For the most part, the natural historian adopts the common names of animals, plants, minerals, gems, and the like, and only endeavours to secure their steady and consistent application. But many of these names imply some peculiar, often fanciful, belief respecting the object.

Various plants derive their names from their supposed virtues, as *herniaria, rupture-wort;* or from legends, as *herba Sancti Johannis, St. John's wort.* The same is the case with minerals: thus the *topaz* was asserted to come from an island so shrouded in mists that navigators could only *conjecture* (τοπάζειν) where it was. In these latter cases, however, the legend is often not the true origin of the name, but is suggested by it.

The privilege of constructing names where they are wanted, belongs to natural historians no less than to the cultivators of physical science; yet in the ancient world, writers of the former class appear rarely to have exercised this privilege, even when they felt the imperfections of the current language. Thus Aristotle repeatedly mentions classes of animals which have no name, as co-ordinate with classes that have names; but he hardly ventures to propose names which may supply these defects[3]. The vast importance of nomenclature in natural history was not recognized till the modern period.

We have, however, hitherto considered only the formation or appropriation of single terms in science; except so far as several terms may in some instances be connected by reference to a common theory. But when the value of technical terms began to be fully appreciated, philosophers proceeded to introduce them into their sciences more copiously and in a more systematic manner. In this way, the modern history of technical language has some features of a different aspect from the ancient; and must give rise to a separate Aphorism.

Aphorism II.

In the Modern Period of Science, besides the three processes anciently employed in the formation of technical terms, there have been introduced Systematic Nomenclature, Systematic Terminology, and the Systematic Modification of Terms to express theoretical relations[1].

Writers upon science have gone on up to modern times forming such technical terms as they had occasion for, by the three processes above described;—

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[3] In his *History of Animals*, (b. i. c. vi.), he says, that the great classes of animals are Quadrupeds, Birds, Fishes, Whales (*Cetaceans*), Oysters (*Testaceans*), animals like crabs which have no general name (*Crustaceans*), soft animals (*Mollusks* and *Insects*). He does, however, call the Crustaces by a name (*Malacostraca*, soft-shelled) which has since been adopted by Naturalists.

[1] On the subject of Terminology and Nomenclature, see also Aphorisms LXXXVIII. and XCVIII. concerning Ideas, and b. viii. c. ii. of the *History of Scientific Ideas*. In those places I have spoken of the distinction of *Terminology* and *Nomenclature*.

namely, appropriating and limiting words in common use;—constructing for themselves words descriptive of the conception which they wished to convey;—or framing terms which by their signification imply the adoption of a theory. Thus among the terms introduced by the study of the connexion between magnetism and electricity, the word *pole* is an example of the first kind; the name of the subject, *electro-magnetism*, of the second; and the term *current*, involving an hypothesis of the motion of a fluid, is an instance of the third class. In chemistry, the term *salt* was adopted from common language, and its meaning extended to denote any compound of a certain kind; the term *neutral* salt implied the notion of a balanced opposition in the two elements of the compound; and such words as *subacid* and *superacid*, invented on purpose, were introduced to indicate the cases in which this balance was not attained. Again, when the phlogistic theory of chemistry was established, the term *phlogiston* was introduced to express the theory, and from this such terms as *phlogisticated* and *dephlogisticated* were derived, exclusively words of science. But in such instances as have just been given, we approach towards a systematic modification of terms, which is a peculiar process of modern times. Of this, modern chemistry forms a prominent example, which we shall soon consider, but we shall first notice the other processes mentioned in the Aphorism.

I. In ancient times, no attempt was made to invent or select a Nomenclature of the objects of Natural History which should be precise and permanent. The omission of this step by the ancient naturalists gave rise to enormous difficulty and loss of time when the sciences resumed their activity. We have seen in the history of the sciences of classification, and of botany in especial[2], that the early cultivators of that study in modern times endeavoured to identify all the plants described by Greek and Roman writers with those which grow in the north of Europe; and were involved

[2] *Hist. Ind. Sc.* b. xvi. c. ii.

in endless confusion[3], by the multiplication of names of plants, at the same time superfluous and ambiguous. The *Synonymies* which botanists (Bauhin and others) found it necessary to publish, were the evidences of these inconveniences. In consequence of the defectiveness of the ancient botanical nomenclature, we are even yet uncertain with respect to the identification of some of the most common trees mentioned by classical writers[4]. The ignorance of botanists respecting the importance of nomenclature operated in another manner to impede the progress of science. As a good nomenclature presupposes a good system of classification, so, on the other hand, a system of classification cannot become permanent without a corresponding nomenclature. Cæsalpinus, in the sixteenth century[5], published an excellent system of arrangement for plants; but this, not being connected with any system of names, was never extensively accepted, and soon fell into oblivion. The business of framing a scientific botanical classification was in this way delayed for about a century. In the same manner, Willoughby's classification of fishes, though, as Cuvier says, far better than any which preceded it, was never extensively adopted, in consequence of having no nomenclature connected with it.

II. Probably one main cause which so long retarded the work of fixing at the same time the arrangement and the names of plants, was the great number of minute and diversified particulars in the structure of each plant which such a process implied. The stalks, leaves, flowers, and fruits of vegetables, with their appendages, may vary in so many ways, that common language is quite insufficient to express clearly and precisely their resemblances and differences. Hence botany required not only a fixed system of *names* of plants, but also an artificial system of phrases fitted to *describe* their parts: not only a *Nomenclature*, but also

[3] *Hist. Ind. Sc.* b. xvi. c. iii. sect. 3.

[4] For instance, whether the *fagus* of the Latins be the beech or the chestnut.

[5] *Ib.* b. xvi. c. iii. sect. 2.

a *Terminology*. The Terminology was, in fact, an instrument indispensably requisite in giving fixity to the Nomenclature. The recognition of the kinds of plants must depend upon the exact comparison of their resemblances and differences; and to become a part of permanent science, this comparison must be recorded in words.

The formation of an exact descriptive language for botany was thus the first step in that systematic construction of the technical language of science, which is one of the main features in the intellectual history of modern times. The ancient botanists, as De Candolle[6] says, did not make any attempt to select terms of which the sense was rigorously determined; and each of them employed in his descriptions the words, metaphors, or periphrases which his own genius suggested. In the History of Botany[7], I have noticed some of the persons who contributed to this improvement. 'Clusius,' it is there stated, 'first taught botanists to describe well. He introduced exactitude, precision, neatness, elegance, method: he says nothing superfluous; he omits nothing necessary.' This task was further carried on by Jung and Ray[8]. In these authors we see the importance which began to be attached to the exact definition of descriptive terms; for example, Ray quotes Jung's definition of *Caulis*, a stalk.

The improvement of descriptive language, and the formation of schemes of classification of plants, went on gradually for some time, and was much advanced by Tournefort. But at last Linnæus embodied and followed out the convictions which had gradually been accumulating in the breasts of botanists; and by remodelling throughout both the terminology and the nomenclature of botany, produced one of the greatest reforms which ever took place in any science. He thus supplied a conspicuous example of such a reform, and a most admirable model of a language, from which

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[6] *Theor. Elem. de Bot.* p. 327.

[7] *Hist. Ind. Sc.* b. xvi. c. iii. sect. 3.

[8] *Hist. Ind. Sc.* b. xvi. c. iii. sect. 3 (about A.D. 1660).

other sciences may gather great instruction. I shall not here give any account of the terms and words introduced by Linnæus. They have been exemplified in the *History of Science*[9]; and the principles which they involve I shall consider separately hereafter. I will only remind the reader that the great simplification in *nomenclature* which was the result of his labours, consisted in designating each kind of plant by a *binary* term consisting of the name of the *genus* combined with that of the *species:* an artifice seemingly obvious, but more convenient in its results than could possibly have been anticipated.

Since Linnæus, the progress of Botanical Anatomy and of Descriptive Botany have led to the rejection of several inexact expressions, and to the adoption of several new terms, especially in describing the structure of the fruit and the parts of cryptogamous plants. Hedwig, Medikus, Necker, Desvaux, Mirbel, and especially Gærtner, Link, and Richard, have proposed several useful innovations, in these as in other parts of the subject; but the general mass of the words now current consists still, and will probably continue to consist, of the terms established by the Swedish Botanist[10].

When it was seen that botany derived so great advantages from a systematic improvement of its language, it was natural that other sciences, and especially classificatory sciences, should endeavour to follow its example. This attempt was made in Mineralogy by Werner, and afterwards further pursued by Mohs. Werner's innovations in the descriptive language of Mineralogy were the result of great acuteness, an intimate acquaintance with minerals, and a most methodical spirit: and were in most respects great improvements upon previous practices. Yet the introduction of them into Mineralogy was far from regenerating that science, as Botany had been regenerated by the Linnæan reform. It would seem that the perpetual

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[9] *Ib.* c. iv. sect. 1—3.

[10] De Candolle, *Th. Elem.* p. 307.

scrupulous attention to most minute differences, (as of lustre, colour, fracture,) the greater part of which are not really important, fetters the mind, rather than disciplines it or arms it for generalization. Cuvier has remarked[11] that Werner, after his first *Essay on the Characters of Minerals*, wrote little; as if he had been afraid of using the system which he had created, and desirous of escaping from the chains which he had imposed upon others. And he justly adds, that Werner dwelt least, in his descriptions, upon that which is really the most important feature of all, the crystalline structure. This, which is truly a definite character, like those of Botany, does, when it can be clearly discerned, determine the place of the mineral in a system. This, therefore, is the character which, of all others, ought to be most carefully expressed by an appropriate language. This task, hardly begun by Werner, has since been fully executed by others, especially by Romé de l'Isle, Haüy, and Mohs. All the forms of crystals can be described in the most precise manner by the aid of the labours of these writers and their successors. But there is one circumstance well worthy our notice in these descriptions. It is found that the language in which they can best be conveyed is not that of words, but of *symbols*. The relations of space which are involved in the forms of crystalline bodies, though perfectly definite, are so complex and numerous, that they cannot be expressed, except in the language of mathematics: and thus we have an extensive and recondite branch of mathematical science, which is, in fact, only a part of the Terminology of the mineralogist.

The Terminology of Mineralogy being thus reformed, an attempt was made to improveits Nomenclature also, by following the example of Botany. Professor Mohs was the proposer of this innovation. The names framed by him were, however, not composed of two but of three elements, designating respectively the Species, the Genus, and the Order[12]: thus he has such species as

[11] *Éloges*, ii. 314.

[12] *Hist. Ind. Sc.* b. xv. c. ix.

Rhombohedral Lime Haloide, Octahedral Fluor Haloide, Prismatic Hal Baryte. These names have not been generally adopted; nor is it likely that any names constructed on such a scheme will find acceptance among mineralogists, till the higher divisions of the system are found to have some definite character. We see no real mineralogical significance in Mohs's Genera and Orders, and hence we do not expect them to retain a permanent place in the science.

The only systematic names which have hitherto been generally admitted in Mineralogy, are those expressing the chemical constitution of the substance; and these belong to a system of technical terms different from any we have yet spoken of, namely to terms formed by systematic modification.

III. The language of Chemistry was already, as we have seen, tending to assume a systematic character, even under the reign of the phlogiston theory. But when oxygen succeeded to the throne, it very fortunately happened that its supporters had the courage and the foresight to undertake a completely new and systematic recoinage of the terms belonging to the science. The new nomenclature was constructed upon a principle hitherto hardly applied in science, but eminently commodious and fertile; namely, the principle of indicating a modification of relations of elements, by a change in the termination of the word. Thus the new chemical school spoke of sulph*uric* and sulph*urous* acids; of sulph*ates* and sulph*ites* of bases; and of sulph*urets* of metals; and in like manner, of phos*phoric* and phos*phorous* acids, of phos*phates*, phos*phites*, phos*phurets*. In this manner a nomenclature was produced, in which the very name of a substance indicated at once its constitution and place in the system.

The introduction of this chemical language can never cease to be considered one of the most important steps ever made in the improvement of technical terms; and as a signal instance of the advantages which may result from artifices apparently trivial, if employed in a manner conformable to the laws of phenomena, and systematically pursued. It was, however, proved that this language, with all its merits, had some defects. The relations of elements in composition were discovered to be more numerous than the modes of expression which the terminations supplied. Besides the sulphurous and sulphuric acids, it appeared there were others; these were called the *hyposulphurous* and *hyposulphuric:* but these names, though convenient, no longer implied, by their form, any definite relation. The compounds of Nitrogen and Oxygen are, in order, the *Protoxide*, the *Deutoxide* or *Binoxide; Hyponitrous* Acid, *Nitrous* Acid, and *Nitric* Acid. The nomenclature here ceases to be systematic. We have three oxides of Iron, of which we may call the first the *Protoxide*, but we cannot call the others the *Deutoxide* and *Tritoxide*, for by doing so we should convey a perfectly erroneous notion of the proportions of the elements. They are called the *Protoxide*, the *Black* Oxide, and the *Peroxide*. We are here thrown back upon terms quite unconnected with the system.

Other defects in the nomenclature arose from errours in the theory; as for example the names of the muriatic, oxymuriatic, and hyperoxymuriatic acids; which, after the establishment of the new theory of chlorine, were changed to *hydrochloric* acid, *chlorine*, and *chloric* acid.

Thus the chemical system of nomenclature, founded upon the oxygen theory, while it shows how much may be effected by a good and consistent scheme of terms, framed according to the real relations of objects, proves also that such a scheme can hardly be permanent in its original form, but will almost inevitably become imperfect and anomalous, in consequence of the accumulation of new facts, and the introduction of new generalizations. Still, we may venture to say that such a scheme does not, on this account, become worthless; for it not only answers its purpose in the stage of scientific progress to which it belongs:—so far as it is not erroneous, or merely conventional, but really systematic and significant of truth, its terms can be translated at once into the language of any higher generalization which is afterwards arrived at. If terms express relations really ascertained to be true, they can never lose their value by any change of the received theory. They are like coins of pure metal, which, even when carried into a country which does not recognize the sovereign whose impress they bear, are still gladly received, and may, by the addition of an explanatory mark, continue part of the common currency of the country.

These two great instances of the reform of scientific language, in Botany and in Chemistry, are much the most important and instructive events of this kind which the history of science offers. It is not necessary to pursue our historical survey further. Our remaining Aphorisms respecting the Language of Science will be collected and illustrated indiscriminately, from the precepts and the examples of preceding philosophers of all periods[13].

We may, however, remark that Aphorisms III., IV., V., VI., VII., respect peculiarly the Formation of Technical Terms by the Appropriation of Common Words, while the remaining ones apply to the Formation of New Terms.

It does not appear possible to lay down a system of rules which may determine and regulate the construction of all technical terms, on all the occasions on which the progress of science makes them necessary or convenient. But if we can collect a few maxims such as have already offered themselves to the minds of philosophers, or such as may be justified by the instances by which we shall illustrate them, these maxims may avail to guide us in doubtful cases, and to prevent our aiming at advantages which are unattainable, or being disturbed by seeming imperfections which are really no evils. I shall therefore state such maxims of this kind as seem most sound and useful.

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[13] See at the end of these Aphorisms, further illustrations of them from the recent history of Comparative Anatomy and Chemistry.

Aphorism III.

In framing scientific terms, the appropriation of old words is preferable to the invention of new ones.

This maxim is stated by Bacon in his usual striking manner. After mentioning *Metaphysic,* as one of the divisions of Natural Philosophy, he adds[1]: 'Wherein I desire it may be conceived that I use the word *metaphysic* in a different sense from that that is received: and in like manner I doubt not but it will easily appear to men of judgment that in this and other particulars, wheresoever my conception and notion may differ from the ancient, yet I am studious to keep the ancient terms. For, hoping well to deliver myself from mistaking by the order and perspicuous expressing of that I do propound; I am otherwise zealous and affectionate to recede as little from antiquity, either in terms or opinions, as may stand with truth, and the proficience of knowledge, . . . To me, that do desire, as much as lieth in my pen, to ground a sociable intercourse between antiquity and proficience, it seemeth best to keep a way with antiquity *usque ad aras;* and therefore to retain the ancient terms, though I sometimes alter the uses and definitions; according to the moderate proceeding in civil governments, when, although there be some alteration, yet that holdeth which Tacitus wisely noteth, *eadem magistratuum vocabula.*'

We have had before us a sufficient number of examples of scientific terms thus framed; for they formed the first of three classes which we described in the First Aphorism. And we may again remark, that science, when she thus adopts terms which are in common use, always limits and fixes their meaning in a technical manner. We may also repeat here the warning already given respecting terms of this kind, that they are peculiarly liable to mislead readers who

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[1] *De Augm.* lib. iii. c. iv.

do not take care to understand them in their technical instead of their common signification. *Force*, *momentum*, *inertia*, *impetus*, *vis viva*, are terms which are very useful, if we rigorously bear in mind the import which belongs to each of them in the best treatises on Mechanics; but if the reader content himself with conjecturing their meaning from the context, his knowledge will be confused and worthless.

In the application of this Third Aphorism, other rules are to be attended to, which I add.

APHORISM IV.

When common words are appropriated as technical terms, their meaning and relations in common use should be retained as far as can conveniently be done.

I WILL state an example in which this rule seems to be applicable. Mr Davies Gilbert[1] has recently proposed the term *efficiency* to designate the work which a machine, according to the force exerted upon it, is capable of doing; the work being measured by the weight raised, and the space through which it is raised, jointly. The usual term employed among engineers for the work which a machine actually does, measured in the way just stated, is *duty*. But as there appears to be a little incongruity in calling that work *efficiency* which the machine *ought* to do, when we call that work *duty* which it really does, I have proposed to term these two quantities *theoretical efficiency* and *practical efficiency*, or *theoretical duty* and *practical duty*[2].

Since common words are often vague in their meaning, I add as a necessary accompaniment to the Third Aphorism the following:—

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[1] *Phil. Trans.* 1827, p. 25.

[2] The term *travail* is used by French engineers, to express *efficiency* or *theoretical duty*. This term has been rendered in English by *labouring force*.

Aphorism V.

When common words are appropriated as technical terms, their meaning may be modified, and must be rigorously fixed.

This is stated by Bacon in the above extract: 'to retain the ancient terms, though I sometimes *alter the uses and definitions*.' The scientific use of the term is in all cases much more precise than the common use. The loose notions of *velocity* and *force* for instance, which are sufficient for the usual purposes of language, require to be fixed by exact measures when these are made terms in the science of Mechanics.

This scientific fixation of the meaning of words is to be looked upon as a matter of convention, although it is in reality often an inevitable result of the progress of science. *Momentum* is conventionally defined to be the product of the numbers expressing the weight and the velocity; but then, it could be of no use in expressing the laws of motion if it were defined otherwise.

Hence it is no valid objection to a scientific term that the word in common language does not mean exactly the same as in its common use. It is no sufficient reason against the use of the term *acid* for a class of bodies, that all the substances belonging to this class are not sour. We have seen that a *trapezium* is used in geometry for any four-sided figure, though originally it meant a figure with two opposite sides parallel and the two others equal. A certain stratum which lies below the chalk is termed by English geologists *the green sand.* It has sometimes been objected to this denomination that the stratum has very frequently no tinge of green, and that it is often composed of lime with little or no sand. Yet the term is a good technical term in spite of these apparent improprieties; so long as it is carefully applied to that stratum which is geologically equivalent to the greenish sandy bed to which the appellation was originally applied.

When it appeared that *geometry* would have to be employed as much at least about the heavens as the earth, Plato exclaimed against the folly of calling the science by such a name; since the word signifies 'earth-measuring;' yet the word *geometry* has retained its place and answered its purpose perfectly well up to the present day.

But though the meaning of the term may be modified or extended, it must be rigorously fixed when it is appropriated to science. This process is most abundantly exemplified by the terminology of Natural History, and especially of Botany, in which each term has a most precise meaning assigned to it. Thus Linnæus established exact distinctions between *fasciculus*, *capitulum*, *racemus*, *thyrsus*, *paniculus*, *spica*, *amentum*, *corymbus*, *umbella*, *cyma*, *verticillus*; or, in the language of English Botanists, *a tuft*, *a head*, *a cluster*, *a bunch*, *a panicle*, *a spike*, *a catkin*, *a corymb*, *an umbel*, *a cyme*, *a whorl*. And it has since been laid down as a rule[1], that each organ ought to have a separate and appropriate name; so that the term *leaf*, for instance, shall never be applied to *a leaflet*, *a bractea*, or *a sepal* of the calyx.

Botanists have not been content with fixing the meaning of their terms by verbal definition, but have also illustrated them by figures, which address the eye. Of these, as excellent modern examples, may be mentioned those which occur in the works of Mirbel[2], and Lindley[3].

Aphorism VI.

When common words are appropriated as technical terms, this must be done so that they are not ambiguous in their application.

An example will explain this maxim. The conditions of a body, as a solid, a liquid, and an air, have been distinguished as different *forms* of the body. But the word *form*, as applied to bodies, has other meanings; so that if we were to inquire in *what form* water exists in a snow-cloud, it might be doubted whether the forms of crystallization were meant, or

[1] De Candolle, *Theor. El.* 328. [2] *Élémens de Botanique.*
[3] *Elements of Botany.*

the different forms of ice, water, and vapour. Hence I have proposed[1] to reject the term *form* in such cases, and to speak of the different *consistence* of a body in these conditions. The term *consistence* is usually applied to conditions between solid and fluid; and may without effort be extended to those limiting conditions. And though it may appear more harsh to extend the term *consistence* to the state of air, it may be justified by what has been said in speaking of Aphorism V.

I may notice another example of the necessity of avoiding ambiguous words. A philosopher who makes method his study, would naturally be termed a *methodist;* but unluckily this word is already appropriated to a religious sect: and hence we could hardly venture to speak of Cæsalpinus, Ray, Morison, Rivinus, Tournefort, Linnæus, and their successors, as *botanical methodists.* Again, by this maxim, we are almost debarred from using the term *physician* for a cultivator of the science of physics, because it already signifies a practiser of physic. We might, perhaps, still use *physician* as the equivalent of the French *physicien*, in virtue of Aphorism V.; but probably it would be better to form a new word. Thus we may say, that while the Naturalist employs principally the ideas of resemblance and life, the *Physicist* proceeds upon the ideas of force, matter, and the properties of matter.

Whatever may be thought of this proposal, the maxim which it implies is frequently useful. It is this.

APHORISM VII.

It is better to form new words as technical terms, than to employ old ones in which the last three Aphorisms cannot be complied with.

THE principal inconvenience attending the employment of new words constructed expressly for the use of science, is the difficulty of effectually introducing them. Readers will not readily take the trouble to learn the meaning of a word, in which the memory is

[1] *Hist. Ind. Sc.* b. x. c. ii. sect. 2.

not assisted by some obvious suggestion connected with the common use of language. When this difficulty is overcome, the new word is better than one merely appropriated; since it is more secure from vagueness and confusion. And in cases where the inconveniences belonging to a scientific use of common words become great and inevitable, a new word must be framed and introduced.

The Maxims which belong to the construction of such words will be stated hereafter; but I may notice an instance or two tending to show the necessity of the Maxim now before us.

The word *Force* has been appropriated in the science of Mechanics in two senses: as indicating the cause of motion; and again, as expressing certain measures of the effects of this cause, in the phrases *accelerating force* and *moving force*. Hence we might have occasion to speak of the accelerating or moving force *of* a certain *force;* for instance, if we were to say that the force which governs the motions of the planets resides in the sun; and that the accelerating force *of* this *force* varies only with the distance, but its moving force varies as the product of the mass of the sun and the planet. This is a harsh and incongruous mode of expression; and might have been avoided, if, instead of *accelerating force* and *moving force,* single abstract terms had been introduced by Newton: if, for instance, he had said that the velocity generated in a second measures the *accelerativity* of the force which produces it, and the momentum produced in a second measures the *motivity* of the force.

The science which treats of heat has hitherto had no special designation: treatises upon it have generally been termed treatises *On Heat.* But this practice of employing the same term to denote the property and the science which treats of it, is awkward, and often ambiguous. And it is further attended with this inconvenience, that we have no adjective derived from the name of the science, as we have in other cases, when we speak of *acoustical* experiments and *optical* theories. This inconvenience has led various persons to suggest names for the Science of Heat. M. Comte terms it *Thermology*. In the *History of the Sciences*, I have named it *Thermotics*, which appears to me to agree better with the analogy of the names of other corresponding sciences, *Acoustics* and *Optics*.

Electricity is in the same condition as Heat; having only one word to express the property and the science. M. Le Comte proposes *Electrology:* for the same reason as before, I should conceive *Electrics* more agreeable to analogy. The coincidence of the word with the plural of Electric would not give rise to ambiguity; for *Electrics*, taken as the name of a science, would be singular, like *Optics* and *Mechanics*. But a term offers itself to express *common* or *machine Electrics*, which appears worthy of admission, though involving a theoretical view. The received doctrine of the difference between Voltaic and Common Electricity is, that in the former case the fluid must be considered as in motion, in the latter as at rest. The science which treats of the former class of subjects is commonly termed *Electrodynamics*, which obviously suggests the name *Electrostatics* for the latter.

The subject of the Tides is, in like manner, destitute of any name which designates the science concerned about it. I have ventured to employ the term *Tidology*, having been much engaged in tidological researches.

Many persons possess a peculiarity of vision, which disables them from distinguishing certain colours. On examining many such cases, we find that in all such persons the peculiarities are the same; all of them confounding scarlet with green, and pink with blue. Hence they form a class, which, for the convenience of physiologists and others, ought to have a fixed designation. Instead of calling them, as has usually been done, 'persons having a peculiarity of vision,' we might take a Greek term implying this meaning, and term them *Idiopts*.

But my business at present is not to speak of the selection of new terms when they are introduced, but to illustrate the maxim that the necessity for their introduction often arises. The construction of new terms will be treated of subsequently.

Aphorism VIII.

Terms must be constructed and appropriated so as to be fitted to enunciate simply and clearly true general propositions.

This Aphorism may be considered as the fundamental principle and supreme rule of all scientific terminology. It is asserted by Cuvier, speaking of a particular case. Thus he says[1] of Gmelin, that by placing the lamantin in the genus of morses, and the siren in the genus of eels, he had rendered every general proposition respecting the organization of those genera impossible.

The maxim is true of words appropriated as well as invented, and applies equally to the mathematical, chemical, and classificatory sciences. With regard to most of these, and especially the two former classes, it has been abundantly exemplified already, in what has previously been said, and in the *History of the Sciences.* For we have there had to notice many technical terms, with the occasions of their introduction; and all these occasions have involved the intention of expressing in a convenient manner some truth or supposed truth. The terms of Astronomy were adopted for the purpose of stating and reasoning upon the relations of the celestial motions, according to the doctrine of the sphere, and the other laws which were discovered by astronomers. The few technical terms which belong to Mechanics, *force*, *velocity*, *momentum*, *inertia*, &c., were employed from the first with a view to the expression of the laws of motion and of rest; and were, in the end, limited so as truly and simply to express those laws when they were fully ascertained. In Chemistry, the term *phlogiston* was useful, as has been shown in the *History*, in classing together processes which really are of the same nature; and the nomenclature of the *oxygen* theory was still preferable, because it enabled the chemist to express a still greater number of general truths.

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[1] *Règne Animal*, Introd. viii.

To the connexion here asserted, of theory and nomenclature, we have the testimony of the author of the oxygen theory. In the Preface to his *Chemistry*, Lavoisier says:—'Thus while I thought myself employed only in forming a Nomenclature, and while I proposed to myself nothing more than to improve the chemical language, my work transformed itself by degrees, without my being able to prevent it, into a Treatise on the Elements of Chemistry.' And he then proceeds to show how this happened.

It is, however, mainly through the progress of Natural History in modern times, that philosophers have been led to see the importance and necessity of new terms in expressing new truths. Thus Harvey, in the Preface to his work on Generation, says:—'Be not offended if in setting out the History of the Egg I make use of a new method, and sometimes of unusual terms. For as they which find out a new plantation and new shores call them by names of their own coining, which posterity afterwards accepts and receives, so those that find out new secrets have good title to their compellation. And here, methinks, I hear Galen advising: If we consent in the things, contend not about the words.'

The Nomenclature which answers the purposes of Natural History is a Systematic Nomenclature, and will be further considered under the next Aphorism. But we may remark, that the Aphorism now before us governs the use of words, not in science only, but in common language also. Are we to apply the name *fish* to animals of the whale kind? The answer is determined by our present rule: we are to do so, or not, accordingly as we can best express true propositions. If we are speaking of the internal structure and physiology of the animal, we must not call them *fish;* for in these respects they deviate widely from fishes: they have warm blood, and produce and suckle their young as land quadrupeds do. But this would not prevent our speaking of the *whale-fishery*, and calling such animals *fish* on all occasions connected with this employment; for the relations thus arising depend upon the animal's living in the water, and being caught in a manner similar to other fishes. A plea that human laws which mention fish do not apply to whales, would be rejected at once by an intelligent judge.

[A bituminiferous deposit which occurs amongst the coal measures in the neighbourhood of Edinburgh was used as coal, and called 'Boghead Cannel Coal.' But a lawsuit arose upon the question whether this, which geologically was not *the coal*, should be regarded in law as *coal*. The opinions of chemists and geologists, as well as of lawyers, were discrepant, and a direct decision of the case was evaded[2].]

APHORISM IX.

In the Classificatory Sciences, a Systematic Nomenclature is necessary; and the System and the Nomenclature are each essential to the utility of the other.

THE inconveniences arising from the want of a good Nomenclature were long felt in Botany, and are still felt in Mineralogy. The attempts to remedy them by *Synonymies* are very ineffective, for such comparisons of synonyms do not supply a systematic nomenclature; and such a one alone can enable us to state general truths respecting the objects of which the classificatory sciences treat. The *System* and the *Names* ought to be introduced together; for the former is a collection of asserted analogies and resemblances, for which the latter provide simple and permanent expressions. Hence it has repeatedly occurred in the progress of Natural History, that good Systems did not take root, or produce any lasting effect among naturalists, because they were not accompanied by a corresponding Nomenclature. In this way, as we have already noticed, the excellent botanical System of Cæsalpinus was without immediate effect upon the science. The work of Willughby, as Cuvier says[1], forms an epoch, and

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[2] Miller's *Chemistry*, iii. 98.

[1] *Hist. des Poissons*, Pref.

a happy epoch in Ichthyology; yet because Willughby had no Nomenclature of his own, and no fixed names for his genera, his immediate influence was not great. Again, in speaking of Schlotheim's work containing representations of fossil vegetables, M. Adolphe Brongniart observes[2] that the figures and descriptions are so good, that if the author had established a nomenclature for the objects he describes, his work would have become the basis of all succeeding labours on the subject.

As additional examples of cases in which the improvement of classification, in recent times, has led philosophers to propose new names, I may mention the term *Pœcilite*, proposed by Mr. Conybeare to designate the group of strata which lies below the oolites and lias, including the new red or variegated sandstone, with the keuper above, and the magnesian limestone below it. Again, the transition districts of our island have recently been reduced to system by Professor Sedgwick and Mr. Murchison; and this step has been marked by the terms *Cambrian* system, and *Silurian* system, applied to the two great groups of formations which they have respectively examined, and by several other names of the subordinate members of these formations.

Thus System and Nomenclature are each essential to the other. Without Nomenclature, the system is not permanently incorporated into the general body of knowledge, and made an instrument of future progress. Without System, the names cannot express general truths, and contain no reason why they should be employed in preference to any other names.

This has been generally acknowledged by the most philosophical naturalists of modern times. Thus Linnæus begins that part of his Botanical Philosophy in which names are treated of, by stating that the foundation of botany is twofold, *Disposition* and *Denomination;* and he adds this Latin line,

Nomina si nescis perit et cognitio rerum.

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[2] *Prodrom. Veg. Foss.* p. 3.

And Cuvier, in the Preface to his *Animal Kingdom*, explains, in a very striking manner, how the attempt to connect zoology with anatomy led him, at the same time, to reform the classifications, and to correct the nomenclature of preceding zoologists.

I have stated that in Mineralogy we are still destitute of a good nomenclature generally current. From what has now been said, it will be seen that it may be very far from easy to supply this defect, since we have, as yet, no generally received system of mineralogical classification. Till we know what are really different species of minerals, and in what larger groups these species can be arranged, so as to have common properties, we shall never obtain a permanent mineralogical nomenclature. Thus *Leucocyclite* and *Tesselite* are minerals previously confounded with Apophyllite, which Sir John Herschel and Sir David Brewster distinguished by those names, in consequence of certain optical properties which they exhibit. But are these properties definite distinctions? and are there any external differences corresponding to them? If not, can we consider them as separate species? and if not separate species, ought they to have separate names? In like manner, we might ask if *Augite* and *Hornblende* are really the same species, as Gustavus Rose has maintained? if *Diallage* and *Hypersthene* are not definitely distinguished, which has been asserted by Kobell? Till such questions are settled, we cannot have a fixed nomenclature in mineralogy. What appears the best course to follow in the present state of the science, I shall consider when we come to speak of the form of technical terms.

I may, however, notice here that the main Forms of systematic nomenclature are two:—terms which are produced by combining words of higher and lower generality, as the binary names, consisting of the name of the genus and the species, generally employed by natural historians since the time of Linnæus;—and terms in which some relation of things is indicated by a change in the form of the word, for example, an alteration of its termination, of which kind of nomenclature we have a conspicuous example in the modern chemistry.

Aphorism X.

New terms and changes of terms, which are not needed in order to express truth, are to be avoided.

As the Seventh Aphorism asserted that novelties in language may be and ought to be introduced, when they aid the enunciation of truths, we now declare that they are not admissible in any other case. New terms and new systems of terms are not to be introduced, for example, in virtue of their own neatness or symmetry, or other merits, if there is no occasion for their use.

I may mention, as an old example of a superfluous attempt of this kind, an occurrence in the history of Astronomy. In 1628 John Bayer and Julius Schiller devised a *Cœlum Christianum*, in which the common names of the planets, &c., were replaced by those of Adam, Moses, and the Patriarchs. The twelve Signs became the twelve Apostles, and the constellations became sacred places and things. Peireskius, who had to pronounce upon the value of this proposal, praised the piety of the inventors, but did not approve, he said[1], the design of perverting and confounding whatever of celestial information from the period of the earliest memory is found in books.

Nor are slight anomalies in the existing language of science sufficient ground for a change, if they do not seriously interfere with the expression of our knowledge. Thus Linnæus says[2] that a fair generic name is not to be exchanged for another though apter one: and[3] if we separate an old genus into several, we must try to find names for them among the synonyms which describe the old genus. This maxim excludes the restoration of ancient names long disused, no less than the needless invention of new ones. Linnæus

[1] Gassendi, *Vita Peireskii*, 300. [2] *Phil. Bot.* 246. [3] *Ib.* 247.

lays down this rule[4]; and adds, that the botanists of the sixteenth century well nigh ruined botany by their anxiety to recover the ancient names of plants. In like manner Cuvier[5] laments it as a misfortune, that he has had to introduce many new names; and declares earnestly that he has taken great pains to preserve those of his predecessors.

The great bulk which the Synonymy of botany and of mineralogy have attained, shows us that this maxim has not been universally attended to. In these cases, however, the multiplication of different names for the same kind of object has arisen in general from ignorance of the identity of it under different circumstances, or from the want of a system which might assign to it its proper place. But there are other instances, in which the multiplication of names has arisen not from defect, but from excess, of the spirit of system. The love which speculative men bear towards symmetry and completeness is constantly at work, to make them create systems of classification more regular and more perfect than can be verified by the facts: and as good systems are closely connected with a good nomenclature, systems thus erroneous and superfluous lead to a nomenclature which is prejudicial to science. For although such a nomenclature is finally expelled, when it is found not to aid us in expressing the true laws of nature, it may obtain some temporary sway, during which, and even afterwards, it may be a source of much confusion.

We have a conspicuous example of such a result in the geological nomenclature of Werner and his school. Thus it was assumed, in Werner's system, that his *First*, *Second*, and *Third Flötz Limestone*, his *Old* and *New Red Sandstone*, were universal formations; and geologists looked upon it as their business to detect these strata in other countries. Names were thus assigned to the rocks of various parts of Europe, which created immense perplexity before they were again ejected. The geological terms which now prevail, for

4 *Phil. Bot.* 248. 5 *Règne Anim.* Pref. p. xvi.

instance, those of Smith, are for the most part not systematic, but are borrowed from accidents, as localities, or popular names; as *Oxford Clay* and *Cornbrash;* and hence they are not liable to be thrust out on a change of system. On the other hand we do not find sufficient reason to accept the system of names of strata proposed by Mr. Conybeare in the *Introduction to the Geology of England and Wales*, according to which the *Carboniferous Rocks* are the *Medial Order*,—having above them the *Supermedial Order* (*New Red Sand, Oolites and Chalk*), and above these the *Superior Order* (*Tertiary Rocks*); and again,—having below, the *Submedial Order* (the *Transition Rocks*), and the *Inferior Order* (*Mica Slate, Gneiss, Granite*). For though these names have long been proposed, it does not appear that they are useful in enunciating geological truths. We may, it would seem, pronounce the same judgment respecting the system of geological names proposed by M. Alexander Brongniart, in his *Tableau des Terrains qui composent l'écorce du Globe.* He divides these strata into nine classes, which he terms *Terrains Alluviens, Lysiens, Pyrogenes, Clysmiens, Yzemiens, Hemilysiens, Agalysiens, Plutoniques, Vulcaniques.* These classes are again variously subdivided: thus the Terrains Yzemiens are *Thalassiques, Pelagiques,* and *Abyssiques;* and the Abyssiques are subdivided into *Lias, Keuper, Conchiliens, Pœciliens, Peneens, Rudimentaires, Entritiques, Houillers, Carbonifers* and *Gres Rouge Ancien.* Scarcely any amount of new truths would induce geologists to burthen themselves at once with this enormous system of new names: but in fact, it is evident that any portion of truth, which any author can have brought to light, may be conveyed by means of a much simpler apparatus. Such a nomenclature carries its condemnation on its own face.

Nearly the same may be said of the systematic nomenclature proposed for mineralogy by Professor Mohs. Even if all his Genera be really natural groups, (a doctrine which we can have no confidence in till they are confirmed by the evidence of chemistry,) there is no necessity to make so great a change in the received names of minerals. His proceeding in this respect, so different from the temperance of Linnæus and Cuvier, has probably ensured a speedy oblivion to this part of his system. In crystallography, on the other hand, in which Mohs's improvements have been very valuable, there are several terms introduced by him, as *rhombohedron*, *scalenohedron*, *hemihedral*, *systems* of crystallization, which will probably be a permanent portion of the language of science.

I may remark, in general, that the only persons who succeed in making great alterations in the language of science, are not those who make names arbitrarily and as an exercise of ingenuity, but those who have much new knowledge to communicate; so that the vehicle is commended to general reception by the value of what it contains. It is only eminent discoverers to whom the authority is conceded of introducing a new system of names; just as it is only the highest authority in the state which has the power of putting a new coinage in circulation.

I will here quote some judicious remarks of Mr. Howard, which fall partly under this Aphorism, and partly under some which follow. He had proposed, as names for the kinds of clouds, the following: *Cirrus*, *Cirrocumulus*, *Cirrostratus*, *Cumulostratus*, *Cumulus*, *Nimbus*, *Stratus*. In an abridgment of his views, given in the Supplement to the *Encyclopædia Britannica*, English names were proposed as the equivalents of these; *Curlcloud*, *Sondercloud*, *Wanecloud*, *Twaincloud*, *Stackencloud*, *Raincloud*, *Fallcloud*. Upon these Mr. Howard observes: 'I mention these, in order to have the opportunity of saying that I do not adopt them. The names for the clouds which I deduced from the Latin, are but seven in number, and very easy to remember. They were intended as *arbitrary terms* for the *structure* of clouds, and the meaning of them was carefully fixed by a definition. The observer having once made himself master of this, was able to apply the term with correctness, after a little experience, to the subject under all its varieties of form, colour, or position. The new names, if meant to be another set of arbitrary terms, are superfluous; if intended to convey in themselves an explanation in English, they fail in this, by applying to some part or circumstance only of the definition; the *whole* of which must be kept in view to study the subject with success. To take for an example the first of the modifications. The term *cirrus* very readily takes an abstract meaning, equally applicable to the rectilinear as to the flexuous forms of the subject. But the name of *curl-cloud* will not, without some violence to its *obvious sense*, acquire this more extensive one: and will therefore be apt to mislead the reader rather than further his progress. Others of these names are as devoid of a meaning obvious to the English reader, as the Latin terms themselves. But the principal objection to English or any other local terms, remains to be stated. They take away from the nomenclature its general advantage of constituting, as far as it goes, an universal language, by means of which the intelligent of every country may convey to each other their ideas without the necessity of translation.'

I here adduce these as examples of the arguments against changing an established nomenclature. As grounds of selecting a new one, they may be taken into account hereafter.

Aphorism XI.

Terms which imply theoretical views are admissible, as far as the theory is proved.

It is not unfrequently stated that the circumstances from which the names employed in science borrow their meaning, ought to be facts and not theories. But such a recommendation implies a belief that facts are rigorously distinguished from theories and directly opposed to them; which belief, we have repeatedly seen, is unfounded. When theories are firmly established, they become facts; and names founded on such theoretical views are unexceptionable. If we speak of the *minor* *axis* of Jupiter's *orbit*, or of his *density*, or of *the angle of refraction*, or *the length of an undulation* of red light, we assume certain theories; but inasmuch as the theories are now the inevitable interpretation of ascertained facts, we can have no better terms to designate the conceptions thus referred to. And hence the rule which we must follow is, not that our terms must involve no theory, but that they imply the theory only in that sense in which it is the interpretation of the facts.

For example, the term *polarization* of light was objected to, as involving a theory. Perhaps the term was at first suggested by conceiving light to consist of particles having poles turned in a particular manner. But among intelligent speculators, the notion of polarization soon reduced itself to the simple conception of opposite properties in opposite positions, which is a bare statement of the fact: and the term being understood to have this meaning, is a perfectly good term, and indeed the best which we can imagine for designating what is intended.

I need hardly add the caution, that names involving theoretical views not in accordance with facts are to be rejected. The following instances exemplify both the positive and the negative application of this maxim.

The distinction of *primary* and *secondary* rocks in geology was founded upon a theory; namely, that those which do not contain any organic remains were first deposited, and afterwards, those which contain plants and animals. But this theory was insecure from the first. The difficulty of making the separation which it implied, led to the introduction of a class of *transition* rocks. And the recent researches of geologists lead them to the conclusion, that those rocks which are termed *primary*, may be the newest, not the oldest, productions of nature.

In order to avoid this incongruity, other terms have been proposed as substitutes for these. Sir C. Lyell remarks[1], that granite, gneiss, and the like, form a class

[1] *Princ. Geol.* iv. 386.

which should be designated by a common name; which name should not be of chronological import. He proposes *hypogene*, signifying 'nether-formed;' and thus he adopts the theory that they have not assumed their present form and structure at the surface, but determines nothing of the period when they were produced.

These hypogene rocks, again, he divides into unstratified or *plutonic*, and altered stratified, or *metamorphic;* the latter term implying the hypothesis that the stratified rocks to which it is applied have been altered, by the effect of fire or otherwise, since they were deposited. That fossiliferous strata, in some cases at least, have undergone such a change, is demonstrable from facts[2].

The modern nomenclature of chemistry implies the oxygen theory of chemistry. Hence it has sometimes been objected to. Thus Davy, in speaking of the Lavoisierian nomenclature, makes the following remarks, which, however plausible they may sound, will be found to be utterly erroneous[3]. 'Simplicity and precision ought to be the characteristics of a scientific nomenclature: words should signify *things*, or the *analogies* of things, and not *opinions*. ... A substance in one age supposed to be simple, in another is proved to be compound, and *vice versâ*. A theoretical nomenclature is liable to continual alterations: *oxygenated muriatic acid* is as improper a term as *dephlogisticated marine acid.* Every school believes itself to be in the right: and if every school assumes to itself the liberty of altering the names of chemical substances in consequence of *new ideas* of their composition, there can be no permanency in the language of the science; it must always be confused and uncertain. Bodies which are *similar* to each other should always be classed together; and there is a presumption that their composition is *analogous*. *Metals, earths, alkalis*, are appropriate names for the bodies they represent, and independent of all speculation: whereas *oxides, sulphurets*, and *muriates* are terms founded upon opinions of the composition of bodies, some of which have been already found erroneous.

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[2] *Elem. Geol.* p. 17.

[3] *Elements of Chem. Phil.* p. 46

The least dangerous mode of giving a systematic form to a language seems to be to signify the analogies of substances by some common sign affixed to the beginning or the termination of the word. Thus as the metals have been distinguished by a termination in *um*, as *aurum*, so their calciform or oxidated state might have been denoted by a termination in *a*, as *aura:* and no progress, however great, in the science could render it necessary that such a mode of appellation should be changed.'

These remarks are founded upon distinctions which have no real existence. We cannot separate *things* from their *properties*, nor can we consider their properties and analogies in any other way than by having *opinions* about them. By contrasting *analogies* with *opinions*, it might appear as if the author maintained that there were certain analogies about which there was no room for erroneous opinions. Yet the analogies of chemical compounds, are, in fact, those points which have been most the subject of difference of opinion, and on which the revolutions of theories have most changed men's views. As an example of analogies which are still recognized under alterations of theory, the writer gives the relation of a metal to its oxide or calciform state. But this analogy of metallic oxides, as Red Copper or Iron Ore, to Calx, or burnt lime, is very far from being self-evident;—so far indeed, that the recognition of the analogy was a great step in chemical *theory*. The terms which he quotes, *oxygenated muriatic acid* (and the same may be said of *dephlogisticated marine acid*,) if improper, are so not because they involve theory, but because they involve false theory;—not because those who framed them did not endeavour to express analogies, but because they expressed analogies about which they were mistaken. Unconnected names, as *metals*, *earths*, *alkalis*, are good as the *basis* of a systematic nomenclature, but they are not substitutes for such a nomenclature. A systematic nomenclature is an instrument of great utility and power, as the modern history of chemistry has shown. It would be highly unphilosophical to reject the use of such an instrument, because, in the course of the revolutions of science, we may have to modify, or even to remodel it altogether. Its utility is not by that means destroyed. It has retained, transmitted, and enabled us to reason upon, the doctrines of the earlier theory, so far as they are true; and when this theory is absorbed into a more comprehensive one, (for this, and not its refutation, is the end of a theory *so far as* it is true,) the nomenclature is easily translated into that which the new theory introduces. We have seen, in the history of astronomy, how valuable the theory of *epicycles* was, in its time: the nomenclature of the relations of a planet's orbit, which that theory introduced, was one of Kepler's resources in discovering the *elliptical* theory; and, though now superseded, is still readily intelligible to astronomers.

This is not the place to discuss the reasons for the *form* of scientific terms; otherwise we might ask, in reference to the objections to the Lavoisierian nomenclature, if such forms as *aurum* and *aura* are good to represent the absence or presence of oxygen, why such forms as *sulphite* and *sulphate* are not equally good to represent the presence of what we may call a smaller or larger dose of oxygen, so long as the oxygen theory is admitted in its present form; and to indicate still the difference of the same substances, if under any change of theory it should come to be interpreted in a new manner.

But I do not now dwell upon such arguments, my object in this place being to show that terms involving theory are not only allowable, if understood so far as the theory is proved, but of great value, and indeed of indispensable use, in science. The objection to them is inconsistent with the objects of science. If, after all that has been done in chemistry or any other science, we have arrived at no solid knowledge, no permanent truth;—if all that we believe now may be proved to be false to-morrow;—then indeed our opinions and theories are corruptible elements, on which it would be unwise to rest any thing important, and which we might wish to exclude, even from our names. But if our knowledge has no more security than this, we can find no reason why we should wish at all to have names of things, since the names are needed mainly that we may reason upon and increase our knowledge such as it is. If we are condemned to endless alternations of varying opinions, then, no doubt, our theoretical terms may be a source of confusion; but then, where would be the advantage of their being otherwise? what would be the value of words which should express in a more precise manner opinions equally fleeting? It will perhaps be said, our terms must express facts, not theories: but of this distinction so applied we have repeatedly shown the futility. Theories firmly established are facts. Is it not a fact that the rusting of iron arises from the metal combining with the oxygen of the atmosphere? Is it not a fact that a combination of oxygen and hydrogen produces water? That our terms should express *such* facts, is precisely what we are here inculcating.

Our examination of the history of science has led us to a view very different from that which represents it as consisting in the succession of hostile opinions. It is, on the contrary, a progress, in which each step is recognized and employed in the succeeding one. Every theory, so far as it is true, (and all that have prevailed extensively and long, contain a large portion of truth,) is taken up into the theory which succeeds and seems to expel it. All the narrower inductions of the first are included in the more comprehensive generalizations of the second. And this is performed mainly by means of such terms as we are now considering;—terms involving the previous theory. It is by means of such terms, that the truths at first ascertained become so familiar and manageable, that they can be employed as elementary facts in the formation of higher inductions.

These principles must be applied also, though with great caution, and in a temperate manner, even to descriptive language. Thus the mode of describing the forms of crystals adopted by Werner and Romé de l'Isle was to consider an original form, from which other forms are derived by *truncations* of the edges and the angles. Haüy's method of describing the same forms, was to consider them as built up of rows of small solids, the angles being determined by the *decrements* of these rows. Both these methods of description involve hypothetical views; and the last was intended to rest on a true physical theory of the constitution of crystals. Both hypotheses are doubtful or false: yet both these methods are good as modes of description: nor is Haüy's terminology vitiated, if we suppose (as in fact we must suppose in many instances,) that crystalline bodies are not really made up of such small solids. The mode of describing an octahedron of fluor spar, as derived from the cube, by decrements of one row on all the edges, would still be proper and useful as a description, whatever judgment we should form of the material structure of the body. But then, we must consider the solids which are thus introduced into the description as merely hypothetical geometrical forms, serving to determine the angles of the faces. It is in this way alone that Haüy's nomenclature can now be retained.

In like manner we may admit theoretical views into the descriptive phraseology of other parts of Natural History: and the theoretical terms will replace the obvious images, in proportion as the theory is generally accepted and familiarly applied. For example, in speaking of the Honeysuckle, we may say that the upper leaves are *perfoliate*, meaning that a single round leaf is perforated by the stalk, or threaded upon it. Here is an image which sufficiently conveys the notion of the form. But it is now generally recognized that this apparent single leaf is, in fact, two opposite leaves joined together at their bases. If this were doubted, it may be proved by comparing the upper leaves with the lower, which are really separate and opposite. Hence the term *connate* is applied to these conjoined opposite leaves, implying that they grow together; or they are called *connato-perfoliate*. Again; formerly the corolla was called *monopetalous* or *polypetalous*, as it consisted of one part or of several: but it is now agreed among botanists that those corollas which appear to consist of a single part, are, in fact, composed of several soldered together; hence the term *gamopetalous* is now employed (by De Candolle and his followers) instead of monopetalous[4].

In this way the language of Natural History not only expresses, but inevitably implies, general laws of nature; and words are thus fitted to aid the progress of knowledge in this, as in other provinces of science.

Aphorism XII.

If terms are systematically good, they are not to be rejected because they are etymologically inaccurate.

Terms belonging to a system are defined, not by the meaning of their radical words, but by their place in the system. That they should be appropriate in their signification, aids the processes of introducing and remembering them, and should therefore be carefully attended to by those who invent and establish them; but this once done, no objections founded upon their etymological import are of any material weight. We find no inconvenience in the circumstance that *geometry* means the measuring of the earth, that the name *porphyry* is applied to many rocks which have no fiery spots, as the word implies, and *oolite* to strata which have no roelike structure. In like manner, if the term *pœcilite* were already generally received, as the name of a certain group of strata, it would be no valid ground for quarrelling with it, that this group was not always variegated in colour, or that other groups were equally variegated: although undoubtedly in *introducing* such a term, care should be taken to make it as distinctive as possible. It often happens, as we have seen, that by the natural progress of changes in language, a word is steadily confirmed in a sense quite different from its etymological import. But though

[4] On this subject, see Illiger, *Versuch einer Systematischen Vollständigen Terminologie für das Thierreich und Pflanzenreich* (1810). De Candolle, *Théorie Élémentaire de la Botanique.*

we may accept such instances, we must not wantonly attempt to imitate them. I say, not wantonly: for if the progress of scientific identification compel us to follow any class of objects into circumstances where the derivation of the term is inapplicable, we may still consider the term as an unmeaning sound, or rather an historical symbol, expressing a certain member of our system. Thus if, in following the course of the *mountain* or *carboniferous* limestone, we find that in Ireland it does not form mountains nor contain coal, we should act unwisely in breaking down the nomenclature in which our systematic relations are already expressed, in order to gain, in a particular case, a propriety of language which has no scientific value.

All attempts to act upon the maxim opposite to this, and to make our scientific names properly descriptive of the objects, have failed and must fail. For the marks which really distinguish the natural classes of objects, are by no means obvious. The discovery of them is one of the most important steps in science; and when they are discovered, they are constantly liable to exceptions, because they do not contain the essential differences of the classes. The natural order *Umbellatæ*, in order to be a natural order, must contain some plants which have not umbels, as *Eryngium*[1]. 'In such cases,' said Linnæus, 'it is of small import what you *call* the order, if you take a proper series of plants, and give it some name which is clearly understood to apply to the plants you have associated.' 'I have,' he adds, 'followed the rule of borrowing the name *à fortiori*, from the principal feature.'

The distinction of crystals into systems according to the degree of symmetry which obtains in them, has been explained elsewhere. Two of these systems, of which the relation as to symmetry might be expressed by saying that one is *square pyramidal* and the other *oblong pyramidal*, or the first *square prismatic* and the second *oblong prismatic*, are termed by Mohs, the first, *Pyramidal*, and the second *Prismatic*. And it may

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[1] See *Hist. Ind. Sc.* b. xvi. c. iv. sect. 5.

be doubted whether it is worth while to invent other terms, though these are thus defective in characteristic significance. As an example of a needless rejection of old terms in virtue of a supposed impropriety in their meaning, I may mention the attempt made in the last edition of Haüy's *Mineralogy*, to substitute *autopside* and *heteropside* for *metallic* and *unmetallic*. It was supposed to be proved that all bodies have a metal for their basis; and hence it was wished to avoid the term *unmetallic*. But the words *metallic* and *unmetallic* may mean that minerals *seem* metallic and unmetallic, just as well as if they contained the element *opside* to imply this seeming. The old names express all that the new express, and with more simplicity, and therefore should not be disturbed.

The maxim on which we are now insisting, that we are not to be too scrupulous about the etymology of scientific terms, may, at first sight, appear to be at variance with our Fourth Aphorism, that words used technically are to retain their common meaning as far as possible. But it must be recollected, that in the Fourth Aphorism we spoke of *common* words *appropriated* as technical terms; we here speak of words *constructed* for scientific purposes. And although it is, perhaps, impossible to draw a broad line between these two classes of terms, still the rule of propriety may be stated thus: In technical terms, deviations from the usual meaning of words are bad in proportion as the words are more familiar in our own language. Thus we may apply the term *Cirrus* to a cloud composed of filaments, even if these filaments are straight; but to call such a cloud a *Curl cloud* would be much more harsh.

Since the names of things, and of classes of things, when constructed so as to involve a description, are constantly liable to become bad, the natural classes shifting away from the descriptive marks thus prematurely and casually adopted, I venture to lay down the following maxim.

Aphorism XIII.

The fundamental terms of a system of Nomenclature may be conveniently borrowed from casual or arbitrary circumstances.

For instance, the names of plants, of minerals, and of geological strata, may be taken from the places where they occur conspicuously or in a distinct form; as *Parietaria*, *Parnassia*, *Chalcedony*, *Arragonite*, *Silurian* system, *Purbeck* limestone. These names may be considered as at first supplying standards of reference; for in order to ascertain whether any rock be *Purbeck* limestone, we might compare it with the rocks in the Isle of Purbeck. But this reference to a local standard is of authority only till the place of the object in the system, and its distinctive marks, are ascertained. It would not vitiate the above names, if it were found that the *Parnassia* does not grow on Parnassus; that *Chalcedony* is not found in Chalcedon; or even that *Arragonite* no longer occurs in Arragon; for it is now firmly established as a mineral species. Even in geology such a reference is arbitrary, and may be superseded, or at least modified, by a more systematic determination. *Alpine* limestone is no longer accepted as a satisfactory designation of a rock, now that we know the limestone of the Alps to be of various ages.

Again, names of persons, either casually connected with the object, or arbitrarily applied to it, may be employed as designations. This has been done most copiously in botany, as for example, *Nicotiana*, *Dahlia*, *Fuchsia*, *Jungermannia*, *Lonicera*. And Linnæus has laid down rules for restricting this mode of perpetuating the memory of men, in the names of plants. Those generic names, he says[1], which have been constructed to preserve the memory of persons who have deserved well of botany, are to be religiously retained. This, he adds, is the sole and supreme reward of the botanist's labours, and must be carefully guarded and

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[1] *Phil. Bot.* 241.

scrupulously bestowed, as an encouragement and an honour. Still more arbitrary are the terms borrowed from the names of the gods and goddesses, heroes and heroines of antiquity, to designate new genera in those departments of natural history in which so many have been discovered in recent times as to weary out all attempts at descriptive nomenclature. Cuvier has countenanced this method. 'I have had to frame many new names of genera and sub-genera,' he says[2], 'for the sub-genera which I have established were so numerous and various, that the memory is not satisfied with numerical indications. These I have chosen either so as to indicate some character, or among the usual denominations, which I have latinized, or finally, after the example of Linnæus, among the names of mythology, which are in general agreeable to the ear, and which are far from being exhausted.'

This mode of framing names from the names of persons to whom it was intended to do honour, has been employed also in the mathematical and chemical sciences; but such names have rarely obtained any permanence, except when they recorded an inventor or discoverer. Some of the constellations, indeed, have retained such appellations, as *Berenice's Hair;* and the new star which shone out in the time of Cæsar, would probably have retained the name given to it, of the *Julian Star*, if it had not disappeared again soon after. In the map of the Moon, almost all the parts have had such names imposed upon them by those who have constructed such maps, and these names have very properly been retained. But the names of new planets and satellites thus suggested have not been generally accepted; as the *Medicean* stars, the name employed by Galileo for the satellites of Jupiter; the *Georgium Sidus*, the appellation proposed by Herschel for Uranus when first discovered[3]; Ceres *Ferdinandea*,

[2] *Règne An.* p. 16.

[3] In this case, the name *Uranus*, selected with a view to symmetry according to the mythological order of descent of the persons (*Uranus*, *Saturn*, *Jupiter*, *Mars*) was adopted by astronomers in general, though not proposed or sanctioned by the

the name which Piazzi wished to impose on the small planet Ceres. The names given to astronomical Tables by the astronomers who constructed them have been most steadily adhered to, being indeed names of books, and not of natural objects. Thus there were the *Ilchanic*, the *Alphonsine*, the *Rudolphine*, the *Carolinian* Tables. Comets which have been ascertained to be periodical, have very properly had assigned to them the name of the person who established this point; and of these we have thus, *Halley's*, *Encke's Comet*, and *Biela's* or *Gambart's Comet*.

In the case of discoveries in science or inventions of apparatus, the name of the inventor is very properly employed as the designation. Thus we have the *Torricellian* Vacuum, the *Voltaic* Pile, *Fahrenheit's* Thermometer. And in the same manner with regard to laws of nature, we have *Kepler's* Laws, *Boyle* or *Mariotte's* law of the elasticity of air, *Huyghens's* law of double refraction, *Newton's* scale of colours. *Descartes'* law of refraction is an unjust appellation; for the discovery of the law of sines was made by Snell. In deductive mathematics, where the invention of a theorem is generally a more definite step than an induction, this mode of designation is more common, as *Demoivre's* Theorem, *Maclaurin's* Theorem, *Lagrange's* Theorem, *Eulerian* Integrals.

In the *History of Science*[4] I have remarked that in the discovery of what is termed galvanism, Volta's

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discoverer of the new planet. In the cases of the smaller planets, *Ceres*, *Pallas*, *Juno*, and *Vesta*, the names were given either by the discoverer, or with his sanction. Following this rule, Bessel gave the name of *Astræa* to a new planet discovered in the same region by Mr. Hencke, as mentioned in the additions to book vii. of the *History* (2nd Ed.). Following the same rule, and adhering as much as possible to mythological connexion, the astronomers of Europe have, with the sanction of M. Le Verrier, given the name of *Neptune* to the planet revolving beyond Uranus, and discovered in consequence of his announcement of its probable existence, which had been inferred by Mr. Adams and him (calculating in ignorance of each other's purpose) from the perturbations of Uranus; as I have stated in the Additions to the Third Edition of the *History*.

[4] b. xiii. c. 1.

office was of a higher and more philosophical kind than that of Galvani; and I have, on this account, urged the propriety of employing the term *voltaic*, rather than *galvanic* electricity. I may add that the electricity of the common machine is often placed in contrast with this, and appears to require an express name. Mr. Faraday calls it *common* or *machine* electricity; but I think that *franklinic* electricity would form a more natural correspondence with *voltaic*, and would be well justified by Franklin's place in the history of that part of the subject.

APHORISM XIV.

The Binary Method of Nomenclature (Names by Genus and Species) is the most convenient hitherto employed in Classification.

THE number of species in every province of Natural History is so vast that we cannot distinguish them and record the distinctions without some artifice. The known species of plants, for instance, were 10,000 in the time of Linnæus, and are now probably 60,000. It would be useless to endeavour to frame and employ separate names for each of these species.

The division of the objects into a subordinated system of classification enables us to introduce a Nomenclature which does not require this enormous number of names. The artifice employed is, to name a specimen by means of two (or it might be more) steps of the successive division. Thus in Botany, each of the Genera has its name, and the species are marked by the addition of some epithet to the name of the genus. In this manner about 1,700 Generic Names, with a moderate number of Specific Names, were found by Linnæus sufficient to designate with precision all the species of vegetables known at his time. And this *Binary Method* of Nomenclature has been found so convenient, that it has been universally adopted in every other department of the Natural History of organized beings.

Many other modes of Nomenclature have been tried, but no other has at all taken root. Linnæus himself appears at first to have intended marking each species by the Generic Name, accompanied by a characteristic Descriptive Phrase; and to have proposed the employment of a *Trivial* Specific Name, as he termed it, only as a method of occasional convenience. The use of these *trivial names*, however, has become universal, as we have said; and is by many persons considered the greatest improvement introduced at the Linnæan reform.

Aphorism XV.

The Maxims of Linnæus concerning the Names to be used in Botany, (Philosophia Botanica, Nomina. Sections 210 to 255) *are good examples of Aphorisms on this subject.*

Both Linnæus and other writers (as Adanson) have given many maxims with a view of regulating the selection of generic and specific names. The maxims of Linnæus were intended as much as possible to exclude barbarism and confusion, and have, upon the whole, been generally adopted.

These canons, and the sagacious modesty of great botanists, like Robert Brown, in conforming to them, have kept the majority of good botanists within salutary limits; though many of these canons were objected to by the contemporaries of Linnæus (Adanson and others[1]) as capricious and unnecessary restrictions.

Many of the names introduced by Linnæus certainly appear fanciful enough. Thus he gives the name *Bauhinia* to a plant which has leaves in pairs, because the Bauhins were a pair of brothers. *Banisteria* is the name of a climbing plant in honour of Banister, who travelled among mountains. But such names once established by adequate authority lose all their inconvenience and easily become permanent, and hence the reasonableness of one of the Linnæan rules[2]:—

That as such a perpetuation of the names of persons

[1] Pref. cxxix. clxxii.

[2] *Phil. Bot.* s. 239.

by the names of plants is the only honour that botanists have to bestow, it ought to be used with care and caution, and religiously respected.

[3rd ed. It may serve to show how sensitive botanists are to the allusions contained in such names, that it has been charged against Linnæus, as a proof of malignity towards Buffon, that he changed the name of the genus *Buffonia*, established by Sauvages, into *Bufonia*, which suggested a derivation from *Bufo*, a toad. It appears to be proved that the spelling was not Linnæus's doing.]

Another Linnæan maxim is (Art. 219), that the generic name must be fixed before we attempt to form a specific name; 'the latter without the former is like the clapper without the bell.'

The name of the genus being fixed, the species may be marked (Art. 257) by adding to it 'a single word taken at will from any quarter;' that is, it need not involve a description or any essential property of the plant, but may be a casual or arbitrary appellation. Thus the various species of *Hieracium*[3] are *Hieracium Alpinum*, *H. Halleri*, *H. Pilosella*, *H. dubium*, *H. murorum*, &c., where we see how different may be the kind of origin of the words.

Attempts have been made at various times to form the names of species from those of genera in some more symmetrical manner. But these have not been successful, nor are they likely to be so; and we shall venture to propound an axiom in condemnation of such names.

APHORISM XVI.

Numerical names in Classification are bad; and the same may be said of other names of kinds, depending upon any fixed series of notes of order.

WITH regard to numerical names of kinds, of species for instance, the objections are of this nature. Besides that such names offer nothing for the imagination to take hold of, new discoveries will probably alter the

[3] Hooker, *Fl. Scot.* 228.

numeration, and make the names erroneous. Thus, if we call the species of a genus 1, 2, 3, a new species intermediate between 1 and 2, 2 and 3, &c. cannot be put in its place without damaging the numbers.

The geological term *Trias*, lately introduced to designate the group consisting of the *three* members (Bunter Sandstein, Muschelkalk, and Keuper) becomes improper if, as some geologists hold, two of these members cannot be separated.

Objections resembling those which apply to numerical designations of species, apply to other cases of fixed series : for instance, when it has been proposed to mark the species by altering the termination of the genus. Thus Adanson[1], denoting a genus by the name *Fonna* (*Lychnidea*), conceived he might mark five of its species by altering the last syllable, *Fonna*, *Fonna-e*, *Fonna-i*, *Fonna-o*, *Fonna-u;* then others by *Fonna-ba*, *Fonna-ka*, and so on. This would be liable to the same evils which have been noticed as belonging to the numerical method[2].

Aphorism XVII.

In any classificatory science names including more than two steps of the classification may be employed if it be found convenient.

Linnæus, in his canons for botanical nomenclature (Art. 212), says that the names of the class and the order are to be *mute*, while the names of the Genus and Species are *sonorous*. And accordingly the names

[1] Pref. clxxvi.

[2] In like manner the names assigned by Mr. Rickman to the successive styles of Gothic architecture in England,—*Early English*, *Decorated*, and *Perpendicular*,—cannot be replaced by numerical designations, *First Pointed*, *Second Pointed*, *Third Pointed*. For—besides that he who first distinctly establishes classes has the right of naming them, and that Mr. Rickman's names are really appropriate and significant—these new names would confound all meaning of language. We should not be able to divide Early English, or Decorated, or Perpendicular into substyles;—for who could talk of *First Second Pointed* and *Second Second Pointed;* and what should we call *that* pointed style—the *Transition* from the Norman—which precedes the *First Pointed?*

of plants (and the same is true of animals) have in common practice been binary only, consisting of a generic and a specific name. The class and the order have not been admitted to form part of the appellation of the species. Indeed it is easy to see that a name, which must be identical in so many instances as that of an Order would be, would be felt as superfluous and burthensome. Accordingly, Linnæus makes it one of his maxims[1], that the name of the Class and Order must not be expressed but understood, and hence, he says, Royen, who took *Lilium* for the name of a Class, rightly rejected this word as a generic name, and substituted *Lirium* with the Greek termination.

Yet we must not too peremptorily assume such maxims as these to be universal for all classificatory sciences. It is very possible that it may be found advisable to use *three* terms, that of Order, Genus, and Species in designating minerals, as is done in Mohs's nomenclature, for example, *Rhombohedral Calc Haloide, Paratomous Hal Baryte.*

It is possible also that it may be found useful in the same science (Mineralogy) to mark some of the steps of classification by the termination. Thus it has been proposed to confine the termination *ite* to the Order *Silicides* of Naumann, as Apophyll*ite*, Stilb*ite*, Leuc*ite*, &c., and to use names of different form in other orders, as Talc *Spar* for Brennerite, Pyramidal Titanium *Oxide* for Octahedrite. Some such method appears to be the most likely to give us a tolerable mineralogical nomenclature.

Aphorism XVIII.

In forming a Terminology, words may be invented when necessary, but they cannot be conveniently borrowed from casual or arbitrary circumstances[2].

It will be recollected that Terminology is a language employed for describing objects, Nomenclature, a body

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1 *Phil. Bot.* s. 215.

2 I may also refer to *Hist. Sc. Id.* b. viii. c. ii. sec. 2, for some remarks on Terminology.

of names of the objects themselves. The *names*, as was stated in the last maxim, may be arbitrary; but the *descriptive terms* must be borrowed from words of suitable meaning in the modern or the classical languages. Thus the whole terminology which Linnæus introduced into botany, is founded upon the received use of Latin words, although he defined their meaning so as to make it precise when it was not so, according to Aphorism V. But many of the terms were invented by him and other botanists, as *Perianth*, *Nectary*, *Pericarp;* so many, indeed, as to form, along with the others, a considerable language. Many of the terms which are now become familiar were originally invented by writers on botany. Thus the word *Petal*, for one division of the corolla, was introduced by Fabius Columna. The term *Sepal* was devised by Neckar to express each of the divisions of the calyx. And up to the most recent times, new denominations of parts and conditions of parts have been devised by botanists, when they found them necessary, in order to mark important differences or resemblances. Thus the general *Receptacle* of the flower, as it is termed by Linnæus, or *Torus* by Salisbury, is continued into organs which carry the stamina and pistil, or the pistil alone, or the whole flower; this organ has hence been termed[2] *Gonophore*, *Carpophore*, and *Anthophore*, in these cases.

In like manner when Cuvier had ascertained that the lower jaws of Saurians consisted always of six pieces having definite relations of form and position, he gave names to them, and termed them respectively the *Dental*, the *Angular*, the *Coronoid*, the *Articular*, the *Complementary*, and the *Opercular* Bones.

In all these cases, the descriptive terms thus introduced have been significant in their derivation. An attempt to circulate a perfectly arbitrary word as a means of description would probably be unsuccessful. We have, indeed, some examples approaching to arbitrary designations, in the Wernerian names of colours,

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[2] De Candolle's *Th. El.* 405.

which are a part of the terminology of Natural History. Many of these names are borrowed from natural resemblances, as *Auricula purple, Apple green, Straw yellow;* but the names of others are taken from casual occurrences, mostly, however, such as were already recognized in common language, as *Prussian blue, Dutch orange, King's yellow.*

The extension of arbitrary names in scientific terminology is by no means to be encouraged. I may mention a case in which it was very properly avoided. When Mr. Faraday's researches on Voltaic electricity had led him to perceive the great impropriety of the term *poles,* as applied to the apparatus, since the processes have not reference to any opposed points, but to two opposite directions of a path, he very suitably wished to substitute for the phrases *positive pole* and *negative pole,* two words ending in *ode,* from ὅδος, a way. A person who did not see the value of our present maxim, that descriptive terms should be descriptive in their origin, might have proposed words perfectly arbitrary, as *Alphode,* and *Betode:* or, if he wished to pay a tribute of respect to the discoverers in this department of science, *Galvanode* and *Voltaode.* But such words would very justly have been rejected by Mr. Faraday, and would hardly have obtained any general currency among men of science. *Zincode* and *Platinode,* terms derived from the metal which, in one modification of the apparatus, forms what was previously termed the pole, are to be avoided, because in their origin too much is casual; and they are not a good basis for derivative terms. The pole at which the zinc is, is the Anode or Cathode, according as it is associated with different metals. Either the *Zincode* must sometimes mean the pole at which the Zinc is, and at other times that at which the Zinc is not, or else we must have as many names for poles as there are metals. *Anode* and *Cathode,* the terms which Mr. Faraday adopted, were free from these objections; for they refer to a natural standard of the direction of the voltaic current, in a manner which, though perhaps not obvious at first sight, is easily understood and retained. *An*ode and *Cath*ode, the *rising* and the *setting* way, are the directions which correspond to east and west in that voltaic current to which we must ascribe terrestrial magnetism. And with these words it was easy to connect *Anïon* and *Cathïon*, to designate the opposite elements which are separated and liberated at the two *Electrodes*.

Aphorism XIX.

The meaning of Technical Terms must be fixed by convention, not by casual reference to the ordinary meaning of words.

In fixing the meaning of the Technical Terms which form the Terminology of any science, at least of the descriptive Terms, we necessarily fix, at the same time, the perceptions and notions which the Terms are to convey to a hearer. What do we mean by *apple-green* or *French grey?* It might, perhaps, be supposed that, in the first example, the term *apple*, referring to so familiar an object, sufficiently suggests the colour intended. But it may easily be seen that this is not true; for apples are of many different hues of green, and it is only by a conventional selection that we can appropriate the term to one special shade. When this appropriation is once made, the term refers to the sensation, and not to the parts of this term; for these enter into the compound merely as a help to the memory, whether the suggestion be a natural connexion as in 'apple-green,' or a casual one as in 'French grey.' In order to derive due advantage from technical terms of this kind, they must be associated *immediately* with the perception to which they belong; and not connected with it through the vague usages of common language. The memory must retain the sensation; and the technical word must be understood as directly as the most familiar word, and more distinctly. When we find such terms as *tin-white* or *pinchbeck-brown*, the metallic colour so denoted ought to start up in our memory without delay or search.

This, which it is most important to recollect with respect to the simpler properties of bodies, as colour and form, is no less true with respect to more compound notions. In all cases the term is fixed to a peculiar meaning by convention; and the student, in order to use the word, must be completely familiar with the convention, so that he has no need to frame conjectures from the word itself. Such conjectures would always be insecure, and often erroneous. Thus the term *papilionaceous*, applied to a flower, is employed to indicate, not only a resemblance to a butterfly, but a resemblance arising from five petals of a certain peculiar shape and arrangement; and even if the resemblance to a butterfly were much stronger than it is in such cases, yet if it were produced in a different way, as, for example, by one petal, or two only, instead of a 'standard,' two 'wings,' and a 'keel' consisting of two parts more or less united into one, we should no longer be justified in speaking of it as a 'papilionaceous' flower.

The formation of an exact and extensive descriptive language for botany has been executed with a degree of skill and felicity, which, before it was attained, could hardly have been dreamt of as attainable. Every part of a plant has been named; and the form of every part, even the most minute, has had a large assemblage of descriptive terms appropriated to it, by means of which the botanist can convey and receive knowledge of form and structure, as exactly as if each minute part were presented to him vastly magnified. This acquisition was part of the Linnæan Reform, of which we have spoken in the *History*. 'Tournefort,' says De Candolle[1], 'appears to have been the first who really perceived the utility of fixing the sense of terms in such a way as always to employ the same word in the same sense, and always to express the same idea by the same word; but it was Linnæus who really created and fixed this botanical language, and this is his fairest claim to glory, for by this fixation of language he has shed clearness and precision over all parts of the science.'

[1] *Théor. Elém.* p. 327.

It is not necessary here to give any detailed account of the terms of botany. The fundamental ones have been gradually introduced, as the parts of plants were more carefully and minutely examined. Thus the flower was successively distinguished into the *calyx*, the *corolla*, the *stamens*, and the *pistils*: the sections of the corolla were termed *petals* by Columna; those of the calyx were called *sepals* by Neckar[2]. Sometimes terms of greater generality were devised; as *perianth* to include the calyx and corolla, whether one or both of these were present[3]; *pericarp* for the part inclosing the grain, of whatever kind it be, fruit, nut, pod, &c. And it may easily be imagined that descriptive terms may, by definition and combination, become very numerous and distinct. Thus leaves may be called *pinnatifid*[4], *pinnatipartite*, *pinnatisect*, *pinnatilobate*, *palmatifid*, *palmatipartite*, &c., and each of these words designates different combinations of the modes and extent of the divisions of the leaf with the divisions of its outline. In some cases arbitrary numerical relations are introduced into the definition: thus a leaf is called *bilobate*[5] when it is divided into two parts by a notch; but if the notch go to the middle of its length, it is *bifid;* if it go near the base of the leaf, it is *bipartite;* if to the base, it is *bisect*. Thus, too, a pod of a cruciferous plant is a *silica*[6] if it be four times as long as it is broad, but if it be shorter than this it is a *silicula*. Such terms being established, the form of the very complex leaf or frond of a fern is exactly conveyed, for example, by the following phrase: 'fronds rigid pinnate, pinnæ recurved subunilateral pinnatifid, the segments linear undivided or bifid spinuloso-serrate[7].'

Other characters, as well as form, are conveyed with the like precision: Colour by means of a classified scale of colours, as we have seen in speaking of the Measures

[2] De Candolle, 329.

[3] For this Erhart and De Candolle use *Perigone*.

[4] De Candolle, 318.

[5] *Ibid.* 493.

[6] *Ibid.* 422.

[7] Hooker, *Brit. Flo.* p. 450. *Hymenophyllum Wilsoni*, Scottish filmy-fern, abundant in the highlands of Scotland and about Killarney.

of Secondary Qualities; to which, however, we must add, that the naturalist employs arbitrary names, (such as we have already quoted,) and not mere numerical exponents, to indicate a certain number of selected colours. This was done with most precision by Werner, and his scale of colours is still the most usual standard of naturalists. Werner also introduced a more exact terminology with regard to other characters which are important in mineralogy, as lustre, hardness. But Mohs improved upon this step by giving a numerical scale of hardness, in which *talc* is 1, *gypsum* 2, *calc spar* 3, and so on, as we have already explained in the History of Mineralogy. Some properties, as specific gravity, by their definition give at once a numerical measure; and others, as crystalline form, require a very considerable array of mathematical calculation and reasoning, to point out their relations and gradations. In all cases the features of likeness in the objects must be rightly apprehended, in order to their being expressed by a distinct terminology. Thus no terms could describe crystals for any purpose of natural history, till it was discovered that in a class of minerals the proportion of the faces might vary, while the angle remained the same. Nor could crystals be described so as to distinguish species, till it was found that the derived and primitive forms are connected by very simple relations of space and number. The discovery of the mode in which characters must be apprehended so that they may be considered as *fixed* for a class, is an important step in the progress of each branch of Natural History; and hence we have had, in the History of Mineralogy and Botany, to distinguish as important and eminent persons those who made such discoveries, Romé de Lisle and Haüy, Cesalpinus and Gesner.

By the continued progress of that knowledge of minerals, plants, and other natural objects, in which such persons made the most distinct and marked steps, but which has been constantly advancing in a more gradual and imperceptible manner, the most important and essential features of similarity and dissimilarity in such objects have been selected, arranged, and fitted with names; and we have thus in such departments, systems of Terminology which fix our attention upon the resemblances which it is proper to consider, and enable us to convey them in words.

The following Aphorisms respect the Form of Technical Terms.

By the *Form* of terms, I mean their philological conditions; as, for example, from what languages they may be borrowed, by what modes of inflexion they must be compounded, how their derivatives are to be formed, and the like. In this, as in other parts of the subject, I shall not lay down a system of rules, but shall propose a few maxims.

Aphorism XX.

The two main conditions of the Form of technical terms are, that they must be generally intelligible, and susceptible of such grammatical relations as their scientific use requires.

These conditions may at first appear somewhat vague, but it will be found that they are as definite as we could make them, without injuriously restricting ourselves. It will appear, moreover, that they have an important bearing upon most of the questions respecting the form of the words which come before us; and that if we can succeed in any case in reconciling the two conditions, we obtain terms which are practically good, whatever objections may be urged against them from other considerations.

1. The former condition, for instance, bears upon the question whether scientific terms are to be taken from the learned languages, Greek and Latin, or from our own. And the latter condition very materially affects the same question, since in English we have scarcely any power of inflecting our words; and therefore must have recourse to Greek or Latin in order to obtain terms which admit of grammatical modification. If we were content with the term *Heat*, to express the *science* of heat, still it would be a bad technical term, for we cannot derive from it an adjective like *thermo-*

tical. If *bed* or *layer* were an equally good term with *stratum*, we must still retain the latter, in order that we may use the derivative *Stratification*, for which the English words cannot produce an equivalent substitute. We may retain the words *lime* and *flint*, but their adjectives for scientific purposes are not *limy* and *flinty*, but *calcareous* and *siliceous;* and hence we are able to form a compound, as *calcareo-siliceous*, which we could not do with indigenous words. We might fix the phrases *bent back* and *broken* to mean (of optical rays) that they are *reflected* and *refracted;* but then we should have no means of speaking of the angles of *Reflection* and *Refraction*, of the *Refractive* Indices, and the like.

In like manner, so long as anatomists described certain parts of a vertebra as *vertebral laminæ*, or *vertebral plates*, they had no adjective whereby to signify the properties of these parts; the term *Neurapophysis*, given to them by Mr. Owen, supplies the corresponding expression *neurapophysial.* So again, the term *Basisphenoid*, employed by the same anatomist, is better than *basilar* or *basial process of the sphenoid*, because it gives us the adjective *basisphenoidal.* And the like remark applies to other changes recently proposed in the names of portions of the skeleton.

Thus one of the advantages of going to the Greek and Latin languages for the origin of our scientific terms is, that in this way we obtain words which admit of the formation of adjectives and abstract terms, and of composition, and of other inflexions. Another advantage of such an origin is, that such terms, if well selected, are readily understood over the whole lettered world. For this reason, the descriptive language of science, of botany for instance, has been, for the most part, taken from the Latin; many of the terms of the mathematical and chemical sciences have been derived from the Greek; and when occasion occurs to construct a new term, it is generally to that language that recourse is had. The advantage of such terms is, as has already been intimated, that they constitute an universal language, by means of which cultivated persons in every country may convey to each other their ideas without the need of translation.

On the other hand, the advantage of indigenous terms is, that so far as the language extends, they are intelligible much more clearly and vividly than those borrowed from any other source, as well as more easily manageable in the construction of sentences. In the descriptive language of botany, for example, in an English work, the terms *drooping*, *nodding*, *one-sided*, *twining*, *straggling*, appear better than *cernuous*, *nutant*, *secund*, *volubile*, *divaricate*. For though the latter terms may by habit become as intelligible as the former, they cannot become more so to any readers; and to most English readers they will give a far less distinct impression.

2. Since the advantage of indigenous over learned terms, or the contrary, depends upon the balance of the capacity of inflexion and composition on the one hand, against a ready and clear significance on the other, it is evident that the employment of scientific terms of the one class or of the other may very properly be extremely different in different languages. The German possesses in a very eminent degree that power of composition and derivation, which in English can hardly be exercised at all, in a formal manner. Hence German scientific writers use native terms to a far greater extent than do our own authors. The descriptive terminology of botany, and even the systematic nomenclature of chemistry, are represented by the Germans by means of German roots and inflexions. Thus the description of *Potentilla anserina*, in English botanists, is that it has *Leaves interruptedly pinnate*, *serrate*, *silky*, *stem creeping*, *stalks axillar*, *one-flowered*. Here we have words of Saxon and Latin origin mingled pretty equally. But the German description is entirely Teutonic. *Die Blume in Achsel; die Blätter unterbrochen gefiedert*, *die Blättchen scharf gesagt*, *die Stämme kriechend*, *die Bluthenstiele einblumig*. We could imitate this in our own language, by saying *brokenly-feathered*, *sharp-sawed;* by using *threed* for *ternate*, as the Germans employ *gedreit;* by saying

fingered-feathered for *digitato-pinnate*, and the like. But the habit which we have, in common as well as scientific language, of borrowing words from the Latin for new cases, would make such usages seem very harsh and pedantic.

We may add that, in consequence of these different practices in the two languages, it is a common habit of the German reader to impose a scientific definiteness upon a common word, such as our Fifth Aphorism requires; whereas the English reader expects rather that a word which is to have a technical sense shall be derived from the learned languages. *Die Kelch* and *die Blume* (the cup and the flower) easily assume the technical meaning of *calyx* and *corolla; die Griffel* (the pencil) becomes *the pistil;* and a name is easily found for the *pollen*, the *anthers*, and the *stamens*, by calling them the dust, the dust-cases, and the dust-threads (*der Staub*, *die Staub-beutel*, or *Staub-fächer*, and *die Staub-fäden*). This was formerly done in English to a greater extent than is now possible without confusion and pedantry. Thus, in Grew's book on the *Anatomy of Plants*, the calyx is called the *impalement*, and the sepals the *impalers;* the petals are called the *leaves of the flower;* the stamens with their anthers are the *seminiform attire*. But the English language, as to such matters, is now less flexible than it was; partly in consequence of its having adopted the Linnæan terminology almost entire, without any endeavour to naturalize it. Any attempt at idiomatic description would interfere with the scientific language now generally received in this country. In Germany, on the other hand, those who first wrote upon science in their own language imitated the Latin words which they found in foreign writers, instead of transferring new roots into their own language. Thus the *Numerator* and *Denominator* of a fraction they call the *Namer* and the *Counter* (*Nenner* and *Zähler*). This course they pursued even where the expression was erroneous. Thus that portion of the intestines which ancient anatomists called *Duodenum*, because they falsely estimated its length at twelve inches, the Germans also term *Zwölffingerdarm* (twelve-inch-gut), though this intestine in a whale is twenty feet long, and in a frog not above twenty lines. As another example of this process in German, we may take the word *Muttersackbauchblatte*, the *uterine peritonæum.*

It is a remarkable evidence of this formative power of the German language, that it should have been able to produce an imitation of the systematic chemical nomenclature of the French school, so complete, that it is used in Germany as familiarly as the original system is in France and England. Thus Oxygen and Hydrogen are *Sauerstoff* and *Wafferstoff;* Azote is *Stickstoff* (suffocating matter); Sulphuric and Sulphurous Acid are *Schwefel-säure* and *Schwefelichte-säure.* The Sulphate and Sulphite of Baryta, and Sulphuret of Baryum, are *Schwefel-säure Baryterde, Schwefelichte-säure Baryterde*, and *Schwefel-baryum.* Carbonate of Iron is *Kohlen-säures Eisenoxydul;* and we may observe that, in such cases, the German name is much more agreeable to analogy than the English one; for the Protoxide of Iron, (*Eisenoxydul,*) and not the Iron itself, is the base of the salt. And the German language has not only thus imitated the established nomenclature of chemistry, but has shown itself capable of supplying new forms to meet the demands which the progress of theory occasions. Thus the Hydracids are *Wasserstoff-säuren;* and of these, the Hydriodic Acid is *Iodwasserstoff-säure*, and so of the rest. In like manner, the translator of Berzelius has found German names for the sulpho-salts of that chemist; thus he has *Wasserstoffschwefliges Schewefel-lithium*, which would be (if we were to adopt his theoretical view) hydro-sulphuret of sulphuret of lithium: and a like nomenclature for all other similar cases.

3. In English we have no power of imitating this process, and must take our technical phrases from some more flexible language, and generally from the Latin or Greek. We are indeed so much accustomed to do this, that except a word has its origin in one of these languages, it hardly seems to us a technical

term; and thus by employing indigenous terms, even descriptive ones, we may, perhaps, lose in precision more than we gain in the vividness of the impression. Perhaps it may be better to say *cuneate, lunate, hastate, sagittate, reniform,* than *wedge-shaped, crescent-shaped, halbert-headed, arrow-headed, kidney-shaped. Ringent* and *personate* are better than any English words which we could substitute for them; *labiate* is more precise than *lipped* would readily become. *Urceolate, trochlear,* are more compact than *pitcher-shaped, pulley-shaped;* and *infundibuliform, hypocrateriform,* though long words, are not more inconvenient than *funnel-shaped* and *salver-shaped.* In the same way it is better to speak (with Dr. Prichard[1],) of *repent* and *progressive* animals, than of *creeping* and progressive: the two Latin terms make a better pair of correlatives.

4. But wherever we may draw the line between the proper use of English and Latin terms in descriptive phraseology, we shall find it advisable to borrow almost all other technical terms from the learned languages. We have seen this in considering the new terms introduced into various sciences in virtue of our Ninth Maxim. We may add, as further examples, the names of the various animals of which a knowledge has been acquired from the remains of them which exist in various strata, and which have been reconstructed by Cuvier and his successors. Such are the *Palæotherium*, the *Anoplotherium*, the *Megatherium*, the *Dinotherium*, the *Chirotherium*, the *Megalichthys*, the *Mastodon*, the *Ichthyosaurus*, the *Plesiosaurus*, the *Pterodactylus*. To these others are every year added; as, for instance, very recently, the *Toxodon, Zeuglodon,* and *Phascolotherium* of Mr. Owen, and the *Thylacotherium* of M. Valenciennes. Still more recently the terms *Glyptodon, Mylodon, Dicynodon, Paloplotherium, Rhynchosaurus,* have been added by Mr. Owen to designate fossil animals newly determined by him.

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1 *Researches*, p. 69.

The names of species, as well as of genera, are thus formed from the Greek: as the Plesiosaurus *dolichodeirus* (long-necked), Ichthyosaurus *platyodon* (broad-toothed), the Irish elk, termed Cervus *megaceros* (large-horned). But the descriptive specific names are also taken from the Latin, as Plesiosaurus *brevirostris*, *longirostris*, *crassirostris;* besides which there are arbitrary specific names, which we do not here consider.

These names being all constructed at a period when naturalists were familiar with an artificial system, the standard language of which is Latin, have not been taken from modern language. But the names of living animals, and even of their classes, long ago formed in the common language of men, have been in part adopted in the systems of naturalists, agreeably to Aphorism Third. Hence the language of systems in natural history is mixed of ancient and modern languages. Thus Cuvier's divisions of the vertebrated animals are *Mammifères* (Latin), *Oiseaux*, *Reptiles*, *Poissons; Bimanes*, *Quadrumanes*, *Carnassières*, *Rongeurs*, *Pachydermes* (Greek), *Ruminans* (Latin), *Cétacés* (Latin). In the subordinate divisions the distribution being more novel, the names are less idiomatic: thus the kinds of Reptiles are *Cheloniens*, *Sauriens*, *Ophidiens*, *Batraciens*, all which are of Greek origin. In like manner, Fish are divided into *Chondropterygiens*, *Malacopterygiens*, *Acanthopterygiens*. The unvertebrated animals are *Mollusques*, *Animaux articulés*, and *Animaux rayonnés;* and the Mollusques are divided into six classes, chiefly according to the position or form of their foot; namely, *Cephalopodes*, *Pteropodes*, *Gasteropodes*, *Acephales*, *Brachiopodes*, *Cirrhopodes*.

In transferring these terms into English, when the term is new in French as well as English, we have little difficulty; for we may take nearly the same liberties in English which are taken in French; and hence we may say *mammifers* (rather *mammals*), *cetaceans* or *cetaces*, *batracians* (rather *batrachians*), using the words as substantives. But in other cases we must go back to the Latin: thus we say *radiate* animals, or *radiata* (rather *radials*), for *rayonnées*. These changes, however, rather refer to another Aphorism.

(Mr. Kirby has proposed *radiary*, *radiaries*, for *radiata*.)

5. When new Mineral Species have been established in recent times, they have generally had arbitrary names assigned to them, derived from some person or places. In some instances, however, descriptive names have been selected; and then these have been generally taken from the Greek, as *Augite*, *Stilbite*, *Diaspore*, *Dichroite*, *Dioptase*. Several of these Greek names imposed by Haüy, refer to some circumstances, often fancifully selected, in his view of the crystallization of the substance, as *Epidote*, *Peridote*, *Pleonast*. Similar terms of Greek origin have been introduced by others, as *Orthite*, *Anorthite*, *Periklin*. Greek names founded on casual circumstances are less to be commended. Berzelius has termed a mineral *Eschynite*, from αἰσχυνὴ, *shame*, because it is, he conceives, a shame for chemists not to have separated its elements more distinctly than they did at first.

6. In Botany, the old names of genera of Greek origin are very numerous, and many of them are descriptive, as *Glycyrhiza* (γλυκὺς and ῥίζα, sweet root) liquorice, *Rhododendron* (rose-tree), *Hœmatoxylon* (bloody wood), *Chrysocoma* (golden hair), *Alopecurus* (fox-tail), and many more. In like manner there are names which derive a descriptive significance from the Latin, either adjectives, as *Impatiens*, *Gloriosa*, *Sagittaria*, or substantives irregularly formed, as *Tussilago* (à tussis domatione), *Urtica* (ab urendo tactu), *Salsola* (à salsedine). But these, though good names when they are established by tradition, are hardly to be imitated in naming new plants. In most instances, when this is to be done, arbitrary or local names have been selected, as *Strelitzia*.

7. In Chemistry, new substances have of late had names assigned them from Greek roots, as *Iodine*, from its violet colour, *Chlorine* from its green colour. In like manner fluorine has by the French chemists been called *Phthor*, from its destructive properties. So the

new metals, *Chrome, Rhodium, Iridium, Osmium,* had names of Greek derivation descriptive of their properties. Some such terms, however, were borrowed from localities, as *Strontia, Yttria,* the names of new earths. Others have a mixed origin, as *Pyrogallic, Pyroacetic,* and *Pyroligneous* Spirit. In some cases the derivation has been extravagantly capricious. Thus in the process for making Pyrogallic Acid, a certain substance is left behind, from which M. Braconnot extracted an acid which he called *Ellagic* Acid, framing the root of the name by reading the word *Galle* backwards.

The new laws which the study of Electro-chemistry brought into view, required a new terminology to express their conditions: and in this case, as we have observed in speaking of the Twelfth Maxim, arbitrary words are less suitable. Mr. Faraday very properly borrowed from the Greek his terms *Electrolyte, Electrode, Anode, Cathode, Anïon, Cathïon, Dielectric.* In the mechanico-chemical and mechanical sciences, however, new terms are less copiously required than in the sciences of classification, and when they are needed, they are generally determined by analogy from existing terms. *Thermo-electricity* and *Electro-dynamics* were terms which very naturally offered themselves; Nobili's *thermo-multiplier,* Snow Harris's *unit-jar,* were almost equally obvious names. In such cases, it is generally possible to construct terms both compendious and descriptive, without introducing any new radical words.

8. The subject of Crystallography has inevitably given rise to many new terms, since it brings under our notice a great number of new relations of a very definite but very complex form. Haüy attempted to find names for all the leading varieties of crystals, and for this purpose introduced a great number of new terms, founded on various analogies and allusions. Thus the forms of calc-spar are termed by him *primitive, equiaxe, inverse, metastatique, contrastante, imitable, birhomboidale, prismatique, apophane, uniternaire, bisunitaire, dodécaèdre, contractée, dilatée, sexduodecimale, bisalterne, binoternaire,* and many others. The

want of uniformity in the origin and scheme of these denominations would be no valid objection to them, if any general truth could be expressed by means of them: but the fact is, that there is no definite distinction of these forms. They pass into each other by insensible gradations, and the optical and physical properties which they possess are common to all of them. And as a mere enunciation of laws of form, this terminology is insufficient. Thus it does not at all convey the relation between the *bisalterne* and the *binoternaire*, the former being a combination of the *metastatique* with the *prismatique*, the latter, of the *metastatique* with the *contrastante:* again, the *contrastante*, the *mixte*, the *cuboide*, the *contractée*, the *dilatée*, all contain faces generated by a common law, the index being respectively altered so as to be in these cases, $3, \frac{3}{2}, \frac{4}{3}, \frac{9}{4}, \frac{5}{9}$; and this, which is the most important geometrical relation of these forms, is not at all recorded or indicated by the nomenclature. The fact is, that it is probably impossible, the subject of crystallography having become so complex as it now is, to devise a system of names which shall express the relations of form. Numerical symbols, such as those of Weiss or Naumann, or Professor Miller, are the proper ways of expressing these relations, and are the only good crystallographic terminology for cases in detail.

The terms used in expressing crystallographic laws have been for the most part taken from the Greek by all writers except some of the Germans. These, we have already stated, have constructed terms in their own language, as *zwei-und-ein gliedrig*, and the like.

In Optics we have some new terms connected with crystalline laws, as *uniaxal* and *biaxal* crystals, *optical axes*, which offered themselves without any effort on the part of the discoverers. In the whole history of the undulatory theory, very few innovations in language were found necessary, except to fix the sense of a few phrases, as *plane-polarized* light in opposition to *circularly-polarized*, and the like.

This is still more the case in Mechanics, Astronomy, and pure mathematics. In these sciences, several of the primary stages of generalization being already passed over, when any new steps are made, we have before us some analogy by which we may frame our new terms. Thus when the *plane of maximum areas* was discovered, it had not some new arbitrary denomination assigned it, but the name which obviously described it was fixed as a technical name.

The result of this survey of the scientific terms of recent formation seems to be this;—that indigenous terms may be employed in the descriptions of facts and phenomena as they at first present themselves; and in the first induction from these; but that when we come to generalize and theorize, terms borrowed from the learned languages are more readily fixed and made definite, and are also more easily connected with derivatives. Our native terms are more impressive, and at first more intelligible; but they may wander from their scientific meaning, and are capable of little inflexion. Words of classical origin are precise to the careful student, and capable of expressing, by their inflexions, the relations of general ideas; but they are unintelligible, even to the learned man, without express definition, and convey instruction only through an artificial and rare habit of thought.

Since in the balance between words of domestic and of foreign origin so much depends upon the possibility of inflexion and derivation, I shall consider a little more closely what are the limits and considerations which we have to take into account in reference to that subject.

Aphorism XXI.

In the composition and inflexion of technical terms, philological analogies are to be preserved if possible, but modified according to scientific convenience.

In the language employed or proposed by writers upon subjects of science, many combinations and forms of derivation occur, which would be rejected and condemned by those who are careful of the purity and correctness of language. Such anomalies are to be avoided as much as possible; but it is impossible to escape them altogether, if we are to have a scientific language which has any chance of being received into general use. It is better to admit compounds which are not philologically correct, than to invent many new words, all strange to the readers for whom they are intended: and in writing on science in our own language, it is not possible to avoid making additions to the vocabulary of common life; since science requires exact names for many things which common language has not named. And although these new names should, as much as possible, be constructed in conformity with the analogies of the language, such extensions of analogy can hardly sound, to the grammarian's ear, otherwise than as solecisms. But, as our maxim indicates, the analogy of science is of more weight with us than the analogy of language: and although anomalies in our phraseology should be avoided as much as possible, innovations must be permitted wherever a scientific language, easy to acquire, and convenient to use, is unattainable without them.

I shall proceed to mention some of the transgressions of strict philological rules, and some of the extensions of grammatical forms, which the above conditions appear to render necessary.

1. The combination of different languages in the derivation of words, though to be avoided in general, is in some cases admissible.

Such words are condemned by Quintilian and other grammarians, under the name of *hybrids*, or things of a mixed race; as *biclinium*, from *bis* and κλίνη; *epitogium*, from ἐπὶ and *toga*. Nor are such terms to be unnecessarily introduced in science. Whenever a homogeneous word can be formed and adopted with the same ease and convenience as a hybrid, it is to be preferred. Hence we must have *ichthyology*, not *piscology*, *entomology*, not *insectology*, *insectivorous*, not *insectophagous*. In like manner, it would be better to say *unoculus* than *monoculus*, though the latter has the sanction of Linnæus, who was a purist in such matters.

Dr Turner, in his *Chemistry*, speaks of *protoxides* and *binoxides*, which combination violates the rule for making the materials of our terms as homogeneous as possible; *protoxide* and *deutoxide* would be preferable, both on this and on other accounts.

Yet this rule admits of exceptions. *Mineralogy*, with its Greek termination, has for its root *minera*, a medieval Latin word of Teutonic origin, and is preferable to *Oryctology*. *Terminology* appears to be better than *Glossology*: which according to its derivation would be rather the science of language in general than of technical terms; and *Horology*, from ὅρος, a term, would not be immediately intelligible, even to Greek scholars; and is already employed to indicate the science which treats of hologes, or time-pieces.

Indeed, the English reader is become quite familiar with the termination *ology*, the names of a large number of branches of science and learning having that form. This termination is at present rather apprehended as a formative affix in our own language, indicating a science, than as an element borrowed from foreign language. Hence, when it is difficult or impossible to find a Greek term which clearly designates the subject of a science, it is allowable to employ some other, as in *Tidology*, the doctrine of the Tides.

The same remark applies to some other Greek elements of scientific words: they are so familiar to us that in composition they are almost used as part of our own language. This naturalization has taken place very decidedly in the element *arch*, (ἀρχὸς, a leader,) as we see in *archbishop*, *archduke*. It is effected in a great degree for the preposition *anti*: thus we speak of *anti-slavery* societies, *anti-reformers*, *anti-bilious*, or *anti-acid* medicines, without being conscious of any anomaly. The same is the case with the Latin preposition *præ* or *pre*, as appears from such words as *pre-engage*, *pre-arrange*, *pre-judge*, *pre-paid;* and in some measure with *pro*, for in colloquial language we speak of *pro-catholics* and *anti-catholics*. Also the preposition *ante* is similarly used, as *ante-nicene* fathers. The preposition *co*, abbreviated from *con*, and implying things to be simultaneous or connected, is firmly established as part of the language, as we see in *coexist, coheir, coordinate;* hence I have called those lines *co-tidal* lines which pass through places where the high water of the tide occurs simultaneously.

2. As in the course of the mixture by which our language has been formed, we have thus lost all habitual consciousness of the difference of its ingredients, (Greek, Latin, Norman-French, and Anglo-Saxon): we have also ceased to confine to each ingredient the mode of grammatical inflexion which originally belonged to it. Thus the termination *ive* belongs peculiarly to Latin adjectives, yet we say *sportive, talkative.* In like manner, *able* is added to words which are not Latin, as *eatable, drinkable, pitiable, enviable.* Also the termination *al* and *ical* are used with various roots, as *loyal, royal, farcical, whimsical;* hence we may make the adjective *tidal* from tide. This ending, *al*, is also added to abstract terms in *ion*, as *occasional, provisional, intentional, national;* hence we may, if necessary, use such words as *educational, terminational.* The ending *ic* appears to be suited to proper names, as *Pindaric, Socratic, Platonic;* hence it may be used when scientific words are derived from proper names, as *Voltaic* or *Galvanic* electricity: to which I have proposed to add *Franklinic.*

In adopting scientific adjectives from the Latin, we have not much room for hesitation; for, in such cases, the habits of derivation from that language into our own are very constant; *ivus* becomes *ive*, as *decursive; inus* becomes *ine*, as in *ferine; atus* becomes *ate*, as *hastate;* and *us* often becomes *ous*, as *rufous; aris* becomes *ary*, as *axillary; ens* becomes *ent*, as *ringent.* And in adopting into our language, as scientific terms, words which in another language, the French for instance, have a Latin origin familiar to us, we cannot do better than form them as if they were derived directly from the Latin. Hence the French adjectives *cétacé, crustacé, testacé*, may become either *cetaceous, crustaceous, testaceous*, according to the analogy of *farinaceous, predaceous*, or else *cetacean, crustacean,* *testacean*, imitating the form of *patrician*. Since, as I shall soon have to notice, we require substantives as well as adjectives from these words, we must, at least for that use, take the forms last suggested.

In pursuance of the same remark, *rongeur* becomes *rodent;* and *edenté* would become *edentate*, but that this word is rejected on another account: the adjectives *bimane* and *quadrumane* are *bimanous* and *quadrumanous.*

3. There is not much difficulty in thus forming adjectives: but the purposes of Natural History require that we should have substantives corresponding to these adjectives; and these cannot be obtained without some extension of the analogies of our language. We cannot in general use adjectives or participles as singular substantives. *The happy* or *the doomed* would, according to good English usage, signify those who are happy and those who are doomed in the plural. Hence we could not speak of a particular scaled animal as *the squamate*, and still less could we call any such animal *a squamate*, or speak of *squamates* in the plural. Some of the forms of our adjectives, however, do admit of this substantive use. Thus we talk of *Europeans, plebeians, republicans;* of *divines* and *masculines;* of the *ultramontanes;* of *mordants* and *brilliants;* of *abstergents* and *emollients;* of *mercenaries* and *tributaries;* of *animals, manuals*, and *officials;* of *dissuasives* and *motives.* We cannot generally use in this way adjectives in *ous*, nor in *ate* (though *reprobates* is an exception), nor English participles, nor adjectives in which there is no termination imitating the Latin, as *happy, good.* Hence, if we have, for purposes of science, to convert adjectives into substantives, we ought to follow the form of examples like these, in which it has already appeared in fact, that such usage, though an innovation at first, may ultimately become a received part of the language.

By attention to this rule we may judge what expressions to select in cases where substantives are needed. I will take as an example the division of the mammalian animals into Orders. These Orders, according to Cuvier, are *Bimanes, Quadrumanes, Carnassiers, Rongeurs, Edentés, Ruminans, Pachydermes, Cétacés;* and of these, *Bimanes, Quadrumanes, Rodents, Ruminants, Pachyderms* are admissible as English substantives on the grounds just stated. *Cetaceous* could not be used substantively; but *Cetacean* in such a usage is sufficiently countenanced by such cases as we have mentioned, *patrician*, &c.; hence we adopt this form. We have no English word equivalent to the French *Carnassiers:* the English translator of Cuvier has not provided English words for his technical terms; but has formed a Latin word, *Carnaria*, to represent the French terms. From this we might readily form *Carnaries;* but it appears much better to take the Linnæan name *Feræ* as our root, from which we may take *Ferine*, substantive as well as adjective; and hence we call this order *Ferines*. The word for which it is most difficult to provide a proper representation is *Edenté, Edentata:* for, as we have said, it would be very harsh to speak of the order as the *Edentates;* and if we were to abbreviate the word into *edent*, we should suggest a false analogy with *rodent*, for as *rodent* is *quod rodit*, that which gnaws, *edent* would be *quod edit*, that which eats. And even if we were to take *edent* as a substantive, we could hardly use it as an adjective: we should still have to say, for example, the *edentate* form of head. For these reasons it appears best to alter the form of the word, and to call the Order the *Edentals*, which is quite allowable, both as adjective and substantive.

[An objection might be made to this term, both in its Latin, French and English form: namely, that the natural group to which it is applied includes many species, both existing and extinct, well provided with teeth. Thus the armadillo is remarkable for the number of its teeth; the megatherium, for their complex structure. But the analogy of scientific language readily permits us to fix, upon the word *edentata*, a special meaning, implying the absence of one particular kind of teeth, namely, incisive teeth. Linnæus called the equivalent order *Bruta*. We could not apply in this case the term *Brutes;* for common language has already attached to the word a wider meaning, too fixedly for scientific use to trifle with it.]

There are several other words in *ate* about which there is the same difficulty in providing substantive forms. Are we to speak of *Vertebrates?* or would it not be better, in agreement with what has been said above, to call these *Vertebrals*, and the opposite class *Invertebrals?*

There are similar difficulties with regard to the names of subordinate portions of zoological classification; thus the Ferines are divided by Cuvier into *Cheiroptéres, Insectivores, Carnivores;* and these latter into *Plantigrades, Digitigrades, Amphibies, Marsupiaux.* There is not any great harshness in naturalizing these substantives as *Chiropters, Insectivores, Carnivores, Plantigrades, Digitigrades, Amphibians,* and *Marsupials.* These words *Carnivores* and *Insectivores* are better, because of more familiar origin, than Greek terms; otherwise we might, if necessary, speak of *Zoophagans* and *Entomophagans.*

It is only with certain familiar adjectival terminations, as *ous* and *ate*, that there is a difficulty in using the word as substantive. When this can be avoided, we readily accept the new word, as *Pachyderms,* and in like manner *Mollusks.*

If we examine the names of the Orders of Birds, we find that they are in Latin, *Predatores* or *Accipitres, Passeres, Scansores, Rasores* or *Gallinæ, Grallatores, Palmipedes* and *Anseres:* Cuvier's Orders are, *Oiseaux de Proie, Passereaux, Grimpeurs, Gallinacés, Echassiérs, Palmipedes.* These may be englished conveniently as *Predators, Passerines, Scansors, Gallinaceans,* (rather than *Rasors,*) *Grallators, Palmipedans,* [or rather *Palmipeds,* like *Bipeds*]. *Scansors, Grallators,* and *Rasors,* are better, as technical terms, than *Climbers, Waders,* and *Scratchers.* We might venture to anglicize the terminations of the names which Cuvier gives to the divisions of these Orders: thus the Predators are the *Diurnals* and the *Nocturnals;* the Passerines are the *Dentirostres,* the *Fissirostres,* the

Conirostres, the *Tenuirostres*, and the *Syndactyls:* the word *lustre* showing that the former termination is allowable. The Scansors are not sub-divided, nor are the Gallinaceans. The Grallators are *Pressirostres*, *Cultrirostres*, *Macrodactyls*. The Palmipeds are the *Plungers*, the *Longipens*, the *Totipalmes* and the *Lamellirostres*.

The next class of Vertebrals is the *Reptiles*, and these are either *Chelonians*, *Saurians*, *Ophidians*, or *Batrachians*. Cuvier writes *Batraciens*, but we prefer the spelling to which the Greek word directs us.

The last or lowest class is the *Fishes*, in which province Cuvier has himself been the great systematist, and has therefore had to devise many new terms. Many of these are of Greek or Latin origin, and can be anglicized by the analogies already pointed out, as *Chondropterygians*, *Malacopterygians*, *Lophobranchs*, *Plectognaths*, *Gymnodonts*, *Sclerodermes*. *Discoboles* and *Apodes* may be English as well as French. There are other cases in which the author has formed the names of Families, either by forming a word in *ides* from the name of a genus, as *Gadoides*, *Gobiöides*, or by gallicizing the Latin name of the genus, as *Salmones* from *Salmo*, *Clupes* from *Clupea*, *Esoces* from *Esox*, *Cyprins* from *Cyprinus*. In these cases Agassiz's favourite form of names for families of fishes has led English writers to use the words *Gadoids*, *Gobioids*, *Salmonoids*, *Clupeoids*, *Lucioids* (for *Esocés*), *Cyprinoids*, &c. There is a taint of hybridism in this termination, but it is attended with this advantage, that it has begun to be characteristic of the nomenclature of family groups in the class *Pisces*. One of the orders of fishes, co-ordinate with the Chondropterygians and the Lophobranchs, is termed *Osseux* by Cuvier. It appears hardly worth while to invent a substantive word for this, when *Bony Fishes* is so simple a phrase, and may readily be understood as a technical name of a systematic order.

The Mollusks are the next Class; and these are divided into *Cephalopods*, *Gasteropods*, and the like. The Gasteropods are *Nudibranchs*, *Inferobranchs*, *Tecti-*

branchs, Pectinibranchs, Scutibranchs, and *Cyclobranchs*. In framing most of these terms Cuvier has made hybrids by a combination of a Latin word with *branchiæ*, which is the Greek name for the gills of a fish; and has thus avoided loading the memory with words of an origin not obvious to most naturalists, as terms derived from the Greek would have been. Another division of the Gasteropods is *Pulmonés*, which we must make *Pulmonians*. In like manner the subdivisions of the Pectinibranchs are the *Trochoidans* and *Buccinoidans*, (*Trochoïdes*, *Buccinoïdes*). The *Acéphales*, another order of Mollusks, may be *Acephals* in English.

After these comes the third grand division, *Articulated Animals*, and these are *Annelidans*, *Crustaceans*, *Arachnidans*, and *Insects*. I shall not dwell upon the names of these, as the form of English words which is to be selected must be sufficiently obvious from the preceding examples.

Finally, we have the fourth grand division of animals, the *Rayonnés*, or *Radiata;* which, for reasons already given, we may call *Radials*, or *Radiaries*. These are *Echinoderms*, *Intestinals*, (or rather *Entozoans*,) *Acalephes*, and *Polyps*. The Polyps, which are composite animals in which many gelatinous individuals are connected so as to have a common life, have, in many cases, a more solid framework belonging to the common part of the animal. This framework, of which coral is a special example, is termed in French *Polypier;* the word has been anglicized by the word *polypary*, after the analogy of *aviary* and *apiary*. Thus Polyps are either *Polyps with Polyparies* or *Naked Polyps*.

Any common kind of Polyps has usually in the English language been called *Polypus*, the Greek termination being retained. This termination in *us*, however, whether Latin or Greek, is to be excluded from the English as much as possible, on account of the embarrassment which it occasions in the formation of the plural. For if we say *Polypi* the word ceases to be English, while *Polypuses* is harsh: and there is the additional inconvenience, that both these forms would indicate the plural of individuals rather than of classes.

If we were to say, 'The Corallines are a Family of the *Polypuses with Polyparies*,' it would not at once occur to the reader that the last three words formed a technical phrase.

This termination *us*, which must thus be excluded from the names of families, may be admitted in the designation of genera; of animals, as *Nautilus*, *Echinus*, *Hippopotamus;* and of plants, as *Crocus*, *Asparagus*, *Narcissus*, *Acanthus*, *Ranunculus*, *Fungus*. The same form occurs in other technical words, as *Fucus*, *Mucus*, *Œsophagus*, *Hydrocephalus*, *Callus*, *Calculus*, *Uterus*, *Fœtus*, *Radius*, *Focus*, *Apparatus*. It is, however, advisable to retain this form only in cases where it is already firmly established in the language; for a more genuine English form is preferable. Hence we say, with Mr. Lyell, *Ichthyosaur*, *Plesiosaur*, *Pterodactyl*. In like manner Mr. Owen anglicizes the termination *erium*, and speaks of the *Anoplothere* and *Paleothere*.

Since the wants of science thus demand adjectives which can be used also as substantive names of classes, this consideration may sometimes serve to determine our selection of new terms. Thus Mr. Lyell's names for the subdivisions of the tertiary strata, *Miocene*, *Pliocene*, can be used as substantives; but if such words as *Mioneous*, *Plioneous*, had suggested themselves, they must have been rejected, though of equivalent signification, as not fulfilling this condition.

4. (*a.*) Abstract substantives can easily be formed from adjectives: from electric we have *electricity;* from galvanic, *galvanism;* from organic, *organization; velocity*, *levity*, *gravity*, are borrowed from Latin adjectives. *Caloric* is familiarly used for the matter of heat, though the form of the word is not supported by any obvious analogy.

(*b.*) It is intolerable to have words regularly formed, in opposition to the analogy which their meaning offers; as when bodies are said to have conduct*ibility* or conduc*ibility* with regard to heat. The bodies are conduct*ive*, and their property is conduct*ivity*.

(*c.*) The terminations *ize* (rather than *ise*), *ism*, and *ist*, are applied to words of all origins: thus we have to *pulverize*, to *colonize*, *Witticism*, *Heathenism*, *Journalist*, *Tobacconist*. Hence we may make such words when they are wanted. As we cannot use *physician* for a cultivator of physics, I have called him a *Physicist*. We need very much a name to describe a cultivator of science in general. I should incline to call him a *Scientist*. Thus we might say, that as an Artist is a Musician, Painter, or Poet, a Scientist is a Mathematician, Physicist, or Naturalist.

(*d*.) Connected with verbs in *ize*, we have abstract nouns in *ization*, as *polarization*, *crystallization*. These it appears proper to spell in English with *z* rather than *s*; governing our practice by the Greek verbal termination ίζω which we imitate. But we must observe that verbs and substantives in *yse*, (*analyse*), belong to a different analogy, giving an abstract noun in *ysis* and an adjective *ytic* or *ytical*; (*analysis*, *analytic*, *analytical*). Hence *electrolyse* is more proper than *electrolyze*.

(*e*.) The names of many sciences end in *ics* after the analogy of *Mathematics*, *Metaphysics*; as *Optics*, *Mechanics*. But these, in most other languages, as in our own formerly, have the singular form *Optice*, *l'Optique*, *Optik*, *Optick*: and though we now write *Optics*, we make such words of the singular number: 'Newton's Opticks is an example.' As, however, this connexion in new words is startling, as when we say 'Thermo-electrics is now much cultivated,' it appears better to employ the singular form, after the analogy of *Logic* and *Rhetoric*, when we have words to construct. Hence we may call the science of languages *Linguistic*, as it is called by the best German writers, for instance, William Von Humboldt.

5. In the derivation of English from Latin or Greek words, the changes of letters are to be governed by the rules which have generally prevailed in such cases. The Greek οι and αι, the Latin *oe* and *ae*, are all converted into a simple *e*, as in *Æ*conomy, Geodesy, penal, Cesar. Hence, according to common usage, we should write phenomena, not phænomena, paleontology, not palæontology, miocene not miocæne, pekilite not pœ-

kilite. But in order to keep more clearly in view the origin of our terms, it may be allowable to deviate from these rules of change, especially so long as the words are new and unfamiliar. Dr. Buckland speaks of the *poikilitic*, not *pecilitic*, group of strata: *palæontology* is the spelling commonly adopted; and in imitation of this I have written *palætiology*. The diphthong ει was by the Latins changed into *i*, as in Arist*i*des; and hence this has been the usual form in English. Some recent authors indeed (Mr. Mitford for instance) write Arist*ei*des; but the former appears to be the more legitimate. Hence we write m*i*ocene, pl*i*ocene, not m*ei*ocene, pl*ei*ocene. The Greek υ becomes *y*, and ου becomes *u*, in English as in Latin, as cr*y*stal, col*u*re. The consonants κ and χ become *c* and *ch* according to common usage. Hence we write *crystal*, not *chrystal*, batra*ch*ian, not batra*c*ian, *c*ryolite, not *ch*ryolite. As, however, the letter *c* before *e* and *i* differs from *k*, which is the sound we assign to the Greek κ, it may be allowable to use *k* in order to avoid this confusion. Thus, as we have seen, poi*k*ilite has been used, as well as pecilite. Even in common language some authors write s*k*eptic, which appears to be better than sceptic with our pronunciation, and is preferred by Dr. Johnson. For the same reason, namely, to avoid confusion in the pronunciation, and also, in order to keep in view the connexion with *cathode*, the elements of an electrolyte which go to the anode and cathode respectively may be termed the anion and cat*h*ion; although the Greek would suggest catĭon, (κατίον).

6. The example of chemistry has shown that we have in the terminations of words a resource of which great use may be made in indicating the relations of certain classes of objects: as sulphur*ous* and sulphur*ic* acids; sulph*ates*, sulph*ites*, and sulph*urets*. Since the introduction of the artifice by the Lavoisierian school, it has been extended to some new cases. The Chlor*ine*, Fluor*ine*, Brom*ine*, Iod*ine*, had their names put into that shape in consequence of their supposed analogy: and for the same reason have been termed Chlore,

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Phlore, Brome, Iode, by French chemists. In like manner, the names of metals in their Latin form have been made to end in *um*, as Osmium, Palladium; and hence it is better to say Platin*um*, Molybden*um*, than Platin*a*, Molybden*a*. It has been proposed to term the basis of Boracic acid Bor*on*; and those who conceive that the basis of Silica has an analogy with Boron have proposed to term it Silic*on*, while those who look upon it as a metal would name it Silici*um*. Seleni*um* was so named when it was supposed to be a metal: as its analogies are now acknowledged to be of another kind, it would be desirable, if the change were not too startling, to term it Sel*en*, as it is in German. Phosph*orus* in like manner might be Phosph*ur*, which would indicate its analogy with Sulph*ur*.

The resource which terminations offer has been applied in other cases. The names of many species of minerals end in *lite*, or *ite*, as Stauro*lite*, Aug*ite*. Hence Adolphe Brongniart, in order to form a name for a genus of fossil plants, has given this termination to the name of the recent genus which they nearly resemble, as Zam*ites*, from Zamia, Lycopod*ites* from Lycopodium.

Names of different genera which differ in termination only are properly condemned by Linnæus[1]; as *Alsine*, *Alsinoides*, *Alsinella*, *Alsinastrum*; for there is no definite relation marked by those terminations. Linnæus gives to such genera distinct names, *Alsine*, *Bufonia*, *Sagina*, *Elatine*.

Terminations are well adapted to express definite systematic relations, such as those of chemistry, but they must be employed with a due regard to all the bearings of the system. Davy proposed to denote the combinations of other substances with chlorine by peculiar terminations; using *ane* for the smallest proportion of Chlorine, and *anea* for the larger, as Cup*rane*, Cupr*anea*. In this nomenclature, common salt would be *Sodane*, and Chloride of Nitrogen would be *Azotane*. This suggestion never found favour. It was

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[1] *Phil. Bot.* 231.

objected that it was contrary to the Linnæan precept, that a specific name must not be united to a generic termination. But this was not putting the matter exactly on its right ground; for the rules of nomenclature of natural history do not apply to chemistry; and the Linnæan rule might with equal propriety have been adduced as a condemnation of such terms as Sulphur*ous*, Sulphur*ic*. But Davy's terms were bad; for it does not appear that Chlorine enters, as Oxygen does, into so large a portion of chemical compounds, that its relations afford a key to their nature, and may properly be made an element in their names.

This resource, of terminations, has been abused, wherever it has been used wantonly, or without a definite significance in the variety. This is the case in M. Beudant's Mineralogy. Among the names which he has given to new species, we find the following (besides many in *ite*), Scolexer*ose*, Opsim*ose*, Exanthel*ose*, &c.; Diacr*ase*, Panab*ase*, Neopl*ase*; Neocl*ese*; Rhodo*ise*, Stibicon*ise*, &c.; Marcel*ine*, Wilhelm*ine*, &c.; Exit*ele*, and many others. In addition to other objections which might be made to these names, their variety is a material defect: for to make this variety depend on caprice alone, as in those cases it does, is to throw away a resource of which chemical nomenclature may teach us the value.

Aphorism XXII.

When alterations in technical terms become necessary, it is desirable that the new term should contain in its form some memorial of the old one.

We have excellent examples of the advantageous use of this maxim in Linnæus's reform of botanical nomenclature. His innovations were very extensive, but they were still moderated as much as possible, and connected in many ways with the names of plants then in use. He has himself given several rules of nomenclature, which tend to establish this connexion of the old and new in a reform. Thus he says, 'Generic names which are current, and are not accompanied with harm to botany, should be tolerated[1].' 'A passable generic name is not to be changed for another, though more apt[2].' 'New generic names are not to be framed so long as passable synonyms are at hand[3].' 'A generic name of one genus, except it be superfluous, is not to be transferred to another genus, though it suit the other better[4].' 'If a received genus requires to be divided into several, the name which before included the whole, shall be applied to the most common and familiar kind[5].' And though he rejects all *generic* names which have not a Greek or Latin root[6], he is willing to make an exception in favour of those which from their form might be supposed to have such a root, though they are really borrowed from other languages, as *Thea*, which is the Greek for goddess; *Coffea*, which might seem to come from a Greek word denoting silence (κωφός); *Cheiranthus*, which appears to mean hand-flower, but is really derived from the Arabic *Keiri*: and many others.

As we have already said, the attempt at a reformation of the nomenclature of Mineralogy made by Professor Mohs will probably not produce any permanent effect, on this account amongst others, that it has not been conducted in this temperate mode; the innovations bear too large a proportion to the whole of the names, and contain too little to remind us of the known appellations. Yet in some respects Professor Mohs has acted upon this maxim. Thus he has called one of his classes *Spar*, because *Felspar* belongs to it. I shall venture to offer a few suggestions on this subject of Mineralogical Nomenclature.

It has already been remarked that the confusion and complexity which prevail in this subject render a reform very desirable. But it will be seen, from the reasons assigned under the Ninth Aphorism, that no permanent system of names can be looked for, till a

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1 *Philosophia Botanica*, Art. 242. 2 Art. 246.
3 Art. 247. 4 Art. 249. 5 Art. 249. 6 Art. 232.

sound system of classification be established. The best mineralogical systems recently published, however, appear to converge to a common point; and certain classes have been formed which have both a natural-historical and a chemical significance. These Classes, according to Naumann, whose arrangement appears the best, are Hydrolytes, Haloids, Silicides, Oxides of Metals, Metals, Sulphurides (Pyrites, Glances, and Blendes), and Anthracides. Now we find;—that the Hydrolytes are all compounds, such as are commonly termed *Salts;*—that the Haloids are, many of them, already called *Spars*, as *Calc Spar*, *Heavy Spar*, *Iron Spar*, *Zinc Spar;*—that the *Silicides*, the most numerous and difficult class, are denoted for the most part, by single words, many of which end in *ite;*—that the other classes, or subclasses, *Oxides*, *Pyrites*, *Glances*, and *Blendes*, have commonly been so termed; as *Red Iron Oxide*, *Iron Pyrites*, *Zinc Blende;*—while pure metals have usually had the adjective *native* prefixed, as *Native Gold*, *Native Copper*. These obvious features of the current names appear to afford us a basis for a systematic nomenclature. The Salts and Spars might all have the word *salt* or *spar* included in their name, as *Natron Salt*, *Glauber Salt*, *Rock Salt; Calc Spar*, *Bitter Spar*, (Carbonate of Lime and Magnesia), *Fluor Spar*, *Phosphor Spar* (Phosphate of Lime), *Heavy Spar*, *Celestine Spar* (Sulphate of Strontian), *Chromic Lead Spar* (Chromate of Lead); the *Silicides* might all have the name constructed so as to be a single word ending in *ite*, as *Chabasite* (Chabasie), *Natrolite* (Mesotype), *Sommite* (Nepheline), *Pistacite* (Epidote); from this rule might be excepted the *Gems*, as *Topaz*, *Emerald*, *Corundum*, which might retain their old names. The Oxides, Pyrites, Glances, and Blendes, might be so termed; thus we should have *Tungstic Iron Oxide* (usually called Tungstate of Iron), *Arsenical Iron Pyrites* (Mispickel), *Tetrahedral Copper Glance* (Fahlerz), *Quicksilver Blende* (Cinnabar), and the metals might be termed *native*, as *Native Copper*, *Native Silver*.

Such a nomenclature would take in a very large proportion of commonly received appellations, especially if we were to select among the synonyms, as is proposed above in the case of *Glauber Salt*, *Bitter Spar*, *Sommite*, *Pistacite*, *Natrolite*. Hence it might be adopted without serious inconvenience. It would make the name convey information respecting the place of the mineral in the system; and by imposing this condition, would limit the extreme caprice, both as to origin and form, which has hitherto been indulged in imposing mineralogical names.

The principle of a mineralogical nomenclature determined by the place of the species in the system, has been recognized by Mr. Beudant as well as Mr. Mohs. The former writer has proposed that we should say *Carbonate Calcaire*, *Carbonate Witherite*, *Sulphate Couperose*, *Silicate Stilbite*, *Silicate Chabasie*, and so on. But these are names in which the part added for the sake of the system, is not incorporated with the common name, and would hardly make its way into common use.

We have already noticed Mr. Mohs's designations for two of the Systems of Crystallization, the *Pyramidal* and the *Prismatic*, as not characteristic. If it were thought advisable to reform such a defect, this might be done by calling them the *Square Pyramidal* and the *Oblong Prismatic*, which terms, while they expressed the real distinction of the systems, would be intelligible at once to those acquainted with the Mohsian terminology.

I will mention another suggestion respecting the introduction of an improvement in scientific language. The term *Depolarization* was introduced, because it was believed that the effect of certain crystals, when polarized light was incident upon them in certain positions, was to destroy the peculiarity which polarization had produced. But it is now well known, that the effect of the second crystal in general is to divide the polarized ray of light into two rays, polarized in different planes. Still this effect is often spoken of as *Depolarization*, no better term having been yet devised. I have proposed and used the term *Dipolarization*, which well expresses what takes place, and so nearly resembles the elder word, that it must sound familiar to those already acquainted with writings on this subject.

I may mention one term in another department of literature which it appears desirable to reform in the same manner. The theory of the Fine Arts, or the philosophy which speculates concerning what is beautiful in painting, sculpture or architecture, and other arts, often requires to be spoken of in a single word. Baumgarten and other German writers have termed this province of speculation *Æsthetics; αἰσθάνεσθαι, to perceive*, being a word which appeared to them fit to designate the perception of beauty in particular. Since, however, *æsthetics* would naturally denote the Doctrine of Perception in general; since this Doctrine requires a name; since the term *æsthetics* has actually been applied to it by other German writers (as Kant); and since the essential point in the philosophy now spoken of is that it attends to Beauty;—it appears desirable to change this name. In pursuance of the maxim now before us, I should propose the term *Callæsthetics*, or rather (in agreement with what was said in page 338) *Callæsthetic*, the science of the perception of beauty.

FURTHER ILLUSTRATIONS OF THE APHORISMS ON SCIENTIFIC LANGUAGE, FROM THE RECENT COURSE OF SCIENCES.

1. Botany.

The nomenclature of Botany as rescued from confusion by Linnæus, has in modern times been in some danger of relapsing into disorder or becoming intolerably extensive, in consequence of the multiplication of genera by the separation of one old genus into several new ones, and the like subdivisions of the higher groups, as subclasses and classes. This inconvenience, and the origin of it, have been so well pointed out by Mr. G. Bentham[1], that I shall venture to adopt his judgment as an Aphorism, and give his reasons for it.

Aphorism XXIII.

It is of the greatest importance that the Groups which give their substantive names to every included species should remain large.

It will be recollected that according to the Linnæan nomenclature, the genus is marked by a substantive, (as *Rosa*), and the species designated by an adjective added to this substantive, (as *Rosa Alpina*); while the natural orders are described by adjectives taken substantively, (as *Rosaceæ*). But this rule, though it has been universally assented to in theory, has often been deviated from in practice. The number of known species having much increased, and the language of Linnæus and the principles of Jussieu having much augmented the facilities for the study of affinities, botanists have become aware that the species of a genus and the genera of an order can be collected into intermediate groups

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1 *Linnæan Society's Proceedings*, vol. ii. p. 30 (June, 1857).

as natural and as well defined as the genera and orders themselves, and names are required for these subordinate groups as much as for the genera and orders.

Now two courses have been followed in providing names for these subordinate groups.

1. The original genera (considering the case of genera in the first place) have been preserved, (if well founded); and the lower groups have been called *subgenera, sections, subsections, divisions*, &c.: and the original names of the genera have been maintained for the purpose of nomenclature, in order to retain a convenient and stable language. But when these subordinate groups are so well defined and so natural, that except for the convenience of language, they might be made good genera, there are given also to these subordinate groups, substantive or substantively-taken adjective names. When these subordinate groups are less defined or less natural, either no names at all are given, and they are distinguished by figures or signs such as *, **, or § 1, § 2, &c. or there are given them mere adjective names.

Or, 2, To regard these intermediate groups between species and the original genera, as so many independent genera; and to give them substantive names, to be used in ordinary botanical nomenclature.

Now the second course is that which has produced the intolerable multiplication of genera in modern times; and the first course is the only one which can save botanical nomenclature from replunging into the chaos in which Linnæus found it. It was strongly advocated by the elder De Candolle; although in the latter years of his life, seeing how general was the disposition to convert his subgenera and sections into genera, he himself more or less gave in to the general practice. The same principle was adopted by Endlichen, but he again was disposed to go far in giving substantive names to purely technical or ill-defined subsections of genera.

The multiplication of genera has been much too common. Botanists have a natural pride in establishing new genera (or orders); and besides this, it is felt how useful it is, in the study of affinities, to define and name all natural groups in every grade, however numerous they may be: and in the immense variety of language it is found easy to coin names indefinitely.

But the arguments on the other side much preponderate. In attempting to introduce all these new names into ordinary botanical language, the memory is taxed beyond the capabilities of any mind, and the original and legitimate object of the Linnæan nomenclature is wholly lost sight of. In a purely scientific view it matters little if the Orders are converted into Classes or Alliances, the Genera into Orders, and the Sections or Subsections into Genera: their relative importance does not depend on the names given to them, but on their height in the scale of comprehensiveness. But for language, the great implement without which science cannot work, it is of the greatest importance, as our Aphorism declares, That the groups which give their substantive names to every species which they include, should remain large. If, independently of the inevitable increase of Genera by new discoveries, such old ones as *Ficus, Begonia, Arum, Erica,* &c. are divided into 10, 20, 30, or 40 independent Genera, with names and characters which are to be recollected before any one species can be spoken of;—if Genera are to be reckoned by tens of thousands instead of by thousands; —the range of any individual botanist will be limited to a small portion of the whole field of the sciences.

And in like manner with regard to Orders, so long as the number of Orders can be kept within, or not much beyond a couple of hundred, it may reasonably be expected that a botanist of ordinary capacity shall obtain a sufficient general idea of their nature and characters to call them at any time individually to his mind for the purpose of comparison: but if we double the number of Orders, all is confusion.

The inevitable confusion and the necessity of maintaining in some way the larger groups, have been perceived by those even who have gone the furthest in lowering the scale of Orders and Genera. As a remedy for this confusion, they propose to erect the old genera into independent orders, and the old orders into classes or divisions. But this is but an incomplete resumption of the old principles, without the advantage of the old nomenclature.

And it will not be asserted, with regard to these new genera, formed by cutting up the old ones, that the new group is better defined than the group above it: on the contrary, it is frequently less so. It is not pretended that *Urostigma* or *Phannacosyce*, new genera formed out of the old genus *Ficus*, are better defined than the genus *Ficus:* or that the new genera which have lately been cut out of the old genus *Begonia*, form more natural groups than *Begonia* itself does. The principle which seems to be adopted in such subdivisions of old genera is this: that the lowest definable group above a species is a genus. If we were to go a step further, every species becomes a genus with a substantive name.

It ought always to be recollected that though the analytical process carried to the uttermost, and separating groups by observation of differences, is necessary for the purpose of ascertaining the facts upon which botany or any other classificatory science is based, it is a judicious synthesis alone, associating individuals by the ties of language, which can enable the human mind to take a comprehensive view of these facts, to deduce from them the principles of the science, or to communicate to others either facts or principles.

2. Comparative Anatomy.

The Language of Botany, as framed by Linnæus, and regulated by his Canons, is still the most notable and successful example of scientific terminology which has obtained general reception among naturalists. But the Language of Anatomy, and especially of the Comparative Anatomy of the skeleton, has of late been an object of great attention to physiologists; and especially to Mr. Owen; and the collection of terms which he has proposed are selected with so much thought and care, that they may minister valuable lessons to us in this part of our subject.

There is, at first sight, this broad difference between the descriptive language of Botany and of Comparative

Anatomy; that in the former science, we have comparatively few parts to describe, (*calyx*, *corolla*, *stamen*, *pistil*, *pericarp*, *seed*, &c.): while each of these parts is susceptible of many forms, for describing which with precision many terms must be provided: in Comparative Anatomy, on the other hand, the skeletons of many animals are to be regarded as modifications of a common type, and the terms by which their parts are described are to mark this community of type. The terminology of Botany has for its object *description;* the language of Comparative Anatomy must have for its basis *morphology*. Accordingly, Mr. Owen's terms are selected so as to express the analogies, or, as he calls them, the *homologies* of the skeleton; those parts of the skeleton being termed *homologues*, which have the same place in the general type, and therefore ought to have the same name.

Yet this distinction of the basis of botanical and anatomical terminology is not to be pushed too far. The primary definitions in botany, as given by Linnæus, are founded on morphological views; and imply a general type of the structure of plants. These are his definitions (*Phil. Bot.* Art. 86).

CALYX, *Cortex* plantæ in Fructificatione præsens.

COROLLA, *Liber* plantæ in Flora præsens.

STAMEN, Viscus pro Pollinis præparatione.

PISTILLUM, Viscus fructui adherens pro Pollinis receptione.

PERICARPIUM, Viscus gravidum seminibus, quæ matura dimittit.

But in what follows these leading definitions, the terms are descriptive merely. Now in Comparative Anatomy, an important object of terms is, to express what part of the type each bone represents—to answer the question, *what* is it? before we proceed, assuming that we know what it is, to describe its shape. The difficulty of this previous question is very great when we come to the bones of the head; and when we assume, as morphology leads us to do, that the heads of all vertebrated animals, including even fishes, are composed of homologous bones. And, as I have already said in the History (b. xvii. c. 7), speaking of Animal Morphology, the best physiologists are now agreed that the heads of vertebrates may be resolved into a series of vertebræ, homologically repeated and modified in different animals. This doctrine has been gradually making its way among anatomists, through a great variety of views respecting details; and hence, with great discrepancies in the language by which it has been expressed. Mr. Owen has proposed a complete series of terms for the bones of the head of all vertebrates; and these names are supported by reasons which are full of interest and instruction to the physiologist, on account of the comprehensive and precise knowledge of comparative osteology which they involve; but they are also, as I have said, interesting and instructive to us, as exemplifying the reasons which may be given for the adoption of words in scientific language. The reasons thus given agree with several of the aphorisms which I have laid down, and may perhaps suggest a few others. Mr. Owen has done me the great honour to quote with approval some of these aphorisms. The terms which he has proposed belong, as I have already said, to the *Terminology*, not to the *Nomenclature* of Zoology. In the latter subject, the Nomenclature (the names of species) the binary nomenclature established by Linnæus remains, in its principle, unshaken, simple and sufficient.

I shall best derive from Mr. Owen's labours and reflexions some of the instruction which they supply with reference to the Language of Science, by making remarks on his terminology with reference to such aphorisms as I have propounded on the subject, and others of a like kind.

Mr. Owen, in his *Homologies of the Vertebrate Skeleton*, has given in a Tabular Form his views of the homology of the bones of the head of vertebrates, and the names which he consequently proposes for each bone, with the synonyms as they occur in the writings of some of the most celebrated anatomical philosophers, Cuvier, Geoffroy, Hallmann, Meckel and Wagner, Agassiz and Soemmering. And he has added to this Table his reasons for dissenting from his predecessors to the extent to which he has done so. He has done this, he says, only where nature seemed clearly to refuse her sanction to them; acting upon the maxim (our Aphorism X.) that new terms and changes of terms which are not needed in order to express truth, are to be avoided. The illustrations which I have there given, however, of this maxim, apply rather to the changes in nomenclature than in terminology; and though many considerations apply equally to these two subjects, there are some points in which the reasons differ in the two cases: especially in this point:—the names, both of genera and of species, in a system of nomenclature, may be derived from casual or arbitrary circumstances, as I have said in Aphorism XIII. But the terms of a scientific terminology ought to cohere as a system, and therefore should not commonly be derived from anything casual or arbitrary, but from some analogy or connexion. Hence it seems unadvisable to apply to bones terms derived from the names of persons, as *ossa wormiana;* or even from an accident in anatomical history, as *os innominatum.*

It is further desirable that in establishing such a terminology, each bone should be designated by a single word, and not by a descriptive phrase, consisting of substantive and adjective. On this ground Mr. Owen proposes *presphenoid* for *sphenoïde anterieur.* So also *prefrontal* is preferred to *anterior frontal,* and *postfrontal* to *posterior frontal.* And the reason which he gives for this is worthy of being stated as an Aphorism, among those which should regulate this subject. I shall therefore state it thus:

Aphorism XXIV.

It is advisable to substitute definite single names for descriptive phrases as better instruments of thought.

It will be recollected by the reader that in the case of the Linnæan reform of the botanical nomenclature of species, this was one of the great improvements which was introduced.

Again: some of the first of the terms which Mr. Owen proposes illustrate, and confirm by their manifest claim to acceptance, a maxim which we stated as Aphorism XXII.: namely,

When alterations in technical terms become necessary, it is desirable that the new term should contain in its form some memorial of the old one.

Thus for 'basilaire,' which Cuvier exclusively applies to the 'pars basilaris' of the occiput, and which Geoffroy as exclusively applies (in birds) to the 'pars basilaris' of the sphenoid, Mr. Owen substitutes the term *basioccipital*.

Again: for the term 'suroccipital' of Geoffroy, Mr. Owen proposes *paroccipital*, to avoid confusion and false suggestion: and with reference to this word, he makes a remark in agreement with what we have said in the discussion of Aphorism XXI.: namely, that the combination of different languages in the derivation of words, though to be avoided in general, is in some cases admissible. He says, 'If the purists who are distressed by such harmless hybrids as "mineralogy," "terminology," and "mammalogy," should protest against the combination of the Greek prefix to the Latin noun, I can only plead that servility to a particular source of the fluctuating sounds of vocal language is a matter of taste: and that it seems no unreasonable privilege to use such elements as the servants of thought; and in the interests of science to combine them, even though they come from different countries, when the required duty is best and most expeditiously performed by their combination.'

So again we have illustrations of our Aphorism XII., that if terms are systematically good they are not to be rejected because they are etymologically inaccurate. In reference to that bone of the skull which has commonly been called *vomer*, the ploughshare: a term which Geoffroy rejected, but which Mr. Owen retains, he says, 'When Geoffroy was induced to reject the term *vomer* as being applicable only to the peculiar form of the bone in a small portion of the vertebrata, he appears not to have considered that the old term, in its wider application, would be used without reference to its primary allusion to the ploughshare, and that becoming, as it has, a purely arbitrary term, it is superior and preferable to any partially descriptive one.'

Another condition which I have mentioned in Aphorism XX., as valuable in technical terms is, that they should be susceptible of such grammatical relations as their scientific use requires.

This is, in fact, one of the grounds of the Aphorism which we have already borrowed from Mr. Owen, that we are to prefer single substantives to descriptive phrases. For from such substantives we can derive adjectives, and other forms; and thus the term becomes, as Mr. Owen says, *a better instrument of thought.* Hence, he most consistently mentions it as a recommendation of his system of names, that by them the results of a long series of investigations into the special homologies of the bones of the head are expressed in simple and definite terms, *capable of every requisite inflection* to express the proportion of the parts.

I may also, in reference to this same passage in Mr. Owen's appeal in behalf of his terminology, repeat what I have said under Aphorism X.: that the persons who may most properly propose new scientific terms, are those who have much new knowledge to communicate: so that the vehicle is commended to general reception by the value of what it contains. It is only to eminent discoverers and profound philosophers that the authority is conceded of introducing a new system of terms; just as it is only the highest authority in the state which has the power of putting a new coinage into circulation. The long series of investigations of which the results are contained in Mr. Owen's table of synonyms, and the philosophical spirit of his generalizations, entitles him to a most respectful hearing when he appeals to the Professors and Demonstrators of Human Anatomy for an unbiassed consideration of the advantages of the terms proposed by him, as likely to remedy the conflicting and unsettled synonymy which has hitherto pervaded the subject.

There is another remark which is suggested by the works on Comparative Anatomy, which I am now considering. I have said in various places that Technical

Terms are a necessary condition of the progress of a science. But we may say much more than this: and the remark is so important, that it deserves to be stated as one of our Aphorisms, as follows:

APHORISM XXV.

In an advanced Science, the history of the Language of the Science is the history of the Science itself.

I HAVE already stated in previous Aphorisms (VIII. and XI.) that Terms must be constructed so as to be fitted to enunciate general propositions, and that Terms which imply theoretical views are admissible for this purpose. And hence it happens that the history of Terms in any science which has gone through several speculative stages, is really the history of the generalizations and theories which have had currency among the cultivators of the science.

This appears in Comparative Anatomy from what we have been saying. The recent progress of that science is involved in the rise and currency of the Terms which have been used by the anatomists whose synonyms Mr. Owen has to discuss; and the reasons for selecting among these, or inventing others, include those truths and generalizations which are the important recent steps of the science. The terms which are given by Mr. Owen in his table to denote the bones of the head are good terms, *if* they *are* good terms, because their adoption and use is the only complete way of expressing the truths of homology: namely, of that Special Homology, according to which all vertebrate skeletons are referred to the human skeleton as their type, and have their parts designated accordingly.

But further: there is another kind of homology which Mr. Owen calls *General* Homology, according to which the primary type of a vertebrate animal is merely a series of vertebræ; and all limbs and other appendages are only developements of the parts of one or another of the vertebræ. And in order to express this view, and in proportion as the doctrine has become current amongst anatomists, the parts of vertebræ have been described by terms of a degree of generality which admit of such an interpretation. And here, also, Mr. Owen has proposed a terminology for the parts of the vertebræ, which seems to convey more systematically and comprehensively than those of preceding writers the truths to which they have been tending. Each vertebra is composed of a *centrum*, *neurapophysis*, *parapophysis*, *pleurapophysis*, *hæmaphysis*, *neural spine*, and *hæmal spine*, with certain exogenous parts.

The opinion that the head, as well as the other parts of the frame of vertebrates, is composed of vertebræ, is now generally accepted among philosophical anatomists. In the *History* (*Hist. I. S.* b. xvii. c. 7, sect. 1), I have mentioned this opinion as proposed by some writers; and I have stated that Oken, in 1807 published a 'Program' *On the signification of the bones of the Skull*, in which he maintained, that these bones are equivalent to four vertebræ: while Meckel, Spix, and Geoffroy took views somewhat different. Cuvier and Agassiz opposed this doctrine, but Mr. Owen has in his *Archetype and Homologies of the Vertebrate Skeleton* (1848), accepted the views of Oken, and argued at length against the objections of Cuvier, and also those of Mr. Agassiz. As I have noted in the last edition of the *History of the Inductive Sciences* (b. xvii. c. 7), he gives a Table in which the Bones of the Head are resolved into four vertebræ, which he terms the Occipital, Parietal, Frontal and Nasal Vertebræ respectively: the neural arches of which agree with what Oken called the Ear-vertebra, the Jaw-vertebra, the Eye-vertebra, and the Nose-vertebra.

Besides these doctrines of *Special Homology* by which the bones of all vertebrates are referred to their corresponding bones in the human skeleton, and of *General Homology*, by which the bones are referred to the parts of vertebræ which they represent, Mr. Owen treats of *Serial Homology*, the recognition of the same elements throughout the series of segments of the same skeleton; as when we shew in what manner the arms correspond to the legs. And thus, he says, in the head also, the *basioccipital*, *basisphenoid*, *presphenoid* and *vomer* are homotypes with the *centrums* of all succeeding vertebræ. The *exoccipitals*, *alisphenoids*, *orbitosphenoids*, and *prefrontals*, are homotypes with the *neurapophyses* of all the succeeding vertebræ. The *paroccipitals*, *mastoids* and *postfrontals*, with the *transverse processes* of all the succeeding vertebræ: and so on. Perhaps these examples may exemplify sufficiently for the general reader both Mr. Owen's terminology, and the intimate manner in which it is connected with the widest generalizations to which anatomical philosophy has yet been led.

The same doctrine, that the history of the Language of a Science is the history of the Science, appears also in the recent progress of Chemistry; but we shall be better able to illustrate our Aphorism in this case by putting forward previously one or two other Aphorisms bearing upon the history of that Science.

Aphorism XXVI.

In the Terminology of Science it may be necessary to employ letters, numbers, and algebraical symbols.

1. Mineralogy.

I have already said, in Aphorism XV., that symbols have been found requisite as a part of the terminology of Mineralogy. The *names* proposed by Haüy, borrowed from the crystalline laws, were so inadequate and unsystematic that they could not be retained. He himself proposed a *notation* for crystalline forms, founded upon his principle of the derivation of such forms from a *primitive* form, by *decrements*, on its *edges* or its *angles*. To denote this derivation he took the first letters of the three syllables to mark the faces of the *PriMiTive* form, *P*, *M*, *T*; the vowels *A*, *E*, *I*, *O* to mark the angles; the consonants *B*, *C*, *D*, &c. to mark the edges; and numerical exponents, annexed in various positions to these letters, represented the law and manner of derivation. Thus when the primitive form was a cube, $\overset{1}{B}$ represented the result of a derivation by a decrement of one row on an edge; that is, a rhombic octahedron; and $\overset{1}{B}P$ represented the combination of this octahedron with the primitive cube. In this way the pentagonal dodecahedron, produced by decrements of 2 to 1 on half the edges of the cube, was represented by $B^2 \overset{\frac{1}{2}}{C} G^2 \, {}^2G$.

Not only, however, was the hypothesis of primitive forms and decrements untenable, but this notation was too unsystematic to stand long. And when Weiss and Mohs established the distinction of Systems of Crystallography[1], they naturally founded upon that distinction a notation for crystalline forms. Mohs had several followers; but his algebraical notation so barbarously violated all algebraical meaning, that it was not likely to last. Thus, from a primitive rhombohedron which he designated by R, he derived, by a certain process, a series of other rhombohedrons, which he denoted by $R+1$, $R+2$, $R-1$, &c.; and then, by another mode of derivation from them, he obtained forms which he marked as $(R+2)^2$, $(R+2)^3$, &c. In doing this he used the algebraical marks of addition and involution without the smallest ground; besides many other proposals no less transgressing mathematical analogy and simplicity.

But this notation might easily suggest a better. If we take a primitive form, we can generally, by two steps of derivation, each capable of numerical measure, obtain any possible face; and therefore any crystalline form bounded by such faces. Hence all that we need indicate in our crystalline laws is the primitive form, and two numerical exponents; and rejecting all superfluity in our symbols, instead of $(R+2)^3$ we might write $2\,R\,3$. Nearly of this kind is the notation of Naumann. The systems of crystallization, the octahedral or tessular, the rhombic, and the prismatic, are marked by the letters O, R, P; and from these are derived, by certain laws, such symbols as

$$3O\tfrac{1}{2},\ \infty\,R\,2,\ \tfrac{1}{2}P\,2,$$

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1 *Hist. Ind. Sc.* b. xv. c. 4.

which have their definite signification flowing from the rules of the notation.

But Professor Miller, who has treated the subject of Crystallography in the most general and symmetrical manner, adopts the plan of marking each crystalline plane by *three* numerical indices. Thus in the Octahedral System, the cube is {100}; the octahedron is {111}; the rhombic dodecahedron is {011}; the pentagonal dodecahedron is π {012}; where π indicates that the form is not *holohedral* but *hemihedral*, only half the number of faces being taken which the law of derivation would give. This system is the most mathematically consistent, and affords the best means of calculation, as Professor Miller has shown; but there appears to be in it this defect, that though an essential part of the scheme is the division of crystalline forms into Systems,—the Octahedral, Pyramidal, Rhombohedral and Prismatic,—this division does not at all appear in the notation.

But whatever be the notation which the crystallographer adopts, it is evident that he must employ some notation; and that, without it, he will be unable to express the forms and relations of forms with which he has to deal.

2. CHEMISTRY.

The same has long been the case in Chemistry. As I have stated elsewhere[2], the chemical nomenclature of the oxygen theory was for a time very useful and effective. But yet it had defects which could not be overlooked, as I have already stated under Aphorism II. The relations of elements were too numerous, and their numerical properties too important, to be expressed by terminations and other modifications of words. Thus the compounds of Nitrogen and Oxygen are the Protoxide, the Deutoxide, Nitrous Acid, Peroxide of Nitrogen, Nitric Acid. The systematic nomenclature here, even thus loosely extended, does not express our knowledge. And the Atomic Theory, when established, brought to view numerical

[2] *Hist. Ind. Sc.* b. xiv. c. 6.

relations which it was very important to keep in sight. If N represents Nitrogen and O Oxygen, the compounds of the two elements just mentioned might be denoted by $N+O$, $N+2O$, $N+3O$, $N+4O$, $N+5O$. And by adopting a letter for each of the elementary substances, all the combinations of them might be expressed in this manner.

But in chemistry there are different orders of combination. A salt, for instance, is a compound of a base and an acid, each of which is already compound. If Fe be iron and C be carbon, $Fe+O$ will be the protoxide of iron, and $C+2O$ will be carbonic acid; and the carbonate of iron (more properly carbonate of protoxide of iron), may be represented by

$$(Fe+O)+(C+2O)$$

where the brackets indicate the first stage of composition.

But these brackets and signs of addition, in complex cases, would cumber the page in an inconvenient degree; and oxygen is of such very wide occurrence, that it seems desirable to abridge the notation so far as it is concerned. Hence Berzelius proposed[3] that in the first stage of composition the oxygen should be expressed by dots over the letter; and thus the carbonate of iron would be $\dot{Fe}+\ddot{C}$. But Berzelius further introduced into his notation indexes such as in algebra denote involution to the square, cube, &c. Thus Cu being copper, the sulphate of copper is represented by $\dddot{S}^2\dot{Cu}$. This notation, when first proposed, was strongly condemned by English chemists, and Berzelius's reply to them may be taken as stating the reasons in favour of such notation. He says[4], 'We answer to the opponents, that undoubtedly the matter may be looked at in various lights. The use of Formulæ has always, for a person who has not accustomed himself to them, something repulsive; but this is easy to overcome. I agree with my opponent,

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[3] *System of Mineralogy*, 1816.

[4] *Jahresbericht*, 1824, p. 119.

who says that nothing can be understood in a Formula which cannot be expressed in words; and that if the words express it as easily as the Formula, the use of the latter would be a folly. But there are cases in which this is not so; in which the Formula says in a glance what it would take many lines to express in words; and in which the expression of the Formula is clearer and more easily apprehended by the reader than the longer description in words. Let us examine such a Formula, and compare it with the equivalent description in words.. Take, for example, crystallized sulphate of copper, of which the Formula is

$$\ddot{Cu}\,\dddot{S}^2 + 10\,H^2O.$$

Now this Formula expresses the following propositions:

'That the salt consists of one atom of copper-oxide combined with 2 atoms of sulphuric acid and with 10 atoms of water; that the copper-oxide contains two atoms of oxygen; and that the sulphuric acid contains 3 atoms of oxygen for one atom of sulphur; that its oxygen is three times as much as that of the oxide; and that the number of atoms of oxygen in the acid is 6; and that the number of atoms of oxygen in the water is 10; that is, 5 times the number in the oxide; and that finally the salt contains, of simple atoms, 1 copper, 2 sulphur, 20 hydrogen, and 18 oxygen.

'Since so much is expressed in this brief Formula, how very long would the explanation be for a more composite body, for example, Alum; for which the Formula is

$$\ddot{K}\,\dddot{S}^2 + 2\dddot{Al}\,\dddot{S}^3 + 48\,H^2\,O.$$

It would take half a page to express all which this Formula contains.

'Perhaps it may be objected that it is seldom that any one wants to know all this at once. But it might reasonably be said in reply, that the peculiar value of the Formula consists in this, that it contains answers to all the questions which can be asked with regard to the composition of the body.

'But these Formulæ have also another application, of which I have sometimes had occasion to make use. Experiments sometimes bring before us combinations which cannot be foreseen from the nomenclature, and for which it is not always easy to find a consistent and appropriate name. In writing, the Formula may be applied instead of a Name: and the reader understands it better than if one made a new name. In my treatise upon the sulphuretted alkalies I found Degrees of Sulphur-combination, for which Nomenclature has no name. I expressed them, for example, by KS^6, KS^8, KS^{10}, and I believed that every one understood what was thereby meant. Moreover, I found another class of bodies in which an electro-negative sulphuretted metal played the part of an Acid with respect to an electro-positive sulphuretted metal, for which a whole new nomenclature was needed; while yet it were not prudent to construct such a nomenclature, till more is known on the subject. Instead of new names I used formulæ; for example,

$$KS^2 + 2As\,S^3,$$

instead of saying the combination of 2 atoms of Sulphuret of Arsenic containing 3 atoms of Sulphur, with one atom of Sulphuret of Potassium (Kali) with the least dose of sulphur.'

Berzelius goes on to say that the English chemists had found themselves unable to find any substitutes for his formulæ when they translated his papers.

Our English chemists have not generally adopted the notation of oxygen by dots; but have employed commas or full stops and symbols (, or . and +), to denote various degrees of union, and numerical indices. Thus the double sulphate of copper and potash is $Cu\,O, SO_3 + KO, SO_3$.

What has been said is applicable mainly to inorganic bodies (as salts and minerals)[5]. In these bodies there is (at least according to the views of many intelligent chemists) a *binary* plan of combination, union taking

[5] Fownes's *Chemistry*. Part iii.

place between *pairs* of elements, and the compounds so produced again uniting themselves to other compound bodies in the same manner. Thus, in the above example, copper and oxygen combine into oxide of copper, potassium and oxygen into potash, sulphur and oxygen into sulphuric acid; sulphuric acid in its turn combines both with oxide of copper and oxide of potassium, generating a pair of salts which are capable of uniting to form the double compound $Cu\,O, SO_3 + KO, SO_3$.

The most complicated products of inorganic chemistry may be thus shown to be built up by this repeated *pairing* on the part of their constituents. But with organic bodies the case is remarkably different; no such arrangement can here be traced. In sugar, which is $C_{12}\,H_{11}\,O_{11}$, or morphia[6], which is $C_{35}\,H_{20}\,NO_6$, the elements are as it were bound together into a single whole, which can enter into combination with other substances, and be thence discharged with properties unaltered; the elements not being obviously arranged in any subordinate groups. Hence the symbols for those substances are such as I have given above, no marks of combination being used.

It is perhaps a consequence of this peculiarity that organic compounds are *unstable* in comparison with inorganic. In unorganic substances generally the elements are combined in such a way that the most powerful affinities are satisfied[7], and hence arises a state of very considerable permanence and durability. But in an organic substance containing three or four elements, there are often opposing affinities nearly balanced, and when one of these tendencies by some accident obtains a preponderance and the equilibrium is destroyed, then the organic body breaks up into two or more new bodies of simpler and more permanent constitution.

There is another property of many organic substances which is called the *Law of Substitution*. The

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[6] Fownes's *Chemistry*, p. 354.

[7] See *Hist. Ind. Sc.* b. xiv. c. 3.

Hydrogen of the organic substance may often be replaced by Chlorine, Bromine, Iodine, or some other elements, without the destruction of the primitive type or constitution of the compound so modified. And this substitution may take place by several successive steps, giving rise to a series of substitution-compounds, which depart more and more in properties from the original substance. This Law also gives rise to a special notation. Thus a certain compound called *Dutch liquid* has the elements $C_4 H_4 Cl_2$: but this substance is affected by chlorine (Cl) in obedience to the law of substitution; one and two equivalents of hydrogen being successively removed by the prolonged action of chlorine gas aided by sunshine. The successive products may be thus written

$$C_4 H_4 Cl_2;\ C_4 \left\{ \begin{matrix} H_3 \\ Cl \end{matrix} \right\} Cl_2;\ C_4 \left\{ \begin{matrix} H_2 \\ Cl_2 \end{matrix} \right\} Cl_2.$$

Perhaps at a future period, chemical symbols, and especially those of organic bodies, may be made more systematic and more significant than they at present are.

APHORISM XXVII.

In using algebraical symbols as a part of scientific language, violations of algebraical analogy are to be avoided, but may be admitted when necessary.

As we must in scientific language conform to etymology, so must we to algebra; and as we are not to make ourselves the slaves of the former, so also, not to the latter. Hence we reject such crystallographical notation as that of Mohs; and in chemistry we use C_2, O_3, rather than C^2, O^3, which signify the square of C and the cube of O. But we may use, as we have said, both the comma and the sign of addition, for chemical combination, for the sake of brevity, though both steps of combination are really addition.

Aphorism XXVIII.

In a complex science, which is in a state of transition, capricious and detached derivations of terms are common; but are not satisfactory.

In this remark I have especial reference to Chemistry; in which the discoveries made, especially in organic chemistry, and the difficulty of reducing them to a system, have broken up in several instances the old nomenclature, without its being possible at present to construct a new set of terms systematically connected. Hence it has come to pass that chemists have constructed words in a capricious and detached way: as by taking fragments of words, and the like. I shall give some examples of such derivations, and also of some attempts which have more of a systematic character.

I have mentioned (Aph. XV. sect. 7) the word *Ellagic* (acid), made by inverting the word *Galle*. Several words have recently been formed by chemists by taking syllables from two or more different words. Thus Chevreul discovered a substance to which he gave the name *Ethol*, from the first syllables of the words *ether* and *alcohol*, because of its analogy to those liquids in point of composition[1]. So Liebig has the word *chloral*[2].

Liebig, examining the product of distillation of alcohol, sulphuric acid and amber, found a substance which he termed *Aldehyd*, from the words *Al*cohol *dehyd*rogenated[3]. This mode of making words has been strongly objected to by Mr. Dumas[4]. Still more has he objected to the word *Mercaptan* (of Zeise), which

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1 Turner's *Chemistry*, 1834, p. 955.

2 Berzelius' *Jahresbericht*, xv. p. 372.

3 *Ibid*. xvi. p. 308.

4 *Leçons de Chimie*, p. 354.

he says rests upon a mere play of words; for it means both *mercurium captans* and *mercurio aptum*.

Dumas and Peligot, working on pyroligneous acids, found reason to believe the existence of a substance[5] which they called *methylene*, deriving the name from *methy*, a spirituous fluid, and *hyle*, wood. Berzelius remarks that the name should rather be *methyl*, and that ὕλη may be taken in its signification of matter, to imply the Radical of Wine: and he proposes that the older Æther-Radical, $C_4 H_{10}$ shall be called *Æthyl*; the newer, $C_2 H_6$, *Methyl*.

This notion of marking by the termination *yl* the hypothetical compound radical of a series of chemical compounds has been generally adopted; and, as we see from the above reference, it must be regarded as representing the Greek word ὕλη: and such hypothetical radicals of bases have been termed in general *basyls*.

Bunsen obtained from Cadet's fuming liquid a substance which he called *Alkarsin* (*alk*ali-*ars*enic?): and the substance produced from this by oxidation he called *Alkargen*[6]. Berzelius was of opinion, that the true view of its composition was that it contained a compound ternary radical $= C^4\ H^{12}\ As^2$, after the manner of organic bodies; and he proposed for this the name[7] *Kakodyl*. Alkarsin is Kakodyl-oxyd, $\dot{K}d$. Alkargen is Kakodyl-acid, $\ddot{K}d$.

The discovery of Kakodyl was the first instance of the insulation of an organic metallic *basyl*[8].

The first of the Hydrocarbon Radicals of the Alcohols was the radical of Tetrylic alcohol obtained by Kolbe from Valerate of Potash, and hence called *Valyl* $C_{16} H_{18}$.

Chloroform is per*chloride* of *formyl*, the hypothetical radical of formic acid[9].

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[5] Berzelius' *Jahresbericht*, xv. (1836).

[6] *Ibid.* xviii. p. 497.

[7] *Ibid.* xx. p. 527.

[8] Miller's *Chemistry*, iii. 220.

[9] Dumas, *Leçons sur la Phil. Chim.* p. 356.

The discovery of such bases goes back to 1815. The substance formerly called *Prussiate of Mercury*, being treated in a particular manner, was resolved into metallic mercury and *Cyanogen*. This substance, *Cyanogen*, is, according to the older nomenclature, *Bicarburet of Nitrogen;* but chemists are agreed that its most convenient name is *Cyanogen*, proposed by its discoverer, Gay-Lussac, in 1815[10]. The importance of the discovery consists in this; that this substance was the first compound body which was distinctly proved to enter into combination with elementary substances in a manner similar to that in which they combine with each other.

The truth of our Aphorism (XXV.) that in such a science as chemistry, the history of the scientific nomenclature is the history of the science, appears from this; that the controversies with respect to chemical theories and their application take the form of objections to the common systematic names and proposals of new names instead. Thus a certain compound of potassa, sulphur, hydrogen, and oxygen, may be regarded either as *Hydrosulphate of Potassa*, or as *Sulphide of Potassium in solution*, according to different views[11]. In some cases indeed, changes are made merely for the sake of clearness. Instead of *Hydrochloric* and *Hydrocyanic* acid, many French writers, following Thenard, transpose the elements of these terms; they speak of *Chlorhydric* and *Cyanhydric* acid; by this means they avoid any ambiguity which might arise from the use of the prefix *Hydro*, which has sometimes been applied to compounds which contain water[12].

An incompleteness in chemical nomenclature was further felt, when it appeared, from the properties of various substances, that mere identity in chemical composition is not sufficient to produce identity of chemical character or properties[13]. The doctrine of

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10 Turner's *Chemistry* (1834), p. 420. Miller's *Chemistry*, ii. 66.

11 Miller's *Chemistry*, vol. ii. p. 583.

12 *Ibid.* ii. 433. 13 *Ibid.* ii. 653.

the existence of compounds identical in ultimate composition, but different in chemical properties, was termed *Isomerism*. Thus chemists enumerate the following compounds, all of which contain carbon and hydrogen in the proportion of single equivalents of each[14]:—*Methylene, Olefiant gas, Propylene, Oil gas, Amylene, Caproylene, Naphthene, Eleene, Peramylene, Cetylene, Cerotylene, Melissine.*

I will, in the last place, propound an Aphorism which has already offered itself in considering the history of Chemistry[15], as having a special bearing upon that Science, but which may be regarded as the supreme and ultimate rule with regard to the language of Science.

Aphorism XXIX.

In learning the meaning of Scientific Terms, the history of science is our Dictionary: the steps of scientific induction are our Definitions.

It is usual for unscientific readers to complain that the technical terms which they meet with in books of science are not accompanied by plain definitions such as they can understand. But such definitions cannot be given.. For definitions must consist of words; and, in the case of scientific terms, must consist of words which require again to be defined: and so on, without limit. *Elementary substances* in chemistry, for instance, what are they? The substances into which bodies can be *analysed*, and by the junction of which they are *composed*. But what is *analysis*? what is *composition*? We have seen that it required long and laborious courses of experiment to answer these questions; and that finally the balance decided among rival answers. And so it is in other cases. In entering upon each science, we come upon a new set of words. And how are we to learn

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14 Miller's *Chemistry*, ii. 654.

15 *Hist. Ind. Sc.* b. xiv. c. 1.

the meaning of this collection of words? In what other language shall it be explained? In what terms shall we define these new expressions? To this we are compelled to reply, that we cannot translate these terms into any ordinary or familiar language. Here, as in all other branches of knowledge, the meaning of words is to be sought in the progress of thought. It is only by going back through the successful researches of men respecting the *composition* and *elements* of bodies, that we can learn in what sense such terms can be understood, so as to convey real knowledge. In order that they may have a meaning for us, we must inquire what meaning they had in the minds of the authors of our discoveries. And the same is the case in other subjects. To take the instance of Morphology. When the beginner is told that every group of animals may be reduced to an *Archetype*, he will seek for a definition of Archetype. Such a definition has been offered, to this effect: the Archetype of a group of animals is a diagram embodying all the organs and parts which are found in the group in such a relative position as they would have had if none had attained an excessive development. But, then, we are led further to ask, How are we in each case to become acquainted with the diagram; to know of what parts it consists, and how they are related; and further; What is the standard of *excess?* It is by a wide examination of particular species, and by several successive generalizations of observed facts, that we are led to a diagram of an animal form of a certain kind, (for example, a vertebrate;) and of the various ways, excessive and defective, in which the parts may be developed.

This craving for definitions, as we have already said, arises in a great degree from the acquaintance with geometry which most persons acquire at an early age. The definitions of geometry are easily intelligible by a beginner, because the idea of space, of which they are modifications, is clearly possessed without any special culture. But this is not and cannot be the case in other sciences founded upon a wide and exact observation of facts.

It was formerly said that there was no Royal Road to Geometry: in modern times we have occasion often to repeat that there is no Popular Road—no road easy, pleasant, offering no difficulty and demanding no toil,—to Comparative Anatomy, Chemistry or any other of the Inductive Sciences.

THE END.

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