MILL
from A SYSTEM of LOGIC

CHAPTER V.

OF DEMONSTRATION, AND NECESSARY TRUTHS.

§ 1. If, as laid down in the two preceding chapters, the foundation of all sciences, even deductive or demonstrative sciences, is Induction; if every step in the ratiocinations even of geometry is an act of induction; and if a train of reasoning is but bringing many inductions to bear upon the same subject of inquiry, and drawing a case within one induction by means of another; wherein lies the peculiar certainty always ascribed to the sciences which are entirely, or almost entirely, deductive? Why are they called the Exact Sciences? Why are mathematical certainty, and the evidence of demonstration, common phrases to express the very highest degree of assurance attainable by reason? Why are mathematics by almost all philosophers, and (by some) even those branches of natural philosophy which, through the medium of mathematics, have been converted into deductive sciences, considered to be independent of the evidence of experience and observation, and characterized as systems of Necessary Truth?

The answer I conceive to be, that this character of necessity, ascribed to the truths of mathematics, and (even with some reservations to be hereafter made) the peculiar certainty attributed to them, is an illusion; in order to sustain which, it is necessary to suppose that those truths relate to, and express the properties of, purely imaginary objects. It is acknowledged that the conclusions of geometry are deduced, partly at least, from the so-called Definitions, and that those definitions are assumed to be correct representations, as far as they go, of the objects with which geometry is conversant. Now we have pointed out that, from a definition as such, no proposition, unless it be one concerning the meaning of a word, can ever follow; and that what apparently follows from a definition, follows in reality from an implied assumption that there exists a real thing conformable thereto. This assumption, in the case of the definitions of geometry, is not strictly true: there exist no real things exactly conformable to the definitions. There exist no points without magnitude; no lines without breadth, nor perfectly straight; no circles with all their radii exactly equal, nor squares with all their angles perfectly right. It will perhaps be said that the assumption does not extend to the actual, but only to the possible, existence of such things. I answer that, according to any test we have of possibility, they are not even possible. Their existence, so far as we can form any judgment, would seem to be inconsistent with the physical constitution of our planet at least, if not of the universe. To get rid of this difficulty, and at the same time to save the credit of the supposed system of necessary truth, it is customary to say that the points, lines, circles, and squares which are the subject of geometry, exist in our conceptions merely, and are part of our minds; which minds, by working on their own materials, construct an *a priori* science, the evidence of which is purely mental, and has nothing whatever to do with outward experience. By howsoever high authorities this doctrine may have been sanctioned, it appears

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to me psychologically incorrect. The points, lines, circles, and squares which any one has in his mind, are (I apprehend) simply copies of the points, lines, circles, and squares which he has known in his experience. Our idea of a point, I apprehend to be simply our idea of the *minimum visibile*, the smallest portion of surface which we can see. A line, as defined by geometers, is wholly inconceivable. We can reason about a line as if it had no breadth; because we have a power, which is the foundation of all the control we can exercise over the operations of our minds; the power, when a perception is present to our senses, or a conception to our intellects, of *attending* to a part only of that perception or conception, instead of the whole. But we can not *conceive* a line without breadth; we can form no mental picture of such a line: all the lines which we have in our minds are lines possessing breadth. If any one doubts this, we may refer him to his own experience. I much question if any one who fancies that he can conceive what is called a mathematical line, thinks so from the evidence of his consciousness: I suspect it is rather because he supposes that unless such a conception were possible, mathematics could not exist as a science: a supposition which there will be no difficulty in showing to be entirely groundless.

Since, then, neither in nature, nor in the human mind, do there exist any objects exactly corresponding to the definitions of geometry, while yet that science can not be supposed to be conversant about nonentities; nothing remains but to consider geometry as conversant with such lines, angles, and figures, as really exist; and the definitions, as they are called, must be regarded as some of our first and most obvious generalizations concerning those natural objects. The correctness of those generalizations, *as* generalizations, is without a flaw: the equality of all the radii of a circle is true of all circles, so far as it is true of any one: but it is not exactly true of any circle; it is only nearly true; so nearly that no error of any importance in practice will be incurred by feigning it to be exactly true. When we have occasion to extend these inductions, or their consequences, to cases in which the error would be appreciable—to lines of perceptible breadth or thickness, parallels which deviate sensibly from equidistance, and the like—we correct our conclusions, by combining with them a fresh set of propositions relating to the aberration; just as we also take in propositions relating to the physical or chemical properties of the material, if those properties happen to introduce any modification into the result; which they easily may, even with respect to figure and magnitude, as in the case, for instance, of expansion by heat. So long, however, as there exists no practical necessity for attending to any of the properties of the object except its geometrical properties, or to any of the natural irregularities in those, it is convenient to neglect the consideration of the other properties and of the irregularities, and to reason as if these did not exist: accordingly, we formally announce in the definitions, that we intend to proceed on this plan. But it is an error to suppose, because we resolve to confine our attention to a certain number of the properties of an object, that we therefore conceive, or have an idea of, the object, denuded of its other properties. We are thinking, all the time, of precisely such objects as we have seen and touched, and with all the properties which naturally belong to them; but, for scientific convenience, we feign them to be divested of all properties, except those which are material to our purpose, and in regard to which we design to consider them.

The peculiar accuracy, supposed to be characteristic of the first principles of geometry, thus appears to be fictitious. The assertions on which the reasonings of the science are founded, do not, any more than in other sciences, exactly correspond with the fact; but we suppose that they do so, for the sake of tracing the consequences which follow from the supposition. The opinion of Dugald Stewart respecting the foundations of geometry, is, I conceive, substantially correct; that it is built on hypotheses; that it owes to this alone the peculiar certainty supposed to distinguish it; and that in any science whatever, by reasoning from a set of hypotheses, we may obtain a body of conclusions as certain as those of geometry, that is, as strictly in accordance with the hypotheses, and as irresistibly compelling assent, *on condition* that those hypotheses are true.*

When, therefore, it is affirmed that the conclusions of geometry are necessary truths, the necessity consists in reality only in this, that they correctly follow from the suppositions from which they are deduced. Those suppositions are so far from being necessary, that they are not even true; they purposely depart, more or less widely, from the truth. The only sense in which necessity can be ascribed to the conclusions of any scientific investigation, is that of legitimately following from some assumption, which, by the conditions of the inquiry, is not to be questioned. In this relation, of course, the derivative truths of every deductive science must stand to the inductions, or assumptions, on which the science is founded, and which, whether true or untrue, certain or doubtful in themselves, are always supposed certain for the purposes of the particular science. And therefore the conclusions of all deductive sciences were said by the ancients to be necessary propositions. We have observed already that to be predicated necessarily was characteristic of the predicable Proprium, and that a proprium was any property of a thing which could be deduced from its essence, that is, from the properties included in its definition.

§ 2. The important doctrine of Dugald Stewart, which I have endeavored to enforce, has been contested by Dr. Whewell, both in the dissertation appended to his excellent *Mechanical Euclid*, and in his elaborate work on the *Philosophy of the Inductive Sciences;* in which last he also replies to an article in the Edinburgh Review (ascribed to a writer of great scientific eminence), in which Stewart's opinion was defended against his former strictures. The supposed refutation of Stewart consists in proving against him (as has also been done in this work) that the premises of geometry are not definitions, but assumptions of the real existence of things corresponding to those definitions. This, however, is doing little for Dr. Whewell's purpose; for it is these very assumptions which are asserted to be hypotheses, and which he, if he denies that geometry is founded

* It is justly remarked by Professor Bain (*Logic*, ii., 134) that the word Hypothesis is here used in a somewhat peculiar sense. An hypothesis, in science, usually means a supposition not proved to be true, but surmised to be so, because if true it would account for certain known facts; and the final result of the speculation may be to prove its truth. The hypotheses spoken of in the text are of a different character; they are known not to be literally true, while as much of them as is true is not hypothetical, but certain. The two cases, however, resemble in the circumstance that in both we reason, not from a truth, but from an assumption, and the truth therefore of the conclusions is conditional, not categorical. This suffices to justify, in point of logical propriety, Stewart's use of the term. It is of course needful to bear in mind that the hypothetical element in the definitions of geometry is the assumption that what is very nearly true is exactly so. This unreal exactitude might be called a fiction, as properly as an hypothesis; but that appellation, still more than the other, would fail to point out the close relation which exists between the fictitious point or line and the points and lines of which we have experience.

on hypotheses, must show to be absolute truths. All he does, however, is to observe, that they, at any rate, are not *arbitrary* hypotheses; that we should not be at liberty to substitute other hypotheses for them; that not only "a definition, to be admissible, must necessarily refer to and agree with some conception which we can distinctly frame in our thoughts," but that the straight lines, for instance, which we define, must be "those by which angles are contained, those by which triangles are bounded, those of which parallelism may be predicated, and the like."* And this is true; but this has never been contradicted. Those who say that the premises of geometry are hypotheses, are not bound to maintain them to be hypotheses which have no relation whatever to fact. Since an hypothesis framed for the purpose of scientific inquiry must relate to something which has real existence (for there can be no science respecting nonentities), it follows that any hypothesis we make respecting an object, to facilitate our study of it, must not involve any thing which is distinctly false, and repugnant to its real nature: we must not ascribe to the thing any property which it has not; our liberty extends only to slightly exaggerating some of those which it has (by assuming it to be completely what it really is very nearly), and suppressing others, under the indispensable obligation of restoring them whenever, and in as far as, their presence or absence would make any material difference in the truth of our conclusions. Of this nature, accordingly, are the first principles involved in the definitions of geometry. That the hypotheses should be of this particular character, is, however, no further necessary, than inasmuch as no others could enable us to deduce conclusions which, with due corrections, would be true of real objects: and in fact, when our aim is only to illustrate truths, and not to investigate them, we are not under any such restriction. We might suppose an imaginary animal, and work out by deduction, from the known laws of physiology, its natural history; or an imaginary commonwealth, and from the elements composing it, might argue what would be its fate. And the conclusions which we might thus draw from purely arbitrary hypotheses, might form a highly useful intellectual exercise: but as they could only teach us what *would* be the properties of objects which do not really exist, they would not constitute any addition to our knowledge of nature: while, on the contrary, if the hypothesis merely divests a real object of some portion of its properties, without clothing it in false ones, the conclusions will always express, under known liability to correction, actual truth.

§ 3. But though Dr. Whewell has not shaken Stewart's doctrine as to the hypothetical character of that portion of the first principles of geometry which are involved in the so-called definitions, he has, I conceive, greatly the advantage of Stewart on another important point in the theory of geometrical reasoning; the necessity of admitting, among those first principles, axioms as well as definitions. Some of the axioms of Euclid might, no doubt, be exhibited in the form of definitions, or might be deduced, by reasoning, from propositions similar to what are so called. Thus, if instead of the axiom, Magnitudes which can be made to coincide are equal, we introduce a definition, "Equal magnitudes are those which may be so applied to one another as to coincide;" the three axioms which follow (Magnitudes which are equal to the same are equal to one another—If equals are added to equals, the sums are equal—If equals are taken from equals,

* *Mechanical Euclid*, pp. 149 *et seqq.*

the remainders are equal), may be proved by an imaginary superposition, resembling that by which the fourth proposition of the first book of Euclid is demonstrated. But though these and several others may be struck out of the list of first principles, because, though not requiring demonstration, they are susceptible of it; there will be found in the list of axioms two or three fundamental truths, not capable of being demonstrated: among which must be reckoned the proposition that two straight lines can not inclose a space (or its equivalent, Straight lines which coincide in two points coincide altogether), and some property of parallel lines, other than that which constitutes their definition: one of the most suitable for the purpose being that selected by Professor Playfair: "Two straight lines which intersect each other can not both of them be parallel to a third straight line."*

The axioms, as well those which are indemonstrable as those which admit of being demonstrated, differ from that other class of fundamental principles which are involved in the definitions, in this, that they are true without any mixture of hypothesis. That things which are equal to the same thing are equal to one another, is as true of the lines and figures in nature, as it would be of the imaginary ones assumed in the definitions. In this respect, however, mathematics are only on a par with most other sciences. In almost all sciences there are some general propositions which are exactly true, while the greater part are only more or less distant approximations to the truth. Thus in mechanics, the first law of motion (the continuance of a movement once impressed, until stopped or slackened by some resisting force) is true without qualification or error. The rotation of the earth in twenty-four hours, of the same length as in our time, has gone on since the first accurate observations, without the increase or diminution of one second in all that period. These are inductions which require no fiction to make them be received as accurately true: but along with them there are others, as for instance the propositions respecting the figure of the earth, which are but approximations to the truth; and in order to use them for the further advancement of our knowledge, we must feign that they are exactly true, though they really want something of being so.

§ 4. It remains to inquire, what is the ground of our belief in axioms—what is the evidence on which they rest? I answer, they are experimental truths; generalizations from observation. The proposition, Two straight lines can not inclose a space—or, in other words, Two straight lines which have once met, do not meet again, but continue to diverge—is an induction from the evidence of our senses.

This opinion runs counter to a scientific prejudice of long standing and great strength, and there is probably no proposition enunciated in this work for which a more unfavorable reception is to be expected. It is, however, no new opinion; and even if it were so, would be entitled to be judged, not by its novelty, but by the strength of the arguments by which it can be supported. I consider it very fortunate that so eminent a champion of the contrary opinion as Dr. Whewell has found occasion for a most elaborate treatment of the whole theory of axioms, in attempting to construct the philosophy of the mathematical and physical sciences on the basis of the doctrine against which I now contend. Whoever is anxious that a discussion should go to the bottom of the subject, must rejoice to see the opposite side of the question worthily represented. If what is said by Dr. Whewell, in support of an opinion which he has made the foundation of a systematic work, can be shown not to be conclusive, enough will have been done, without going elsewhere in quest of stronger arguments and a more powerful adversary.

* We might, it is true, insert this property into the definition of parallel lines, framing the definition so as to require, both that when produced indefinitely they shall never meet, and also that any straight line which intersects one of them shall, if prolonged, meet the other. But by doing this we by no means get rid of the assumption; we are still obliged to take for granted the geometrical truth, that all straight lines in the same plane, which have the former of these properties, have also the latter. For if it were possible that they should not, that is, if any straight lines in the same plane, other than those which are parallel according to the definition, had the property of never meeting although indefinitely produced, the demonstrations of the subsequent portions of the theory of parallels could not be maintained.

It is not necessary to show that the truths which we call axioms are originally *suggested* by observation, and that we should never have known that two straight lines can not inclose a space if we had never seen a straight line: thus much being admitted by Dr. Whewell, and by all, in recent times, who have taken his view of the subject. But they contend, that it is not experience which *proves* the axiom; but that its truth is perceived *a priori*, by the constitution of the mind itself, from the first moment when the meaning of the proposition is apprehended; and without any necessity for verifying it by repeated trials, as is requisite in the case of truths really ascertained by observation.

They can not, however, but allow that the truth of the axiom, Two straight lines can not inclose a space, even if evident independently of experience, is also evident from experience. Whether the axiom needs confirmation or not, it receives confirmation in almost every instant of our lives; since we can not look at any two straight lines which intersect one another, without seeing that from that point they continue to diverge more and more. Experimental proof crowds in upon us in such endless profusion, and without one instance in which there can be even a suspicion of an exception to the rule, that we should soon have stronger ground for believing the axiom, even as an experimental truth, than we have for almost any of the general truths which we confessedly learn from the evidence of our senses. Independently of *a priori* evidence, we should certainly believe it with an intensity of conviction far greater than we accord to any ordinary physical truth: and this too at a time of life much earlier than that from which we date almost any part of our acquired knowledge, and much too early to admit of our retaining any recollection of the history of our intellectual operations at that period. Where then is the necessity for assuming that our recognition of these truths has a different origin from the rest of our knowledge, when its existence is perfectly accounted for by supposing its origin to be the same? when the causes which produce belief in all other instances, exist in this instance, and in a degree of strength as much superior to what exists in other cases, as the intensity of the belief itself is superior? The burden of proof lies on the advocates of the contrary opinion: it is for them to point out some fact, inconsistent with the supposition that this part of our knowledge of nature is derived from the same sources as every other part.*

* Some persons find themselves prevented from believing that the axiom, Two straight lines can not inclose a space, could ever become known to us through experience, by a difficulty which may be stated as follows: If the straight lines spoken of are those contemplated in the definition—lines absolutely without breadth and absolutely straight—that such are incapable of inclosing a space is not proved by experience, for lines such as these do not present themselves in our experience. If, on the other hand, the lines meant are such straight lines as we do meet with in experience, lines straight enough for practical purposes, but in reality slightly zigzag, and with some, however trifling, breadth; as applied to these lines the axiom is not

This, for instance, they would be able to do, if they could prove chronologically that we had the conviction (at least practically) so early in infancy as to be anterior to those impressions on the senses, upon which, on the other theory, the conviction is founded. This, however, can not be proved: the point being too far back to be within the reach of memory, and too obscure for external observation. The advocates of the *a priori* theory are obliged to have recourse to other arguments. These are reducible to two, which I shall endeavor to state as clearly and as forcibly as possible.

§ 5. In the first place it is said, that if our assent to the proposition that two straight lines can not inclose a space, were derived from the senses, we could only be convinced of its truth by actual trial, that is, by seeing or feeling the straight lines; whereas, in fact, it is seen to be true by merely thinking of them. That a stone thrown into water goes to the bottom, may be perceived by our senses, but mere thinking of a stone thrown into the water would never have led us to that conclusion: not so, however, with the axioms relating to straight lines: if I could be made to conceive what a straight line is, without having seen one, I should at once recognize that two such lines can not inclose a space. Intuition is "imaginary looking;"* but experience must be real looking: if we see a property of straight lines to be true by merely fancying ourselves to be looking at them, the ground of our belief can not be the senses, or experience; it must be something mental.

To this argument it might be added in the case of this particular axiom (for the assertion would not be true of all axioms), that the evidence of it from actual ocular inspection is not only unnecessary, but unattainable. What says the axiom? That two straight lines *can not* inclose a space; that after having once intersected, if they are prolonged to infinity they do not meet, but continue to diverge from one another. How can this, in any single case, be proved by actual observation? We may follow the lines to any distance we please; but we can not follow them to infinity: for aught our senses can testify, they may, immediately beyond the farthest point to which we have traced them, begin to approach, and at last meet. Unless, therefore, we had some other proof of the impossibility than observation affords us, we should have no ground for believing the axiom at all.

To these arguments, which I trust I can not be accused of understating, a satisfactory answer will, I conceive, be found, if we advert to one of the characteristic properties of geometrical forms—their capacity of being painted in the imagination with a distinctness equal to reality: in other words, the exact resemblance of our ideas of form to the sensations which suggest them. This, in the first place, enables us to make (at least with a little practice) mental pictures of all possible combinations of lines and angles, which resemble the realities quite as well as any which we could make on paper; and in the next place, make those pictures just as fit subjects of geometrical experimentation as the realities themselves; inasmuch as pictures, if sufficiently accurate, exhibit of course all the properties which would be manifested by the realities at one given instant, and on simple inspection: and in geometry we are concerned only with such properties, and not with that which pictures could not exhibit, the mutual action of bodies one upon another. The foundations of geometry would therefore be laid in direct experience, even if the experiments (which in this case consist merely in attentive contemplation) were practiced solely upon what we call our ideas, that is, upon the diagrams in our minds, and not upon outward objects. For in all systems of experimentation we take some objects to serve as representatives of all which resemble them; and in the present case the conditions which qualify a real object to be the representative of its class, are completely fulfilled by an object existing only in our fancy. Without denying, therefore, the possibility of satisfying ourselves that two straight lines can not inclose a space, by merely thinking of straight lines without actually looking at them; I contend, that we do not believe this truth on the ground of the imaginary intuition simply, but because we know that the imaginary lines exactly resemble real ones, and that we may conclude from them to real ones with quite as much certainty as we could conclude from one real line to another. The conclusion, therefore, is still an induction from observation. And we should not be authorized to substitute observation of the image in our mind, for observation of the reality, if we had not learned by long-continued experience that the properties of the reality are faithfully represented in the image; just as we should be scientifically warranted in describing an animal which we have never seen, from a picture made of it with a daguerreotype; but not until we had learned by ample experience, that observation of such a picture is precisely equivalent to observation of the original.

These considerations also remove the objection arising from the impossibility of ocularly following the lines in their prolongation to infinity. For though, in order actually to see that two given lines never meet, it would be necessary to follow them to infinity; yet without doing so we may know that if they ever do meet, or if, after diverging from one another, they begin again to approach, this must take place not at an infinite, but at a finite distance. Supposing, therefore, such to be the case, we can transport ourselves thither in imagination, and can frame a mental image of the appearance which one or both of the lines must present at that point, which we may rely on as being precisely similar to the reality. Now, whether we fix our contemplation upon this imaginary picture, or call to mind the generalizations we have had occasion to make from former ocular observation, we learn by the evidence of experience, that a line which, after diverging from another straight line, begins to approach to it, produces the impression on our senses which we describe by the expression, "a bent line," not by the expression, "a straight line."*

true, for two of them may, and sometimes do, inclose a small portion of space. In neither case, therefore, does experience prove the axiom.

Those who employ this argument to show that geometrical axioms can not be proved by induction, show themselves unfamiliar with a common and perfectly valid mode of inductive proof; proof by approximation. Though experience furnishes us with no lines so unimpeachably straight that two of them are incapable of inclosing the smallest space, it presents us with gradations of lines possessing less and less either of breadth or of flexure, of which series the straight line of the definition is the ideal limit. And observation shows that just as much, and as nearly, as the straight lines of experience approximate to having no breadth or flexure, so much and so nearly does the space-inclosing power of any two of them approach to zero. The inference that if they had no breadth or flexure at all, they would inclose no space at all, is a correct inductive inference from these facts, conformable to one of the four Inductive Methods hereinafter characterized, the Method of Concomitant Variations; of which the mathematical Doctrine of Limits presents the extreme case.

* Whewell's *History of Scientific Ideas*, i., 140.

* Dr. Whewell (*Philosophy of Discovery*, p. 289) thinks it unreasonable to contend that we know by experience, that our idea of a line exactly resembles a real line. "It does not appear," he says, "how we can compare our ideas with the realities, since we know the realities only by our ideas." We know the realities by our sensations. Dr. Whewell surely does not hold the "doctrine of perception by means of ideas," which Reid gave himself so much trouble to refute.

The preceding argument, which is, to my mind unanswerable, merges, however, in a still more comprehensive one, which is stated most clearly and conclusively by Professor Bain. The psychological reason why axioms, and indeed many propositions not ordinarily classed as such, may be learned from the idea only without referring to the fact, is that in the process of acquiring the idea we have learned the fact. The proposition is assented to as soon as the terms are understood, because in learning to understand the terms we have acquired the experience which proves the proposition to be true. "We required," says Mr. Bain,* "concrete experience in the first instance, to attain to the notion of whole and part; but the notion, once arrived at, implies that the whole is greater. In fact, we could not have the notion without an experience tantamount to this conclusion. .... When we have mastered the notion of straightness, we have also mastered that aspect of it expressed by the affirmation that two straight lines can not inclose a space. No intuitive or innate powers or perceptions are needed in such cases. .... We can not have the full meaning of Straightness, without going through a comparison of straight objects among themselves, and with their opposites, bent or crooked objects. The result of this comparison is, *inter alia*, that straightness in two lines is seen to be incompatible with inclosing a space; the inclosure of space involves crookedness in at least one of the lines." And similarly, in the case of every first principle,† "the same knowledge that makes it understood, suffices to verify it." The more this observation is considered the more (I am convinced) it will be felt to go to the very root of the controversy.

§ 6. The first of the two arguments in support of the theory that axioms are *a priori* truths, having, I think, been sufficiently answered; I proceed to the second, which is usually the most relied on. Axioms (it is asserted)

If Dr. Whewell doubts whether we compare our ideas with the corresponding sensations, and assume that they resemble, let me ask on what evidence do we judge that a portrait of a person not present is like the original. Surely because it is like our idea, or mental image of the person, and because our idea is like the man himself.

Dr. Whewell also says, that it does not appear why this resemblance of ideas to the sensations of which they are copies, should be spoken of as if it were a peculiarity of one class of ideas, those of space. My reply is, that I do not so speak of it. The peculiarity I contend for is only one of degree. All our ideas of sensation of course resemble the corresponding sensations, but they do so with very different degrees of exactness and of reliability. No one, I presume, can recall in imagination a color or an odor with the same distinctness and accuracy with which almost every one can mentally reproduce an image of a straight line or a triangle. To the extent, however, of their capabilities of accuracy, our recollections of colors or of odors may serve as subjects of experimentation, as well as those of lines and spaces, and may yield conclusions which will be true of their external prototypes. A person in whom, either from natural gift or from cultivation, the impressions of color were peculiarly vivid and distinct, if asked which of two blue flowers was of the darkest tinge, though he might never have compared the two, or even looked at them together, might be able to give a confident answer on the faith of his distinct recollection of the colors; that is, he might examine his mental pictures, and find there a property of the outward objects. Bnt in hardly any case except that of simple geometrical forms, could this be done by mankind generally, with a degree of assurance equal to that which is given by a contemplation of the objects themselves. Persons differ most widely in the precision of their recollection, even of forms: one person, when he has looked any one in the face for half a minute, can draw an accurate likeness of him from memory; another may have seen him every day for six months, and hardly know whether his nose is long or short. But every body has a perfectly distinct mental image of a straight line, a circle, or a rectangle. And every one concludes confidently from these mental images to the corresponding outward things. The truth is, that we may, and continually do, study nature in our recollections, when the objects themselves are absent; and in the case of geometrical forms we can perfectly, but in most other cases only imperfectly, trust our recollections.

* *Logic*, i., 222.

† Ibid., 226.

are conceived by us not only as true, but as universally and necessarily true. Now, experience can not possibly give to any proposition this character. I may have seen snow a hundred times, and may have seen that it was white, but this can not give me entire assurance even that all snow is white; much less that snow *must* be white. "However many instances we may have observed of the truth of a proposition, there is nothing to assure us that the next case shall not be an exception to the rule. If it be strictly true that every ruminant animal yet known has cloven hoofs, we still can not be sure that some creature will not hereafter be discovered which has the first of these attributes, without having the other. .... Experience must always consist of a limited number of observations; and, however numerous these may be, they can show nothing with regard to the infinite number of cases in which the experiment has not been made." Besides, Axioms are not only universal, they are also necessary. Now "experience can not offer the smallest ground for the necessity of a proposition. She can observe and record what has happened; but she can not find, in any case, or in any accumulation of cases, any reason for what *must* happen. She may see objects side by side; but she can not see a reason why they must ever be side by side. She finds certain events to occur in succession; but the succession supplies, in its occurrence, no reason for its recurrence. She contemplates external objects; but she can not detect any internal bond, which indissolubly connects the future with the past, the possible with the real. To learn a proposition by experience, and to see it to be necessarily true, are two altogether different processes of thought."* And Dr. Whewell adds, "If any one does not clearly comprehend this distinction of necessary and contingent truths, he will not be able to go along with us in our researches into the foundations of human knowledge; nor, indeed, to pursue with success any speculation on the subject."†

In the following passage, we are told what the distinction is, the non-recognition of which incurs this denunciation. "Necessary truths are those in which we not only learn that the proposition *is* true, but see that it *must be* true; in which the negation of the truth is not only false, but impossible; in which we can not, even by an effort of imagination, or in a supposition, conceive the reverse of that which is asserted. That there are such truths can not be doubted. We may take, for example, all relations of number. Three and Two added together make Five. We can not conceive it to be otherwise. We can not, by any freak of thought, imagine Three and Two to make Seven."‡

Although Dr. Whewell has naturally and properly employed a variety of phrases to bring his meaning more forcibly home, he would, I presume, allow that they are all equivalent; and that what he means by a necessary truth, would be sufficiently defined, a proposition the negation of which is not only false but inconceivable. I am unable to find in any of his expressions, turn them what way you will, a meaning beyond this, and I do not believe he would contend that they mean any thing more.

This, therefore, is the principle asserted: that propositions, the negation of which is inconceivable, or in other words, which we can not figure to ourselves as being false, must rest on evidence of a higher and more cogent description than any which experience can afford.

Now I can not but wonder that so much stress should be laid on the circumstance of inconceivableness, when there is such ample experience to

* *History of Scientific Ideas*, i., 65–67. † Ibid., i., 60. ‡ Ibid., 58, 59.

show, that our capacity or incapacity of conceiving a thing has very little to do with the possibility of the thing in itself; but is in truth very much an affair of accident, and depends on the past history and habits of our own minds. There is no more generally acknowledged fact in human nature, than the extreme difficulty at first felt in conceiving any thing as possible, which is in contradiction to long established and familiar experience; or even to old familiar habits of thought. And this difficulty is a necessary result of the fundamental laws of the human mind. When we have often seen and thought of two things together, and have never in any one instance either seen or thought of them separately, there is by the primary law of association an increasing difficulty, which may in the end become insuperable, of conceiving the two things apart. This is most of all conspicuous in uneducated persons, who are in general utterly unable to separate any two ideas which have once become firmly associated in their minds; and if persons of cultivated intellect have any advantage on the point, it is only because, having seen and heard and read more, and being more accustomed to exercise their imagination, they have experienced their sensations and thoughts in more varied combinations, and have been prevented from forming many of these inseparable associations. But this advantage has necessarily its limits. The most practiced intellect is not exempt from the universal laws of our conceptive faculty. If daily habit presents to any one for a long period two facts in combination, and if he is not led during that period either by accident or by his voluntary mental operations to think of them apart, he will probably in time become incapable of doing so even by the strongest effort; and the supposition that the two facts can be separated in nature, will at last present itself to his mind with all the characters of an inconceivable phenomenon.* There are remarkable instances of this in the history of science: instances in which the most instructed men rejected as impossible, because inconceivable, things which their posterity, by earlier practice and longer perseverance in the attempt, found it quite easy to conceive, and which every body now knows to be true. There was a time when men of the most cultivated intellects, and the most emancipated from the dominion of early prejudice, could not credit the existence of antipodes; were unable to conceive, in opposition to old association, the force of gravity acting upward instead of downward. The Cartesians long rejected the Newtonian doctrine of the gravitation of all bodies toward one another, on the faith of a general proposition, the reverse of which seemed to them to be inconceivable—the proposition that a body can not act where it is not. All the cumbrous machinery of imaginary vortices, assumed without the smallest particle of evidence, appeared to these philosophers a more rational mode of explaining the heavenly motions, than one which involved what seemed to them so great an absurdity.†

* "If all mankind had spoken one language, we can not doubt that there would have been a powerful, perhaps a universal, school of philosophers, who would have believed in the inherent connection between names and things, who would have taken the sound *man* to be the mode of agitating the air which is essentially communicative of the ideas of reason, cookery, bipedality, etc."—De Morgan, *Formal Logic*, p. 246.

† It would be difficult to name a man more remarkable at once for the greatness and the wide range of his mental accomplishments, than Leibnitz. Yet this eminent man gave as a reason for rejecting Newton's scheme of the solar system, that God *could not* make a body revolve round a distant centre, unless either by some impelling mechanism, or by miracle: "Tout ce qui n'est pas explicable," says he in a letter to the Abbé Conti, "par la nature des créatures, est miraculeux. Il ne suffit pas de dire: Dieu a fait une telle loi de nature; donc la chose est naturelle. Il faut que la loi soit exécutable par les natures des créatures. Si Dieu donnait cette loi, par exemple, à un corps libre, de tourner à l'entour d'un certain centre, *il faudrait ou qu'il y joignît d'autres corps qui par leur impulsion l'obligeassent de rester toujours dans son orbite circulaire, ou qu'il mît un ange à ses trousses, ou enfin il faudrait qu'il y concourût extraordinairement;* car naturellement il s'écartera par la tangente."—*Works of Leibnitz*, ed. Dutens, iii., 446.

And they no doubt found it as impossible to conceive that a body should act upon the earth from the distance of the sun or moon, as we find it to conceive an end to space or time, or two straight lines inclosing a space. Newton himself had not been able to realize the conception, or we should not have had his hypothesis of a subtle ether, the occult cause of gravitation; and his writings prove, that though he deemed the particular nature of the intermediate agency a matter of conjecture, the necessity of *some* such agency appeared to him indubitable.

If, then, it be so natural to the human mind, even in a high state of culture, to be incapable of conceiving, and on that ground to believe impossible, what is afterward not only found to be conceivable but proved to be true; what wonder if in cases where the association is still older, more confirmed, and more familiar, and in which nothing ever occurs to shake our conviction, or even suggest to us any conception at variance with the association, the acquired incapacity should continue, and be mistaken for a natural incapacity? It is true, our experience of the varieties in nature enables us, within certain limits, to conceive other varieties analogous to them. We can conceive the sun or moon falling; for though we never saw them fall, nor ever, perhaps, imagined them falling, we have seen so many other things fall, that we have innumerable familiar analogies to assist the conception; which, after all, we should probably have some difficulty in framing, were we not well accustomed to see the sun and moon move (or appear to move), so that we are only called upon to conceive a slight change in the direction of motion, a circumstance familiar to our experience. But when experience affords no model on which to shape the new conception, how is it possible for us to form it? How, for example, can we imagine an end to space or time? We never saw any object without something beyond it, nor experienced any feeling without something following it. When, therefore, we attempt to conceive the last point of space, we have the idea irresistibly raised of other points beyond it. When we try to imagine the last instant of time, we can not help conceiving another instant after it. Nor is there any necessity to assume, as is done by a modern school of metaphysicians, a peculiar fundamental law of the mind to account for the feeling of infinity inherent in our conceptions of space and time; that apparent infinity is sufficiently accounted for by simpler and universally acknowledged laws.

Now, in the case of a geometrical axiom, such, for example, as that two straight lines can not inclose a space—a truth which is testified to us by our very earliest impressions of the external world—how is it possible (whether those external impressions be or be not the ground of our belief) that the reverse of the proposition *could* be otherwise than inconceivable to us? What analogy have we, what similar order of facts in any other branch of our experience, to facilitate to us the conception of two straight lines inclosing a space? Nor is even this all. I have already called attention to the peculiar property of our impressions of form, that the ideas or mental images exactly resemble their prototypes, and adequately represent them for the purposes of scientific observation. From this, and from the intuitive character of the observation, which in this case reduces itself to simple inspection, we can not so much as call up in our imagination two straight lines, in order to attempt to conceive them inclosing a space, without by that very act repeating the scientific experiment which establishes the contrary. Will it really be contended that the inconceivableness of the thing, in such circumstances, proves any thing against the experimental origin of the conviction? Is it not clear that in whichever mode our belief in the proposition may have originated, the impossibility of our conceiving the negative of it must, on either hypothesis, be the same? As, then, Dr. Whewell exhorts those who have any difficulty in recognizing the distinction held by him between necessary and contingent truths, to study geometry—a condition which I can assure him I have conscientiously fulfilled—I, in return, with equal confidence, exhort those who agree with him, to study the general laws of association; being convinced that nothing more is requisite than a moderate familiarity with those laws, to dispel the illusion which ascribes a peculiar necessity to our earliest inductions from experience, and measures the possibility of things in themselves, by the human capacity of conceiving them.

I hope to be pardoned for adding, that Dr. Whewell himself has both confirmed by his testimony the effect of habitual association in giving to an experimental truth the appearance of a necessary one, and afforded a striking instance of that remarkable law in his own person. In his *Philosophy of the Inductive Sciences* he continually asserts, that propositions which not only are not self-evident, but which we know to have been discovered gradually, and by great efforts of genius and patience, have, when once established, appeared so self-evident that, but for historical proof, it would have been impossible to conceive that they had not been recognized from the first by all persons in a sound state of their faculties. "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 colored 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 can not 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. 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..... 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.*"*

This last proposition is precisely what I contend for; and I ask no more, in order to overthrow the whole theory of its author on the nature of the

* *Novum Organum Renovatum*, pp. 32, 33.

evidence of axioms. For what is that theory? That the truth of axioms can not have been learned from experience, because their falsity is inconceivable. But Dr. Whewell himself says, that we are continually led, by the natural progress of thought, to regard as inconceivable what our forefathers not only conceived but believed, nay even (he might have added) were unable to conceive the reverse of. He can not intend to justify this mode of thought: he can not mean to say, that we can be right in regarding as inconceivable what others have conceived, and as self-evident what to others did not appear evident at all. After so complete an admission that inconceivableness is an accidental thing, not inherent in the phenomenon itself, but dependent on the mental history of the person who tries to conceive it, how can he ever call upon us to reject a proposition as impossible on no other ground than its inconceivableness? Yet he not only does so, but has unintentionally afforded some of the most remarkable examples which can be cited of the very illusion which he has himself so clearly pointed out. I select as specimens, his remarks on the evidence of the three laws of motion, and of the atomic theory.

With respect to the laws of motion, Dr. Whewell says: "No one can doubt that, in historical fact, these laws were collected from experience. That such is the case, is no matter of conjecture. We know the time, the persons, the circumstances, belonging to each step of each discovery."* After this testimony, to adduce evidence of the fact would be superfluous. And not only were these laws by no means intuitively evident, but some of them were originally paradoxes. The first law was especially so. That a body, once in motion, would continue forever to move in the same direction with undiminished velocity unless acted upon by some new force, was a proposition which mankind found for a long time the greatest difficulty in crediting. It stood opposed to apparent experience of the most familiar kind, which taught that it was the nature of motion to abate gradually, and at last terminate of itself. Yet when once the contrary doctrine was firmly established, mathematicians, as Dr. Whewell observes, speedily began to believe that laws, thus contradictory to first appearances, and which, even after full proof had been obtained, it had required generations to render familiar to the minds of the scientific world, were under "a demonstrable necessity, compelling them to be such as they are and no other;" and he himself, though not venturing "absolutely to pronounce" that *all* these laws "can be rigorously traced to an absolute necessity in the nature of things,"† does actually so think of the law just mentioned; of which he says: "Though the discovery of the first law of motion was made, historically speaking, by means of experiment, we have now attained a point of view in which we see that it might have been certainly known to be true, independently of experience."‡ Can there be a more striking exemplification than is here afforded, of the effect of association which we have described? Philosophers, for generations, have the most extraordinary difficulty in putting certain ideas together; they at last succeed in doing so; and after a sufficient repetition of the process, they first fancy a natural bond between the ideas, then experience a growing difficulty, which at last, by the continuation of the same progress, becomes an impossibility, of severing them from one another. If such be the progress of an experimental conviction of which the date is of yesterday, and which is in opposition to first appearances, how must it fare with those which are conformable to

* *History of Scientific Ideas*, i., 264. † Ibid., i., 263. ‡ Ibid., 240.

appearances familiar from the first dawn of intelligence, and of the conclusiveness of which, from the earliest records of human thought, no skeptic has suggested even a momentary doubt?

The other instance which I shall quote is a truly astonishing one, and may be called the *reductio ad absurdum* of the theory of inconceivableness. Speaking of the laws of chemical composition, Dr. Whewell says:* "That they could never have been clearly understood, and therefore never firmly established, without laborious and exact experiments, is certain; but yet we may venture to say, that being once known, they possess an evidence beyond that of mere experiment. *For how in fact can we conceive combinations, otherwise than as definite in kind and quality?* If we were to suppose each element ready to combine with any other indifferently, and indifferently in any quantity, we should have a world in which all would be confusion and indefiniteness. There would be no fixed kinds of bodies. Salts, and stones, and ores, would approach to and graduate into each other by insensible degrees. Instead of this, we know that the world consists of bodies distinguishable from each other by definite differences, capable of being classified and named, and of having general propositions asserted concerning them. And as *we can not conceive a world in which this should not be the case*, it would appear that we can not conceive a state of things in which the laws of the combination of elements should not be of that definite and measured kind which we have above asserted."

That a philosopher of Dr. Whewell's eminence should gravely assert that we can not conceive a world in which the simple elements should combine in other than definite proportions; that by dint of meditating on a scientific truth, the original discoverer of which was still living, he should have rendered the association in his own mind between the idea of combination and that of constant proportions so familiar and intimate as to be unable to conceive the one fact without the other; is so signal an instance of the mental law for which I am contending, that one word more in illustration must be superfluous.

In the latest and most complete elaboration of his metaphysical system (the *Philosophy of Discovery*), as well as in the earlier discourse on the *Fundamental Antithesis of Philosophy*, reprinted as an appendix to that work, Dr. Whewell, while very candidly admitting that his language was open to misconception, disclaims having intended to say that mankind in general can *now* perceive the law of definite proportions in chemical combination to be a necessary truth. All he meant was that philosophical chemists in a future generation may possibly see this. "Some truths may be seen by intuition, but yet the intuition of them may be a rare and a difficult attainment."† And he explains that the inconceivableness which, according to his theory, is the test of axioms, "depends entirely upon the clearness of the Ideas which the axioms involve. So long as those ideas are vague and indistinct, the contrary of an axiom may be assented to, though it can not be distinctly conceived. It may be assented to, not because it is possible, but because we do not see clearly what is possible. To a person who is only beginning to think geometrically, there may appear nothing absurd in the assertion that two straight lines may inclose a space. And in the same manner, to a person who is only beginning to think of mechanical truths, it may not appear to be absurd, that in mechanical processes, Reaction should be greater or less than Action; and so, again, to a person who has not thought steadily about Substance, it may not appear inconceivable, that by chemical operations, we should generate new matter, or destroy matter which already exists."* Necessary truths, therefore, are not those of which we can not conceive, but "those of which we can not *distinctly* conceive, the contrary."† So long as our ideas are indistinct altogether, we do not know what is or is not capable of being distinctly conceived; but, by the ever increasing distinctness with which scientific men apprehend the general conceptions of science, they in time come to perceive that there are certain laws of nature, which, though historically and as a matter of fact they were learned from experience, we can not, now that we know them, distinctly conceive to be other than they are.

* *Hist. Scientific Ideas*, ii., 25, 26. † *Phil. of Disc.*, p. 339.

The account which I should give of this progress of the scientific mind is somewhat different. After a general law of nature has been ascertained, men's minds do not at first acquire a complete facility of familiarly representing to themselves the phenomena of nature in the character which that law assigns to them. The habit which constitutes the scientific cast of mind, that of conceiving facts of all descriptions conformably to the laws which regulate them—phenomena of all descriptions according to the relations which have been ascertained really to exist between them; this habit, in the case of newly-discovered relations, comes only by degrees. So long as it is not thoroughly formed, no necessary character is ascribed to the new truth. But in time, the philosopher attains a state of mind in which his mental picture of nature spontaneously represents to him all the phenomena with which the new theory is concerned, in the exact light in which the theory regards them: all images or conceptions derived from any other theory, or from the confused view of the facts which is anterior to any theory, having entirely disappeared from his mind. The mode of representing facts which results from the theory, has now become, to his faculties, the only natural mode of conceiving them. It is a known truth, that a prolonged habit of arranging phenomena in certain groups, and explaining them by means of certain principles, makes any other arrangement or explanation of these facts be felt as unnatural: and it may at last become as difficult to him to represent the facts to himself in any other mode, as it often was, originally, to represent them in that mode.

But, further (if the theory is true, as we are supposing it to be), any other mode in which he tries, or in which he was formerly accustomed, to represent the phenomena, will be seen by him to be inconsistent with the facts that suggested the new theory—facts which now form a part of his mental picture of nature. And since a contradiction is always inconceivable, his imagination rejects these false theories, and declares itself incapable of conceiving them. Their inconceivableness to him does not, however, result from any thing in the theories themselves, intrinsically and *a priori* repugnant to the human faculties; it results from the repugnance between them and a portion of the facts; which facts as long as he did not know, or did not distinctly realize in his mental representations, the false theory did not appear other than conceivable; it becomes inconceivable, merely from the fact that contradictory elements can not be combined in the same conception. Although, then, his real reason for rejecting theories at variance with the true one, is no other than that they clash with his experience, he easily falls into the belief, that he rejects them because they are inconceivable, and that he adopts the true theory because it is self-evident, and does not need the evidence of experience at all.

* *Phil. of Disc.*, p. 338. † Ibid., p. 463.

This I take to be the real and sufficient explanation of the paradoxical truth, on which so much stress is laid by Dr. Whewell, that a scientifically cultivated mind is actually, in virtue of that cultivation, unable to conceive suppositions which a common man conceives without the smallest difficulty. For there is nothing inconceivable in the suppositions themselves; the impossibility is in combining them with facts inconsistent with them, as part of the same mental picture; an obstacle of course only felt by those who know the facts, and are able to perceive the inconsistency. As far as the suppositions themselves are concerned, in the case of many of Dr. Whewell's necessary truths the negative of the axiom is, and probably will be as long as the human race lasts, as easily conceivable as the affirmative. There is no axiom (for example) to which Dr. Whewell ascribes a more thorough character of necessity and self-evidence, than that of the indestructibility of matter. That this is a true law of nature I fully admit; but I imagine there is no human being to whom the opposite supposition is inconceivable—who has any difficulty in imagining a portion of matter annihilated: inasmuch as its apparent annihilation, in no respect distinguishable from real by our unassisted senses, takes place every time that water dries up, or fuel is consumed. Again, the law that bodies combine chemically in definite proportions is undeniably true; but few besides Dr. Whewell have reached the point which he seems personally to have arrived at (though he only dares prophesy similar success to the multitude after the lapse of generations), that of being unable to conceive a world in which the elements are ready to combine with one another "indifferently in any quantity;" nor is it likely that we shall ever rise to this sublime height of inability, so long as all the mechanical mixtures in our planet, whether solid, liquid, or aëriform, exhibit to our daily observation the very phenomenon declared to be inconceivable.

According to Dr. Whewell, these and similar laws of nature can not be drawn from experience, inasmuch as they are, on the contrary, assumed in the interpretation of experience. Our inability to "add to or diminish the quantity of matter in the world," is a truth which "neither is nor can be derived from experience; for the experiments which we make to verify it presuppose its truth. . . . . When men began to use the balance in chemical analysis, they did not prove by trial, but took for granted, as self-evident, that the weight of the whole must be found in the aggregate weight of the elements."* True, it is assumed; but, I apprehend, no otherwise than as all experimental inquiry assumes provisionally some theory or hypothesis, which is to be finally held true or not, according as the experiments decide. The hypothesis chosen for this purpose will naturally be one which groups together some considerable number of facts already known. The proposition that the material of the world, as estimated by weight, is neither increased nor diminished by any of the processes of nature or art, had many appearances in its favor to begin with. It expressed truly a great number of familiar facts. There were other facts which it had the appearance of conflicting with, and which made its truth, as a universal law of nature, at first doubtful. Because it was doubtful, experiments were devised to verify it. Men assumed its truth hypothetically, and proceeded to try whether, on more careful examination, the phenomena which apparently pointed to a different conclusion, would not be found to be consistent with it. This turned out to be the case; and from that time

* *Phil. of Disc.*, pp. 472, 473.

the doctrine took its place as a universal truth, but as one proved to be such by experience. That the theory itself preceded the proof of its truth—that it had to be conceived before it could be proved, and in order that it might be proved—does not imply that it was self-evident, and did not need proof. Otherwise all the true theories in the sciences are necessary and self-evident; for no one knows better than Dr. Whewell that they all began by being assumed, for the purpose of connecting them by deductions with those facts of experience on which, as evidence, they now confessedly rest.*

* The *Quarterly Review* for June, 1841, contained an article of great ability on Dr. Whewell's two great works (since acknowledged and reprinted in Sir John Herschel's Essays) which maintains, on the subject of axioms, the doctrine advanced in the text, that they are generalizations from experience, and supports that opinion by a line of argument strikingly coinciding with mine. When I state that the whole of the present chapter (except the last four pages, added in the fifth edition) was written before I had seen the article (the greater part, indeed, before it was published), it is not my object to occupy the reader's attention with a matter so unimportant as the degree of originality which may or may not belong to any portion of my own speculations, but to obtain for an opinion which is opposed to reigning doctrines, the recommendation derived from a striking concurrence of sentiment between two inquirers entirely independent of one another. I embrace the opportunity of citing from a writer of the extensive acquirements in physical and metaphysical knowledge and the capacity of systematic thought which the article evinces, passages so remarkably in unison with my own views as the following:

"The truths of geometry are summed up and embodied in its definitions and axioms....... Let us turn to the axioms, and what do we find? A string of propositions concerning magnitude in the abstract, which are equally true of space, time, force, number, and every other magnitude susceptible of aggregation and subdivision. Such propositions, where they are not mere definitions, as some of them are, carry their inductive origin on the face of their enunciation....... Those which declare that two straight lines can not inclose a space, and that two straight lines which cut one another can not both be parallel to a third, are in reality the only ones which express characteristic properties of space, and these it will be worth while to consider more nearly. Now the only clear notion we can form of straightness is uniformity of direction, for space in its ultimate analysis is nothing but an assemblage of distances and directions. And (not to dwell on the notion of continued contemplation, *i. e.*, mental experience, as included in the very idea of uniformity; nor on that of transfer of the contemplating being from point to point, and of experience, during such transfer, of the homogeneity of the interval passed over) we can not even propose the proposition in an intelligible form to any one whose experience ever since he was born has not assured him of the fact. The unity of direction, or that we can not march from a given point by more than one path direct to the same object, is matter of practical experience long before it can by possibility become matter of abstract thought. *We can not attempt mentally to exemplify the conditions of the assertion in an imaginary case opposed to it, without violating our habitual recollection of this experience, and defacing our mental picture of space as grounded on it.* What but experience, we may ask, can possibly assure us of the homogeneity of the parts of distance, time, force, and measurable aggregates in general, on which the truth of the other axioms depends? As regards the latter axiom, after what has been said it must be clear that the very same course of remarks equally applies to its case, and that its truth is quite as much forced on the mind as that of the former by daily and hourly experience,...... *including always, be it observed, in our notion of experience, that which is gained by contemplation of the inward picture which the mind forms to itself in any proposed case, or which it arbitrarily selects as an example—such picture, in virtue of the extreme simplicity of these primary relations, being called up by the imagination with as much vividness and clearness as could be done by any external impression, which is the only meaning we can attach to the word intuition, as applied to such relations.*"

And again, of the axioms of mechanics: "As we admit no such propositions, other than as truths inductively collected from observation, even in geometry itself, it can hardly be expected that, in a science of obviously contingent relations, we should acquiesce in a contrary view. Let us take one of these axioms and examine its evidence: for instance, that equal forces perpendicularly applied at the opposite ends of equal arms of a straight lever will balance each other. What but experience, we may ask, in the first place, can possibly inform us that a force so applied will have any tendency to turn the lever on its centre at all? or that force can be so transmitted along a rigid line perpendicular to its direction, as to act elsewhere in space than along its own line of action? Surely this is so far from being self-evident that it has even a paradoxical appearance, which is only to be removed by giving our lever thickness, material composition, and molecular powers. Again, we conclude, that the two forces, being equal and applied under precisely similar circumstances, must, if they exert any effort at all to turn the lever, exert equal and opposite efforts: but what *a priori* reasoning can possibly assure us that they *do* act under precisely similar circumstances? that points which differ in place *are* similarly circumstanced as regards the exertion of force? that universal space may not have relations to universal force—or, at all events, that the organization of the material universe may not be such as to place that portion of space occupied by it in such relations to the forces exerted in it, as may invalidate the absolute similarity of circumstances assumed? Or we may argue, what have we to do with the notion of angular movement in the lever at all? The case is one of rest, and of quiescent destruction of force by force. Now how is this destruction effected? Assuredly by the counter-pressure which supports the fulcrum. But would not this destruction equally arise, and by the same amount of counteracting force, if each force simply pressed its own half of the lever against the fulcrum? And what can assure us that it is not so, except removal of one or other force, and consequent tilting of the lever? The other fundamental axiom of statics, that the pressure on the point of support is the sum of the weights...... is merely a scientific transformation and more refined mode of stating a coarse and obvious result of universal experience, viz., that the weight of a rigid body is the same, handle it or suspend it in what position or by what point we will, and that whatever sustains it sustains its total weight. Assuredly, as Mr. Whewell justly remarks, 'No one probably ever made a trial for the purpose of showing that the pressure on the support is equal to the sum of the weights.'...... But it is precisely because in every action of his life from earliest infancy he has been continually making the trial, and seeing it made by every other living being about him, that he never dreams of staking its result on one additional attempt made with scientific accuracy. This would be as if a man should resolve to decide by experiment whether his eyes were useful for the purpose of seeing, by hermetically sealing himself up for half an hour in a metal case."

On the "paradox of universal propositions obtained by experience," the same writer says: "If there be necessary and universal truths expressible in propositions of axiomatic simplicity and obviousness, and having for their subject-matter the elements of all our experience and all our knowledge, surely these are the truths which, if experience suggest to us any truths at all, it ought to suggest most readily, clearly, and unceasingly. If it were a truth, universal and necessary, that a net is spread over the whole surface of every planetary globe, we should not travel far on our own without getting entangled in its meshes, and making the necessity of some means of extrication an axiom of locomotion....... There is, therefore, nothing paradoxical, but the reverse, in our being led by observation to a recognition of such truths, as *general* propositions, co-extensive at least with all human experience. That they pervade all the objects of experience, must insure their continual suggestion *by* experience; that they are true, must insure that consistency of suggestion, that iteration of uncontradicted assertion, which commands implicit assent, and removes all occasion of exception; that they are simple, and admit of no misunderstanding, must secure their admission by every mind."

"A truth, necessary and universal, relative to any object of our knowledge, must verify itself in every instance where that object is before our contemplation, and if at the same time it be simple and intelligible, its verification must be obvious. *The sentiment of such a truth can not, therefore, but be present to our minds whenever that object is contemplated, and must therefore make a part of the mental picture or idea of that object which we may on any occasion summon before our imagination*....... *All propositions, therefore, become not only untrue but inconceivable*, if...... axioms be violated in their enunciation."

Another eminent mathematician had previously sanctioned by his authority the doctrine of the origin of geometrical axioms in experience. "Geometry is thus founded likewise on observation; but of a kind so familiar and obvious, that the primary notions which it furnishes might seem intuitive."—*Sir John Leslie*, quoted by Sir William Hamilton, *Discourses*, etc., p. 272.

CHAPTER VI.

THE SAME SUBJECT CONTINUED.

§ 1. In the examination which formed the subject of the last chapter, into the nature of the evidence of those deductive sciences which are commonly represented to be systems of necessary truth, we have been led to the following conclusions. The results of those sciences are indeed necessary, in the sense of necessarily following from certain first principles, commonly called axioms and definitions; that is, of being certainly true if those axioms and definitions are so; for the word necessity, even in this acceptation of it, means no more than certainty. But their claim to the character of necessity in any sense beyond this, as implying an evidence independent of and superior to observation and experience, must depend on the previous establishment of such a claim in favor of the definitions and axioms themselves. With regard to axioms, we found that, considered as experimental truths, they rest on superabundant and obvious evidence. We inquired, whether, since this is the case, it be imperative to suppose any other evidence of those truths than experimental evidence, any other origin for our belief of them than an experimental origin. We decided, that the burden of proof lies with those who maintain the affirmative, and we examined, at considerable length, such arguments as they have produced. The examination having led to the rejection of those arguments, we have thought ourselves warranted in concluding that axioms are but a class, the most universal class, of inductions from experience; the simplest and easiest cases of generalization from the facts furnished to us by our senses or by our internal consciousness.

While the axioms of demonstrative sciences thus appeared to be experimental truths, the definitions, as they are incorrectly called, in those sciences, were found by us to be generalizations from experience which are not even, accurately speaking, truths; being propositions in which, while we assert of some kind of object, some property or properties which observation shows to belong to it, we at the same time deny that it possesses any other properties, though in truth other properties do in every individual instance accompany, and in almost all instances modify, the property thus exclusively predicated. The denial, therefore, is a mere fiction, or supposition, made for the purpose of excluding the consideration of those modifying circumstances, when their influence is of too trifling amount to be worth considering, or adjourning it, when important to a more convenient moment.

From these considerations it would appear that Deductive or Demonstrative Sciences are all, without exception, Inductive Sciences; that their evidence is that of experience; but that they are also, in virtue of the peculiar character of one indispensable portion of the general formulæ according to which their inductions are made, Hypothetical Sciences. Their conclusions are only true on certain suppositions, which are, or ought to be, approximations to the truth, but are seldom, if ever, exactly true; and to this hypothetical character is to be ascribed the peculiar certainty, which is supposed to be inherent in demonstration.

What we have now asserted, however, can not be received as universally true of Deductive or Demonstrative Sciences, until verified by being applied to the most remarkable of all those sciences, that of Numbers; the theory of the Calculus; Arithmetic and Algebra. It is harder to believe of the doctrines of this science than of any other, either that they are not truths *a priori*, but experimental truths, or that their peculiar certainty is owing to their being not absolute but only conditional truths. This, therefore, is a case which merits examination apart; and the more so, because on this subject we have a double set of doctrines to contend with; that of the *a priori* philosophers on one side; and on the other, a theory the most opposite to theirs, which was at one time very generally received, and is still far from being altogether exploded, among metaphysicians.

§ 2. This theory attempts to solve the difficulty apparently inherent in the case, by representing the propositions of the science of numbers as merely verbal, and its processes as simple transformations of language, substitutions of one expression for another. The proposition, Two and one is equal to three, according to these writers, is not a truth, is not the assertion of a really existing fact, but a definition of the word three; a statement that mankind have agreed to use the name three as a sign exactly equivalent to two and one; to call by the former name whatever is called by the other more clumsy phrase. According to this doctrine, the longest process in algebra is but a succession of changes in terminology, by which equivalent expressions are substituted one for another; a series of translations of the same fact, from one into another language; though how, after such a series of translations, the fact itself comes out changed (as when we demonstrate a new geometrical theorem by algebra), they have not explained; and it is a difficulty which is fatal to their theory.

It must be acknowledged that there are peculiarities in the processes of arithmetic and algebra which render the theory in question very plausible, and have not unnaturally made those sciences the stronghold of Nominalism. The doctrine that we can discover facts, detect the hidden processes of nature, by an artful manipulation of language, is so contrary to common sense, that a person must have made some advances in philosophy to believe it: men fly to so paradoxical a belief to avoid, as they think, some even greater difficulty, which the vulgar do not see. What has led many to believe that reasoning is a mere verbal process, is, that no other theory seemed reconcilable with the nature of the Science of Numbers. For we do not carry any ideas along with us when we use the symbols of arithmetic or of algebra. In a geometrical demonstration we have a mental diagram, if not one on paper; AB, AC, are present to our imagination as lines, intersecting other lines, forming an angle with one another, and the like; but not so a and b. These may represent lines or any other magnitudes, but those magnitudes are never thought of; nothing is realized in our imagination but a and b. The ideas which, on the particular occasion, they happen to represent, are banished from the mind during every intermediate part of the process, between the beginning, when the premises are translated from things into signs, and the end, when the conclusion is translated back from signs into things. Nothing, then, being in the reasoner's mind but the symbols, what can seem more inadmissible than to contend that the reasoning process has to do with any thing more? We seem to have come to one of Bacon's Prerogative Instances; an *experimentum crucis* on the nature of reasoning itself.

Nevertheless, it will appear on consideration, that this apparently so decisive instance is no instance at all; that there is in every step of an arithmetical or algebraical calculation a real induction, a real inference of facts from facts; and that what disguises the induction is simply its comprehensive nature, and the consequent extreme generality of the language. All numbers must be numbers of something: there are no such things as numbers in the abstract. *Ten* must mean ten bodies, or ten sounds, or ten beatings of the pulse. But though numbers must be numbers of something, they may be numbers of any thing. Propositions, therefore, concerning numbers, have the remarkable peculiarity that they are propositions concerning all things whatever; all objects, all existences of every kind, known to our experience. All things possess quantity; consist of parts which can be numbered; and in that character possess all the properties which are called properties of numbers. That half of four is two, must be true whatever the word four represents, whether four hours, four miles, or four pounds weight. We need only conceive a thing divided into four equal parts (and all things may be conceived as so divided), to be able to predicate of it every property of the number four, that is, every arithmetical proposition in which the number four stands on one side of the equation. Algebra extends the generalization still farther: every number represents that particular number of all things without distinction, but every algebraical symbol does more, it represents all numbers without distinction. As soon as we conceive a thing divided into equal parts, without knowing into what number of parts, we may call it a or x, and apply to it, without danger of error, every algebraical formula in the books. The proposition, $2\,(a+b) = 2\,a + 2\,b$, is a truth co-extensive with all nature. Since then algebraical truths are true of all things whatever, and not, like those of geometry, true of lines only or of angles only, it is no wonder that the symbols should not excite in our minds ideas of any things in particular. When we demonstrate the forty-seventh proposition of Euclid, it is not necessary that the words should raise in us an image of all right-angled triangles, but only of some one right-angled triangle: so in algebra we need not, under the symbol a, picture to ourselves all things whatever, but only some one thing; why not, then, the letter itself? The mere written characters, a, b, x, y, z, serve as well for representatives of Things in general, as any more complex and apparently more concrete conception. That we are conscious of them, however, in their character of things, and not of mere signs, is evident from the fact that our whole process of reasoning is carried on by predicating of them the properties of things. In resolving an algebraic equation, by what rules do we proceed? By applying at each step to a, b, and x, the proposition that equals added to equals make equals; that equals taken from equals leave equals; and other propositions founded on these two. These are not properties of language, or of signs as such, but of magnitudes, which is as much as to say, of all things. The inferences, therefore, which are successively drawn, are inferences concerning things, not symbols; though as any Things whatever will serve the turn, there is no necessity for keeping the idea of the Thing at all distinct, and consequently the process of thought may, in this case, be allowed without danger to do what all processes of thought, when they have been performed often, will do if permitted, namely, to become entirely mechanical. Hence the general language of algebra comes to be used familiarly without exciting ideas, as all other general language is prone to do from mere habit, though in no other case than this can it be done with complete safety.

But when we look back to see from whence the probative force of the process is derived, we find that at every single step, unless we suppose ourselves to be thinking and talking of the things, and not the mere symbols, the evidence fails.

There is another circumstance, which, still more than that which we have now mentioned, gives plausibility to the notion that the propositions of arithmetic and algebra are merely verbal. That is, that when considered as propositions respecting Things, they all have the appearance of being identical propositions. The assertion, Two and one is equal to three, considered as an assertion respecting objects, as for instance, "Two pebbles and one pebble are equal to three pebbles," does not affirm equality between two collections of pebbles, but absolute identity. It affirms that if we put one pebble to two pebbles, those very pebbles are three. The objects, therefore, being the very same, and the mere assertion that "objects are themselves" being insignificant, it seems but natural to consider the proposition, Two and one is equal to three, as asserting mere identity of signification between the two names.

This, however, though it looks so plausible, will not bear examination. The expression "two pebbles and one pebble," and the expression "three pebbles," stand indeed for the same aggregation of objects, but they by no means stand for the same physical fact. They are names of the same objects, but of those objects in two different states: though they *de*note the same things, their *con*notation is different. Three pebbles in two separate parcels, and three pebbles in one parcel, do not make the same impression on our senses; and the assertion that the very same pebbles may by an alteration of place and arrangement be made to produce either the one set of sensations or the other, though a very familiar proposition, is not an identical one. It is a truth known to us by early and constant experience: an inductive truth; and such truths are the foundation of the science of Number. The fundamental truths of that science all rest on the evidence of sense; they are proved by showing to our eyes and our fingers that any given number of objects—ten balls, for example—may by separation and re-arrangement exhibit to our senses all the different sets of numbers the sums of which is equal to ten. All the improved methods of teaching arithmetic to children proceed on a knowledge of this fact. All who wish to carry the child's *mind* along with them in learning arithmetic; all who wish to teach numbers, and not mere ciphers—now teach it through the evidence of the senses, in the manner we have described.

We may, if we please, call the proposition, "Three is two and one," a definition of the number three, and assert that arithmetic, as it has been asserted that geometry, is a science founded on definitions. But they are definitions in the geometrical sense, not the logical; asserting not the meaning of a term only, but along with it an observed matter of fact. The proposition, "A circle is a figure bounded by a line which has all its points equally distant from a point within it," is called the definition of a circle; but the proposition from which so many consequences follow, and which is really a first principle in geometry, is, that figures answering to this description exist. And thus we may call "Three is two and one" a definition of three; but the calculations which depend on that proposition do not follow from the definition itself, but from an arithmetical theorem presupposed in it, namely, that collections of objects exist, which while they impress the senses thus, $^{\circ}{}_{\circ}{}^{\circ}$, may be separated into two parts, thus, $\circ\circ\ \circ$. This proposition being granted, we term all such parcels Threes, after which the enunciation of the above-mentioned physical fact will serve also for a definition of the word Three.

The Science of Number is thus no exception to the conclusion we previously arrived at, that the processes even of deductive sciences are altogether inductive, and that their first principles are generalizations from experience. It remains to be examined whether this science resembles geometry in the further circumstance, that some of its inductions are not exactly true; and that the peculiar certainty ascribed to it, on account of which its propositions are called Necessary Truths, is fictitious and hypothetical, being true in no other sense than that those propositions legitimately follow from the hypothesis of the truth of premises which are avowedly mere approximations to truth.

§ 3. The inductions of arithmetic are of two sorts: first, those which we have just expounded, such as One and one are two, Two and one are three, etc., which may be called the definitions of the various numbers, in the improper or geometrical sense of the word Definition; and secondly, the two following axioms: The sums of equals are equal, The differences of equals are equal. These two are sufficient; for the corresponding propositions respecting unequals may be proved from these by a *reductio ad absurdum*.

These axioms, and likewise the so-called definitions, are, as has already been said, results of induction; true of all objects whatever, and, as it may seem, exactly true, without the hypothetical assumption of unqualified truth where an approximation to it is all that exists. The conclusions, therefore, it will naturally be inferred, are exactly true, and the science of number is an exception to other demonstrative sciences in this, that the categorical certainty which is predicable of its demonstrations is independent of all hypothesis.

On more accurate investigation, however, it will be found that, even in this case, there is one hypothetical element in the ratiocination. In all propositions concerning numbers, a condition is implied, without which none of them would be true; and that condition is an assumption which may be false. The condition is, that $1=1$; that all the numbers are numbers of the same or of equal units. Let this be doubtful, and not one of the propositions of arithmetic will hold true. How can we know that one pound and one pound make two pounds, if one of the pounds may be troy, and the other avoirdupois? They may not make two pounds of either, or of any weight. How can we know that a forty-horse power is always equal to itself, unless we assume that all horses are of equal strength? It is certain that 1 is always equal in *number* to 1; and where the mere number of objects, or of the parts of an object, without supposing them to be equivalent in any other respect, is all that is material, the conclusions of arithmetic, so far as they go to that alone, are true without mixture of hypothesis. There are such cases in statistics; as, for instance, an inquiry into the amount of the population of any country. It is indifferent to that inquiry whether they are grown people or children, strong or weak, tall or short; the only thing we want to ascertain is their number. But whenever, from equality or inequality of number, equality or inequality in any other respect is to be inferred, arithmetic carried into such inquiries becomes as hypothetical a science as geometry. All units must be assumed to be equal in that other respect; and this is never accurately true, for one actual pound weight is not exactly equal to another, nor one measured mile's length to another; a nicer balance, or more accurate measuring instruments, would always detect some difference.

What is commonly called mathematical certainty, therefore, which comprises the twofold conception of unconditional truth and perfect accuracy, is not an attribute of all mathematical truths, but of those only which relate to pure Number, as distinguished from Quantity in the more enlarged sense; and only so long as we abstain from supposing that the numbers are a precise index to actual quantities. The certainty usually ascribed to the conclusions of geometry, and even to those of mechanics, is nothing whatever but certainty of inference. We can have full assurance of particular results under particular suppositions, but we can not have the same assurance that these suppositions are accurately true, nor that they include all the data which may exercise an influence over the result in any given instance.

§ 4. It appears, therefore, that the method of all Deductive Sciences is hypothetical. They proceed by tracing the consequences of certain assumptions; leaving for separate consideration whether the assumptions are true or not, and if not exactly true, whether they are a sufficiently near approximation to the truth. The reason is obvious. Since it is only in questions of pure number that the assumptions are exactly true, and even there only so long as no conclusions except purely numerical ones are to be founded on them; it must, in all other cases of deductive investigation, form a part of the inquiry, to determine how much the assumptions want of being exactly true in the case in hand. This is generally a matter of observation, to be repeated in every fresh case; or if it has to be settled by argument instead of observation, may require in every different case different evidence, and present every degree of difficulty, from the lowest to the highest. But the other part of the process—namely, to determine what else may be concluded if we find, and in proportion as we find, the assumptions to be true—may be performed once for all, and the results held ready to be employed as the occasions turn up for use. We thus do all beforehand that can be so done, and leave the least possible work to be performed when cases arise and press for a decision. This inquiry into the inferences which can be drawn from assumptions, is what properly constitutes Demonstrative Science.

It is of course quite as practicable to arrive at new conclusions from facts assumed, as from facts observed; from fictitious, as from real, inductions. Deduction, as we have seen, consists of a series of inferences in this form—a is a mark of b, b of c, c of d, therefore a is a mark of d, which last may be a truth inaccessible to direct observation. In like manner it is allowable to say, *suppose* that a were a mark of b, b of c, and c of d, a would be a mark of d, which last conclusion was not thought of by those who laid down the premises. A system of propositions as complicated as geometry might be deduced from assumptions which are false; as was done by Ptolemy, Descartes, and others, in their attempts to explain synthetically the phenomena of the solar system on the supposition that the apparent motions of the heavenly bodies were the real motions, or were produced in some way more or less different from the true one. Sometimes the same thing is knowingly done, for the purpose of showing the falsity of the assumption; which is called a *reductio ad absurdum.* In such cases, the reasoning is as follows: a is a mark of b, and b of c; now if c were also a mark of d, a would be a mark of d; but d is known to be a mark of the absence of a; consequently a would be a mark of its own absence, which is a contradiction; therefore c is not a mark of d.

§ 5. It has even been held by some writers, that all ratiocination rests in the last resort on a *reductio ad absurdum;* since the way to enforce assent to it, in case of obscurity, would be to show that if the conclusion be denied we must deny some one at least of the premises, which, as they are all supposed true, would be a contradiction. And in accordance with this, many have thought that the peculiar nature of the evidence of ratiocination consisted in the impossibility of admitting the premises and rejecting the conclusion without a contradiction in terms. This theory, however, is inadmissible as an explanation of the grounds on which ratiocination itself rests. If any one denies the conclusion notwithstanding his admission of the premises, he is not involved in any direct and express contradiction until he is compelled to deny some premise; and he can only be forced to do this by a *reductio ad absurdum*, that is, by another ratiocination: now, if he denies the validity of the reasoning process itself, he can no more be forced to assent to the second syllogism than to the first. In truth, therefore, no one is ever forced to a contradiction in terms: he can only be forced to a contradiction (or rather an infringement) of the fundamental maxim of ratiocination, namely, that whatever has a mark, has what it is a mark of; or (in the case of universal propositions), that whatever is a mark of any thing, is a mark of whatever else that thing is a mark of. For in the case of every correct argument, as soon as thrown into the syllogistic form, it is evident without the aid of any other syllogism, that he who, admitting the premises, fails to draw the conclusion, does not conform to the above axiom.

We have now proceeded as far in the theory of Deduction as we can advance in the present stage of our inquiry. Any further insight into the subject requires that the foundation shall have been laid of the philosophic theory of Induction itself; in which theory that of Deduction, as a mode of Induction, which we have now shown it to be, will assume spontaneously the place which belongs to it, and will receive its share of whatever light may be thrown upon the great intellectual operation of which it forms so important a part.

CHAPTER VII.

EXAMINATION OF SOME OPINIONS OPPOSED TO THE PRECEDING DOCTRINES.

§ 1. Polemical discussion is foreign to the plan of this work. But an opinion which stands in need of much illustration, can often receive it most effectually, and least tediously, in the form of a defense against objections. And on subjects concerning which speculative minds are still divided, a writer does but half his duty by stating his own doctrine, if he does not also examine, and to the best of his ability judge, those of other thinkers.

In the dissertation which Mr. Herbert Spencer has prefixed to his, in many respects, highly philosophical treatise on the Mind,* he criticises some of the doctrines of the two preceding chapters, and propounds a theory of his own on the subject of first principles. Mr. Spencer agrees with me in considering axioms to be "simply our earliest inductions from experience." But he differs from me "widely as to the worth of the test of inconceiva-

* *Principles of Psychology.*