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the 1980s. The 1980s have been a decade of rapid change in the world of work.

There are a number of reasons for this. The most obvious is the rapid pace of technological change. This has led to a demand for a more flexible and adaptable workforce.

Another reason is the increasing emphasis on quality and customer service. This has led to a demand for a more motivated and committed workforce.

Finally, there is the increasing emphasis on diversity and inclusion. This has led to a demand for a more open and accepting workforce.

These changes have led to a new paradigm of work. One that is more flexible, more adaptive, and more focused on quality and customer service.

This new paradigm of work is one that is more in line with the needs of the 21st century. It is one that is more focused on the future.

As we move into the 21st century, we must continue to adapt and evolve. We must continue to embrace change and innovation.

Only then can we ensure that we are prepared for the challenges of the future.

The future is bright. The future is full of opportunity. The future is ours.

Let us embrace the future. Let us embrace change. Let us embrace innovation.

Let us create a better future for ourselves and for the world.

Let us make the future a reality.

Let us build a better world.

Let us create a brighter future.

Let us make the future a reality.

Let us build a better world.

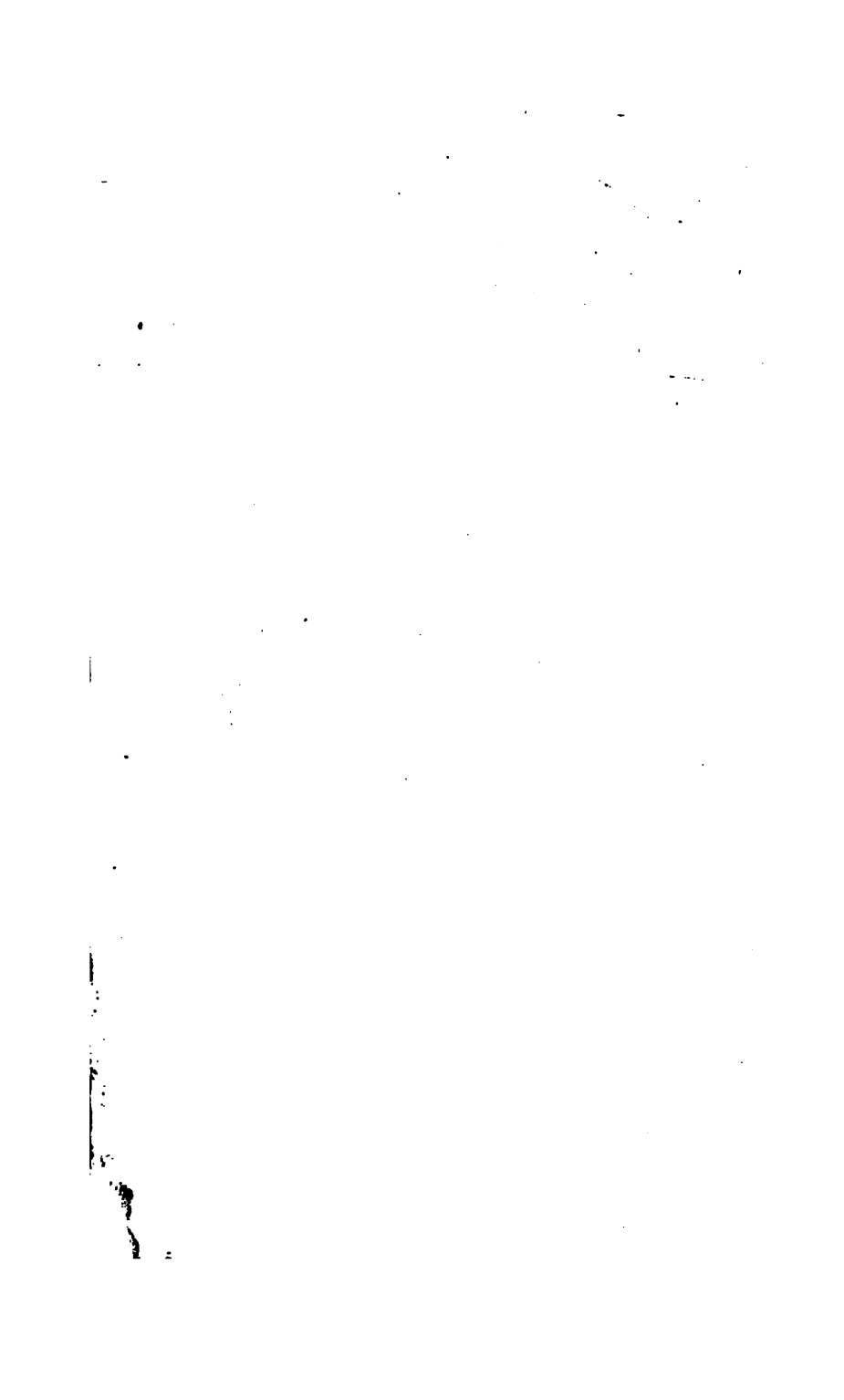
Let us create a brighter future.





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ANALYSIS
OF
MR. MILL'S SYSTEM OF LOGIC.

LONDON: PRINTED BY
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J. C. Stebbing

ANALYSIS

OF

MR. MILL'S SYSTEM OF LOGIC.

BY

W. STEBBING, M.A.

FELLOW OF WORCESTER COLLEGE, OXFORD.

NEW EDITION.

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PREFACE

TO

THE SECOND EDITION.



THE AUTHOR'S aim has been to produce such a condensation of the original work as may recall its contents to those who have read it, and may serve those who are now reading it in the place of a full body of marginal notes. Mr. Mill's conclusions on the true province and method of Logic have a high substantive value, independent even of the arguments and illustrations by which they are supported; and these conclusions may be adequately, and, it is believed, with much practical utility, embodied in an epitome. The processes of reasoning on which they depend, can, on the other hand, be represented in outline only. But it is hoped that the substance of every paragraph, necessary for the due comprehension of the several

steps by which the results have been reached, will be here found at all events suggested.

The author may be allowed to add, that Mr. Mill, before publication, expressed a favourable opinion of the manner in which the work had been executed. Without such commendation the volume would hardly have been offered to the public.

LONDON : *Dec.* 21, 1865.

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ANALYSIS

OF

MILL'S LOGIC.

INTRODUCTION.

No adequate definition is possible till the properties of the thing to be defined are known. Previously we can define only the scope of the inquiry. Now, Logic has been considered as both the science of reasoning, i.e. the analysis of the mental process when we reason, and the art of reasoning, i.e. the rules for the process. The term *reasoning*, however, is not wide enough. *Reasoning* means either syllogising, or (and this is its truer sense) the drawing inferences from assertions already admitted. But the Aristotelian or Scholastic logicians included in Logic terms and propositions, and the Port Royal logicians spoke of it as equivalent to the art of thinking. Even popularly, accuracy of classification, and the extent of command over premisses, are thought clearer signs of logical powers than accuracy of deduction. On the other hand, the definition of logic as a 'science treating of the operations of the understanding in the search of truth,' though wide enough, would err through including truths known from intuition; for, though doubtless many seeming intuitions are processes of inference, questions as to

what facts are *real intuitions* belong to Metaphysics, not to Logic.

Logic is the science, not of Belief, but of Proof, or Evidence. Almost all knowledge being matter of inference, the fields of Logic and of Knowledge coincide; but the two differ in so far that Logic does not find evidence, but only judges of it. All science is composed of data, and conclusions thence: Logic shows what relations must subsist between them. All inferential knowledge is true or not, according as the laws of Logic have been obeyed or not. Logic is Bacon's *Ars Artium*, the science of sciences. Genius sometimes employs laws unconsciously; but only genius: as a rule, the advances of a science have been ever found to be preceded by a fuller knowledge of the laws of Logic applicable to it. Logic, then, may be described as the science of the operations of the understanding which aid in the estimation of evidence. It includes not only the process of proceeding from the known to the unknown, but, as auxiliary thereto, Naming, Definition, and Classification. Conception, Memory, and other like faculties, are not treated by it; but it presupposes them. Our object, therefore, must be to analyse the process of inference and the subsidiary operations, besides framing canons to test any given evidence. We need not, however, carry the analysis beyond what is necessary for the practical uses of Logic; for one step in analysis is good without a second, and our purpose is simply to see the difference between good and ill processes of inference. Minuter analysis befits Metaphysics; though even that science, when stepping beyond the interrogation of our consciousness, or rather of our memory, is, as all other sciences, amenable to Logic.

BOOK I.

NAMES AND PROPOSITIONS.

CHAPTER I.

ON THE NECESSITY OF COMMENCING WITH AN ANALYSIS OF LANGUAGE IN LOGIC.

THE fact of Logic being a portion of the art of thinking, and of thought's chief instrument being words, is one reason why we must first inquire into the right use of words. But further, the import of propositions cannot really be examined apart from that of words; and (since whatever can be an object of belief assumes the form of a proposition, and in propositions all truth and error lie) this is a paramount reason why we must, as a preliminary, consider the import of names, the neglecting which, and confining ourselves to things, would indeed be to discard all past experience. The right method is, to take men's classifications of things as shown by names, correcting them as we proceed.

CHAPTER II.

NAMES.

HOBBS'S assertion that a name is a sign, not of a thing, but of our conception of it, is untrue (unless he merely mean that the conception, and not the thing itself, is imparted to the hearer); for we intend

by a name, not only to make men conceive what we conceive, but to inform them what we believe as to the things themselves.

Names may be divided according to five principles of classification. The *first* way of dividing them is into General (not as equivalent to Collective) and Individual names; the *second*, into Concrete, i.e. the names of objects, and Abstract, i.e. the names of attributes (though Locke improperly extends the term to all names gained by abstraction, that is, to all general names). An abstract name is sometimes general, e.g. colour, and sometimes singular, e.g. milk-whiteness. It may be objected to calling attributes abstract, that also concrete adjectives, e.g. white, are attributes. But a word is the name of the things of which it can be predicated. Hence, white is the name of all things so coloured, given indeed because of the quality, but really the name of the thing, and no more the name of the quality than are names generally, since every one of them, if it signifies anything at all, must imply an attribute.

The *third* division is into Connotative and Non-connotative (the latter being wrongly called Absolute). By *connotative* are meant, not (as Mr. James Mill explains it) words which, pointing directly to one thing, tacitly refer to another, but words which denote a subject and imply an attribute; while *non-connotatives* signify a subject only, or attribute only. All concrete general names are connotative. They are also called *denominative*, because the subject denoted receives a common name (e.g. snow is named white) from the attribute connoted. Even some abstracts are connotative, for attributes may have attri-

butes ascribed to them, and a word which denotes attributes may connote an attribute of them; e.g. fault connotes hurtfulness. Proper names, on the other hand, though concrete, are not connotative. They are merely distinguishing marks, given perhaps originally for a reason, but, when once given, independent of it, since the reason is proved to be no part of the sense of the word by the fact that the name is still used when the reason is forgotten. But other individual names are connotative. Some of these, viz. those connoting some attribute or some set of attributes possessed by one object only, e.g. Sun, God, are really general names, though happening to be predicable only of a single object. But there are also real connotative individual names, part of whose meaning is, that there exists only one individual with the connoted attribute, e.g. The first Emperor, The father of Socrates; and it is so with many-worded names, made up of a general name limited by other words, e.g. The present Prime Minister of England. In short, the meaning of all names, which have any meaning, resides, not in what they denote, but in what they connote. There perpetually, however, arises a difficulty of deciding how much they do connote, that is, what difference in the object would make a difference in the name. This vagueness comes from our learning the connotation, through a rude generalisation and analysis, from the objects denoted. Thus, men use a name without any precise reference to a definite set of attributes, applying it to new objects on account of superficial resemblance, so that at length all common meaning disappears. Even scientific writers, from ignorance, or from the aversion

which men at large feel to the use of new names, often force old terms to express an ever-growing number of distinctions. But every concrete general name should be given a definite connotation with the least possible change in the denotation; and this is what is aimed at in every definition of a general name already in use. But we must not confound the use of names of indeterminate connotation, which is so great an evil, with the employment, necessitated by the paucity of names as compared with the demand, of the same words with different connotations in different relations.

A *fourth* division of names is into Positive and Negative. When the positive is connotative, so is the corresponding negative, for the non-possession of an attribute is itself an attribute. Names negative in form, e.g. unpleasant, are often really positive; and others, e.g. idle, sober, though seemingly positive, are really negative. Privatives are names which are equivalent each to a positive and a negative name taken together. They connote both the absence of certain attributes, and the presence of others, whence the presence of the defaulting ones might have been expected. Thus, blind would be applied only to a non-seeing member of a seeing class.

The *fifth* division is into Relative and (that we may economise the term Absolute for an occasion when none other is available) Non-relative names. Correlatives, when concrete, are of course connotative. A relation arises from two individuals being concerned in the same series of facts, so that the signification of neither name can be explained except by mentioning another: and any two correlatives connote, not the same attribute indeed, but just this series of facts, which is exactly the same in both cases.

Some make a *sixth* division, viz. Univocals, i.e. names predicated of different individuals in the same sense, and Æquivocals, i.e. names predicated of different individuals in different senses. But these are not two kinds of names, but only two modes of using them; for an æquivocal name is two names accidentally coinciding in sound. An intermediate case is that of a name used analogically or metaphorically, that is, in two senses, one its primary, the other its secondary sense. The not perceiving that such a word is really two has produced many fallacies.

CHAPTER III.

THE THINGS DENOTED BY NAMES.

LOGIC is the theory of Proof, and everything provable can be exhibited as a proposition, propositions alone being objects of belief. Therefore, the import of propositions, that is, the import of predication, must be ascertained. But, as to make a proposition, i.e. to predicate, is to assert one *thing* of another *thing*, the way to learn the import of predication is, by discovering what are the *things* signified by names which are capable of being subject or predicate. It was with this object that Aristotle formed his Categories, i.e. an attempted enumeration of all nameable things by the *summa genera* or highest predicates, one or other of which must, he asserted, be predicable of everything. His, however, is a rude catalogue, without philosophical analysis of the rationale even of familiar distinctions. For instance,

his Relation properly includes Action, Passivity, and Local Situation, and also the two categories of Position *ποτέ* and *ποῦ*, while the difference between *ποῦ* and *κεῖσθαι* is only verbal, and *ἔχειν* is not a *summum genus* at all. Besides—only substantives and attributes being there considered—there is no category for sensation and other mental states, since, though these may rightly be placed, so far as they express their relation, if active, to their objects, if passive to their causes, in the Categories of Actio and Passio, the things, viz., the mental states, do not belong there.

The absence of a well-defined concrete name answering to the abstract *existence*, is one great obstacle to renewing Aristotle's attempt. The words used for the purpose commonly denote substances only, though attributes and feelings are equally existences. Even *being* is inadequate, since it denotes only *some* existences, being used by custom as synonymous with *substance*, both material and spiritual. That is, it is applied to what excites feelings and has attributes, but not to feelings and attributes themselves; and if we called extension, virtue, &c., *beings*, we should be accused of believing in the Platonic self-existing ideas, or Epicurus's sensible forms—in short, of deeming attributes substances. To fill this gap, the abstract, *entity*, was made into a concrete, equivalent to *being*. Yet even *entity* implies, though not so much as *being*, the notion of substance. In fact, every word originally connoting simply existence, gradually enlarges its connotation to mean *separate* existence, i.e. existence freed from the condition of belonging to a substance, so as to exclude

attributes and feelings. Since, then, all the terms are ambiguous, that among them (and the same principle applies to terms generally) will be employed here which seems on each occasion to be *least* ambiguous: and terms will be used even in improper senses, when these by familiar association convey the proper meaning.

Nameable things are—I. Feelings or States of Consciousness.—A feeling, being anything of which the mind is conscious, is synonymous with *state of consciousness*. It is commonly confined to the sensations and emotions, or to the emotions alone; but it is properly a genus, having for species, Sensation, Emotion, Thought, and Volition. By thought is meant all that we are internally conscious of when we think; e.g. the idea of the sun, and not the sun itself, is a thought; and so, not even an imaginary thing like a ghost, but only the idea of it, is a thought. In like manner, a sensation differs both from the object causing it, and the attribute ascribed to the object. Yet language (except in the case of the sensations of hearing) has seldom provided the sensations with separate names; so that we have to name the sensation from the object or the attribute exciting it, though we might *conceive* the sensation to exist, though it never actually does, without an exciting cause. Again, another distinction has to be attended to, viz. the difference between the sensation and the state of the bodily organs, which is the physical agency producing it. This distinction escapes notice partly by reason of the division of the feelings into bodily and mental. But really there is no such division, even sensations being states of the sentient

mind, and not of the body. The difference, in fact, between sensations, thoughts, and emotions, is only in the different agency producing the feeling; it being, in the case of the sensations, a bodily, and, for the other two, a mental state. Some suppose, after the sensation, in which, they say, the mind is passive, a distinct active process called perception, which is the direct recognition of an external object, as the cause of the sensation. Probably, perceptions are simply cases of belief claiming to be intuitive, i.e. free of external evidence. But, at any rate, any question as to their nature is irrelevant to an inquiry like the present, viz. how we get the non-original part of our knowledge. And so also is the distinction in German metaphysics, between the mind's *acts* and its passive *states*. Enough for us now that they are all states of the mind.

II. Substances.—Logicians think they have defined substance and attribute, when they have shown merely what difference the use of them respectively makes in the grammar of a sentence. They say an attribute must be an attribute *of* something, but that a substance is self-existent (being followed, if a relative, by *of*, not *quâ* substance, but *quâ* the relation). But this *of*, as distinguishing attributes, itself needs explanation: besides, we can no more conceive a substance independent of attributes, than an attribute independent of a substance. Metaphysicians go deeper into the distinction than logicians. Substances, most of them say, are either bodies or minds; and, of these, a body is the external cause to which we ascribe sensations. Berkeley and the Idealists, however, deny that there exists any cause of sensations (except, indeed, a First Cause). They argue that the *whole* of our notion

of a body consists of a number of our own or others' sensations occurring together habitually (so that, the thought of one being associated with the thought of the others, we get what Hartley and Locke call a complex idea). They deny that a residuum would remain if all the attributes were pared off; for that, though the sensations are bound together by a law, the existence of a *substratum* is but one of many forms of mentally realising the connection. And they ask how it is,—since so long as the sensations occurred in the old order, we should not miss such a *substratum*, supposing it to have once existed *and to have perished*—that we can know it exists even now? Their opponents used formerly to reply, that the uniform order of sensations implies an external cause determining the law of the order; and that the attributes *inhere* in this external cause or substratum, viz. matter. But at last it was seen that the existence of matter could not be proved by extrinsic evidence; consequently, now the answer to the idealist argument simply is, that the belief in an external cause of sensations is universal, and as intuitive as our knowledge of sensations themselves. Even Kant allows this (notwithstanding his belief in the existence of a universe of *things in themselves*, i.e. *Noümena*, as contrasted with the mental representation of them, where the sensations, he thinks, furnish the matter, and the laws of the mind, the form). Brown even traced up to the sensations of touch, combined with the sensations seated in the muscular frame, those very properties, viz., extension and figure, which Reid referred to as proving that some qualities must exist, not in the sensations, but

in the things themselves, *since* they cannot possibly be copies of any impression on the senses. We have, in truth, no right to consider a thing's sensible qualities akin to its nature, unless we suppose an absurdity, viz. that a cause must, as such, resemble its effects. In any case, the question whether Ontology be a possible science, concerns, not Logic, but the nature and laws of intuitive knowledge. And the question as to the nature of Mind is as out of place here as that about Body. As body is the unknown exciting cause of sensations, so mind, the other kind of substance, is the unknown recipient both of the sensations and of all the other feelings. Though I call a something *myself*, as distinct from the series of feelings, the 'thread of consciousness,' yet this self shows itself only through its capacity of feeling or being conscious; and I can, with my present faculties, conceive the gaining no new information but about as yet unknown faculties of feeling. In short, as body is the unsentient cause of all feelings, so mind is the sentient *subject* (in the German sense) of them, viz. that which feels them. About this inner nature we know nothing, and Logic cares nothing.

III. Attributes.—Qualities are the first class of attributes. Now, if we know nothing about bodies but the sensations they excite, we can mean nothing by the attributes of bodies but sensations. Against this it has been urged that, though we know nothing of sensible objects except the sensations, the quality which we ascribe on the *ground* of the sensation may yet be a real hidden power or quality in the object, of which the sensation is only the evidence. Seemingly, this doctrine arises only from the tendency to

suppose that there must be two different things to answer to two names when not quite synonymous. Quality and sensation are probably names for the same thing viewed in different lights. The doctrine of an entity *per se*, called quality, is a relic of the scholastic *occult causes*; the only intelligible cause of sensation being the presence of the assemblage of phenomena, called the *object*. Why the presence of the object causes the sensation, we know not; and, granting an *occult cause*, we are still in the dark as to how *that* produces the effect. However, the question belongs to metaphysics; and it suits this doctrine, as well as the opposed one, to say that a quality has for its *foundation* a sensation.

Relations form the second class of attributes. In all cases of relation there exists some fact into which the relatives enter as parties concerned; and this is the *fundamentum relationis*. Whenever two things are involved in some one fact, we may ascribe to them a relation grounded on it, however general the fact may be. As, then, a quality is an attribute based on the fact of a sensation, so a relation is an attribute based on a fact into which two objects enter jointly. This fact in both is always composed entirely of states of consciousness; and this, whether it be complicated, as in many legal relations, or simple, as in the relations expressed by *antecedent* and *consequent* and by *simultaneous*, where the fact consists merely of the two things so related, since the consciousness either of the succession or of the simultaneousness of the two sensations which represent the things, is a feeling not added to, but involved in *them*, being a condition under which we must suppose things. And so, like-

wise, with the relations of likeness and unlikeness. The feeling of these sometimes cannot be analysed, when the *fundamentum relationis* is, as in the case of two simple sensations, e.g. two sensations of white, only the two sensations themselves, the consequent feeling of their resemblance being, like that of their succession or simultaneousness, apparently involved in the sensations themselves. Sometimes, again, the likeness or unlikeness is complex, and therefore can be analysed into simpler cases. In any case, likeness or unlikeness must resolve itself into likeness or unlikeness between states of our own or some other mind; and this, whether the feeling of the resemblance or dissimilarity relate to bodies or to attributes, since the former we know only through the sensations they are supposed to excite, and the latter through the sensations on which they are grounded. And so, again, when we say that two relations are alike (one of the many senses of analogy), we simply assert resemblance between the facts constituting the two *fundamenta relationis*. Several relations, called by different names, are really cases of resemblance. Thus, equality, i.e. the exact resemblance existing between things in respect of their quantity, is often called identity.

The *third* species of attributes is Quantity. The assertion of likeness or unlikeness in quantity, as in quality, is always founded on a likeness or unlikeness in the sensations excited. What the difference is all who have had the sensations know, but it cannot be explained to those who never had them.

In fine, all the attributes classed under Quality and Quantity are the powers bodies have of exciting certain

sensations. So, Relation generally is but the power which an object has of joining its correlative in producing the series of sensations, which is the only sign of the existence of the fact on which they both are grounded. The relations of succession and simultaneousness, indeed, are not based on any fact (i.e. any feeling) distinct from the related objects. But these relations are themselves states of consciousness; resemblance, for example, being nothing but our feeling of resemblance: at least, we ascribe these relations to objects or attributes simply because they hold between the feelings which the objects excite and on which the attributes are grounded. And as with the attributes of bodies, so also those of minds are grounded on states of consciousness. Considered in itself, we can predicate of a mind only the series of its own feelings: e.g. by *devout* we mean that the feelings implied in that word form an oft-recurring part of the series of feelings filling up the sentient existence of that mind. Again, attributes may be ascribed to a mind as to a body, as grounded on the thoughts or emotions (not the sensations, for only bodies excite them) which it excites in others: e.g. when we call a character admirable, we mean that it causes feelings in us of admiration. Sometimes, under one word really two attributes are predicated, one a state of the mind, the other of other minds affected by thinking of it: e.g. He is generous. Sometimes, even bodies have the attribute of producing an emotion: e.g. That statue is beautiful.

The general result is, that there are three chief kinds of nameable things:—1. Feelings distinct from the objects exciting and the organs supposed to con-

vey them, and divisible into four classes, perceptions being only a particular case of belief, which is itself a sort of thought, while actions are only volitions followed by an effect. 2. Substances, i.e. the unknown cause and the unknown recipient of our sensations. 3. Attributes, subdivisible into Quality, Relation, Quantity. Of these (α) qualities, like substances, are known only by the states of consciousness which they excite, and on which they are based, and by which alone, though they are treated as a distinct class, they can be described. (β) Relations also, with four exceptions, are based on some fact, i.e. a series of states of consciousness. (γ) Quantity is, in the same way, based on our sensations. In short, all attributes are only our sensations and other feelings, or something involved in them. We may, then, classify nameable things thus:—1, Feelings; 2, Minds; 3, Bodies, together with the properties whereby they are *popularly* (though the evidence is very deficient) supposed to excite sensations; 4, the relations of Succession and Coexistence, Likeness and Unlikeness, which subsist really only between states of consciousness.

These four classes are a substitute for Aristotle's abortive Categories. As they comprise all nameable things, every fact is made up of them or some of them; those that are called *subjective* facts being composed wholly of feelings as such, and the *objective* facts, though composed wholly or partly of substances and attributes, being grounded on corresponding subjective facts.

CHAPTER IV.

PROPOSITIONS.

THE copula is a mere sign of predication, though it is often confounded with *to be*, the verb of existence (and that not merely by Greeks, but even by moderns, whose larger experience how one word in one language often answers to several in another, should have saved them from thinking that things with a common name must have a common nature). The *first* division of propositions is into Affirmative and Negative, the copula in the latter being *is not*. Hobbes and others, by joining the *not* to the predicate, made the latter what they call a *negative name*. But as a negative name is one expressing the *absence* of an attribute, we thus in fact merely deny its presence, and therefore the affirmative guise these thinkers give to negative propositions is only a fiction. Again, *modal* propositions cannot be reduced to the common form by joining the modality to the predicate, and turning, e.g. The sun *did* rise, into, The sun is a thing having risen; for the past time is not a particular kind of rising, and it affects not the predicate, but the predication, i.e. the applicability of the predicate to the subject. There are, however, certain cases in which the qualification may be detached from the copula; e.g. in such expressions as, *may be*, *is perhaps*; for, then we really do not mean to assert anything about the fact, but only about the state of our mind about it, so that it is not the predication which is affected: e.g. Cæsar *may be* dead,

may properly be rendered, I am not sure that he is alive.

The *second* division is into Simple and Complex. Several propositions joined by a conjunction do not make a complex proposition. The conjunction, so far from making the two one, adds another, as being an abbreviation generally of an additional proposition: e.g. *and* is an abbreviation of one additional proposition, viz. We must think of the two together; while *but* is an abbreviation of two additional propositions, viz. We must think of them together, and we must recollect there is a contrast between them. But hypothetical propositions, i.e. both disjunctives and conditionals, are true complex propositions, since with several terms they contain but a single assertion. Thus, in, If the Koran comes from God, Mahomet is God's prophet, we do not assert the truth of either of the simple propositions therein contained (viz. the Koran comes from God, and Mahomet is God's prophet), but only the inferribility of one from the other. The only difference, then, between a hypothetical and a categorical proposition, is that the former is always an assertion about an assertion (though some categoricals are so likewise; e.g. That the whole is greater than its parts, is an axiom). Their conspicuous place in treatises on Logic arises from this attribute which they predicate of a proposition (for a proposition, like other things, has attributes), viz. its being an inference from something else, being, with reference to Logic, its chief attribute.

The *third* common division is into Universal, Particular, Indefinite, and Singular. A proposition whose subject is an individual name, even if not a

proper name, is singular, e.g. The founder of Rome was killed. In particular propositions, if the part of the class meant by the *some* were specified, the proposition would become either singular, or universal with a different subject including all the part. Indefinite in Logic is a solecism like *doubtful gender* in grammar, for the speaker must mean to make either a particular or a universal assertion.

CHAPTER V.

THE IMPORT OF PROPOSITIONS.

THE object of an inquiry into the nature of propositions must be to analyse, either, 1, the state of mind called belief, or 2, what is believed. Philosophers have usually, but wrongly, thought the former, i.e. an analysis of the act of judgment, the chief duty of Logic, considering a proposition to consist in the denying or affirming one *idea* of another. True, we must have the two ideas in the mind together, in order to believe the assertion about the two *things*; but so we must also in order to disbelieve it. True also, that besides the putting the ideas together, there may be a mental process; but this has nothing to do with the import of propositions, since they are assertions about things, i.e. facts of external nature, not about the ideas of them, i.e. facts in our mental history. Logic has suffered from stress being laid on the relation between the ideas rather than the phenomena, nature thus coming to be studied by logicians second-hand, that is to say, as represented

in our minds. Our present object, therefore, must be to investigate judgments, not judgment, and to inquire what it is which we assert when we make a proposition.

Hobbes (though he certainly often shows his belief that all propositions are not merely about the meaning of words, and that general names are given to things on account of their attributes) declares that what we assert, is our belief that the subject and predicate are names of the same thing. This is, indeed, a property of all true propositions, and the only one true of all. But it is not the scientific definition of propositions; for though the mere collocation which makes a proposition a proposition, signifies only this, yet that *form*, combined with other *matter*, conveys much more meaning. Hobbes's principle accounts *fully* only for propositions where both terms are proper names. He applied it to others, through attending, like all nominalists, to the denotation, and not the connotation of words, holding them to be, like proper names, mere marks put upon individuals. But when saying that, e.g. Socrates is wise, is a true proposition, because of the conformity of import between the terms, he should have asked himself why *Socrates* and *wise* are names of the same person. He ought to have seen that they are given to the same person, not because of the intention of the maker of each word, but from the resemblance of their connotation, since a word means properly certain attributes, and, only secondarily, objects denoted by it. What we really assert, therefore, in a proposition, is, that where we find certain attributes, we shall find a certain other one,

which is a question not of the meaning of names, but of the laws of nature.

Another theory virtually identical with Hobbes's, is that commonly received, which makes predication consist in referring things to a *class*; that is (since a class is only an indefinite number of individuals denoted by a general name), in viewing them as some of those to be called by that general name. This view is the basis of the *dictum de omni et nullo*, on which is supposed to rest the validity of all reasoning. Such a theory is an example of *ὑστερον πρότερον*: it explains the cause by the effect, since the predicate cannot be known for a class name which includes the subject, till several propositions having it for predicate have been first assented to. This doctrine seems to suppose all individuals to have been made into parcels, with the common name outside; so that, to know if a general name can be predicated correctly of the subject, we need only search the roll so entitled. But the truth is, that general names are marks put, not upon definite objects, but upon collections of objects ever fluctuating. We may frame a class without knowing a single individual belonging to it: the individual is placed in the class because the proposition is true; the proposition is not made true by the individual being placed there.

Analysis of different propositions shows what is the real import of propositions not simply verbal. Thus, we find that even a proposition with a proper name for subject, means to assert that an individual thing has the attributes connoted by the predicate, the name being thought of only as means for giving

information of a physical fact. This is still more the case in propositions with connotative subjects. In these the denoted objects are indicated by some of their attributes, and the assertion really is, that the predicate's set of attributes constantly accompanies the subject's set. But as every attribute is grounded on some fact or phenomenon, a proposition, when asserting the attendance of one or some attributes on others, really asserts simply the attendance of one phenomenon on another; e.g. When we say Man is mortal, we mean that where certain physical and moral facts called humanity are found, there also will be found the physical and moral facts called death. But analysis shows that propositions assert other things besides (although this is indeed their ordinary import) this coexistence or sequence of two phenomena, viz. two states of consciousness. Assertions in propositions about those unknowable entities (*noümena*) which are the hidden causes of phenomena, are made, indeed, only in virtue of the knowable *phenomena*. Still, such propositions do, besides asserting the sequence or coexistence of the phenomena, assert further the *existence* of the noümena; and, moreover, in affirming the existence of a noümenon, which is an unknowable *cause*, they assert *causation* also. Lastly, propositions sometimes assert *resemblance* between two phenomena. It is not true that, as some contend, every proposition whose predicate is a general name affirms resemblance to the other members of the class; for such propositions generally assert only the possession by the subject of certain common peculiarities; and the assertion would be true though there were no mem-

bers of the class besides those denoted by the subject. Nevertheless, *resemblance alone* is sometimes predicated. Thus, when individuals are put into a class as belonging to it, not absolutely, but rather than to any other, the assertion is, not that they have the attributes connoted, but that they resemble those having them more than they do other objects. So, again, *only resemblance* is predicated, when, though the predicate is a class name, the class is based on general unanalysable resemblance. The classes in question are those of the simple feelings; the names of feelings being, like all concrete general names, connotative, but only of a mere resemblance.

In short, one of *five* things, viz. Existence, Coexistence (or, to be more particular, Order in Place), Sequence (or, more particularly, Order in Time, which comprises also the *mere fact of Coexistence*), Causation, and Resemblance, is asserted or denied in every proposition. This division is an exhaustive classification with respect to all things that can be believed. Although only propositions with concrete terms have been spoken of, it is equally the fact that, in propositions with an abstract term or terms, we predicate one of these same *five* things. There cannot be any difference in the import of these two classes of propositions, since there is none in the import of their terms, for the real signification of a concrete term resides in its connotation (so that in a concrete proposition we really predicate an attribute), and what the concrete term connotes forms the whole sense of the abstract. Thus, all propositions with abstract terms can be turned into equivalent ones with concrete, the new terms being either the names which connote

the attributes, or names of the facts which are the *fundamenta* of the attributes: e.g. Thoughtlessness is danger, is equivalent to, Thoughtless actions (the *fundamentum*) are dangerous.

Finally, as these *five* are the only things affirmable, so are they the only things deniable.

CHAPTER VI.

PROPOSITIONS MERELY VERBAL.

THE object of Logic is to find how propositions are to be proved. As preliminary to this, it has been already shown that the Conceptualist view of propositions, viz. that they assert a relation between two ideas, and the Nominalist, that they assert agreement or disagreement between the meanings of two names, are both wrong as general theories: for that *generally* the import of propositions is, to affirm or deny respecting a phenomenon, or its hidden source, one of five kinds of facts. There is, however, a class of propositions which relate not to matter of fact, but to the meaning of names, and which, therefore, as names and their meanings are arbitrary, admit not of truth or falsity, but only of agreement or disagreement with usage. These *verbal* propositions are not only those in which both terms are proper names, but also some, viz. *essential* propositions, thought to be more closely related to things than any others. The Aristotelians' belief that objects are made what they are called by the inherence of a certain *general substance* in the individuals which get from it all their essential pro-

perties, prevented even Porphyry (though more reasonable than the mediæval Realists) from seeing that the only difference between altering a non-essential (or *accidental*) property, which, he says, makes the thing *ἀλλοῖον*, and altering an essential one, which makes it *ἄλλο* (i.e. a different thing), is, that the latter change makes the object change its name. But even when it was no longer believed that there are real entities answering to general terms, the doctrine based upon it, viz. that a thing's essence is that without which the thing could neither be, nor be conceived to be, was still generally held, till Locke convinced most thinkers that the supposed essences of classes are simply the significations of their *names*. Yet even Locke supposed that, though the essences of classes are *nominal*, *individuals* have *real* essences, which, though unknown, are the causes of their sensible properties.

An accidental proposition (i.e. in which a property not connoted by the subject is predicated of it) tacitly asserts the existence of a thing corresponding to the subject; otherwise, such a proposition, as it does not explain the name, would assert nothing at all. But an essential proposition (i.e. in which a property connoted by the subject is predicated of it) is identical. The only use of such propositions is to *define* words by unfolding the meaning involved in a name. When, as in mathematics, important consequences seem to follow from them, such really follow from the tacit assumption, through the ambiguity of the copula, of the real existence of the *object* named.

Accidental propositions include, 1, those with a proper name for subject, since an individual has no

essence (although the schoolmen, and rightly, according to their view of genera and species' as entities inhering in the individuals, attributed to the individual the essence of his class); and, 2, all general or particular propositions in which the predicate connotes any attribute not connoted by the subject. Accidental propositions may be called *real*; they add to our knowledge. Their import may be expressed (according as the attention is directed mainly, either to what the proposition means, or to the way in which it is to be used), either, by the formula: The attributes of the subject are always (or never) accompanied by those signified by the predicate; or, by the formula: The attributes of the subject are evidence, or a mark, of the presence of those of the predicate. For the purposes of reasoning, since propositions enter into *that*, not as ultimate results, but as means for establishing other propositions, the latter formula is preferable.

CHAPTER VII.

THE NATURE OF CLASSIFICATION, AND THE FIVE PREDICABLES.

It is merely an accident when general names are names of classes of real objects: e.g. The unity of God, in the Christian sense, and the non-existence of the things called dragons, do not prevent those names being general names. The using a name to connote attributes, turns the things, whether real or imaginary, into a class. But, in predicating the name, we predicate only the attributes; and even

when a name (as, e.g. those in Cuvier's system) is introduced as a means of grouping certain objects together, and not, as usually, as a means of predication, it still signifies nothing but the possession of certain attributes.

Classification (as resulting from the use of general language) is the subject of the Aristotelians' Five Predicables, viz. *Genus*, *Species*, *Differentia*, *Proprium*, *Accidens*. These are a division of general names, not based on a distinction in their meaning, i.e. in the attributes connoted, but on a distinction in the class denoted. They express, not the meaning of the predicate itself, but its relation (a varying one) to the subject. Commonly, the names of any two classes (or, popularly, the classes themselves), one of which includes all the other and more, are called respectively *genus* and *species*. But the Aristotelians, i.e. the schoolmen, meant by *differences in kind* (*genere* or *specie*) something which was in its nature (and not merely with reference to the connotation of the name) distinct from *differences in the accidents*. Now, it is the fact that, though a fresh class may be founded on the smallest distinction in attributes, yet that some classes have, to separate them from other classes, no common attributes except those connoted by the name, while others have innumerable common qualities (from which we have to select a few samples for connotation) not referrible to a common source. The ends of language and of classification would be subverted if the latter (not if the former) sorts of *difference* were disregarded. Now, it was these only that the Aristotelians called *kinds* (*genera* or *species*), holding *differences* made up of *certain* and *definite*

properties to be *differences* in the *accidents* of things. In conformity with this distinction—and it is a true one—any class, e.g. negro as opposed to white man, may, according as physiology shall show the *differences* to be infinite or finite, be discovered to be a distinct *kind* or *species* (though not according to the naturalist's construction of *species*, as including all descended from the same stock), or merely a subdivision of the *kind* or *species*, Man. Among *kinds*, a *genus* is a class divisible into other *kinds*, though it may be itself a *species* in reference to higher *genera*; that which is not so divisible, is an individual's *proximate kind* or *infima species* (*species prædicabilis* and also *subjicibilis*), whose common properties must include all the common properties of every other real *kind* to which the individual can be referred.

The Aristotelians said that the *differentia* must be of the *essence* of the subject. They vaguely understood, indeed, by the *essence* of a thing, that which makes it the *kind* of thing that it is. But, as a *kind* is such from innumerable qualities not flowing from a common source, logicians selected the qualities which make the thing be what it is called, and termed these the *essence*, not merely of the *species*, but, in the case of the *infima species*, of the individual also. Hence, the distinction between the predicables, *Differentia*, *Proprium*, and *Accidens*, is founded, not on the nature of things, but on the connotation of names. The *specific difference* is that which must be added to the connotation of the *genus* to complete the connotation of the *species*. A *species* may have various *differences*, according to the principle of the particular classification. A *kind*, and not

merely a class, may be founded on any one of these, if there be a host of properties behind, of which this one is the index, and not the source. Sometimes a name has a technical as well as an ordinary connotation (e.g. the name *Man*, in the Linnæan system, connotes a certain number of incisor and canine teeth, instead of its usual connotation of rationality and a certain general form); and then the word is in fact ambiguous, i.e. two names. *Genus* and *Differentia* are said to be of the essence; that is, the properties signified by them are connoted by the name denoting the *species*. But both *proprium* and *accidens* are said to be predicated of the species *accidentally*. A *proprium* of the species, however, is predicated of the species necessarily being an attribute, not indeed connoted by the name, but following from an attribute connoted by it. It follows, either by way of demonstration as a conclusion from premisses, or by way of causation as effect from cause; but, in either case, *necessarily*. Inseparable accidents, on the other hand, are attributes universal, so far as we know, to the species (e.g. blackness to crows), but not *necessary*; i.e. neither involved in the meaning of the name of the species, nor following from attributes which are. Separable accidents do not belong to all, or if to all, not at all times (e.g. the fact of being born, to man), and sometimes are not constant even in the same individual (e.g. to be hot or cold)

CHAPTER VIII.

DEFINITION.

A DEFINITION is a proposition declaring either the special or the ordinary meaning, i.e. in the case of connotative names, the connotation, of a word. This may be effected by stating directly the attributes connoted; but it is more usual to predicate of the subject of definition one name of synonymous, or several which, when combined, are of equivalent, connotation. So that, a definition of a name being thus generally the sum total of the essential propositions which could be framed with that name for subject, is really, as Condillac says, an *analysis*. Even when a name connotes only a single attribute, it (and also the corresponding abstract name itself) can yet be defined (in this sense of being analysed or resolved into its elements) by declaring the connotation of that attribute, whether, if it be a union of several attributes (e.g. Humanity), by enumerating them, or, if only one (e.g. Eloquence), by dissecting the fact which is its foundation. Even when the fact which is the foundation of the attribute is a simple feeling, and therefore incapable of analysis, still, if the simple feeling have a name, the attribute and the object possessing it may be defined by reference to the fact: e.g. a white object is definable as one exciting the sensation of white; and whiteness, as the power of exciting that sensation. The only names, abstract or concrete, incapable of analysis, and therefore of definition, are proper names, as having no meaning, and also the names of the simple feelings themselves,

since these can be explained only by the resemblance of the feelings to former feelings called by the same or by an exactly synonymous name, which consequently equally needs definition.

Though the only accurate definition is one declaring all the facts involved in the name, i.e. its connotation, men are usually satisfied with anything which will serve as an index to its denotation, so as to guard them from applying it inconsistently. This was the object of logicians when they laid down that a species must be defined *per genus et differentiam*, meaning by the *differentia* one attribute included in the essence, i.e. in the connotation. And, in fact, one attribute, e.g. in defining man, Rationality (Swift's Houyhnhms having not been as yet discovered) often does sufficiently mark out the objects denoted. But, besides that a definition of this kind ought, in order to be complete, to be *per genus et differentias*, i.e. by *all* the connoted attributes not implied in the name of the *genus*, still, even if all were given, a *summum genus* could not be so defined, since it has no superior genus. And for merely marking out the objects denoted, Description, in which none of the connoted attributes are given, answers as well as logicians' so-called *essential* definition. In Description, any one or a combination of attributes may be given, the object being to make it exactly coextensive with the name, so as to be predicable of the same things. Such a description may be turned into an essential definition by a change of the connotation (not the denotation) of the name; and, in fact, thus are manufactured almost all scientific definitions, which, being landmarks of

classification, and not meant to declare the meaning of the name (though, in fact, they do declare it in its new use), are ever being modified (as is the definition of a science itself) with the advance of knowledge. Thus, a technical definition helps to expound the artificial classification from which it grows; but ordinary definition cannot expound, as the Aristotelians fancied it could, the natural classification of things, i.e. explain their division into *kinds*, and the relations among the *kinds*: for the properties of every *kind* are innumerable, and all that definition can do is to state the connotation of the name.

Both these two modes, viz. the essential but incomplete Definition, and the accidental, or Description, are imperfect; but the Realists' distinction between definition of names and of things is quite erroneous. Their doctrine is now exploded; but many propositions consistent with it alone (e.g. that the science of geometry is deduced from definitions) have been retained by Nominalists, such as Hobbes. Really a definition, as such, cannot explain a thing's nature, being merely an identical proposition explaining the meaning of a word. But definitions of names *known to be names of really existing objects*, as in geometry, include two propositions, one a definition and another a postulate. The latter affirms the existence of a thing answering to the name. The science is based on the postulates (whether they rest on intuition or proof), for the demonstration appeals to them alone, and not on the definitions, which indeed might, though at some cost of brevity, be dispensed with entirely. It has been argued that, at

any rate, definitions are premisses of science, *provided* they give such meanings to terms as suit existing things: but even so, the inference would obviously be from the existence, not of the name which means, but of the thing which has the properties.

One reason for the belief that demonstrative truths follow from the definitions, not from the postulates, was because the postulates are never quite true (though in reality so much of them is true as is true of the conclusions). Philosophers, therefore, searching for something more accurately true, surmised that definitions must be statements and analyses, neither of words nor of things, as such, but of ideas; and they supposed the subject-matter of all demonstrative sciences to be abstractions of the mind. But even allowing this (though, in fact, the mind cannot so abstract one property, e.g. length, from all others; it only *attends* to the one exclusively), yet the conclusions would still follow, not from the mere definitions, but from the postulates of the real existence of the ideas.

Definitions, in short, are of names, not things: yet they are not therefore arbitrary; and to determine what *should be* the meaning of a term, it is often necessary to look at the objects. The obscurity as to the connotation arises through the objects being named before the attributes (though it is from the latter that the concrete general terms get their meaning), and through the same name being popularly applied to different objects on the ground of general resemblance, without any distinct perception of their common qualities, especially when these are complex. The philosopher, indeed, uses general

names with a definite connotation ; but philosophers do not make language—it grows : so that, by degrees, the same name often ceases to connote even general resemblance. The object in remodelling language is to discover if the things denoted have common qualities, i.e. if they form a class ; and, if they do not, to form one artificially for them. A language's rude classifications often serve, when retouched, for philosophy. The transitions in signification, which often go on till the different members of the group seem to connote nought in common, indicate, at any rate, a striking resemblance among the objects denoted, and are frequently an index to a real connection ; so that arguments turning apparently on the double meaning of a term, may perhaps depend on the connection of two ideas. To ascertain the link of connection, and to procure for the name a distinct connotation, the resemblances of things must be considered. Till the name has got a distinct connotation, it cannot be defined. The philosopher chooses for his connotation of the name the attributes most important, either directly, or as the differentia leading to the most interesting propria. The enquiry into the more hidden agreement on which these obvious agreements depend, often itself arises under the guise of enquiries into the definition of a name.

BOOK II.

REASONING.

CHAPTER I.

INFERENCE, OR REASONING IN GENERAL.

THE preceding book treated, not of the proper subject of logic, viz. the nature of proof, but of assertion. Assertions (as, e.g. definitions) which relate to the meaning of words, are, since *that* is arbitrary, incapable of truth or falsehood, and therefore of proof or disproof. But there are assertions which are subjects for proof or disproof, viz. the propositions (the real, and not the verbal) whose subject is some fact of consciousness, or its hidden cause, about which is predicated, in the affirmative or negative, one of five things, viz. existence, order in place, order in time, causation, resemblance: in which, in short, it is asserted, that some given subject does or does not possess some attribute, or that two attributes, or sets of attributes, do or do not (constantly or occasionally) coexist.

A proposition not believed on its own evidence, but inferred from another, is said to be *proved*; and this process of inferring, whether syllogistically or not, is *reasoning*. But whenever, as in the deduction of a particular from a universal, or, in Conversion, the assertion in the new proposition is the same as the

whole or part of the assertion in the original proposition, the inference is only apparent; and such processes, however useful for cultivating a habit of detecting quickly the concealed identity of assertions, are not reasoning.

Reasoning, or Inference, properly so called, is, 1, Induction, when a proposition is inferred from another, which, whether particular or general, is less general than itself; 2, Ratiocination, or Syllogism, when a proposition is inferred from others equally or more general; 3, a kind which falls under neither of these descriptions, yet is the basis of both.

CHAPTER II.

RATIOCINATION, OR SYLLOGISM.

THE syllogistic figures are determined by the position of the middle term. There are four, or, if the fourth be classed under the first, three. But syllogisms in the other figures can be reduced to the first by conversion. Such reduction may not indeed be necessary, for different arguments are suited to different figures; the first figure, says Lambert, being best adapted to the discovery or proof of the properties of things; the second, of the distinctions between things; the third, of instances and exceptions; the fourth, to the discovery or exclusion of the different species of a genus. Still, as the premisses of the first figure, got by reduction, are really the same as the original ones, and as the only arguments of great scientific importance, viz. those in which the conclusion is a universal affirmative, can be proved in the first figure

alone, it is best to hold that the two elementary forms of the first figure are the universal types, the one in affirmatives, the other in negatives, of all correct ratiocination.

The *dictum de omni et nullo*, viz. that whatever can be affirmed or denied of a class can be affirmed or denied of everything included in the class, which is a true account generalised of the constituent parts of the syllogism in the first figure, was thought the basis of the syllogistic theory. The fact is, that when universals were supposed to have an independent objective existence, this dictum stated a supposed law, viz. that the *substantia secunda* formed part of the properties of each individual substance bearing the name. But, now that we know that a class or universal is nothing but the individuals in the class, the dictum is nothing but the identical proposition, that whatever is true of certain objects is true of each of them, and, to mean anything, must be considered, not as an axiom, but as a circuitous definition of the word *class*.

It was the attempt to combine the nominalist view of the signification of general terms with the retention of the dictum as the basis of all reasoning, that led to the self-contradictory theories disguised under the ultra-nominalism of Hobbes and Condillac, the ontology of the later Kantians, and (in a less degree) the abstract ideas of Locke. It was fancied that the process of inferring new truths was only the substitution of one arbitrary sign for another; and Condillac even described science as *une langue bien faite*. But language merely enables us to remember and impart our thoughts; it strengthens, like an

artificial memory, our power of thought, and is thought's powerful instrument, but not its exclusive subject. If, indeed, propositions in a syllogism did nothing but refer something to or exclude it from a class, then certainly syllogisms might have the dictum for their basis, and import only that the classification is consistent with itself. But such is not the primary object of propositions (and it is on this account, as well as because men will never be persuaded in common discourse to *quantify* the predicate, that Mr. De Morgan's or Sir William Hamilton's *quantification of the predicate* is a device of little value). What is asserted in every proposition which conveys real knowledge, is a fact dependent, not on artificial classification, but on the laws of nature; and as ratiocination is a mode of gaining real knowledge, the principle or law of all syllogisms, with propositions not purely verbal, must be, for affirmative syllogisms, that; Things coexisting with the same thing coexist with one another; and for negative, that; A thing coexisting with another, with which a third thing does not coexist, does not coexist with that third thing. But if (see *suprà*, p. 26) propositions (and, of course, all combinations of them) be regarded, not speculatively, as portions of our knowledge of nature, but as memoranda for practical guidance, to enable us, when we know that a thing has one of two attributes, to infer it has the other, these two axioms may be translated into one, *viz.* Whatever has any mark has that which it is a mark of; or, if both premisses are universal, Whatever is a mark of any mark, is a mark of that of which this last is a mark.

CHAPTER III.

THE FUNCTIONS AND LOGICAL VALUE OF THE
SYLLOGISM.

THE question is, whether the syllogistic process is one of inference, i.e. a process from the known to the unknown. Its assailants say, and truly, that in every syllogism, considered as an argument to prove the conclusion, there is a *petitio principii*; and Dr. Whately's defence of it, that its object is to unfold assertions wrapped up and implied (i.e. in fact, *asserted unconsciously*) in those with which we set out, represents it as a sort of trap. Yet, though no reasoning from generals to particulars can, as such, prove anything, the conclusion is a *bonâ fide* inference, though not an inference from the general proposition. The general proposition (i.e. in the first figure, the major premiss) contains not only a record of many particular facts which we have observed or inferred, but also instructions for making inferences in unforeseen cases. Thus the inference is completed in the major premiss; and the rest of the syllogism serves only to decipher, as it were, our own notes.

Dr. Whately fails to make out that syllogising, i.e. reasoning from generals to particulars, is the *only* mode of reasoning. No additional evidence is gained by interpolating a general proposition, and therefore we may, if we please, reason directly from the individual cases, since it is on these alone that the general proposition, if made, would rest. Indeed, thus are in fact drawn, as well the inferences of

children and savages, and of animals (which latter having no signs, can frame no general propositions), as even those drawn by grown men generally, from personal experience, and particularly the inferences of men of high practical genius, who, not having been trained to generalise, can apply, but not state, their principles of action. Even when we have general propositions we need not use them. Thus Dugald Stewart showed that the axioms need not be expressly adverted to in order to make good the demonstrations in Euclid; though he held, inconsistently, that the definitions must be. All general propositions, whether called axioms, or definitions, or laws of nature, are merely abridged statements of the particular facts, which, as occasion arises, we either think we may proceed on as proved, or intend to assume.

In short, all inference is from particulars to particulars; and general propositions are both registers or memoranda of such former inferences, and also short formulæ for making more. The major premiss is such a formula; and the conclusion is an inference drawn, not from, but according to that formula. The *actual* premisses are the particular facts whence the general proposition was collected inductively; and the syllogistic rules are to guide us in reading the register, so as to ascertain what it was that we formerly thought might be inferred from those facts. Even where ratiocination is independent of induction, as, when we accept from a man of science the doctrine that all A is B; or from a legislator, the law that all men shall do this or that, the operation of drawing thence any particular conclusion is a process, not of

inference, but of interpretation. In fact, whether the premisses are given by authority, or derived from our own (or predecessors') observation, the object is always simply to interpret, by reference to certain marks, an intention, whether that of the propounder of the principle or enactment, or that which we or our predecessors had when we framed the general proposition, so that we may draw no inferences that were not *intended* to be drawn. We assent to the conclusion in a syllogism on account of its consistency with what we interpret to have been the intention of the framer of the major premiss, and not, as Dr. Whately held, because the supposition of a false conclusion from the premisses involves a contradiction, since, in fact, the denial, e.g. that an individual now living will die, is not *in terms* contradictory to the assertion that his ancestors and their contemporaries (to which the general proposition, as a record of facts, really amounts) have all died.

But the syllogistic form, though the process of inference, which there always is when a syllogism is used, lies not in this form, but in the act of generalisation, is yet a great collateral security for the correctness of that generalisation. When all possible inferences from a given set of particulars are thrown into one general expression (and, if the particulars support one inference, they always will support an indefinite number), we are more likely both to feel the need of weighing carefully the sufficiency of the experience, and also, through seeing that the general proposition would equally support some conclusion which we *know* to be false, to detect any defect in the evidence, which, from bias or negligence, we

might otherwise have overlooked. But the syllogistic form, besides being useful (and, when the validity of the reasoning is doubtful, even indispensable) for verifying arguments, has the acknowledged merit of all general language, that it enables us to make an induction once for all. We *can*, indeed, and in simple cases habitually *do*, reason straight from particulars; but in cases at all complicated, all but the most sagacious of men, and they also, unless their experience readily supplied them with parallel instances, would be as helpless as the brutes. The only counterbalancing danger is, that general inferences from insufficient premisses may become hardened into general maxims, and escape being confronted with the particulars.

The major premiss is not really part of the argument. Brown saw that there would be a *petitio principii* if it were. He, therefore, contended that the conclusion in reasoning follows from the minor premiss alone, thus suppressing the appeal to experience. He argued, that to reason is merely to analyse our general notions or abstract ideas, and that, *provided* that the relation between the two ideas, e.g. of *man* and of *mortal*, has been first perceived, we can evolve the one directly from the other. But (to waive the error that a proposition relates to ideas instead of things), besides that this *proviso* is itself a surrender of the doctrine that an argument consists simply of the minor and the conclusion, the perception of the relation between two ideas, one of which is not implied in the name of the other, must obviously be the result, not of analysis, but of experience. In fact, both the minor premiss, and also

the expression of our former experience, must *both* be present in our reasonings, or the conclusion will not follow. Thus, it appears that the universal type of the reasoning process is: Certain individuals possess (as I or others have observed) a given attribute; An individual resembles the former in certain other attributes: Therefore (the conclusion, however, not being conclusive from its form, as is the conclusion in a syllogism, but requiring to be sanctioned by the canons of induction) he resembles them also in the given attribute. But, though this, and not the syllogistic, is the universal type of reasoning, yet the syllogistic process is a useful test of inferences. It is expedient, *first*, to ascertain generally what attributes are marks of a certain other attribute, so as, subsequently, to have to consider, *secondly*, only whether any given individuals have those former marks. Every process, then, by which anything is inferred respecting an unobserved case, we will consider to consist of both these last-mentioned processes. Both are equally induction; but the name may be conveniently confined to the process of establishing the general formula, while the interpretation of this will be called 'Deduction.'

CHAPTER IV.

TRAINS OF REASONING, AND DEDUCTIVE SCIENCES.

THE minor premiss always asserts a resemblance between a new case and cases previously known. When this resemblance is not obvious to the senses,

or ascertainable at once by direct observation, but is itself matter of inference, the conclusion is the result of a train of reasoning. However, even then the conclusion is really the result of induction, the only difference being that there are two or more inductions instead of one. The inference is still from particulars to particulars, though drawn in conformity, not to one, but to several formulæ. This need of several formulæ arises merely from the fact that the marks by which we perceive that an inference can be drawn (and of which marks the formulæ are records) happen to be recognisable, not directly, but only through the medium of other marks, which were, by a previous induction, collected to be marks of them.

All reasoning, then, is induction: but the difficulties in sciences often lie (as, e.g. in geometry, where the inductions are the simple ones of which the axioms and a few definitions are the formulæ) not at all in the inductions, but only in the formation of trains of reasoning to prove the minors; that is, in so combining a few simple inductions as to bring a new case, by means of one induction within which it evidently falls, within others in which it cannot be directly seen to be included. In proportion as this is more or less completely effected (that is, in proportion as we are able to discover marks of marks), a science, though always remaining inductive, tends to become also *deductive*, and, to the same extent, to cease to be one of the *experimental* sciences, in which, as still in chemistry, though no longer in mechanics, optics, hydrostatics, acoustics, thermology, and astronomy, each generalisation rests on a special induction, and the reasonings consist but of one step each

An experimental science may become deductive by the mere progress of experiment. The mere connecting together of a few detached generalisations, or even the discovery of a great generalisation working only in a limited sphere, as, e.g. the doctrine of chemical equivalents, does not make a science deductive as a whole; but a science is thus transformed when some comprehensive induction is discovered connecting hosts of formerly isolated inductions, as, e.g. when Newton showed that the motions of all the bodies in the solar system (though each motion had been separately inferred and from separate marks) are all marks of one like movement. Sciences have become deductive usually through its being shown, either by deduction or by direct experiment, that the varieties of some phenomenon in them uniformly attend upon those of a better known phenomenon, e.g. every variety of sound, on a distinct variety of oscillatory motion. The science of number has been the grand agent in thus making sciences deductive. The truths of numbers are, indeed, affirmable of all things only in respect of their quantity; but since the variations of *quality* in various classes of phenomena have (e.g. in mechanics and in astronomy) been found to correspond regularly to variations of *quantity* in the same or some other phenomena, every mathematical formula applicable to quantities so varying becomes a mark of a corresponding general truth respecting the accompanying variations in quality; and as the science of quantity is, so far as a science can be, quite deductive, the theory of that special kind of qualities becomes so likewise. It was thus that Descartes and Clairaut made geometry,

which was already partially deductive, still more so, by pointing out the correspondence between geometrical and algebraical properties.

CHAPTERS V. AND VI.

DEMONSTRATION AND NECESSARY TRUTHS.

ALL sciences are based on induction; yet some, e.g. mathematics, and commonly also those branches of natural philosophy which have been made deductive through mathematics, are called Exact Sciences, and systems of Necessary Truth. Now, their necessity, and even their alleged certainty, are illusions. For the conclusions, e.g. of geometry, flow only seemingly from the definitions (since from definitions, as such, only propositions about the meaning of words can be deduced): really, they flow from an implied assumption of the existence of real things corresponding to the definitions. But, besides that the existence of such things is not actual or possible consistently with the constitution of the earth, neither can they even be *conceived* as existing. In fact, geometrical points, lines, circles, and squares, are simply copies of those in nature, to a part alone of which we choose to *attend*; and the definitions are merely some of our first generalisations about these natural objects, which being, though equally true of all, not exactly true of any one, must, actually, when extended to cases where the error would be appreciable (e.g. to lines of perceptible breadth), be corrected by the joining to them of new propositions about the aberration. The exact correspondence, then, between the facts

and those first principles of geometry which are involved in the so-called definitions, is a fiction, and is merely *supposed*. Geometry has, indeed (what Dugald Stewart did not perceive), some first principles which are true without any mixture of hypothesis, viz. the axioms, as well those which are indemonstrable (e.g. Two straight lines cannot enclose a space) as also the demonstrable ones; and so have all sciences some exactly true general propositions: e.g. Mechanics has the first law of motion. But, generally, the necessity of the conclusions in geometry consists only in their following necessarily from certain *hypotheses*, for which same reason the ancients styled the conclusions of all deductive sciences *necessary*. That the hypotheses, which form part of the premisses of geometry, must, as Dr. Whewell says, not be arbitrary—that is, that in their positive part they are observed facts, and only in their negative part hypothetical—happens simply because our aim in geometry is to deduce conclusions which may be true of real objects: for, when our object in reasoning is not to investigate, but to illustrate truths, arbitrary hypotheses (e.g. the operation of British political principles in Utopia) are quite legitimate.

The ground of our belief in axioms is a disputed point, and one which, through the belief arising too early to be traced by the believer's own recollection, or by other persons' observation, cannot be settled by reference to actual dates. The axioms are really only generalisations from experience. Dr. Whewell, however, and others think that, though suggested, they are not proved by experience, and that their truth is recognised *à priori* by the

constitution of the mind as soon as the meaning of the proposition is understood. But this assumption of an *à priori* recognition is gratuitous. It has never been shown that there is anything in the facts inconsistent with the view that the recognition of the truth of the axioms, however exceptionally complete and instant, originates simply in experience, equally with the recognition of ordinary physical generalisations. Thus, that we see a property of geometrical forms to be true, without inspection of the material forms, is fully explained by the capacity of geometrical forms of being painted in the imagination with a distinctness equal to reality, and by the fact that experience has informed us of that capacity; so that a conclusion on the faith of the imaginary forms is really an induction from observation. Then, again, there is nothing inconsistent with the theory that we learn by experience the truth of the axioms, in the fact that they are conceived by the mind as universally and necessarily true, that is, that we cannot figure them to ourselves as being false. Our capacity or incapacity of conceiving depends on our associations. Educated minds can break up their associations more easily than the uneducated; but even the former not entirely at will, even when, as is proved later, they are erroneous. The Greeks, from ignorance of foreign languages, believed in an inherent connection between names and things. Even Newton imagined the existence of a subtle ether between the sun and bodies on which it acts, because, like his rivals the Cartesians, he could not conceive a body acting where it is not. Indeed, inconceivableness depends so completely on

the accident of our mental habits, that it is the essence of scientific triumphs to make the contraries of once inconceivable views themselves appear inconceivable. For instance, suppositions opposed even to laws so recently discovered as those of chemical composition appear to Dr. Whewell himself to be inconceivable. What wonder, then, that an acquired incapacity should be mistaken for a natural one, when not merely (as in the attempt to conceive space or time as finite) does experience afford no model on which to shape an opposed conception, but when, as in geometry, we are unable even to call up the geometrical ideas (which, being impressions of form, exactly resemble, as has been already remarked, their prototypes), e.g. of two straight lines, in order to try to conceive them inclosing a space, without, by the very act, repeating the scientific experiment which establishes the contrary.

Since, then, the axioms and the misnamed definitions are but inductions from experience, and since the definitions are only hypothetically true, the deductive or demonstrative sciences—of which these axioms and definitions form together the first principles—must really be themselves inductive and hypothetical. Indeed, it is to the fact that the results are thus only conditionally true, that the necessity and certainty ascribed to demonstration are due.

It is so even with the Science of Number, i.e. arithmetic and algebra. But here the truth has been hidden through the errors of two opposite schools; for while many held the truths in this science to be *à priori*, others paradoxically considered them to

be merely verbal, and every process to be simply a succession of changes in terminology, by which equivalent expressions are substituted one for another. The excuse for such a theory as this latter was, that in arithmetic and algebra we carry no ideas with us (not even, as in a geometrical demonstration, a mental diagram) from the beginning, when the premisses are translated into signs, till the end, when the conclusion is translated back into things. But, though this is so, yet in every step of the calculation, there is a real inference of facts from facts: but it is disguised by the comprehensive nature of the induction, and the consequent generality of the language. For numbers, though they must be numbers of something, may be numbers of anything; and therefore, as we need not, when using an algebraical symbol (which represents all numbers without distinction), or an arithmetical number, picture to ourselves all that it stands for, we may picture to ourselves (and this not as a sign of things, but as being itself a thing) the number or symbol itself as conveniently as any other single thing. That we are conscious of the numbers or symbols, in their character of things, and not of mere signs, is shown by the fact that our whole process of reasoning is carried on by predicating of them the properties of things.

Another reason why the propositions in arithmetic and algebra have been thought merely verbal, is that they seem to be *identical* propositions. But in 'Two pebbles and one pebble are equal to three pebbles,' equality but not identity is affirmed; the subject and predicate, though names of the same

objects, being names of them in different states, that is, as producing different impressions on the senses. It is on such inductive truths, resting on the evidence of sense, that the Science of Number is based; and it is, therefore, like the other deductive sciences, an inductive science. It is also, like them, hypothetical. Its inductions are the definitions (which, as in geometry, assert a fact as well as explain a name) of the numbers, and two axioms, viz. The sums of equals are equal; the differences of equals are equal. These axioms, and so-called definitions are themselves exactly, and not merely hypothetically, true. Yet the conclusions are true only on the assumption that, $1 = 1$, i.e. that all the numbers are numbers of the same or equal units. Otherwise, the certainty in arithmetical processes, as in those of geometry or mechanics, is not *mathematical*, i.e. unconditional certainty, but only certainty of inference. It is the enquiry (which can be gone through once for all) into the inferences which can be drawn from assumptions, which properly constitutes all demonstrative science.

New conclusions may be got as well from fictitious as from real inductions; and this is even consciously done, viz. in the *reductio ad absurdum*, in order to show the falsity of an assumption. It has even been argued that all ratiocination rests, in the last resort, on this process. But as this is itself syllogistic, it is useless, as a proof of a syllogism, against a man who denies the validity of this kind of reasoning process itself. Such a man cannot in fact be forced to a contradiction in terms, but only to a contradiction, or rather an infringement, of the fundamental maxim of ratiocination, viz. 'Whatever has a mark, has

what it is a mark of;’ and, since it is only by admitting premisses, and yet rejecting a conclusion from them, that this axiom is infringed, consequently nothing is *necessary* except the connection between a conclusion and premisses.

BOOK III.

INDUCTION.

CHAPTER I.

PRELIMINARY OBSERVATIONS ON INDUCTION IN GENERAL.

As all knowledge not intuitive comes exclusively from inductions, induction is the main topic of Logic; and yet neither have metaphysicians analysed this operation with a view to practice, nor, on the other hand, have discoverers in physics cared to generalise the methods they employed.

Inferences are equally *inductive*, whether, as in science, which needs its conclusions for record, not for instant use, they pass through the intermediate stage of a general proposition (to which class Dr. Whewell, without sanction from facts, or from the usage of Reid and Stewart, the founders of modern English metaphysical terminology, limits the term induction), or are drawn direct from particulars to a supposed parallel case. Neither does it make any difference in the *character* of the induction, whether the process be experiment or ratiocination, and whether the object be to infer a general proposition or an individual fact. That, in the latter case, the difficulty of the practical enquiries, e.g. of a judge or an advocate, lies chiefly in selecting from among all approved general propositions those inductions which suit his

case (just as, even in deductive sciences, the ascertaining of the inductions is easy, their combination to solve a problem hard) is not to the point: the legitimacy of the inductions so selected must at all events be tried by the same test as a new general truth in science. Induction, then, may be treated here as though it were the operation of discovering and proving general propositions; but this is so only because the evidence which justifies an inference respecting one unknown case, would justify a like inference about a whole class, and is really only another form of the same process: because, in short, the logic of science is the universal logic applicable to all human enquiries.

CHAPTER II.

INDUCTIONS IMPROPERLY SO CALLED.

INDUCTION is the process by which what is true at certain times, or of certain individuals, is inferred to be true in like circumstances at all times, or of a whole class. There must be an inference from the known to the unknown, and not merely from a less to a more general expression. Consequently, there is no valid induction, 1, in those cases laid down in the common works on Logic as the only perfect instances of induction, viz. where what we affirm of the class has already been ascertained to be true of each individual in it, and in which the seemingly general proposition in the conclusion is simply a number of singular propositions written in an abridged form;

or, 2, when, as often in mathematics, the conclusion, though really general, is a mere summing up of the different propositions from which it is drawn (whether actually ascertained, or, as in the case of the uncalculated terms of an arithmetical series, when once its law is known, readily to be understood); or, 3, when the several parts of a complex phenomenon, which are only capable of being observed separately, have been pieced together by one conception, and made, as it were, one fact represented in a single proposition.

Dr. Whewell sets out this last operation, which he terms the *colligation of facts*, as induction, and even as the type of induction generally. But, though induction is always colligation, or (as we may, with equal accuracy, characterise such a general expression obtained by abstraction simply connecting observed facts by means of common characters) *description*, colligation, or description, as such, though a necessary preparation for induction, is not induction. Induction explains and predicts (and, as an incident of these powers, describes). Different explanations collected by real induction from supposed parallel cases (e.g. the Newtonian and the *Impact* doctrines as to the motions of the heavenly bodies), or different predictions, i.e. different determinations of the conditions under which similar facts may be expected again to occur (e.g. the stating that the position of one planet or satellite so as to overshadow another, and, on the other hand, that the impending over mankind of some great calamity, is the condition of an eclipse), cannot be true together. But, for a colligation to be correct, it is enough that it enables the mind to represent to itself as a whole all the separate facts ascertained at

a given time, so that successive tentative descriptions of a phenomenon, got by guessing till a guess is found which tallies with the facts, may, though conflicting (e.g. the theories respecting the motions of the heavenly bodies), be *all correct so far as they go*. Induction is proof, the inferring something unobserved from something observed; and to provide a proper test of proof is the special purpose of inductive logic. But colligation simply sums up the facts observed, as seen under a new point of view. Dr. Whewell contends that, besides the sum of the facts, colligation introduces, as a principle of connection, a conception of the mind not existing in the facts. But, in fact, it is only because this conception is a copy of something in the facts, although our senses are too weak to recognise it directly, that the facts are rightly classed under the conception. The conception is often even got by abstraction from the facts which it colligates; but also when it is a hypothesis, borrowed from strange phenomena, it still is accepted as true only because found actually, and as a fact, whatever the origin of the knowledge of the fact, to fit and to describe as a whole the separate observations. Thus, though Kepler's consequent inference that, *because* the orbit of a planet is an ellipse, the planet would *continue* to revolve in that same ellipse, was an induction, his previous application of the conception of an ellipse, abstracted from other phenomena, to sum up his direct observations of the successive positions occupied by the different planets, and thus to describe their orbits, was no induction. It altered only the *predicate*, changing—The successive places of, e.g. Mars, are A, B, C, and so forth, into—The succes-

sive places of, e.g. Mars, are points in an ellipse: whereas induction always widens the *subject*.

CHAPTER III.

THE GROUND OF INDUCTION.

INDUCTION is generalisation from experience. It assumes, that whatever is true in any one case, is true in all cases of a certain description, whether past, present, or future (and not merely in future cases, as is wrongly implied in the statement by Reid's and Stewart's school, that the principle of induction is 'our intuitive conviction that the future will resemble the past'). It assumes, in short, that the course of nature is uniform, that is, that all things take place according to general laws. But this general axiom of induction, though by it were discovered the obscure laws of nature, is no explanation of the inductive process, but is itself an induction (not, as some think, an intuitive principle which experience *verifies* only), and is arrived at after many separate phenomena have been first observed to take place according to general laws. It does not, then, *prove* all other inductions. But it is a *condition* of their proof. For any induction can be turned into a syllogism by supplying a major premiss, viz. What is true of this, that, &c. is true of the whole class; and the process by which we arrive at this immediate major may be itself represented by another syllogism or train of syllogisms, the major of the ultimate syllogism, and which therefore is the warrant for the immediate

major, being this axiom, viz. that there is uniformity, at all events, in the class of phenomena to which the induction relates, and a uniformity which, if not foreknown, may now be known.

But though the course of nature is uniform, it is also infinitely various. Hence there is no certainty in the induction in use with the ancients, and all non-scientific men, and which Bacon attacked, viz. ‘Inductio per enumerationem simplicem, ubi non reperitur instantia contradictoria’—*unless*, as in a few cases, we must have known of the contradictory instances if existing. The scientific theory of induction alone can show why a general law of nature may sometimes, as when the chemist first discovers the existence and properties of a before unknown substance, be inferred from a single instance, and sometimes (e.g. the blackness of all crows) not from a million.

CHAPTER IV.

LAWS OF NATURE.

THE uniformity of the course of nature is a complex fact made up of all the separate uniformities in respect to single phenomena. Each of these separate uniformities, if it be not a mere case of and result from others, is a law of nature; for, though *law* is used for any general proposition expressing a uniformity, *law of nature* is restricted to cases where it has been thought that a separate act of creative will is necessary to account for the uniformity. Laws of nature, in the aggregate, are the fewest general

propositions from which all the uniformities in the universe might be deduced. Science is ever tending to resolve one law into a higher. Thus, Kepler's three propositions, since having been resolved by Newton into, and shown to be cases of the three laws of motion, may be indeed called laws, but not laws of nature.

Since every correct inductive generalisation is either a law of nature, or a result from one, the problem of inductive logic is to unravel the web of nature, tracing each thread separately, with the view, 1, of ascertaining what are the *several* laws of nature, and, 2, of following them into their results. But it is impossible to frame a scientific method of induction, or test of inductions, unless, unlike Descartes, we start with the hypothesis that some trustworthy inductions have been already ascertained by man's involuntary observation. These spontaneous generalisations must be revised; and the same principle which common sense has employed to revise them, correcting the narrower by the wider (for, in the end, experience must be its own test), serves also, only made more precise, as the real type of scientific induction. As preliminary to the employment of this test, nature must be surveyed, that we may discover which are respectively the invariable and the variable inductions at which man has already arrived unscientifically. Then, by connecting these different ascertained inductions with one another through ratiocination, they become mutually confirmative, the strongest being made still stronger when bound up with the weaker, and the weakest at least as strong as the weakest of those from which they are deduced (as in the case of the Torricellian experiment) while those leading deductively to

incompatible consequences become each other's test, showing that one must be given up (e.g. the old farmers' bad induction that seed never throve if not sown during the increase of the moon). It is because a survey of the uniformities ascertained to exist in nature makes it clear that there are certain and universal uniformities serving as premisses whence crowds of lower inductions may be deduced, and so be raised to the same degree of certainty, that a logic of induction is possible.

CHAPTER V.

THE LAW OF UNIVERSAL CAUSATION.

PHENOMENA in nature stand to each other in two relations, that of simultaneity, and that of succession. On a knowledge of the truths respecting the succession of facts depends our power of predicting and influencing the future. The object, therefore, must be to find some law of succession not liable to be defeated or suspended by any change of circumstances, by being tested by, and deduced from which law, all other uniformities of succession may be raised to equal certainty. Such a law is not to be found in the class of laws of number or of space; for though these are certain and universal, no laws except those of space and number can be deduced from them by themselves (however important *elements* they may be in the ascertainment of uniformities of succession). But causation is such a law; and of this, moreover, all cases of succession whatever are examples.

This *Law of Causation* implies no particular theory as to the ultimate production of effects by *efficient* causes, but simply implies the existence of an invariable order of succession (on our assurance of which the validity of the canons of inductive logic depends) found by observation, or, when not yet observed, believed, to obtain between an invariable antecedent, i.e. the *physical* cause, and an invariable consequent, the effect. This sequence is generally between a consequent and the *sum* of several antecedents. The cause is really the sum total of the conditions, positive and negative; the negative being stated as one condition, the same always, viz. the absence of counteracting causes (since one cause generally counteracts another by the same law whereby it produces its own effects, and, therefore, the particular mode in which it counteracts another may be classed under the positive causes). But it is usual, even with men of science, to reserve the name *cause* for an antecedent *event* which completes the assemblage of conditions, and begins to exist immediately before the effect (e.g. in the case of death from a fall, the slipping of the foot, and not the weight of the body), and to style the permanent facts or *states*, which, though existing immediately before, have also existed long previously, the *conditions*. But indeed, popularly, any condition which the hearer is least likely to be aware of, or which needs to be dwelt upon with reference to the particular occasion, will be selected as the cause, even a negative condition (e.g. the sentinel's absence from his post, as the cause of a surprise), though from a mere negation no consequence can really proceed.

On the other hand, the object which is popularly regarded as standing in the relation of *patient*, and as being the mere theatre of the effect, is never styled *cause*, being included in the phrase describing the effect, viz. as the object, of which the effect is a *state*. But really these so-called *patients* are themselves agents, and their properties are positive conditions of the effect. Thus, the death of a man who has taken prussic acid is as directly the effect of the organic properties of the man, i.e. the *patient*, as of the poison, i.e. the *agent*.

To be a cause, it is not enough that the sequence *has been* invariable. Otherwise, night might be called the cause of day; whereas it is not even a condition of it. Such relations of succession or co-existence, as the succession of day and night (which Dr. Whewell contrasts as *laws of phenomena* with *causes*, though, indeed, the latter also are laws of phenomena, only more universal ones), result from the coexistence of real causes. The causes themselves are followed by their effects, not only invariably, but also *necessarily*, i.e. *unconditionally*, or subject to none but negative conditions. *This* is material to the notion of a cause. But another question is not material, viz. whether causes *must* precede, or may, at times, be simultaneous with (they certainly are never preceded by) their effects. In some, though not in all cases, the causes do invariably continue *together with* their effects, in accordance with the schools' dogma, *Cessante causâ, cessat et effectus*; and the hypothesis that, in such cases, the effects are produced *afresh* at each instant by their cause, is only a verbal explanation. But the question

does not affect the theory of causation, which remains intact, even if (in order to take in cases of simultaneity of cause and effect) we have to define a cause, as the assemblage of phenomena, which occurring, some other phenomenon invariably and unconditionally commences, or has its origin.

There exist certain original natural agents, called permanent causes (some being objects, e.g. the earth, air, and sun; others, cycles of events, e.g. the rotation of the earth), which together make up nature. All other phenomena are immediate or remote effects of these causes. Consequently, as the state of the universe at one instant is the consequence of its state at the previous instant, a person (but only if of more than human powers of calculation, and subject also to the possibility of the order being changed by a new volition of a supreme power) might predict the whole future order of the universe, if he knew the original distribution of all the permanent causes, with the laws of the succession between each of them and its different mutually independent effects. But, in fact, the distribution of these permanent causes, with the reason for the proportions in which they coexist, has not been reduced to a law; and this is why the sequences or coexistences among the effects of several of them together cannot rank as laws of nature, though they are invariable while the causes coexist. For this same reason (since the proximate causes are traceable ultimately to permanent causes) there are no original and independent uniformities of coexistence between effects of different (proximate) causes, though there may be such between different effects of the same cause.

Some, and particularly Reid, have regarded man's voluntary agency as the true type of causation and the exclusive source of the idea. The facts of inanimate nature, they argue, exhibit only antecedence and sequence, while in volition (and this would distinguish it from physical causes) we are conscious, prior to experience, of power to produce effects: volition, therefore, whether of men or of God, must be, they contend, an efficient cause, and the only one, of all phenomena. But, in fact, they bring no positive evidence to show that we could have known, apart from experience, that the effect, e.g. the motion of the limbs, would follow from the volition, or that a volition is more than a physical cause. In lieu of positive evidence, they appeal to the supposed conceivableness of the direct action of will on matter, and inconceivableness of the direct action of matter on matter. But there is no inherent law, to this effect, of the conceptive faculty: it is only because our voluntary acts are, from the first, the most direct and familiar to us of all cases of causation, that men, as is seen from the structure of languages (e.g. their active and passive voices, and impersonations of inanimate objects), get the *habit* of borrowing them to explain other phenomena by a sort of original Fetichism. Even Reid allows that there is a tendency to assume volition where it does not exist, and that the belief in it has its sphere gradually limited, in proportion as fixed laws of succession among external objects are discovered.

This proneness to require the appearance of some necessary and natural connection between the cause and its effect, i.e. some reason *per se* why the one

should produce the other, has infected most theories of causation. But the selection of the particular agency which is to make the connection between the physical antecedent and its consequent seem *conceivable*, has perpetually varied, since it depends on a person's special habits of thought. Thus, the Greeks, Thales, Anaximenes, and Pythagoras, thought respectively that water, air, or number is such an agency explaining the production of physical effects. Many moderns, again, have been unable to *conceive* the production of effects by volition itself, without some intervening agency to connect it with them. This medium, Leibnitz thought, was some *per se* efficient physical antecedent; while the Cartesians imagined for the purpose the theory of Occasional Causes, that is, supposed that God, not *quâ* mind, or *quâ* volition, but *quâ* omnipotent, intervenes to connect the volition and the motion: so far is the mind from being forced to think the action of mind on matter more *natural* than that of matter on matter. Those who believe volition to be an efficient cause are guilty of exactly the same error as the Greeks, or Leibnitz or Descartes; that is, of requiring an *explanation* of physical sequences by something *ἀνευ οὐ τὸ αἴτιον οὐκ ἄν ποτ' εἴη αἴτιον*. But they are guilty of another error also, in inferring that volition, even if it is an *efficient* cause of so peculiar a phenomenon as nervous action, must therefore be the efficient cause of all other phenomena, though having scarcely a single circumstance in common with them.

CHAPTER VI.

THE COMPOSITION OF CAUSES.

AN effect is almost always the result of the concurrence of several causes. When all have their full effect, precisely as if they had operated *successively*, the joint effect (and it is not inconsistent to give the name of *joint effect* even to the mutual obliteration of the separate ones) may be *deduced* from the laws which govern the causes when acting separately. Sciences in which, as in mechanics, this principle, viz. the *composition of causes*, prevails, are deductive and demonstrative. Phenomena, in effect, do generally follow this principle. But in some classes, e.g. chemical, vital, and mental phenomena, the laws of the elements when called on to work together, cease and give place to others, so that the joint effect is not the sum of the separate effects. Yet even here the more general principle is exemplified. For the new *heteropathic* laws, besides that they never supersede *all* the old laws (thus, The weight of a chemical compound is equal to the sum of the weight of the elements), have been often found, especially in the case of vital and mental phenomena, to enter *unaltered* into composition with one another, so that complex facts may thus be *deducible* from comparatively simple laws. It is even possible that, as has been already partly effected by Dalton's law of definite proportions, and the law of isomorphism, chemistry itself, which is now the least deductive of sciences, may be made deductive, through the laws of the combinations being ascertained to be, though

not compounded of the laws of the separate agencies, yet derived from them according to a fixed principle.

The proposition, that effects are proportional to their causes, is sometimes laid down as an independent axiom of causation : it is really only a particular case of the composition of causes ; and it fails at the same point as the latter principle, viz. when an addition does not become compounded with the original cause, but the two together generate a new phenomenon.

CHAPTER VII.

OBSERVATION AND EXPERIMENT.

SINCE the whole of the present facts are the infallible result of the whole of the past, so that if the prior state of the entire universe could recur it would be followed by the present, the process of ascertaining the relations of cause and effect is an analysis or resolution of this complex uniformity into the simpler uniformities which make it up. We must first mentally analyse the facts, not making this analysis minuter than is needed for our object at the time, but at the same time not regarding (as did the Greeks their verbal classifications) a mental decomposition of facts as ultimate. When we have thus succeeded in looking at any two successive chaotic masses (for such nature keeps at each instant presenting to us) as so many distinct antecedents and consequents, we must analyse the facts themselves, and try, by varying the circumstances, to discover

which of the antecedents and consequents (for many are always present together) are related to each other.

Experiment and observation are the two instruments for thus varying the circumstances. When the enquiry is, What are the effects of a given cause? experiment is far the superior, since it enables us not merely to produce many more and more opportune variations than nature, which is not arranged on the plan of facilitating our studies, offers spontaneously, but, what is a greater advantage, though one less attended to, also to insulate the phenomenon by placing it among known circumstances, which can be then infinitely varied by introducing a succession of well-defined new ones.

Observation cannot ascertain the effects of a given cause, because it cannot, except in the simplest cases, discover what are the concomitant circumstances; and therefore sciences in which experiment cannot be used, either at all, as in astronomy, or commonly, as in mental and social science, must be mainly deductive, not inductive. When, however, the object is to discover causes by means of their effects, observation alone is primarily available, since new effects could be artificially produced only through their causes, and these are, in the supposed case, unknown. But even then observation by itself cannot directly discover causes, as appears from the case of zoology, which yet contains many recognised uniformities. We have, indeed, ascertained a real uniformity when we observe some one antecedent to be invariably found along with the effects presented by nature. But it is only by reversing the process, and experi-

mentally producing the effects by means of that antecedent, that we can prove it to be unconditional, i.e. the cause.

CHAPTER VIII. AND NOTE TO CHAPTER IX.*

THE FOUR METHODS OF EXPERIMENTAL ENQUIRY.

FIVE canons may be laid down as the principles of experimental enquiry. The first is that of the Method of Agreement, viz. : *If two or more instances of the phenomenon under investigation have only one circumstance in common, the circumstance in which alone all the circumstances agree is the cause or the effect of the given phenomenon.* The second canon is that of the Method of Difference, viz. : *If an instance in which the phenomenon occurs and an instance in which it does not occur have every circumstance in common, save one, and that one occurs only in the former, that one circumstance is the effect, or the cause, or a necessary part of the cause, of the phenomenon.*

These two are the simplest modes of singling out from the facts which precede or follow a phenomenon, those with which it is connected by an invariable law. Both are methods of elimination, their basis being, for the method of agreement, that whatever can be eliminated *is not*, and for that of difference, that whatever cannot be eliminated *is* connected with the given phenomenon by a law. It is only, however,

* Chap. IX. consists of 'Miscellaneous Examples of the Four Methods,' which cannot be well represented in an abridged form.

by the method of difference, which is a method of artificial experiment (and by experiment we can introduce into the pre-existing facts a change perfectly definite), that we can, at least by direct experience, arrive with certainty at causes. The method of agreement is chiefly useful as preliminary to and suggestive of applications of the method of difference, or as an inferior resource in its stead, when, as in the case of many spontaneous operations of nature, we have no power of producing the phenomenon.

When we have power to produce the phenomenon, but only by the agency, not of a single antecedent, but of a combination, the method of agreement can be improved (though it is even then inferior to the direct method of difference) by a double process being used, each proof being independent and corroborative of the other. This may be called the Indirect Method of Difference, or the Joint Method of Agreement and Difference, and its canon will be: *If two or more instances in which the phenomenon occurs have only one circumstance in common, while two or more instances in which it does not occur have nothing in common save the absence of that circumstance, the circumstance in which alone the two sets of instances differ, is the effect, or the cause, or a necessary part of the cause, of the phenomenon.*

The fourth canon is that of the Method of Residues, viz.: *Subduct from any phenomenon such part as is known by previous inductions to be the effect of certain antecedents, and the residue of the phenomenon is the effect of the remaining antecedents.* This method is a modification of the method of

difference, from which it differs in obtaining, of the two required instances, only the positive instance, by observation or experiment, but the negative, by deduction. Its certainty, therefore, in any given case, is conditional on the previous inductions having been obtained by the method of difference, and on there being in reality no remaining antecedents *besides* those given as such.

The fifth canon is that of the Method of Concomitant Variations, viz.: *Whatever phenomenon varies in any manner whenever another phenomenon varies in some particular manner, is either a cause or an effect of that phenomenon, or (since they may be effects of a common cause) is connected with it through some fact of causation.* Through this method alone can we find the laws of the permanent causes. For, though those of the permanent causes whose influence is local may be escaped from by changing the scene of the observation or experiment, many can neither be excluded nor even kept isolated from each other; and, therefore, in such cases, the method of difference, which requires a negative instance, and that of agreement, which requires the different instances to agree only in one circumstance, in order to prove causation, are (together with the methods which are merely forms of these) equally inapplicable. But, though many permanent antecedents insist on being always present, and never present alone, yet we have the resource of making or finding instances in which (the accompanying antecedents remaining unchanged) their influence is *varied* and *modified*. This method can be used most effectually when the variations of the cause are

variations of quantity; and then, if we know the absolute quantities of the cause and the effect, we may affirm generally that, at least within our limits of observation, the variations of the cause will be attended by similar variations of the effect; it being a corollary from the principle of the composition of causes, that more of the cause is followed by more of the effect. This method is employed usually when the method of difference is impossible; but it is also of use to determine according to what law the quantity or different relations of an effect ascertained by the method of difference follow those of the cause.

These four methods are the only possible modes of experimental enquiry. Dr. Whewell attacks them, first, on the ground (and the canon of ratiocination was attacked on the same) that they assume the reduction of an argument to formulæ, which (with the procuring the evidence) is itself the chief difficulty. And this is in truth the case: but, to reduce an argument to a particular form, we must first know what the form is; and in showing us this, Inductive Logic does a service the value of which is tested by the number of faulty inductions in vogue. Dr. Whewell next implies a complaint that no discoveries have ever been made by these four methods. But, as the analogous argument against the syllogism was invalidated by applying equally as against all reasoning, which must be reducible to syllogism, so this also falls by its own generality, since, if true against these methods, it must be true against all observation and experiment, since these must ever proceed by one of the four. And, moreover, even if the four

methods were not methods of discovery, as they are, they would yet be subjects for logic, as being, at all events, the sole methods of Proof, which (unless Dr. Whewell be correct in his view that inductions are simply conceptions consistent with the facts they colligate) is the principal topic of logic.

CHAPTER X.

PLURALITY OF CAUSES, AND INTERMIXTURE OF EFFECTS.

THE difficulty in tracing the laws of nature arises chiefly from the Intermixture of Effects, and from the Plurality of Causes. The possibility of the latter in any given case—that is, the possibility that the same effect may have been produced by different causes—makes the Method of Agreement (when applied to positive instances) inconclusive, if the instances are few; for that Method involves a tacit supposition, that the same effect in different instances, which have *one* common antecedent, must follow in all from the same cause, viz. from their common antecedent. When the instances are varied and very many (how many, it is for the Theory of Probability to consider), the supposition, that the presence in all of the common antecedent may be simply a coincidence, is rebutted; and this is the sole reason why mere *number* of instances, differing only in immaterial points, is of any value. As applied, indeed, to negative instances, i.e. to those resembling each other in the absence of a certain circumstance, the Method of Agreement

is not vitiated by Plurality of Causes. But the negative premiss cannot generally be worked unless an affirmative be joined with it: and then the Method is the Joint Method of Agreement and Difference. Thus, to find the cause of Transparency, we do not enquire in what circumstance the numberless *non-transparent* objects agree; but we enquire, first, in what the few transparent ones agree; and then, whether all the opaque do not agree in the *absence* of this circumstance.

Not only may there be Plurality of Causes, the whole of the effect being produced now by one, now by another antecedent; but there may also be Intermixture of Effects, through the interference of different causes with each other, so that part of the total effect is due to one, and part to another cause. This latter contingency, which, more than all else, complicates the study of nature, does not affect the enquiry into those (the exceptional) cases, where, as in chemistry, the total effect is something quite different to the separate effects, and governed by different laws. There the great problem is to discover, not the properties, but the cause of the new phenomenon, i.e. the particular conjunction of agents whence it results; which could indeed never be ascertained by specific enquiry, were it not for the peculiarity, not of all these cases (e.g. not of mental phenomena), but of many, viz. that the heterogeneous effects of combined causes often reproduce, i.e. are *transformed into* their causes (as, e.g. water into its components, hydrogen and oxygen). The great difficulty is *not* there to discover the properties of the new phenomenon itself, for these can be found by experiment like the *simple*

effects of any other cause ; since, in this class of cases the effects of the separate causes give place to a new effect, and thereby cease to need consideration as separate effects. But in the far larger class of cases, viz. when the total effect is the exact sum of the separate effects of all the causes (the case of the Composition of Causes), at no point may it be overlooked that the effect is not simple but complex, the result of various separate causes, all of which are always tending to produce the whole of their several natural effects ; having, it may be, their *effects* modified, disturbed, or even prevented by each other, but always preserving their *action*, since laws of causation cannot have exceptions.

These complex effects must be investigated by *deducing* the law of the effect from the laws of the separate causes on the combination of which it depends. No inductive method is conclusive in such cases (e.g. in physiology, or *à fortiori*, in politics and history), whether it be the method of simple observation, which compares instances, whether positive or negative, to see if they agree in the presence or the absence of one common antecedent, or the empirical method, which proceeds by directly trying different combinations (either made or found) of causes, and watching what is the effect. Both are inconclusive ; the former, because an effect may be due to the concurrence of many causes, and the latter, because we can rarely know what all the coexisting causes are ; and still more rarely whether a certain portion (if not all) of the total effect is not due to these other causes, and not to the combination of causes which we are observing.

CHAPTER XI.

THE DEDUCTIVE METHOD.

THE deductive method is the main source of our knowledge of complex phenomena, and the sole source of all the theories through which vast and complicated facts have been embraced under a few simple laws. It consists of processes of Induction, Ratiocination, and Verification. First, by one of the four inductive methods, the simple laws (whence may be *deduced* the complex) of each separate cause which shares in producing the effect, must be first ascertained. This is difficult, when the causes or rather tendencies cannot be observed singly. Such is the case in physiology, since the different agencies which make up an organized body cannot be separated without destroying the phenomenon; consequently there our sole resource is to produce experimentally, or find (as in the case of diseases), pathological instances in which only one organ at a time is affected. Secondly, when the laws of the causes have been found, we calculate the effect of any given combination of them by ratiocination, which may have (though not necessarily) among its premisses the theorems of the sciences of number and geometry. Lastly, as it might happen that some of the many concurring agencies have been unknown or overlooked, the conclusions of ratiocination must be *verified*; that is, it must be explained why they do not, or shown that they do, accord with *observed* cases of at least equal complexity, and (which is the most effectual test) that the empirical laws and uniformities, if any, arrived at by direct observation,

can be deduced from and so accounted for by them, as, e.g. Kepler's laws of the celestial motions by Newton's theory.

CHAPTERS XII. AND XIII.

THE EXPLANATION AND EXAMPLES OF THE EXPLANATION OF LAWS OF NATURE.

THE aim, in the deductive method, is either to discover the law of the effect, or to account for it by *explaining* it, that is, by pointing out some more general phenomenon (though often less familiar to us) of which this is a case and a partial exemplification, or some laws of causation which produce it by their joint or successive action. This explanation may be made, either—1. By resolving the laws of the complex effect into its elements, which consist as well of the separate laws of the causes which share in producing it, as also of their collocation, i.e. the fact that these separate laws have been so combined; or—2. By resolving the law which connects two links, not proximate, in a chain of causation, into the laws which connect each link with the intermediate links; or—3. By the *subsumption* or gathering up of several laws under one which amounts to the sum of them all, and which is the recognition of the same sequence in different sets of instances. In the first two of the processes, laws are resolved into others, which both extend to more cases, i.e. are more *general*, and also, as being laws of nature, of which the complex laws are but results, are more *certain*, i.e. more *unconditional*

and more *universally true*. In the third process, laws are resolved into others which are indeed more *general*, but not more *certain*, since they are in fact the same laws, and therefore, subject to the same exceptions.

Liebig's researches, e.g. into the Contagious Influence of Chemical Action, and his Theory of Respiration, are among the finest examples, since Newton's exposition of the law of gravitation, of the use of the deductive method for *explanation*.* But the method is as available for explaining mental as physical facts. It is destined to predominate in philosophy. Before Bacon's time deductions were accepted as sufficient, when neither had the premisses been established by proper canons of experimental enquiry, nor the results tested by verification by specific experience. He therefore changed the method of the sciences from deductive to experimental. But, now that the principles of deduction are better understood, it is rapidly reverting from experimental to deductive. Only it must not be supposed that the inductive part of the process is yet complete. Probably, few of the great generalisations fitted to be the premisses for future deductions will be found among truths now known. Some, doubtless, are yet unthought of; others known only as laws of some limited class of facts, as electricity once was. They will probably appear first in the shape of hypotheses, needing to be tested by canons of legitimate induction.

* These, and other illustrations in chap. xiii., cannot be usefully represented in an abridged form.

CHAPTER XIV.

THE LIMITS TO THE EXPLANATION OF LAWS OF NATURE.
HYPOTHESES.

THE constant tendency of science, operating by the Deductive Method, is to resolve all laws, even those which once seemed ultimate and not derivative, into others still more general. But no process of *resolving* will ever reduce the number of ultimate laws below the number of those varieties of our feelings which are distinguishable in quality, and not merely in quantity or degree. The *ideal* limit of the explanation of natural phenomena is to show that each of these ultimate facts has (since the differences in the different cases of it affect our sensations as differences in degree only, and not in quality) only one sort of cause or mode of production; and that all the seemingly different modes of production or causes of it are resolvable into one. But *practically* this limit is never attained. Thus, though various laws of Causes of Motion have been resolved into others (e.g. the fall of bodies to the earth, and the motions of the planets, into the one law of mutual attraction), many causes of it remain still unresolved and distinct.

Hypotheses are made for the sake of this resolving and explaining of laws. When we do not *know* of any more general laws into which to resolve an uniformity, we then (either on no or on insufficient evidence) *suppose* some, imagining either causes (as, e.g. Descartes did the Vortices), or the laws of their operation (as did Newton respecting the planetary central force); but we never feign both cause and law. The use of

a hypothesis is to enable us to apply the Deductive Method before the laws of the causes have been ascertained by Induction. In those cases where a false law could not have led to a true result (as was the case with Newton's hypothesis as to the law of the Attractive force) the third part of the process in the Deductive Method, viz. Verification, which shows that the results deduced are true, amounts to a complete induction, and one conforming to the canon of the Method of Difference. But this is the case only when either the cause is known to be one given agent (and only its law is unknown), or to be one of several given agents.

An assumed cause, on the other hand, cannot be accepted as true simply *because* it explains the phenomena (since two conflicting hypotheses often do this even originally, or, as Dr. Whewell himself allows, may at any rate by modifications be made to do it); nor *because* it moreover leads to the prediction of other results which turn out true (since this shows only what was indeed apparent already from its agreement with the old facts, viz. that the phenomena are governed by laws partially identical with the laws of other causes); nor *because* we cannot imagine any other hypothesis which will account for the facts (since there may be causes unknown to our present experience which will equally account for them). The utility of such assumptions of *causes* depends on their being, in their own nature, *capable* (as Descartes' Vortices were not, though possibly the Luminiferous Ether may be) of being, at some time or other, proved directly by independent evidence to be the causes. And this was, perhaps, all that Newton meant

by his *veræ causæ*, which alone, he said, may be assigned as causes of phenomena. Assumptions of causes, which fulfil this condition, are, in science, even indispensable, with a view both to experimental inquiry, and still more to the application of the Deductive Method. They may be accepted, not indeed, as Dr. Whewell thinks they may be, as proof, but as suggesting a line of experiment and observation which may result in proof. And this is actually the method used by practical men for eliciting the truth from involved statements. They first extemporise, from a few of the particulars, a rude theory of the mode in which the event happened; and then keep altering it to square with the rest of the facts, which they review one by one.

The attempting, as in Geology, to conjecture, in conformity with known laws, in what former collocations of known agents (though *not* known to have been formerly present) individual existing facts may have originated, is not Hypothesis but Induction; for then we do not *suppose* causes, but legitimately infer from known effects to unknown causes. Of this nature was Laplace's theory, whether weak or not, as to the origin of the earth and planets.

CHAPTER XV.

PROGRESSIVE EFFECTS, AND CONTINUED ACTION OF CAUSES.

SOMETIMES a complex effect results, not (as has been supposed in the last four chapters) from several, but from *one* law. The following is the way.

Some effects are instantaneous (e.g. some sensa-

tions), and are prolonged only by the prolongation of the causes; others are in their own nature permanent. In some cases of the latter class, the original is also the proximate cause (e.g. Exposure to moist air is both the original and the proximate cause of iron rust). But in others of the same class, the permanency of the effect is only the permanency of a series of changes. Thus, e.g. in cases of Motion, the original force is only the *remote* cause of any link (after the very first) in the series; and the motion immediately preceding it, being itself a compound of the original force and any retarding agent, is its *proximate* cause. When the original cause is permanent as well as the effect (e.g. Suppose a continuance of the iron's exposure to moist air), we get a progressive series of effects arising from the cause's accumulating influence; and the sum of these effects amounts exactly to what a number of successively introduced similar causes would have produced. Such cases fall under the head of Composition of Causes, with this peculiarity, that, as the causes (to regard them as plural) do not come into play all at once, the effect at each instant is the sum of the effects only of the then acting causes, and the result will appear as an ascending series. Each addition in such case takes place according to a fixed law (equal quantities in equal times); and therefore it can be computed deductively. Even when, as is sometimes the case, a cause is at once permanent and progressive (as, e.g. the sun, by its position becoming more vertical, increases the heat in summer) so that the quantities added are unequal, the effect is still progressive, resulting from its cause's continuance and progressiveness combined.

In *all* cases whatever of progressive effects, the succession not merely between the cause and the effect, but also between the first and latter stages of the effect, is uniform. Hence, from the invariable sequence of two terms (e.g. Spring and Summer) in a series going through any continued and uniform process of variation, we do not presume that one is the cause and the others the effect, but rather that the whole series is an effect.

CHAPTER XVI.

EMPIRICAL LAWS.

EMPIRICAL laws are derivative laws, of which the derivation is not known. They are observed uniformities, which we compare with the result of any deduction to verify it; but of which the *why*, and also the limits, are unrevealed, through their being, though resolvable, not yet resolved into the simpler laws. They depend usually, not solely on the ultimate laws into which they are resolvable; but on those, together with an ultimate fact, viz. the mode of coexistence of some of the component elements of the universe. Hence their untrustworthiness for scientific purposes; for, till they have been resolved (and then a derivative law ceases to be empirical), we cannot know whether they result from the different effects of one cause, or from effects of different causes; that is, whether they depend on laws, or on laws and a collocation. And if they thus depend on a collocation, they can be received as true only within the limits of time and

space, and also circumstance, in which they have been observed, since the mode of the collocation of the permanent causes is not reducible to a law, there being no principle known to us as governing the distribution and relative proportions of the primæval natural agents.

Uniformities cannot be proved by the Method of Agreement alone to be laws of causation; they must be tested by the Method of Difference, or explained deductively. But laws of causation themselves are either ultimate or derivative. Signs, previous to actual proof by *resolution* of them, of their being derivative, are, either that we can *surmise* the existence of a link between the known antecedent and the consequent, as e.g. in the laws of chemical action; or, that the antecedent is some very complex fact, the effects of which are probably (since most complex cases fall under the Composition of Causes) compounded of the effects of its different elements. But the laws which, though laws of causation, are thus presumably derivative laws only, need, equally with the uniformities which are not known to be laws of causation at all, to be explained by deduction (which they then in turn verify), and are less *certain* than when they have been resolved into the ultimate laws. Consequently they come under the definition of *Empirical Laws*, equally with uniformities not known to be laws of causation. However, the latter are far more *uncertain*; for as, till they are resolved, we cannot tell on how many collocations, as well as laws, they may not depend, we must not rely on them beyond the exact limits in which the observations were made. Therefore, the name *Empirical Laws* will generally be confined here to these.

CHAPTER XVII.

CHANCE, AND ITS ELIMINATION.

EMPIRICAL LAWS are certain only in those limits within which they have been *observed* to be true. But, even within those limits, the connection of two phenomena may, as the same effect may be produced by several different causes, be due to Chance; that is, it may, though being, as all facts must be, the result of some law, be a coincidence whence, simply because we do not know all the circumstances, *we* have no ground to infer an uniformity. When neither Deduction, nor the Method of Difference, can be applied, the only way of inferring that coincidences are not casual, is by observing the frequency of their occurrence, not their absolute frequency, but whether they occur *more* often than chance would (that is, more often than the positive frequency of the phenomena would) account for. If, in such cases, we could ascend to the causes of the two phenomena, we should find at some stage some cause or causes common to both. Till we can do this, the fact of the connection between them is only an empirical law; but still it is a law.

Sometimes an effect is the result partly of chance, and partly of law: viz. when the total effect is the result partly of the effects of casual conjunctions of causes, and partly of the effects of some constant cause which they blend with and modify. This is a case of Composition of Causes. The object being to find *how much* of the result is attributable to a given

constant cause, the only resource, when the variable causes cannot be wholly excluded from the experiment, is to ascertain what is the effect of all of *them* taken together, and then to eliminate this, which is the casual part of the effect, in reckoning up the results. If the results of frequent experiments, in which the constant cause is kept invariable, oscillate round one point, that average or middle point is due to the constant cause, and the variable remainder to chance; that is, to causes the coexistence of which with the constant cause was merely casual. The test of the sufficiency of such an induction is, whether or not an increase in the number of experiments materially alters the average.

We can thus discover not merely *how much* of the effect, but even whether *any* part of it whatever is due to a constant cause, when this latter is so unimportant as otherwise to escape notice (e.g. the loading of dice). This case of the Elimination of Chance is called *The discovery of a residual phenomenon by eliminating the effects of chance*.

The mathematical doctrine of chances, or Theory of Probabilities, considers what deviation from the average chance by itself can possibly occasion in some number of instances smaller than is required for a fair average.

CHAPTER XVIII.

THE CALCULATION OF CHANCES.

IN order to calculate chances, we must know that of several events one, and no more, must happen, and also not know, or have any reason to suspect, which of them that one will be. Thus, with the simple knowledge that the issue must be one of a certain number of possibilities, we *may* conclude that one supposition is most probable *to us*. For this purpose it is not *necessary* that specific experience or reason should have also proved the occurrence of each of the several events to be, as a fact, equally frequent. For, the probability of an event is not a quality of the event (since every event is in itself certain), but is merely a name for the degree of ground *we* have, with our present evidence, for expecting it. Thus, if we know that a box contains red, white, and black balls, though we do not know in what proportions they are mingled, we have numerically appreciable grounds for considering the probability to be two to one against any one colour. Our judgment may indeed be said in this case to rest on the experience we have of the laws governing the frequency of occurrence of the different cases; but such experience is universal and axiomatic, and not specific experience about a particular event. Except, however, in games of chance, the purpose of which requires ignorance, such specific experience can generally be, and should be gained. And a slight improvement in the data profits more than the most elaborate application of the calculus of probabilities to the bare original data,

e.g. to such data, when we are calculating the credibility of a witness, as the proportion, even if it could be verified, between the number of true and of erroneous statements a man, *quâ* man, may be supposed to make during his life. Before applying the Doctrine of Chance, therefore, we should lay a foundation for an evaluation of the chances by gaining positive knowledge of the facts. Hence, though not a *necessary*, yet a most usual condition for calculating the probability of a fact is, that we should possess a *specific* knowledge of the proportion which the cases in which facts of the particular sort occur bear to the cases in which they do not occur.

Inferences drawn correctly according to the Doctrine of Chances depend ultimately on causation. This is clearest, when, as sometimes, the probability of an event is deduced from the frequency of the occurrence of the causes. When its probability is calculated by merely counting and comparing the number of cases in which it has occurred with those in which it has not, the law, being arrived at by the Method of Agreement, is only empirical. But even when, as indeed generally, the numerical data are obtained in the latter way (since usually we can judge of the frequency of the causes only through the medium of the empirical law, which is based on the frequency of the effects), still then, too, the inference really depends on causation alone. Thus, an actuary infers from his tables that, of any hundred living persons under like conditions, five will reach a given age, not simply because that proportion have reached it in times past, but because that fact shows the existence there of a particular proportion between

the *causes* which shorten and the causes which prolong life to the given extent.

CHAPTER XIX.

THE EXTENSION OF DERIVATIVE LAWS TO ADJACENT CASES.

DERIVATIVE LAWS are inferior to ultimate laws, both in the extent of the propositions, and in their degree of certainty within that extent. In particular, the uniformities of coexistence and sequence which obtain between effects depending on different primæval causes, vary along with any variation in the collocation of these causes. Even when the derivative uniformity is between different effects of the same cause, it cannot be trusted to, since one or more of the effects may be producible by another cause also. The effects, even, of derivative laws of *causation* (resulting, i.e. the laws, from the combination of several causes) are not independent of collocations; for, though laws of causation, whether ultimate or derivative, are themselves universal, being fulfilled even when counteracted, the peculiar probability of the latter kind of laws of causation being counteracted (as compared with ultimate laws, which are liable to frustration only from one set of counteracting causes) is fatal to the universality of the derivative uniformities made up of the sequences or coexistences of their effects; and, therefore, such derivative uniformities as the latter are to be relied on only when the collocations are known not to have changed.

Derivative laws, not causative, may certainly be extended beyond the limits of observation, but only to cases *adjacent* in time. Thus, we may not predict that the sun will rise this day 20,000 years, but we can predict that it will rise to-morrow, on the ground that it has risen every day for the last 5,000 years. The latter prediction is lawful, *because*, while we know the causes on which its rising depends, we know, also, that there has existed hitherto no perceptible cause to counteract them; and that it is opposed to experience that a cause imperceptible for so long should start into immensity in a day. If the uniformity is empirical only, that is, if we do not know the causes, and if we infer that they remain uncounteracted from their effects alone, we still can extend the law to adjacent cases, but only to cases still more closely adjacent in time; since we can know neither whether changes in these unknown causes may not have occurred, nor whether there may not exist now an adverse cause capable after a time of counteracting them.

An empirical law cannot generally be extended, in reference to *Place*, even to adjacent cases (since there is no uniformity in the collocations of primæval causes). Such an extension is lawful only if the new cases are *presumably* within the influence of the same individual causes, even though unknown. When, however, the causes are known, and the conjunction of the effects is deducible from laws of the causes, the derivative uniformity may be extended over a wider space, and with less abatement for the chance of counteracting causes.

CHAPTER XX.

ANALOGY.

ONE of the many meanings of *Analogy* is, Resemblance of Relations. The value of an analogical argument in this sense depends on the showing that, on the common circumstance which is the *fundamentum relationis*, the rest of the circumstances of the case depend. But, generally, to *argue from analogy* signifies to infer from resemblance in some points (not necessarily in *relations*) resemblance in others. Induction does the same: but analogy differs from induction in not requiring the previous proof, by comparison of instances, of the invariable conjunction between the known and the unknown properties; though it requires that the latter should not have been ascertained to be *unconnected* with the common properties.

If a fair proportion of the properties of the two cases are known, every resemblance affords ground for expecting an indefinite number of other resemblances, among which the property in question may perhaps be found. On the other hand, every dissimilarity will lead us to expect that the two cases differ in an indefinite number of properties, including, perhaps, the one in question. These dissimilarities may even be such as would, in regard to one of the two cases, imply the absence of that property; and then every resemblance, as showing that the two cases have a similar nature, is even a reason for presuming against the presence of that property. Hence, the value of an analogical argument depends

on the extent of ascertained resemblance as compared, first, with the amount of ascertained difference, and next, with the extent of the unexplored region of unascertained properties.

The conclusions of analogy are not of direct use, unless when the case to which we reason is a case *adjacent*, not, as before, in time or place, but in *circumstances*. Even then a complete induction should be sought after. But the great value of analogy, even when faint, in science, is that it may suggest observations and experiments, with a view to establishing positive scientific truths, for which, however, the hypotheses based on analogies must never be mistaken.

CHAPTER XXI.

THE EVIDENCE OF THE LAW OF UNIVERSAL CAUSATION.

THE validity of all the four inductive methods depends on our assuming that there is a cause for every event. The belief in this, i.e. in the law of universal causation, some affirm, is an instinct which needs no warrant other than all men's disposition to believe it; and they argue that to demand evidence of it is to appeal to the intellect from the intellect. But, though there is no appeal from the faculties all together, there may be from one to another: and, as belief is not proof (for it may be generated by association of ideas as well as by evidence), a case of belief does require to be proved by an appeal to something else, viz. to the faculties of sense and consciousness.

The law of universal causation is, in fact, a generalisation from many partial uniformities of sequence. Consequently, like these, which cannot have been arrived at by any strict inductive method (for all such methods presuppose the law of causation itself), it must itself be based on inductions *per simplicem enumerationem*, that is, generalisations of observed facts, from the mere absence of any known instances to the contrary. This unscientific process is, it is true, usually delusive; but only because, and in proportion as, the subject-matter of the observation is limited in extent. Its results, whenever the number of coincidences is too large for chance to explain, are empirical laws. These are ordinarily true only within certain limits of time, place, and circumstance, since, beyond these, there may be different collocations or counteracting agencies. But the subject-matter of the law of universal causation is so diffused that there is no time, place, or set of circumstances, at least within the portion of the universe within our observation, and adjacent cases, but must prove the law to be either true or false. It has, in fact, never been found to be false, but in ever increasing multitudes of cases to be true; and phenomena, even when, from their rarity or inaccessibility, or the number of modifying causes, they are not reducible *universally* to any law, yet *in some instances* do conform to this. Thus, it may be regarded as coextensive with all human experience, at which point the distinction between empirical laws and laws of nature vanishes. Formerly, indeed, it was only a very high probability; but, with our modern experience, it is, practically, absolutely certain, and

it confirms the particular laws of causation, whence itself was drawn, when there seem to be exceptions to them. All narrower inductions got by simple enumeration are unsafe, till, by the application to them of the four methods, the supposition of their falsity is shown to contradict *this* law, though it was itself arrived at by simple enumeration.

CHAPTER XXII.

UNIFORMITIES OF COEXISTENCE NOT DEPENDENT ON CAUSATION.

BESIDES uniformities of succession, which always depend on causation, there are uniformities of coexistence. These also, whenever the coexisting phenomena are effects of causes, whether of one common cause or of several different causes, depend on the laws of their cause or causes; and, till resolved into these laws, are mere empirical laws. But there are some uniformities of coexistence, viz. those between the ultimate properties of *kinds*, which do not depend on causation, and therefore seem entitled to be classed as a peculiar sort of laws of nature. As, however, the presumption always is (except in the case of those *kinds* which are called *simple substances* or elementary natural agents), that a thing's properties really depend on causes though not traced, and we *never* can be certain that they do not; we cannot safely claim (though it *may* be an ultimate truth) higher certainty than that of an empirical law for any generalisation about coexistence, that is to say

(since *kinds* are known to us only by their properties, and, consequently, all assertions about them are assertions about the coexistence of something with those properties), about the properties of *kinds*.

Besides, no rigorous inductive system can be applied to the uniformities of coexistence, since there is no general axiom related to them, as is the law of causation to those of succession, to serve as a basis for such a system. Thus, Bacon's practical applications of his method failed, from his supposing that we can have previous certainty that a property must have an invariable coexistent (as it must have an invariable antecedent), which he called its form. He ought to have seen that his great logical instrument, elimination, is inapplicable to coexistences, since things which agree in having certain apparently ultimate properties, often agree in nothing else; even the properties which (e.g. Hotness) are effects of causes, generally being not connected with the ultimate resemblances or diversities in the objects, but depending on some outward circumstance.

Our only substitute for an universal law of coexistence is the ancients' induction *per enumerationem simplicem ubi non reperitur instantia contradictoria*, that is, the improbability that an exception, if any existed, could have hitherto remained unobserved. But the certainty thus arrived at can be only that of an empirical law, true within the limits of the observations. For the coexistent property must be either a property of the *kind*, or an accident, that is, something due to an extrinsic cause, and not to the *kind* (whose own indigenous properties are always the same). And the ancients'

class of induction can only prove that *within given limits*, either (in the latter case) one common, though unknown, cause has always been operating, or (in the former case) that no new *kind* of the object has *as yet* or *by us* been discovered.

The evidence is, of course (with respect both to the derivative and the ultimate uniformities of co-existence), stronger in proportion as the law is more general; for the greater the amount of experience from which it is derived, the more probable is it that counteracting causes, or that exceptions, if any, would have presented themselves. Consequently, it needs more evidence to establish an exception to a very general, than to a special, empirical law. And common usage agrees with this principle. Still, even the greater generalisations, when not based on connection by causation, are delusive, unless grounded on a separate examination of *each* of the included *infimæ species*, though certainly there is a probability (no more) that a sort of parallelism will be found in the properties of different kinds; and that their degree of unlikeness in one respect bears some proportion to their unlikeness in others.

CHAPTER XXIII.

APPROXIMATE GENERALISATIONS, AND PROBABLE EVIDENCE.

THE inferences called *probable* rest on approximate generalisations. Such generalisations, besides the

inferior assurance with which they can be applied to individual cases, are *generally* almost useless as premisses in a deduction; and therefore in *Science* they are valuable chiefly as steps towards universal truths, the discovery of which is its proper end. But in *practice* we are forced to use them—1, when we have no others, in consequence of not knowing what general property distinguishes the portion of the class which have the attribute predicated, from the portion which have it not (though it is true that we can, in such a case, usually obtain a collection of exactly true propositions by subdividing the class into smaller classes); and, 2, when we *do* know this, but cannot examine whether that general property is present or not in the individual case; that is, when (as usually in *moral* inquiries) we could get universal majors, but not minors to correspond to them. In any case an approximate generalisation can never be more than an empirical law. Its authority, however, is less when it composes the whole of our knowledge of the subject, than when it is merely the most available form of our knowledge for practical guidance, and the causes, or some certain mark of the attribute predicated, being known to us as well as the effects, the proposition can be tested by our trying to deduce it from the causes or mark. Thus, our belief that most Scotchmen can read, rests on our knowledge, not merely that most Scotchmen that we have known about could read, but also that most have been at efficient schools.

Either a single approximate generalisation may be applied to an individual instance, or several to the same instance. In the former case, the proposi-

tion, as stating a general average, must be applied only to average cases; it is, therefore, generally useless for guidance in affairs which do not concern large numbers, and simply supplies, as it were, the first term in a series of approximations. In the latter case, when two or more approximations (not connected with each other) are *separately* applicable to the instance, it is said that two (or more) *probabilities are joined by addition*, or, that there is a *self-corroborative chain* of evidence. Its type is: Most A are B; most C are B; this is both an A and a C; therefore it is probably a B. On the other hand, when the subsequent approximation or approximations is or are applicable only by virtue of the application of the first, this is joining two (or more) probabilities, *by way of Deduction*, which produces a *self-infirmative chain*; and the type is: Most A are B; most C are A; this is a C; therefore it is probably an A; therefore it is probably a B. As, in the former case, the probability increases at each step, so, in the latter, it progressively dwindles. It is measured by the probability arising from the first of the propositions, abated in the ratio of that arising from the subsequent; and the error of the conclusion amounts to the aggregate of the errors of all the premisses.

In two classes of cases (exceptions which prove the rule) approximate can be employed in deduction as usefully as complete generalisations. Thus, first, we stop at them sometimes, from the inconvenience, not the impossibility, of going further; and, by adding provisos, we might change the approximate into an universal proposition; the sum of the provisos being

then the sum of the errors liable to affect the conclusion. Secondly, they are used in Social Science with reference to masses with *absolute* certainty, even without the addition of such provisos. Although the premisses in the Moral and Social Sciences are only probable, these sciences differ from the exact only in that we cannot decipher so many of the laws, and not in the conclusions that we do arrive at being less scientific or trustworthy.

CHAPTER XXIV.

THE REMAINING LAWS OF NATURE.

THERE are, we have seen, five facts, one of which every proposition must assert, viz. Existence, Order in Place, Order in Time, Causation, and Resemblance. Causation is not fundamentally different from Coexistence and Sequence, which are the two modes of Order in Time. They have been already discussed. Of the rest, Existence, if of things in themselves, is a topic for Metaphysics, Logic regarding the existence of *phenomena* only; and as this, when it is not perceived directly, is proved by proving that the unknown phenomenon is connected by *succession or coexistence* with some known phenomenon, the fact of Existence is not amenable to any *peculiar* inductive principles. There remain Resemblance and Order in Place.

As for Resemblance, Locke indeed, and, in a more unqualified way, his school, asserted that all reasoning is simply a comparison of two ideas by means of a

third, and that knowledge is only the perception of the agreement or disagreement, that is, the resemblance or dissimilarity, of two ideas: they did not perceive, besides erring in supposing ideas, and not the phenomena themselves, to be the subjects of reasoning, that it is only sometimes (as, particularly, in the sciences of Quantity and Extension) that the agreement or disagreement of two things is the one thing to be established. Reasonings, however, about *Resemblances*, whenever the two things cannot be directly compared by the virtually simultaneous application of our faculties to each, do agree with Locke's account of reasoning; being, in fact, simply such a comparison of two things through the medium of a third. There are laws or formulæ for guiding the comparison; but the only ones which do not come under the principles of Induction already discussed, are the mathematical axioms of Equality, Inequality, and Proportionality, and the theorems based on them. For these, which are true of all phenomena, or, at least, without distinction of origin, have no connection with laws of Causation, whereas all other theorems asserting resemblance have, being true only of special phenomena originating in a certain way, and the resemblances between which phenomena must be derived from, or be identical with, the laws of their causes.

In respect to Order in Place, as well as in respect to Resemblance, some mathematical truths are the only general propositions which, as being independent of Causation, require separate consideration. Such are certain geometrical laws, through which, from the position of certain points, lines, or spaces, we infer

the position of others, without any reference to their physical causes, or to their special nature, except as regards position or magnitude. There is no other peculiarity as respects Order in Place. For, the Order in Place of effects is of course a mere consequence of the laws of their causes; and, as for primæval causes, in *their* Order in Place, called their *collocation*, no uniformities are traceable.

Hence, only the methods of Mathematics remain to be investigated; and they are partly discussed in the Second Book. The directly inductive truths of Mathematics are few: being, first, certain propositions about existence, tacitly involved in the so-called definitions; and secondly, the axioms, to which latter, though resting only on induction, *per simplicem enumerationem*, there could never have been even any apparent exceptions. Thus, every arithmetical calculation rests (and this is what makes Arithmetic the type of a deductive science) on the evidence of the axiom: The sums of equals are equals (which is coextensive with nature itself)—combined with the definitions of the numbers, which are severally made up of the explanation of the name, which connotes the way in which the particular agglomeration is composed, and of the assertion of a fact, viz. the physical property so connoted.

The propositions of Arithmetic affirm the modes of formation of given numbers, and are true of all things under the condition of being divided in a particular way. Algebraical propositions, on the other hand, affirm the equivalence of different modes of formation of numbers generally, and are true of all things under condition of being divided in *any* way.

Though the laws of Extension are not, like those of Number, remote from visual and tactual imagination, Geometry has not commonly been recognised as a strictly physical science. The reason is, first, the possibility of collecting its facts as effectually from the ideas as from the objects; and secondly, the illusion that its ideal data are not mere hypotheses, like those in now deductive physical sciences, but a peculiar class of realities, and that therefore its conclusions are *exceptionally* demonstrative. Really, all geometrical theorems are laws of external nature. They might have been got by generalising from actual comparison and measurement; only, that it was found practicable to deduce them from a few obviously true general laws, viz. The sums of equals are equals; things equal to the same thing are equal to one another (which two belong to the Science of Number also); and, thirdly (what is no merely verbal definition, though it has been so called): Lines, surfaces, solid spaces, which can be so applied to one another as to coincide, are equal. The rest of the premisses of Geometry consist of the so-called definitions, which assert, together with one or more properties, the real existence of objects corresponding to the names to be defined. The reason why the premisses are so few, and why Geometry is thus almost entirely deductive, is, that all questions of position and figure, that is, of quality, may be resolved into questions of quantity or magnitude, and so Geometry may be reduced to the one problem of the measurement of magnitudes; that is, to the finding the equalities between them.

Mathematical principles can be applied to other

sciences. All causes operate according to mathematical laws; an effect being ever dependent on the quantity or a function of the agent, and generally on its position too. Mathematical principles cannot, indeed, as M. Comte has well explained, be usefully applied to physical questions, whenever the causes are either too inaccessible for their numerical laws to be ascertained, or are too complex for us to compute the effect, or are ever fluctuating. And, in proportion as physical questions cease to be abstract and hypothetical, mathematical solutions of them become imperfect. But the great value of mathematical training is, that we learn to use its *method* (which is the most perfect type of the Deductive Method), that is, we learn to employ the laws of simpler phenomena to explain and predict those of the more complex.

CHAPTER XXV.

THE GROUNDS OF DISBELIEF.

THE result of examining evidence is not always belief, or even suspension of judgment, but is sometimes positive disbelief. This can ensue only when the affirmative evidence does not amount to full proof, but is based on some approximate generalisation. In such cases, if the negative evidence consist of a stronger, though still only an approximate, generalisation, we think the fact improbable, and disbelieve it provisionally; but if of a complete generalisation based on a rigorous induction, it is disbelieved by us totally, and thought impossible. Hence, Hume declared miracles incredible, as being, he considered, contrary

to a complete induction. Now, it is true that *in the absence of any adequate counteracting cause*, a fact contrary to a complete induction is incredible, whatever evidence it may be grounded on ; unless, indeed, the evidence go to prove the supposed law inconsistent with some better established one. But when a miracle is asserted, the presence of an adequate counteracting cause *is* asserted, viz. a direct interposition of an act of the will of a Being having power over nature. Therefore, all that Hume proved is, that we cannot believe in a miracle unless we believe in the power, and *the will*, of the Deity to interfere with existing causes by introducing new ones ; and that, in default of such belief, not the most satisfactory evidence of our senses or of testimony can hinder us from holding a seeming miracle to be merely the result of some unknown natural cause. The argument of Dr. Campbell and others against Hume, however, is untenable, viz. that, as we do not disbelieve an alleged fact (which may be something conforming to the uniform course of experience) merely because the chances are against it, therefore we need never disbelieve any fact supported by credible testimony (even if contrary to the uniform course of experience). But this is to confound *improbability before the fact*, which is *not* always a ground for disbelief, with *improbability after the fact*, which always is.

Facts which conflict with special laws of causation are only improbable before the fact ; that is, our disbelief depends on the improbability that there could have been present, without our knowledge, at the time and place of the event, an adequate counteracting cause. So, too, with facts which conflict with

the properties of *kinds* (which are uniformities of mere coexistence not proved to be dependent on causation), that is, facts which assert the existence of a new *kind*; such facts we disbelieve only if, the generalisation being sufficiently comprehensive, some properties are said to have been found in the supposed new *kind* disjoined from others which always have been known to accompany them. When the assertion would amount, if admitted, only to the existence of an unknown cause or an anomalous *kind*, *unconformable*, but, as Hume puts it, *not contrary* to experience, in circumstances so little explored, that it is credible hitherto unknown things may there be found, and when prejudice cannot have tempted to the assertion, one ought neither to admit nor to reject the testimony, but to suspend judgment till it be confirmed or disproved from other sources. Only facts, then, which are contradictory to the laws of Number, Extension, and Universal Causation (since these know no counteraction or anomaly), or to laws nearly as general, are improbable after, as well as before the fact, and only these we should term *absolutely impossible*, calling other facts *improbable* only, or, at most, *impossible in the circumstances of the case*.

Between these two species of improbabilities lie *coincidences*; that is, combinations of chances presenting some unexpected regularity assimilating them in so far to the results of law. It was thought by d'Alembert that, though regular combinations are as probable as others according to the mathematical theory, some physical law prevents them from occurring so often. Now, stronger testimony

may indeed be needed to support the assertion of such a combination as, e.g. ten successive throws of sixes at dice, because such a regular series is more likely than an irregular series to be the result of design; and because even the desire to excite wonder is likely to tempt men to assert the occurrence falsely, though this probability must be estimated afresh in every instance. But though such a series *seems* peculiarly improbable, it is only because the comparison is tacitly made, not between it and any one particular previously fixed series of throws, but between all regular and all irregular successions taken together. The fact is not in itself more improbable; and no stronger evidence is needed to give it credibility, apart from the reasons above mentioned, than in the case of ordinary events.

BOOK IV.

OPERATIONS SUBSIDIARY TO INDUCTION.

CHAPTER I.

OBSERVATION AND DESCRIPTION.

THE mental process which Logic deals with, viz. the investigation of truth by means of evidence, is always a process of Induction. Since Induction is simply the extension to a class of something observed to be true of certain members of it, Observation is the first preliminary to it. It is, therefore, right to consider, not indeed how or what to observe (for this belongs to the art of Education), but under what conditions observation is to be relied on. The sole condition is, that the supposed observation should really be an observation, and not an inference, whereas it is usually a compound of both, there being, in our propositions, besides observation which relates only to the sensations, an inference from the sensations to the objects themselves. Thus so-called errors of sense are only erroneous inferences from sense. The sensations themselves must be genuine; but, as they generally arise on a certain arrangement of outward objects being present to the organs, we, as though by instinct, infer this arrangement even when not existing. The sole object, then, of the logic of observation, is to separate the inferences from observation from the

observations themselves, the only thing really observed by the mind (to waive the metaphysical problem as to the *perception* of objects) being its own feelings or states of consciousness, outward, viz. Sensations, and inward, viz. Thoughts, Emotions, and Volitions.

As in the simplest observation much is inference, so, in describing an observed fact, we not merely describe the fact, but are always forced to class it, affirming the resemblance, in regard of whatever is the ground of the name being given, between it and all other things denoted by the name. The resemblance is sometimes perceived by direct comparison of the objects together; sometimes (as, e.g. in the description of the earth's figure as globular and so forth) it is inferred through intermediate marks, i.e. deductively. When a hypothesis is made (e.g. by Kepler, as to the figure of the earth's orbit), and then verified by comparison with actual observations, Dr. Whewell calls the process Colligation of Facts by appropriate Conceptions, and affirms it to be the whole of Induction. But this also is only description, being really the ordinary process of ascertaining resemblance by a comparison of phenomena; and, though subsidiary to Induction, it is not itself Induction at all.

CHAPTER II.

ABSTRACTION, OR THE FORMATION OF CONCEPTIONS.

This Chapter is a digression.

ABSTRACT IDEAS, that is, General Conceptions, certainly do exist, however Metaphysics may decide as

to their composition. They *represent* in our minds the whole classes of things called by the general names; and, being implied in the mental operation whereby classes are formed, viz. in the comparison of phenomena, to ascertain in what they agree, cannot be dispensed with in induction, since such a comparison is a necessary preliminary to an induction, and more than two objects cannot well be compared without a type, in which capacity conceptions serve.

But, though implied in the comparison, it does not follow that, as Dr. Whewell supposes, they must have existed in the mind prior to comparison. Sometimes, but only sometimes, they are pre-existent to the comparison of the particular facts in question, being, as was Kepler's hypothesis of an ellipse, familiar conceptions borrowed from different facts, and *super-induced*, to use Dr. Whewell's expression, on the facts in question. But even such conceptions are the results of former comparisons of individual facts. And much more commonly (and these are the more difficult cases in science) conceptions are not pre-existent even in this sense; but they have to be got (e.g. the Idea of Polarity) by abstraction, that is, by comparison, from among the very phenomena which they afterwards serve to arrange, or, as Dr. Whewell says, to *connect*. They seem to be pre-existent; but this is only because the mind keeps ever forming conceptions from the facts, which at the time are before it, and then tentatively applies these conceptions (which it is always remodelling, dropping some which are found not to suit after-found facts, and generalising others by a further effort of abstraction) as types of

comparison for phenomena subsequently presented to it; so that, being found in these later stages of the comparison already in the mind, they appear in the character simply of types, and not as being also themselves results of comparison. Really they are always both; and the term *comparison* expresses as well their origin as (and this far more exactly than to *connect* or to *superinduce*) their function.

Dr. Whewell says that conceptions must be *appropriate* and *clear*. They must, indeed, be appropriate relatively to the purpose in view (for appropriateness is only relative); but they cannot avoid being appropriate (though one may be more so than another) if our comparison of the objects has led to a conception corresponding to any real agreement in the facts: the ancients' and schoolmen's conceptions were often absolutely inappropriate, because grounded on only apparent agreement. So, again, they must be *clear* in the following sense; that is to say, a *sufficient number* of facts must have been *carefully observed*, and accurately *remembered*. It is also a condition (and one implied in the latter qualities) of clearness, that the conception should be *determinate*, that is, that we should know precisely what agreements we include in it, and never vary the connotation except consciously.

Activity, carefulness, and accuracy in the observing and comparing faculties are therefore needed; the first quality to produce appropriateness, and the latter two, clearness. Moreover, *scientific imagination*, i.e. the faculty of mentally arranging known elements into new combinations, is necessary for forming true conceptions; and the mind should be stored with

previously acquired conceptions, kindred to the subject of inquiry, since a comparison of the facts themselves often fails to suggest the principle of their agreement; just as, in seeking for anything lost, we often have to ask ourselves in what places it may be hid, that we may search for it there.

CHAPTER III.

NAMING AS SUBSIDIARY TO INDUCTION.

AS reasoning is from particulars to particulars, and consists simply in recognising one fact as a mark of another, or a mark of a mark of another, the only necessary conditions of the exertion of the reasoning power are senses, to perceive that two facts are conjoined; and association, as the law by which one of the two facts raises up the idea of the other. The existence of artificial signs is not a third *necessary* condition. It is only, however, the rudest inductions (and of such even brutes are capable) that can be made without language or other artificial signs. Without such we could avail ourselves but little of the experience of others; and (except in cases involving our intenser sensations or emotions) of none of our own long past experience. It is only through the medium of such permanent signs that we can register uniformities; and the existence of uniformities is necessary to justify an inference, even in a single case, and they can be ascertained once for all.

General names are not, as some have argued, a mere contrivance to economise words. For, if there

were a name for every individual object, but no general names, we could not record one uniformity, or the result of a single comparison. To effect this, all indeed, that are *indispensable*, are the abstract names of attributes; but, in fact, men have always given general names to objects also.

CHAPTER IV.

THE REQUISITES OF A PHILOSOPHICAL LANGUAGE, AND THE PRINCIPLES OF DEFINITION.

CONCRETE general names (and the meaning of abstract names depends on the concrete) should have a fixed and knowable connotation. This is easy enough when, as in the case of new technical names, we choose the connotation for ourselves; but it is hard when, as generally happens with names in common use, the same name has been applied to different objects, from only a vague feeling of resemblance. For, then, after a time, general propositions are made, in which predicates are applied to those names; and these propositions make up a loose connotation for the class name, which, and the abstract at about this same period formed from it, are consequently never understood by two people, or by the same person at different times, in the same way. The logician has to fix this fluctuating connotation, but so that the name may, if possible, still *denote* the things of which it is currently affirmed. To effect this double object (which is called, though improperly, defining *not the name but the thing*), he must select from the

attributes in which the denoted objects agree, choosing, as the common properties are always many, and, in a *kind*, innumerable, those which are familiarly predicated of the class, and out of them, if possible, or otherwise, even in preference to them, the ones on which depend, or which are the best marks of, those thus familiarly predicated. To do this successfully, presumes a knowledge of all the common properties of the class, and the relations between them of causation and dependence. Hence the discussion of non-verbal definitions (which Dr. Whewell calls the Explication of Conceptions) is part of the business of discovery. Hence, too, disputes in science have often assumed the form of a battle of definitions; such definitions being not arbitrary, but made with a view to some tacitly assumed principle needing expression.

We ought, if possible, to define in consonance with the denotation. But sometimes this is impossible, through the name having accumulated *transitive* applications, in its gradual extension from one object, in relation to which it connotes one property, to another which resembles the former, but in quite a different attribute. These *transitive* applications, even when found to correspond in different languages, may have arisen, not from any common quality in the objects, but from some association of ideas founded on the common nature and condition of mankind. When the association is so natural and habitual as to become virtually indissoluble, the *transitive* meanings are apt to coalesce in one complex conception; and every new transition becomes a more comprehensive generalisation of the term in

question. In such cases the ancients and schoolmen did not suspect, what otherwise they carefully watched for, viz. ambiguities: not Plato, though his Comparisons and Abstractions preparatory to Induction are perfect; not even Bacon, in his speculations on Heat. Hence have sprung the various vain attempts to trace a common idea in all the uses of a word, such as *Cause* (Efficient, Material, Formal, and Final *Cause*), *the Good*, *the Fit*.

When a term is applied to many different objects agreeing *all* only in *one* quality (e.g. things *beautiful*, in *agreeableness*), though *most* agree in something besides, it is better to exclude part of the denotation than of the connotation, however indistinct: else language ceases to keep alive old experience, alien perhaps to present tendencies. In any case, words are always in danger of losing part of their connotation. For, just one or two out of a complex cluster of ideas, and sometimes merely the look or sound of the word itself, is often all that is absolutely necessary for the suggesting another set of ideas to continue the process of thought; and consequently, some metaphysicians have even fancied that all reasoning is but the mechanical use of terms according to a certain form. If persons be not of active imaginations, the only antidote against the propensity to let slip the connotation of names, is the habit of predicating of them the properties connoted; though even the propositions themselves, as may be seen from the way in which maxims of Religion, Ethics, and Politics are used, are often repeated merely mechanically, not being questioned, but also not being felt. Much of our knowledge recorded in words is ever

oscillating between its tendency, in consequence of different generations attending exclusively to different properties in names, to become partially dormant, and the counter-efforts of individuals, at times, to revive it by tracing the forgotten properties historically in the almost mechanically repeated formulas of propositions; and, when they have been there rediscovered, promulgating them, not as discoveries, but with authority as what men still profess to believe. The danger is, lest the formula itself be dismissed by clear-headed narrow-minded logicians, and the connotation fixed by them (in order that the denotation may be extended) in accordance with the present use of the term. Then, if the truths be at any time rediscovered, the prejudice is against them as novelties. The *selfish* theory of morals partly fell because the inconsistency of received formulas with it prompted a reconsideration of its basis. What would have been the result if the formulas attaching odium to selfishness, praise to self-sacrifice, had been dismissed, if this indeed had been possible! Language, in short, is the depository of all experience, which, being the inheritance of posterity, we have a right to vary, but none to curtail. We may improve the conclusions of our ancestors; we should not let drop any of their premisses; we may alter a word's connotation; but we must not destroy part of it.

CHAPTER V.

THE NATURAL HISTORY OF THE VARIATION IN THE MEANING OF TERMS.

THE connotation of names shifts not only by reason of gradual inattention to some of the common properties,

which, if language were ruled by convention alone, would be in their entirety both the perpetual and the sole constituents of the connotation; but also from the incorporation in the connotation, in addition to these, and often, finally, to the *exclusion* of them altogether, of other circumstances at first only casually associated with it. These collateral associations are the cause why there are so few exact synonymes; and why the dictionary meaning, or Definition, is so bad a guide to its uses, as compared with its history, since the latter explains the law of the succession by showing the causes which determined the successive uses.

Two counter-movements are always going on in language. One is generalisation, by which words are ever losing part of their connotation, and becoming more general. This arises, partly from men, such as historians and travellers, using words, especially those expressing complicated mental and social facts strange to them, in a loose sense, in ignorance of the true connotation; partly, from known things multiplying faster than names for them; partly, also, from the wish to give people some notion of a new object by reference to a known thing resembling it however slightly. The other movement is specialisation; and by it words (even the same words which, as, e.g. *pagan* and *villain*, later get generalised in a new direction) are ever taking a fresh connotation, through their denotation being diminished. Specialisations often occur even in scientific nomenclature, a word which expressed general characters becoming confined to a specific substance in which these characters are predominant. So it is when any

set of persons has to think of one species oftener than of any other contained in the genus: e.g. some sportsmen mean partridges by the term *birds*. But, as ideas of our pleasures and pains and their supposed causes, cling, most of all, by association to what they have been once connected with, the great source of specialisation is the addition of the ideas of agreeableness or painfulness, and approbation or censure, to the connotation. And hence arises the fallacy of *question-begging* names referred to later on.

It is the business of logicians not to ignore, for they cannot prevent, transformations of terms in common use, but to trace and embody them, and men's half unconscious reasons for them, in distinct definitions.

CHAPTER VI.

TERMINOLOGY AND NOMENCLATURE.

Not only must words have a fixed and knowable meaning; but also, no important meaning should be without its word: that is, there should be a name for everything which we have often to make assertions about. There should be, therefore, first, names suited to describe all the individual facts; secondly, a name for every important common property detected by comparing those facts; and, thirdly, a name for every *kind*.

First, it conduces to brevity and clearness to have separate names for the oft-recurring combinations of feelings; but, as these can be defined without reference back to the feelings themselves, it is *enough*

for a *descriptive* terminology, if there be a name for every variety of elementary feeling, since none of these can be defined, or indicated to a person, except either by his having the sensation itself, or being referred through a known mark to his remembrance of it. The meaning of the name when given to a feeling is fixed, in the first instance, by convention, and must be associated *immediately*, not through the usage of ordinary language, with the feeling, so that it may at once recall the latter. But even among the elementary feelings, those purely mental, and also sensations, such as those from disease, the identity of which in different persons cannot be determined, cannot be exactly *described*. It is only the impressions on the outward senses, or those inward feelings connected uniformly with outward objects (and, consequently, sciences, such as botany, conversant with outward objects), which are susceptible of an exact descriptive language.

Secondly, there must also be a separate name for every important common property recognised through that comparison of observed instances which is preparatory to induction (including names for the classes which we artificially construct in virtue of those properties). For, although a definition would often convey the meaning, both time and space are saved, perspicuity promoted, and the attention excited and concentrated, by giving a brief and compact name to each of the new *general conceptions*, as Dr. Whewell calls them, that is, the new results of abstraction. Thenceforward the name nails down and clenches the unfamiliar combination of ideas, and suggests its own definition.

Thirdly, as, besides the artificial classes which are marked out from neighbouring classes by definite properties to be arrived at by abstraction, there are classes, viz. *kinds*, distinguished severally by an unknown multitude of independent properties (and about which classes therefore many assertions will be made), there must be a name for every *kind*. That is, besides a terminology, there must be a nomenclature, i.e. a collection of the names of all the lowest *kinds*, or *infimæ species*. The Linnæan arrangements of plants and animals, and the French of chemistry, are nomenclatures. The peculiarity of a name which belongs to a nomenclature is, not that its meaning resides in its denotation instead of its connotation (for it resides in its connotation, like that of other concrete general names); but that, besides connoting certain attributes which its definition explains, it also connotes that these attributes are distinctive of a *kind*; and this fact its definition cannot explain.

A philosophical language, then, must possess, first, precision, and next (the subject of the present chapter), completeness. Some have argued that, in addition, names are fitted for the purposes of thought in proportion as they approximate to mere symbols in compactness, through meaninglessness, and capability of use as counters without reference to the various objects which, though utterly different, they may thus at different times equally well represent. Such are, indeed, the qualities enabling us to employ the figures of arithmetic and the symbols of algebra perfectly mechanically according to general technical rules. But, in the first place, in our direct inductions,

at all events, depending as they do on our perception of the particulars of the agreement and difference of the phenomena, we could never dispense with a distinct mental image of the latter. Further, even in deduction, though a syllogism is conclusive from its mere form, if the terms are unambiguous, yet the *practical* validity of the reasoning depends on the hypothesis that no counteracting cause has interfered with the truth of the premisses. We can assure ourselves of this only by studying the phenomena at every step. For it is only in geometry and algebra that there is no danger from the Composition of Causes, or the superseding of one set of laws by another; and that, therefore, the propositions are categorically true. In sciences in general, then, the object should be, so far from keeping individualising peculiarities out of sight, to contrive the greatest possible obstacles to a merely mechanical use of language: we should carefully keep alive a consciousness of its meaning, by referring, by aid of derivation and the analogies between the ideas of the roots and the derivatives, to the origin of words; and as words, however philosophically constructed, are always tending, like coins, to have their inscription worn off, we should be ever stamping them afresh. This we shall effect, if we contemplate habitually, not the *formulas* which record the laws of the phenomena (for, if so, the formulas will themselves progressively lose their meaning), but the phenomena whence the laws were collected; and we must conceive these phenomena in the concrete, and clothe them in circumstances.

CHAPTER VII.

CLASSIFICATION, AS SUBSIDIARY TO INDUCTION.

EVERY name which connotes an attribute thereby divides, but only incidentally, all things, known and unknown, real and imagined, into two classes, viz. those which have, and those which have not the attribute. But sometimes the naming itself is but the secondary and subsidiary, and the classification, the primary object. The general problem of such classification is, to provide that things shall be thought of in such groups, and the groups in such an order, as will best promote the remembrance and ascertainment of their laws. Its subjects are *real things* exclusively, but *all* real things, since, to place one object in a group, we ought to know the divisions of nature at large.

Any property may be the basis for a classification, but those best suited are properties which are causes, or, next, as the cause of a class's chief peculiarities seldom serves as its diagnostic, any effect which is a sure mark both of the cause and of the other effects. Only a classification so grounded is scientific; the same also is not technical or artificial, but natural, and emphatically *natural* (as compared with classifications in an inferior degree also *natural*, which are based on properties important with reference to the reasoner's special practical objects), when the classification is based on those properties which would most impress one who knew all the properties, but was not interested particularly in any one. Further, it is a great recommendation of a classification, that

it groups together things of like general aspect; but this is not a *sine quâ non*: a group may be *natural* even if based on very *unobvious* properties, provided these are marks of many other properties, though certainly then there should be also some more obvious property to act as a mark of the unobvious ones which form the real basis.

As the first principle of *natural* classification is that the classes must be so formed that the objects composing each may have as many properties in common as possible to serve as predicates, all *kinds* should have places among the *natural* groups, since the common properties of *kinds*, and, therefore, the general assertions that can be made about them, are innumerable. But *kinds* are too few to make up the whole of a classification: other classes also may be eminently *natural*, though marked out from each other only by a definite number of properties. Of neither sort of *natural* groups is Dr. Whewell's theory *strictly* true, viz. that every *natural* group is not determined by definition, that is, by definite characters which can be expressed in words, but is fixed by Type. He explains that a type is an example of any class, for instance, a species of a genus, which possesses all the characters and properties of the genus in a marked way; that round this type-species are grouped all the other species, which, though deviating from it in various directions and degrees, yet are of closer affinity to it than to the centre of any other group; and that this is the reason why propositions about *natural* groups so often state matters as being true not in all cases, but only in most. Now, there is a truth, but only a

partial truth, in this doctrine. It is this: in forming *natural* groups, species which want certain of the class-characters, some one, and others another, are classed with those (the majority) that have them all, because they are more like (that is, in fact, have more of the common characters of) that particular group than of any other. On account of the feeling of vagueness hence engendered, we certainly, in deciding if an object belong to the group, do generally (and *must*, when the classification is made expressly with a view to a special inductive enquiry) refer mentally, not as a substitute for, but in illustration of the definition of the group, to some standard specimen which has *all* the characters well developed. But not the less, therefore, are all *natural*, equally with all artificial, groups framed with distinct reference to certain definite characters. In the case of *kinds*, a few characters are chosen as marks of the rest. In the case of other *natural* groups, the formation of the larger groups, into which we collect the *infimæ species*, is suggested indeed by resemblance to types (since we form each such larger group round a selected *kind* which serves as its exemplar); but the group itself, when formed, is determined by definite characters.

Class names should by the mode of their construction help those who have learnt about the thing, to remember it, and those who have not learnt, now to learn, by being merely told the name. This is best effected, in the case of *kinds*, when the word indicates by its very formation the properties it connotes. But this is seldom possible. For, though a *kind*-name connotes not all the *kind*-properties, but some only

which serve as sure marks of the rest, even these have been found too many to be included conveniently in a name (except in Elementary Chemistry, where every compound substance has one distinctive index-property, viz. the chemical composition). A subsidiary resource is to point out the *kind's* nearest natural affinities. For instance, in the binary Nomenclature of Botany and of Zoology, the name of every species consists of the name of the *natural* group next above, with a word added expressive of some quality in the nature or mode of discovery, or what not, of the particular species itself. By this device (obtaining at present only in Botany and Zoology), as well is the expression, in the name, of many of the *kind's characters* secured, as the use of names economised, and the memory relieved. Except for some such plan, what hope of naming the 60,000 known species of Plants?

CHAPTER VIII.

CLASSIFICATION BY SERIES.

THE object of Classification generally is to bring our ideas of objects into the order best fitted for prosecuting inductive enquiries into the laws of the phenomena generally. But a Classification which aims at facilitating an inductive enquiry into the laws of some special phenomenon, must be based on that phenomenon itself. The requisites of such a classification are, first, the bringing into one class all *kinds* of things which exhibit the phenomenon; next,

the arranging them in a *series*, according to the degrees in which they exhibit it. Such a classification has been largely applied in Comparative Anatomy and Physiology (and these alone), since there has been found a recognisable difference in the degree in which animals possess one main phenomenon, viz. Animal Life.

This arrangement of the instances, whence the law is to be collected, in a series, is that which is always implied in and is a condition of *any* application of the method, viz. that of Concomitant Variations, which must be used when conjoined circumstances cannot easily be separated by experiment. But sometimes (and it is so in Zoology) the law of the subject of the special enquiry (e.g. Animal Life) has such influence over the general character of the objects, that all other differences among them seem mere modifications of it; and then the classification required for the special purpose becomes the determining principle of the classification of the same objects for general purposes.

To recognise the identity of phenomena which thus differ only in degree, we must assume a type-species. This will be that *kind* which has the class-properties in their greatest intensity (and, therefore, most easily studied with all their effects); and we must conceive the other varieties as instances of degeneracy from that type.

The divisions of the series must be determined by the principles of *natural* grouping in general (that is, in effect, by natural affinity); in subordination, however, to the principle of a natural series; that is, in the same group must not be placed things

which ought to occupy different points of the general scale.

Zoology affords the only *complete* example of the true principles of rational classification, whether as to the formation of groups or of series. Yet the same principles are applicable to all cases (to art and business as well as science) where the various parts of a wide subject have to be brought into mental co-ordination.

BOOK V.

FALLACIES.

CHAPTER I.

FALLACIES IN GENERAL.

THE habit of reasoning well is the only complete safeguard against reasoning ill, that is, against drawing conclusions with insufficient evidence, a practice which the various contradictory opinions, particularly about the phenomena relating to Man, show to be even now common, and that too among the most enlightened. But, to be able to explain an error is a necessary condition of seeing the truth; for, 'Contrariorum eadem est Scientia.' Consequently, a work on Logic must classify Fallacies, that is, the varieties of Apparent Evidence; for they *can* be classified, though not in respect of their negative quality of being either not evidence at all, or inconclusive, yet in respect of the positive property they have of *appearing* to be evidence.

As Logic has been here treated as embracing the whole reasoning process, so it must notice the fallacies incident to any part of it (not to Ratiocination merely), whether arising from faulty Induction, or from faulty Ratiocination, or from dispensing wholly with either or both of them. It does not treat of errors from negligence, or from inexpertness in using

right methods, nor does it treat of errors from moral causes, viz. Indifference to truth, or Bias by our wishes or our fears; for the moral causes are but the *remote* and *predisposing*, not the *exciting* causes of opinions; and therefore inferences from them, since they must always involve the intellectual operation of admitting insufficient evidence as sufficient, really come under a classification of the things which wrongly *appear* evidence to the *understanding*.

Fallacies may be arranged, with reference either to the cause which makes them (erroneously) appear evidence, or to the particular kind of evidence they simulate. The following classification is grounded on both these considerations jointly.

CHAPTER II.

CLASSIFICATION OF FALLACIES.

THE business of Logic is, not to enumerate false opinions, but to enquire what property in the facts led to them, that is, what peculiarity of relation between two facts made us suppose them habitually conjoined or disjoined, and thus regard the presence or absence of the one as evidence of that of the other. For every such property in the facts, or our mode of considering them, there is a corresponding class of Fallacies.

As the supposed habitual connexion or repugnance of two facts may be admitted, either as a self-evident and axiomatic truth, or as itself an inference, the first great division is into Fallacies of Simple In-

spection or *à priori* Fallacies, and Fallacies of Inference. But there is also an intermediate class. For, sometimes an inference is erroneous through our not conceiving what our premisses precisely are, and from our therefore substituting new premisses for the old, or a new conclusion for the one we undertook to prove; and this is called the Fallacy of Confusion. Under this head, indeed, of Fallacies of Confusion, might strictly be brought almost any fallacy, though falling also under some other head: for, some of the links in an argument, especially if sophistical, are sure to be suppressed; and, it being left doubtful which is the proposition to be supplied, we can seldom tell with certainty under *which* class the fallacy absolutely comes. It is, however, convenient to reserve the name *Fallacy of Confusion* for cases where Confusion is the *sole* cause of the error.

Cases, then, where there is more or less ground for the error in *the nature of the apparent evidence itself*, the evidence being assumed to be of a certain sort, and a false conclusion being drawn from it, may be classed as Fallacies of Inference. According as the apparent evidence consists of particular facts, or of foregone generalisations, we call the errors Fallacies of Induction or of Deduction. Each of these classes, again, may be subdivided into two species, according as the apparent evidence is either false, or, though true, inconclusive. Such subdivisions of the Fallacy of Induction are respectively called, in the former case, Fallacies of Observation (including cases where the facts are not directly observed, but inferred), and, in the latter, Fallacies of Generalisation. Among Fallacies of Deduction, those which proceed

on false premisses have no specific name, for they must fall under one of the other heads of Fallacies; but those, the premisses of which, though true, do not support the conclusion, compose a subdivision, which may be specified as Fallacies of Ratiocination.

CHAPTER III.

FALLACIES OF SIMPLE INSPECTION; OR, À PRIORI FALLACIES.

THERE must be some *à priori* knowledge, some propositions to be received without proof; for there cannot be a chain suspended from nothing. What these are is disputed, one school recognising as ultimate premisses only the facts of our subjective consciousness, e.g. Sensations, while Ontologists hold that the mind intuitively, and not through experience, recognises as realities other existences, e.g. Substances, which are suggested by, though not inferrible from, those facts of consciousness. But, as both schools, in fact, allow that the mind infers the *reality* from the *idea* of a thing, and that it may do this unduly, there results a class of Fallacies resting on the tacit assumption that the objects in nature have the same order as our ideas of them. Hence not only arose the vulgar belief that facts which make us think of an event are omens foreboding (e.g. lucky or unlucky names), or even causing its occurrence; but even men of science both did and do fall into this Fallacy. The following dogmas express the different forms of this error:—

1. *a. Things which we cannot help thinking of together must coexist*; thus Descartes held that, because existence is involved (though really only by the thinker himself) in the idea of a geometrical figure, a thing like the idea must exist. *β. Whatever is inconceivable is false.* The latter proposition has been defended by drawing a distinction between the principle, and its possibly wrong application to facts, e.g. to Antipodes; but how can we ever know that it has been rightly applied? Coleridge, again, has distinguished between the unimaginable, which he thinks may possibly be true, and the inconceivable, which he thinks cannot be; but Antipodes were imaginable at the same period when they were inconceivable. In fact, as even to Newton it seemed inconceivable, that a thing should act where it is not (e.g. that the sun should act upon the earth without the medium of an ether), simply because his mind was not familiar with the idea, so it *may* be with *our* incapability (if not, indeed, resulting merely from our limited faculties) of *conceiving*, e.g. that matter cannot think; that space is infinite; that *ex nihilo nihil fit*. Leibnitz's tenet that all *natural* phenomena must be explicable *à priori*, and the further assumption by some that Nature always acts by the simplest, i.e. by the most easily conceivable means (and that, therefore, e.g. the heavenly bodies have a circular movement), exhibit vividly this Fallacy of Simple Inspection.

2. *Whatever can be thought of apart, or has a separate name, exists apart as a separate entity*, e.g. Nature, Time, qualities, as e.g. Whiteness, and, worst of all, the *Substantiæ Secundæ*. Mysticism is this

habit of ascribing objective existence to the subjective creations of the mind, and reasoning from them to the things themselves.

3. *A fact must follow a certain law, because we see no reason for its deviating from it in one way rather than in another.* This, which is the same as the Principle of the Sufficient Reason, has been used to prove the Law of Inertia (the very point to be proved, viz. that only external force can be a sufficient reason for motion *in a particular direction*, being assumed), and also the First Law of Motion, the argument being, in the latter case, that a moving body, if it do *not* continue of itself to move uniformly in a straight line, must deviate right or left, and that there is *no reason* for its going one way more than the other: to which the answer is, that, apart from experience, we could not know whether or not there were a reason. Geometers often fall into this Fallacy.

4. *The differences in nature must correspond to our received distinctions* (in names and classifications). Thus, the Greeks thought that, by determining the meanings of words, they ascertained facts. Aristotle usually starts with 'We say thus or thus.' So, with the *Doctrine of Contraries*, in which the Pythagoreans and others assumed that oppositions in language imply similar ones in nature. Hence, too, the ancient belief in the essential difference between the laws of things terrestrial and things celestial, and in man's incapability of imitating nature's works. Bacon's error (which vitiates his inductive system) was analogous, in looking (either through his eagerness for practical results, or a lingering belief that causes were the sole object of philosophy) for the cause

of given effects rather than the effects of a given cause. Hence sprang his tacit assumption (and that in enquiries into the causes of a thing's sensible qualities, where it was especially fatal), that in all cases, e.g. of heat or cold, the *forma*, or set of conditions, is *one* thing. A similar notion, viz. that each property of gold, as of other things, has its one *forma*, produced the belief in Alchemy.

5. The conditions of a phenomenon often do resemble the phenomenon itself, e.g. in cases of Motion, Contagion, Feelings; but it is a Fallacy to suppose that *they must or probably will*. By this fancied law men guided their conjectures. Thus, the *Doctrine of Signatures* was, that substances showed their uses as medicines by external resemblance, either to their supposed effect, or to the disease. So, the Cartesians, and even Leibnitz, argued, that nothing physical but previous motion could account for motion, explaining the human body's voluntary motions by Nervous Vibrations or by Animal Spirits. Hence, too, the inference that there is a correspondence between the physical qualities of the cause, and like or like-named ones, either of the phenomenon (e.g. between sharp particles and a sharp taste), or of its effects (e.g. between the redness of Mars, and fire and slaughter as results of that planet's influence). In metaphysics, the Epicureans' doctrine of *species sensibiles*, and the moderns' of *perception through ideas*, arose from this fallacy (combined with another, viz. that a thing cannot act where it is not). Again, the conditions of a thing are sometimes spoken of even as though they were the thing itself. Thus, in the *Novum Organon*, heat (i.e. really the conditions of the

feeling of it) is called a kind of motion ; and Darwin, in his *Zoonomia*, after describing *idea* as a kind of *notion of external things*, defines it as *a motion of the fibres*. Cousin says: 'Tout ce qui est vrai de l'effet est vrai de la cause,' though the reverse *might* be true; and Coleridge affirms, as *an evident truth*, that mind and matter, as having no common property, cannot act on each other. The same fallacy led Leibnitz to his *pre established harmony*, and Malebranche to his *occasional causes*. So, Cicero argues that mental pleasures, if arising from the bodily, could not, as they do, exceed their cause ; and Descartes, that the Efficient Cause must have all the perfections of the effect. Conversely Descartes, too, and persons who assail, e.g. the Principle of Population by reference to Divine benevolence (thus implying that, because God is perfect, therefore what *they* think perfection must obtain in nature), assume that effects must resemble their causes.

CHAPTER IV.

FALLACIES OF OBSERVATION.

A FALLACY of Observation (the first of the three fallacies of Proof) may be either negative or positive.

1. The former, which is called Non-observation, is a case, not of a positive mis-estimate of evidence, or of the proper faculties (whether the senses or reason) not having been employed, but simply of the non-employment of any of the faculties. It arises (*a*) from neglect of instances. Sometimes this is

when there is a stronger motive to remember the instances on the one side, and the observers have neglected the principle of the Elimination of Chance. Hence (the mind, as Bacon says, being more moved by affirmative than by negative instances) the belief in predictions, e.g. about the weather, because they occasionally turn out correct; and the credit of the proverb, that 'Fortune favours fools,' since the cases of a wise man's success through luck are forgotten in his more numerous successes through genius. But a preconceived opinion is the *chief* cause why opposing instances are overlooked. Hence originate the errors about physical facts (e.g. of Copernicus's foes, and friends, too, about the falling stone), and *à fortiori*, on moral, social, and religious subjects, where yet stronger feelings are involved.

The fallacy of Non-observation may occur (β) from neglect, not of the material instances wholly, but of some material facts in them, e.g. in cases of cures by quack remedies (such as Kenelm Digby's 'sympathetic powder'), of some attendant fact (as exclusion of air from a wound, rest, regimen, and the like) which really worked the cure. Sometimes the neglected fact is one ascertainable, not by the senses, but by reasoning, which has been overlooked. Thus, Cousin's argument that, if the sole end of punishment were to prevent crime by intimidating intending criminals, the punishment of the innocent, indiscriminately with the guilty, would have the same effect, ignores the fact that the innocent would then be equally intimidated, and so the punishment would be of no use as an example to criminals. So, in Political Economy, where the effects of a cause often consist of two sets

of phenomena, the one obvious, the other deeper under the surface, and exactly contrary, the latter is often neglected. This was why the rapidly spent capital of the prodigal was supposed formerly to employ more labour than the invested savings of the parsimonious, and the purchase of native goods to encourage native industry more than the purchase of foreign.

2. The error in Mal-observation, which is the *positive* kind of Mis-observation, is not the overlooking facts, but the seeing them wrong. It arises from mistaking what is in fact inference (as much *must* be, whenever we try to observe or to describe) for perception, which is infallible evidence of what is really perceived. The Anti-Copernicans, when they appealed to common sense, made this mistake. So do untrained persons generally in describing facts, especially natural phenomena (e.g. apothecaries and nurses in stating symptoms), and that, too, in proportion to their ignorance. We might expect this, since usually the actual perceptions of the senses (e.g. the colour and extension) are not of interest, except as marks whence to draw inferences about something else (e.g. about the body, to which these qualities belong). Painters, therefore, to know what the sensation actually was, have to go through a special training. But this confusion of inference with perception is still more likely in highly abstract subjects; and, consequently, in these, mere, and often false inferences, have continually been regarded as intuitive judgments.

CHAPTER V.

FALLACIES OF GENERALISATION.

THIS class includes whatever errors of generalisation are not mere blunders, but arise from some wrong general conception of the inductive process. Only a few kinds can be noted. 1. Under this Fallacy come generalisations which *cannot* be established by experience, e.g. inferences from the order in the Solar System to other and unknown parts of the universe; and also, except when a particular effect would contradict either the laws of number and extension, or the universal law of causality, all inferences from the fact that *we* have never known of a particular effect to its impossibility. 2. Those generalisations also are fallacious which resolve, either, as in early Greece, all things into one element, or, as often in modern times, impressions on the senses, differing in quality, and not merely in degree, into the same; e.g. heat, light, and (through vibrations) sensation, into motion; mental, into nervous states; and vital phenomena, into mechanical or chemical processes. In these theories, one fact has its laws applied to another. It may possibly be a condition of that other; but even then the mode in which the new fact is actually produced would have to be explained by its own law, and not by that of the condition. 3. Again, generalisations got by Simple Enumeration, fall under this Fallacy. That sort of Induction ‘*precaridè concludit*,’ says Bacon, ‘*et periculo exponitur ab instantiâ contradictoriâ, . . . ex his tantummodò quæ præsto sunt pronuncians.*’ The ancients used it; and in

value, and its value on its scarcity). Such, also, were the Pythagorean inferences that there is a music of the spheres, because the intervals between the planets have the same proportion as the divisions of the monochord; and, again, that the movements of the stars as being *divine* must be regular, because so are those even of orderly *men*. So, Aristotle and other ancients supposed perfection to obtain in all natural facts, because it appeared to exist in some; and so, the Stoics tried to prove the equality of all crimes by reference to various similes and metaphors (as, that the man held half an inch below the surface will be drowned as certainly as the man at the bottom of the sea; and that want of skill is shown as much in steering a straw-laden boat as a treasure galleon on to the rocks). But, in fact, the connection by causation between the known and the inferred resemblance, which is *assumed* by these metaphors, is the very thing which they are brought to prove. The real use of such cases of analogy as metaphors is that they serve, not as an argument, but as an assertion that one exists. Though they cannot prove, they sometimes suggest the proof, and point to a case in which the same grounds for a conclusion have been found adequate. Such are d'Alembert's classification of successful politicians as either eagles or serpents; and the statement, as an argument for education, that, in waste land weeds will spring up; and such is *not* Bacon's inference from the levity of floating straw to the worthlessness of the *extant* scientific works of the ancients.

The great source of fallacious generalisation is bad classification, by which things with no, or no important, common properties, are grouped together.

Worst is it, when a word which commonly signifies some definite fact is applied to other facts only slightly similar. Bacon (who has himself thus erred in his enquiries into heat) specifies, as examples of this, the various applications (got, by unscientific abstraction, from the original sense) of the word 'wet,' to flame, air, dust, and glass, as well as to water. The application by Plato, Aristotle, and other ancients, of the terms Generation, Corruption, and *κίνησις* to many heterogeneous phenomena, with a mixture of the ideas belonging to them severally, caused many perplexities, which may be noticed under Fallacies of Confusion.

CHAPTER VI.

FALLACIES OF RATIOCINATION.

THESE fallacies (to which the name *Fallacy* is commonly applied exclusively) would generally be detected if the arguments were set out formally; and the value of the syllogistic rules is, that they force the reasoner to be aware what it is that he is really asserting. The frequent errors in processes such as Conversion and Opposition, which are in appearance, though not in reality, inferences from premisses, may for convenience be here referred to. Such are the simple conversion of an universal affirmative; the corresponding error in a hypothetical proposition of inferring the truth of the antecedent from that of the consequent; and the confusing of a contrary with a contradictory, which amounts, in practice, to mistaking the reverse of wrong for right. But fallacies of Ratiocination properly lie in syllogisms. They

commonly resolve themselves, when in a single syllogism, into the having more than three terms, whether covertly, as through an undistributed middle, or an illicit process, or avowedly. But the most dangerous and the commonest of these fallacies arise in a chain of argument from *changing the premisses*. One of the obscurer forms of this is the fallacy *a dicto secundum quid* (i.e. with a qualification, or condition, expressed, or, more usually, understood) *ad dictum simpliciter*. Thus, the Mercantile Theory was in favour of prohibiting all trade which tends to carry out more money than it brings in, on the ground that money is riches, though it is so only if the money can be *freely* spent. Such, too, was the argument (used to support the doctrine that tithes fall on the landlord) that, because now the rent of tithe-free land exceeds that of tithed land, the rent from the latter would be increased by the abolition of all tithes. There was a similar fallacy in the use of the maxim, that individuals are the best judges of their pecuniary interests, against Mr. Wakefield's scheme for concentrating settlers. Cases in which the condition of *time* is dropped, fall under this same particular fallacy, as, when the maxim that prices always find their level, is construed as meaning that they are always *at* their level. It is the same with the reasoning (especially in political and social subjects), upon principles, which are true in the absence of all modifying causes, as though no such causes *could* exist. Other analogous fallacies are those *a dicto simpliciter ad dictum secundum quid* (the converse of the preceding), and *a dicto secundum quid ad dictum secundum alterum quid*.

CHAPTER VII.

FALLACIES OF CONFUSION.

UNDER this head come all fallacies which arise, not so much from a false estimate of the probative force of known evidence, as from an indistinct conception what the evidence is.

1. Thus, where there is an ambiguous middle, or a term used in different senses in the premisses and in the conclusion, the argument proceeds as though there were evidence to the point, when, in fact, there is none. This error does not occur much in direct inductions, since the things themselves are there present to the senses or memory; but chiefly, in Ratiocination, where we are deciphering our own or others' notes. The ambiguity arises very often from assuming that a word corresponds precisely in meaning with the root itself (e.g. *representative*), or with cognate words from the same root, called *paronymous* words (as, *artful*, with *art*). Other examples of ambiguities are: 'Money,' which, meaning both the currency and also capital seeking investment, is often thought to be scarce in the former sense, because scarce in the latter; 'Influence of Property,' which, signifying equally the influence of respect for the power for good, and of fear of the power for evil, which is possessed by the rich, is represented as being assailed under its former form when attacked really only under the latter; 'Theory,' which, because applied popularly to the accounting for an effect apart from facts, is ridiculed, even when expressing, as it properly does, the result of philosophical induction

from experience; 'The Church,' which refers (as in the question of the inviolability of *Church* property) sometimes to the clergy alone, sometimes to all its members; 'Good,' in the Stoic argument that virtue, as alone *good* (in the Stoic sense), must therefore include freedom and beauty, because these are *good* (in the popular sense). So, the meaning of 'I' shifts from *the laws of my nature* to *my will*, in Descartes' *à priori* argument for the being of a God, viz. that there must be an external archetype whence I got the conception, for if *I* (i.e. *the laws of my nature*) made it, *I* (i.e. *my will*, and not, as it should consistently be, *the laws of my nature*) could unmake it; but *I* (i.e. *my will*) cannot. In the Free-Will controversy, 'I' is used ambiguously for volitions, actions, and mental dispositions, and 'Necessity' both for *Certainty* and for *Compulsion*. From the application of 'same,' 'one,' 'identical,' which primarily refer to a single object, to several objects because *similar*, grew up (for the purpose of accounting for the supposed *oneness* in things said to have the *same* nature or qualities) both the Platonic *Ideas*, and also the *Substantial Forms* and *Second Substances* of the Aristotelians, even though the latter did see the distinction between things differing both *specie* and *numero*, and those differing *numero* only. And thence, too, sprang Berkeley's proof of the existence of a Universal Mind from the supposed need of such a Being to harbour, in the interval, the idea, which, one and the same (really, only two *similar* ideas), a man's mind has entertained at two distinct times. The difficulty in *Achilles and the Tortoise* arises from the use of *infinity*, or, *for ever*, in the premisses,

to signify a finite time which is infinitely divisible, and, in the conclusion, to signify an infinite time. Thus, again, 'right' is used to express, both what others have no right to stop a man from doing, and also what it is not against his own duty to do; both what people are entitled to expect from, and also what they may enforce from others. The Fallacy of Composition and Division, i.e. the use of the same term in a syllogism, at one time in a collective, at another in a distributive sense, is one of the Fallacies of Ambiguous Terms. Examples of it are the arguments, that *great men* (collectively) could be dispensed with, because the place of any particular great man might have been supplied (i.e., in fact, by some other great man); and, that a high prize in a lottery may be reasonably expected (by *a certain individual*, viz. oneself), because a high prize is commonly gained (*by some one or other*).

2. In *Petitio Principii*, the premisses are not even verbally sufficient for the conclusion, since one premiss is either clearly the same as the conclusion, or actually proved from it, or not susceptible of any other proof. Men commonly fall into it, through believing that the premiss *was* verified, though they have forgotten how. But the variety, termed Reasoning in a Circle, implies a conscious attempt to prove two propositions reciprocally from each other. This formal proof is not often attempted, except under the pressure of controversy; but, from mistaking mutual coherency for truth, propositions, which cannot be proved except from each other, are often *admitted*, when expressed in different language, without other proof. Frequently a proposition is

presented in abstract terms as a proof of the same in concrete, as, in Molière's parody, 'L'opium endormit parcequ'il a une vertu soporifique.' So, some qualities of a thing selected arbitrarily are termed its nature or essence, and then reasoned from as though not able to be counteracted by any of the rest. 'Question-begging appellatives,' particularly, are cases of *Petitio Principii*, e.g. the styling any reform an *innovation*, which it really is, only that *innovation* conveys, besides its dictionary meaning, a covert sense of something extreme. Thus, in Cicero's *De Finibus*, 'Cupiditas,' which usually implies vice, is used to express certain desires the moral character of which is the point in question. Again, the infinite divisibility of matter was assumed by the argument which was used to prove it, viz. that the least portion of matter must have both an upper and an under surface (which, as every other Fallacy of Confusion, when cleared up, appears as a fallacy of a different sort, under shelter of which, as indeed in ratiocinative fallacies generally, the mere verbal juggle at first escapes detection). Such, again, was Euler's argument, that *minus* multiplied by *minus* gives *plus*, because it could not give the same as *minus* multiplied by *plus*, which gives *minus*. So, some ethical writers begin by assuming, that certain general sentiments are the *natural* sentiments of mankind, and thence argue that any which differ are morbid and *unnatural*. Thus, lastly, Hobbes and Rousseau rested the existence of government and law on a supposed social compact, and not on men's perception of the interests of society, which, however, could be the only ground for their abiding by such compact if a fact.

3. In Ignoratio Elenchi, or, the Fallacy of Irrelevant Conclusion, the error lies not either in mistaking the import of the premisses, or in forgetting what they are, but in mistaking what is the conclusion to be proved. Sometimes, a particular is substituted for the universal as the proposition needing proof, and sometimes, a proposition with different terms. Under this fallacy come the cases, not only of proving what was not denied, but of disproving what was not asserted; e.g. the argument used against Malthus (whose own position was, that population increases only *in so far as not kept down* by prudence, or by poverty and disease), that, at times, population has been nearly stationary; or again, that, in some country or other, population and comfort are increasing together, Malthus himself having asserted that this might be so, if capital has increased. Similarly, even Reid, Stewart, and Brown (not merely Dr. Johnson) urged that Berkeley ought, if consistent, to have run his head against a post, as though the non-recognition of an occult *cause* of sensations implies disbelief in any *fixed order among them*.

BOOK VI.

ON THE LOGIC OF THE MORAL SCIENCES.

CHAPTER I.

INTRODUCTORY REMARKS.

MANY complex problems have been resolved through the use of the Scientific Methods, and thus only. The most complex of all problems are the problems relating to Man himself; and of them those concerned with the Mind and Society have never been scientifically resolved. They can be rescued from empiricism, if at all, only by being submitted to some of the methods already characterised as applicable to science in general. Which of these methods must be selected, and why; what are the causes of previous failures; and what degree of success now is possible or probable, will be considered in this book, when a preliminary objection (*based on the theory of free will*), that men's actions are not, like other natural events, subject to invariable laws, has been first removed.

CHAPTER II.

LIBERTY AND NECESSITY.

THE theory of *free will*, viz. that the will is determined by itself, and not by antecedents, was invented

as being more in accordance with the dignity of human nature and our consciousness of freedom, than *philosophical necessity*. The latter doctrine, in laying down simply that our volitions and actions are invariable consequents of our antecedent states of mind, and that, given our motives, character, and disposition, other men could predict our conduct as certainly as any physical event, states indeed nothing which is in itself either contradicted by our consciousness, or degrading; yet the doctrine of causation, as applied to volition, is supposed, from the natural tendency of the mind to imagine falsely that a mysterious constraint is exercised by *any* antecedent over the consequent, to imply some state of dependence which our consciousness does contradict. Moreover, the erroneous notion that something more than uniformity of order and capability of being predicted is meant, has been favoured by the use of the ambiguous term *necessity* (which, it is true, commonly implies irresistibleness), to signify simply that the given cause will be followed by the effect subject to all possibilities of counteraction by other causes. Most necessarians have been themselves deceived by the expression: they are apt to be partially fatalists as to their own actions, with a weaker spirit of self-culture than the believers in free-will, and to fail to see that the fact of their character being formed *for* them, that is, by their circumstances, including their own organisation, is consistent with its being formed *by* themselves, as intermediate agents, moulding it in any particular way which they may *wish*. The belief that the *wishing* is excited by external causes, e.g. by education, casual aspirations, and experience of

ills resulting from our previous character, can be of no practical harm, and does not conflict with our feeling of moral freedom, that is, of power, *if we wish*, to modify or conquer our own character.

The ambiguity of the word *motive* has also caused confusion. A motive, when used to signify that which determines the will, means not always or only the anticipation of a pleasure or a pain, but often the desire of the action itself. The action having finally become by association in itself desirable, we may get the habit of willing it (that is, get a *purpose*) without reference to its being pleasurable. We are then said to have a confirmed character.

CHAPTER III.

THERE IS, OR MAY BE, A SCIENCE OF HUMAN NATURE.

ANY facts may be a subject of science, if they follow one another according to constant laws; and this, whether, although the ultimate laws are known, yet, of the derivative laws on which a phenomenon directly depends, either *none*, as in Meteorology, or, as in Tidology, *only* the laws of the greater causes on which the chief part of a phenomenon directly depends, have been ascertained, and not those of all the minor modifying causes; or, as in Astronomy (which is therefore called an *exact* science), both the ultimate laws are known, and also the derivative laws as well of the greater as of all the minor causes. The science of Human Nature cannot be exact, the causes of human conduct being only approximately known. Hence it is impossible to predict *with scientific*

accuracy any one man's acts, resulting as they do partly from his circumstances, which, in the future, cannot be precisely foreseen, and, partly, from his character, which can never be exactly calculated, because the causes which have determined it are sure, in the aggregate, not to be entirely like those which have determined any other man's. But approximate generalisations, though only probably true as to the acts and characters of individuals, will be certainly true as to those of masses, whose conduct is determined by general causes chiefly; and they are therefore sufficient for political and social science. They must, however, be connected deductively with the universal laws of human nature on which they rest, or they will be only low empirical laws. This is the text of the next two chapters.

CHAPTER IV.

THE LAWS OF MIND.

By the laws of mind (i.e. as considered in this treatise, the laws of mental phenomena) are meant the laws according to which one state of mind is produced by another. If M. Comte and others be right in saying that, in like manner with the mental phenomena called sensations, all the other states of mind have for their proximate causes nervous states, there would be no original laws of mind, and Psychology would be a mere branch of Physiology. But at present, this tenet is not proved, however highly probable; and, at all events, the characteristics of those nervous

states are quite unknown; consequently the uniformities of succession among the mental phenomena, which undoubtedly do exist, and which are not proved to result from more general laws, must be considered as the subject of a distinct science called Psychology. We can ascertain only by experiment the simple laws of Mind, such as—1. That a state of consciousness can be reproduced in the absence of the cause which first excited it (i.e. that every mental impression has its idea), and—2. That these secondary mental states themselves are produced according to the three laws of ideas. But the complex laws are got from these simple laws, according either to the Composition of Causes, when the complex idea is said to *consist of* the Simple Ideas, or to chemical combination, when it is said to be *generated by* them. Hartley and Mr. James Mill indeed hold *all* the mental phenomena to be generated by chemical combination from simple ideas of *sensation*, however unlike to the alleged results; but even though they had proved their theory, employing the Method of Difference, and not only the Method of Agreement (which latter itself they have used only partially), we should still have to study the complex ideas themselves inductively, before we could ascertain their sequences.

The analytical enquiry (neglected alike by the German metaphysical school, and by M. Comte) into the general laws of mind, will show that the mental differences of individuals are not ultimate facts, but may be referred generally to their particular mental history, their education and circumstances, but sometimes also to organic differences influencing the mental phenomena, not directly, but through the

medium of the psychological causes of the latter. Men's animal instincts, however, are probably, equally with the mere sensations, connected directly with physical conditions of the brain and nerves. Whether or not there be any direct relation between organic causes and any other mental phenomena, Physiology is likely in time to show; but at least Phrenology does not embody the principles of the relation.

CHAPTER V.

ETHOLOGY, OR THE SCIENCE OF THE FORMATION OF CHARACTER.

TILL the Empirical laws of Mind, i.e. the truths of common experience, are *explained* by being resolved into the causal laws (the subject of the last chapter), they are mere approximate generalisations which cannot be safely applied beyond the limits in which they were collected by observation. But this does not prove aught against the universality and simplicity of the ultimate mental laws; for the same is the case with the empirical laws even in astronomy, where each effect results from but few causes; *à fortiori*, therefore, will it be so in regard to man's character, which is influenced by each of his circumstances, which differ in the case of each nation, generation, and individual. But though mankind have not one universal character, yet there exist universal laws of the formation of character. These universal laws cannot be discovered experimentally, i.e. either by artificial experiment, since we can seldom vary the

experiment sufficiently, and exclude all but known circumstances, or by observation, since, even in the most favourable instances for the latter, viz. National acts, only the Method of Agreement can be applied. Observation has its uses in relation to this subject; but only as verification of the results arrived at by the Deductive Method. The Deductive Method must be employed to obtain the laws of the formation of character. They are got by supposing any given circumstances, and then considering how these will, according to the general laws of mind, influence the formation of character. So, contrary to Bacon's rule, laid down wrongly as universal, for the discovery of principles, the highest generalisations must be first ascertained by the experimental science of Psychology; and then will come what is in fact a system of corollaries from the latter science, viz. Ethology, i.e. (as dealing only with tendencies) the *exact* science of human character, or of education both national and individual, and which has for its principles the middle principles (*axiomata media*) of mental science. It does not yet, but it will soon, exist as a science. Its object must be to determine, from the general laws of mind, combined with man's general position in the universe, what circumstances will aid or check the growth of good or bad qualities, so that the Art of Education will be merely the transformation of these middle principles into precepts and their adaptation to the special cases. But at every step these middle principles, got by deduction, must be verified *à posteriori* by empirical laws, and by specific experience respecting the assumed circumstances.

CHAPTER VI.

GENERAL CONSIDERATIONS ON THE SOCIAL SCIENCE.

POLITICAL and social phenomena have been thought too complex for scientific treatment. Practitioners hitherto have been the only students; and so, as in medicine, before the rise of Physiology and Natural History, *experimenta fructifera*, and not *lucifera*, have been sought. The scheme of such a science has even been thought quackery, through the vain attempts of some theorists to frame universal precepts, as though their failure (arising from the variety of human circumstances) proved that the phenomena do not conform to universal laws. Social phenomena, however, being phenomena of human nature in masses, must, as human nature is itself subject to fixed laws, obey fixed laws resulting from the fixed laws of human nature. The number and changefulness of the data (unlike those of Astronomy) will prevent our ever predicting the far future of society. But, when general laws have been ascertained, an application of them to the individual circumstances of a given age and country will show us the causes and tendencies of, and the means of modifying, its actual condition. A consideration of two methods, erroneously used for this science, viz. the Experimental or Chemical, and the Abstract or Geometrical, will introduce us to the true one.

CHAPTER VII.

THE CHEMICAL, OR EXPERIMENTAL, METHOD IN THE
SOCIAL SCIENCE.

THE followers of this method do not recognise the laws of social phenomena as merely a composition of the laws of individual human nature. They demand specific experience in all cases; and they attempt to make effects, which depend on the greatest possible complication of causes, the subject of induction by observation and experiment. The attempt must fail; for, we can neither get by experiment appropriate *artificial* instances, nor, by observation, *spontaneous* instances (from history), with the circumstances enough varied for a true induction. Neither the *direct* nor the *indirect* Method of Difference can be applied, for we cannot find either two single instances differing in nothing but the presence or absence of a given circumstance (the *direct*), or two classes respectively agreeing in nothing but the presence of a circumstance on one side and its absence on the other (the *indirect*). Then, again, the Method of Agreement is of small value, because social phenomena admit the widest plurality of causes; and so also is that of Concomitant Variations, on account of the mutual action of the coexisting elements of society being such that what affects one affects all. The Method of Residues is better suited to social enquiries than the other three. But *it* is not a method of pure observation and experiment. It presupposes that we know, by previous deduction from principles of human nature, the causes of part of the effect.

But if thus part of the truths are, why may not all be, ascertained by Deduction, and the experimental argument be confined to the verifying of the deductions?

CHAPTER VIII.

THE GEOMETRICAL, OR ABSTRACT, METHOD.

THE Methods of Elementary Chemistry are applied to social phenomena from carelessness as to, or ignorance of, any of the higher physical sciences: the Geometrical Method, from the belief that Geometry, that is, a science of coexistent, not successive facts, where there are no conflicting forces, is, and that the now deductive physical sciences of Causation, where there are conflicting forces, are *not*, the type of deductive science. Thus, it seems to have been supposed by many philosophers, that each social phenomenon results from only one force, one single property of human nature. For instance, Hobbes assumed (eking out his assumption by the fiction of an original contract), that government is founded on fear. Even the scientific Bentham School based a general theory on one premiss, viz. that men's actions are always determined by their interests, meaning probably thereby, that the bulk of the conduct of any succession, or of the majority of any body of men, is determined by their private or worldly interests. They inferred thence, that those rulers alone will govern according to the interest of the governed, whose selfish interests are identified with it (forgetting that, apart from the philanthropy and sense

of duty of many, the conduct of *all* rulers must be influenced by the habits of mind, both of the whole community, and also of their own class in it, and by the maxims of their predecessors). Lastly, they laid down that this sense of identity of interest with the governed is producible only by responsibility (whereas the personal interest of rulers often prompts them to acts, e.g. for the suppression of anarchy, which are also for the interest of the governed). In fact, this school was pleading for parliamentary reform, and saw truly, that it is against the selfish interests of rulers that constitutional checks are needed, and that, in modern Europe, a feeling in the governors of identity of interest, when not active enough, can be roused only by responsibility to the governed. Their mistake was, that they based on just these few premisses a general theory of government, in forgetfulness that such should proceed by deduction from *the whole* of the laws of human nature, since each effect is an aggregate result of many causes operating now through the same ones, now through different ones, of these laws.

CHAPTER IX.

THE PHYSICAL, OR CONCRETE DEDUCTIVE, METHOD.

THE complexity in social effects arises from the number, not of the laws, but of the data. Therefore, Sociology, i.e. Social Science, must use the Concrete Deductive Method, compounding with one another the laws of all the causes on which any one effect depends, and inferring its law from them all. As in

the easiest case to which the Method of Deduction applies, so in this, the most difficult, the conclusions of ratiocination must be *verified* by collation with the concrete phenomena, or, if possible, with their empirical laws; and then the only effect of an increase in the complication of the subject will be a tendency to a disturbance, and sometimes even to an inversion (which, indeed, M. Comte thinks inseparable from all Sociological enquiries) in the order of the two processes, obliging us, first, to conjecture the conclusions by specific experience, and then verify them by *à priori* reasonings showing their connection with the principles of human nature.

Sociology is a system not of positive predictions, but of tendencies. Of tendencies themselves, not many can be laid down as true of all societies alike. Even in the case of any single feature of society, the *consensus* which exists in the body politic, as in the body natural, makes it uncertain whether a cause with a special tendency in one age or country will have quite the same in another. General propositions, therefore, in this deductive science, as, to be true, they must be hypothetical, and state the operation of a given cause in *given circumstances*, so, to be of any utility, should be limited to those classes of facts, which, though influenced by all sociological agents, are yet influenced *immediately* by a few only, certain fixed combinations of which are likely to recur often. Thus, Political Economy, taking the one psychological law that men prefer a greater gain to a smaller, and ignoring every other motive, except what are perpetually adverse principles to this, viz. men's aversion to labour and desire of present costly

pleasures, assumes, in enquiring what acts this desire of gain will produce, that, within the department of human affairs, where it is actually the main end, it is the *sole* end. Yet its general propositions are of great practical use, even though it thus provisionally overlooks as well miscellaneous concurrent causes (with some exceptions, as e.g. the principle of population), as also the fact of the non-existence elsewhere of the conditions of any one particular country (e.g. the peculiarly British mode of distribution of the produce of industry among three classes). Another hypothetical or abstract science, which can be carved out of Sociology, is the as yet unexplored Political Ethology, i.e. the theory of the causes which determine a people's, or age's, type of character, which collective character, besides being the most interesting phenomenon in the particular state of society, is the *main* cause of the social state which follows, and moulds *entirely* customs and laws. The neglect of national diversities sometimes (as e.g. the assumption by our political economists, that in commercial populations everywhere, equally as in Great Britain and America, all motives yield to the desire of gain) vitiates only the practical application of a proposition; but when the national character is mixed up at every step with the phenomena (as is the case in questions respecting the tendencies of forms of government), the phenomena cannot properly be insulated in a separate branch of Sociology.

As in Ethology and other deductive sciences, so in Statistics and History there are empirical laws. The immediate causes of social facts are often not open to direct observation; and the deductive science

can determine only what causes produce a given effect, and not the frequency and quantities of them; in such cases, the empirical law of the causes (which, however, can be applied to new cases only if we know that the remoter causes, on which these latter causes depend, remain unchanged) must be found through that of the effects, the Deductive Science relying then for its data on indirect observation. But, in the separate branches of Sociology, we cannot obtain empirical laws by specific experience. It is so particularly (on account both of the number of the causes, and also the fewness of the instances to be compared with the one in point) when the effect of any one (e.g. Corn Laws) of many simultaneous social causes has to be determined. We can, however, in such cases, verify *indirectly* a theory as to the influence of a particular cause in given circumstances, by seeing if the same theory accounts for the *existing* state of actual social facts which that cause has a tendency to influence.

CHAPTER X.

THE INVERSE DEDUCTIVE, OR HISTORICAL, METHOD.

THE *general* Science of Society, as contrasted with the branches, shows, not what effect will follow from a given cause under given circumstances, but what are the causes and characteristic phenomena of States of Society generally. A *State of Society* is the simultaneous state of all the chief social facts (e.g. employments, beliefs, laws). It is a condition of the

whole organism; and, when analysed, it exhibits uniformities of coexistence between its different elements. But, as this correlation between the phenomena is itself a law resulting from the laws which regulate the succession between one state of society and another, the fundamental problem of Social Science is to find these latter laws. The form of this succession, by which (on account of the exceptionally constant reaction, in social facts, of the effects, i.e. human character, on their causes, i.e. human circumstances) one social state is ever in process of changing into a different one, is now allowed to be, not, as in the solar system, a cycle, but a *progress* (by which is not here *necessarily* meant *improvement*, whatever the fact may be). In France it has been thought, that a law of progress, to be found by an analysis of the course of history, would enable us to predict the whole future. But such a law would be empirical, and not true beyond its own facts; for the succession of mental and social states cannot have an independent law. Empirical laws must indeed be found; or a *general* Science of Society would be impossible: for, the character of any one generation is so much the result of the characters of all prior ones, that *men* could not compute so long a series from the elementary laws producing it. But the empirical laws, when found (as they can be, since the series of the effects as a whole is ever growing in uniformity), must be shown by deductions to be, if not the only possible, or even the most probable, at least possible, consequences of the laws of human nature.

The empirical laws of society are uniformities,

either of coexistence, or of succession. The former are ascertained and verified by Social Statics (which is the theory of the *consensus*, i.e. the mutual actions and reactions, of contemporaneous social elements); the latter, by Social Dynamics (the theory of Society considered as in a state of progress). As to Social Statics—there is, M. Comte thinks, a perpetual reciprocity of influence between all aspects of the same organism, and to such an extent, that the condition of any one which we cannot directly observe can be estimated by that of another which we can. There is, he considers, such an interdependence, not only between the different sciences and arts among themselves, but between the sciences in general and the arts in general, even between the condition of different nations of the same age, and between a form of government and the civilisation of the period. Social Statics will ascertain for us the requisites of stable political union: it will enquire what special circumstances have always attended on such union, increasing and decreasing in proportion to its completeness; and will then verify these facts as requisites by deducing them from general laws of human nature. Thus, history indicates as such requisites and conditions of free political union:

1. A system of educational discipline checking man's tendency to anarchy;
2. Loyalty, i.e. a feeling of there being something, whether persons, institutions, or individual freedom and political and social equality, which is not to be, at least in practice, called in question;
3. That which the Roman Empire, notwithstanding all its tyranny, established, viz. a strong sense of common interest among fellow-citizens (a very

different feeling, by the bye, to mere antipathy to foreigners).

Social Dynamics regards sequences. But the *consensus* in social facts prevents our tracing the leading facts in one generation to separate causes in a prior one. Therefore, we must find the law of the correspondence not only between the simultaneous states, but between the simultaneous changes of the elements of society. To find this law, which, when duly verified, will be the scientific derivative law of the development of humanity, we must combine the statical view of the phenomena with the dynamical. Fortunately, the state of mankind's speculative faculties and beliefs, being the prime agent of the social movement, furnishes a clue in the maze of social elements, since the order of human progression in all respects will mainly depend on the order of progression of this prime agent. That the other dispositions which aid in social progress (e.g. the desire for increased material comfort) owe their means of working to this (however relatively weak a propensity it may be) is a conclusion from the laws of human nature; and this conclusion is in accordance also with the course of history, in which internal social changes have ever been preceded by proportionate intellectual changes. To determine the law of the successive transformations of opinions all past time must be searched, since such changes appear definitely only at long intervals. M. Comte alone has followed out this conception of the Historical Method; and his generalisation, to the effect that speculation has, on all subjects, three successive stages, has high scientific value.

The Historical Method will trace the derivative laws of social order and progress. It will enable us both to predict the future, and (thus founding the noblest part of the Political Art) partly to shape it. At present, both the Science and the Art are in the rudiments; but they are progressing.

CHAPTER XI.

THE LOGIC OF PRACTICE, OR ART; INCLUDING MORALITY AND POLICY.

PRACTICAL ETHICS, i.e. Morality, is an art; and therefore its Method must be that of Art in general. Now, Art from the major premiss, supplied by itself, viz. that the end is desirable, and from the theorem, lent by Science, of the combinations of circumstances by which the end can be reached, concludes that to secure this combination of circumstances is desirable; if it also appear practicable, it turns the theorem into a rule. Unless Science's report as to the circumstances is a full one, the rule may fail; and as, in any case, rules of conduct cannot comprise more than the ordinary conditions of the effect (or they would be too cumbrous for use), they must, at least in moral subjects, be considered, till confronted with the theorems, which are the reasons of them, provisional only. Practical maxims, therefore, till so confronted, are not universally true even for a given end, much less for conduct generally, and must not

be used, as they are by the *geometrical* school, as ultimate premisses.

Any particular art consists of its rules, *together with* the theorems on which they depend; and Art in general consists of the truths of Science; only these must be arranged in the order most convenient, not, as in Science (which is an enquiry into the course of nature), for thought, but for practice. Intermediate scientific truths must be framed to serve as first principles of the various arts: and through them the end or purpose of an art will be connected with the means for realising the conditions of its attainment. The end itself, however, is defined by the art, not by the science. Each art has one first principle or major premiss which does not, as the propositions of Science, assert that a thing *is* or *will be*, but recommends it as what *ought to be*. A scientific theory, however complete, of the history and tendencies of society does not show us (without Teleology, i.e. the Doctrine of Ends) what are the preferable ends. Art itself has its *Philosophia Prima*, for ascertaining the standard of ends. There can be but one such standard or general principle to which all rules of practice should conform; for, if there were several, a higher yet would be needed, as umpire when they disagreed. In Morality the felt need of a standard has been sometimes supplied by the hypothesis of intuitive moral principles: but a standard would still be wanted for the other two branches of the Art of Life, viz. Prudence or Policy, and Taste; and *their* standard when found would serve for Morality as well. The true standard, or general principle, is, *the promotion of the happiness of ALL sentient beings.*

This is not the *sole* end; for instance, ideal nobleness of will or conduct should be pursued in preference to the *specific* pursuit of happiness; but all ends whatsoever must be justified and should be controlled by it.



