A System of logic

Ratiocinative and Inductive

Presenting a Connected View of the Principles of Evidence and the Methods of Scientific Investigation

John Stuart Mill

[Brackets] enclose editorial explanations. Small ·dots· enclose material that has been added, but can be read as though it were part of the original text. Occasional •bullets, and also indenting of passages that are not quotations, are meant as aids to grasping the structure of a sentence or a thought. Every four-point ellipsis . . . . indicates the omission of a brief passage that seems to present more difficulty than it is worth. In this work such omissions are usually of unneeded further examples or rewordings. Longer omissions are reported between brackets in normal-sized type.—When a word is spoken about in this version, it is usually put between quotation marks; Mill himself does that with phrases and sentences but not with single words.—Mill here refers to contemporaries by their surnames; in the original he is less abrupt—'Dr Whewell', 'Professor Bain', and so on.

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Glossary

A&NP: Acronym of the 'all and nothing principle', to which Mill refers only by its Latin title, *dictum de omni et nullo*. Explained on page 79.

art: In this work, 'art' is a vehicle for several related ideas: rules, skill, techniques.

assertion: Mill uses this in about the way we use 'proposition'. For there to be an 'assertion', in his sense, nobody needs to have *asserted* anything.

basic: This replaces Mill's 'original' in some of its occurrences.

begging the question: Sometimes (not always) this replaces the Latin *petitio principii*. Mill's sense of this phrase is the only sense it had until fairly recently: 'beg the question' was to offer a 'proof' of P from premises that include P. It now means 'raise the question' ('That begs the question of what he was doing on the roof in the first place.') It seems that complacently illiterate journalists (of whom there are many) encountered the phrase, liked it, guessed at its meaning, and plunged ahead without checking.

cognition: Cognitions are items of knowledge, in a weak sense of 'knowledge' such that a cognition doesn't have to be true. In the context of page 133 they aren't significantly different from beliefs.

connoting: To say that word W connotes attribute A is to say that the meaning of W is such that it can't apply to anything that doesn't have A. For example, 'man' connotes humanity.

identical proposition: Strictly speaking, this is a proposition of the form 'x is x', where the subject and predicate are identical. But the phrase came also to be used for any proposition where the meaning of the predicate is a part (or all) of the meaning of the subject.

import: In Mill's use of it, this means about the same as 'meaning'; but he does use both those words, and the present version will follow him in that.

meaning: In most places this is the word Mill has used, but sometimes it replaces his 'acceptation'. It sometimes appears in the singular though the plural would seem more natural; that's how Mill wrote it.

modify: To 'modify' a description is to amplify it adjectivally or adverbially, e.g. modifying 'man' with 'irritable', and 'run' with 'swiftly'.

***mutatis mutandis*:** A Latin phrase that is still in current use. It means '(*mutatis*) with changes made (*mutandis*) in the things that need to be changed'. The use of it implies that it's obvious what the needed changes are.

***noumenon*:** A Greek word, much used by Kant, meaning 'thing considered as it is in its own nature' in contrast with 'thing considered in terms of how it *appears*', i.e. phenomenon. The plural is *noumena*.

popular: Even as late as Mill's time this mainly meant 'of the people' or 'for the people', usually the not highly educated people. It didn't mean 'liked by the people'.

real: On page 71 the word 'real' is tightly tied to its origin in the Latin *res* = 'thing'. So the contrast between 'real' propositions and 'verbal' ones involves the contrast between things and words.

***reductio ad absurdum*:** Standard (Latin) name for either of two forms of argument. **(i)** Proving P by showing that not-P logically implies P. **(ii)** Proving P by showing that not-P logically implies some Q that is obviously and indisputable false.

science: Any intellectual discipline whose doctrines are are highly organised into a logical structure. It doesn't have to involve experiments, or to be empirical. Many philosophers thought that theology is a science.

signification: This seems to mean about the same as 'meaning', but Mill uses both words, and this version will respect his choices.

universal type of. . . : The basic central paradigm of. . . .

vortices: Plural of 'vortex'. According to Descartes's highly speculative astronomy, each planet was nested in a band of matter—a vortex—circling around the sun.

vulgar: Applied to people who have no social rank, are not much educated, and (the suggestion often is) not very intelligent.

Book II: Reasoning

Chapter 1: Inference of reasoning in general

§1. The topic of Book I was not the nature of proof but the nature of assertion [see Glossary]: the import conveyed by a proposition, whether or not it is true, not the means by which to distinguish true propositions from false ones. The proper subject of logic is proof, but before we could understand what proof is we had to understand •what it is that gets proved, •what it is that can be a subject of belief or disbelief, of affirmation or denial. in short, •what the different kinds of propositions assert.

I have pushed this preliminary inquiry far enough to get a definite result. **(i)** Assertion relates either to the meaning of words or to some property of the things that words signify. **(ii)** Assertions about the meaning of words, among which definitions are the most important, have an indispensable role in philosophy. **(iii)** But because the meaning of words is essentially arbitrary, this class of assertions can't be true or false, and so can't be proved or disproved. **(iv)** Assertions respecting things, or what may be called real [see Glossary] propositions, as against verbal ones, are of various sorts. I have analysed the import [see Glossary] of each sort, and have ascertained the nature of the things they relate to and the nature of what they say about those things. **(v)** I showed that whatever the form is of a proposition, and whatever its ostensible subject or predicate, the *real* subject of each proposition is •one or more facts or phenomena of consciousness, or •one or more of the hidden causes or powers to which we ascribe those facts; and that what is asserted or denied concerning those phenomena or powers is always either existence, order in place, order in time, causation, or resemblance. That's the theory of the import of propositions reduced to its ultimate elements: but there's a simpler way of putting it that doesn't dig so deep but is scientific enough for many of the purposes for which such a general expression is required. . . . It goes like this:

> Every proposition asserts that some given subject does (or that it doesn't) have some attribute; or that some attribute is (or that it isn't) conjoined with some other attribute in all or some of the subjects that have it.

Let us now move on to the special problem of the science [see Glossary] of logic, namely 'How are assertions proved or disproved?' This is being asked about propositions that are appropriate subjects of (dis)proof, not about ones that can be known through direct consciousness, i.e. intuition.

We say of a fact or statement that it is 'proved' when our belief in its truth is based on some other fact or statement from which it is said to follow. Most propositions. . . .that we believe are believed not •because they are obviously true but •because we think they can be inferred from something we have already accepted. Inferring a proposition from a previous proposition—giving credence to it or claiming credence for it as a conclusion from something else—is *reasoning*, in the most extensive sense of the term. There's a narrower sense in which 'reasoning' is confined to the form of inference known as 'ratiocination'—of which syllogism is the general type [see Glossary]. Early in Book I reasons were given for not using 'reason(ing)' in this restricted sense, and additional motives will be suggested by the considerations that I am now embarking on.

§2. Before coming to *real* inference, I should say a little about kinds of *apparent* inference, getting them out of the way so that they aren't confused with the real thing. I'll discuss four of them.

(a) The first sort occurs when the proposition Q that is ostensibly inferred from another proposition P turns out under analysis to be merely a repetition of all or a part of the assertion contained in P. All the textbook examples of equipollency—i.e. equivalence—of propositions are of this kind. Thus, if we were to argue

- •Every man is rational; therefore no man is incapable of reason,
- •No man is exempt from death; therefore all men are mortal,

it would be obvious that we weren't proving anything but merely offering two wordings for a single proposition. One wording may have some advantages over the other, but it doesn't offer a shadow of *proof*.

(b) Secondly, from a universal proposition we pretend to infer another that differs from it only in being particular:

- •All A is B, therefore Some A is B;
- •No A is B, therefore Some A is not B.

Here again we aren't inferring one proposition from *another*, but merely asserting something and then repeating part of it.

(c) A third sort: From a proposition that affirms a predicate of a given subject we 'infer' a proposition affirming of the same subject something connoted by the former predicate—e.g. 'Socrates is a man, therefore Socrates is a living creature', where everything connoted by 'living creature' was affirmed of Socrates when he was said to be 'a man'. (If the propositions are negative, we must reverse their order: 'Socrates is not a living creature, therefore he is not a man'.) These are not really cases of inference; yet the trivial examples by which logic textbooks illustrate the rules of the syllogism are often of this ill-chosen kind—formal 'demonstrations' of conclusions to which anyone who understands the words in the premises has already *consciously* assented.

(d) The most complex case of this sort of apparent inference is 'conversion' of propositions: turning the predicate into a subject, and the subject into a predicate, and making out of the same terms thus reversed another proposition that must be true if the former is true. Thus, from the particular affirmative proposition *Some A is B* we may infer *Some B is A*. From the universal negative *No A is B* we may infer *No B is A*. From the universal affirmative proposition *All A is B* it can't be inferred that all B is A, but it can be inferred that some B is A. . . . From *Some A is not B* we can't even infer that some B is not A—some men are not Englishmen but it doesn't follow that some Englishmen are not men. The only recognised way of converting such a particular negative proposition is by changing *Some A is not B* to *Some A is a-thing-that-is-not-B*; this is a particular affirmative, which can be simply converted to *Some thing that is not B is A*. . . .

In all these cases there's no real inference; the conclusion presents no new truth, nothing but what was already •asserted in the premise and •obvious to anyone who understands it. The fact asserted in the conclusion is all or a part of the fact asserted in the premise. [Mill explains and defends this in terms of his Book I account of the import of propositions. One bit of this will be enough:] When we say that some lawful sovereigns are tyrants, what do we mean? That the attributes connoted by 'lawful sovereign' and the attributes connoted by 'tyrant' sometimes co-exist in one person. Now this is also precisely what we mean when we say that some tyrants are lawful sovereigns! So the latter isn't a second proposition inferred from the first, any more than the English translation of Euclid's *Elements* is a collection of theorems that are different from those contained in the Greek original—different from them and inferred from them. . . .

[In a footnote Mill explains some technical terms that won't be needed in the rest of the work, except for the next paragraph. They are:

Contraries:	All A is B	No A is B
Subcontraries:	Some A is B	Some A is not B
Contradictories:	All A is B	Some A is not B
"	No A is B	Some A is B
Subalternate:	All A is B	Some A is B
"	No A is B	Some A is not B]

Although you can't call it 'reasoning' or 'inference' when something that is asserted is then asserted again in different words, it is extremely important to develop a skill in spotting, rapidly and accurately, cases where a single assertion is ·showing up twice·, disguised under diversity of language. And the cultivation of this skill falls strictly within the province of the art [see Glossary] of logic. That is the main function •of any logical treatise's important chapter about the 'opposition' of propositions, and •of the excellent technical language logic provides for distinguishing the different kinds of opposition. Such considerations as these:

- •Contrary propositions can both be false but can't both be true;
- •Subcontrary propositions can both be true but can't both be false;
- •Of two contradictory propositions one must be true and the other false;
- •Of two subalternate propositions the truth of the universal proves the truth of the particular, and the falsity of the particular proves the falsity of the universal, but not vice versa;

are apt to appear at first sight to be very technical and mysterious; but when they're explained they seem almost too obvious to need to be stated so formally. . . . In this respect, however, these axioms of logic are on a level with those of mathematics. *Things that are equal to the same thing are equal to one another*—this is as obvious in any particular case as it is in the general statement; and if no such general maxim had ever been laid down, the demonstrations in Euclid would never have been stopped in their tracks by the gap which is at present bridged by this axiom. Yet no-one has ever censured writers on geometry for putting a list of these elementary generalisations at the start of their treatises as a first exercise of the ability to grasp a general truth, this being something the learner needs at every step. And the student of logic, in the discussion even of such truths as are cited above, acquires habits of •wary interpretation of words and of •exactly measuring the length and breadth of his assertions. Such habits are among the most indispensable conditions of any considerable mental attainment, and it's one of the primary objects of logical discipline to cultivate them.

§3. . . . .Let us now move on to cases where the progression from one truth to another really does involve *inference* in the proper sense of the word—-ones where we set out from known truths to arrive at others that are really distinct from them.

Reasoning, in the extended sense in which I use the word, in which it is synonymous with 'inference', is commonly said to be of two kinds:

(1) induction: reasoning from particular propositions to general ones,

(2) ratiocination or syllogism: reasoning from general propositions to particular ones.

I shall show that there's a third species of reasoning that doesn't fit either of those descriptions but is nevertheless valid and is indeed the foundation of both the others.

I have to point out that the expressions 'reasoning from particular propositions to general ones' and 'reasoning from general propositions to particular ones' don't adequately mark the distinction between •induction (in the sense I am giving it) and •ratiocination—or anyway they don't mark it without the aid of a commentary. [We are to understand, it seems, that the required 'commentary' is the rest of this paragraph.] What these expressions mean is that induction is inferring a proposition from propositions less general than itself, and ratiocination is inferring a proposition from propositions equally or more general. When, from the observation of a number of instances we ascend to a general proposition, or when by combining a number of general propositions we conclude from them another proposition still more general, the process—which is substantially the same in both cases—is called 'induction'. When from a general proposition combined with other propositions we infer a proposition of the same degree of generality as itself, or a less general proposition, or a merely individual proposition, the process is ratiocination. ·Why 'combined with other propositions'? Because· from a single proposition nothing can be concluded that isn't involved in ·the meanings of· the terms. . . .

Given that all experience begins with individual cases and proceeds from them to general propositions, it might seem that the natural order of thought requires that induction should be treated of before we reach ratiocination. But in a science that aims to trace our acquired knowledge to its sources, it is best that the inquirer should start with the later rather than the earlier stages of the process of constructing our knowledge—tracing derivative truths *back* to the truths they •are deduced from and •depend on for their believability—before trying to pin-point the spring from which both ultimately take their rise. There's no need for me to justify or explain this here; the advantages of this order of proceeding will show themselves as we advance.

So all I'll say about induction here is that it is without doubt a process of real inference. The conclusion in an induction takes in more than is contained in the premises. The principle or law collected from particular instances—the general proposition in which we embody the result of our experience—covers a much bigger territory than the individual experiments on which it is based. A principle arrived at on the basis of experience is more than a mere summing up of individual observations; it's a generalisation •grounded on those cases and •expressive of our belief that what we found true in them is true in indefinitely many cases that we haven't examined and probably never will. The nature and grounds of this inference, and the conditions necessary to make it legitimate, will be the topic of Book III; but it can't be doubted that such inference really does take place. . . .

So induction is a real process of reasoning or inference. Whether, and in what sense, as much can be said of the syllogism remains to be decided by the examination that begins now.

Chapter 2: Reasoning, or syllogism

§1. The analysis of the syllogism has been so accurately and fully performed in the common logic textbooks that the present work, which is not designed as a textbook, needs only to recapitulate the leading results of that analysis, as a basis for what I'll say later about the functions of the syllogism and the place it holds in science.

In a legitimate syllogism there have to be exactly three propositions—the proposition to be proved (the conclusion) and two other propositions which together prove it (the premises). There must be exactly three terms—the subject and predicate of the conclusion, and the 'middle term', which must occur in both premises because its role is to connect the other two terms. The predicate of the conclusion is called the major term of the syllogism; the subject of the conclusion is called the minor term. Each of these must occur in just one of the premises, together with the middle term which occurs in both. The premise containing the major term is called the major premise; that which contains the minor term is called the minor premise.

Syllogisms are divided by most logicians into four 'figures'. . . .according to the position of the middle term, which may either be the subject in both premises, the predicate in both, or the subject in one and the predicate in the other. The most common case is that in which the middle term is the subject of the major premise and the predicate of the minor. This is reckoned as the first figure. When the middle term is the predicate in both premises, the syllogism belongs to the second figure; when it is the subject in both, to the third. In the fourth figure the middle term is the subject of the minor premise and the predicate of the major. . . .

[The following schema provides a simple way of remembering what each of the 'figures' is:

First	Second	Third	Fourth
M—C	C—M	M—C	C—M
A—M	A—M	M—A	M—A

Draw a line through M, sloping down, then up, then down, then sloping up: the result is a W. You'll have little need for this as you read on, and even less for the stuff on this page and the next about the 'moods' of the syllogistic figures. Its inclusion here is mere act of piety towards Mill.]

Each figure is divided into moods, according to what are called the propositions' quantity (i.e. whether they are universal or particular) and their quality (i.e. whether they are affirmative or negative). Here are schemas for all the moods in which the conclusion does follow from the premises. A is the minor term, C the major, M the middle term.

First Figure

All M is C	No M is C	All M is C	No M is C
All A is M	All A is M	Some A is M	Some A is M
∴	∴	∴	∴
All A is C	No A is C	Some A is C	Some A is not C

SECOND FIGURE

No C is M	All C is M	No C is M	All C is M
All A is M	No A is M	Some A is M	Some A is not M
∴	∴	∴	∴
No A is C	No A is C	Some A is not C	Some A is not C

THIRD FIGURE

All M is C	No M is C	Some M is C	All M is C	Some M is not C	No M is C
All M is A	All M is A	All M is A	Some M is A	All M is A	Some M is A
∴	∴	∴	∴	∴	∴
Some A is C	Some A is not C	Some A is C	Some A is C	Some A is not C	Some A is not C

FOURTH FIGURE

All C is M	All C is M	Some C is M	No C is M	No C is M
All M is A	No M is A	All M is A	All M is A	Some M is A
∴	∴	∴	∴	∴
Some A is C	Some A is not C	Some A is C	Some A is not C	Some A is not C

In these blank forms for making syllogisms, no place is assigned to singular propositions. They are of course used in ratiocination; but because their predicate is affirmed or denied of the whole of the subject they are ranked for the purposes of the syllogism with universal propositions. So these two syllogisms—

- All men are mortal, All kings are men, therefore All kings are mortal
- All men are mortal, Socrates is a man, therefore Socrates is mortal

—are precisely similar arguments and are both ranked in the first mood of the first figure. [Mill has here an enormous footnote critically discussing Bain's view that singular propositions don't belong in syllogisms. He argues convincingly that Bain's case for this rests on assuming that proper names have meanings, although elsewhere in his work he affirms Mill's view that they don't.]

If you want to know why the above forms are legitimate and that no others are, you could probably work that out for yourself, or learn it from just about any ordinary common-school book on syllogistic logic, or you could go to Whately's *Elements of Logic*, where the whole of the common doctrine of the syllogism is stated with philosophical precision and explained with remarkable clarity.

All valid ratiocination—all reasoning from admitted general propositions to other propositions equally or less general—can be exhibited in some of the above forms. The whole of Euclid, for example, could easily be expressed in a series of syllogisms, regular in mood and figure.

Though a syllogism fitting any of these formulae is a valid argument, any correct ratiocination can be stated in syllogisms of the first figure. The rules for putting an argument in one of the other figures into the first figure are called rules for the 'reduction' of syllogisms. It is done by converting one or both of the premises. Thus an argument in the first mood of the **second figure**—

No C is M
All A is M
∴
No A is C,

can be reduced as follows. The premise *No C is M* can be replaced by *No M is C*, which I have shown to be the very same assertion in other words. With that change made, the syllogism becomes

No M is C
All A is M
∴
No A is C,

which is a good syllogism in the second mood of the **first figure**. It's equally easy to reduce a syllogism in the first mood of the **third figure**—

All M is C
All M is A
∴
Some A is C,

—to one in the third mood of the **first figure**—

All M is C
Some A is M
∴
Some A is C.

That involves replacing 'All M is A' by 'Some A is M'; that's not the same proposition, but it asserts a part of what 'All M is A' asserts, and that part suffices to prove the conclusion. Similar moves enable every mood of every other figure to be reduced to one or other of the moods of the first figure; those with affirmative conclusions reduce to the first or third moods of the first figure, those with negative conclusions reduce to the second or fourth. . . .

Sometimes an argument falls more naturally into one of the other three figures, with its conclusiveness being more immediately obvious in some figure other than the first. Compare this third-figure syllogism

Aristides was virtuous,
Aristides was a pagan, therefore
Some pagan was virtuous

with the first-figure syllogism that it reduces to:

Aristides was virtuous,
Some pagan was Aristides, therefore
Some pagan was virtuous.

The third-figure version of the argument is more natural, and more immediately convincing, than the first-figure version.

[Mill mentions a 1764 account by the German philosopher Johann Heinrich Lambert of the purposes for which each syllogistic figure is most natural. Although he is respectful towards this work of Lambert's, Mill concludes:] We are at liberty, in conformity with the general opinion of logicians, to consider the two elementary forms of the first figure as the universal types [see Glossary] of all correct ratiocination—one when the conclusion to be proved is affirmative, the other when it is negative. [One of the 'two elementary forms' is the first and third moods, the other is the second and fourth moods.] Some arguments may have a tendency to clothe themselves in the forms of the second, third, and fourth figures; but this can't possibly happen with the only arguments that are of first-rate scientific importance, namely those in which the conclusion is a universal affirmative, because such conclusions can be proved only in the first figure.[1]

§2. On examining, then, these two general formulae, we find that in both of them, one premise, the major, is a universal proposition; and according as this is affirmative or negative, the conclusion is so too. All ratiocination, therefore, starts from a general proposition, principle or assumption in which a predicate is affirmed or denied of an entire class. . . .i.e. of an indefinite number of objects distinguished by a common characteristic and on that basis designated by a common name. [Remember that what makes this the 'major' premise isn't its •being written first but its •containing the predicate of the conclusion.]

The other ('minor') premise is always affirmative, and asserts that something—an individual or a part or all of a class—belongs to the class of which something was affirmed or denied in the major premise. So the attribute affirmed or denied of the entire class may (if that affirmation or denial was correct) be affirmed or denied of the object(s) said to be included in the class; which is just what the conclusion asserts.

Is that an adequate account of the constituent parts of the syllogism? We'll soon see. But it is at least true as far as

1 [This footnote originally discussed work by William Hamilton and by Augustus De Morgan. The former of these—about 'the quantification of the predicate'—is omitted here, as a dead end. Some of the latter is retained because, despite Mill's coolness about it, it did lead somewhere. Incidentally, these two writers later had a controversy about the 'quantification of the predicate', in which (according to C. S. Peirce) 'the reckless Hamilton flew like a dor-bug into the brilliant light of De Morgan's mind'.]

Since this chapter was written a treatise has appeared which aims at a further improvement in the theory of the forms of ratiocination, namely De Morgan's *Formal Logic; or the Calculus of Inference, Necessary and Probable*. In the more popular [see Glossary] parts of this volume there's an abundance of valuable observations felicitously expressed; but its the principal feature of originality is an attempt to bring within strict technical rules the cases where a conclusion can be drawn from premises of a form usually classified as 'particular'. De Morgan rightly says that from the premises *Most Ms are Cs* and *Most Ms are As* it strictly follows that *Some As are Cs*, because two portions of the class M, each containing more than half, must have some overlap. Following out this line of thought, it is equally evident that if we knew exactly what proportion the 'most' in each of the premises bear to the entire class M, we could correspondingly increase the definiteness of the conclusion. If 60% of M are included in C, and 70% in A. . . . the number of As that are Cs must be $\geq$ 30% of the class M. Proceeding on this conception of 'numerically definite propositions', and extending it to such forms as these [details omitted by this version, not by Mill] and examining what inferences can be drawn from the various possible combinations of premises of this description, De Morgan establishes universal formulae for such inferences; creating for that purpose not only a new technical language but a formidable array of symbols analogous to those of algebra.

The inferences presented by De Morgan are legitimate, and the ordinary theory •of syllogisms• doesn't deal with them; so I don't say that it wasn't worthwhile to show in detail how they could expressed in formulae as rigorous as those of Aristotle. . . . But I doubt that these results of his are worth studying and mastering for any practical purpose. The practical use of technical forms of reasoning is to keep out fallacies; but in ratiocination properly so called the fallacies that threaten arise from the incautious use of *ordinary* forms of language, and the logician must track the fallacies into that territory, rather than waiting for them on his territory. While the logician remains among propositions with the numerical precision of the calculus of probabilities, his enemy is left in possession of the only ground on which he can be formidable. Very few of the non-universal propositions that a thinker has to depend on for purposes either of speculation or of practice, can be made numerically precise, so common reasoning can't be translated into De Morgan's forms, which therefore can't throw any light on it.

it goes. It has accordingly been generalised, and erected into the logical maxim that *whatever can be affirmed or denied of a class may be affirmed or denied of everything included in the class.* This so-called 'all and nothing principle' is said to be the basis for all ratiocination—so much so that the answer to 'What *is* ratiocination?' is said to be 'Applying the all and nothing principle'. [Mill gives the principle its standard Latin name, *Dictum de omni et nullo*. The present version will use the English name, usually abbreviated to 'A&NP'.]

But this maxim, considered as a principle of reasoning, seems suited to a metaphysic that •was once generally accepted but •has for the last two centuries been considered as finally abandoned (though even today there are attempts to revive it). I'm talking about the metaphysical view that

> what are called 'universals' are *substances* of a special kind, having an objective existence distinct from the individuals that are classified in terms of them.

If *that* were right, the A&NP would convey an important meaning. ·According to the dead metaphysical view about the nature of universals, we should think of 'All men are rational' as meaning 'Man is rational', where 'Man' stands for a substantial universal that has a certain relation R to each individual man. Then it would be a solid bit of news that the rationality involved in the nature of Man is also involved in the nature of each thing to which Man has the relation R, i.e. of each man·. That everything predicable of the universal is predicable of the various individuals contained under it is not an identical proposition [see Glossary] but a statement of a fundamental law of the universe. The assertion that the entire nature of the substantial universal forms part of the nature of each individual substance called by the same name—that the properties of Man, for example, are properties of all men—was a proposition of real significance when 'Man' did not mean all men but something inherent in men and vastly superior to them in dignity. But now that we know that

- •a class—a universal, a genus or species—is not an entity in its own right but merely the individual substances that are placed in it, and that
- •there's nothing real in this situation except those substances, a common name given to them, and common attributes indicated by the name,

please tell me what we *learn* by being told that whatever can be affirmed of a class can be affirmed of every object in it! The class is nothing but the objects contained in it, and the A&NP amounts to the identical proposition that whatever is true of certain objects is true of each of them. [The crucial point here is Mill's rejection of what he sees as the dead metaphysical view that when a substance has a certain property this involves *two things and a relation between them* rather than one thing that is thus-and-so.] If all ratiocination were merely the application of this maxim to particular cases, the syllogism would indeed be 'solemn trifling', which it has often been accused of being. The A&NP is on a par with another truth that also used to be regarded as highly important, namely 'Whatever is, is'. To give any real meaning to the A&NP we must regard it not as an axiom but as a definition; we must look on it as intended to be a round-about account of the meaning of the word 'class'.

An error that seemed to be finally refuted and dislodged from thought often needs only put on a new suit of phrases to be welcomed back to its old lodgings, and allowed to rest unquestioned for another cycle of years. Modern philosophers have been ruthless in *expressing* their contempt for the scholastic dogma that:

> Genera and species are a special peculiar kind of *substances*—general substances that are the only permanent things—while the individual substances that come under them are continually changing; so

> that knowledge, which necessarily brings stability, must concern those general substances or universals, and not the facts or particulars that come under them.

Yet this nominally rejected doctrine has never ceased to poison philosophy. It has done this under the guise •of 'abstract ideas' in the work of Locke (though this has been less spoiled by it than the work of any other writer who has been infected with it), •of the ultra-nominalism of Hobbes and Condillac, or •of the ontology of the later German metaphysicians,

Once men got used to thinking of scientific investigation as essentially a study of universals, they didn't drop this habit of thought when they stopped thinking of universals as having an independent existence. Even those who came to regard universals as mere names couldn't free themselves from the notion that the investigation of truth is at least some kind of conjuration or juggle with those names. [In that striking phrase Mill is suggesting something like pulling a rabbit out of a hat.] When a philosopher •accepted the nominalist view of the signification [see Glossary] of general language also •accepted the A&NP [see Glossary] as the basis of all reasoning, those two premises committed him to some rather startling conclusions! Some writers who were deservedly celebrated held that the process of •arriving at new truths by reasoning consists merely in •substituting of one set of arbitrary signs for another—a doctrine that they think is conclusively confirmed by the example of algebra. . . . The culminating point of this absurd philosophy is Condillac's aphorism that a science is almost nothing but *une langue bien faite*—i.e. that way to discover the properties of objects is to name them properly! The truth of course is the reverse of that: you can't name things properly until you know what their properties are. . . . Common sense holds—and philosophical analysis confirms this—that the function of names is only to enable us to remember and communicate our thoughts. It's true that they also enormously increase the power of thought itself, but there's nothing mysterious about *how* they do this. They do it by the power inherent in an *artificial memory*, an instrument whose immense potency has been largely neglected. As an artificial memory, language truly is what it is often called, namely *an instrument of thought*; but it's one thing to be the instrument and another to be the exclusive subject on which the instrument is exercised!. . . . There can't be a greater error than to imagine that thought can be carried on with nothing in our mind but names, or that we can make the names think for us.

§3. Those who considered the A&NP as the foundation of the syllogism had a view of •arguments that corresponded to Hobbes's wrong view about propositions (see I.5.2). Because some propositions are merely verbal, Hobbes—apparently wanting a definition that would cover all the cases—defined 'proposition' in a way implying that no proposition declares anything except the meaning of words. If he were right about this—if that's all that could be said about the import of propositions—the theory we'd have to accept about what happens in a syllogism is the commonly accepted one. If the minor premise says only that something A belongs to class M, and the major premise says only that M is included in another class C, the conclusion would be only that whatever is in A is also in C; which tells us only that the classification is consistent with itself. But we have seen that there's more to the meaning of a proposition than its merely putting something into or out of a class. Every proposition that conveys real [see Glossary] information asserts a matter of fact that depends not on classification but on the laws of nature. It asserts that a given object does/doesn't have a given attribute; or it asserts that two attributes or sets of attributes do/don't always or sometimes co-exist.... Any theory of ratiocination that doesn't respect this import of propositions can't possibly be the true one.

Applying this view of propositions to the two premises of a syllogism, here's what we get. The major premise (which, remember, is always universal) says that all things that have a certain attribute (or attributes) A_1 do/don't also have a certain other attribute (or attributes) A_2. The minor premise says that the thing or set of things which are the subject of that premise have A_1; and the conclusion is that they do/don't also have A_2. Thus in our former example,

All men are mortal,
Socrates is a man, therefore
Socrates is mortal,

the subject and predicate of the major premise are connotative terms, denoting objects and connoting [see Glossary] attributes. The assertion in the major premise is that the attributes connoted by 'man' are always conjoined with the attribute called 'mortality'. The minor premise says that the individual named 'Socrates' has the former attributes; and the conclusion is that he also has the attribute *mortality*. [Mill then goes through it again, with 'Socrates is' replaced by 'All kings are'.]

If the major premise is negative, e.g. 'No men are omnipotent', it says that the attributes connoted by 'man' never exist *with* the ones connoted by 'omnipotent'; from which, together with the minor premise, it is concluded that the same incompatibility exists between the attribute *omnipotence* and those constituting *a king*. We can analyse any other syllogism in the same general way.

[In a footnote Mill explains that in this next paragraph 'A_1 coexists with A_2' means only that some one thing has both—not that it has them at the same time.] If we look for the principle or law involved in every such inference, and presupposed in every syllogism whose premises and conclusion aren't merely verbal, what we find is not the unmeaning A&NP but two fundamental principles that strikingly resemble the axioms of mathematics.

(i) The principle of affirmative syllogisms: things that co-exist with the same thing co-exist with one another. Or, more precisely: a thing that co-exists with another thing, which in turn co-exists with a third thing, also co-exists with that third thing.

(ii) The principle of negative syllogisms: a thing that co-exists with another thing which does not co-exist with a certain third thing doesn't itself co-exist with that third thing. These axioms plainly relate to •facts, not to •conventions; and one or other of them is the basis for the legitimacy of every argument in which facts and not conventions are the subject-matter.

[At this point Mill launches a very long footnote responding to Herbert Spencer's criticism of this account of syllogisms. The criticism rests on the assumption that the attribute *humanity* that you have is *like* the attribute *humanity* that I have, but that it's not the very same attribute. We needn't go through Mill's entire treatment of this, but one part of it ought to be given here. Namely:] The meaning of any general name is some outward or inward phenomenon, ultimately consisting of feelings. If the continuity of these feelings is for an instant broken, they are no longer *the same* feelings, in the sense of individual identity. What, then, is the common 'something' that gives a meaning to the general name? Spencer can only say that it is the similarity of the feelings. I reply that *the attribute is precisely that similarity*. The names of attributes are in the last analysis names for the resemblances of our sensations (or other feelings).

§4. I showed in I.6.5 that there are two languages in which we can express all propositions, and therefore all combinations of propositions; I have used one of the two in giving my account of the syllogism, and I should now show how to translate the account into the other language. One of the two is theoretical, the other practical:

- Theoretical: the proposition is regarded as a portion of our knowledge of nature: an affirmative general proposition asserts the speculative truth that whatever has a certain attribute also has a certain other attribute.
- Practical: the proposition is regarded as a memorandum for our guidance—not a part of our knowledge but an aid in our practical activities, enabling us when we learn that an object has attribute A_1 to infer that it also has A_2, thus employing A_1 as a mark or evidence of A_2.

[Mill might have sharpened the contrast between what is *theoretical* (or *speculative*) and what is *practical* by expressing the latter in terms of imperatives: 'When you find that something has A_1, expect it to turn out also to have A_2.']

With propositions looked at in the second way, every syllogism comes within the following general formula:

Attribute A_1 is a mark of attribute A_2,
The given object has the mark A_1, therefore
The given object has the attribute A_2.

For example:

The attributes of *man* are a mark of the attribute *mortality*,
Socrates has the attributes of *man*, therefore
Socrates has the attribute *mortality*.

[And Mill does something similar with the other two syllogisms he has presented—with 'All kings' replacing 'Socrates', and 'are not omnipotent' replacing 'are mortal'.]

To correspond with this alteration in the form of the syllogisms, the underlying axioms ·stated a page back· must also be altered. In this altered phraseology, both those axioms can be brought under one general expression:

Whatever has any mark, has that *of* which it is a mark.

Or when both premises are universal:

Whatever is a mark_1 of any mark_2 is a mark of whatever mark_2 is a mark of.

To check that these mean the same as the previously state ones can be left to the intelligent reader. As we proceed we'll find that this ·practical· phraseology is very convenient. It's the best way I know of to express with precision and force what is aimed at, and what is actually accomplished, in every case where truth is learned by ratiocination.[1]

[1] [This footnote began with a long response to a fairly weak criticism by Bain. A second 'more fundamental objection' of Bain's is also discussed: it turns on whether Mill's 'practical' axiom is fitting for what Bain calls 'Deductive Reasoning', which he says consists in 'the application of a general principle to a special case'. 'Anything that fails to make prominent this circumstance', Bain says, 'is not adapted as a foundation for the syllogism', so the right fundamental axiom is A&NP. Mill says that Bain is stipulating an unduly narrow meaning for the phrase 'deductive reasoning'; and he also counter-attacks: 'If the A&NP makes prominent the fact of the application of a general principle to a particular case, the axiom I propose makes prominent the condition which alone makes that application a real inference.' He continues:]

I conclude, therefore, that both forms have their value and their place in logic. The A&NP should be retained as the fundamental axiom of the logic of mere consistency, often called 'formal logic'; and I have never quarreled with the use of it in that role, or proposed to banish it from treatises on formal logic. But the other is the proper axiom for the logic of the pursuit of truth by way of deduction; and you have to recognise it if you want to show how deductive reasoning can be a road to truth.

Chapter 3: The functions and logical value of the syllogism

§1. I have shown •what the real nature is of the truths that syllogisms deal with (against the common theory's more superficial account of their import), and •what the fundamental axioms are on which the force or syllogisms depends. Our next question about •the syllogistic process, that of reasoning from generals to particulars, is this:

> Is •it a process of inference? a progress from the known to the unknown? a means of reaching items of knowledge that we didn't know before?

Logicians have been remarkably unanimous in their way of answering this question, ·or at least of *implying* an answer·. Everyone says that a syllogism is bad if there's anything more in the conclusion than was assumed in the premises; but that's equivalent to saying that a syllogism can't prove anything that wasn't already known or assumed. Are we to conclude, then, that ratiocination isn't a process of inference? And that the syllogism—which has often been said to be the only genuine reasoning—isn't really entitled to be called 'reasoning' at all? This seems to follow, and indeed everyone who writes about syllogisms accepts that a syllogism can't prove anything not involved in its premises. Yet some writers who explicitly acknowledge this still hold •that the syllogism is the correct analysis of what the mind does when discovering and proving the bulk of the things we believe, in science and in daily life; while those who have avoided this inconsistency have been led to claim that •the syllogistic theory itself is useless and frivolous because of the begging of the question [see Glossary] that they allege to be inherent in every syllogism. I believe that both these opinions are basically wrong, and that the defenders and the attackers of the syllogistic theory seem to have overlooked (or barely glanced at) certain considerations that *have to* be taken seriously if we are to understand the true character of the syllogism and the functions it performs in philosophy.

§2. It must be granted that in every syllogism, considered as an argument to prove the conclusion, there is a begging of the question. When we say,

> All men are mortal,
> Socrates is a man, therefore
> Socrates is mortal,

the enemies of the syllogistic theory are certainly right in saying •that the proposition *Socrates is mortal* is presupposed in *All men are mortal*; •that we can't be sure of the mortality of all men unless we are already sure of the mortality of each individual man; •that any doubt we have about the mortality of Socrates (or anyone else we choose to name) creates the same amount of doubt regarding *All men are mortal*; •that the general principle, instead of being given as evidence of the particular case, can't itself be accepted as true without exception until every shadow of doubt that could affect any individual case within it has been dispelled by evidence from some other source, leaving nothing for the syllogism to prove; •that (in short) no reasoning from general propositions to particular ones can prove anything, because the only particulars we can infer from a general principle are ones that the general principle itself assumes as known.

This doctrine appears to me indestructible. Logicians who couldn't fault it have usually tried to explain it away—not because they found any flaw in the argument itself, but because the contrary opinion seemed to rest on equally indisputable arguments. In the above syllogism or in any of my other examples, isn't it obvious that the conclusion may be a truth that is genuinely new to the person to whom the syllogism is presented? Isn't it a daily experience that truths previously unthought of—facts that haven't been and can't be directly observed—are arrived at through general reasoning? We believe that the Duke of Wellington is mortal. We don't know this by direct observation as long as he isn't yet dead. If we are asked 'Then how do you know he is mortal?' we would probably answer 'Because all men are mortal'. Here, therefore, we arrive at the knowledge of a truth that can't yet be learned by observation, reaching it by reasoning that can be exhibited in the following syllogism:

All men are mortal,
The Duke of Wellington is a man, therefore
The Duke of Wellington is mortal.

And since much of our knowledge is acquired in this way, logicians have persisted in representing the syllogism as a process of inference or proof; though none of them has solved the problem of reconciling that assertion with the thesis that if there's anything in the conclusion that wasn't already asserted in the premises the argument is bad. We can't attach any serious scientific value to the distinction drawn between being •involved by implication in the premises and being •directly asserted in them. When Whately says that the object of reasoning is 'merely to expand and unfold the assertions wrapped up and implied in those with which we set out, and to bring a person to see the full force of what he has admitted', he doesn't meet the real problem that confronts him, namely explaining how a science like geometry can be all 'wrapped up' in a few definitions and axioms. Also, this *defence* of the syllogism doesn't differ much from the *accusation* that its assailants urge against it when they charge it with being useless for pressing the consequences of an admission that an opponent has been trapped into accepting without having understood its full force. When you accepted the major premise you asserted the conclusion; but, says Whately, you asserted it merely by implication, which must mean that you asserted it unconsciously—that you didn't know you were asserting it. But then the difficulty re-appears in this shape—*Oughtn't* you to have known? Were you warranted in asserting the general proposition without having satisfied yourself of the truth of everything that it includes? And if not, isn't the syllogistic art obviously what its attackers affirm it to be, a contrivance for catching you in a trap and holding you fast in it?[1]

§3. There seems to be just one way out of this difficulty. The proposition that the Duke of Wellington is mortal is evidently an inference, something reached as a conclusion from something else. But do we *really* conclude it from the proposition *All men are mortal*? I answer No.

I think the error has consisted in overlooking the distinction between two parts of the process of philosophising, the •inferring part and the •registering part; and ascribing to the latter the functions of the former. . . . If a person is asked a question that he can't answer right now, he may refresh

[1] *Of course* I am not defending any such absurdity as that we actually 'ought to have known' and considered the case of every individual man, past, present, and future, before affirming that all men are mortal: although this interpretation has been put upon what I have been saying. I don't differ from Whately, or from any other defender of the syllogism, on the practical part of the matter; I'm only pointing out an inconsistency in the logical theory of the syllogism as conceived by almost all writers. I don't say that a person who before the Duke of Wellington was born affirmed that all men are mortal *knew* that the Duke of Wellington was mortal; but I do say that he *asserted* it; and I ask for an explanation of the apparent logical fallacy of adducing in proof of the Duke of Wellington's mortality, a general statement that presupposes it. Finding no good solution of this problem in any of the writers on logic, I have tried to provide one.

his memory by turning to a memorandum that he carries about with him. But if he were asked 'How did you come to know that?' he won't answer 'From its being written in my note-book'—unless the book was written, like the Koran, with a quill from the wing of the angel Gabriel!

Assuming that *the Duke of Wellington is mortal* is immediately inferred from *All men are mortal*, where do we get our knowledge of that general truth from? Of course from observation. Now, all we can observe are individual cases. All general truths must be drawn from •individual cases, and they can be analysed back into •these; for a general truth is merely an aggregate of particular truths, a comprehensive expression by which indefinitely many individual facts are affirmed or denied at once. But a general proposition is not merely a compendious form for recording and preserving in the memory a number of particular facts, all of which have been observed. Generalisation is not a process of mere naming, it is also a process of inference. From instances that we have observed we feel [Mill's word] entitled to conclude that what we found true in those instances holds in *all* similar cases, past, present and future. We then use the valuable contrivance of language that enables us to speak of many as if they were one, and record all that we have observed, together with all that we infer from our observations, in one concise expression; and thus we have only one proposition to remember or to communicate, instead of an endless number of them. The results of many observations and inferences, and instructions for making innumerable inferences in unforeseen cases, are compressed into one short sentence.

Thus, when we conclude from the death of John and Thomas, and everyone else we ever heard of who has died, that the Duke of Wellington is mortal like the rest; we may indeed pass through *All men are mortal* as an intermediate stage; but *the inference* doesn't occur in the latter half of the process, the descent from *all men* to *the Duke of Wellington*. The inference is finished when we have asserted that all men are mortal. All we have to do then is to decipher our own notes.

[This paragraph starts with a rejection of a rather obscure view of Whateley's. We can safely rejoin Mill as he emerges from that episode:] If from our experience of John and Thomas and all the others who lived and then died we're entitled to conclude that *all human beings are mortal*, surely we are entitled to conclude immediately from those instances that *the Duke of Wellington is mortal*. The mortality of John and the others is all the evidence we have for the mortality of the Duke of Wellington. *Nothing* is added to the proof by interpolating a general proposition. Since the individual cases are all the evidence we can have—evidence that can't be strengthened by any choice of logical form for it—. . . .I can't see why we should obey the arbitrary fiat of logicians who •forbid us to take the shortest route from these premises to the conclusion, and •require us to get there by travelling the 'high *priori* road! I can't see why it should be impossible to journey from one place to another unless we 'march up a hill, and then march down again'. It may be the safest road, and at the top of the hill there may be a resting-place with a good view of the surrounding country; but for the mere purpose of arriving at our journey's end, our taking that road is optional—it's a question of time, trouble, and danger.

Reasoning from one particular proposition to another without passing through a general proposition—that is not only possible, it's something we do all the time. All our earliest inferences are like that. We draw inferences as soon as we can think at all, and we don't learn to use general propositions until years later. The child, who, having burned his fingers, avoids putting them into the fire a second time has reasoned or inferred, but he hasn't thought of the general maxim *Fire burns*. He knows from memory that he has been burned, and on this evidence he believes that if he puts his finger into the flame of a candle he will be burned again. He believes this each time he encounters flame, but he isn't looking beyond this present flame. He isn't generalising; he's inferring a particular from particulars. That is the way the lower animals reason. There's no evidence that any of them can use signs of the sort that are needed to make general propositions possible. . . . Not only the burned child, but the burned dog, dreads the fire.

When we draw inferences from our personal experience and not from maxims handed down to us by books or tradition, I think we do this by going from particulars to particulars •directly, much oftener than by going •through the intermediate agency of a general proposition. We are constantly reasoning from ourselves to other people, or from one person to another, without taking the trouble to erect our observations into general maxims about human or external nature. When we conclude that '*That*'s how he will act when. . . etc.' we are less likely to be •relying on some general view about people like him, or about people generally, than to be merely •recollecting his feelings and conduct on some previous occasion or •considering how *we* would feel or act ourselves. The village matron who is consulted about the health of a neighbour's child identifies the illness and its remedy simply by remembering what she regards as the similar case of her Lucy. And we all guide ourselves in the same way when we have no definite maxims to steer by; and if we have had an extensive experience, and strongly retain our impressions of it, we can in this manner acquire a considerable power of accurate judgment, without being able to justify it or communicate it to others. . . . An old warrior, after a rapid glance at the outlines of the ground, can immediately give the necessary orders for a skillful arrangement of his troops; and if he hasn't had much theoretical instruction or often been required to answer to other people for his conduct, he may never have had in his mind a single general theorem about the relation between terrain and deployment. His experience of encampments in somewhat similar circumstances has left a number of vivid, unexpressed, ungeneralised analogies in his mind; and the most appropriate of these instantly suggests itself and leads him to a judicious arrangement of his troops.

[After a paragraph giving examples of several different sorts of practical skill based on past experience unaccompanied by any general rules, Mill continues:] Almost every one knows Lord Mansfield's advice to a man of practical good sense who had been appointed governor of a colony and had to preside in its courts of justice, without previous judicial practice or legal education:

> Give your decision boldly, for it will probably be right; but never give reasons for it, for they will almost certainly be wrong.

In cases like this (they are quite common) it would be absurd to suppose that the bad reason was the source of the good decision. Lord Mansfield knew that if any reason were given it would necessarily be an afterthought; the judge is guided by impressions from past experience, without taking the roundabout route through general principles based on them, and if he tries to construct any such principles he will assuredly fail. But Lord Mansfield wouldn't have doubted that a man of equal experience who *also* had a mind stored with general propositions derived by legitimate induction from that experience would be greatly preferable as a judge to one who couldn't be trusted to explain and justify his judgments, however wise they were. When talented men do wonderful things without knowing how, these are examples of the roughest and most spontaneous form of the operations of superior minds. It's a defect in such men, and often a source of errors, not to have generalised as they went on; but generalisation, although it is a help—the most important of all helps—isn't an essential.

Even scientifically educated people who have, in the form of general propositions, a systematic record of the results of the experience of all mankind, needn't always bring in those general propositions when applying that experience to a new case. Dugald Stewart rightly says that although the reasonings in mathematics depend entirely on the axioms, we can see that a proof is conclusive without explicitly bringing in the axioms. The inference that AB is equal to CD because each of them is equal to EF will be accepted by *everybody*, including those who have never heard of the general truth that *things that are equal to the same thing are equal to one another.* When this remark of Stewart's is consistently followed out, I think it goes to the root of the philosophy of ratiocination; and it's unfortunate that he himself stopped short at a much more limited application of it. [Mill explains that Stewart thought he had a good point about *axioms* only, whereas really it holds for *general propositions* of all kinds. He continues:] This thoughtful and elegant writer has perceived an important truth, but only by halves. Having found that in the case of geometrical axioms general names have no magic power to conjure up new truths out of the well where they lie hidden, and not seeing that this is equally true of every kind of generalisation, he contended that axioms are barren of consequences and that the really fruitful truths—the real first principles of geometry—are the definitions. . . . Yet everything he had said about the limited function of the axioms in geometrical demonstrations is equally true of •the definitions. Every demonstration in Euclid could be carried on without •them. You can see this from the ordinary business of proving a geometrical proposition of by means of a diagram. What are our premises when we set out to demonstrate by a diagram some properties of the circle? Not that in all circles the radii are equal, but only that in *this* circle ABC the radii are equal. It's true that to justify this assumption we appeal to the definition of *circle* in general; but all we need if for the assumption to be granted in the case of this particular circle ABC. From this singular proposition, combined with other propositions of a similar kind and other axioms, we prove that a certain conclusion is true not of all circles but of this particular circle ABC; or at least that the conclusion is true of this circle if our assumptions square with the facts. The. . . .general theorem that stands at the head of the demonstration is not the proposition that is actually demonstrated. Only one instance of it is demonstrated; but when we consider *how* this was done we see that the demonstration could be exactly copied in indefinitely many other instances—in *every* instance that conforms to certain conditions. The device of general language provides us with terms that connote these conditions, which lets us assert this indefinite multitude of truths in a single expression, and this expression is the general theorem. By dropping diagrams and replacing 'ABC' etc. by general phrases, we could prove the general theorem directly, i.e. demonstrate all the cases at once (of course having as our premises the axioms and definitions in their general form). But this only means that if we can prove an individual conclusion by assuming an individual fact, then whenever we are entitled to make an exactly similar assumption we can draw an exactly similar conclusion. The definition is a sort of notice to ourselves and others of what assumptions we think we're entitled to make. . . .

§4. From the points I have been making the following conclusions seem to be established. All inference is from particulars to particulars: general propositions are merely registers of such inferences already made, and short formulae for making more. The major premise of a syllogism, therefore, is a formula of that sort, and the conclusion is not •an inference drawn *from* the formula but •an inference drawn *according to* the formula, because the real premise is the particular facts from which the general proposition was collected by induction. Those facts, and the individual instances that supplied them, may have been forgotten, but a record of them remains. Of course it isn't a record that describes the facts themselves; but it shows what marks off the cases regarding which the facts (when they were known) were regarded as justifying a given inference. Guided by this record we draw our conclusion: which is in effect a conclusion from the forgotten facts! This requires us to read the record correctly, and the rules of the syllogism are a set of precautions to ensure our doing so.

This view of the functions of the syllogism is confirmed by consideration of the very cases that might be expected to be least favourable to it, namely those where ratiocination is independent of any previous induction. I have remarked that an ordinary syllogism is only the second half of the journey from premises to conclusion; but there are special cases in which it is the whole journey. All we can observe are particulars, so all knowledge derived from observation must begin with particulars; but in cases of certain descriptions our knowledge can be thought of as coming to us from sources other than observation. **(a)** It may present itself as coming from *testimony*, which in the given case is accepted as authoritative; and the information communicated by the testimony may involve not only particular facts but general propositions. That's what happens when a scientific doctrine is accepted on the authority of writers, or a theological doctrine is accepted on the authority of Scripture. **(b)** Sometimes the generalisation isn't an *assertion* (in the ordinary sense of the word) at all, but •a command, a •law in the moral and political sense of that word—an expression of a superior's desire that we, or any number of other persons, shall act in accordance with certain general instructions. The fact which this asserts, namely a volition of the legislator, is an individual one; so the proposition is not a general proposition. But it contains a *general* description of the conduct the legislator wants his subjects to perform. The proposition asserts not that all men *are* anything, but that all men *are to do* something.

[The next two paragraphs use 'authority' and 'witness' in ways that might be found confusing but can be understood. Mill's topic in **(a)** is *testimony* considered as providing the premise of an argument; in the context he is discussing the testimony is accepted, taken as •authoritative, and the testifier is a •'witness' in the now outdated sense of someone who asserts or assures us of something. The notion of a witness to particular events isn't at work here, though it is in some other places in this work.]

In both these cases the generalities are the original data, and the particulars are derived from them by a process that is correctly represented as a series of syllogisms. But the real nature of the supposed deductive process is obvious enough. The only question is

- **(a)** Did the authority who declared the general proposition intend to include this case in it? Or
- **(b)** Did the legislator intend his command to apply to the present case?

This is answered by examining how the present case relates to what the authority or legislator had in mind. The object of the inquiry is to discover the witness's or the legislator's intention, through the indication given by their words. . . . The operation is a process not of inference but of interpretation.

That last phrase appears to me to characterise, more aptly than any other, the functions of the syllogism in all cases. When the premises are given by authority, the function of reasoning is to ascertain the testimony of a witness, or the will of a legislator, by interpreting the signs by which one has expressed his assertion and the other his command. Similarly, when the premises are derived from observation, the function of reasoning is to ascertain what we (or our predecessors) formerly thought might be inferred from the observed facts, and to do this by interpreting a memorandum of ours, or of theirs. The memorandum reminds us that evidence (more or less carefully weighed) led us to think that a certain attribute could be inferred wherever we perceive a certain mark. The proposition *All men are mortal* (for instance) shows that we have had experience from which we thought it followed that the attributes connoted by the term 'man' are a mark of mortality. But when we conclude that the Duke of Wellington is mortal, we infer this *not* from the memorandum but from the former experience. . . .

This view of the nature of the syllogism makes sense of something that is otherwise obscure and confused in the theory of Whately and other defenders of the syllogism regarding the limits to its usefulness. They say absolutely explicitly that the only role of general reasoning is to prevent inconsistency in our opinions—i.e. to prevent us from assenting to anything that would contradict something we had previously (on good grounds) assented to. And they say that the only reason a syllogism gives for assenting to the truth of the conclusion is that the supposition that it is false, combined with the supposition that both the premises are true, would lead to a contradiction. This would be a lame account of the real grounds we have for believing the facts that we learn from •reasoning as against •observation. The real reason why we believe that the Duke of Wellington will die is that his fathers and our fathers and all their contemporaries have died. Those facts are the real premises of the reasoning. But what leads us to infer the conclusion from those premises isn't a need to avoid inconsistency! There's no contradiction in supposing that all those persons have died and that the Duke of Wellington will live forever. But there *would* be a contradiction if we •were led by those same premises to make a general assertion including the case of the Duke of Wellington, and then •refused to stand by it in the individual case. We do have to avoid an inconsistency between •the memorandum we make of the inferences that can be justly drawn in future cases and •the inferences we actually draw in those cases. Just as a judge interprets a law so as to avoid giving any decision that doesn't conform to the legislator's intention, so also we *interpret* our own formula so as to avoid drawing inferences that don't conform to our former intention. The rules for this interpretation are the rules of the syllogism: and its sole purpose is to maintain consistency between •the conclusions we draw in every particular case and •the previous general directions for drawing them—whether those general directions were formed by ourselves as the result of induction, or given to us by some competent authority.

§5. I think I have shown that although there's always a process of reasoning or inference where a syllogism is used, the syllogism is not a correct analysis of that process, which in fact is an inference from particulars to particulars (except when it is a mere inference from testimony). This inference is authorized by a previous inference from particular propositions to general ones, and is substantially the same as that; so the inference is an instance of *induction*. But while these conclusions seem to me undeniable, I protest as strongly as Whately does against the doctrine that the syllogistic art is useless for the purposes of reasoning. The reasoning lies in the •act of generalisation, not in •interpreting the record of that act; but the syllogistic form runs an indispensable check on the correctness of the generalisation itself. [The point of that last clause: in arriving at 'Bats have kidneys' from 'Bats are mammals' and 'All mammals have kidneys' we are implicitly running a check on whether we were right to infer 'All mammals have kidneys' from the particular data from which we inferred it.]

We've seen that when we have a collection of particular propositions sufficient for grounding an induction we don't have to form a general proposition; we may instead reason immediately from those particulars to other particulars. But if we're entitled to infer a new •particular proposition, we are also entitled to infer a •general one. If from observation and experiment we can conclude to one new case, we can conclude to indefinitely many cases. . . . Every induction that suffices to prove one fact proves an indefinite multitude of facts; the experience that justifies a single prediction must suffice to support a general theorem. It's extremely important to discover this theorem and state it in a general a form as possible declare; in that way we place before our minds the whole range of what our evidence must prove if it proves anything.

. . . .In reasoning from a set of individual observations to some new and unobserved case that we're not perfectly acquainted with (or we wouldn't be inquiring into it), and that we are probably especially interested in (if not, why are we inquiring into it?), there's very little to protect us from •becoming careless, or from •letting our thought be biased by our wishes or our imagination and thus •accepting insufficient evidence as sufficient. But if instead of concluding straight to the particular case x we place before ourselves an entire class of facts—a general proposition every bit of which is legitimately inferable from our premises if the inference to x is legitimate—then it's quite likely that if the premises don't support the generalisation it will contain within it some factual proposition which we already know to be false; in that way we'll discover the error in our generalisation ·and be led by that to back off from the inference to x·.

Consider a Roman citizen during the reign of Marcus Aurelius who expects that the emperor's son Commodus will be a just ruler; he is led to that by a natural bias in his thinking produced by the ·excellent· lives and characters of the last few emperors and the present one; and he has a nasty shock when Commodus becomes emperor. He might be saved from this by reflecting that his expectation regarding Commodus couldn't be justifiable unless his evidence for it *also* entitled him to infer some general proposition, e.g. that all Roman emperors are just rulers; that would immediately have reminded him of Nero, Domitian, and other bad emperors, showing him •the falsity of the general proposition and therefore •the insufficiency of his evidence for it, and thus the insufficiency of that same evidence to prove the favourable proposition about Commodus.

Everyone agrees that when an inference is challenged, it's a help to consider parallel cases. Well, by ascending to the general proposition we bring into view not a mere *one* parallel case but *all possible* parallel cases—all cases to which the same set of evidentiary considerations are applicable.

·Summing up·: When we argue from a number of known cases to another case that we think is analogous, it is always possible and usually worthwhile to take our argument by the longer route through •an induction from those known cases to a general proposition followed by •an application of that general proposition to the unknown case. This second part of the operation, which is essentially a process of interpretation, will come down to a syllogism or a series of syllogisms in which the major premises will be general propositions covering whole classes of cases; and every one of these must be true across its whole range if the argument is maintainable. Thus, if any fact in the range of one of these general propositions—and consequently asserted by it—is known or suspected to be other than the proposition asserts it to be, this leads us to know or suspect that the original observations that are the real grounds of our conclusion are not sufficient to support it. And the greater chance of our detecting the weakness of our evidence, the more confident we are entitled to have ·in our conclusion· if no weakness in the evidence appears.

So the value of the syllogistic form and of the rules for using it correctly does not consist in their

> being the form and the rules according to which our reasonings must be made or even usually are made;

but in their

> •providing us with a way of formulating those reasonings that is admirably fitted to bring their inconclusiveness to light if they *are* inconclusive.

An induction from particular propositions to general ones, followed by a syllogistic process from the latter to other particulars. . . ., is a form in which we may reason whenever we choose, and must adopt when there's doubt as to whether we are reasoning validly; though when the case is familiar and not very complicated, and there's no suspicion of error, we may and *do* reason immediately from the known particular cases to unknown ones.[1]

The further uses of the syllogism in the general course of our intellectual operations hardly require illustration, because they are just the known uses of general language. They amount to this:

> The inductions can be made once for all: a single careful interrogation of experience may be enough; and the result can be registered in the form of a general proposition that is committed to memory or to writing, and can then be put to work in syllogisms. The details of our experiments can then be dismissed from the memory (which couldn't retain them all); while the knowledge those details provided for future use. . . .is retained in a commodious and immediately available shape by means of general language.

The down-side of this advantage is that inferences made on insufficient evidence become consecrated—hardened (so to speak) into general maxims—and the mind clings to them from habit, after it has outgrown any liability to be misled by similar appearances if they were now presented for the first time. Because it has forgotten the details, it doesn't think of revising its own former decision. This is an inevitable •drawback, and not a trivial one; but it is greatly outweighed by the immense •benefits of general language.

The use of the syllogism is simply the use of general propositions in reasoning. We can reason without them; in simple and obvious cases we *do*; minds of great sagacity can do it in cases that aren't simple and obvious, provided their experience has provided them with instances essentially similar to every combination of circumstances likely to arise. But lesser minds, and the same minds when not so well supplied with relevant personal experience, are helpless without the aid of general propositions, wherever the case presents the smallest complication. If we didn't make general

[1] The language of ratiocination would fit the real nature of the process better if the relevant general propositions were expressed not in the form 'All men are mortal' or 'Every man is mortal' but rather in the form 'Any man is mortal'. Then we would have the likes of 'The men A, B, C etc. are thus-and-so, therefore any man is thus-and-so'—which is a better representation of the truth that •inductive reasoning is always basically inference from particulars to particulars, and that •the only role of general propositions in reasoning is to vouch for the legitimacy of such inferences.

propositions, few of us would get much further than the simple inferences drawn by the more intelligent of the lower animals. Though not necessary to **reasoning**, general propositions are necessary to any considerable **progress in reasoning**. So it's natural—indeed *indispensable*—to split the process of investigation into two parts: **(i)** obtain general formulae for determining what inferences may be drawn, then **(ii)** draw the inferences. In drawing them we are applying the formulae; and the rules of syllogism are a system of securities for the correctness of the application.

§6. Given that the syllogism is not the universal type [see Glossary] of the reasoning process, what *is* the real type? This comes down to the question:

> What is the nature of the minor premise, and how does it contribute to establishing the conclusion?

We now fully understand that the place that the **major** premise *nominally* occupies in our reasonings *really* belongs to the individual facts of which it expresses the general result. It isn't a real part of the argument, but only an intermediate halting-place for the mind, inserted between the real premises and the conclusion as an important safeguard of the correctness of the process. But the **minor** premise is an indispensable part of the syllogistic expression of an argument: it certainly •is or •corresponds to an equally indispensable part of the argument itself, and we have only to inquire what part. [Note the distinction between 'part of the expression of the argument' and 'part of the argument itself'.]

Thomas Brown. . . .saw the begging of the question that is inherent in every syllogism if we ·wrongly· consider the major premise to be itself the evidence by which the conclusion is proved; he didn't see the immense advantage in security-for-correctness that we get from interposing this step between the real evidence and the conclusion; but he entirely deleted the major premise from the reasoning process, without putting anything in its place. He maintained that our reasonings consist only of the minor premise and the conclusion: *Socrates is a man, therefore Socrates is mortal*, thus suppressing the appeal to former experience as an unnecessary step in the argument! He didn't see the absurdity of this because of his opinion that •reasoning is merely •analysing our own general notions or abstract ideas, and that the proposition *Socrates is mortal* is evolved from the proposition *Socrates is a man* simply by recognising the notion of *mortality* as already contained in our notion of *man*.

[Mill devotes a long paragraph to pursuing Brown. The central point is that Brown sees the need for *something* to connect 'Socrates is a man' with 'Socrates is mortal'; tries to supply it with a thesis about connections between 'ideas'; and Mill argues convincingly that this move either fails utterly or is a reworded version of the generalisation, the erstwhile 'major premise' that Brown has banished. Now, with Brown moved out of the way, Mill returns to the question of the 'universal type' of the reasoning process:]

In the argument proving that Socrates is mortal, one indispensable part of the premises will be this:

> 'My father and his father and A and B and C and indefinitely many other persons, were mortal'

which is merely one way of saying that they died. This is the major premise, no longer begging the question and cut down to what is really known by direct evidence.

To link this proposition with the conclusion 'Socrates is mortal' what is needed is the proposition:

> 'Socrates resembles my father and his father and A and B and C and all the other individuals specified.'

That's what we assert when we say that *Socrates is a man*. By saying this we also assert in what respect he resembles them, namely in the attributes connoted by 'man'. And we *conclude* that he further resembles them in the attribute *mortality*.

§7. So now we have what we were looking for, a universal type of the reasoning process, which comes down to this: *Certain individuals have a given attribute; one or more individuals resemble the former in certain other attributes; therefore they resemble them also in the given attribute.* This doesn't claim, as the syllogism does, to be conclusive merely because of its form; it can't possibly be so. That Q does or doesn't assert the fact that was already asserted in P may be shown by the forms of the expressions, i.e. by a comparison of P's wording with Q's; but if they assert facts that are genuinely different, the question of whether P's truth proves Q's can't be shown by •the language they're expressed in, but must depend on •other considerations. Given the attributes in which Socrates resembles the men who have already died, is it permissible to infer that he resembles them also in being mortal? That is a question of **induction**, and is to be decided by the principles or canons which test the correct performance of that great mental operation. I'll come to those in due course.

If an inference can be drawn regarding •Socrates then it can be drawn regarding •everyone who resembles the observed individuals in the same way that he resembles them, i.e. regarding all mankind. If the argument is admissible in the case of Socrates, therefore, we're free once for all to treat the possession of the attributes of man as a mark, i.e. as satisfactory evidence, of the attribute of mortality. We do this by asserting the universal proposition *All men are mortal* and interpreting this in its application to Socrates and to others as occasion arises. This conveniently divides the entire logical operation into two steps:

(1) ascertaining what attributes are marks of mortality;
(2) ascertaining whether any given individuals possess those marks.

And when we are theorising about the reasoning process, it will generally be advisable to regard this double operation as actually happening, pretending that all reasoning is carried on in the form that it has to be given if we are to subject it to any test of its correct performance.

Every process of thought in which the basic premises are particular propositions—whether the conclusion is a general formula or other particular propositions—is a case of induction; but I'll stay in line with ordinary usage by reserving the name 'induction' for the process of establishing the general proposition, and I'll give to the remaining operation—which is substantially the process of interpreting the general proposition—its usual name 'deduction'. And I'll regard every process in which something is inferred regarding an unobserved case as consisting of •an induction followed by •a deduction. The process doesn't have to be carried out in this form, but it always *can* be, and it *must* be put into this form when assurance of scientific accuracy is needed and desired.

§8. The theory of the syllogism I have been presenting has been accepted by some important thinkers, three of whom are especially valuable allies: Sir John Herschel, Dr. Whewell, and Mr. Bailey. Of these, Herschel regards the doctrine as 'one of the greatest steps that have yet been made in the philosophy of logic.'[1] 'When we consider' (quoting Herschel) 'the deeply ingrained status of the habits and prejudices that it has cast to the winds', there's no cause for anxiety in

[1] He says that it's not strictly 'a discovery' because Berkeley got there first. In a recent careful re-reading of Berkeley's whole works, I haven't found this doctrine in them. Herschel probably meant that it's implied in Berkeley's argument against abstract ideas. But I can't find that Berkeley saw the implication, or ever asked himself what bearing his argument had on the theory of the syllogism. . . .

the fact that other equally respectable thinkers have formed a very different estimate of it. Their principal objection is compactly stated in a sentence by Whately:

> 'In every case where an inference is drawn from induction (unless the name 'induction' is to be given to a mere random guess without any grounds at all) we must form a judgment •that the instance or instances adduced are sufficient to authorise the conclusion; •that it is allowable to take these instances as a sample warranting an inference regarding the whole class'

and the expression of this judgment in words (it has been said by several of my critics) is the major premise.

The major ·premise· affirms the sufficiency of the evidence on which the conclusion rests—I don't just admit this; it's the essence of my own theory. Anyone who admits that the major premise is *only* this adopts my theory in its essentials.

But this recognition of the sufficiency of the evidence—i.e. of the correctness of the induction—is not a part of the induction itself. (If it is, we'll have to accept that a part of everything we do is to satisfy ourselves that we've done it rightly!) We conclude from known instances to unknown by the impulse of our generalising propensity; it's only after much practice and mental discipline that we raise the question of the sufficiency of the evidence; and when we raise it we *go back* along our path and examine whether we were justified in doing what we have provisionally done. To speak of this reflex operation as part of the original one, having to be expressed in words so that the verbal formula will depict the psychological process, strikes me as false psychology.[1] We review our syllogistic processes as well as our inductive ones, and recognise that they have been correctly performed; but logicians don't add a third premise to the syllogism to express this act of recognition. A careful copyist verifies his transcript by collating it with the original;. . . .but we don't call *this* process a part of the act of copying!

The conclusion in an induction is inferred from •the evidence itself, not from •a recognition of the sufficiency of the evidence. I infer that my friend is walking towards me because •I see him, and not because •I recognise that my eyes are open and that eyesight is a means of knowledge. In all operations that require care it's good to assure ourselves that the process has been performed accurately; but the testing of the process is not the process; and even if testing is omitted altogether, the process may still be correct. It's just because the testing *is* omitted in ordinary unscientific reasoning that anything is gained in certainty by putting reasoning into the syllogistic form. Doing our best to make sure that it isn't omitted, we make the testing operation a part of the reasoning process itself: we insist that the inference from particulars to particulars shall pass through a general proposition. But this is •a security for good reasoning, not •a condition of all reasoning; and in some cases it isn't even a security. All our most familiar inferences are made before we learn the use of general propositions; and a person with high untrained intelligence will skillfully apply his acquired experience to adjacent cases, though he would bungle grievously if he tried to fix the limits of the appropriate general theorem. But though he may conclude rightly, he doesn't strictly *know* whether he has done so, because he hasn't tested his reasoning. That is exactly what *forms* of reasoning do for us. We need them to enable us not *to reason* but *to know whether we reason correctly*.

[1] See the important chapter on Belief in Bain's great treatise *The Emotions and the Will*.

And here's another point against the objection. Even when the test has been applied and the sufficiency of the evidence recognised, if it's sufficient to support the general proposition it is also sufficient to support an inference from particulars to particulars without passing through the general proposition. . . . The general conclusion is never legitimate unless the particular one would be so too; and in no intelligible sense can the particular conclusion be said to be 'drawn from' the general one. Whenever there is ground for drawing •any conclusion from particular instances there's ground for •a general conclusion; but however useful it may be to actually draw this conclusion, this can't be required for the validity of the inference in the particular case. . . .

[Mill ends this section with a long footnote replying to an unnamed reviewer of an earlier edition of this work. It's interesting, but doesn't add much to what has been said in the main text. One lordly put-down is memorable: 'If the reviewer does not see that there is a difficulty here, I can only advise him to reconsider the subject until he does: after which he will be a better judge of the success or failure of an attempt to remove the difficulty.']

§9. These considerations enable us to understand •the true nature of what recent writers have called 'formal logic', and •the relation between it and logic in the widest sense of that term. Logic as I conceive it is the entire theory of the ascertainment of reasoned or inferred truth. So formal logic—which Hamilton and Whately have both, from their different points of view, represented as the whole of logic properly so-called—is really a very subordinate part of it, because it's not directly concerned with reasoning or inference in the sense in which that process is a part of the investigation of truth. What, then, is formal logic? The name seems to be properly applied to all the doctrine relating to the equivalence of different modes of expression; the rules for determining when assertions in a given form imply or presuppose the truth or falsity of other assertions. This includes the theory

- •of the import of propositions, and of their conversion, equivalence and opposition;
- •of the wrongly so-called 'inductions' where the 'generalisation' is a mere abridged statement of cases known individually (I'll discuss these in III.2.2); and
- •of the syllogism.

The theory of Naming, and of (what is inseparably connected with it) Definition, though belonging more to the other and larger kind of logic than to formal logic, is a necessary preliminary to the latter also. The end aimed at by formal logic, and attained by obeying its rules, is not truth but consistency. I have shown that this is the only direct purpose of the rules of the syllogism; their intention and effect is simply to keep our inferences or conclusions consistent with our general formulae or directions for drawing them. The logic of consistency is a necessary auxiliary to the logic of truth, for two reasons. **(i)** What is inconsistent with itself or with other propositions that are true can't itself be true. **(ii)** Truth can be successfully pursued only by drawing inferences from experience; if these are justifiable at all they can be generalised, and for their justification to be tested they have to be stated in a generalised form; after which the correctness of their application to particular cases is a question that specially concerns the logic of consistency. This logic doesn't require any previous knowledge of the processes or conclusions of the various sciences, so it can profitably be studied at a much earlier stage of education than can the logic of truth. . . .

Chapter 4: Trains of reasoning, and deductive sciences

§1. In my analysis of the syllogism, we saw that the •minor premise always affirms a resemblance between a new case and some cases previously known; while the •major premise asserts something which, having been found true of those known cases, we think we're entitled to hold true of any other case resembling the former in certain given respects.

In each example presented in chapter 3, the minor premise asserts a resemblance that is obvious to the senses, such as 'Socrates is a man'. If every ratiocination had a minor premise like that, there would be no need for trains of reasoning, and deductive or ratiocinative sciences [see Glossary] wouldn't exist. Trains of reasoning exist only for the sake of extending an induction that is based (as all inductions must be) on observed cases to other cases in which we can't directly observe the fact which is to be proved and can't even directly observe the mark that is to prove it.

§2. Consider the syllogism:

•All cows ruminate,
•This animal right here a cow; therefore
•This animal ruminates.

If the minor premise is true it is obviously so; it's only the major premise that has to be established through a previous process of inquiry; and provided the induction that the major premise expresses was correctly performed, the conclusion about the present animal will be instantly drawn because as soon as she is compared with the formula she will be identified as being included in it. But now consider this:

•All arsenic is poisonous;
•This substance right here is arsenic; therefore
•This substance is poisonous.

The truth of this minor premise may not be obvious at first sight; it may be known only by inference as the conclusion of another argument which, put into the syllogistic form, goes like this:

•Anything which when lighted produces a dark spot on a piece of white porcelain held in the flame, the spot being soluble in hypochloride of calcium, is arsenic;
•This substance right here conforms to this condition; therefore
•This substance is arsenic.

Thus, to establish the final conclusion that this substance is poisonous we need a process which. . . .stands in need of two syllogisms; and we have a train of reasoning.

But when in this way we add syllogism to syllogism, we're really adding induction to induction. For this chain of inference to be possible there must have been two separate inductions. They may well have been based on different sets of individual instances, but they'll have converged in their results so that the instance that is now the subject of inquiry comes within the range of them both. The record of these inductions is contained in the major premises of the two syllogisms. **First observation:** we or others have examined various objects which under the given circumstances yielded a dark spot with the given property, and found that they had the properties connoted by 'arsenic'—they were metallic, volatile, their vapour had a smell of garlic, and so on. **Second observation:** We or others have examined various specimens that had this metallic and volatile character, whose vapour had this smell, etc., and have found them all to be poisonous. **First induction:** We judge that we may extend the first observation to all substances yielding that particular kind of dark spot. **Second induction:** We judge that we may extend the second observation to all metallic and volatile substances resembling those we examined; and consequently not only to those that have •been seen to be such, but also to those that are •concluded to be such by the first induction. The substance before us is only seen to come within the scope of the first induction; but by means of this it is brought within the scope of the second. We are still concluding from particulars to particulars; but now we are concluding from observed particulars to other particulars that aren't—as in the simple ·one-syllogism· case—•seen to resemble them in the relevant respects but are •inferred to do so because they resemble them in something that we have been led by quite a different set of instances to consider as a mark of the former resemblance.

This first example of a train of reasoning is extremely simple—a series consisting of only two syllogisms. Here's a somewhat more complicated example:

•No government that earnestly seeks the good of its subjects is likely to be overthrown;
•The government of X earnestly seeks the good of its subjects; therefore
•The government of X is not likely to be overthrown.

I'm supposing that the major premise is not derived from considerations *a priori* but is a generalisation from history. So it was based on observation of governments concerning whose desire for the good of their subjects there was no doubt. It has been found, or thought to be found, that these governments were not easily overthrown, and it has been judged that those instances justified an extension of the same predicate (·not easily overthrown·) to every government that resembles them in the attribute of desiring earnestly the good of its subjects. But does the government of X resemble them in this respect? This. . . .would have to be proved by another induction, for we can't directly observe the sentiments and desires of the members of the government of X. To prove the minor premise, therefore, we need an argument in this form:

•Every government that acts in manner M desires the good of its subjects;
•The government of X acts in manner M: therefore
•The government of X desires the good of its subjects.

But is it true that the government acts in manner M? This minor also may require proof, by still another induction:

•Whatever is asserted by intelligent and disinterested witnesses may be believed to be true;
•That the government of X acts in manner M is asserted by intelligent and disinterested witnesses; therefore
•That the government of X acts in manner M may be believed to be true.

So the argument consists of three steps. Having the evidence of our senses that the case of the government of X resembles a number of former cases in having something said about it by intelligent and disinterested witnesses, we infer **(i)** that as in those former instances so also in this one, the assertion is true. The assertion in question was that the government of X acts in manner M; other governments or persons have been observed to act in that manner, and they are known to have desired the good of the people; and we infer **(ii)** that the government of X resembles those others not only in its manner of governing but also in desiring the good of its people. This brings the government of X into known resemblance with the other governments that were thought likely to escape revolution; and so by a third induction we infer **(iii)** that the government of X is also likely to escape. This is still reasoning from particulars to particulars, but here we are reasoning to the new instance from three distinct sets of former instances. With only one of these sets of instances—

governments that have been said by intelligent and disinterested witnesses to act in manner M

—do we directly perceive the government of X to be similar; from that similarity we inductively infer that it has the attribute which makes it resemble the second set—

governments that act in manner M

—and that resemblance is our basis for a further induction through which we assimilate the government of X with a third set of instances—

governments that desire the good of their subjects

—and from there we perform our final induction, bringing the government of X into the class of

governments that are not likely to be overthrown

which gives us the final conclusion. [Mill rightly says '*three* sets of instances'; the fourth set he has mentioned is not something we are reasoning *from*.]

§3. Everything that I said in chapter 3 about the general theory of reasoning holds just as much for these more complex examples as it did for chapter 3's simpler ones. The successive general propositions are not steps in the reasoning; they aren't intermediate links in the chain of inference between the observed particulars and the conclusions we draw from them. If we had big enough memories, and enough power to maintain order among a huge mass of details, the reasoning could go through without any general propositions; they are mere formulae for inferring particulars from particulars. [Mill now repeats his thesis about the role of general propositions in reasoning, summing up thus:] The real inference is always from particulars to particulars, from the observed instances to an unobserved one: but in drawing this inference we conform to a formula that we have adopted for our guidance in such operations; it's a record of the criteria by which we thought we could draw the line between legitimate and illegitimate inferences. The real premises are the individual observations. We may have forgotten them (or indeed have never known them, because they weren't made by us). But we have before us ·the general proposition, which provides· proof that we or others once thought those observations to be sufficient for an induction; and any new case has marks tell us whether it would have fallen within the scope of the original induction if it had been known at that time. We may recognise these marks at once, or we may recognise them through the aid of other marks which we take to be marks of the first, on the strength of a previous induction. It may be that these marks of marks are recognised only through a third set of marks;. . . and so on. We can have a train of reasoning of any length to bring a new case within the scope of an induction based on particulars whose similarity to the new case is ascertained only in this indirect manner.

Thus, in the preceding example, the final inductive inference was that the government of X was not likely to be overthrown; this inference was drawn according to a formula in which *desire for the public good* was set down as a mark of *not being likely to be overthrown*; a mark of this mark was *acting in manner M*; and a mark of acting in manner M was *being asserted to do so by intelligent and disinterested witnesses*: and our senses told us that the government of X possessed this mark. Hence that government fell within the last induction, which brought it within all the others. The perceived resemblance of the case to one set of observed particular cases brought it into known resemblance with another set, and that with a third.

[In this paragraph '→' replaces Mill's 'a mark of'.] In the more complex branches of knowledge, the deductions seldom consist of a single chain—

a → b
b → c
c → d, therefore
a → d.

They consist (to carry on the same metaphor) of several chains united at the end-point, like this, for example:

a → d
b → e
c → f
d e f → n, therefore
a b c → n.

[Mill gives an example of a complex inference in optics that has exactly that form, and comments:] Most chains of physical deduction are of this more complicated type; and they occur frequently in mathematics, e.g. in all propositions where the hypothesis includes numerous conditions: 'If a circle be taken, and if within that circle a point be taken, not the centre, and if straight lines be drawn from that point to the circumference, then. . . ' etc.

§4. The view I have taken of reasoning might seem hard to reconcile with the fact that there are deductive or ratiocinative sciences. This might be said:

> 'If all reasoning is induction, *all* the difficulties of philosophical investigation must lie in the inductions; and when these are easy and not open to doubt or hesitation, there could be no science, or anyway no difficulties in science. For example, the existence of an extensive science of mathematics, requiring the highest scientific genius in those who contributed to its creation, and calling for a most continued and vigorous exertion of intellect in order to appropriate it when created, is hard to account for on Mill's theory.'

But what I have been saying in this chapter enables me to remove this difficulty. I have shown that even when the inductions themselves are obvious, it may be really difficult to discover whether the particular case Q we are investigating comes within their scope; and there's plenty of room for scientific ingenuity in combining various inductions in such a way that, by means of one that obviously has Q in its range, Q can be brought within the scope of others that aren't obviously relevant to it.

When in a science the more obvious inductions from direct observations have been made, and general formulas have been framed setting the limits to the range of applicability of these inductions, if every new case that comes up can be at once seen to fall under one of the formulas, the induction is applied to the new case and the business is ended. But it often happens that a new case x arises that doesn't obviously come within the range of any formula that could answer the question we're asking about x. Let us take an instance from geometry.[1] My example will be the fifth proposition of the first book of Euclid. The question to be answered is: Are the angles at the base of an isosceles triangle equal or unequal? Well, what inductions [meaning: was inductively reached conclusions] do we have from which we can infer equality or inequality. For inferring equality:

- Things that coincide when they are applied to each other are equals.
- Things that are equal to the same thing are equals.
- A whole and the sum of its parts are equals.
- The sums of equal things are equals.
- The differences of equal things are equals.

[1] Because this is only an illustration, please allow me to assume that the basic principles of geometry are results of induction. I'll try to prove this in chapter 5.

There are no other basic formulae to prove equality. For inferring inequality:

- •A whole and its parts are unequals.
- •The sums of equal things and unequal things are unequals.
- •The differences of equal things and unequal things are unequals.

In all, eight formulae. The angles at the base of an isosceles triangle don't obviously come within the range of any of these. The formulae specify certain marks of equality/inequality, but the angles can't be perceived intuitively to have any of those marks. But on examination it appears that they have; and we eventually succeed in bringing them under the formula 'The differences of equal things are equal'. Why is it hard to recognise these angles as the differences of equal things? Because each of them is the difference not of merely one pair but of innumerable pairs of angles; and we had to imagine and select two that could either •be intuitively perceived to be equals or •had some of the marks of equality set down in the various formulae. By an exercise of ingenuity (the first time it was done it was a *considerable* exercise of ingenuity) two pairs of angles were identified that united these requisites: **(1)** That their differences were the angles at the base ·of an isosceles triangle· could be perceived intuitively; and **(2)** they had one of the marks of equality, namely coincidence when applied to one another. This coincidence wasn't perceived intuitively, but inferred in conformity with another formula.

For greater clearness, I offer an analysis of the demonstration. Euclid demonstrates his fifth proposition by means of the fourth; but I can't do that because I have undertaken to trace deductive truths not to •prior deductions but to •their original inductive foundation. So I must use the premises of the fourth proposition instead of its conclusion, and prove the fifth directly from first principles. This requires six formulas. [Mill does a conscientious job of proving the proposition and showing how each step in the proof fits with his theory of deduction. It makes for laborious reading, though, and we can skip it at this stage without harming our ability to what follows. The proof is given at the end of Book II on page 138.]

The main problem here was to see the two angles at the base of the triangle ABC as *remainders* made by cutting one pair of angles out of another, while the members of each pair are corresponding angles of triangles that have two sides and the intervening angle equal. It's this happy contrivance that brings so many different inductions to bear on this one particular case. And because this far from obvious procedure has a role to play so near to the threshold of mathematics, you can see how much scope there may be for scientific dexterity in the higher branches of that and other sciences, in order to combine a few simple inductions so as to bring within each of them countless cases that aren't obviously included in it. And you can also see that the processes needed for bringing the inductions together in the right way may be long and complicated, even when each separate induction is easy and simple. All the inductions involved in all of geometry are comprised in those simple ones, the formulae of which are the axioms and a few of the so-called 'definitions'. The remainder of the science is made up of the work of bringing unforeseen cases within these inductions; or (in syllogistic language) of proving the minor premises needed to complete the syllogisms—the major premises being the definitions and axioms. Those definitions and axioms present all the marks by a skillful combination of which it has been found possible to discover and prove everything that is proved in geometry. The marks are few in number, and the inductions that provide them are obvious and familiar; so the whole difficulty of geometry has to do with connecting several of them together so as to construct deductions, i.e. trains of reasoning. Doing this *is* the science of geometry. . . ., so geometry is a deductive science.

§5. In III.4.3 and elsewhere I'll show that there are weighty scientific reasons for making every science a deductive science as far as possible. That is, we should try to construct the science from the fewest and the simplest possible inductions, and to make these—by any combinations, however complicated—enough to prove the science's results. And all these, even the very complex results, *could* if we chose be proved by inductions from specific experience. Every branch of natural science was originally experimental; each generalisation rested on a special induction, and was derived from its own separate set of observations and experiments. From being so-called 'sciences of pure experiment'—or, more correctly, sciences in which most of the reasonings involve only one step and are expressed by single syllogisms—all these sciences have become to some extent sciences of pure reasoning, in which many truths already known by induction from many different sets of experiments are exhibited as deductions or corollaries from inductive propositions of a simpler and more universal character. (I said 'to some extent'; with some sciences it is nearly their whole extent.) Thus mechanics was made mathematical, then hydrostatics, then optics, then acoustics, then thermology; and Newton brought astronomy within the laws of general mechanics. . . . Although by this progressive transformation all sciences become increasingly •deductive, that doesn't mean that they become less •inductive; **every step in a deduction is an induction**. The opposition is not between deductive and inductive, but between deductive and experimental. A science is *experimental* to the extent that every new case with new features needs a new set of observations and experiments—a fresh induction. It is *deductive* to the extent that it can deal with cases of a new kind by bringing them under old inductions, doing this by ascertaining that cases that can't be observed to have the relevant marks do have marks of those marks.

[In this paragraph and the next, 'a → b' replaces Mill's 'a is a mark of b', and 'a ↔ b' replaces his 'a and b are marks of one another'.] So now we can see what the general distinction is between •sciences that can be made deductive and •sciences that must as yet remain experimental. It depends on whether we have been able to discover marks of marks. If our various inductions haven't let us get any further than such propositions as

a → b or a ↔ b
c → d or c ↔ d

without anything to connect a or b with c or d, then we have a science of detached and mutually independent generalisations, such as

•'Acids redden vegetable blues,
•Alkalis colour them green,

from neither of which propositions could we directly or indirectly infer the other; and to the extent that a science is composed of propositions like that, it is purely experimental. Chemistry, in the present state of our knowledge, hasn't yet escaped from being like this. [An essential part of that escape was the discovery of the Periodic Table by Dmitri Mendeleev in 1869—26 years after the first edition of Mill's *System of Logic* and 13 years before the eighth edition, which is what we are reading.] But there are other sciences containing propositions of this kind:

•a → b
•b → c
•c → d
•d → e

and so on. In these sciences we can climb the ladder from a to e by a process of ratiocination; we can conclude that a is a mark of e, and that every object that has the mark a has the property e; even if we have never been able to observe a and e together; and even if d, our only direct mark of e, isn't •perceptible but only •inferable in the objects to which we attribute it. Or, moving from 'chains' to a different metaphor, we may be said to get from a to e *underground*: the marks b, c, d, which indicate the route must all be possessed somewhere by the objects we are investigating, but they are below the surface. The only visible mark is a, and by it we can trace in succession all the rest.

§6. We can now understand how an experimental science may become deductive science merely by the progress of experiment. In an experimental science, the inductions are detached—a → b, c → d, e → f, and so on—but at any time a new set of instances, and thus a new induction, may bridge the gap between two of these unconnected arches. For example., it may turn out that b → c, which enables us now to prove deductively that a → c. And it sometimes happens that some comprehensive induction raises an arch high in the air, bridging over hosts of gaps all at once, so that b, d, f and all the rest turn out to be marks of some one thing, or of different things between which a connection has already been traced. Newton discovered that *all* the motions of *all* the bodies in the solar system (each of which motions had been inferred by a separate logical operation, from separate marks) were marks of

> moving around a common centre, with a centripetal force varying directly as the mass, and inversely as the square of the distance from that centre.

This is the greatest example that has yet occurred of a science that is to a large extent merely experimental being transformed in one stroke into a deductive science.

Transformations like that but on a smaller scale continually take place in the less advanced physical sciences without enabling them to escape the status of experimental sciences. Thus regarding the unconnected propositions cited in §5—

- •Acids redden vegetable blues
- •Alkalis make them green

—Liebig has found that there is nitrogen in all blue colouring matters that are reddened by acids as well as all red colouring matters that are turned blue by alkalis; and this fact may some day provide a connection between those two propositions, by showing that the antagonistic action of acids and alkalis in producing or destroying the colour blue is the result of some one more general law. Whenever detached generalisations come to be connected, that is something gained; but it doesn't do much to give a deductive character to any science as a whole, because the observations and experiments that enable us to inter-connect a few general truths usually reveal to us a greater number of unconnected new ones. Generalisations in chemistry are continually being extended and simplified in this way, but chemistry is mainly an experimental science, and is likely to remain so unless some comprehensive induction is arrived at, which (like Newton's) inter-connects a vast number of the smaller known inductions and immediately changes the whole method of the science. Chemistry has already one great generalisation, which possesses this comprehensive character within one part of chemistry: namely, Dalton's principle—the 'atomic theory' or the doctrine of 'chemical equivalents'—which enables us to a certain extent to know in advance the proportions in which two substances will combine. This is undoubtedly a source of new chemical truths obtainable by deduction, as well as a connecting principle for all similar truths that were previously obtained by experiment.

§7. The discoveries that change a science from experimental to deductive mostly consist in establishing (by deduction or by direct experiment) that the varieties of some familiar kind of phenomenon are uniformly accompanied by varieties of some other phenomenon. The science of sound, which previously stood in the lowest rank of merely experimental sciences, became deductive when experiments showed that every variety of sound was a result of, and therefore a mark of, a distinct and definable variety of wave motion among the particles of the transmitting medium. When this was ascertained, it followed that every relation of succession or co-existence which obtained between phenomena of the more familiar class (·sound·) obtained also between the corresponding phenomena in the other class (·wave-motion·). Every sound, being a mark of a particular wave-motion, became a mark of everything that could be inferred from that motion by the laws of dynamics; and everything that was (according to those same laws) a mark of any wave-motion among the particles of an elastic medium became a mark of the corresponding sound. In this way many previously unsuspected truths about sound can be deduced from the known laws of the propagation of motion through an elastic medium; while empirically known facts about sound come to indicate previously unknown properties of vibrating bodies.

But the grand agent for turning experimental sciences into deductive ones is the science of number. The properties of number are the only known phenomena that are in the strictest sense properties of all things whatsoever. It's not the case that all things have colour, weight, or even size, but all things are numerable [= 'can be counted']. And if we consider this science in its whole extent, from common arithmetic up to the calculus of variations, there seem to be almost countless truths already known, with a promise of indefinitely more.

These truths apply to things only in respect of their •quantity. But if we discover that variations of •quality in some class of phenomena correspond regularly to variations of •quantity in those same phenomena or in some others, every mathematical formula that applies to quantities which vary in that particular manner becomes a mark of a corresponding general truth about the variations in quality that accompany them; and because the science of quantity is entirely deductive (as far as any science can be), the theory of that particular kind of qualities becomes to this extent deductive likewise.

The most striking example of this kind of transformation is the revolution in geometry that originated with Descartes and was completed by Clairaut. This didn't involve an experimental science's becoming deductive; it started with a science that was already deductive and increased—to an unparalleled extent—the range of its deductive processes. These great mathematicians pointed out the importance of the fact that to every variety of

- •position of points,
- •direction of lines, and
- •shape of curves or surfaces

—all of which are •qualities—there is a corresponding relation of •quantity between either two or three straight-line co-ordinates. The upshot of this is that if we know the law according to which those co-ordinates vary relatively to one another, we can infer every other geometrical property—quantitative or qualitative—of the line or surface in question. From this it followed that every geometrical problem could be solved if the corresponding algebraic one could; and geometry received an accession (actual or potential) of new truths, corresponding to every property of numbers that the progress of the calculus had brought (or might in future bring) to light. Mechanics, astronomy, and (in a lesser degree) every branch of natural science have been made algebraic in the same general manner. . . . The varieties of physical phenomena that those sciences deal with have been found to correspond to discoverable varieties in the quantity of some variable. . . .

In these various transformations, the propositions of mathematics are merely doing what is proper to all propositions forming a train of reasoning—namely, enabling us to arrive indirectly, by marks of marks, at properties of objects that we can't (or can't so easily) ascertain by experiment. We travel from a given visible or tangible fact through mathematical truths to the facts that answer our questions. The given fact is a mark that a certain relation holds between the quantities Q_1 of some of the elements that are involved; the proposition that answers our question involves a certain relation between the quantities Q_2 of some other elements; if the quantities Q_2 are dependent in some known manner on Q_1 or vice versa, we can argue from the numerical relations between the quantities Q_1 to determine the relation that holds between the quantities Q_2, the links in the argument being provided by theorems of the calculus. And thus one physical fact becomes a mark of another by being a mark of a mark of a mark of it.

Chapter 5. Demonstration, and necessary truths

§1. If I have been right in chapters 3 and 4 in maintaining that

- •Induction is the basis of all sciences, even the deductive or demonstrative ones,
- •Every step in the ratiocinations even of geometry is an act of induction, and
- •All that happens in a train of reasoning is that many inductions are brought to bear on a single subject of inquiry, using one induction to bring something within the range of another,

what are we to make of the sciences that are wholly or mostly deductive? What gives them the special certainty that is always ascribed to them? Why are they called the *exact sciences*? Why are the phrases 'mathematical certainty' and 'demonstratively evident' commonly used to express the highest degree of assurance that reason can attain? Why is it that almost all philosophers, and even some practitioners of the branches of natural science that have been converted into deductive sciences by the application of mathematics, •hold that mathematics is independent of the evidence of experience and observation and •regard it as a system of 'necessary truth'?

The answer, I think, is that this character of *necessity* that is ascribed to the truths of mathematics is an illusion; and so is the special *certainty* that they are credited with (though I'll later explain some reservations about this). In order to maintain this illusion, it is necessary to suppose that those truths concern the properties of purely imaginary objects. [Mill reminds us of his view that a definition can't imply any proposition except one about the meaning of the defined expression, and that any further content that a 'definition' seems to have comes from its suppressed assumption that *there are* things answering to the expression as thus defined. He continues:] This assumption is not strictly true:

there are no real things that exactly fit the definitions in geometry. There are no

- •points with no size,
- •lines with no breadth,
- •perfectly straight lines,
- •circles with all their radii exactly equal,
- •squares with all their angles perfectly right-angled.

You may want to say that the assumption isn't that such things actually exist but only that they could. I answer that, by any test we have of possibility, they couldn't! So far as we can tell, their existence is inconsistent with the physical constitution of our planet, and perhaps the physical constitution of the universe. To remove this difficulty while holding onto the supposed system of 'necessary truth', it is usually said that the points, lines, circles, and squares that geometry deals with exist merely in our conceptions—they're part of our minds. And our minds, by working on their own materials, construct an *a priori* science the evidentness of which •is purely mental and •has nothing whatever to do with outward experience. This doctrine may have been endorsed by some high authorities, but it appears to me psychologically incorrect. The points, lines, circles and squares that anyone has in his mind are (I submit) simply copies of the points, lines, circles and squares that he has known in his experience. Our idea of a point is simply our idea of the. . . .smallest portion of surface that we can see. A geometrically defined 'line' is wholly inconceivable. We can reason about a line as if it had no breadth; because when a perception is present to our senses, or a conception to our intellects, we can attend to a part of that perception or conception instead of to the whole. (Our ability to do that is the basis for all the control we can have over the operations of our minds.) But we can't conceive of a line without breadth; we can't form a mental picture of such a line; all the lines we have in our minds are lines with breadth. If you doubt this, think about your own experience! I really doubt that anyone who fancies that he can conceive a mathematical 'line' thinks so on the evidence of his consciousness; I suspect that it's rather because he thinks that if such a conception were impossible mathematics couldn't exist as a science. I'll have no trouble showing that this is entirely groundless.

So the definitions of geometry don't exactly correspond to anything in nature or in the human mind; but we can't suppose that the subject-matter of geometry is nonentities. Our only option now is to consider geometry as dealing with lines, angles and figures that really exist; and its 'definitions' must be regarded as some of our first and most obvious generalisations concerning those natural objects. Those generalisations, considered just as generalisations, are flawlessly correct: the equality of all the radii of a circle is true of •all circles insofar as it is true of any •one; but it's not *exactly* true of any circle; it is only *nearly* true—so nearly that we can pretend it to be exactly true without being led into any error of practical importance. When we have occasion to extend these inductions (or their consequences) to cases where the error would be appreciable—to lines of perceptible breadth or thickness, parallels which deviate perceptibly from equidistance, and the like—we correct our conclusions, by combining with them a fresh set of propositions relating to the aberration; just as we also take in propositions relating to the physical or chemical properties of the material, if those properties happen to make any difference to the result. . . . But as long as there's no practical need to attend to any of the properties of the object except its geometrical properties, or to any of the natural irregularities in those, it is convenient to ignore the other properties and the irregularities in these ones, and to reason as if they didn't exist; and we formally announce in the definitions that we intend to proceed on this plan. But our decision to confine our attention to just a few of an object's properties doesn't imply that we conceive or have an idea of the object denuded of its other properties! We are continually thinking of precisely the sorts of objects as we have seen and touched, and with all the properties they naturally have; but for scientific convenience we pretend they have been stripped of all properties except the ones that are relevant to our purpose.

So the unique accuracy the first principles of geometry are supposed to have appears to be fictitious. The assertions on which geometrical reasonings are based don't *exactly* correspond with the facts, any more than do the bases of other sciences; but we suppose that they do, so as to trace the consequences of that supposition. Stewart's is substantially right, I think, in his opinion about the foundations of geometry, namely

•that it is built on hypotheses;
•that this is the sole source of the special certainty it is supposed to have; and
•that in *any* science we can, by reasoning from a set of hypotheses, reach conclusions as certain as those of geometry;

i.e. as strictly in accordance with the hypotheses, and as irresistibly compelling assent on condition that the founding hypotheses are true.[1]

So when the conclusions of geometry are said to be 'necessary truths', their necessity consists only in their validly following from the suppositions from which they are deduced. 'But aren't those suppositions necessary?' They're not even true! They purposely depart, more or less widely, from the truth. The only sense in which the conclusions of any scientific investigation can be called 'necessary' is as a way of saying that they legitimately follow from some assumption which, by the conditions of that inquiry, is *not to be questioned*. . . . The conclusions of all deductive sciences were said by the ancients to be necessary propositions, but that's because they didn't understand the 'not to be questioned' status of the premises. . . .

§2. The important doctrine of Dugald Stewart that I have tried to secure has been contested by Whewell. . . . The supposed refutation of Stewart consists in proving against him (as I have also done here) that the premises of geometry are not •definitions but •assumptions of the real existence of things corresponding to those definitions. But those assumptions are the items that I call 'hypotheses', yet Whewell denies that geometry is founded on hypotheses. So he needs to show that the founding assumptions are absolute truths. But the furthest he goes in that direction is to say that at any rate they aren't *arbitrary* hypotheses; that we aren't at liberty to substitute other hypotheses for them; that. . . .the straight lines that we define, for instance, must be 'those by which angles are contained, those by which triangles are bounded, those of which parallelism may be predicated, and

[1] Bain rightly says that 'hypothesis' is being used here in a somewhat peculiar sense. When in science something is called an 'hypothesis' this usually means that it is **not known** to be true but is surmised to be so because that would account for certain known facts; and the final result of the theoretical inquiry may be to prove its truth. The hypotheses I have spoken of here are not like that; they are **known not** to be literally true, and as much of them as is true is not hypothetical but certain. With 'hypotheses' in either sense, however, we reason not from a truth but from an assumption, and the truth therefore of the conclusions is conditional, not categorical. That is enough to justify. . . .Stewart's use of the term. But we mustn't forget that the hypothetical element in a geometrical definition is the assumption that what is •very nearly true is •exactly so. This unreal exactitude might be called 'a fiction' as properly as 'an hypothesis'; but 'fiction' would be even further than 'hypothesis' would from reminding us of how closely the fictitious point or line is related to the points and lines of which we have experience.

the like'. This is true, but no-one has said that it isn't. Those who say that the premises of geometry are hypotheses aren't bound to maintain them to be hypotheses with no relation whatever to fact. An hypothesis framed for the purpose of scientific inquiry must relate to something x that has real existence (there can't be a science of nonentities), so

- •any hypothesis we make regarding x, to facilitate our study of it, mustn't involve anything that is clearly false and in conflict with x's real nature;
- •we mustn't ascribe to x any property that it doesn't have;
- •all we are free to do is to exaggerate slightly some of x's properties (assuming it to be •completely what it really is •very nearly), and suppressing others.
- •We're absolutely obliged to restore the suppressed properties when their presence or absence would make a significant difference in the truth of our conclusions.

That's the status of the first principles involved in the definitions of geometry. Is it *necessary* for a founding hypothesis to satisfy those constraints? Yes, if no other hypotheses could enable us to deduce conclusions which (with appropriate corrections) would be true of real objects; and the constraints can be brushed aside when our aim is only to •illustrate truths and not to •investigate them. We might suppose an imaginary animal and work out by deduction from the known laws of physiology its natural history; or an imaginary commonwealth, and from the elements composing it work out what would be its fate. Drawing conclusions from such purely arbitrary hypotheses might be a highly useful intellectual exercise; but the conclusions themselves could only teach us what *would be* the properties of objects that don't really exist, so they don't add anything to our knowledge of nature; whereas with an hypothesis that merely strips a real object of some of its properties, without clothing it in false ones, the conclusions will always express. . . .actual truth.

§3. But although Whewell hasn't shaken Stewart's doctrine about the hypothetical status of the first principles of geometry that are involved in the so-called 'definitions', I think he has greatly the advantage of Stewart on another important point—namely the necessity of including axioms as well as definitions among those first principles. Some of Euclid's axioms could be exhibited in the form of definitions, or deduced by reasoning from propositions similar to 'definitions'. Instead of the axiom

> Magnitudes that can be made to coincide are equal,

we could introduce the definition

> Equal magnitudes are those that may be so applied to one another as to coincide;

and then the three next axioms—

- •Magnitudes that are equal to the same are equal to one another
- •If equals are added to equals, the sums are equal
- •If equals are taken from equals, the remainders are equal

—can be proved by an imaginary superposition like the one used in the demonstration of the fourth proposition of the first book of Euclid. But though these and several others may be deleted from the list of first principles because they can be demonstrated (though they don't need to be), the list of axioms will still contain two or three fundamental truths that can't be demonstrated. For example,

- •Two straight lines can't enclose a space

or its equivalent

- •Straight lines that coincide at two points coincide altogether and something to the effect that

- •Two straight lines that intersect each other can't both be parallel to a third straight line.[1]

Unlike the other class of fundamental principles that are involved in the definitions, the axioms—those that can't be demonstrated as well as those that can—are true without any mixture of hypothesis. [He means: they are just *true*, not merely 'true-if-hypothesis-H-is true'.] 'Things that are equal to the same thing are equal to one another' is just as true of the lines and figures in nature as it would be of the imaginary ones assumed in the definitions. It's like this in most other sciences too. Almost all sciences have some general propositions that are •exactly true, while most are only •approximations to the truth. For example, •the first law of motion (the continuance of a movement until stopped or slackened by some resisting force) is true without qualification or error. •The rotation of the earth in twenty-four hours. . . .has gone on since the first accurate observations, without the increase or diminution of one second in all that period. These inductions don't need any fiction—·any hypothesis·—to be accepted as accurately true; but there are others, such as the propositions about the shape of the earth, that are only approximations to the truth. To use them in the advance of our knowledge we have to pretend that they are exactly true, though they really fall short of that.

§4. What is the ground of our belief in axioms? What is our evidence for them? I answer: they are experimental truths, generalisations from observation. The proposition 'Two straight lines can't enclose a space'—or in other words 'Two straight lines that have once met don't meet again, but continue to diverge'—is an induction from the evidence of our senses.

This opinion goes against a scientific prejudice of long standing and great strength, and probably nothing that I say in this work will be as unfavourably received as this will. But it isn't a new opinion; and even if it were, it should be judged not by its novelty but by the strength of the arguments in its favour. It is very fortunate that such an eminent defender of the contrary opinion as Whewell has presented an elaborate treatment of the whole theory of axioms, trying to construct the philosophy of the mathematical and physical sciences on the basis of the doctrine that I am now opposing. Anyone who wants to get to the bottom of a subject must rejoice to see the opposite side worthily represented. If what Whewell says in support of an opinion that he has made the foundation of a systematic work can be shown not to be conclusive, that will be *enough*—I shan't need to look elsewhere for stronger arguments and a more powerful adversary!

I don't have to *show* that the truths we call 'axioms' were first suggested by observation, and that we would never have known that two straight lines can't enclose a space if we had never seen a straight line. Whewell admits this, as do all the recent supporters of his position. But they contend that experience doesn't *prove* the axiom; that its truth is perceived *a priori* by the constitution of the mind itself, from the first moment when the meaning of the proposition is grasped, with no need to verify it by repeated trials as we have to do with truths that really are ascertained by observation.

But they can't deny that the truth of the axiom 'Two straight lines can't enclose a space', even if it is evident

[1] We could insert this last property into the definition of 'parallel', making it require both that **(i)** when produced indefinitely the lines will never meet and **(ii)** that any straight line that intersects one of them will, if prolonged, meet the other. But this still doesn't remove the need for the assumption; we are still obliged to take for granted the geometrical truth that all straight lines in the same plane that have property **(i)** also have **(ii)**. For if that weren't so, the demonstrations of later parts of the theory of parallels would fail.

independently of experience, is also evident *from* experience. Whether or not the axiom •needs confirmation, it •receives confirmation in almost every instant of our lives: if we look at two straight lines that intersect one another we can't help seeing that from the point of intersection they continue to diverge more and more. Empirical evidence crowds in on us in such endless profusion, with *no* cases where one might suspect an exception to the rule, that we would soon have stronger ground for believing the axiom than we have for almost any of the general truths that we confessedly learn from the evidence of our senses. [•Why 'we *would* soon have. . . '? Because Mill is saying 'Even if experience were all we had to go on, we would. . . '. He thinks that experience *is* all we have, but he is temporarily setting that aside and writing as though from Whewell's point of view. •By 'we confessedly learn. . . ' he means that it's generally agreed, not a matter of controversy, that we learn. . . .] Independently of *a priori* evidence, we would certainly believe it with an intensity of conviction far greater than we give to any ordinary physical truth; and we do this so early in our life that we don't remember what was going on with us when we acquired this knowledge. So why assume that our recognition of these truths has a different origin from the rest of our knowledge, when its existence is perfectly accounted for by supposing its origin to be the same?. . . . The onus of proof lies on the advocates of the contrary opinion: it is for them to point out some *fact* that conflicts with the supposition that this part of our knowledge of nature has the same source as every other part.[1]

They could do this if they could prove that we had the conviction (at least practically) so early in infancy that we hadn't yet received any of the sense-impressions that are the basis of the rival theory, ·the one I accept·. But no-one can prove this, because it concerns a time that is too far back for memory and too obscure for external observation. The friends of the *a priori* theory are obliged to rely on other arguments—basically just two of them. I'll try to state them as clearly and forcibly as possible.

·FIRST ARGUMENT FOR A PRIORI GEOMETRICAL KNOWLEDGE·

§5. The first argument goes like this:

> If our assent to the proposition that *two straight lines can't enclose a space* were derived from the senses, the only way we could be convinced of its truth would be by actual trial, i.e. by seeing or feeling the straight lines; but in fact it is seen to be true by merely thinking of them. *That a stone thrown into water*

[1] Some writers say that 'Two straight lines can't enclose a space' couldn't become known to us through experience for this reason:

> If the straight lines in question are absolutely without breadth and absolutely straight, experience can't show us that they can't enclose a space because we don't *have* any experience of such lines. And if the lines are of the sort we do meet with in experience—straight enough for practical purposes but actually slightly zigzag, and with some thickness—the axiom isn't true, for two of those lines can and sometimes do enclose a small portion of space. In neither case, therefore, does experience prove the axiom.

Those who use this argument to show that geometrical axioms can't be proved by induction show themselves unfamiliar with a common and perfectly valid mode of inductive proof—namely proof by approximation. Though experience doesn't present lines so unimpeachably straight that two of them are incapable of enclosing the smallest space, it presents us with gradations of lines having less and less thickness or zigzagging; this constitutes a series of which the 'straight line' of the geometrical definition is the ideal limit. What observation shows us is that the nearer the straight lines of experience come to having no thickness and no kinks, the nearer the space-enclosing power of any two of them comes to zero. The inference that if they had no thickness or kinks at all they would enclose no space at all is a correct inductive inference from these facts. It fits one of the four inductive methods that I'll present in III.8, namely the Method of Concomitant Variations. . . .

> *goes to the bottom* can be perceived by our senses, and we couldn't be led to it by merely thinking of a stone thrown into water. It's not like that with the axioms about straight lines: if I could be made to conceive what a straight line is, without having seen one, I would at once recognise that two such lines can't enclose a space. Intuition is 'imaginary looking' (Whewell's phrase) but experience must be real looking: if we see a property of straight lines to be true by merely imagining ourselves to be looking at them, the ground for our belief can't be the senses or experience; it must be something mental.

Something that could be added to this argument for this particular axiom (not for all of them) is the following:

> The evidence of eyesight isn't merely unnecessary for this axiom; it is downright impossible. What does the axiom say? That two straight lines *cannot* enclose a space; that after intersecting once they will continue to diverge from one another even if they are prolonged to infinity. How could we, in any single case, *see* that this is so? However far we follow the lines, we can't follow them to infinity; so we have to stop somewhere; and for all our senses can say to the contrary, the lines may just beyond our stopping-point begin to approach, and eventually to meet again. So if we didn't have a proof of the impossibility other than what observation provides for us, we would have no ground for believing the axiom at all.

I don't think I can be accused of understating these arguments.

A satisfactory answer to this line of thought will be found, I think, if we bring in one of the characteristic properties of geometrical forms—their ability to be depicted in the imagination with a distinctness equal to reality, i.e. the way our ideas of form exactly resemble the sensations that suggest them. This enables us **(i)** to make (at least with a little practice) mental pictures of all possible combinations of lines and angles, pictures that resemble the realities as well as any that we could make on paper; and **(ii)** to make those pictures just as fit subjects for geometrical experimentation as the realities themselves. ·How can that be? Well·, if the pictures are accurate enough, they will exhibit all the properties that the realities would show at one given instant and on simple inspection; and in geometry those are the only properties we are concerned with; we don't care about the thing that pictures couldn't exhibit, namely the inter-actions between bodies. So even if the 'experiments' (which in this case consist merely in attentive contemplation) were practised not on external objects but solely on what we call our 'ideas', i.e. on the diagrams in our minds, the foundations of geometry would still be laid in direct experience. In *all* systems of experimentation we take some objects to serve as representatives of everything that resembles them; and in our present case the conditions that qualify a real object to be the representative of its class are completely fulfilled by an object existing only in our imagination. Thus, without denying that we could satisfy ourselves that two straight lines can't enclose a space by merely thinking of straight lines without actually looking at them, I contend that we don't believe this truth simply on the basis of the imaginary intuition, but because we know that the imaginary lines exactly resemble real ones, and that we can conclude from

- •imaginary lines to real ones

with as much certainty as we could conclude from

- •one real line to another real line.

So the conclusion is still an induction from observation. And we wouldn't be entitled to substitute observation of our mental image for observation of the reality if we hadn't learned from long experience that the properties of the reality are faithfully represented in the image; just as we would be scientifically justified in describing an animal that we have never seen, on the basis of a picture made of it with a photograph; but only after we have learned from ample experience that observation of such a picture is precisely equivalent to observation of the original.

These considerations also remove the objection arising from the impossibility of looking the whole way along the lines as they extend to infinity. It's true that we couldn't actually see that two given lines never meet unless we followed them to infinity; but we do know that if they ever do meet—or if after diverging from one another they begin again to approach—this must happen at a *finite* distance from us. Supposing that it does happen, we can go there in imagination and form a mental image of the appearance that one or both of the lines must present at that point; and we can rely on that as being precisely similar to the reality. Now, whether we •focus on this imaginary picture or •remember the generalisations we have made from former views, we learn from our experience that a line which, after diverging from another straight line, begins to approach to it, produces the impression on our senses that we call 'a bent line', not 'a straight line'.[1]

The preceding argument, which I think is unanswerable, merges in a still more comprehensive one that has been stated most clearly and conclusively by Bain. The psychological reason why axioms. . . .can be learned from the •idea only, without referring to the •fact, is that in the process of •acquiring the idea we have •learned the facts. . . . He writes:

> 'We needed concrete experience in the first instance, to get the notion of whole and part; but once the notion is acquired it implies that the whole is greater than the part. In fact, we couldn't *have* the notion without an experience tantamount to this conclusion. . . When we have mastered the notion of straightness, we have also mastered the aspect of it expressed by the statement that two straight lines can't enclose a space. No intuitive or innate powers or perceptions are needed. . . We can't have the full meaning of straightness without

[1] Whewell thinks it unreasonable to contend that we know by experience that our idea of a line exactly resembles a real line. He writes: 'I don't see how we can compare our ideas with the realities, given that we know the realities only by our ideas.' We know the realities by our *sensations*. Whewell surely doesn't hold the 'doctrine of perception by means of ideas' which Reid took so much trouble to refute. If Whewell doubts whether we compare our ideas with the corresponding sensations and assume that they are alike, let me ask him: Why do we judge that a portrait of someone not present is like the original? Surely because it is like our idea or mental image of the person, and because that idea is like the man himself.

Whewell also challenges the thesis that this resemblance of ideas to the sensations of which they are copies is a special feature of ideas of *space*. I reply that I assert no such thesis. Ideas of space are special only in *how closely and exactly* they resemble the corresponding sensations. No-one would claim to imagine a colour or odour as closely and accurately as almost everyone can mentally reproduce an image of a straight line or a triangle. [Mill goes on to say that the imagining of colours or odours can be put to use. 'Which has the darker blue—the flower that I gave you a week ago or the one I put on your father's grave last month?' Someone *might* be able to answer this by comparing his mental pictures of the flowers and reading off his answer from that comparison. He *might*, but, Mill continues:] People differ widely in how precisely they can recollect things: one person, when he has looked someone in the face for half a minute, can draw an accurate likeness of him from memory; another may have seen him every day for six months yet hardly know whether his nose is long or short. But everyone has a perfectly distinct mental image of a straight line, a circle, or a rectangle. And everyone confidently argues from these mental images to the corresponding outward things. We can and continually do study nature in our recollections, when the objects themselves are absent; and in the case of geometrical forms we can perfectly trust our recollections, while in most other cases we can trust them only imperfectly.

> comparing straight objects with one another and with bent or crooked objects. One result of this comparison is that •straightness in two lines is seen to be incompatible with •enclosing a space; the enclosure of space involves crookedness in at least one of the lines.'

And similarly, in the case of every first principle, 'the same knowledge that makes it understood, suffices to verify it'. The more this observation is considered the more (I am convinced) it will be felt to go to the very root of the controversy.

·SECOND ARGUMENT FOR A PRIORI GEOMETRICAL KNOWLEDGE·

§6. Now for the second argument in support of the theory that axioms are *a priori* truths. It goes like this:

> Axioms are conceived by us not only as •true but as •universally and necessarily true. Now, experience can't possibly tell us this about any proposition. I may have seen snow a hundred times, and seen that it was white, but this can't give me entire assurance even that all snow is white, let alone that ·all· snow *must* be white.

[Mill continues with repetitions of this line of thought, quoted from Whewell, including:] 'Experience. . . .contemplates external objects, but it can't detect any internal bond that indissolubly connects the future with the past, the possible with the real. To learn a proposition by experience, and to see it to be necessarily true, are two altogether different processes of thought.' And Whewell adds: 'If anyone doesn't clearly grasp this distinction between necessary and contingent truths, he won't be able to join in our researches into the foundations of human knowledge—or indeed to pursue with success any speculation on the subject.'

What *is* the distinction the non-recognition of which incurs this denunciation? Whewell answers:

> 'Necessary truths are those in which we not only learn that the proposition is true but see that it must be true; in which the negation of the truth is not only false but impossible; in which we can't—even by an effort of imagination, or in a supposition—conceive the reverse of what is asserted. That there are such truths can't be doubted: all relations of number, for example. Three and two added together make five. We can't conceive it to be otherwise. We can't by any freak of thought imagine three and two to make seven.'

Whewell has naturally and properly used a variety of phrases to bring his meaning more forcibly home, but I presume that he would allow that they are all equivalent, and that what he means by 'a necessary truth' would be sufficiently defined as 'a proposition the negation of which is not only false but inconceivable'. I can't find in any of his expressions. . . .a meaning beyond this, and I don't think he would contend that they mean anything more.

So this is the principle asserted: that if the negation of proposition P is inconceivable,. . . .P must rest on evidence of a higher and more forceful description than any that experience can provide.

I'm surprised that so much stress should be laid on inconceivability, when there's so much empirical evidence that our (in)ability to conceive x has very little to do with x's possibility, and a great deal to do with. . . .the past history and habits of our own minds. We find it extremely difficult to conceive as possible something that contradicts long established and familiar experience, or even merely old familiar habits of thought. Everyone knows this; it's a necessary result of the fundamental laws of the human mind, specifically of the primary law of *association*. When we have often seen and thought of two things together, and have *never* seen or thought of them separately, there's an increasing difficulty—which may in the end become insuperable—of conceiving them apart. This is most conspicuous in uneducated persons, who are in general utterly unable to separate any two ideas that have become firmly associated in their minds; and if persons with developed intellects do any better in this, it's only because—having seen and heard and read more, and being more accustomed to exercise their imagination—they have experienced their sensations and thoughts in more varied combinations, which has prevented them from forming many of these inseparable associations. But. . . .·even· the most practised intellect is not exempt from the universal laws of our conceptive faculty. If daily habit presents to someone for a long time two facts in combination, and if he isn't led during that period. . . .to •think of them apart, he will probably in time become unable to •do that even by the strongest effort; and the supposition that the two facts can be separated in nature will eventually present itself to his mind with all the marks of an *inconceivable* phenomenon. There are remarkable examples of this in the history of science: cases where highly educated men rejected as impossible, because inconceivable, things that their posterity. . . .found it quite easy to conceive and that everybody now knows to be true. There was a time when men with the most cultivated intellects and the greatest freedom from the domination of early prejudice couldn't believe in the existence of antipodes, because they couldn't conceive the force of gravity acting upward instead of downward. The Cartesians long rejected the Newtonian doctrine of the gravitation of all bodies toward one another on the strength of the proposition that *a body can't •act where it •is not*, the reverse of which seemed to them inconceivable. All the cumbrous machinery of imaginary vortices [see Glossary], assumed with *no* evidence, appeared to these philosophers a more rational account of the heavenly motions than one involving what seemed to them such a great absurdity.[1]

No doubt they found it as impossible to conceive **(a)** a body acting on the earth from the distance of the sun or moon as we find it to conceive **(b)** an end to space or time, or **(c)** two straight lines enclosing a space. Newton himself wasn't able to conceive **(a)**, which is why we have his hypothesis of a 'subtle ether', the hidden cause of gravitation; and his writings show that although he regarded the particular nature of the intermediate agency to be a matter of conjecture, he had no doubt that there must be some such agency.

If it's so natural to the human mind, even in a high state of culture, to •be unable to conceive and therefore to •believe impossible something that is later found to be not only conceivable but true, it's not surprising that in cases where

> the association is still older, more confirmed, and more familiar, and nothing ever occurs to shake our conviction, or even suggest to us any conception at variance with the association,

the •acquired incapacity continues and is mistaken for a •natural incapacity! Our experience of the varieties in nature does enable us, within limits, to conceive other varieties

[1] It would be hard to name a man more remarkable for the •greatness and the wide •range of his mental accomplishments than Leibniz. Yet this eminent man rejected Newton's account of the solar system on the grounds that God *could not* make a body revolve round a distant centre except by some mechanism that pushed them into moving like that, or by miracle. He wrote to the Abbé Conti: 'Anything that can't be explained by the nature of created things is miraculous. It won't do to say "God has made such-and-such a law of nature, so the thing is natural." The law has to be something that can be carried out by the natures of created things. For example, if God gave a law compelling a free body to turn around a certain centre, he would have to •connect it with other bodies which by pushing it forced it always to stay in its circular orbit, or •give that job to an angel, or •do it by a miracle; because what the body would do *naturally* is to leave the orbit along a tangent.'

analogous to them. . . . But when experience affords no model on which to shape the new conception, how *could* we form it? How can we imagine an end to space or time? We never saw an object without something beyond it, or experienced a feeling without something following it. So when we try to conceive the last point of space, the idea of other points beyond it arises irresistibly. When we try to imagine the last instant of time, we can't help conceiving another instant after it. There is no need to assume, as do the Kantians, a peculiar fundamental law of the mind to account for the feeling of infinity inherent in our conceptions of space and time; that apparent infinity is well enough accounted for by simpler laws that everyone accepts.

Now turn back to geometrical axioms such as *Two straight lines can't enclose a space*—a truth that is confirmed by our very earliest impressions of the external world—how could its falsity *possibly* be conceivable to us?. . . . What analogy do we have, what similar order of facts in any other branch of our experience, to help us to conceive two straight lines enclosing a space? Also, remember my point that our ideas or mental images of *form* exactly resemble the things they are ideas of, and represent them well enough for the purposes of scientific observation. From this, and from the intuitive character of the observation (which in this case reduces itself to simple inspection), we can't call up in our imagination two straight lines, so as to try to conceive them enclosing a space, without by that very act repeating the scientific experiment that establishes the contrary. Will it really be contended that in a case like this the inconceivability of the axiom's falsity is evidence against the thesis that our belief in the axiom has an empirical origin? Isn't it clear that however that belief originated, the impossibility of our conceiving the negative of it will be the same? Whewell urges those who have difficulty recognising his distinction between necessary and contingent truths to *study geometry*, and I can assure him that I have conscientiously done that. Now I in return, with equal confidence, urge those who agree with him to *study the general laws of association.* I'm convinced that a moderate familiarity with those laws is all that is needed to dispel the illusion that ascribes a special necessity to our earliest inductions from experience, and measures the •possibility of things in themselves by •the human ability to conceive them.

Whewell himself has both **(a)** confirmed by his testimony the effect of habitual association in making an empirical truth appear to be necessary, and **(b)** provided a striking instance of the law of in his own person.

·(a) WHEWELL **IMPLYING** THAT ASSOCIATIONS CREATE INCONCEIVABILITY·

In his *Philosophy of the Inductive Sciences* Whewell repeatedly says something striking about now-established propositions that we know were discovered gradually and by great efforts of genius and patience. Once they are established, he says, we find it •hard to conceive that they weren't recognised from the outset by everyone whose mind was in good order. If we didn't know the history of their discovery we would find it •impossible.

> 'We now despise the opponents of Copernicus who couldn't conceive the sun's appearing to move when really it doesn't;. . . .the opponents of Newton who held there was something absurd in his doctrine that differently coloured light-rays are refracted at different angles; those who thought that when elements combine, their sensible qualities must show up in the compound; and those who were reluctant to give up the classification vegetables into herbs, shrubs, and trees. We can't help thinking that men must have been

> thick-headed to find a difficulty in admitting what is to us so plain and simple. . . . But most of those people who were on the losing side were no more prejudiced or stupid or narrow-minded than most of us are today; and the side they backed was far from being obviously wrong until it had been condemned as wrong by the result of the war ·between theories·. . . In most of these cases the victory of truth has been so complete that now we can hardly imagine the struggle to have been necessary. *The very essence of these triumphs is that they lead us to regard the views we reject as not only false but inconceivable.*'

This last proposition is precisely what I contend for; and it's all I need to overthrow Whewell's theory about the nature of the evidentness of axioms. For what is that theory? That the truth of axioms can't have been learned from experience because their falsity is inconceivable. But Whewell himself says that we're continually led in the natural progress of thought to regard as inconceivable propositions that our forefathers not only conceived but believed—and indeed (he might have added) were unable to conceive to be false. . . . After such a complete admission that inconceivability is an accidental thing, not inherent in the phenomenon itself but dependent on the mental history of the person who tries to conceive it, how can he ever call on us to reject a proposition as impossible simply because it is inconceivable? Yet he does so; and along the way he unintentionally provides some very remarkable examples of the very illusion—·the illusion of inherent inconceivability·—that he has himself so clearly pointed out. I select his remarks on the evidentness of the three laws of motion, and of the atomic theory.

With respect to the laws of motion, Whewell says:

> 'No-one can doubt that in historical fact these laws were collected from experience. This isn't a mere conjecture. We know the time, the persons, the circumstances, belonging to each step of each discovery.'

. . . .And not only were these laws far from intuitively evident, but some of them were originally paradoxes. The first law was especially so. *A moving body will continue moving at the same speed for ever unless some new force acts on it*—this was a proposition that mankind for a long time found almost impossible to believe. It went against apparent experience of the most familiar kind, which taught that it was the nature of motion •to lessen gradually and at last •to stop. Yet once the contrary doctrine was firmly established, Whewell points out, mathematicians rapidly began to believe that these laws—contradictory to first appearances, and hard to make familiar to the minds of the scientific world even after full proof had been obtained—were under 'a demonstrable necessity, compelling them to be such as they are and no other'; and Whewell himself, though not venturing 'absolutely to pronounce' that all these laws 'can be rigorously traced to an absolute necessity in the nature of things', *does* have that view of the law I have mentioned, of which he says:

> 'Though the discovery of the first law of motion was made, historically speaking, by means of experiment, we have now attained a point of view in which we see that it could have been certainly known to be true, independently of experience.'

Can there be a more striking instance than this of the effect of association that I have described? Philosophers for generations have tremendous difficulty putting certain ideas together; they at last succeed in doing so; and after repeating this process often enough they first imagine a natural bond between the ideas, and then experience a growing difficulty, which eventually grows to an impossibility, of separating them from one another. If that's what happens to empirical beliefs that began only yesterday and are in opposition to first appearances, how must it fare with beliefs that square with appearances that are familiar from the first dawn of intelligence. . . .?

·(b) WHEWELL **ILLUSTRATING IN HIMSELF** THE POWER OF ASSOCIATIONS TO CREATE INCONCEIVABILITY·

In discussing the atomic theory Whewell provides a truly astonishing example—it could be called the *reductio ad absurdum* [see Glossary] of the theory of inconceivability. Speaking of the laws of chemical composition, he says:

> 'It's certain that these laws could never have been clearly understood, and therefore never firmly established, without laborious and exact experiments. But I venture to say that once they are known, they have an evidentness that mere experiment could never provide. For how in fact *can* we conceive of combinations otherwise than as definite in kind and quality? If we were to suppose each element ready to combine with any other indifferently, and indifferently in any quantity, we would have a world where all would be confusion and indefiniteness. There would be no fixed kinds of bodies. Salts, and stones, and ores would gradually shade into each other. But we know that the world consists of bodies distinguishable from each other by definite differences, capable of being classified and named, and of having general propositions asserted about them. And as we can't conceive a world that is not like that, it seems that we can't conceive a state of things in which the laws of the combination of elements should not be of that definite and measured kind that I have been discussing.

That a philosopher of Whewell's eminence should gravely assert that we can't conceive a world in which the simple elements combined in other than definite proportions; that by meditating on a scientific truth, the original discoverer of which was still living, he made the association in his own mind between the idea of •combination and the idea of •constant proportions so familiar and intimate that he can't conceive of one fact without the other; is such a striking *instance* of the mental law that I am defending that there's no need for me to offer a word of comment on it!

[Mill now reports a move that Whewell makes in his most recent writings on this topic. He says that the necessity of the atomic theory is merely something that 'philosophical chemists in a future generation may possibly see'. And that what he is talking about is the inability to conceive something 'distinctly': something that is really impossible may be (vaguely) conceived by the man in the street; and it may be (a little vaguely) conceived by a scientist until he gets *up* to the level of finding it inconceivable. Thus:] Necessary truths are not those of which we can't conceive the contrary, but those of which we can't distinctly conceive the contrary. . . . By the ever-increasing distinctness with which scientific men grasp the general conceptions of science, they eventually come to perceive that there are certain laws of nature which, though as a matter of fact they were learned from experience, we can't, now that we know them, distinctly conceive to be other than they are. [This paragraph has been entirely a report on Whewell.]

I would give a somewhat different account of this progress of the scientific mind. After a general law of nature has been ascertained, men's minds don't right away become easily able to think of natural phenomena strictly in terms of it. The habit that constitutes the scientific cast of mind—

> the habit of conceiving facts of all kinds in ways that square with the laws that regulate them, conceiving phenomena of all kinds according to the relations that have been found really to exist between them

—this habit, in the case of newly-discovered relations, comes gradually; and until it is thoroughly formed, the new truth isn't regarded as necessary. But eventually the philosopher [here = 'thoughtful scientist'] achieves a state of mind in which his mental picture of nature spontaneously represents to him all the phenomena that the new theory deals with in exactly the light in which the theory regards them; all images or conceptions derived from any other theory, or from the confused view he had before he had any theory, entirely disappear from his mind. The way of representing facts that results from the theory has now become the only way of conceiving them that he finds natural. A prolonged habit of arranging phenomena in certain groups, and explaining them by means of certain principles, makes any other arrangement or explanation of these facts feel unnatural: and he may eventually find it as difficult to represent the facts to himself in •any other way as it used to be to represent them in •that way.

. . . .A contradiction is always inconceivable; so our scientist's imagination rejects false theories and says it can't conceive them. But their inconceivability to him doesn't result from anything in the theories themselves, any inherent conflict with the human mind; it results from the conflict between them and some of the facts; and the scientist found the false theory conceivable as long as he didn't know those facts. . . . So his real reason for rejecting theories at variance with the true one is just that they clash with his experience, but he easily slides into believing that he •rejects them because they are inconceivable, •adopts the true theory because it is self-evident, and •has no need for it to be made evident by experience.

I think this is the real explanation of the paradoxical truth stressed by Whewell, that having a scientifically cultivated mind makes one unable to conceive suppositions that a common man conceives with no difficulty. There's nothing inconceivable in the suppositions themselves. . . . In the case of many of Whewell's 'necessary truths' the negative of the axiom is as easily conceivable as the affirmative, and will probably be so as long as the human race lasts. Consider the axiom that **matter is indestructible**, which is as high as anything on Whewell's list of propositions that are necessary and self-evident. I quite agree that this is a true law of nature, but I don't think that *anyone* has any difficulty in ·conceiving or· imagining a portion of matter being annihilated. It wouldn't have to look different from events we see all the time—water drying up, fuel being consumed. And the law that **bodies combine chemically in definite proportions** is undeniably true; but few people have reached the point that Whewell seems personally to have arrived at, of being unable to conceive a world in which the elements combine with one another 'indifferently in any quantity'. Whewell dares to prophesy similar success to the multitude only after the lapse of generations; but it's not likely that we'll ever rise to this sublime height of inability, so long as all the mechanical mixtures in our planet—whether solid, liquid, or gaseous—exhibit to our daily observation the very phenomenon declared to be inconceivable.

Whewell says that these and similar laws of nature can't be drawn from experience because they are assumed in the interpretation of experience. Our inability to 'add to or diminish the quantity of matter in the world' is a truth that 'neither is nor can be derived from experience; for the experiments we make to verify it presuppose its truth. . . When men began to use the balance in chemical analysis, they. . . .but took for granted as self-evident that the weight of the whole must be the sum of the weights of the elements.' True, it is assumed; but only in the way that all experimental inquiry provisionally assumes some theory or hypothesis, which is to be finally accepted or not according as the experiments decide. The hypothesis. . . .that the material of the world, as estimated by weight, is neither increased nor diminished by any of the processes of nature or art, had many appearances in its favour to begin with. . . . There were other facts that appeared to conflict with it, so experiments were devised to verify it. Men assumed its truth hypothetically, and proceeded to try whether the phenomena that apparently pointed to a different conclusion would on further investigation be found to be consistent with it. This turned out to be the case; and from then on the doctrine took its place as a universal truth—proved to be such by experience. That the theory itself preceded the proof of its truth—that it had to be conceived before it could be proved—doesn't imply that it was self-evident and didn't need proof. Otherwise all the true theories in the sciences are necessary and self-evident; for no-one knows better than Whewell that they all began by being assumed, for the purpose of connecting them by deductions with the facts of experience that now count as evidence in their favour.

·Long footnote appended to chapter 5·

The *Quarterly Review* for June 1841 contained a very able article on Whewell's two great works, an article that maintains on the subject of axioms the doctrine I have been defending here—that axioms are generalisations from experience—and supports that opinion by a line of argument strikingly like mine. Nearly all the present chapter was written before I had seen the article, but in saying this I'm not claiming originality but merely calling your attention to the fact that two inquirers have, entirely independently of one another, arrived at an opinion that is opposed to reigning doctrines. I'm glad to have this opportunity to quote passages that are remarkably in unison with my own views—passages written by someone whose extensive physical and metaphysical knowledge, and capacity for systematic thought, are shown by the article. [The writer was Sir John Herschel.]

'The truths of geometry are summed up and embodied in its definitions and axioms. . . Let us turn to the axioms, and what do we find? A string of propositions concerning magnitude in the abstract, which are equally true of space, time, force, number, and every other magnitude that can be added and divided. Such propositions, apart from those that are not mere definitions, carry their inductive origin on the surfaces of the sentences expressing them. . . The only ones that express characteristic properties of space are "Two straight lines can't enclose a space" and "Two straight lines that intersect can't both be parallel to a third". Let us have a closer look at these. The only clear notion we can form of •straightness is •uniformity of direction, for space in the final analysis is nothing but an assemblage of distances and directions. And. . . .we can't even make the proposition intelligible to anyone whose experience ever since he was born hasn't assured him of the fact. The unity of direction—i.e. that we can't march *straight* from x to y by more than one route is matter of practical experience long before it could possibly be matter of abstract thought. *We can't attempt to imagine a situation in which it would be false, without •violating our habitual recollection of this experience and •defacing the mental picture of space that we have based on it.* What other than experience could possibly assure us of the homogeneity of the parts of distance, time, force, and measurable aggregates in general, on which the truth of the other axioms depends?. . . .'

Concerning axioms of mechanics: '. . . .Let us take one of these axioms and ask what makes it evidently true: for instance, that *equal forces perpendicularly applied at the* *opposite ends of equal arms of a straight lever will balance each other*. What other than experience can possibly inform us that a force so applied will have any tendency to turn the lever on its centre at all? or that force can be transmitted along a rigid line perpendicular to its direction in such a way as to act at a place that isn't along its own line of action? This is so far from being •self-evident that it even seems •paradoxical until we bring in the lever's thickness, material composition, and molecular powers. Again, we conclude that the two forces, being equal and applied under precisely similar circumstances, must if they exert any effort at all to turn the lever *exert equal and opposite efforts*; but what *a priori* reasoning can possibly assure us that they *do* act under precisely similar circumstances? that their being in different places doesn't affect the forces that they exert? [The argument continues, in the spirit of Mill's discussion. Then a further axiom:] The other fundamental axiom of statics, that *the pressure on the point of support is the sum of the weights*. . . is merely a scientifically more refined statement of a coarse and obvious result of universal experience, namely that the weight of a rigid body is the same, however we handle it or suspend it, and that whatever holds it up holds up its total weight. Whewell rightly says: 'Probably no-one ever did an experiment to show that the pressure on the support is equal to the sum of the weights.'. . . But that's because in every action of someone's life from earliest infancy he is continually doing the experiment and seeing it done by every other living being about him, so that he never dreams of staking its result on one additional attempt made with scientific accuracy. This would be like sealing yourself up for half an hour in a metal case so as to discover whether your eyes are useful for seeing.'

On the 'paradox of universal propositions obtained by experience' the writer says: 'If there are necessary and universal truths expressible in propositions of axiomatic simplicity and obviousness, and having for their subject-matter the elements of all our experience and all our knowledge, surely *these* are the truths that experience ought to suggest most readily, clearly, and unceasingly. If it were a universal and necessary truth that a net is spread over the whole surface of every planetary globe, we wouldn't travel far on our own without getting entangled in its meshes, and making the necessity of some means of extrication [i.e. our need for some such means] an axiom of locomotion! So there is nothing paradoxical—quite the reverse—in our being led by our senses to a recognition of such truths as general propositions that are at least true of all human experience. That •they pervade all the objects of experience ensures their continually being suggested by experience; that •they are true ensures the consistency of suggestion. . . that commands complete assent; that •they are simple and can't be misunderstood secures their admission by every mind.'

'A necessary and universal truth about any object of our knowledge must verify itself in every state of affairs where we are thinking about that object, and if at the same time it is simple and intelligible, its verification must be obvious. Thus the sentiment of such a truth can't *not* be present to our minds whenever that object is contemplated, and must therefore be part of the mental picture or idea of that object that we may sometimes bring before our imagination. . . That's why all propositions become not only untrue but inconceivable if. . . axioms are violated in the statement of them. . . .'

·END OF LONG FOOTNOTE·

Chapter 6: Demonstration and necessary truths (cont'd)

§1. The discussion in chapter 5 of the deductive sciences that are commonly said to be systems of necessary truth has led to the following conclusions. The results of those sciences are indeed 'necessary' in the sense of 'necessarily following from "axioms" and definitions', i.e. of being certainly true if those axioms and definitions are true. (Even in this sense, you see, 'necessity' simply means 'certainty'.) But if any scientific result R is to count as *necessary* in any sense beyond this—any sense implying that R is evidently true in a way that doesn't depend on observation and experience—we must first establish that the definitions and axioms from which R is inferred are themselves necessary in that sense. ·And we have seen that this can't be established because it isn't true·. I have shown that **axioms** considered as experimental [= 'empirical'?] truths rest on superabundant and obvious ·empirical· evidence. I then asked:

> Are we then compelled to suppose •that such 'axioms' are evident because of something other than experimental evidence, •that our acceptance of them has a non-empirical basis?

I argued that if anyone answers Yes, the burden of proof lies with him; and I thoroughly examined the arguments they have produced. These all failed the test, which I took as a justification for concluding that axioms are merely one class, the most universal class, of inductions from experience—the simplest and easiest cases of generalisation from the facts delivered by our senses or by our internal consciousness.

In contrast with that, the improperly so-called **'definitions'** in demonstrative sciences turned out to be generalisations from experience that aren't even truths, strictly speaking. Why? Because

> They are propositions in which we •assert of some kind of object that it has some property or properties that observation shows to belong to it, but also •deny that it has any other properties, though in each individual instance the thing *does* have other properties, nearly always ones that modify [see Glossary] the one that is asserted in the definition.

The point is that the denial is a mere fiction—a supposition—made for the purpose of •excluding the consideration of those details when their influence is of too trifling amount to be worth considering, or (if it's not trivial) postponing it to a more convenient moment. [The word 'details' replaces Mill's 'modifying circumstances'—see 'modify' in the indented passage.]

From all this it seems that deductive or demonstrative sciences are *all* inductive sciences; that what makes them evident is the evidentness of experience; but they are also *hypothetical* sciences because of the special character of one indispensable ingredient in the general formulae according to which their inductions are made. Their conclusions are true only on certain suppositions, which are or ought to be approximations to the truth, but are seldom if ever exactly true. And this hypothetical character is the real source of the special certainty that is supposed ·by some theorists· to be inherent in demonstration!

The position I have been defending, however, has no chance of being accepted as true of *all* deductive or demonstrative sciences until it has been checked against the most remarkable of all those sciences, that of *numbers*—i.e. the theory of •the calculus, •arithmetic and •algebra. It's harder to believe of the doctrines of this science than of any other •that they are not known *a priori* but are experimental truths, or •that their special certainty comes from their not being absolute but only conditional truths. [Just to make sure that the last clause is understood: however great the chance is that a proposition Q (absolute) is true, there may be a much better chance (and there can't be a worse one) that 'If P then Q' (conditional) is true.] So the science of numbers needs to be examined separately, especially given that on this subject *two* opposing doctrines have to be dealt with: •that of the *a priori* philosophers and •a second one that is the most opposite to theirs, used to be very generally accepted, and is still far from being altogether exploded among metaphysicians.

§2. This ·second· theory ·is called 'nominalism' (from the Latin *nomen* = 'name'); it· represents the propositions of the science of numbers as merely verbal, and its processes as mere substitutions of one expression for another. The proposition 'Two and one is equal to three', according to these writers, is not a truth, is not the assertion of a really existing fact, but a definition of 'three'; a statement that mankind have agreed to use 'three' as a short name for anything that is called by the clumsy phrase 'two and one'. According to this doctrine, even the longest calculation in algebra is just a series of changes in terminology, in which equivalent expressions are substituted one for another—a series of translations of a single fact from one language into another—though the friends of this theory haven't explained how such a series of translations can have as output a different fact, as when we demonstrate a new geometrical theorem by algebra; and this failure is fatal to their theory.

It must be acknowledged that the processes of arithmetic and algebra have some special features that make this theory in question very plausible, and have naturally made those sciences the stronghold of nominalism. The doctrine that

> we can discover facts—detect the hidden processes of nature—by a skillful manipulation of language

is so contrary to common sense that a person must have made some advances in philosophy to believe it! What drives people to this paradoxical belief is their perceived need to avoid some even greater difficulty that the vulgar [see Glossary] don't see. Many people have come to think that reasoning is a merely verbal process because no other theory seemed reconcilable with the nature of the science of numbers. The facts about that science that have impressed them are these:

> When we use the symbols of arithmetic or of algebra, we don't carry any ideas along with us. In a geometrical demonstration we *do* have a diagram, in our minds if not on paper, so that (for example) AB, AC are present to our imagination as lines that intersect and form an angle. But not so with the *a* and *b* of algebra: *they* can represent lines or any other magnitudes, but those magnitudes are never thought of; nothing on show in our imagination but *a* and *b*. The ideas that they happen to represent on the particular occasion are banished from the mind throughout the process between •the start where the premises are translated from things into signs and •the end where the conclusion is translated back from signs into things.

Given that there is nothing in the reasoner's mind but the symbols, how can the reasoning process be concerned with anything other than the symbols?. . . .

But when we think about it we'll see that •this apparently decisive instance is really not an instance at all; that •in every step of an arithmetical or algebraic calculation there's a real induction, a real inference of facts from facts; and that •what disguises the induction is simply its comprehensive nature and thus the extreme generality of the language it uses. All numbers must be numbers of **something**: there are no such things as numbers in the abstract. *Ten* must mean ten bodies, or ten sounds, or ten beats of the pulse. But they can be numbers of **anything**. So propositions about numbers have the remarkable special feature that they are propositions about everything—all objects, all existents of every kind that we have encountered. All things have quantity, consist of parts that can be numbered, so they have all the properties that are called properties of numbers. That half of four is two must be true whatever 'four' represents, whether four hours, four miles, or four pounds weight. We need only to conceive a thing as divided into four equal parts (and everything can be conceived as so divided) to be able to predicate of it every property of the number four, i.e. every arithmetical proposition in which the number four stands on one side of the equation. Algebra extends the generalisation still further: every number represents *that particular number* of all things without distinction, but every algebraic symbol represents *all numbers* without distinction. As soon as we conceive a thing divided into equal parts, without knowing how many parts, we can call it a or x, and apply to it, with no risk of error, every algebraic formula in the books. The proposition $2(a + b) = 2a + 2b$ is a truth throughout all nature. And because algebraic truths are true of all things whatever. . . ., it's no wonder that the symbols don't arouse in our minds ideas of anything in particular. . . . We don't need under the symbol a to picture to ourselves all things whatever, but only some one thing, ·*any* one thing·; so why not the letter itself? The mere written characters, a, b, x, y, z represent *things in general* as well as any more complex and apparently more concrete conception. What shows that we are conscious of them as •things and not as mere •signs is the fact that throughout our reasoning we predicate of them the properties of •things. In solving an algebraic equation, by what rules do we proceed? By applying at each step to a, b, and x the propositions

•that equals added to equals make equals;and
•that equals taken from equals leave equals;

and other propositions based on those two. These aren't properties of language, or of signs as such, but *of magnitudes*, which amounts to saying *of all things*. So the successive inferences concern things, not symbols. And although any things whatever will serve our purpose, there's no need for us to keep the idea of the thing at all distinct; and because of that, there's no risk in allowing our process of thought to become entirely mechanical, which is what thought-processes do (if permitted) when they are performed often.

Thus the general language of algebra comes to be used familiarly without arousing ideas. All general language is apt to do this from mere habit, though algebra is the only context where it can be done with complete safety. But when we look back to see what gave the process its force as a proof, we find that every single step brings us along only if we suppose ourselves to be thinking and talking about the things and not the mere symbols.

The notion that the propositions of arithmetic and algebra are merely verbal gets even more plausibility from something else. It's the fact that when propositions are considered as being about things, they all look like identical propositions [see Glossary]. Consider 'Two and one is equal to three', considered as an assertion about objects—e.g. 'Two pebbles and one pebble are equal to three pebbles'. This doesn't affirm •equality between two collections of pebbles but •absolute identity. It affirms that if we add one pebble to two pebbles *those very pebbles* are three. We have the same objects throughout, and the mere assertion that 'objects are themselves' is empty, so it seems only natural to regard 'Two and one is equal to three' as merely asserting that the two names have the same signification.

Plausible as this looks, it won't bear examination. The expression 'two pebbles and one pebble' and the expression 'three pebbles' do indeed stand for the same collection of objects, but they don't stand for the same physical fact. They're names of the same objects, but of those objects in two different states: though they •denote the same things, their •meaning is different. Three pebbles in two separate parcels don't make the same impression on our senses as three pebbles in one parcel; and the assertion that the very same pebbles can be arranged so as to produce either of those sets of sensations, though a very familiar proposition, is not an identical one. It is a truth known to us by early and constant experience, an inductive truth; and such truths are the foundation of the science of number. The fundamental truths of that science all rest on the evidence of the senses; they are proved by showing to our eyes and fingers that any given number of objects—ten balls, say—can by separation and re-arrangement exhibit to our senses all the different sets of numbers the sum of which is equal to ten. All the improved methods of teaching arithmetic to children are based on a knowledge of this fact. All who wish to carry the child's mind along with them in learning arithmetic—all who wish to teach numbers and not mere ciphers—now teach it through the evidence of the senses in the way I have described.

We *can* call the proposition 'Three is two and one' a definition of the number three, and describe arithmetic (as geometry has been described) as a science based on definitions. But they're 'definitions' in the geometrical sense, not the logical; i.e. they assert not only •the meaning of a term but also •an observed matter of fact. The proposition 'A circle is a figure bounded by a line which has all its points equally distant from a point within it' is called the definition of *circle*; but the proposition from which so many consequences follow—the proposition that really *is* a first principle in geometry—is that figures answering to this description exist. Similarly, we may call 'Three is two and one' a definition of *three*; but the calculations that depend on that proposition follow not from the definition itself but from an arithmetical theorem presupposed in it, namely that there are collections of objects which while they impress the senses thus:

•••

can be separated into two parts thus:

•   • •

This proposition being granted, we call all such parcels 'threes'; and then the statement of the above-mentioned physical fact will serve also as a definition of 'three'.

The science of number is thus no exception to the conclusion I have argued for, that the processes even of deductive sciences are entirely inductive, and that their first principles are generalisations from experience. One last question remains. You'll remember this finding about geometry:

> Some of its inductions are not exactly true; the special certainty ascribed to it, leading men to call its propositions 'necessary truths', is fictitious and hypothetical, being true only in the sense that those propositions validly follow from the *hypothesis* of the truth of premises, which are admittedly mere approximations to truth.

Is that also true of the propositions of the science of number?

§3. The inductions of arithmetic are of two sorts: **(i)** the likes of 'One and one are two', 'Two and one are three' etc., which can be called the definitions of the various numbers in the improper or geometrical sense of 'definition'; and

(ii) the two axioms 'The sums of equals are equal', 'The differences of equals are equal'. These two are enough, for the corresponding propositions about unequals can be proved from these by a *reductio ad absurdum*.

These axioms and the so-called definitions are (I repeat) results of induction; true of all objects whatever, and they may seem to

> be exactly true, just as they stand

rather than merely

> having the status of exact truths *conditionally*, i.e. on the condition that a certain assumption is true.

So it is natural to infer that the conclusions of the science of number are exactly true, making it unlike the other demonstrative sciences in having results that are *categorically* certain, not merely *hypothetically* so.

When we look more closely, though, we find that even here there is one hypothetical element in the ratiocination. In all propositions about numbers a condition is implied, without which none of them would be true; and that condition is an assumption that may be false. The condition is that **1 = 1**, i.e. that all the numbers are numbers of the same or of equal units. [That clause is verbatim from Mill.] If this is doubtful, not one of the propositions of arithmetic will hold true. How can we know that *one pound and one pound make two pounds* if one of the pounds may be troy and the other avoirdupois?. . . . How can we know that a forty-horse power is always equal to itself, unless we assume that all horses are of equal strength? It's certain that 1 is always equal *in number* to 1; and where all that matters is the mere number of objects or object-parts, the conclusions of arithmetic are true without mixture of hypothesis. There are such cases in statistics, e.g. in an inquiry into the size of the population of any country. In that inquiry we don't care whether they are adults or children, strong or weak, tall or short; all we want to ascertain is their number. But whenever from (in)equality of number we infer (in)equality in any •other respect, the arithmetic we bring to such inquiries becomes as hypothetical a science as geometry. All units must be assumed to be equal in that •other respect; and this is never precisely true because one actual pound weight is not exactly equal to another, nor one measured mile's length to another; a more exact balance or more accurate measuring instruments would always detect some difference.

What is commonly called 'mathematical certainty', therefore, which implies •unconditional truth and •perfect accuracy, is an attribute not of all mathematical truths but only of those that relate to pure *number* as distinguished from *quantity* in the more enlarged sense; and only if we abstain from supposing that the numbers are a precise pointer to actual quantities. . . .

§4. Thus, we find that the method of all deductive sciences is hypothetical. They trace the consequences of certain assumptions, leaving the questions

> •Are the assumptions true? and
> •If not, are they near enough to true?

to be answered later. The reason ·for proceeding in this way· is obvious. Setting aside the special case of propositions that are purely about number and not applied to anything else, we can see that in every other case of deductive investigation we need to determine how far short of *exactly true* the relevant assumptions are. This is generally a matter of observation, to be repeated in every new case; and if it has to be settled by argument rather than observation, those arguments may vary in length, complexity, and other factors from case to case. But the other part of the process—namely, determining what we can conclude if (and to the extent that) we find the assumptions to be true—can be done once for all, and the results held ready to be used when needed. We are doing beforehand everything that *can* be done beforehand, so as to minimize the work that has to be done in particular cases that press us for a decision. **This inquiry into the inferences that can be drawn from assumptions is what is properly called demonstrative science.**

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

§5. Some writers have held that *all* ratiocination ultimately comes down to *reductio ad absurdum*; because we can enforce assent to a conclusion P. . . .by showing that if the P is denied we must deny at least one of the premises, and as they are all supposed to be true, that would be a contradiction. In line with this, many people have thought that the special nature of the evidentness of ratiocination consists in the impossibility of admitting the premises and rejecting the conclusion without a contradiction in terms. This theory, however, can't explain the grounds on which ratiocination itself rests. If someone denies the conclusion despite his admission of the premises, he isn't involved in any direct and explicit contradiction until he is compelled to deny some premise; and he can only be forced to do this by a *reductio ad absurdum*, i.e. by another ratiocination. But if he denies the validity of the reasoning process itself, he can't be forced to assent to the second syllogism any more than he can to the first. Thus, no-one is ever forced to a contradiction in terms: he can only be forced to an infringement of the fundamental maxim of ratiocination, namely that whatever has a mark also has what it is a mark of. . . .

That's as far as I can go just now in the theory of deduction. Further insight into the subject requires that we lay the foundation of the philosophic theory of induction itself; when we do that, the theory of deduction will automatically fall into place because deduction is, as I have shown, a kind of induction. In that context, deduction will receive its share of whatever light may be thrown on the great intellectual operation of which it forms such an important part.

Chapter 7: Examining some of the opposition to the preceding doctrines

§1. An opinion that stands in need of much illustration can often receive it most effectively and least tediously in the form of a defence against objections. And someone who advances a doctrine on a subject concerning which theorists are still divided has a duty to examine, and to the best of his ability to judge, the opinions of other thinkers. [The first four sections of this chapter are addressed to Herbert Spencer; the fifth to Sir William Hamilton.]

Mr. Herbert Spencer has criticised some of the doctrines of chapters 5 and 6, and propounded a theory of his own on the subject of first principles. He agrees with me in considering axioms to be 'simply our earliest inductions from experience'. But he differs 'widely' from me 'concerning the worth of the test of inconceivability'. He thinks that it is the ultimate test of all beliefs, a conclusion that he reaches by two steps. **(i)** We never can have any stronger ground for believing anything than that the belief of it 'invariably exists'. Whenever any fact or proposition is invariably believed—which I think Spencer means 'believed by everyone (oneself included) at all times'—it is entitled to be accepted as one of the primitive truths or original premises of our knowledge. **(ii)** The criterion by which we decide whether something is invariably believed to be true is our inability to conceive it as false. 'The inconceivability of its negation is the test by which we ascertain whether a given belief invariably exists or not.' 'The only reason we can give for our primary beliefs is the fact of their invariable existence, tested by our trying and failing *not* to have them.' He thinks that this our only reason for believing in our own sensations. If I believe that I feel cold, I accept this as true only because I can't conceive that I am not feeling cold. 'While the proposition remains true, the negation of it remains inconceivable.' There are numerous other beliefs that Spencer thinks rest on the same basis; most of them being propositions that the metaphysicians of the Reid and Stewart school regard as truths of immediate intuition—

- There exists a material world;
- This is the very world that we directly and immediately perceive, and not merely the hidden cause of our perceptions;
- Space, time, force, extension, figure are not modes of our consciousness, but objective realities;

—these are regarded by Spencer as truths known by the inconceivability of their negations. He holds that we can't by any effort conceive these objects of thought as mere states of our mind—as not existing external to us. So their real existence is as certain as our sensations themselves. According to this doctrine, truths of which we have direct knowledge are known to be truths only by the inconceivability of their negations; and truths of which we don't have direct knowledge are known to be truths only because they are derived from truths of the first sort; and those derivations are believed to be valid only because we can't conceive them not to be. So inconceivability is the ultimate ground of all assured beliefs.

Up to here, Spencer's doctrine doesn't differ much from the ordinary view of philosophers of the intuitive school, from Descartes to Whewell; but at this point he parts company with them. For he doesn't follow them in setting up the test of inconceivability as infallible. On the contrary, he holds that the test may be fallacious, not from any fault in the test itself but because 'men have thought to be inconceivable some things that were not inconceivable'. And he himself denies plenty of propositions that are usually regarded as among the most striking examples of truths whose negations are inconceivable. But all tests, he says, occasionally fail; if such failure undercuts 'the test of inconceivability', it 'must similarly undercut all tests whatever'. He continues:

> 'We consider a conclusion logically inferred from established premises to be true. Yet in millions of cases men have been wrong in the inferences they have thought were logical. Should we infer from this fact that it's absurd to consider a conclusion as true simply because it is logically drawn from established premises? No: we should say this:
>
> > Although men may have regarded as logical some inferences that were not logical, there *are* logical inferences, and until we are better instructed we are justified in assuming the truth of what seem to us to be such.
>
> Similarly, although men may have regarded as inconceivable some things that were not so, there may still *be* inconceivable things; and our inability to conceive the negation of P may still be our best warrant for believing that P. It may sometimes turn out to be an imperfect test; but it's the best test we have for our most certain beliefs; so doubting a belief because we have no higher guarantee for it is really doubting all beliefs.'

So Spencer's doctrine doesn't erect the curable limitations of the human conceptive faculty into laws of the outward universe; only the incurable limitations.

§2. Spencer has two arguments to support his doctrine that 'a belief that is proved by the inconceivability of its negation to invariably exist is true'. One is positive, the other negative.

The **positive argument** says that every such belief represents the aggregate of all past experience. Spencer writes:

> 'Conceding the entire truth of the view that •during any phase of human progress what men can specifically conceive depends entirely on the experiences they have had; and that •by widening their experiences they may eventually become able to conceive things that used to be inconceivable to them, it can still be argued that
>
> > because *at any time* the best warrant men can have for a belief is the perfect agreement of all their previous experience in support of it, it follows that *at any time* the inconceivability of its negation is the deepest test any belief is capable of. . .
>
> Objective facts are always impressing themselves upon us; our experience is a record of these objective facts; and something's being inconceivable implies that it is wholly at variance with the record. Even if this were the whole story, it's not clear how, if every truth is primarily inductive, any better test of truth could exist. But it must be remembered that while many of these facts that impress themselves upon us are •occasional, and others are merely •very general, some are •universal and unchanging. These universal and unchanging facts are, by the hypothesis, certain to establish beliefs the negations of which are inconceivable; while the others are not certain to do this; and if they do, subsequent facts will reverse their action. So if after an immense accumulation of experiences there remain beliefs the negations of which are still inconceivable, most and perhaps all of them must correspond to universal objective facts. If

- •there are. . . absolute uniformities in nature, if
- •these uniformities produce (as they must) absolute uniformities in our experience, and if. . .
- •these absolute uniformities in our experience make us unable to conceive their negations, then
- •corresponding to each absolute uniformity in nature that we can know, there must exist in us a belief the negation of which is inconceivable, and which is absolutely true.

In this wide range of cases subjective inconceivability must correspond to objective impossibility. Further experience will produce correspondence where it may not yet exist; and we may expect the correspondence to become ultimately complete. In nearly all cases this test of inconceivability must be valid now'

—I wish I could think we were so near to omniscience!—

•'and where it isn't, it still expresses the net result of our experience up to the present time, which is the most that any test can do.'

To this I have two answers. **(1)** It is by no means true that the inconceivability by us of the negative of a proposition proves that any—let alone *all*—'previous experience' has been in favour of the affirmative. There may have been no such previous existing experiences but only a mistaken supposition of them. How did the inconceivability of antipodes prove that experience had given any testimony against their possibility? How did the incapacity men felt of conceiving sunset as anything but a motion of the sun, represent any 'net result' of experience in support of its being the sun and not the earth that moves? What is represented is not •experience but only •a superficial semblance of experience. All that is proved with regard to real experience is the negative fact that men have not had experience that would have made the inconceivable proposition conceivable.

(2) Even if it were true that inconceivability represents [Spencer's word was 'expresses'] the net result of all past experience, why should we settle for the representative when we can get at the thing represented? If our inability to conceive the negation of P is proof of P's truth, because it proves that our experience so far has been uniformly in its favour, the real evidence for P is not •the inconceivability but •the uniformity of experience. If all past experience is in favour of P, let this be stated and the belief openly based on it ·without an irrelevant detour through the inconceivability of not-P·. And then we can consider what that fact ·about experience· is worth as evidence of P's truth. In some cases uniformity of experience is strong evidence, in some it is weak, and in others again it scarcely amounts to evidence at all. That *all metals sink in water* was a uniform experience, from the origin of the human race to the discovery of potassium in the present century by Sir Humphry Davy. That *all swans are white* was a uniform experience down to the discovery of ·black swans in· Australia. In the few cases where uniformity of experience does amount to the strongest possible proof, as with propositions such as these,

- •Two straight lines can't enclose a space,
- •Every event has a cause,

it's not because their negations are inconceivable, which is not always the fact, but because the experience that has been uniform in their favour pervades all nature. I'll shown in Book III that none of the conclusions either of induction or of deduction can be considered certain except as far as their truth is shown to be inseparably bound up with truths of this class.

I maintain then **(2)** that uniformity of past experience is •far from being a universally sound criterion of truth, and

(1) that inconceivability is even •further from being a test of that test. Uniformity of contrary experience is only one of many causes of inconceivability. One of the commonest is tradition handed down from a period of more limited knowledge. The mere familiarity of one way of producing a phenomenon is often enough to make every other way seem inconceivable. Whatever connects two ideas by a strong association may, and continually does, make it impossible to separate them in thought; as Spencer frequently recognises in other parts of his work. Why were the Cartesians unable to conceive that one body could produce motion in another without contact? It wasn't their lack of relevant experience. They had as much experience of that way of producing motion as they had of other ways. The planets had revolved, and heavy bodies had fallen, every hour of their lives. But they fancied these phenomena to be produced by a hidden machinery that they •didn't see, because without it they couldn't conceive what they •did see. The inconceivability, instead of representing their experience, dominated and overrode their experience. I now turn to Spencer's negative argument, on which he lays more stress.

§3. The **negative** argument says: whether the inconceivability of not-P is good evidence or bad evidence for P, no stronger evidence can be obtained. That *what is inconceivable can't be true* is postulated in every act of thought. It is the foundation of all our original premises, and is assumed still more ·strongly· in all conclusions from those premises. The invariability of belief, tested by the inconceivability of its negation, 'is our sole warrant for every demonstration. Logic is simply a systematisation of the process by which we •indirectly obtain this warrant for beliefs that don't •directly possess it. To gain the strongest conviction possible regarding any complex fact, we either •work back through propositions that it comes from, unconsciously testing each by the inconceivability of its negation, until we reach some axiom or truth that we have similarly tested; or we •work forward through propositions that are implied by it, testing each in the same way. In either case we connect some isolated belief with a belief that invariably exists, by a series of intermediate beliefs that invariably exist.' This sums up the theory:

> 'When we perceive that the negation of a belief is inconceivable, we have all possible warrant for asserting the invariability of its existence; and in asserting this we express both •our logical justification for it and •the inexorable necessity we are under of holding it. . . We have seen that this is the assumption on which every conclusion whatever ultimately rests. We have no other guarantee
>
> - •for the reality of consciousness, of sensations, of personal existence;
> - •for any axiom;
> - •for any step in a demonstration.
>
> Hence, as being taken for granted in every act of the understanding, it must be regarded as the universal postulate.'

But this postulate that we are under an 'inexorable necessity' of holding true is sometimes false; 'beliefs that once were shown by the inconceivability of their negations to exist invariably have since been found untrue'; and 'beliefs that now have this character may some day share the same fate'; ·and Spencer knows all this, so· the canon of belief he lays down is that 'the most certain conclusion' is the one that 'involves the postulate the fewest times'. So reasoning ought never to prevail against one of the immediate beliefs (the belief in matter, in the outward reality of extension, space, and the like), because each of these involves the postulate only once; while a bit of reasoning involves the postulate in the premises and involves it again at every step of the ratiocination. Why? Because each step in the argument is recognised as valid only because we can't conceive the conclusion not to follow from the premises.

It will be convenient to take the last part of this argument first. In every reasoning, according to Spencer, the assumption of the postulate is renewed at every step. At each inference we judge that the conclusion follows from the premises, our sole warrant for that judgment being that we can't conceive it not to follow. Consequently if the postulate is fallible, the conclusions of reasoning are harmed by that uncertainty more than direct intuitions are; and the more steps the argument has the greater the disproportion.

To test this doctrine, let us start with an argument consisting only of a single step, which would be represented by one syllogism. This argument does rest on an assumption, and we have seen what it is, namely that *whatever has a mark has what it is a mark of.* I shan't discuss here the basis for this axiom; let us suppose (with Spencer) that it rests on the inconceivability of its reverse.

Let us now add a second step to the argument: we require. . . what? Another assumption? No—the same assumption a second time; and so on to a third, and a fourth. I don't see how, on Spencer's own principles, the repetition of the assumption weakens the argument's force. If the second step required us to assume some other axiom, the argument *would* be weakened, because it would now run two risks of falsity instead of only one. But in fact only one axiom is required, and if it is true once it is true every time; if a 100-step argument assumed the axiom a hundred times, these hundred assumptions would create only one chance of error altogether. On Spencer's theory the deductions of pure mathematics are among the most uncertain of argumentative processes, because they are the longest. But the number of steps in an argument does not subtract from its reliableness, if no new premises of an uncertain character are taken up along the way. [Mill here has a long footnote stating and replying to two arguments that Spencer presented against what Mill has been saying in this section. The first argument is flatly wrong, while the second is at best marginal. There is nothing much to be learned from this exchange.]

Now for the premises. Spencer holds that our assurance of their truth—whether they are generalities or individual facts—is based on the inconceivability of their being false. Now, the word 'inconceivable' is ambiguous; Spencer is aware of this ambiguity and would sincerely deny that he is founding an argument on it; but it is in fact at work helping him to make his case. 'Inconceivability' sometimes means inability to form or get rid of an •idea; sometimes inability to form or get rid of a •belief. The former meaning is the better one because a 'conception' is always an idea and never a belief. But in philosophical discussion the word is used with its wrong meaning at least as often as with the right one; and the intuitive school of metaphysicians needs both. To see the difference, consider these two contrasted examples. **(a)** The early scientists considered antipodes incredible because 'inconceivable'. But antipodes weren't inconceivable in the original sense of the word. An idea of them could be formed without difficulty: they could be completely pictured to the mental eye. What was difficult—and to them seemed impossible—was to find them believable. They could assemble the idea of men sticking on by their feet to the under side of the earth; but the belief would follow that they must fall off. Antipodes were not unimaginable, but they were unbelievable.

(b) On the other hand, when I try to conceive an end to space, the two ideas refuse to come together. When I try to form a conception of the last point of space, I can't help picturing to myself a vast space beyond that last point. The combination is, under the conditions of our experience, unimaginable. It's important to bear in mind this double meaning of 'inconceivable', because the argument from inconceivability almost always depends on switching between those two meanings.

When Spencer tests the truth of a proposition by asking whether its negation is 'inconceivable', which of the two senses is he giving to that word? I inferred from the course of his argument that he meant 'unbelievable'; but he has recently disclaimed this meaning and declared that by an 'inconceivable' proposition he always means 'a proposition the terms of which can't by any effort be brought before consciousness in the relation that the proposition asserts between them—a proposition the subject and predicate of which offer an insurmountable resistance to union in thought'. So now we know that Spencer always •tries to use 'inconceivable' in its proper sense; but there's evidence that he doesn't always •succeed, and that the other sense of the word—the popular sense—sometimes creeps in with its associations and prevents him from clearly separating the two. For example, when he says that when I feel cold I can't conceive that I'm not feeling cold, he can't mean 'I can't conceive myself not feeling cold', for it's obvious that I can. In this context, therefore, 'conceive' is being used to express the recognition of a matter of fact—the perception of truth or falsehood; and I take this to be about belief as distinguished from simple conception. Again, Spencer calls the attempt to conceive something that is inconceivable 'an abortive effort to cause the non-existence. . . '—not of a conception or mental representation but of a belief. So we need to revise a considerable part of what Spencer writes, if it is to be kept consistent with his definition of 'inconceivability'.

Mill's next sentence: But in truth the point is of little importance, since inconceivability in Spencer's theory is only a test of truth, inasmuch as it is a test of believability.

what he should have said, given what follows: But in fact we *can't* amend what he says so that it consistently uses 'inconceivable' in its proper sense; because in his theory inconceivability is a test of truth only because it is a test of believability; which means that the improper sense of 'inconceivable' has a structural role in his theory.

The inconceivability of P is the extreme case of P's unbelievability. This is the very foundation of Spencer's doctrine. For him the invariableness of the belief is the real guarantee. The attempt to •conceive the negative is made so as to test the inevitableness of the •belief, so it should be called an attempt to •believe the negative. When Spencer says that while looking at the sun a man can't conceive that he is looking into darkness, he should have said that the man can't believe that he is doing so. For it is surely possible in broad daylight to imagine oneself looking into darkness.[1] As Spencer himself says, speaking of the belief in our own existence, 'He can conceive well enough that he might not exist, but that he does not exist he finds it impossible to conceive', i.e. to believe. So his statement comes down to this: 'I believe that I exist and that I have sensations, because I can't believe otherwise.' And in this case everyone will agree that the impossibility is real. Each person inevitably believes in his present sensations or other states of subjective consciousness. They are facts known through themselves ·and not as conclusions inferred from premises·; it is impossible

[1] Spencer distinguishes 'conceiving *myself* looking into darkness' from 'conceiving *that I am* looking into darkness'. This switch from 'myself. . . ' to 'that I am. . . ' marks the transition from conception to belief. The form 'to conceive that P' is not consistent with using 'conceive' in its proper sense.

to ascend beyond them [= 'to find any source or origin for them']. Their negation is really unbelievable, so there's never any question of believing it. Spencer's theory is not needed for these truths.

But according to him there are other beliefs, relating to things other than our own subjective feelings, for which we have the same guarantee—which are similarly invariable and necessary. These other beliefs *can't* be necessary because they don't always exist. There have been and still are many people who don't believe in the reality of an external world, let alone the reality of extension and shape as the forms of that external world; who don't believe that space and time exist independently of the mind—or any other of Spencer's objective intuitions. The negations of these allegedly invariable beliefs are not unbelievable, for they are believed! It isn't obviously wrong to say that we can't *imagine* tangible objects as mere states of our own and other people's consciousness; that the perception of them irresistibly suggests to us the *idea* of something external to ourselves: and I'm not in a position to say that this is not the fact (though I don't think anyone is entitled to affirm it of anyone else). But many thinkers have •believed—whether or not they could •conceive it—that what we represent to ourselves as material objects are mere states of consciousness, complex feelings of touch and of muscular action. Spencer may think the inference from the unimaginable to the unbelievable is correct because he holds that belief is merely the persistence of an idea, so that what we can succeed in imagining we can't at that moment help finding believable. But what does it matter what we find *at the moment* if the moment is in contradiction to the permanent state of our mind? A man who was as an infant frightened by stories of ghosts, though he disbelieves them in later years (and perhaps never believed them), may throughout the rest of his life be disturbed by being in a dark place in circumstances that stimulate his imagination. The idea of ghosts, with all its attendant terrors, is irresistibly called up in his mind by the outward circumstances. Spencer may say that while the man is under the influence of this terror he has a temporary and uncontrollable belief in ghosts. Be it so; but if that is how things stand, what is the truest to say about this man on the whole—that he believes in ghosts, or that he doesn't believe in them? Assuredly that he doesn't believe in them. It's like that with those who disbelieve a material world. Though they can't get rid of the idea; though while looking at a solid object they can't help having the conception of (and therefore, according to Spencer's metaphysics, the momentary belief in) its externality; even at that moment they would sincerely deny holding that belief; and it would be incorrect to call them anything but disbelievers of the doctrine. So the belief is not invariable; and the 'inconceivability' test for whether someone has the belief fails.

For a familiar illustration of the fact that it's perfectly possible to •believe something without finding it •conceivable. . . ., consider an educated person's state of mind regarding sunrise and sunset. Every educated person knows by investigation or believes on the authority of science that it's the earth and not the sun that moves: but there are probably few who habitually conceive this phenomenon as anything but the ascent or descent of the sun. Certainly no-one can do this without working at it for a long time; and it's probably no easier now than in the first generation after Copernicus. Spencer does not say 'In looking at sunrise it's impossible not to conceive that it is the sun that moves, therefore this is what everybody believes, and we have all the evidence for it that we can have for any truth'. Yet this would be an exact parallel to what he says about the belief in matter. [The conceptual feat that Mill here describes as difficult is indeed so. For help in performing the feat, see the famous passage on pp. 30–34 of Paul Churchland's *Scientific Realism and the Plasticity of Mind* (Cambridge U.P., 1979).]

The existence of matter and other noumena [see Glossary], as distinct from the phenomenal world, has long been and still is a matter for debate; and the very general belief in them—though it's not necessary or universal—stands as a psychological phenomenon to be explained, either on the hypothesis of its truth or on some other. The belief isn't a conclusive proof of its own truth. . . .; but it's a fact that challenges antagonists to show how such a general and apparently spontaneous belief can have originated if not from the real existence of the thing believed. And its opponents have never hesitated to accept this challenge.[1] How much success they have in meeting it will probably determine the ultimate verdict of philosophers on the question.

§4. In a recent writing Spencer resumes what he rightly calls the 'friendly controversy that has been long pending between us'; and expresses his regret (which I cordially share) that 'this lengthened exposition of a single point of difference, not accompanied by an exposition of our many points of agreement, inevitably produces an appearance of much more disagreement than actually exists'. I agree with Spencer that the difference between us, if measured by our **conclusions**, is 'superficial rather than substantial'; and I greatly value being, in the field of analytic psychology, in so much agreement with a thinker of his force and depth. But I also agree with him that the difference between his **premises** and mine has 'profound importance, philosophically considered'; and neither of us should walk out on it until the whole case for each side has been fully examined and discussed.

In his latest statement of the universal postulate Spencer has replaced 'beliefs that invariably exist' by 'cognitions [see Glossary] of which the predicates invariably exist along with their subjects'. And he argues like this:

(i) A failed attempt to conceive the negation of a proposition shows that the cognition it expresses is one the predicate of which invariably exists along with its subject; and

(ii) The discovery that the predicate invariably exists along with its subject is the discovery that this cognition is one we are compelled to accept.

Therefore

(iii) A failed attempt to conceive the negation of a proposition P shows that we are compelled to accept P.

I accept both premises of Spencer's syllogism, but in different senses of the middle term (·'the predicate of which invariably exists along with its subject'·. If this is understood in its most obvious meaning, as an existence in actual nature—i.e. in our objective sensation-involving experience—I do of course admit **(ii)** that when we have ascertained this we are compelled to accept the proposition: but then I don't admit **(i)** that the failure of an attempt to conceive the negation proves the predicate to be always co-existent with the subject in **actual nature**. But I think that Spencer intends his middle term in its other sense, in which 'the invariable existence of the predicate along with the subject' means only that the one is inseparable from the other in **our thoughts**; then indeed **(i)** the inability to separate the two ideas proves their inseparable conjunction, here and now, in the mind that has failed in the attempt; but this inseparability in thought does not prove **(ii)** a corresponding inseparability of subject and predicate in fact—or even in the thoughts of other people, or of the same person in a possible future.

[1] I have myself accepted the contest, and fought it out on this battle-ground, in my *Examination of Sir William Hamilton's Philosophy*, chapter 11.

'That some propositions have been wrongly accepted as true, because their negations were thought to be inconceivable when they were not', does not, in Spencer's opinion, 'disprove the validity of the test'. He gives two reasons why. **(a)** *Any* test 'is liable to yield untrue results because of stupidity or carelessness in those who use it.', **(b)** The propositions in question 'were complex propositions, not to be established by a test that is valid only for propositions that have no analysable complexity'. 'A test that is legitimate for a simple proposition whose subject and predicate are directly related is not legitimate for a complex proposition whose subject and predicate are indirectly related through the many simple propositions implied.' 'That things which are equal to the same thing are equal to one another is a fact that can be known by direct comparison of actual or ideal relations. . . But that the square of the hypotenuse of a right-angled triangle equals the sum of the squares of the other two sides can't be known immediately by comparison of two states of consciousness: here the truth can be reached only through a series of simple judgments concerning the (un)likenesses of certain relations.'. . . .

It's only fair to give Spencer's doctrine the benefit of the limitation he claims—namely that it is applicable only to propositions that are assented to on simple inspection, without any need for proof. But this limitation doesn't exclude some of the most conspicuous examples of propositions that are now known to be false or groundless but whose negations were once found to be inconceivable—such as the proposition that in sunrise and sunset it is the sun that moves, that gravitation may exist without an intervening medium, and even the case of antipodes. . . . When consciousness is confronted by one of Spencer's 'complex' propositions, without an accompanying proof, it gives no verdict at all: it doesn't find it inconceivable •that the square of the hypotenuse equals the sum of the squares of the other two sides, or •that it doesn't equal that sum. But in the three cases that I have just cited, the inconceivability seems to be found directly; no train of argument was needed to obtain the verdict of consciousness on the point. . . . They are cases where one of two opposite predicates seemed immediately ·and intuitively· to be incompatible with the subject, and the other therefore to be proved always to exist with it.[1]

As now limited by Spencer, the ultimate cognitions fit to undergo his test are only ones that are so universal and elementary that they are represented in the earliest and most unvarying experience (or apparent experience) of all mankind. If in such a case the negation really is inconceivable, that is explained by the experience. And I have asked: Why should the truth be tested by inconceivability, when we can go further back for proof—namely to the experience itself? Spencer replies that the experiences can't be all recalled to mind, and if they could be recalled there would be too

[1] In one of the cases Spencer surprisingly thinks that the belief of mankind 'cannot rightly be said to have undergone' the change I allege. Spencer himself still thinks we can't conceive gravitation acting through empty space. 'If an astronomer avowed that he could conceive gravitative force as exercised through space absolutely void, my private opinion would be that he mistook the nature of conception. Conception implies representation. Here the elements of the representation are the two bodies and an agency by which either affects the other. To conceive this agency is to represent it in some terms derived from our experience—i.e. from our sensations. As this agency gives us no sensations, we are obliged (if we try to conceive it) to use symbols idealized from our sensations—imponderable units forming a medium.' [That sentence is verbatim from Spencer.]

If Spencer means that the action of gravitation gives us no sensations, that's one of the most startling things I have ever heard from a philosopher. We have the sensation of one body moving toward another—What more do we need? The 'elements of the representation' are not two bodies and an 'agency' but two bodies and an effect, namely the fact of their approaching one another. . . .

many of them for us to manage. He seems to understand 'test a proposition by experience' as meaning that 'before accepting as certain the proposition that any rectilineal figure must have as many angles as it has sides' I have 'to think of every triangle, square, pentagon, hexagon etc. that I have ever seen, and to verify the asserted relation in each case.' I can only say, with surprise, that I don't take this to be the meaning of 'appeal to experience'. It is enough to know that one has been seeing the fact all one's life without ever noticing any instance to the contrary, and that other people. . . .unanimously declare the same thing. . . . These remarks don't lose their force even if we believe as Spencer does that mental tendencies originally derived from experience impress themselves permanently on the cerebral structure and are transmitted by inheritance, so that modes of thinking that are acquired by the race become *innate* and *a priori* in the individual, representing the experience of his ancestors in addition to his own. All that would follow from this is that a conviction might be really innate—i.e. prior to individual experience—and yet not be true, because the inherited tendency to accept it may have originally been caused by something other than its truth. . . .

§5. Sir William Hamilton holds, as I do, that inconceivability is no criterion of impossibility. 'There is no ground for inferring a certain fact to be impossible merely from our inability to conceive its possibility.' 'There are things that may—indeed *must*—be true though the understanding is wholly unable to construe to itself the possibility.' But Hamilton is a firm believer in the *a priori* character of many axioms and of the sciences deduced from them; and he's so far from basing those axioms on experience that he declares some of them to be true even of noumena—of 'the unconditioned'—which he says our faculties can't give us any knowledge of. He credits two axioms with this exceptional emancipation from the limits that confine all our other possibilities of knowledge—two chinks through which a ray of light finds its way to us from behind the curtain that veils from us the mysterious world of 'things in themselves'. He follows the scholastics in naming them:

- •the principle of contradiction, which says that two contradictory propositions can't both be true; and
- •the principle of excluded middle, which says that two contradictory propositions can't both be false.

Armed with these logical weapons, we can boldly face 'things in themselves' and confront them with a choice, knowing for sure that they absolutely *must* choose one side or the other, though we may be never be able to discover which. To take his favourite example, we can't conceive •the infinite divisibility of matter, and we can't conceive a minimum, i.e. an end to divisibility; yet one or the other must be true.

As I haven't yet said anything about those the two axioms, this is not a bad place to consider them. The former asserts that an affirmative proposition and the corresponding negative proposition can't both be true; which has generally been held to be intuitively evident. Hamilton and the Germans see it as a statement in words of a law of our thinking faculty. Other equally considerable philosophers deem it to be an identical proposition [see Glossary], an assertion that comes from the meaning of terms, a way of defining 'negation' and 'not'.

I can go one step with the latter group. An affirmative assertion and its negation are not two independent assertions connected with each other only by the relation of as mutually incompatibility. *If the negation is true the affirmative must be false* really is a mere identical proposition; for the negation asserts nothing but the falsity of the other. So we should drop the ambitious phraseology that gives the *principium* *contradictionis* the air of a fundamental antithesis pervading nature, and express it in the simpler form *A proposition can't be false and true at the same time.* But I can't follow the nominalists in taking the further step of declaring this to be a merely verbal proposition. I think it is like other axioms in being one of our first and most familiar generalisations from experience. The ultimate foundation of it I take to be that belief and disbelief are two different mental states, excluding one another. We know this by the simplest observation of our own minds. And if we carry our observation outward, we also find that

•light and darkness,
•sound and silence,
•motion and stillness,
•equality and inequality,
•preceding and following,
•successiveness and simultaneity,

any positive phenomenon whatever and its negation, are distinct phenomena, pointedly contrasted, and the one always absent where the other is present. I consider the principle of contradiction to be a generalisation from all these facts.

The principle of excluded middle (or that one of two contradictories must be true) means that an assertion must be either true or false: either the affirmative is true or otherwise its negation is true in which case the affirmative is false. It's surprising to have this principle described as a so-called necessity of thought, because it isn't even true just as it stands. A proposition must be either true or false *if the predicate can in any intelligible sense be attributed to the subject*; it's because this is always assumed to be the case in treatises on logic that the axiom is always presented as absolutely ·rather than merely hypothetically· true. 'Abracadabra is a second intention' is neither true nor false. Between •true and •false there's a third possibility, •meaningless: and this alternative is fatal to Hamilton's extension of the maxim to noumena. 'Matter must either have a minimum of divisibility or be infinitely divisible'—that's more than we can ever know. **(a)** Matter, in any but the phenomenal sense of the term, may not exist; and it will hardly be said that a nonentity must be either infinitely or finitely divisible! **(a)** And although matter, considered as the hidden cause of our sensations, really does exist, what we call 'divisibility' may be an attribute only of our sensations of sight and touch and not of their unknowable cause. Perhaps it doesn't make sense to predicate divisibility of things in themselves, or therefore of matter in itself; in which case it isn't true that matter in itself must be either infinitely or finitely divisible.

I'm glad to be in complete agreement on this question with Herbert Spencer. I now quote a paragraph from a recent paper of his; the germ of an idea identical with his may be in what I have written here, but in Spencer it is not an undeveloped thought but a philosophical theory.

'When remembering a certain thing as in a certain place, the place and the thing are mentally represented together; while to think of the non-existence of the thing in that place implies a consciousness in which the place is represented but the thing isn't. Similarly, if instead of thinking of an object as colourless we think of its having colour, the change consists in the addition to the concept of an element that was absent from it before—the object can't be thought of first as red and then as not red, without one component of the thought being totally expelled from the mind by another. The law of the excluded middle, then, is simply a generalisation of the universal experience that some mental states are directly destructive of other states. It formulates a certain absolutely constant law, that the appearance of any positive mode of consciousness can't occur without excluding a corresponding negative mode; and that the negative mode can't occur without excluding the corresponding positive mode—the positive/negative antithesis being merely an expression of this experience. Hence it follows that if consciousness is not in one of the two modes it must be in the other.'

I must now close this supplementary chapter, and with it Book II. The theory of induction, in the most comprehensive sense of the term, will be the subject of Book III.

·Mill's proof of the geometrical theorem on page 100·

I'm working with an equilateral triangle whose vertices are A, D, E, with point B on the side AD, and point C on the side AE, such that BC is parallel to DE. We must begin, as in Euclid, by prolonging the equal sides AB and AC to equal distances, and joining the extremities BE and DC.

First formula: The sums of equals are equal.

AD and AE are sums of equals by the supposition. Having that mark of equality, they are concluded by this formula to be equal.

Second formula: When equal straight lines or angles are applied to one another, they coincide.

AC and AB are within this formula by supposition; AD, AE, have been brought within it by the preceding step. The angle at A considered as an angle of the triangle ABE, and the same angle considered as an angle of the triangle ACD, are of course within the formula. All these pairs, therefore, possess the property which, according to the second formula, is a mark that when applied to one another they will coincide. Conceive them, then, applied to one another, by turning over the triangle ABE, and laying it on the triangle ACD in such a manner that AB of the one shall lie upon AC of the other. Then, by the equality of the angles, AE will lie on AD. But AB and AC, AE and AD are equals; therefore they will coincide altogether, and of course at their extremities, D, E, and B, C.

Third formula: Straight lines, having their extremities coincident, coincide.

BE and CD have been brought within this formula by the preceding induction; they will, therefore, coincide.

Fourth formula: Angles, having their sides coincident, coincide.

The third induction having shown that BE and CD coincide, and the second that AB, AC, coincide, the angles ABE and ACD are thereby brought within the fourth formula, and accordingly coincide.

Fifth formula: Things which coincide are equal.

The angles ABE and ACD are brought within this formula by the induction immediately preceding. This train of reasoning being also applicable, mutatis mutandis [see Glossary], to the angles EBC, DCB, these also are brought within the fifth formula. And, finally,

Sixth formula: The differences of equals are equal.

The angle ABC being the difference of ABE, CBE, and the angle ACB being the difference of ACD, DCB; which have been proved to be equals; ABC and ACB are brought within the last formula by the whole of the previous process.