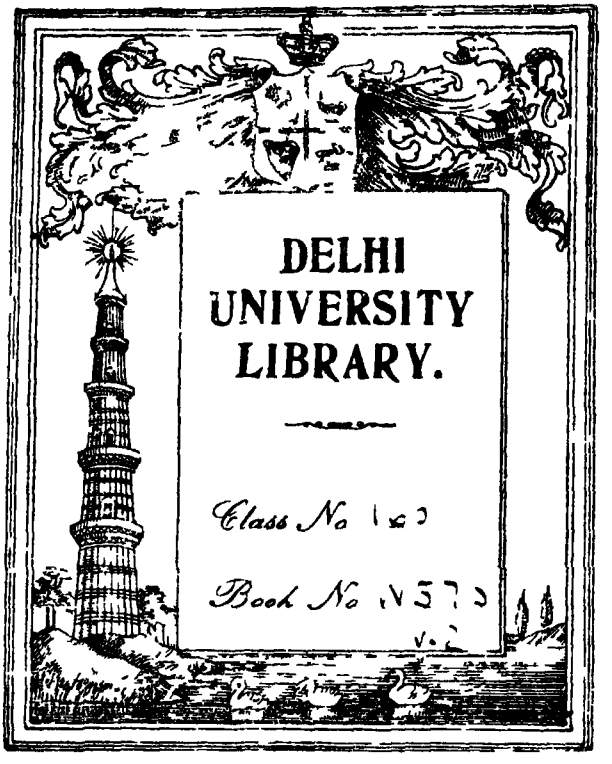

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STATEMENT AND INFERENCE

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STATEMENT AND INFERENCE

WITH OTHER PHILOSOPHICAL PAPERS

BY

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EDITED FROM THE MSS., &c

BY

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FELLOW OF UNIVERSITY COLLEGE

WITH A PORTRAIT, MEMOIR, AND SELECTED CORRESPONDENCE

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PART III

INFERENCE

I

THE GENERAL NATURE OF INFERENCE

§ 208 THE main object of the part of logic which precedes Inference is to study the forms and characteristics of propositions and terms (or, as is sometimes said, of judgements and conceptions) when they have been arrived at, rather than the manner in which they are attained, though the latter may require some consideration as subsidiary to the main inquiry. We have given reasons for preferring to speak of judgement, opinion, and apprehension in this connexion, and have maintained that the distinction between judging and inferring is false and, if assumed without scrutiny, leads to difficulties [1] Logic is, however, also concerned with the way in which we arrive at judgements [2] and opinions, not with every way—not, for instance, with the processes of perception—but with that which is called inferring

Before we begin the study of logic we are familiar with the idea of inference It is an operation we conduct in everyday life and in the sciences We do not learn it from logic nor did

[1] See the criticism of this doctrine in Part II, ch 1, §§ 39–41

[2] On the use and abuse of the word judgement see Part II, ch 2

Chapters 1–4 were part of the Logic Lectures, though from time to time given as a separate course with the chapters on Induction This accounts for some confusion in the opening which was never properly reconsidered The chapters were continually revised in manuscript note-books of pupils, and new sections added The structure goes back, however, to an early period and retains marks of what may be called the Kantian period of Wilson's thought The criticism of the syllogism and of modern mathematical theories (Chapter 7) is, however, as Wilson would have left it in principle Chapters 5–7 are from a separate course on Hypothetical Reasoning Substantial additions and modifications were made in 1906. I have supplemented them from the original manuscript

Wilson nearly always put the minor premiss of a syllogism before the major I have altered it throughout as he nowhere justifies the breach of convention, and it is very confusing where, as in this part, the argument is largely formal

logicians invent it, though they have often affected to teach it. Words like conclude and conclusion, proof and prove, are not part of a technical vocabulary invented by philosophers; they belong to the inferences of ordinary life Although, then, some of the technicalities of the schools do make their way into the language of ordinary men, most of these terms are the familiar product of a certain natural logic and come simply from the fact that men have gradually been led to reflect upon the operations of their own thinking.

§ 209 If we consider instances in which we should naturally speak of concluding or inferring, we shall find that they are not instances of experiencing Our attitude in experience seems to us to be mainly receptive, but in inference we appear to exercise an originative activity either in the discovery of truth or in probable conjecture It is this originative activity of ours, as opposed to experience, which is one of the main characteristics of inference

But we do not suppose ourselves entirely originative. In the first place, we do not think that we originate truth but that we discover it. Secondly, we do not think that our method of discovery, in the process of inferring, is entirely independent and unassisted from without; we think rather that our activity consists in deriving the truth discovered from something already known and ultimately from experience This brings us to another main characteristic of inferring; the knowledge it gets is derived from other knowledge. Hence we again have the idea that this kind of knowledge is in some way dependent. Thus, from the subjective side, inferred knowledge comes to be looked upon as indirect, as not immediate knowledge but mediated, while on the objective side the inferred facts are sometimes looked upon as dependent for their existence on those from which they are inferred We sometimes even tend to put inferred knowledge on a lower level, somehow, than that from which it is derived. This is a natural outcome of the idea of dependence. It happens that an inferred opinion may be called a *mere* inference, and, with an implication of defectiveness, we may say of some kind of knowledge or opinion that it is only inferential, implying a want of something we think better and should probably call direct apprehension of the subject-matter.

Thus, it is often said, whether rightly or not, that we can only infer the existence of other people and do not directly apprehend their being. And here it must be pointed out that the depreciation of an inference as a mere inference seems to be confined to cases where the inference is not a certain one but a probability because of the uncertainty of the matter, so that the defect is not here caused solely by the *inferential* character of the process. It is otherwise in cases like our belief in the existence of other people, for there we do not doubt the truth which we say we arrive at only by inference, and yet we do feel a want of direct apprehension.

§ 210. Now this very dissatisfaction may so react upon us when we reflect upon it that we may incline to think that the inference in such a case is after all not really certain. We may reflect that it is somehow not like mathematical demonstration and suppose that therefore it is not certain. Yet, to take an everyday illustration, we should under ordinary circumstances be sure from the expression of a man's face that he was angry and show the firmness of our conviction by acting upon it, committing ourselves perhaps in consequence to some serious line of conduct. Here we should naturally call our thinking an inference. We inferred his state of mind, where we could not have the direct knowledge which he himself has of his own emotions

On the other hand, we find no such depreciation as this in the sciences There the highest value is given to what claims to be proof, that is, to what is rightly inferred. Nevertheless, in reflection upon the method of science, though the inferred knowledge is not depreciated as being inferred, the idea, nevertheless, of a dependent character in what is inferred often persists, as well as the idea of a certain indirectness and want of immediate apprehension [1]

Whether all these ideas and tendencies are justified or not we shall be better able to judge when we have advanced further in our study of the subject. We must at the threshold be prepared for the possibility that the problems we have indicated may involve different kinds of inference, and that the answers to them may therefore be divergent. It might be a question,

[1] This subject is resumed in ch 4, infra

for example, whether there is the same directness, or even the same sense of the word, in a probable inference about an object of experience (say the inference that the pea is under the thimble) as in certain geometrical proofs. The inference about the pea and the thimble seems to point to a real difference in kind between inferential opinion or judgement and another method of framing a conclusion on the same subject-matter.

§ 211. The most general account, then, of the problem of logic in Inference is perhaps this: to study the forms and the nature of that activity of the mind by which we advance from given judgements or opinions to new judgements or opinions necessitated or made probable by the former, not by experience but by some other operation of the mind. Yet here also there are difficulties This operation would usually, perhaps always, be called thinking as opposed to experiencing; yet there is often considerable vagueness as to what 'thought' should precisely mean,[1] and difficulties arise if it is realized that perception is somehow intimately connected with what would naturally be called thought, though the nature of that connexion has not yet been cleared up or investigated This at all events is true, that the advance in knowledge which is meant is, as we saw, not one made by getting some new perception to add to the given statements from which in some sense the advance is made. For when inference is supposed to be due to the operation of thinking we find a tendency sharply to distinguish this thinking as pure thought from both experience and imagination. This is undoubtedly the case with the formal logicians, who suppose that formal reasoning, which really means reasoning from the mere forms of statement without any matter, proceeds entirely by pure thinking.

Yet imagination is absolutely necessary to the processes of those sciences which seem to be the most perfect type of what can be produced in the way of new and certain judgements by inference. Geometrical thinking, and indeed all mathematical inference, is impossible without imagination, and it is even true that the abstract reasoning treated of in formal logic (which consists of syllogisms in which the premisses seem to be mere forms of statement and can therefore be expressed symbolically),

[1] Part I, ch 2

reasoning which is often taken as the type of pure thinking, is quite impossible without the use of imagination. Again, imagination depends upon experience, and thus inference stands in essential relation to experience. But notwithstanding this connexion with experience and imagination, it still remains true that what is really meant by inference is that the new knowledge we reach we do not reach simply by getting fresh experience.[1]

In view of these very real difficulties the best way to form a correct idea of inference appears to be to study first those inferences which are certain and which constitute knowledge, or which at least we suppose to be certain, for it seems obvious that we can, in a given subject, only understand the imperfect type from a consideration of the perfect We shall accordingly begin with a consideration of this kind of inference

§ 212. It will be observed that the above general account, in common with the usual definitions of inference, has an important presupposition which is not always sufficiently reflected upon Thought in this inferring process is spoken of not as absolutely originating truth,[a] but as starting from some truth, whether that is given in experience or not. In the technical language of logic the judgements or opinions from which the thinking process thus starts are called premisses The judgement or opinion to which thought advances by its own operation is called the conclusion Now it is obvious that the conclusion must be different from the premisses and in some sense really new The propositions 'all A is B' and 'some A is B' are different, but the latter is not new if we have the former. That this should be so is self-evident, but that it is also recognized explicitly in logical treatises is shown, for instance, in the objection to the syllogism that it is a *petitio principii*. For the meaning of that is that the conclusion does not seem to be new as compared with the major premiss. It should be observed that this demand is made even in an empirical logic like that of Mill[b] He will not allow an argument to be a true inductive

[1] Evidence of the really distinct character of inference as a form of apprehension may be drawn from the hypothetical statement There we have at any rate knowledge or opinion expressed which can be got only by inference. Cf. §§ 102 and 298

[a] Cf my note to § 39

[b] 'Cases of inference in the proper acceptation of the term, those in which

inference unless it contains more than is contained in any single premiss. Now this involves a presupposition which he has not reflected upon: it implies that the new knowledge is not the result of experience and must therefore be due to the inferring process itself. Thus, the conclusion is unavoidable that in some important sense a mental process which is not experience can originate knowledge. It is futile to object that the mind merely works on the material which is given in experience, for this implies that we are able in the process to get on to new knowledge not in the material. This then must be due to the mental process which brings the new result. Such origination contradicts the very foundation of an empirical philosophy like that of Locke and Mill.

Here we are not so much concerned with this criticism as with the light which it throws on the important principle presupposed in the ordinary idea of inference, in so far as we find that it forces itself even upon those philosophers whose doctrines make it, one would have thought, impossible to admit it.

§ 213. Inference is usually divided into mediate and immediate. To judge from the instances by which the distinction is illustrated, the idea at bottom of it seems to be that in immediate inference we pass directly from one premiss, i.e. from one given judgement or opinion, to the conclusion by a mere reflection on the given premiss. In mediate inference something comes between a given premiss and the conclusion, that something being another judgement or premiss. Thus, immediate inference has only one premiss, mediate has at least two. We may represent this mediation in another way. In a given premiss it would be said ordinarily that we have two conceptions in a certain relation. To be more accurate, the premiss states a relation between two objects of apprehension.[1] The inference is immediate if we arrive at some other relation merely by considering the given conceptions themselves, by considering, that is, what we apprehend in these two objects in the act of apprehension represented by the given premiss. It is mediate if we get a new relation by the help or mediation of some new

[1] See Part II, ch. 14, on the use of the term conception.

we set out from known truths to arrive at others really distinct from them. *System of Logic*, II. 1, § 3.]

conception not contained in the premiss; by the help, rather, of the apprehension of something else not contained in our previous apprehension.

Now clearly this latter can only be done by relating the given conception (so called) to this other conception, and such acts of relation are judgements or opinions, and hence we get more than one premiss. We get exactly two if we represent the process as follows we find ourselves unable in a matter of two conceptions, on the strength of the knowledge which makes them what they are for us at the moment, to relate them in some particular way. Thus (we should say), we cannot connect them directly. We then mediate their connexion by a new conception to which each is related. each relation is the matter of a judgement or opinion, and thus we get two premisses.

§ 214 The above account of mediate and immediate inference is not offered as a satisfactory definition of what such terms ought to mean, or even as implying that inference is properly so divided It is intended rather as an explanation of what the traditional distinction amounts to Thus, if we find it stated that an immediate inference is one in which a judgement follows immediately from another judgement, it might well seem a more natural construction to put upon the distinction of mediate and immediate inference to say that if a judgement A necessitates another judgement B directly, that is immediate inference, and if that judgement B in turn necessitates another judgement C directly, the inference from A to C is mediate, because the connexion of A and C is only acquired through B. But that is not the traditional sense of mediate inference, for, in the ordinary deductive logic, the syllogism is the main type of mediate inference, and such a definition does not suit the syllogism because in the syllogism the first premiss is not supposed to necessitate the second. In the end we shall depart so much from tradition as to show reason for calling all inference in an important sense immediate. No doubt such a view seems at first sight paradoxical, if we are under the impression which the account of immediate inference in formal logic naturally makes upon us, for we should not expect that such so-called inference is real inference at all, but that immediate inference only gets its name by a kind of analogy.

§ 215. The syllogistic logic in treating of inference has only before it the general form S is P or S is not P, with the quantitative distinctions all S, no S, some S, and this S. What it does in effect in the case of immediate inference is to ask, given a certain judgement or proposition in one of these merely general forms, what relation can be inferred between the subject and predicate conceptions (more accurately, between what corresponds to them in reality) or their negations,[a] besides those stated in the given proposition? Further, it is an element in the problem as usually conceived that the conclusion of the inference should be restricted to the form in which the so-called original subject and predicate conceptions and their negatives are to make up the subject and predicate conceptions in the conclusion For example, from all A is B, we may infer some B is A, or no A is not-B, but not, for instance, that A and B are compatible; for the predicate 'compatible' does not occur in the premisses. Again, the inference from all A is B to some B is all A is not recognized as an immediate inference, because a restriction is made that the so-called predicate is to have the original adjectival form, or at all events is not to be preceded by a quantitative word like all, or some

Such restrictions are clearly artificial, but so, too, is the whole account of immediate inference. It is not meant that these restrictions are formulated and consciously made, they are at first simply the result of an uncritical tradition The inference B is less than A from A is greater than B conforms indeed entirely to what is essential in the definition of immediate inference as it seems to be understood in syllogistic logic, and we note also that it is this kind of immediate inference which is most frequent and important in ordinary life, and in the procedure of the sciences According to the doctrine of the syllogistic logic, it would be said that in such cases the immediate inference is from the matter of the proposition, not from the form, whereas in the immediate inferences treated of the inference is from the form and not from the matter.

Now even if this were true, as it is not, it would not be any reason for neglecting the 'material' immediate inferences, unless it could be shown that they are comprised under various kinds

[a viz. what formal logic names their *contradictory* terms]

of immediate inference from the form merely. Now clearly they are not so comprised; the relation of A to B in the proposition all A is B is neither the same as A is greater than B nor does it include this relation, for in 'A is greater than B', A and B cannot be represented as subject and predicate respectively, nor as subject and attribute, nor could the given inference from A is greater than B be derived from it by any rule of immediate inference given in the syllogistic logic

Nor, again, can these material inferences be represented as syllogistic; the attempt to represent them as such would result solely in stating as a premiss of the given inference that if A is greater than B, then B is less than A, and next in subsuming under it a particular A and a particular B; whereas of course it is not the proposition that this B is less than this A which we are concerned with. The inference is the alleged premiss itself

The fallacy of such verbal reductions to the syllogistic form will become more apparent when we criticize the traditional reduction of all demonstrative reasoning to the syllogistic form. There it will be shown that this form is but one among many of a certain class and that the other members of the class cannot be reduced to it

We can now see what the general notion is which is common to both of these kinds of immediate inference, and we can see that the type of immediate inference studied in formal logic is but one among many. The general type is, that from some given relation between two elements X and Y we infer directly, and without the addition of any other statement about them, some other relation between X and Y. Now the syllogistic logic confines itself to one only out of many possible relations, that which is usually expressed by the term predication The term, however, is used uncritically, for subject and predicate do not here mean logical subject and logical predicate proper. In this reference, the proposition is supposed to have the form S is P, or S is not P, and it is meant that in this form P is the predicate. Now the true relation of S and P (the relation which the syllogistic logic has here in view) is this: P-ness is a kind of being which S, the so-called subject, is stated to have or not to have. The relation therefore is an objective one.[1] But any other

[1] Part II, § 68.

relation between two such objects upon which an immediate inference can be grounded is equally entitled to a place in any theory of immediate inference.

To return now to a previous point, it might be said that our ability to infer immediately from the statement A is greater than B that B is less than A depends, as it really does, upon our knowledge of the *matter* of the relation, in other words, on our knowledge of the special character of the relation greater and less, whereas the other kind of inference depends upon the general *form* of the judgement or statement without any matter. Now this would be a false distinction. The one kind of inference is as material as the other Just as from the statement A is greater than B, our inference that B is less than A depends upon our knowledge of the relation of magnitude, so also our inference some B is A from '*all* A is B' depends upon our knowledge of the nature of the special objective relation which has here, through a confusion, got the name of predication (which properly designates a merely subjective relation). Moreover, as we shall see hereafter, the rules of the syllogistic logic depend upon the special nature of this relation, and, in this sense, are as much material inference as any other. The subject of immediate inference is treated in the ordinary logic manuals under such heads as conversion and opposition. We are not, however, here concerned to follow this in detail, but only to consider in general the relation of the conclusion to the premiss in such immediate inferences, to ask how far they constitute a real step in thinking and what claims they have to be called inference at all We shall also have to speak of other kinds of immediate inference besides these so-called 'formal' ones

§ 216 The statement some A is B might be called an immediate inference from all A is B, but it does not satisfy an essential characteristic of inference it is less than we knew in the premiss. Consider, however, the relation of some B is A to all A is B (the only form of immediate inference by conversion from all A is B allowed in formal logic). The inference in the form some B is A appears no longer to be, like some A is B, a mere part of what we knew before, in the sense of being less than what we knew before, for the true full inference is some

B is all A.[a] We ask, then, the verbal form being different, is there a difference of meaning such as we require for inference? In the facts expressed by statements such as all A is B, or A is equal to B, or A is a friend of B, the realities to which A and B refer stand in a reciprocal relation to one another, and the nature of each is affected by its relation to the other in the obvious sense that the being of each includes the relation. But the relations included in the complex fact of the reciprocal relation of A and B are various. Sometimes they are different in species, and then the difference is seen at once, as in A is the father of B and so B is the son of A. But the same is true even when the relations are the same in species; if A and B are friends, A's friendship for B is different from B's friendship for A. Even in such an instance as A is equal to B the same holds. Each of these different relations is expressed by a separate verbal form of statement, e. g by all A is B and some B constitutes all A. Even if we confine ourselves to the traditional form some B is A, that expresses a relation of B to A different from the relation of A to B Such difference of relation we may illustrate more concretely, thus: A is half B; in that way A is related to B; an immediate inference is that B is twice A, representing the relation of B to A. These two relations are obviously different, the half and the double. But now the relation of A to B and the relation of B to A being different, they nevertheless necessitate one another and the act of judgement or opinion involves in either case, for the person forming it, the other relation, though he does not express it verbally. The objective fact, indeed, to which the first statement relates is a unity, having two sides represented fully by two statements; the two sides are not merely parts of an aggregate, but are inseparable; the complete fact to which the statements refer being their unity. The expression, however, in either of the two statements is as expression one-sided;[b] which side it will happen to take depends on what we have taken as our starting conception or logical subject.

We see, then, that the so-called inference is in a sense not

[[a] This is to adopt the extreme view of one school of formal logicians. But then, in Wilson's sense, there is no inference since all A is some B and some B is all A are statements of identity

[b] 'Consider the implications.' *MS note.*]

new; and this is what causes us our difficulty and makes us doubt whether the inference should be called inference at all. It is not new; the conclusion is involved in the act of thought which makes the premiss; the truth being that the premiss brings out one side of the act of thinking, while the conclusion brings out the other side. Yet we cannot call one of these statements identical with the other, nor is one a part of the other: each of them involves the whole, which gets its expression verbally in both together. Now it is because of their difference, which, as we have seen, is not that one is merely part of the other but that they express two different relations, that such inferences are entitled after all to the name of inference. They satisfy this definition, that the one is different from the other and necessitates the other. The process from the one to the other is not on that account the less inferential that it is so simple, and it will actually turn out in the end that this immediate necessitation by one element, or complex of elements, in a whole, of another element, or complex, in the same whole is what always constitutes inference.

Inasmuch, however, as the simplicity of the process tends to mislead us and to make us think that in the immediate inference there is merely a repetition and at most only a change in the verbal expression, it is useful to observe certain examples. Consider the immediate inference [a] from all A is B to no A is not-B. Suppose we start from all A is B: that necessitates no A is not-B, and the latter states explicitly an element in the whole thought to which the first statement, as a partial statement, corresponds. For we ask first whether A can, or cannot, be B, and have before us the possibility both of all A is B and of some A is not-B. In deciding for the first we exclude the second.

The two, then, are inseparable aspects of the same truth, but the difference between them appears at once when we consider how each can be got. Though each is necessary to the other, they are not in our thinking co-ordinate, for we find we can only possess the universal negative in the form of an immediate inference from the affirmative. We might think at first that we could get each independently; that, while we get all A is B by finding that the nature of A necessitates B, in getting the

[a 'Rewrite this more clearly' *MS. note.* See Part II, ch. 12]

negative we might, though ignorant of the reason why all A is B, have arrived at no A is not-B simply by finding that there was no A outside B. But, as we have already seen, in many instances (and indeed in every instance of a scientific universal), this last process cannot be performed independently, because the area of not-B is infinite, and hence we can only tell what is in this sphere or not by considering positively the nature of A and the nature of B. In the cases where we seem to form no A is not-B without the affirmative all A is B, we really depend entirely upon affirmations, which divide up exhaustively the indefinite sphere of not-B. Thus, the exclusion of A from not-B could not be an independent act involving mere negation; there is no way of arriving at no A is not-B except by establishing all A is B. Hence, we cannot arrive at the negative statement in question at all except as an immediate inference from all A is B. All A is B may be immediate in the sense that I see immediately that A necessitates B, no A is not-B cannot be itself an immediate apprehension, but can only be acquired as an immediate inference

§ 217 [a] It is sometimes said that the inconceivability of the contradictory of a statement is the test of its truth, and specially that it is *the* test from which we really derive the certainty of axiomatic truth Now the preceding discussion of the relation of no A is not-B to all A is B gives us one criticism of this theory. The theory at bottom assumes that we can start with the negative A cannot be not-B, and upon that are able to ground the statement that A must be B; for the assertion that we cannot conceive the contradictory of A must be B really means that we affirm that A cannot be not-B and that we apprehend that directly, together with the implication that this judgement is acquired immediately. That is what is meant by calling it the *ultimate* test. But, as we have seen, we can only pronounce this negative judgement because we have already seen that A must be B. That is to say, this inconceivability of the contradictory supposed to be a test of the axiom is only the consequence of our already having apprehended the truth of the axiom. All that is true in the doctrine is this: that, if

[a Referring to Mill's controversy with H Spencer *System of Logic*, ii, ch. 7; cf. infra, §§ 355 seq]

we really see the necessity of a thing, we cannot conceive it otherwise.

§ 218. We may now give another kind of example of the reality of the step made in immediate inference. In what is called pure formal reasoning—the simplest of all—we sometimes find it either necessary or convenient to draw an immediate inference from one or more of the premisses in order to get our conclusion. Now this shows the reality of the process that we go through; that it must be something more than a merely verbal change. It will indeed sometimes be found that a complex of formal premisses, though obviously dealing with the simplest relations possible, causes us considerable difficulty, and we see our way through a complicated system of premisses by help of a number of mere immediate inferences. Indeed such difficulty is sometimes found with quite a few premisses. For instance, given that no AC is B, and no D which is not-B is A. The solution becomes quite easy if we first transform by immediate inference the second premiss, and put it in the form

All A which is not-B is not-D (= No A-not-B is D).

The original first premiss, then, being transformed into all AC is A-not-B, we have a simple syllogism in Celarent with A-not-B as the middle term, and the conclusion is that no AC is D.

Or, again, given that all AB is C, and all A-not-B is D. This is solved simply by immediate inference from the first premiss to all A-not-C is not-B, that is, all A-not-C is A-not-B. This gives us with the second premiss as major a syllogism in Barbara of which A-not-B is the middle term, and the conclusion is seen to be that all A-not-C is D.

§ 219. There are certain processes which we should on reflection not be inclined to call inference (nor are they usually recognized as such in logic) which yet have the verbal form of an inference and, if judged by the test which we have just applied to immediate inference, appear entitled to be called inferences because the argument seems to require the first step which is preceded by the word 'therefore'. Moreover, they seem sometimes to exhibit in the conclusion a real difference from the sum of the premisses. For instance, if we know that $\alpha\beta$ is C and find first that A is α and then that A is β, it follows that A is C. This would usually and naturally be expressed as

follows: A is α and A is β, *therefore* A is $\alpha\beta$. But $\alpha\beta$ is C and therefore A is C. Here, while the step A is $\alpha\beta$ appears necessary, it yet seems to be only the two premisses together. Again, suppose AB exists, and we have A is α and B is β, *therefore* AB is $\alpha\beta$; but we know $\alpha\beta$ is C, therefore AB is C. The step AB is $\alpha\beta$ seems necessary. Further, it seems this time to differ from the premisses, because from it given alone we could not get the premisses. It is compatible, for instance, with A is β and B is α. Euclid in the first proposition of his first book uses an argument of the form A = B, and C = B: *therefore* A and C = the same thing B; but things equal to the same thing = one another: therefore A = C.

Here the first inference seems a mere restatement of the premisses, yet the step is in fact made and also seems necessary to the complete argument.

Again, it differs from the premisses either singly or together because the term 'the same thing' occurs in neither, and this seems got by a comparison of the premisses. This again seems a new act and not a mere restatement.

§ 220 To solve such difficulties certain distinctions have to be made which ought to be preliminary to any theory of inference, and are yet commonly, perhaps always, neglected, to the confusion of certain parts of the subject.

We must distinguish first between the thought which the verbal form given to a judgement expresses and the whole thought which produced the expression, for the former may not be the whole of the latter. As the judgement really is the whole thought which produces the imperfect expression, if the latter is taken as the true expression of the judgement, there arises the fallacy, common in logic, of distinguishing the judgement as a result from the thought said to produce it; an impossible abstraction, for this thought is the full judgement. What is called the process of arriving at the judgement is really the act of judging. Thus, in B is C, A is B. therefore A is C, A is C is represented as a judgement resulting by inference from the other two. But this inference is exactly the judging that A is C, and thus A is C expresses only a part and not the full judgement. The full expression is A is C because B is C and A is B. This solves the difficulty just raised about the proposition AB is $\alpha\beta$,

namely, that the premisses cannot be got out of it. AB is $\alpha\beta$ is only judged on the ground A is α and B is β, and so the full expression of the judgement (that is, of the thought which is necessary to make the verbal expression AB is $\alpha\beta$ possible) is AB is $\alpha\beta$ because A is α and B is β. Thus the premisses must appear in the only way in which AB is $\alpha\beta$ can really be a judgement, and the difficulty raised is a fallacy caused by the false abstraction of a judgement from the way in which it is judged.

We must again distinguish between the apprehension and the fact which is apprehended. It will be found that much depends upon the question whether the premisses are taken to represent the one or the other.

Thirdly, we must distinguish between our apprehension of a fact and our memory that the fact was apprehended (which is not necessarily a memory of the apprehension itself). Observe that if the apprehension was an experience, the memory of the experience (not the mere memory that it was experienced) is not itself an experience and is not a repetition of the previous experience. If, however, the apprehension was a process of proof, the memory of the full proof is itself the process of proof, or we may call it a repetition of the proof

By help of these distinctions we shall see that in the cases under consideration the fact represented by the conclusion is equivalent to the facts represented by the premisses in conjunction and not something different from them necessitated by their conjunction, and that in this sense the conclusions are not inferences. On the other hand, we shall see that the apprehension represented by the conclusion, or corresponding to it, is not the same as the apprehensions represented by the so-called premisses, nor is it the same as these in conjunction, and the difference is not merely one of verbal expression

§ 221. The difficulties may be resolved in this way. Consider first the premisses as representing the facts apprehended. The fact of A's being α, and the fact of A's being β, that is, the co-existence of these facts, does not necessitate A's being $\alpha\beta$ as something different from itself. On the contrary, it is the fact that A is both α and β. And the other cases may be treated similarly. Consider next the premisses [a] as representing acts of

[a ' Change the example, since α here necessitates β.' *MS. note*]

apprehension. The facts A is α and A is β are in themselves not separate, but the apprehensions of them may be separate and, more than that, in some cases it may be impossible to have these apprehensions together. For example, the triangle formed by the diameter of a circle and by two straight lines drawn from its extremities to a point in the circumference is a right-angled triangle. It is true of the same triangle that the square on the diameter is equal to the sum of the squares on the other two sides. The apprehension of the first property is the proof given by Euclid in Proposition 31 of his third book. The apprehension of the second property is the proof given in the 47th Proposition of the first book, and nothing shorter. Neither of these apprehensions contains the other, nor can we have them simultaneously, as we cannot conduct two proofs simultaneously. Here then the judgement A is both α and β, which we undoubtedly ground somehow on the apprehension of A as α and the apprehension of A as β, is not the same as these apprehensions nor the same as their conjunction. It is therefore in some sense a judgement which is different from them but which they necessitate. It is on this account that the process has a resemblance to inference, and on this account also it is natural in the statement of the argument to add to A is α and A is β the statement, *therefore* A is $\alpha\beta$.

To see whether there really is an inference we must ask what exactly the apparently new judgement, A is α and β, is. Suppose we prove that A is α, and then prove that A is β. It would probably be said that in the proof that A is β, or at the end of it, we remember the result of the proof that A is α, though we have not the proof. The word *result* is somewhat misleading: it rather implies that we remember the mere fact that A is α without reference to the proof, because it is something different and resulting from the proof. But this is quite impossible; the accurate expression is, that we remember neither the proof that A is α nor the mere fact A is α, as a result, but the fact *that we proved A is* α. To put it otherwise, we are not really apprehending A's being α but remembering that we once did apprehend it.[1] In the proof of A's being β, *if it is all before us*, we have the apprehension that A is β. If we have this together with

[1] Observe, *not* remembering the apprehension.

the memory that A was proved to be α, this would be verbally expressed in the two inadequate formulae of judgement, A is α and A is β simply. And now is the judgement A is $\alpha\beta$ an inference from the apprehension A is β, and the *memory* that we apprehended that 'A is α'? It is not such an inference, for it is obviously nothing but having these two judgements together. The same is true if the verbal form of judgement A is β also represented not the proof that A is β but the memory that there was a proof. But again we may ask, is it an inference from the original apprehensions or apprehension? If 'A is β' happens to represent the second apprehension, and 'A is α' the memory that we had the first, the difference is only in what 'A is α' stands for, and this is only the difference between apprehending that A is α and remembering that we once apprehended it. But now no one would call the memory that something happened in our apprehension an inference from the apprehension of it, though it is true that the apprehension here conditions something different from itself, that is, the memory that it happened. For an inference is always understood to be from what we have before us, what we are now apprehending. Now by hypothesis the given apprehension is not before us. The same is obviously true if both 'A is α' and 'A is β' represent memories that there were proofs of them; and if one or both of the original apprehensions was an experience and not a proof, exactly the same treatment applies Thus, finally, though it would follow that the judgement 'A is $\alpha\beta$' is not properly called an inference from 'A is α' and 'A is β', we seem to have the explanation why it is natural to put in this step (therefore A is $\alpha\beta$) expressly. The reason is that when it is *naturally* introduced it represents a stage of consciousness which is different from the apprehensions (in the proper sense of the word) that A is α and A is β, is indeed a step necessary to the proof

§ 222. The preceding investigation seems to bring out the reason why we hesitate to call certain processes inferential even when the step taken seems necessary, or at least natural, and therefore not a mere restatement of what has preceded. In the processes which we do not hesitate to call inferences the facts or fact apprehended in the premisses necessitate the fact apprehended in the conclusion as a fact different from them-

selves, and the latter fact is apprehended as thus necessitated. Now we have seen that this definition at once decides the cases under consideration not to be inferences. Again, it is these processes (i.e such as correspond to this definition) which are those actually recognized in logic as inferences, though without a clear consciousness that this is so and of all that it implies. Now a principle which is correctly used in particular instances is not always clearly recognized in the abstract or correctly formulated. This has happened with regard to what seems the true principle of inference; the apprehension, one may repeat shortly, of one fact as necessitated by a different fact or facts. For inference in general is sometimes incorrectly represented as the necessitation of one judgement by another or others. If this were so, a memory, as necessitated by a given apprehension, should be regarded as an inference But, as we have seen, nobody thinks of maintaining *that*; the real reason being that we are guided in this particular instance by a sound instinct and are using the true principle For though in the given case the memory is necessitated by the apprehension, the memory itself is not an apprehension of this necessitation.

§ 223. The account given of the process A is α and B is β: *therefore* AB is $\alpha\beta$ would, from one point of view, make an inference in the third figure of the syllogism into an immediate inference.

The form of the figure is M is P, M is S, therefore some S is P (or some P is S).

The conclusion follows because the same thing M is both S and P. We have then M is P, M is S, therefore M is both S and P. The latter we have ruled not to be an inference from the premisses, but merely a statement of them as both holding together (which, we must notice, does not differ from the simple statement of them), and the inferences from it of some S is P and some P is S are immediate. Thus there would be nothing in the third figure but immediate inference. The syllogistic logic is committed to this anyhow, if 'M is PS' is regarded as one judgement, because that logic does not recognize 'M is both S and P' as an inference. But now this analysis is not confined to the third figure. Consider the first figure: all B is C, all A is B, therefore all A is C. The premisses, all B is C and all A

is B, may be combined in one statement, just as in the third figure. Thus some B is all A and is at the same time C, or some B is C and all A, and from this the inference to all A is C is obviously immediate. This, however, is only an anticipation of what will be maintained later, that the relation of the conclusion to the complex of premisses (whether syllogistic or not makes no difference) is always immediate, is not mediated by anything intervening.

§ 224. We have been led to recognize a principle which holds not only in the non-inferential processes we have been considering but in inference proper What we have said of two proofs each with a single conclusion, that the apprehensions which they constitute may possibly not be present together, may hold within one of these proofs. For it may have parts which cannot be had as simultaneous apprehensions. The verbal form of a premiss A is B, used in drawing an inference, may correspond not to the apprehension of A's being B, but to our memory that we have had such an apprehension, so that in the strict sense we are actually not judging the judgement 'A is B'. This, though not the exception but the rule in the vast majority of proofs, seems quite ignored in the usual treatment of inference. It has important consequences and, among other things, it seems to be a part at least of the key to the possibility of a kind of error in the exact sciences which is a stumbling-block to theories of knowledge and error.

§ 225. We may return to the contention that the judgement A and C are equal to the same thing differs from the judgements A = B and C = B, because the term 'the same thing' does not appear in either of these two latter, which may be accounted premisses. It is true that it does not appear in the verbal expression, but it is contained in the thought which corresponds to this expression. For in judging C = B, we must recognize B as the same B which = A or we should not use the common term B at all. If we had forgotten the judgement B = A this would not be true, but by hypothesis we have not forgotten it. The reason for introducing the step in the verbal expression is that which we have already given for the introduction of the step A is $\alpha\beta$ or AB is $\alpha\beta$.

§ 226. Inference is often spoken of as if it were essentially

a connexion of judgements, as though the premisses were judgements which necessitated our forming another judgement. Shall we say simply that the judgement of the premisses (that is, the judging of them) necessitates the judgement of the conclusion? We have seen already that this, whether true or not, is too wide to be taken for a definition of inference, for it would include memory.

Now first observe that whether this is true or not it is in any case not the meaning of our verbal statement. If we say A is C because B is C and A is B, we do not mean that our judgements B is C and A is B necessitate our judgement that A is C, but we mean that the *facts* B is C and A is B necessitate the *fact* 'A is C'. In science again the value of such inference is not that somebody believes the conclusion because he believes the premisses, but that an objective necessitation is actually apprehended. Thus it is the connexion of the objects apprehended which is meant, it is this which is apprehended in inference and which is of interest and importance to science.

Secondly, we may truly say that I apprehend the conclusion, or have the apprehension represented by the conclusion, because I have the apprehensions represented by the premisses In this sense one apprehension may be said to necessitate another. But, though true, this statement has a form which may mislead. Apprehension, we have seen, cannot be abstracted from what is apprehended, and there is a danger in separating them, as we have seen in discussing the *a priori* view of knowledge and the theory of the inconceivability of the opposite as a test of truth. There we have maintained that a necessary apprehension is only intelligible as meaning an apprehension of an objective necessity. We shall find this account verified in the case before us. How is it exactly that the apprehensions B is C and A is B can be truly said to cause or necessitate the apprehension A is C? Only in this way. I apprehend the facts B is C and A is B and then apprehend these facts as necessitating A is C Now that means that the necessitation of the apprehension A is C by the apprehensions B is C and A is B is after all just the apprehension of the necessitation of the fact A is C by the facts B is C and A is B. Thus, as before, the necessary apprehension is only necessary because it is an apprehension of a necessity, and the

question as to how necessity in the thinking can correspond with necessity in the object cannot arise.

Finally, suppose that one or more of the premisses of an inference is not an apprehension proper of the fact that B is C or that A is B, but our memory that we apprehended the fact (experienced it) or proved it. If we remember proving that B is C and A is B, even though it be said that memory is uncertain, yet at least we *know* that, if we remember rightly that B is C and that A is B, the facts would necessitate A's being C.

§ 227. The definition of inference that we have given does not state that the connexion on which it depends is always one of universals, and yet we know that we always find it to be so.[a] Now this is not because we define inference beforehand as only dealing with such connexion. We find in any instance where we could be said at all reasonably to apprehend the necessitation of a particular fact by another particular or particulars, that such necessitation is only a particularization of a necessary connexion of universals, and in the inferences which we are about to examine we shall always find that we have to do with this universal necessitation. Aristotle recognizes the universal character of inference in so far as he makes the reason or ground [1] to be universal, but he recognizes this fact as familiar without reflecting on it and without realizing the necessity of asking any questions about it, as is often the case with facts with which we are familiar. Clearly the question must be asked why inference should have this character. There is a danger of avoiding the difficulty by somehow including universality in our definition of inference, as some modern treatises do,[b] thus over-

[1] αἴτιον, καθόλου. Cf. § 237

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[a] 'Consider and embody the fact that we seem to infer sometimes from what is peculiar to the individual' *MS note* I have put in a foot-note (p. 434), from a hasty scribble, what was the difficulty Wilson felt. Cf p 481

[b] I am not sure of the reference 'The universal in its differences is then the basis of mediate judgement or inference' Bosanquet, *Logic*, II 1 1.

'The general principle on which the validity of every conclusion rests may be expressed by the formula. "What falls under the condition of a rule, falls under this rule itself".' Kant, *Logic*, § 57. Cf § 58. (See also infra, § 262.)

The statement about Aristotle is obscure The general reference may be

looking an important point and evading a difficult investigation. We may defer the question until we have further examined the nature of inference in science.[1]

[1] § 262 The nature of the universality is evident when we argue from a mark of what is of a certain kind But there are instances where we argue from what is a mark of a particular individual alone, so that if there is a universal proposition implied it is of a different sort from the other, and it is important to see whether the view that inference is universal can here be vindicated

to ἐν ἅπαντι (συλλογισμῷ) δεῖ τὸ καθόλου ὑπάρχειν (*An Pr* 1 24) This is the formal rule *ex mere particularibus nihil sequitur*, but the reference to the cause appears to relate to the doctrine of the *Posterior Analytics*, that the aim of ἐπιστήμη is to connect the predicate with the subject through the proximate cause of the predicate The major premiss, Aristotle therefore insists, must be καθόλου Moreover, science is the search for primary, i e most universal, causes (*An Po* 72ᵃ 4–5) But, on the other hand, the *Posterior Analytics* is full of reflection on the question whether demonstration is or is not universal, and on the superiority of true demonstration because it is universal (see e g *An. Po* 1 13 and 24) In fact the *obiter dictum* would perhaps have been reconsidered]

II

THE SYLLOGISM

§ 228. The problem which a syllogistic logic proposes seems to be to discover the general forms under one or other of which all demonstrative argument must fall. This may obviously become the problem, from this point of view, of determining the general type of inference from two premisses. Now, in order apparently to get a quite general solution, this logic makes abstraction of the so-called matter of the propositions and deals only with what is called the form of the propositions. The premisses and conclusion, that is, are treated only under the general form of the relation of subject and predicate (where subject and predicate have the special meaning which has been criticized above), and appear only in these shapes: all S is P, no S is P, some S is P, and some S is not P. Singular judgments such as this S is P rank as universal. But the generality thus sought for is by no means attained by this method. In actual working the syllogistic logic has unconsciously taken quite a limited problem about a special kind of inference and of that problem itself has given but a limited solution.

§ 229. Even if the problem proposed were correct, namely, to find general forms for all inference (and we shall see that it is not), yet we shall find that the rules of inference laid down as preliminary to the discovery of valid syllogistic forms show that the solution actually offered is not the most general solution of the problem which is proposed. For instance, a rule of syllogistic logic is that no conclusion can be drawn from two negative premisses. Yet from no P is M and no S is M we do get a relation of S and P not given by either premiss alone, and that relation might be important. Again, if the middle is not distributed in one premiss at least, we are supposed to get no conclusion. Thus from all P is M and all S is M there should be no conclusion. Yet here again a relation is established

between S and P which may be of importance for some further inference. The fact is obvious that wherever there is a common element in any two judgements, that must serve to relate the elements in the two judgements to one another.[1] The truth is, that this logic does not inquire what inferences in general can be drawn from two premisses in the given general forms. It really asks, given two premisses with a common term (i e. having the same conception as subject-conception or predicate-conception in each), when can we conclude from these to a proposition of which the subject-conception is one of the subject-conceptions or predicate-conceptions in the given premisses while the predicate is the remaining subject-conception or predicate-conception Thus from no P is M and no S is M the problem really is to find a conclusion such that the subject-conception of that conclusion is either S or P and the predicate-conception either P or S Now there is no conclusion from the given premisses which can have the given form, and that is the only justification of the rule that inference from two negative premisses is impossible. The same may be said of the rule about an undistributed middle With this proviso, then, this special restriction as to the form of the conclusion, the rules of the syllogistic logic are correct, but as usually stated, without the necessary proviso, they are incorrect because they violate the elementary principle of all reasoning whatever, that things related to the same thing are thereby related to one another.

§ 230. The syllogistic theory is often spoken of as if it were an analysis[2] of the forms of demonstrative reasoning, but the striking thing and one which has not been sufficiently observed by logicians is that its method is by no means what is usually called analytical. It does not take arguments and abstract from them their universal forms, which is what would be understood by analysis Indeed, if it did so, it could not have the kind of completeness and certainty within its own limits which it

[1] Observe that in § 219 the contention was that A is both α and β is not a real inference, not that there is no inference as to the relation of α and β. In fact α and β are related in the conclusions some α is β, or some β is α, in consequence of their relation to A, as in the ordinary view of the third figure of the syllogism What was pointed out about this was that the inference is immediate from A is both α and β, not that there is no inference at all.

[2] Cf the Aristotelian term *τὰ ἀναλυτικά*.

undoubtedly has. It might show by examples the evidence for a form of argument, but its judgements would be empirical, wanting in universality and necessity. Its actual method is as much *a priori* and 'constructive' as that of any pure mathematical science. It starts with the general conception of a proposition, with a distinction of subject and predicate; it then distinguishes the possible varieties of proposition exhaustively *a priori* and not by any analysis or empirical examination of actual propositions. Then, again, it determines *a priori* all possible combinations of two premisses and determines from them *a priori* all possible varieties of conclusion of the limited kind described This is exactly parallel to the method of a mathematical science, and it will become clear as we go on that the determination of the rules, figures, and moods of the syllogism, which occupies so large a part of this logic, is no part of true logic whatever, though valid enough in itself, but is a science in the same sense as pure mathematics.

§ 231. It must not, however, be supposed that this *a priori* thinking which we have been describing can proceed by pure abstraction only, although it does deal with forms which to a certain extent are abstract. We may perhaps think that in this kind of logic we work with the general form of the syllogism from the first and that we derive from that any application to particular cases Now that is altogether impossible; we cannot understand these forms except by taking definite instances to show what the symbols mean, that is by having matter as well as form. Take, for example, all M is P, all S is M, therefore all S is P. To see the validity of this we must take a particular syllogism with actual propositions, and in that instance we must see directly the proper conclusion, which is as specific and definite as the premisses themselves. We must further see on reflection how the general form of the conclusion depends on the general characteristics of the form of the premisses. The first step, namely seeing the conclusion in a particular case, is the condition of our being able to reason at all in the particular way in question; the second, namely the abstracting process, is the condition of our being able to make the general logical abstraction [a] of the syllogism It is also directly self-evident

[a] '"logical" reconsider.' *MS. note.*]

to us that the form we are abstracting is universally valid, because we can see that nothing in it depends upon the matter peculiar to the instance. Its method therefore is the apprehension of the universal in the particular, and we see how both imagination and perception are necessary to that abstract investigation, *a priori* as it is, which determines the syllogistic rules. This procedure does not indeed prove any rules of inference; it is simply the immediate recognition of them.

§ 232. The syllogistic rules, being but abstractions from the actual procedure of the human reason, cannot be described as rules discovered by the logician and laid down for the guidance of our reason: they tell us what reason necessarily *is*, in a certain limited department, and we cannot prescribe rules for what cannot be otherwise. This is not realized by the syllogistic logicians when they draw up a list of fallacies to be avoided. As reason has rules, they seem to think that these rules may be broken and sometimes expressly say so, and then they imagine that this is confirmed by the fact of error. They are rules, though the phrase is misleading, but they cannot be broken; if they could, no logic could ever instruct a reason capable of such error. On inspection it will readily be seen that the so-called formal fallacies arise from a misrepresentation due to the logician himself of certain actual trains of argument; these he treats as though they were intended to be demonstrations, whereas they are but probable arguments, which no one supposes to have proved their conclusions. The precept, for instance, about the distribution of the middle in syllogistic argument is absolutely nugatory; no one can perform the intellectual feat of arguing with an undistributed middle and supposing that he has a necessary demonstration.

§ 233 [a] The ordinary analysis of a proposition on which the syllogistic logic is based, all S is P, implies a type of proposition in which the predicate is attached to each individual comprehended in S, or each case of the universal S-ness, independently and not as being in some relation to the other individuals. Take the statement that all triangles have any two of their sides together greater than the third; here we can say that

[a This section is marked 'to be rewritten'. I have found no redraft and left it in its present very lame form.]

each triangle has any two sides together greater than the third because each has the given property quite independently of the existence of other triangles. But now take the statement that things which are equal to the same thing are equal to one another. Here we can get no single subject expressed at all which will do for our purpose We cannot, for instance, make 'each thing' the subject and 'equal to one another' the predicate. Thus the ordinary form of the syllogism is adapted to the first case but not to the second. In this second case it normally requires some trouble as well as considerable periphrasis to force the statement into the proper verbal expression required for the syllogism. We might put a given argument into this form. Every group of things such that its members are equal to the same thing is a group of which the members are equal to one another But AB is a group of which the members A and B are equal to the same thing. Therefore AB is a group of which the members (A and B) are equal to one another. This conclusion we then have to interpret into A is equal to B or A and B are equal to one another, the natural modes of expression. The necessity of this form of reduction comes from the fact that A and B do not appear in the statement as separate subjects of a given predicate, but only as related members of a group. The artificiality of the reduction is obvious and the reasoning is clearer and simpler in the ordinary non-syllogistic statement of the argument. We shall see presently how such artificial forms arise, and that the true form of reasoning in such a case is not syllogistic at all. But besides this there are certain processes which concern statements of this kind (which we may define as statements of which the grammatical subject is a group of particulars considered as related in a certain way to one another), which the syllogistic logic leaves altogether out of account. We have already had an instance where A and C are equal to the same thing is derived from the two statements A = B and C = B. Now, on the one hand, this process explicitly appears in the syllogistic presentation of certain arguments in geometry and, on the other hand, there is no account given of it in the deductive or syllogistic logic, for obviously it cannot be a syllogism that will give such a result. This shows how little the syllogistic logic is based upon an analysis

of given arguments; for the very first proposition in Euclid's *Elements* contains an example of reasoning of this kind, with the difficulties of which, from the point of view of syllogistic logic, we have been dealing.

§ 234. The syllogistic logic then has not solved its own problem completely We shall now show that its problem is not the complete problem of demonstrative reasoning. We observe that in the syllogism the subject and predicate terms in the conclusion are related through their relation to one and the same term; and this relation is of the special kind called here predication. Now this is clearly a species of something more general, the relation of two terms to one another in virtue of their relation to a third term. The general problem, then, if conceived on the analogy of the problem of the syllogistic logicians, would be to find the general rules which can be laid down for determining the relation between two terms which follows from their relation to a third term In the nature of the case the answer must be that there are no general rules. For obviously the rule of inference in the case of each relation must depend on the particular nature of the given relation itself. It must be got by our knowledge of the special subject-matter and cannot possibly depend upon any general forms of thinking or inference, as, for instance, on what is called formal thinking, which is supposed indeed (though, as we have seen, wrongly) to make abstraction of all such matter.

Take, for instance, the argument most B is C, most B is A, therefore some A is C This argument depends upon our special knowledge or intuition in the department of quantity, that if we take of a given whole a quantity more than half and if we take in this same whole another quantity greater than half, there must be something common to the two parts This is a matter of absolutely direct intuition, and no reason can be given for it other than itself. Clearly the rules of the syllogism would not give us the required conclusion. If we force the argument itself into the verbal form of the syllogism we shall find that we can only do so by a verbal transformation in which we make the argument itself its own major premiss. Thus: Two parts of the same whole which are each more than half of it must have a part in common, A and C are such parts of

a whole B, therefore some A is C. But the major premiss is nothing but putting into ordinary words what is meant by the symbolic expression If most B is C (= BC) and most B is A (= BA), some (B)A is (B)C, for B stands for any whole whatever, and (B)A and (B)C for any parts of it greater than a half. Again, the plane figure A is inside the place figure B, the plane figure B inside the plane figure C, therefore A is inside C. This time the inference depends on our spatial apprehension. It is self-evident. The two premisses are quite sufficient by themselves, yet no syllogistic rule can get the required conclusion out of them.

§ 235. What now is the relation of the syllogism to this very general conception of inference? The answer makes clear the limitations of the syllogism. The syllogism does deal with a definite relation usually, though inaccurately, expressed as the relation of subject and predicate. This predicate is in the affirmative statement a kind of being which the subject has, either covering the whole of it or but a part of it, in which latter case the relation is called that of subject and attribute, and the same holds, *mutatis mutandis*, for the negative statement. It is our consideration of the special character of this relation that gives us the rules of the syllogism, in fact we recognize the rules of inference here as elsewhere because we have a direct intuition of the character of the special relation before us. We must, nevertheless, avoid the error of supposing that this relation is one which covers all other relations. It does not, for instance, cover the relation expressed by A is equal to B, where the relation of A and B is not that of predication. This relation of predication has perhaps been unconsciously confused with the general form of every relation because everything can stand in such a relation. But what does this latter exactly mean? The relation of father and son can, for instance, be put into the so-called predicational relation, if we say, The father is father of the son. Yet the predicational relation here obviously is not the relation of father and son, nor the genus of which the latter is the particular, for the son cannot be predicated of the father or the father of the son. These two terms do not stand in the relation of predication.

§ 236. All this mediate inference has a middle term, but it is

clearly not always that of the syllogism. Its definition is simply that it is a term to which the others are related, and as this relation is not always that of subject and predicate, so the middle term cannot always be the syllogistic middle term. We may easily verify the fact that in scientific investigations we are mainly concerned in seeking relations between given terms which are not in that relation here called predication. There is another consideration which will bring out the fact that the syllogism deals with a special relation and therefore with what is called matter in reasoning as opposed to form We shall always find that the premisses are not the mere abstract form of the proposition. They are very abstract, it is true, but they are not all form; there is left in them just enough matter to make reasoning possible. In Figure I, for instance, there are two premisses, each of which may represent a universal affirmative. Now they could be mere form only if they represented the universal affirmative in its simplicity. But of that there is only one form (all S is P). Thus if we are really dealing with abstract form only, both premisses must have exactly the same form and identical symbols Thus in the expressions all M is P and all S is M we have not abstracted all the matter. The term M is to be the same in both premisses; P and S are to be different in general and different from M. This, then, is matter which has not been abstracted. If it were abstracted, both premisses would be reduced to one and the same form In fact all M is P and all S is M, as a general form, is the general form of two propositions which are different from one another. Thus the matter in them is that the subject-conception M in the one is different from the subject-conception S in the other, and the predicate-conception P in one from the predicate-conception M in the other, and finally that the subject-conception M of the one proposition is identical with the predicate-conception M in the other. This matter which has been left is precisely what makes the inference possible, for without it there would not have been two premisses but only one. We may illustrate this by an analogy from geometry A theorem about two circles intersecting is universal, and it is right to say that it is universal because it treats of the relations of universals. But there is a sense in which it does not treat simply of the universal of the circle, for there is only

one such universal, and we can't have the universal circle cutting the universal circle. That about which the theorem is demonstrated is the universal of two particular intersecting circles, or the universal of which the particulars are groups of two circles intersecting one another, and this depends indeed ultimately upon the universal of the circle.

§ 237. It is no doubt supposed by some to be decisive for the claims of the syllogism to be the general form of demonstrative reasoning that all demonstrative arguments can be reduced to the syllogistic form. This is taken as a fact readily verifiable though without an attempt to show that it must always be so. If we inquire why it really is that this apparent reduction should be possible, we shall discover the nature of the fallacy. If we take the syllogistic argument all M is P, all S is M, therefore all S is P, we can represent this as the application of a general principle to a particular case; we apply, that is, the general principle that all M is P to the particular case of the M's which are S. Now every particular inference which concerns a given kind of relation can of course be represented as the application of a general principle to a particular case, that is, as the application to the particular inference of the rule of inference which belongs to the given relation. Thus we can bring the argument verbally into the form of a syllogism. This obviously involves making the rule of the inference the major premiss in this syllogism, and the particular application of the rule of inference, the conclusion, will be represented as inferred from the rule of inference itself, as if the latter were a premiss. This in fact we shall find to be exactly what is done in the so-called reduction of what are really non-syllogistic arguments to the syllogistic form. Moreover, if the inference is put in a certain form which is really general, though employing special symbols, the reduction will take the form of which we have already had examples, viz. the inference to the general form, disguised in symbols, from itself stated without symbols. And it is clearly a fallacy to represent the rule according to which an inference is to be drawn from premisses as one of the premisses themselves. We should anticipate that this must somehow produce an infinite regress, and that this is so can easily be shown. But now observe that there *are* general forms of argument to which the proposed

reduction cannot be applied because the major premiss (in the reduction), which is the rule of inference, is nothing but the general form of the inference or argument itself. Thus the argument B is greater than C, A is greater than B, therefore A is greater than C, would be reduced to the syllogistic form by taking as major premiss the following statement, when of three magnitudes the first is greater than the second and the second greater than the third, the first is greater than the third, and as minor, A, B, and C stand in this relation to one another. But the expression, if B is greater than C, and A is greater than B, A is greater than C, is perfectly general, because taken to be true whatever A, B, and C may be, and it is because it is general that it can have this symbolic form Thus it is the exact equivalent of the supposed major premiss from which it is pretended to be deduced. It may here be remarked that the theory of the so-called *dictum de omni et nullo* being an axiom of the syllogism or a canon of syllogistic reasoning is only an amusing instance of this same fallacy This dictum is nothing but the syllogistic figure of which it is supposed to be the axiom written down in ordinary words instead of being partly written in symbols. To return to our supposed reduction. If we like to give the symbols a more particular meaning, so that A_1 represents not A in general but a particular A, we shall get by the proposed reduction an infinite regress Thus let the argument be · $A_1 = B_1, B_1 = C_1$, therefore $A_1 = C_1$. The rule which has to be put as major premiss is, things which are equal to the same thing are equal to one another. Under this we subsume A_1 and C_1 are things equal to the same thing, and so draw the conclusion that they are equal to one another This is syllogism I. Now syllogism I, which is of the form MP, SM, SP, in its turn exemplifies another rule of inference which is the so-called *dictum de omni et nullo* This must now appear as a major premiss The resulting syllogism may be put variously; the following short form will serve. Every inference which obeys the dictum is correct, the inference of syllogism I obeys the dictum; therefore it is correct. This is a new syllogism (II) which again has for rule of inference the same dictum; hence a new syllogism (III) and so on *in saecula saeculorum.*

The foregoing is an indirect refutation, of the nature of *reductio ad absurdum*. A direct refutation may, however, be given as follows. In the above procedure the rule of inference is made a premiss and a particular inference is represented as deduced from it. But, as we have seen, that is an inversion of the true order of thought. The validity of the general rule of inference can only be apprehended in a particular inference. If we could not see the truth directly in the particular inference, we should never get the general rule at all. Thus it is impossible to deduce the particular inference from the general rule, and the so-called reduction is merely verbal with no corresponding process of thought.

It is of interest here to remark that there is a natural and justifiable way of speaking which promotes that confusion of thought which has suggested these verbal reductions to syllogistic form. Take, for example, $B_1 = C_1$, $A_1 = B_1$, therefore $A_1 = C_1$. If we are asked why $A_1 = C_1$, we may answer, because A_1 and C_1 are both equal to B_1, or because A_1 and C_1 are equal to the same thing. Now if a statement preceded by the word *because* is given as the reason for another, we tend to assume that if the reason is a true one the said statement must be a premiss from which the other is deduced. And this is in effect the doctrine of Aristotle. Yet we have seen that that is quite impossible in the particular examples we have examined. It is not true that the word because, however valid the reason it introduces, must introduce a premiss. What then does it precisely mean? In the given case we see that $A_1 = C_1$ in virtue of the fact that both are equal to the particular thing B_1. That is the first step in our thought. But we reflect further that the equality of A_1 and C_1, as apprehended, does not depend on anything else in them except that they both are equal to B_1, nor on anything in B_1 except that it is the one thing to which both are equal. Thus we see that the inference may be generalized and that we can say that things which are equal to the same thing are equal to one another. This therefore, as being the only essential, we truly call *the* reason, and it is rightly preceded by the word because. Yet, as our analysis has shown, we can only get this generalization by an act in which we recognize it in the particular instance, an act which cannot

possibly be preceded by the generalization as a premiss. We recognize then that the true reason of a fact which we apprehend is not necessarily a premiss from which we deduce the apprehension of it, and we arrive at the very converse of Aristotle's view that in the proof the fact must be deduced from its cause (αἴτιον) as premiss [1]

This is the result of one of those principles which we have maintained to be a necessary preliminary of any account of inference, the distinction, namely, between what a given premiss means and our apprehension of it. To sum up, such arguments as B = C, A = B, therefore A = C, or, most A is C, most A is B, therefore some B is C, or any other reasoning which proceeds by relating two terms to one and the same term, are, in this general symbolic form, on the same footing with respect to logical analysis as the syllogistic forms, such as M is P, S is M, therefore S is P, themselves. The analysis is as ultimate in the one case as in the other, and if the first kind requires further analysis, in the sense that the rule of inference is to be explicitly stated, so also does the second; the truth being that in either case it is nugatory, for it turns out to be a mere restatement of the argument itself In fact in every one of the inferences in question, the rule of inference is explicitly stated, requires therefore no further statement, the rule being precisely this statement.

§ 238. We have now to inquire what is the relation of the conclusion to either premiss of an inference taken by itself. We will consider generally the inference in which the terms, say, A and B are related to one another through their relation to the same thing M. A convenient symbolism is Br_1M, Ar_2M, $\therefore Ar_3B$, where the precise nature of r_3 depends on a knowledge of the relation r (the general form of the relation has the same main symbol r because the relations must obviously be of the same kind). The question how the conclusion is related to each premiss occurs in the ordinary logic in a special form which concerns the syllogism only, since it is the syllogism only which

[1] Aristotle never distinguishes the objective reason of a fact from the order of apprehension in our thought, he speaks as though the apprehension of the reason could precede the apprehension of the application to a particular case

is considered there. In the syllogism the form of argument is such that a special difficulty has been felt about the relation of the conclusion to one of the premisses, the major. It has been objected that the major premiss, all M is P, must include the conclusion all S is P as a part of what it states, inasmuch as S is part of M. Thus instead of proving that S is P, we seem simply to assume a wider statement, which includes it, and this statement is the major premiss. This is what is meant by saying that the syllogism is a *petitio principii*. Let us, however, consider the more general form, which includes the syllogism. If the act of thought which gives us Br_1M is not such as to show us the side of M in which A stands in the relation r to it, then this act of thought Br_1M certainly cannot yield the relation r_3 between A and B. It requires to be supplemented by the other premiss, Ar_2M, which shows us that aspect of M to which A is related, before a conclusion is possible. The same is true if we ask whether the conclusion is contained in the premiss Ar_3M. Thus the argument requires both premisses, which proves that the conclusion cannot be said to be contained in one alone. Once more we observe the importance of distinguishing between what a premiss means objectively and our apprehension of this

§ 239 Although the above reasoning includes the syllogism, we may consider the latter separately Let the major premiss be all M is B. This premiss may be got in one of two ways. There are cases where the group M is exhausted; here the predicate B may be attached to each member separately, giving the general statement all M is B. Consider now a member A of the group M. The act of thought by which B was attached to M in general was by hypothesis one whereby B was attached directly to A. Thus the condition of inference is not realized and there is no true syllogism. The supposed conclusion A is B is an *explicit* part of the judgement or opinion all M is B. But, in the first place, no one in such a case would seriously give A is B as the conclusion. These exhaustible groups are of little importance The universals studied by the sciences have infinite possibilities in the way of individuals, and even when, in the case of exhaustible groups, we have attached the predicate B to each member, we do not get the scientific judgement that

we require till we have found the common reason present in each member which causes its Bness: only then has *all* ceased to be an all of enumeration.

In a second group of cases all M is B may be arrived at on the ground that the universal nature of M necessitates B, and that in two ways. We may be able to see immediately that the nature of M, as such, independent of particular manifestations of it, necessitates B; that a three-sided rectilinear closed figure, for instance, must have three angles; or, again, this necessary connexion of M and B in their universal nature may be inferred as the consequence of some other fact.

But, secondly, all M is B may be based on a so-called inductive argument. For our present purpose this is in exactly the same position as the previous case, which may be called either the *a priori* intuition or the proof that Mness as such necessitates Bness. For though this statement is based upon particular instances of M, these instances are only of value because a comparison of them has led us to believe that B does not depend on their special and individual character, but upon the M which they have in common. In either case then the statement all M is B represents a connexion, known in the one case, believed probable in the other, between the universals Mness and Bness. Now this act of thought is clearly not an attachment of Bness separately to each individual, for, in the case of what may be called *a priori* connexion, no individual is considered as individual at all, while in inductive connexion the value of our induction depends upon our being able to apply Bness to cases which we have *not* considered, and this we do because we suppose the connexion to depend solely on the universal character of M however manifested. Thus, in the case of *a priori* intuition, the thought of M not containing all the species of M, we may require the minor premiss to inform us that there is such a species of it as A. In the second case the thought of Mness has not included every one of its realizations, and we may make abstraction of the particularity even of those we have observed. The connexion is between universals just as much as if we had never argued from any particulars. Thus when we apply a universal judgement or opinion got inductively to instances we have not observed, there is as before a true syllogism; we require in fact

a minor premiss to inform us that M is realized in the new instance.

§ 240. The conclusion is often said to be drawn from the premisses by an act of inference. This would seem to imply something more than the possession of the premisses, some operation in fact performed upon them. The operation may be described as, first, the combination of the premisses, and, secondly, the getting the conclusion from the premisses. What is in the first place clear is that there is an immediate connexion between the fact represented by the conclusion and the fact represented by the premisses taken together. There is, then, no reason why the premisses necessitate the conclusion, for they are themselves the reason. To put it otherwise, we can interpose no link between the conclusion and the premisses. Thus, what is rightly called from one point of view a mediate inference (as when I proceed from B is C and A is B to A is C) is nevertheless essentially immediate as regards the connexion of the conclusion with the complex of the premisses; and, in this sense, all mediate inference is as immediate as that kind of inference to which the name immediate is usually confined. It is obvious that we must always have immediate necessitation. Suppose that A and C are connected mediately through some link B. This, we might say, necessitates that A is connected with B and B with C, and so, if all connexion were by links, we must interpose something again between A and B, and so on *ad infinitum*. We can state then the true character of the necessitation of which inference is the apprehension, as follows. 'one element of reality, whether simple or complex, immediately and of itself necessitates the existence of another element, the elements being different.' It is this ultimate fact which makes what is called a *synthetic* universal judgement possible, and we may add that we may call those universal synthetic judgements, which are not acquired by a train of inference in the ordinary sense but are self-evident, inferences on this account, because they simply mean the immediate necessitation by one thing of something different from itself. So far therefore they share the nature of inference; whether they are really inferences we may consider later.

Let us now consider the subjective side. Since the objective facts corresponding to the premisses necessitate the fact expressed by the conclusion, with nothing intervening, it might seem that

when we have both the premisses we must have the conclusion, and that there can be no place for an act of inference to draw the conclusion out of the premisses, given the premisses themselves. This does not agree with the familiar usage of language when we speak of *drawing* the conclusion from the premisses, and yet ordinary speech is, in an important sense, justified. The possession of the conclusion is certainly not merely having the premisses in their original form. The act of thought which gives us such a premiss as Mr_1B may not show us that side of M of which we apprehend Mr_2A (and similarly for the act of thought which gives Mr_2A) Merely, then, to have these premisses in their original form would no more conduce to the inference than if we had them alternately We must have something more. It would usually be said that we have to combine the premisses or put them together This is no help, these terms are mere metaphors, we have seen their insufficiency already in their application to processes of thought, when discussing the combination, or putting together, of ideas. We must look into the nature of the process itself to explain them. What is right, however, is the recognition that *some* act of unification is required. Aristotle recognized the necessity of the unification of the premisses; indeed it could hardly escape notice when logical reflection had begun at all; but he did not happen to realize the necessity of elucidating it, and this causes at least an appearance of contradiction between the *Prior Analytics* and the *Nicomachean Ethics*[1] In the *Analytics*[2] he quite clearly held that a man may have both premisses and yet not put them together, and so not have the conclusion There is something in this, but it is not quite correct, and it requires careful examination, which it did not get from Aristotle, and it is this which produced the apparent contradiction

Suppose the order of apprehension is Ar_1M, Mr_2B Since the

[1] In the *Nicomachean Ethics*, Book VII, ch 3, 10 1147^a 24 et seq, he assumes, throughout his discussion of the practical syllogism, that if you have two premisses you must have the conclusion. For his argument in the *Ethics*, the point of view of the *Analytics* would have suited admirably I am forbidden to take sweets, this is a sweet thing, the necessary conclusion is not drawn because it is in the interest of 'appetite' to refuse to combine the premisses

[2] *Prior Analytics*, II. 21, especially, *οὐδὲν δὲ κωλύει εἰδότα καὶ ὅτι τὸ* A *ὅλῳ τῷ* B *ὑπαρχει καὶ πάλιν τοῦτο τῷ* Γ, *οἰηθῆναι μὴ ὑπάρχειν τὸ* A *τῷ* Γ . . . *οὐ γὰρ ἐπίσταται ὅτι τὸ* A *τῷ* Γ, *μὴ συνθεωρῶν τὸ καθ ἑκάτερον* 67^a 33.

second act of thought does not include Ar_1M, that is, M in its relation to A, we may not in the second act of thought, which corresponds to Mr_2B, be thinking of M as in the relation r_1 to A at all, and, if that be so, no inference can result. We therefore have to remember that M, which is r_2B, was apprehended as it is in Ar_1M. But this is not the apprehension that corresponds to the first premiss; it is the memory that it was apprehended, which memory, however, is itself a way, though another way, of apprehending the original fact (Ar_1M). Now this act of memory need not necessarily supervene on the two premisses. It may be present at once with the second. The judgement therefore Mr_2B is modified in the sense that we now judge that M which appears in the second premiss as r_2B is that which stands in the relation r_1M to A in the first premiss; that is, we have the complex judgement Ar_1Mr_2B. The next step is that we see, because of our knowledge of the relation r, that Ar_1Mr_2B immediately necessitates Ar_3B. Now Ar_1Mr_2B may be considered as a modification of Mr_2B by the substitution for M as there apprehended, viz. as apprehended in the second premiss, of M as apprehended in this present premiss Ar_1Mr_2B Similarly, if we start from Mr_2B, we arrive by a similar substitution at the same Ar_1Mr_2B. Clearly then this substitution is what makes the reasoning possible. But how is such modification of the premiss justifiable? Ar_1M was true of M as presented in the act of apprehension which gave us that premiss How can we justify the substitution for M in that premiss of something different from M as there presented? And yet, if it is not different, what could be the use of the substitution? It would not be justified if M were confined in the first judgement to the particular nature of M referred to in that judgement. But it is both understood that M has an indefinite margin of unknown possibilities, and it is also understood that what is known in the first judgement is such that it cannot be affected, in the sense of being contradicted, by any further knowledge of the undetermined possibilities of M. The judgement Ar_1M then cannot have its truth impaired by any further information we may get about M as, for instance, Mr_2B, and this is the justification of the substitution.

The last consideration provides us with an adequate criticism of the view that truth lies [a] in the complete agreement and

[a] The reference is to Mr. Joachim's *The Nature of Truth*. Cf. §§ 117, 545]

mutual support of all items of experience, and therefore that the truth of anything which seems to be known is liable to be modified by future experience. The very nature of all inference is seen to contradict this doctrine. That this is so may be seen, in its simplest form, in any syllogism; and, shortly, the contradiction of this theory is a presupposition of any mediate inference whatsoever, however simple.

The foregoing is very simply vindicated in the case of the syllogism Suppose we start with the minor premiss, all A is M, and the act of thought by which we apprehend that premiss does not by hypothesis contain the apprehension that Mness involves Bness, or that all M is B. Suppose next that we apprehend this latter, the major premiss. We now substitute in the minor the information which we have about M in the major, so that A is M gives way to A is MB. This provides us with the so-called conclusion We may say that the original premiss A is M does not remain in consciousness in its original form, and this is due to the substitution for M, as known in the original form of this premiss, of M as known in the major premiss We have therefore substituted for something in the original statement a new form of M The question then is how to justify the substitution For if we only substitute something which, though verbally different, is quite identical with the original M as originally apprehended, it is futile It must therefore be something different to be of any use How then can it be justified? The reply is that, in the original proposition, our notice of M was certainly not confined to what we happened to be apprehending in it. We were aware that there was an indefinite margin of reality in M which we were not apprehending; and when we say that A is M, having a certain definitive apprehension of M before us, our thought implies that A has this definite character of M together with any other definite quality in the unknown margin of the reality of M. We obviously presuppose then, in this very simple syllogistic argument, that the truth that A is M cannot possibly be in any sense impaired by anything which we may subsequently learn about M, so that this simple presupposition of syllogistic argument absolutely contradicts the metaphysical theory of which we have spoken.

§ 241. It is part of the traditional theory of the syllogism that

Figures II and III can be reduced to Figure I. This goes back to Aristotle, who, in the *Prior Analytics*,[1] shows the validity of conclusions in these figures by such a reduction. But, as we have seen, we must not abstract a judgement from the judging of it. The conclusion therefore of a syllogism must not be regarded as a separate judgement, a result forming a judgement by itself apart from the premisses. The full judgement of the conclusion contains the judgements of the premisses and consequently must vary if those judgements vary. It is interesting to notice that this truth is in one passage [2] virtually recognized by Aristotle, who there insists that the definition of a property which he supposes to be arrived at by Figure I must contain the cause of the property This cause is the middle term of the syllogism and therefore, as he rightly observes, the definition is nothing but the syllogism itself written down in other words. Now this is, in effect, to recognize that the conclusion is properly speaking the whole syllogism [3] The result is that we cannot say that one argument is reduced to another so long as the premisses are different There will only be an appearance of reduction, in so far as the conclusions have the same verbal form. But the thought corresponding to the verbal form is really different, and it is easy to show, for instance, that the mood Darapti, in Figure III, which contains two universal affirmatives, cannot be reduced to Darii,[a] in Figure I

[1] *An Pr* i 7 [2] *Anal Post* ii 8 [3] § 220

[a] 'Darapti to Darii' Wilson nowhere explains what he means by this. Presumably he means that the minor premiss is converted to Y and not to I, cf § 216]

III

PRINCIPLES AND METHOD OF THE PURE DEMONSTRATIVE SCIENCES

§ 242 [a] It was long held from Aristotle onwards that the syllogism was the method of advance in geometry and mathematics generally. These sciences were supposed to start from certain general principles which they do not themselves demonstrate, principles arrived at by intuition or induction, and the progress from these principles was supposed to be made by syllogistic inference. This theory is the direct consequence of that *a priori* theory of demonstration which we have described. It is owing to such a view that we have at the beginning of the various books of Euclid's *Elements* a collection of definitions, axioms, and postulates, for these represent the effort to collect all the supposed starting principles or assumptions. We might suspect this theory on the ground that the advance in science would then rest on no specific knowledge of the matter of the science, beyond what was contained in the prefixed collection of principles. It would depend solely on the use of syllogistic forms supposed to be common to all demonstration. But this we know is directly contrary to the fact. Again, the supposed assumptions being taken to be limited in number, their combinations in an argument must be limited and so the conclusions must be limited in number. In consequence geometry should have been exhausted long ago. The fact seems to be that those

[a Wilson assumes throughout that Euclid develops his geometry syllogistically. Thus he speaks repeatedly as if the axioms which Euclid quotes in his propositions were intended to be premisses. This is a curious mistake. It was much later that Euclid's *Elements* were attempted to be set out syllogistically. An example is given in Mansel's edition of Aldrich, *Artis Logicae Rudimenta*, Appendix I. Generally Wilson regarded his own view as derived from Kant (e.g. *Kritik*, 1st ed., pp. 707 et seq.). It is in fact more like Schleiermacher's (see e.g. *Dialogues*, § 327, quoted by Ueberweg, *Logic*, § 101). Leibniz, *Meditationes de cognitione* (pp. 540–1), puts the other view, 'non contemnenda veritatis enunciationum criteria sunt regulae communis logicae, quibus etiam Geometrae utuntur. firma autem demonstratio est quae praescriptam a logica formam servat.']

who hold this doctrine of demonstration have not accurately realized what their own doctrine should mean. It ought to mean that the original principles or premisses being granted, any further conclusions are got by combining them into syllogisms. That would be the whole operation of thought in the way of discovery and inference. We may add accordingly that this is refuted by a very simple experiment, for if we take the definitions, postulates, and axioms of Euclid and try to operate upon them with the syllogistic forms, we shall make no discovery at all; we shall not get *one* of Euclid's theorems; we shall get no conclusion whatever worth having.

§ 243. If we examine any discovery in geometry and note down everything that is necessary, we shall find that we can do nothing at all unless we first draw a figure. The process advances by making new constructions, the validity of such constructions is a matter of immediate spatial intuition, and so also are the consequences inferred from these constructions in the way of other geometrical relations which they necessitate. For the discovery of the right construction there cannot be any rule, it depends upon the manner in which the particular case suggests the steps we have to take. Thus the finding of the construction is the real difficulty in the discovery, and there is a great difference between one mind and another in the capacity of seeing what the problem suggests. When the construction is found the problem is solved and the proof also is complete, we do not need to add to it a chain of argument such as we find in Euclid, for though we can thus explain to a learner the connexion of the premisses and the validity of the conclusion, the best way is to retrace the process of discovery. Further, such procedure as that in Euclid may sometimes not show at all how the construction is found. For instance, though in Pythagoras' theorem (*Elements*, I. 47) we see by means of the argument both that the conclusion is justified and the use of the construction, the real process by which thought advanced in the discovery of this construction is kept out of sight. The process of discovery then is not an analysis of propositions already given us; it does not consist in putting into syllogistic form any given material of premisses. We add to the given material by the constructive acts, through which we apprehend

something other than the material, that is, something which that material necessitates. We do this in geometry by constructing new spatial relations the validity of which we see in the act of construction, and thus we apprehend the universal in the particular.

Now, as these constructions are in the nature of the case particular, how is the result universal? The answer lies in the nature of our mathematical faculty itself In constructing the particular we see immediately the universal validity of our construction. The main thought here, notwithstanding differences of exposition, is due to Kant [1] It seems clear that Kant thought the method confined to mathematics, but that is not really so: it is universally true that we can only apprehend the relation of universals in apprehending the corresponding particulars. Though it may be difficult to understand this theory at first owing to preconceptions derived from current theories of knowledge, it is nevertheless the fact, and can be clearly verified in any actual demonstration in which we see that we nowhere use the particularity of the figure as such in our argument. The ultimate fact therefore is that in geometry and mathematics generally we are able to see the universal in the particular and so to arrive at a universal judgement. It is this transcendence of the particular, though the particular must be used, which makes mathematical method what is called *a priori*. In the empirical sciences, on the other hand, we cannot thus see the universal in the particular, in other words, given the connexion of two elements in a particular experience we do not see the universal connexion, that is, we do not apprehend the reason.

§ 244. A kind of paradox remains to be considered: the figure on which everything depends may be imperfect and not an adequate realization of the universal. It is on this account that students of the subject have so often failed to recognize the importance of the figure and have tended to suppose that the reality of the process lay somehow in pure thought as opposed to any particular and imperfect constructions. It is this fallacy which is mainly responsible for the chimera of non-Euclidean space. The writers who maintain this do not realize the quite

[1] *Kritik d r Vernunft* [1st ed., p 712; 2nd ed., p 740]. There is an anticipation, however, of the truth in Aristotle, *Metaphysics*, 1051[a] 22.

fatal objection which lies in the impossibility of any constructions to correspond to such space. This is because they do not realize the place of the figure in Euclidean geometry. They do not even suspect that it is precisely the construction of the figure which is the true process of the advance of thought in geometrical discovery. In truth a thing cannot be 'thinkable but not representable'. The paradox we are now considering, the possible imperfection of the figure, cannot really be removed by supposing that we *imagine* a perfect figure. A figure in experience might be in itself perfect. A falling rain-drop, for instance, might be a perfect sphere or an accurate ellipsoid, yet we should have no means of ascertaining that it was perfect, and so its perfection would not be experienced by us. Now there is exactly the same difficulty about any figure in our imagination; we may want it to be accurate but we can no more be sure that it is so than if it were actually found in our experience. We certainly must think of a perfect figure, for on that our proof depends, but that is not the same thing as saying that the figure in our imagination is perfect; we apprehend, that is to say, a particular, and in some sense we apprehend the perfect particular. That apprehension is not an experience and not an imagination, but, nevertheless, it is an apprehension of what the nature of the particular must be. This is undoubtedly true, and it remains to attempt to characterize this kind of apprehension. We seem to arrive at a new and important distinction in our faculty of apprehension

We must clearly apprehend the nature of the perfect individual figure, for on that our proof depends. But the apprehension, as we have seen, is not an experience, because though a particular given figure may indeed be perfect we cannot apprehend it as such with certainty in experience. Nor is it the apprehension of an imagined particular, and for the same reason. It is an apprehension which we have with such experience and with such imagination. This peculiar kind of apprehension does not appear to have been recognized in any theory of knowledge or of inference. The fact is that the difficulty above described has not been recognized and the solution of it has therefore not been suggested.

§ 245. It may be objected that in actual geometry the axioms

do appear as major premisses and that although we must admit that the process of discovery is not syllogistic, in the strict sense, yet when the theorem has been found and the connexion is presented as something to be understood, it consists of syllogisms as in the ordinary Euclid. We must distinguish two kinds of major premisses. Axiomatic premisses, which are not demonstrated, including definitions, and premisses which are themselves products of demonstration.

To take the former first. Euclid shows (*Elements*, i. 1) that, in the triangle ACB, AC and BC are equal, each of them, to AB, and being equal to the same thing are equal to one another, because of the axiom. Here the axiom appears in the last step as the major premiss. This we have explained already. The truth of the axiom is only apprehended in the individual instances which we here pretend to derive from it. In the case before us we see the truth directly in the particular instance or we should never see it at all, and therefore to represent it as deduced from the axiom is an inversion of the actual order of thought. We recognize, however, that the truth we see has a universal character, not depending on the particular magnitudes, and this recognition of the universal is precisely the axiom itself and follows *after* that particular act of thought which is wrongly represented in the syllogistic process as derived from it.

§ 246. We do, however, find in geometry cases where there seems to be a real syllogistic process; where, as it would be said, the result of a previous proof is applied to a particular instance Consider the proof (*Elements*, iii. 31) that the triangle, of which the base is the diameter of a circle and the vertex a point on the circumference, has a right angle at the vertex. If this theorem is subsumed as a minor premiss under the truth of Pythagoras' theorem as a major premiss, we infer the same property for this triangle. This may be put thus. If two straight lines are drawn from a point in the circumference of a circle to the extremities of a diameter, the sum of the squares on the two lines is equal to the square on the diameter. The act of thought, viz. the whole proof, which here gives us the minor premiss, does not give us the major, we don't thereby prove Pythagoras' theorem. Again, the act of thought which gives us the major premiss does not give us the minor, does

not, that is, prove *Elements*, iii. 31.[1] The act of thought therefore which gives either premiss does not give the other and thus a condition of valid syllogistic inference seems to be satisfied. Here, then, there seems to be a syllogistic process. It is of the simplest character, an operation found in all departments of thinking, causing no difficulty, and not depending, be it observed, on any knowledge of the special matter of geometry. On the other hand, what is most valuable to the progress of the particular science as such is not this mere obvious subsumption of the particular under the universal, but the process by which we discover the universal in the particular which is subsumed under it. That depends not on the syllogism but on construction with apprehension. Now when we examine more carefully the nature of the major premiss, we shall find that in the actual process of thinking whereby we know it fully and do not merely remember the result of the demonstration, in other words, in the full apprehension of the judgement represented by the major premiss, the syllogistic subsumption disappears. Strictly the major premiss, if the judgement expressed in that premiss is properly judged, can only be applied by our reproducing the whole proof. If we do this, we find that in the case of the triangle in the semicircle we first demonstrate by construction that the vertical angle is a right angle and then go on by a system of constructions (which is nothing but the proof of Pythagoras' theorem) to prove that the square on the base is equal to the sum of the squares on the sides In this complicated geometrical proposition, just as much as in any of the more primitive ones, provided every judgement used is treated in the same way, we shall find the whole process of thought consisting of these constructions with the corresponding apprehensions. We shall find no process of advance by syllogism here any more than there. The syllogism also itself will again have the same explanation, as arising from the recognition of the universal character of our constructions and their consequences.

§ 247. If, then, instead of conducting the proof in the particular case that A is B and the proof in the particular case that B is C, we merely remember either result or both, and so do not fully apprehend the facts corresponding to the premisses, we

[1] The angle in a semicircle is a right angle.

apparently have the syllogistic form, all B is C, all A is B, therefore all A is C; and that is where the syllogistic form would seem to come explicitly into geometry. But we must not suppose that the syllogistic form is related only to imperfect apprehension and due only to that. It corresponds to something in the complete proof, that is, in the complete act of apprehension. Consider the complete proof when the syllogistic form does not appear. In the case of a particular triangle in a semicircle represented by the figure before me, where ABC represents the triangle itself, I apprehend by Euclid's proof that the triangle must have a right angle at the point represented by B. Then adding the construction of Pythagoras' theorem, I apprehend by Euclid's proof that the right angle at B necessitates that $AC^2 = AB^2 + BC^2$. I thus apprehend that the particular triangle in the particular semicircle has this relation between the squares on its sides. But now the process is seen not to depend upon the particularity of the figure, and thus we apprehend the universal, namely, that any triangle in a semicircle has a right angle at the vertex and, because of that, the relation between the squares on its sides The verbal expression of the generalization which we have thus apprehended in the consideration of the particular is that any such triangle in a semicircle has a right angle; any triangle which has a right angle has the Pythagorean property; therefore any triangle in a semicircle has the aforesaid property Thus in the properly complete apprehension we have this syllogism, not, however, as the process by means of which we have discovered and inferred, but as something the truth of which has only been fully apprehended in the precedent apprehensions concerned with the particular figure In other words we only apprehend this syllogism fully by the process in question. Now suppose I have neither proof before me. I remember that I proved that the triangle in a semicircle has a right angle, and I remember that I proved that a right-angled triangle has the Pythagorean property. Even if it is objected that memory is not trustworthy, I know at least that if the facts are as I remember them, they necessitate that any triangle in a semicircle has the property in question Thus I have a true hypothetical judgement. But it would be affectation to say that there are no cases in which

I do not absolutely trust my memory. I know, for instance, that it was proved that the triangle in a semicircle has a right angle; then the facts represented by the premisses are known; each is apprehended, the apprehension consisting in a memory that it was proved. But clearly the knowledge of the syllogism is incomplete as compared with the knowledge of it in the complete apprehension above described and depends in the last resort for its validity on the fact that we can replace the memory of the proof by the proof itself. We thus see how the possession of the syllogism in the one case differs from the possession of it in the other, although the difference does not appear in the verbal expression. It follows, then, that the syllogism is not due merely to defective apprehension; it exists anyhow but is only fully comprehended in the case where we have the proofs of which it is the generalization.

§ 248. The foregoing is not confined to geometry or mathematics, but holds of any syllogism in which one or more of the premisses is a universal proposition, the truth of which is known. For such a universal, if really known and not merely probable and based on inductive evidence, can only be apprehended in the particular; that is, we can only apprehend that all A is B by apprehending in an instance A, that A is B as being A. This dependence then upon the particular is not confined to mathematics as Kant seems to have supposed.[a]

The conclusion to which we are led may be summed up as follows. The true syllogistic process in reasoning being the possession of a major premiss followed or preceded by a minor which is not included in it explicitly; the apprehension of the minor again not including that of the major, and a conclusion following which is necessitated for our thought by the possession of the premisses, we never have such a process in mathematical science, unless the major premiss is possessed, not as the apprehension of the facts stated in it, but as the memory merely that it has been apprehended. Thus the syllogistic form as a process

[a 'Nur die Mathematik nicht aus Begriffen, sondern der Konstruction derselben . ihr Erkenntniss ableitet . . Das philosophische Erkenntniss das Allgemeine jederzeit *in abstracto* (durch Begriffe) betrachten muss, indessen dass Mathematik das Allgemeine *in concreto* (in der einzelnen Anschauung) und doch durch reine Vorstellung *a priori* erwägen kann.' l c, p 734 (762, 2nd ed)]

of thinking is followed in mathematics only when knowledge in the most complete form is not present, that is, when the facts stated in the major premiss are not apprehended in the full and strict sense.

Such a syllogism is not complete in itself, because it depends on the absent proof of the major premiss. Thus such a syllogism could not be an absolute starting-point in knowledge. Now it can easily be shown that this must always be so in the case of universal statements in matters of knowledge, not merely in mathematics, as Kant seems to have supposed. Obviously the only case we have to consider is that in which the major premiss is not demonstrated. (Induction is excluded because it cannot give knowledge but only a high degree of probability. Even if it could give knowledge, that knowledge would be demonstrative knowledge and the syllogism would depend upon it for its major premiss.) The major then in question must be self-evident. But the universal fact represented by the universal statement can only be matter of apprehension in a particular, for we cannot possibly apprehend the nature of any universal except by thinking of a particular in which it is manifested. Now if the universal is self-evident, it is seen in any particular whatsoever, and therefore our apprehension of the nature of any of its particulars involves that of the universal. It cannot then be preceded by the apprehension of the universal so as to be subsumed under it. If, for instance, the universal is all B is C, and the minor all A is B, the A's are particular instances of B; the apprehension then of any of them involves the apprehension that A as being B is C. It is only after reflection on this that we see that the thing's being C depended only on its being B; which is just what produces the universal statement represented by the major premiss.

It is well to add here for clearness that the formula 'seeing the universal in the particular' means that in the case, for example, of a particular A, though I am obliged to think of a particular, I apprehend that it is B only as being A, or only because it is A, and not through anything particular or confined to itself.

§ 249. The validity of the general form of the syllogism, that is, of the syllogistic rule, is like any rule of inference recognized

in any particular syllogism, and the particular syllogism is, as we have seen, not deduced from it. The question then arises whether every particular syllogism is not itself similarly the generalization of a particular and the apprehension of a universal in a particular. Suppose that in the syllogism B is C, A is B, therefore A is C, the statements BC and AB are full apprehensions; we have then a truly known universal and we can only apprehend, as was said above, the universal all A is C because all B is C and all A is B, by considering a particular. The only question is how this is done. Take the particular A_1. I apprehend that A_1 as A must be B, and so is B_1 (as in the geometrical example). Again, to get the universal 'all B is C' when I can really have such a statement as certain, I apprehend in B_1 that as B it must be C. thus I apprehend A_1 as being B because it is A, and as being C because it is B; but to apprehend A_1 as B because it is A, is to apprehend A_1 to be B not as a particular but only through its universal character A. Thus we apprehend that all A is B or any A is B. The above apprehension then is apprehending any A to be C because any A is B and any B is C, which is the syllogism, and the syllogism therefore is a universal apprehended thus in a particular.

Next, suppose we only remember that all A is B was proved, that is, was apprehended in a particular instance, and so for all B is C It is not enough to say I think of a particular A, and imagine it B, for that is not the judgement any A must be B which is required for the syllogism I remember that A_1 as A was shown to be B. This is an apprehension through memory that A_1 as A is B (viz. B_1) Similarly we have an apprehension through memory that B_1 as B is C Thus though the nature of the apprehension is different, the general form is the same as in the preceding case and can be treated in the same way. Thus we again have the syllogism as a universal apprehended in a particular case. Lastly, suppose I am uncertain whether A is B and whether B is C, yet I know that if A is B and if B is C, then A is C. I think of a particular A, viz. A_1. Now this means that I apprehend the nature of an A as particular, while imagining a particular (if I am not experiencing or remembering an actual particular), and I apprehend the fact that if a particular A, as A_1, were B, and if as

B it were C, A_1 would be C, which is apprehending also that any A, if it were B, would be C, if all B is C.

§ 250. We can now determine more precisely the nature of what is called the application of the universal to the particular, a phrase by which the syllogism is sometimes characterized, and whether or not this is ever an inference. We shall be able at the same time to see what is the ground and excuse for the difficulty made about a *petitio principii* in the major premiss. Suppose, as in the example given from Euclid, we prove in the instance B_1 that all B is C, which is equivalent to saying that we apprehend that B_1 is C because it is B. Suppose we apply this, let us say, to a new instance B_2 If now in so doing we fully apprehend that all B is C, that means we apprehend that B_2 as B is C, and this will be a repetition of the former proof. Here there is no inference that this B (= B_2) is C from all B is C. But now suppose we do not thus prove that B is C but remember that we proved it; we might express the process thus all B was proved to be C, *therefore* this B (= B_2) is C. This reasoning has the verbal form of an inference. In proving that B was C we proved really that any particular B was C, for we found a particular B_1 was C, only because it was B, and in this we recognized that any B was C. Thus the recognition that this B (= B_2) is C, because it is B, is no new act of apprehension but is truly comprised in the previous apprehension. Now this is the true (though not understood) ground of the feeling that the major premiss really contains the conclusion, for however the judgement all B is C is got, it is felt to mean that any B whatever, as B, is C. Now the feeling would be right if B_2 was apprehended merely as a particularization of B, but this is not the case. B_2 is apprehended as a particularization of another universal A, so that B_2 is also A_2, and A_2 in fact is identical with B_2. The recognition of that is the minor premiss. Now the apprehension that any B, as B, is C, represented by the major premiss all B is C, did not in any way contain the apprehension that any B is A, and so not the apprehension that A_2 is B, and consequently not the apprehension that A_2, as B, is C, which is the conclusion. We thus come back to our former solution.

§ 251. The first principles of geometry are usually divided

into definitions, axioms, and postulates; the last appearing as practical rules stating what constructions are allowed. Properly speaking, however, the science of geometry as such is not concerned with the practical construction of geometrical figures.[1] The real value of the postulates is that they represent an imperfect recognition of the necessity of constructions, that is, of particular figures, either in experience or in imagination. They should then be a classification of various kinds of theoretic construction. The classification as given is by no means exhaustive. It does not even exhaust the constructions in the *Elements* of Euclid; nor is this surprising, since the classification was made without any consciousness of its real significance. But, though it might have been exhaustive of what is actually found in the *Elements*, it could not possibly be complete for geometrical science. The various kinds of construction only become known in the solution of various problems and they cannot possibly be anticipated beforehand Thus there is no provision in Euclid's *Elements* for the various familiar constructions of conic sections.

§ 252. Whether in geometry or elsewhere, axioms are usually regarded either as simple intuitive truths and contrasted as such with truths which are inferred; or as not intuitive indeed but the result of an inductive process. This second view is artificial, arising solely in the interest of a given theory. Even in the second alternative, however, they are still considered to be simple by contrast with the complexity of demonstrated truths and not inferred by a process of demonstration strictly so called. We shall maintain that the axioms are, on the one hand, not the result of induction, and, on the other, are, though certainly intuitive, not to be distinguished from demonstrations as these are usually regarded The demonstrations themselves are all intuitively apprehended, and if they are got by some constructive process, so are the axioms Every axiom is synthetic and not a mere identity Thus it contains different elements whose connexion is apprehended, and this apprehension takes place in a construction. We must, that is, have a particular figure imagined or perceived, and the universal validity of the axiom is seen in the particular construction

[1] Vide Plato, *Republic*, Bk. VI, 510 D.

because we there see that the connexion apprehended is independent of the particularity of the instance chosen. But this is exactly the process that we have described already for demonstration The construction for an axiom is simple; in demonstration it is more complicated. The usual distinction then between axioms and demonstrations is quite unreal; the act of thought in which we apprehend an axiom or a demonstration is exactly the same in kind in both cases. Again in demonstration, the successive constructions are all intuitive and may all be termed *axiomatic*; the inference from one end of a series of constructions to another is, when generalized, an axiom Its meaning, however, is recognized only in these very acts of construction .

§ 253 In Euclid we find two kinds of definition. Both refer to reality, but one kind we may call nominal as in some sense giving the meaning of a name without the construction necessary to decide whether any real object corresponds to it or not. Thus the definitions of obtuse-, acute-, and right-angled triangles are at first nominal, being only justified when Euclid has proved that the three interior angles of a triangle are together equal to two right angles This restricts the number of right or obtuse angles to one and allows a triangle to contain three acute angles. Thus definition is of no use till it is justified by construction, in other words, by demonstration There is a second kind which is not nominal, for example, the definitions of a triangle or a circle, because we see the possibility of those figures at once, and we may think we do this before conducting a process of inference, such as is necessary in the former kinds of definition. But the truth is that we see this possibility only by a particular construction, which assures us in the case of a triangle, say, that space can be enclosed by three and not by two straight lines. So for the definition of a circle We see that the revolving-point, at a fixed radius, must return to the same position and so form a closed figure It is the simplicity of these constructions which causes them to be overlooked, and thus the false idea has arisen that a definition is merely the description of a 'conception' (an expression which will hardly bear examination) and distinct from the acts of thought which involve proof. The necessity for a constructive process becomes obvious in the

definitions which we have called nominal, and this helps us to see that there is construction involved in the simpler cases also.

§ 254. In the empirical as compared with the mathematical sciences we observe a very important difference in the position of definition. In an empirical science we cannot start from a definition and advance from it as we appear to do in mathematics. The difference cannot be accounted for by the ordinary theory of definition, which implies more or less unconsciously that definition only describes the content of conceptions; a view which depends on a false antithesis, a wrong distinction between objects and conceptions As to this antithesis it is supposed, for example, that within limits we can make conceptions at will and so make definitions It is admitted that there are elements which we cannot make in thought but must accept from experience; given these, we are still supposed to have a freedom of combination. Definition thus gets a subjective arbitrary character and cannot be treated as something from which we can derive information about reality If this were indeed so, we could not understand the position of definition in mathematics. This conceptualistic view depends upon a misunderstanding which may be illustrated from Mill's *Logic*. Mill thinks, as we have seen, that we can make a definition of a dragon, that this like all definitions is merely nominal, that it only expresses the meaning of a name, which he sometimes appears to distinguish even from an idea. It turns out, however, that there is something behind the name. He means that to 'dragon' corresponds an idea or conception so independent of reality that we do not believe that a corresponding object can exist. He then seems driven to explain why the procedure of mathematics results in no mere imaginary theorems, by supposing that to a mathematical definition is added a postulate of the reality of its object. But, more than this, Mill's expressions would commit him to the view not merely that no deduction 'affecting matters of fact' is possible from the definition but that no deduction at all is possible from the definition unless the postulate is added.[1] That he does not realize this is probably due to the strange confusion by which he distinguishes the view of defini-

[1] Mill, *A System of Logic*, I. viii, §§ 5–6.

tion as a 'statement and analysis of the mere meaning of a word' from definition as 'the statement and analysis of the content of an idea'. What from his point of view could 'meaning' be but idea? In the second book, where he is discussing mathematical demonstrations, he has to assume that deduction can be made from the content of the idea itself apart from the postulate.[1] The former doctrine, however, is obviously absurd, for clearly the possibility of deducing anything from a conception lies in the 'content' of the conception itself, and, supposing the content (as is presupposed) were something different from reality, the postulate of existence could in no way facilitate the process of deduction The only effect it could have would be that, while the inferential process proceeded from a conception, exactly as if no postulate had been made, we should merely add at the end of every deduced conception the words 'and this conception corresponds to a real object' The existence therefore of a postulate would not explain the peculiar process of mathematics. Mill's account is a part of the old mistake that mathematical inference is merely formal, consisting of syllogistic deduction from given premisses. It is amusing to observe that Mill thus involves himself in a characteristic contradiction, for in the next chapter, in order to meet some difficulty about mathematics, he maintains that real objects do not correspond to mathematical definitions, so that the postulate after all is false and mere hypothesis.[2]

§ 255. We have seen that in the case of the definition of a dragon it has been maintained that we can have an imaginary conception to which no real object corresponds. To that we answer first that these conceptions turn out on analysis not to be conceptions at all in the strict sense They have elements supposed to correspond to parts of reality and in thinking of them we violate no rule necessary to reality. The dragon's body, for instance, as extended in space is never thought of as violating a geometrical principle. But the combination of these elements (living tissue and fire) is not really performed in thought and it is quite untrue that we have combined or can combine in

[1] Id., II v, §§ 1–2

[2] It is true that he tries (unsuccessfully) to pave the way for this by a change of front in Book I, VIII, § 6.

thought what we know or believe cannot be combined in reality.

Secondly, if the theory is put in the form that we can conceive objects which we *know* cannot exist, it is easy to show that this is self-contradictory. We can only know that these elements are not combinable by our knowledge of what these elements are, that is to say, by our 'conceptions' of them. It is the conceptions, therefore, which have this incompatibility, and that is equivalent to saying that we cannot combine these elements in conception.

The question also is easily settled as a corollary of the account already given of the meaning of conception as properly apprehension of reality.[1]

§ 256. In empirical science there appear to be two courses open to us, in the matter of definition, if we assume certain elements as apprehended in experience.

We might define an object either as combining certain of these elements, without referring to experience to see whether the combination occurred, or as combining elements which we have observed combined. In the first alternative there could be no security that something real corresponded to our definition. Yet, if a not uncommon view of definition in mathematics were right, we could see no reason why we should not construct a body of hypothetical physical science with the same precision as in geometry. But we know that this is not possible The second alternative, where we at least know that a real object corresponds, seems therefore preferable But though this is real knowledge, it would not enable us any better to draw conclusions from the definitions The reason is one which is common to both definitions. As we have seen, a conception is not completed when we have stated the elements of it and added merely the demand that they are to be combined. It is obviously not completed until we know that they *are* combined. This is the position in empirical definition. Even when we apprehend in experience certain elements, we do not see how they are combined. In geometry, on the other hand, we not only apprehend the elements but also how they are put together; we understand the nature of their unity and we realize it to ourselves in an act

[1] Part II, ch 14, especially p. 315.

of construction in which the unity of the elements is as much apprehended as the elements themselves. We know exactly how they go together Now this is just what we cannot do with the elements of an empirical definition. There is something in language which corresponds to this. In a statement of what an empirical object is, we find attributes in general only unified by their reference to the same subject; they appear as so many separate adjectives, each attached independently to a substantive—'this body is hard *and* round *and* sweet', &c. In a mathematical definition, on the other hand, the words form an organic grammatical unity and not a mere aggregate of elements joined by 'and'. We do not say a triangle has threefoldness and sides and is rectilinear and encloses a space. Knowing how these elements are put together, we express their unity thus: a triangle is a three-sided rectilinear closed figure, or (better), a triangle has three rectilinear sides which enclose a space.[1]

§ 257.[a] The usual theory [2] that the exact sciences start from definitions of their objects, assigning them certain attributes, and then derive other facts about the objects from these definitions, involves a sharp distinction between the attributes comprised in the definition, and those 'derived' from them by demonstration. This distinction is connected with a technical distinction of essence from property, essence being the attributes constituting the definition A further distinction is to be made; not everything which is necessitated by the essence is property in the strict technical sense. A property is a necessary attribute which is derived from the essence alone and not from anything else. Thus, in triangle, that the interior angles are together equal to two right angles is a property, because it is derived from nothing else than the essence in the definition. But that the exterior angles formed by producing the sides successively in the same direction are equal to two right angles, though necessary to a triangle, is not a property, because it follows from something less than the whole essence and is true of every rectilinear

[1] Cf § 259 ad fin

[2] Cf. §§ 173-4

[a] On Aristotle's doctrine, see Mr Joachim's *Aristotle on Coming-to-be and Passing-away*, Introduction]

figure of a certain kind (viz. without re-entrant angles). The distinction goes back at least to Aristotle. For him property [1] as something both necessary to the subject of the essence [2] and peculiar to it is distinguished from essence, but he nowhere explains how it is that of two kinds of attributes, both necessary and both peculiar, one kind should be called essence. We may, however, derive an Aristotelian definition from the actual use which he makes of the distinction. What is everywhere presupposed in the *Posterior Analytics* comes to this · the essence is a group of attributes of the thing defined, such that no one of them is derivable from any other or others, while everything else necessary to the thing is derivable from this essence. In that treatise the terms [3] for these derivatives which are properties suggest an existence in some way derivative and dependent. The distinction was very likely suggested by geometrical science, and may seem to accord with the procedure of Euclid; yet the conception, if we examine it, seems almost paradoxical. For how can we know the essence except by knowing that it accounts for everything in the object? And how can that be done unless we know everything in the object and see that the essence accounts for everything? Such knowledge is clearly impossible The answer might seem to be that we may at any rate approximate to the essence. We may divide what we know of a thing into essence and property, essence being the group of attributes which we cannot derive, and from which we can derive the rest of what we know. Essence then would be relative to the amount of our knowledge, increasing when we found new attributes which we could not derive from our original essence, and decreasing when, through improvement in our knowledge, we found some part of the supposed essence to be really derivable from some other part. A perfect definition of essence, then, would never be attained; essence would in fact be an ideal to which we are always trying to approximate, and that fairly agrees with the actual condition of the empirical sciences

§ 258. This answer to the difficulty does not, however, suit the mathematical essence That is not provisional; we are sure

[1] ἴδιον [2] οὐσία.

[3] συμβεβηκὸς καθ' αὑτό, πάθος or πάθημα καθ' αὑτό.

that it entirely covers the object to which it relates, and yet we could never verify our belief by seeing that it does in fact account for all the other attributes, for these are infinite in number. Again in mathematics we appear to get our knowledge of the properties from the essence instead of depending on *a priori* knowledge of the properties to determine the essence.

Now how can we be sure that the definition does cover everything? The answer, though in a way decisive, seems at first a mere tautology When in geometry we define a given figure we mean by the object of the definition, an object which has only the attributes assigned in the definition, and those which necessarily follow from them. This account is obviously confirmed by geometry itself, and in this way the sufficiency of the essence is guaranteed from the first. Yet it seems to involve a very great difficulty. We naturally ask why we should not secure the same completeness in physical science by stating that the object defined is only what is determined by the definition. Now sometimes this could not be done, the essence of gold, for instance, is something given to us as a problem; it is not constituted by, or apprehended in, our construction. We may answer, however, that in geometry we are not concerned with the total reality of an individual thing as we are when studying gold, but only with a part of it, in other words, with its shape. But in physical science we cannot even make a definition of an object, that is, a part of reality, which does not embrace the whole reality of the object. We do not know, given the elements, how they are combined In geometry we can take any elements whatever, the unity of which in a whole we can apprehend in a construction. Whenever this can be done in an immediate intuition realized in a construction we have an object which is a unity of different elements and have thus completely determined an object, not necessarily the total reality of an individual thing as in physics, but still an object. Such essence is an aggregate of elements seen to cohere directly in a unity, without the intervention of connecting links We must here define more precisely what this coherence means. Suppose the elements A and B in this unity necessitate C: we should not include C in the definition, though A, B, and C cohere. What we do include

are the elements which we can see may form a unity but do not necessitate one another, for example, a three-sided rectilinear figure. Threefoldness does not necessitate threefoldness of sides; three sides do not necessitate that space should be enclosed, and a three-sided closed figure is not necessarily rectilinear. Now such a construction leaves the elements indeterminate in some directions. In the definition of a triangle we see the coherence of the elements in the unity of one object without determining the magnitude of the sum of the interior angles, though they must have some determinate magnitude. So again we leave indeterminate the relations of the sides as depending upon the relations of the angles, though these again are in themselves of course determinate. This indeterminateness, however, and the ignorance which it implies does not prevent us from apprehending the determinate character of the unity of the elements so far as we have apprehended them. We see, for instance, that space can be enclosed by three straight lines, though we are as yet ignorant of the properties just referred to. Now what are called the properties of a figure as opposed to its definition or essence are simply the further determinations of the parts left indeterminate in the combination represented by the definition, which determinations are conditioned by the possibility of further determination in each element taken by itself and by the nature of the unity in which they are put. These new determinations are discovered by that process of construction which we have described and which corresponds to what is usually called the demonstration.

§ 259. The idea of essence as the undemonstrated from which all else in the object can be demonstrated conveys the impression that the essence is something absolute. In Aristotle the distinction *is* absolute, yet we find that different definitions may be given of the same geometrical figure, for example, of the ellipse. Each being an independent starting-point for the investigation of the whole figure, and each demonstrable from any other, property becomes essence and conversely. Now this can be explained from the account already given of geometrical definition. The definition is nothing but the apprehension of the unity of certain elements, the point being that we really do apprehend this unity and do not merely suppose it to exist. This

we may put subjectively as follows: it is the conception made by combining certain elements and we really do combine them. This subjective expression seems to answer to the act of construction but is misleading; it is not true that we make the combination, we apprehend or know the nature of the combination in the empirical act of drawing the figure. Thus the elements and their combination are in no sense arbitrary, but necessary. The further procedure is simply the apprehension of further connexions by further construction Now, if in this process of demonstration or construction we come to another group of attributes of such a kind that we can start from that also (consisting, that is, of elements whose unity is a matter of direct intuition and therefore independent really of the process by which we have arrived at it from the previous construction), this second group will form a definition just as much as the original group. Further, if the second group of attributes can be shown to condition the first, if, that is, we can not only start from the second group as a first principle but by a constructive process derive the first group from it, the second group will be a definition of the same object as the first Thus either of the two groups can appear as essence or property alternatively. In this way we arrive at a conclusion not anticipated in the Aristotelian theory, or in the later systems derived from it, namely, that the same thing may be both self-evident and demonstrable But we may go further. the conditioned when properly understood [a] (that is, as the *whole* of what is *necessary* to determine the conditioned and *no more* than is necessary) always reciprocally necessitates the conditioning; a point which we shall vindicate in the discussion of the plurality of causes.[1] Thus each necessitates the other reciprocally, and in geometry, as in any truly demonstrative science, essence and property are always reciprocal All propositions therefore which are properly

[1] § 352

[a] Wilson always insisted upon this, but the truth appears to be that many propositions in geometry (for example) are by their nature not convertible The Greek geometers' views as to geometrical conversion are explained by Sir Thomas Heath, *The Thirteen Books of Euclid's Elements*, vol 1, p 256 'It is clear that a conversion of the *leading* type must be unique, while there may be many *partial* conversions of a theorem according to the number of hypotheses from which it starts' (p 257)]

proved, namely, those in which the fact is proved from the *sufficient* and *necessary* condition, have their converse true. Thus Euclid, *Elements*, i. 48, is the converse of i. 47 (Pythagoras' theorem). A triangle has its three interior angles together equal to two right angles; this truth having been properly proved, that is, made to depend upon triangularity as the sufficient and necessary condition, is convertible. Every rectilineal figure whose interior angles are together equal to two right angles is a triangle; we might prove this property of an equilateral triangle, but we should not have assigned the true condition, we should have put in too much, and the proposition is therefore not convertible. It is important to add, however, that though two elements of reality A and B may reciprocally necessitate one another, there nevertheless may be a priority of the one over the other in the order of our apprehension. Suppose we can start independently with the construction of A and demonstrate from that the existence of B. Suppose again that in proving the converse, that B necessitates A, we presuppose the construction of A. Then, though we have a real converse proof, B has not formed an independent starting-point and cannot form the material proper to a definition. This is what constantly happens in geometry. There are two forms: First, suppose that starting from A we derive B, then in the proof of A from B we may either use the proof of B from A without a *reductio ad absurdum*, as is done in Euclid, *Elements*, i. 48, or alternatively we may use the former proof (B derived from A) in a *reductio ad absurdum*. Thus Euclid first proves that if a quadrilateral is inscribed in a circle the opposite angles are together equal to two right angles. The converse he then proves by *reductio ad absurdum*,[1] the form of which is that he assumes a quadrilateral whose opposite angles are equal to two right angles and assumes that it is not inscribable in a circle. Then, that being assumed, a quadrilateral inscribed in a circle is proved *not* to have

[1] It can, observe, also be proved without such a method [a]

[a] Wilson always maintained that a direct proof of all theorems is possible. This does not appear to be the case, see, for example, Euclid, *Elements*, iii. 1 (Heath, 1 c, vol. ii, p. 8). Wilson spent great energy over the attempted conversion in the last part of this section. It is clear from his manuscript that he was not satisfied with the result reached in the text]

the given property of the opposite angles being equal to two right angles, and this is contrary to what was proved in the original theorem In some cases then we find that we can make a proof of A from B independently of the proof of B from A, and that we can also have another proof in which we make the demonstration of A from B depend upon and presuppose the demonstration of B from A But now let us take the proposition that the interior angles of a triangle are together equal to two right angles and attempt to prove the converse, which is true. We shall find that we presuppose that there is such a figure as a triangle and that we do not start from the mere conception of a closed figure whose interior angles are equal to two right angles For, if we undertake the construction necessary to vindicate that conception, we find we presuppose the possibility of three straight lines enclosing a space Any use of the axiom of parallels involves this, because the axiom presupposes the construction of a triangle What is the reason of this? The relation of the magnitude of the angles to the sides of a figure (if we do not introduce the condition that the figure is closed) remains indeterminate unless we like to admit the limitation that any one angle cannot exceed two right angles Thus, if we fix the magnitude of the sum of the angles of the figure (estimated on what might be called one 'side' of the figure) to be two right angles, we shall determine neither the number of the sides nor whether the figure is closed or not What effects the determination is the closing of the figure; that it is which determines the relations of the angles, that is, the various determinations of that closing are due to the determination of the magnitude of the interior angles. It is on this account that we find ourselves compelled to start from the determination of the closedness in respect to the number of sides.

§ 260 The science of pure quantity, though so abstract, depends as much as geometry on a perception or imagination of particular individuals To prove the elementary propositions which are the basis of this science we do not think merely of numbers in general but we must represent definite groups of numbered things For the theorems in arithmetic we must count definite individual instances, and for more general theorems, as in algebra, where we consider not merely the sums or multiples

of definite numbers but sums and multiples in general, we still require groups of units which are of definite number, though in our argument we do not take into account the precise number which they have. Our results are universal here as in geometry because we see that they do not depend on the particularity of the instances. Our attitude is the same as in geometry; in the individual instances which we construct empirically we see the universality of the results. The latter are self-evident and we never think of appealing for confirmation to new particular instances.

§ 261. In the case of the science of pure quantity the procedure above described is supplemented by another process, that of symbolic representation and symbolic operation, so that whereas we start with definite quantities and operate upon these, when symbols are used we seem to operate on symbols. This is the difference between algebra with its kindred sciences and the direct procedure of pure geometry, and requires special consideration. Clearly nothing can be effected through mere symbols taken by themselves; they depend always for their use on our power of constructing or apprehending what they symbolize, and thus we might incline to underrate their importance, thinking of them as a convenience rather than a necessity. But in truth symbolism is the necessary instrument of the sciences to which it belongs and great advances are made by the discovery of new symbols for new problems The first purpose of symbols in algebra is to distinguish various quantities from one another as merely distinct quantities without definite number. It is impossible for us to work with the mere abstract notions of quantities, conceived as merely different from one another, without some perceptive or imaginative units representing them and so enabling us to keep them distinct and in our memory Such perceptive units are supplied in algebra by the letters of the alphabet. By their difference in form they represent the difference of the abstract quantities and, as they are made to differ in a way easily recognizable, they are not confused with one another. Thus we remember what quantity had what operations performed upon it. Secondly, a most important function of symbolism is to represent conveniently not only the quantities but the nature of the operation performed. They serve here the

important purpose of helping the memory and enabling us to hold together with certainty a great variety of mental constructions and operations which we should otherwise inevitably forget or confuse Thus they render possible a train of argument which would be quite impossible without them. Generally speaking, any symbols will do, if sufficiently distinct in form; those being most suitable which, while simple, are sufficiently distinct to prevent confusion A good deal again depends upon convenience in writing or printing and representation to the eye. If we must appeal to the touch, as with the blind, that may produce a very important difference in the nature of the symbols. Thirdly, the symbolism itself serves in a remarkable manner to save the effort of the several mental constructions which are necessary, because to each construction correspond certain definite changes in the symbolism These changes are easily remembered as valid, without the necessity each time of going through the actual processes of thinking which make them valid. Thus a difficult train of argument is reduced in great part to a number of merely mechanical changes in our symbols which we perform by rote, not apprehending their validity each time but remembering that a proof has been given which establishes it [1] However, mechanical as this process may seem, it depends for its worth on the possibility of our making those mental constructions which the changes in the symbols represent. Finally, the processes thus symbolized are real constructions and never have a merely analytical character. They represent, even the simplest of them, synthetic judgements so called.

[1] Compare what was said in § 247 on the use of remembered theorems in geometry.

[NOTE —The use of the word 'construction' in this chapter requires to be modified in the light of the author's more mature views See, e g , pp. 522 and 524]

IV

THE UNIVERSAL CHARACTER OF INFERENCE AND ITS RELATION TO IMMEDIATE APPREHENSION

§ 262. We now return, as we promised,[1] to the question of the universal character of all inferring. The question is, can inference involve particulars only, without reference to universals; can it, in other words, be the apprehension of a particular fact as necessitated by another, without the apprehension of a necessary connexion of universals? It has actually been asserted that all inference is from particulars to particulars,[2] that from particular instances of A observed to be B we infer that other particular instances of A are B This is easily refuted. The real process of thought is this From the examination of the particular instances of the universal A we find reason in the nature of the instances to think it probable that Aness is a condition of Bness. We then infer in the case of the second set of A's that they are B's, because we judge that probably all A is B. The essential thing then is the supposed connexion of the universals Aness and Bness. If we take inference which is certain and not merely probable to be the apprehension of a fact as necessitated by another apprehended fact, then, if a universal necessitates another,[3] this carries with it the necessitation of a particular fact by a particular. Thus, if Aness necessitates Bness, a particular A as such must be a particular B. The question is whether the converse is true; whether, if one particular fact necessitates another, this can only be because one universal necessitates another. Suppose the particular fact F_1 necessitates the particular fact F_2; suppose that what in F_1 necessitates F_2 is A_1 and that it is particular. A_1 cannot itself be merely particular; it must be the particularization of a uni-

[1] § 227. [2] As by Mill, *System of Logic*, III. 3, §§ 4, 5, and 7.[a]

[3] For an explanation of the proper sense of necessitation of one universal by another (not connexion), see § 82.

[a] Cf. Locke, *Essay*, IV. 17, § 8.

versal of Aness. It cannot be the mere particularity of A_1 which conditions another definite particular; that would give no ground of any definite determination, for, as mere particular, it would be like every other particular. It can only determine anything as being a particular of definite kind or quality, that is, as a particularization of Aness, a universal. If, now, Aness necessitates Bness, a particularization of Aness as Aness necessitates one of Bness, indeed this is the meaning of the expression that the universal Aness necessitates Bness Can, however, the particularity of A_1 condition something other than what the nature of Aness thus necessitates? Clearly not, for, as before, mere particularity, apart from the universal particularized, contains no ground for anything This may be put otherwise. We have maintained that the nature of the particular is entirely covered by that of the universal, and there is nothing in the particular but what the nature of the universal necessitates. Thus anything which is necessitated by the nature of the particular must be due to the nature of the universal. This will be made clearer if we consider a possible objection. It might be said that a particular has in it something more definite than being an instance of a given universal We can say more of this circle than that it is this circle or a circle As a particular it has a definite position and definite relations, a relation, for instance, to another circle which it may touch or intersect. But we answer that this belongs really to the universal itself and is after all but a particularization of it, or part of its particularization. It belongs to the nature of circularity, for instance, that particular circles should be capable of intersecting or touching In general any relation R_1, in which two particulars A_1 and B_1 of two universals stand to one another, is itself a particularization of a universal R and depends on the natures of the universals Aness and Bness The kind of relation in which a particular of a certain sort stands to another particular must depend on the kind to which that other particular belongs as well as on the kind to which the first particular belongs. This therefore must be true of the relation of necessitation as obtaining between two particulars. This accounts for the fact that every actual inference, we find, depends on a relation between universals, and we cannot make an inference which is

not a particular instance of a general principle, which principle again is a connexion of universals. The general principle is what we have called the rule of the inference; the inference drawn is simply a particularization of it. This particularization is not derived from the rule, nor subsumed under it as its premiss. The rule is only apprehended *in* its particularization and only as particularized. Thus both rule and validity of a particular inference are apprehended together; the apprehension of the rule cannot precede the apprehension of the particularization. The fallacies which spring from subsuming the inference under its own rule, taken as a premiss preceding the particular inferences, have been already explained and refuted.[1] Shortly, they amount to inferring the inference itself.

Finally, there are cases where we might say that we inferred and yet we cannot feel sure that there is anything which would be fairly called a connecting universal, giving rise to a true conclusion. These are cases where, observing in an individual (say) two features peculiar to it, seeing one I infer the other. If I know no necessary connexion between the two, the inference for me contains no universal principle But, for the same reason, it is not a certain inference What has been said is true of all *certain* inference and concerns real necessitation.

§ 263.[a] We now return to certain associations of the word Inference which were mentioned in the introductory sections to this Part.[2]

Inferred knowledge is considered to be something dependent, as distinguished from the knowledge from which it is derived. It is conceived as indirect in contrast again with that same knowledge, which is itself regarded as direct. Finally, there are cases where it is accordingly regarded as inferior. So we speak sometimes of a *mere* inference It is true that dependence and indirectness do not themselves seem necessarily to convey inferiority in respect of certainty. Yet there are cases where we think that what is inferred would be better apprehended otherwise, and in such cases our judgement is about different ways of apprehending the same fact and doesn't depend on the

[1] § 237. [2] §§ 209–10

[a] § 263 to end of the chapter was never revised, nor apparently properly reconsidered]

comparison of a fact not got by inference with one derived from the same fact by inference.

§ 264. The judgement of inferiority arises in the case of particular facts apprehended in experience, where inference implies a kind of knowledge different from that by which the same facts are directly apprehended. The completest apprehension of a particular fact in the present is perception; the completest apprehension of it as a past fact is our memory of the perception. When we infer to a fact which belongs either to past or present, though we imagine it, we yet have not this completer kind of apprehension Here also our inference can at most be only probable, based as it must be on particular observations about which we cannot be certain; so that it does not constitute an apprehension of the fact at all I may infer, for instance, that another person has a sensation of a certain kind; but my inference is not certain while his perceptive knowledge is certain.[1] Again, the exact character of his sensation *he* apprehends while I cannot possibly apprehend it Here I am shut off from the only complete knowledge, which is perception.

This uncertainty, while often passed over in ordinary life—so much so that we treat the highly probable as if it were certain—is yet forced upon us in cases of extreme practical importance, notably in what is called circumstantial evidence in criminal trials Here we become aware of the fact that our most careful deductions from what is given in experience cannot be equivalent to the judgement of an eyewitness. In the sciences again, surprises about opinions and theories which rested on strong empirical evidence are not infrequent.

§ 265 In the demonstrative sciences, whether or not inferred knowledge is thought of as independent or indirect, its certainty is not depreciated, but deemed equal to that of the premisses themselves. Here we are not trying to apprehend particular facts as particular facts, we are concerned only with the connexion of universals Yet the difference is not that there is no consideration of universals in the former cases. We have already tried to show[2] that it is only through universals that *any* inference can be made, and this truth may be verified in any instance whatever The distinction is that in these demon-

[1] Compare what was said in § 209

[2] §§ 227 and 262.

strative sciences we are not concerned with the nature of any particular as *that* particular; not therefore with singular judgements but with the connexion of universals, and therefore only with universal judgements.

Now the apprehension of the universal in this connexion cannot be perception at all, though it takes place in perception or imagination. And so we have not here the contrast of apprehending, or trying to apprehend, the same thing in two ways which are so different in kind as perception and a thinking process which is *not* perception. And when we examined the nature of geometrical argument we saw that all the apprehensions, both of the premisses and of what they necessitate inferentially, came in the same way, namely, in a geometrical construction. Accordingly what was apprehended as necessitated by a premiss or premisses was as certain as the premiss or premisses apprehended That is why in geometry we do not have the opposition of inferred knowledge to what is not inferred, as of less certain to certain. The question before us therefore concerns the dependence and indirectness of inferred knowledge and the sense in which these terms are rightly or wrongly applied. Further, supposing the same fact to be apprehended without inference, the question is whether there is any reason for preferring the one kind of apprehension to the other.

§ 266. We have maintained that inference depends upon the objective fact that one element of reality, simple or complex, may necessitate another distinguishable from it, and we have considered inference to be always the apprehension of one fact as necessitated by another, though not asserting that every such apprehension is accounted an inference in the ordinary use of language. The characteristic of the cases which are not called inference is that in them a relation between two elements of reality is apprehended immediately from our experience of them, not by the help of relating them to something else; that is, not by the help of additional knowledge consisting in the apprehension of their relation to something else. But we shall see that the converse is not true in the ordinary usage; for not every such apprehension is accounted non-inferential. Again, if two distinguishable judgements necessitate a third; if, that is, we apprehend that the two different facts apprehended in the

two different judgements necessitate something else, this apprehension is usually accounted an inference. But to this also we shall see there may be exceptions

Suppose we know one thing as A and another as B and from this do not see the relation r_3 between them. Suppose that, when we get the additional knowledge that A stands in a relation r_1 to M (Ar_1M) and B in a relation r_2 to M (Br_2M), we see that this of itself necessitates that A stands in the relation r_3 to B (Ar_3B) This would be called an inference on both of the above grounds, both because of the relation of A and B to the third thing and because of the two judgements necessitating a third.

Here the fact Ar_1M and Br_2M, as a complex, necessitates without anything further (*ex hypothesi*) the fact Ar_3B, and the apprehension of this complex Ar_1Mr_2B is enough (*ex hypothesi*) to give at once the apprehension Ar_3B. There is no indirectness or mediateness either in the relation of necessitation or in the connexion of our knowledge of the one with our knowledge of the other (a point which has already been maintained).

As to the dependence, we have seen that in the mathematical sciences the necessitation is reciprocal This is evident enough in a non-temporal consequence, and we have seen that in general, when we take the true condition, the existence of the conditioned necessitates that of the condition Yet there may seem to be in a true sense a certain dependence of an effect in time on previous conditions because they exist first But an event and its temporal conditions are particularizations of universals (called, some of them at least, in such reference 'laws') whose nature is not something which happens in time and whose connexion also is something non-temporal The propositions in science concern these universals and their connexions. For a scientific proposition is that *such and such* a temporal condition C has *such and such* an effect E, and we have seen that, if C is the true condition, the universal E necessitates the universal C Thus we can say briefly that if X, as the true condition, necessitates Y, whether temporally or not, neither fact is independent of the other, for neither fact can exist save in its relation to the other There is, however, a dependence of one of our *apprehensions* on another, if we only know the

fact Y by knowing first the fact X and seeing that it necessitates Y, and if we are not, at least at the time, able to apprehend Y in any way which is independent of our apprehension of X.[1] [a]

Whereas there is in one way a directness in our apprehension of Ar_3B, as necessitated by Ar_1Mr_2B, there is in another sense an indirectness, because the relation of A to B is not apprehended by considering A and B alone, but by comparing each of them with M.

Now, first, is the knowledge of the relation of A to B, when derived from the apprehension of A and B alone, to be regarded as direct and non-inferential?

Secondly, if Ar_3B is not derived from the conceptions of A and B alone, but only by apprehending their relation to a third thing, is that knowledge always to be called indirect and inferential?

Neither principle is strictly observed in practice.

In Euclid, *Elements*, i 4, the two triangles are not related to a third, but the process is undoubtedly looked on as inferential and as a demonstration. But the truth is this: the magnitudes compared, in this instance the bases of the triangles and the angles at their bases, are produced by a construction which is one and the same for both and can only yield one length for the lines and one size for the angles. Consequently the magnitude must be the same in the case of both triangles. The relation between the two triangles is therefore got in the most direct manner possible, got, that is, from what we know of them, which is their construction. The so-called demonstration by the application of one triangle to the other is only a way of putting this (and is not always applicable in the case of spherical triangles).

Here, then, we have a case where the immediate necessitation of one fact by another is accounted an inference; or, more accurately, the apprehension that A and B stand in a relation *r* is seen directly from A and B without reference to a third something and is yet accounted an inference.

This, however, is not done consciously, and so we cannot say that the principle which we maintain to be the ground of all inference—viz. that one fact necessitates another different from it—would always be held to make an apprehension an inference.

[1] As e.g. in Euclid, i. 35. See below, p. 487.

[a] 'Expand this' *MS. note.*]

In the other case we might anticipate no exception. But possibly what we know of A and B may necessarily contain a relation to M, and this may be inseparable from A and B in our apprehension, i.e neither A nor B may be definable at all without the relation, or, in other words, A *as such* may be Ar_1M, and B *as such* Br_2M Now suppose it to be a consequence of these relations to M that A stands in a relation r_3 to B. Since this, though it has the verbal form of what is called indirect or inferential knowledge, is after all got immediately from the knowledge we have of A and B, can it rightly be called indirect and inferential?

In practice we find that it sometimes is and sometimes is not. Thus in Euclid, *Elements*, I. 1, AC and BC are only known as each constructed equal to AB. Yet we find that their consequent equality to one another is looked on as an inference. On the other hand it is assumed without proof in geometry in general [1] that the diagonals of a four-sided 'convex' rectilineal figure cut one another within the figure. Now each diagonal is entirely determined by its relation to the given four-sided figure and can only be apprehended in that relation. Their intersection [a] is the immediate consequence of this relation. Here the proposition is taken as self-evident and not treated as an inference What is distinctive of such cases is that we do not apprehend A as A and *also* as ArM We can apprehend A only as ArM, because objectively A is only defined as ArM And similarly for B. The position and length of the diagonal, which are required for its definition, are only determined by its being a diagonal, i.e. by its joining two opposite angles of the quadrilateral Similarly the lines AC and BC of Euclid, I. 1, are, as we saw, constructed only as equal to AB. In cases such as these, then, the knowledge must be called *direct*, for obviously in the nature of the thing there could not be any knowledge *more direct* [b]

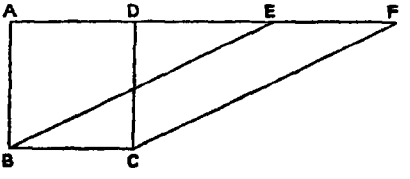
Here an important distinction has to be made. Suppose that

[1] See e g Euclid, *Elements*, IV. 9, where the diagonals of a square are assumed to cut one another within the square

[a] Modern geometry would require a proof of this.
[b] 'Expand this' *MS note*]

A, as A, is Ar_1M, and B, as B, is Br_2M: in consequence A may stand in a relation r_3 to B. Nevertheless we may not apprehend Ar_1M and Br_2M as directly necessitating Ar_3B. It may be that M is so related to N that we apprehend in the case of A (i.e. of Ar_1M) the fact Ar_4N, and of B (Br_2M) the fact Br_5N. We then see that Ar_4N and Br_5N directly necessitate Ar_3B.

Now this satisfies the definition of indirect or mediated knowledge of the relation between A and B, because it depends on some additional knowledge not contained in the definitions of A and B. For, though the relations to M are contained in these definitions, by hypothesis the relations to N are not. An instance of this is the argument in Euclid, *Elements*, i. 35, where the two parallelograms AC and BF are constructed in relation to a common base and the two parallels BC and AF; their relation



to the base and the line parallel to it (the latter complex = M) belongs to the definition of each of them. But the relation is not enough to give us the relation of their magnitudes to one another. We must further apprehend a relation of M to the figure ABCF (= N). In this way we apprehend the relations of the parallelograms to the same figure,[1] and these relations we see immediately necessitate their equality. One might indeed dispute the application of direct and indirect to this reasoning, because the knowledge wanted is precisely about the relation of the two areas to something distinguishable from them. Thus the 'equality' of the parallelograms means that a unit of area can be found which would be contained the same number of times in both. This is the same thing as to show that one can be cut up into pieces which will exactly cover the other.

On the other hand, in some instances, if we can have the mediate apprehension we can also have the immediate. In the last case there was no choice of alternatives; in this case we

[1] The last steps are: $\Delta FDC = \Delta EAB$, $\therefore ABCF - FDC = ABCF - EAB$.

have both alternatives. Thus, starting with one definition X of an ellipse, we prove Y of it. Y turns out to be itself a definition of the same figure We have proved that there may be such a figure as Y in consequence of the existence of such a figure as X, but we can also see that such a figure as Y is possible immediately.

We may say provisionally that the knowledge of Ar_3B is direct and unmediated when what is contained in the definition of A and B is seen of itself, i e. without any other knowledge, to necessitate Ar_3B, but is mediated when what is contained in these definitions requires to be supplemented by some other knowledge. Hence, whereas the relation of two things to one another by virtue of their relations to a third thing *may* be mediated knowledge, it is not so if the relation to the third is a necessary part of the definitions of the first two.

§ 267. The question which faces us now is whether, when two distinguishable facts necessitate a third fact distinguishable from each of them, the apprehension of this third fact should always be accounted an inference, and, if an inference, whether it is to be always considered *indirect* in the sense we have given to indirect.

We have seen that the apprehension is not always accounted an inference. Two straight lines cannot enclose a space. Here are two facts, the existences of the two straight lines, necessitating a third, viz 'that they do not enclose a space'. That is, they either do not meet at all or do not meet twice. The apprehension of this is not taken as an inference, but as self-evident, and therefore as not indirect.

We might argue that there is no apprehension of necessitation which cannot be reduced to this form

Let A of itself necessitate B and suppose this known 'directly'. The existences of A and B are two different facts and the apprehensions of them two different apprehensions. The relation between them, it might be said, is a third fact

Here we must beware of a verbal fallacy. In some cases it is at once obvious that the reduction would be only verbal. For the fact of the necessitation of B by A may not be a fact distinguishable from what we must define them to be. In other words, A and B cannot be defined apart from the given relation

between them. A three-sided rectilinear figure must have three angles. The angles, it is true, are different from the straight lines; it is not, however, as angles merely that we compare them but as the angles between the lines forming the figure. In general, if we apprehend A and only apprehend B as necessitated by A, the necessitation of B by A cannot be apprehended as a third fact distinguishable from A and B as we apprehend them For our apprehension of the fact that A necessitates B cannot be distinguished from our apprehension of B.

But suppose A and B can be apprehended as realities apart from the necessitation of either by the other. Can we then say that the apprehension of the fact that A necessitates B is the apprehension of a third fact, different from either of them, which they together necessitate? We reply that the necessitation of B by A is part of the whole nature of A and therefore is not something different from the fact of A's existence, nor a third fact different from A and B as facts. Nor can we intelligibly say that the nature of A and B necessitates the necessitation of B by A—a mode of expression indispensable to the view we are criticizing—for the necessitation of B by A is included in the nature of A and B and we should thus be describing the necessitation as necessitating itself Again, it is only a verbal fallacy to represent a co-operation of A and B as necessitating the necessitation of B by A For the nature of A is enough. If we make the verbal change, the above becomes 'the nature of A necessitates the necessitation of B by A'. This, however, is only intelligible if it means that the nature of A necessitates B Thus, if A necessitates B, the necessitation of B is not another fact necessitated by A in addition to the B which it necessitates, and to say that A necessitates the necessitation of B is either unintelligible or a way of stating that A necessitates B. So also, obviously, if we have two really different facts A and B necessitating in combination another fact C, the necessitating of C cannot truly, but only verbally, be described as a fourth fact necessitated by A, B, and C.

§ 268. The foregoing inquiry enables us to understand in what sense the distinction ordinarily made between axiomatic and inferred or demonstrated truths can be vindicated.

We understand also that in the ordinary treatment of what

is in itself a justifiable distinction, there are certain inconsistencies. We were concerned before in showing that in an important sense the knowledge of axioms is of the same kind as the knowledge of demonstrations. This knowledge we maintained to be an apprehension of a necessary connexion of universals in the construction of a particular case, the apprehension being always direct and the axioms requiring a construction as much as the theorems In this sense, moreover, the knowledge was seen to be all self-evident

But now, this being so, a relation between A and B which is apprehended from the definitions of A and B alone, without comparison with a third thing M, is different from a relation which is only apprehended by the relation of A and B to an element M, not comprised in their definitions. The first may be called self-evident and direct in reference to our knowledge of A and B, and corresponds to what is called axiomatic knowledge. The second is inferential and indirect in relation to the same knowledge.

Yet the premisses Ar_1M and Br_2M necessitate the conclusion Ar_3B without reference to any other fact to mediate the connexion or necessitation, so that this necessitation is self-evident, direct, and axiomatic. This gets an important recognition even in the ordinary use The necessitation of Ar_3B immediately by Ar_1M and Br_2M is, as we have seen, a rule of inference. Now some of the axioms stated as axioms are nothing but rules of inference. 'Equals to the same thing are equal to one another', for instance, is only the rule of inference, expressed otherwise in 'C = B, A = B, then A = C'

Finally, in answer to the question whether direct or unmediated knowledge is superior to mediated, we reply that in the demonstrative sciences at least there is no difference in their certainty. We ought to have both, for neither is complete without the other, each belonging to the nature of the facts before us.

V

SIMPLE AND COMPLEX IDEAS AND PROBLEMATIC CONCEPTIONS

§ 269. LANGUAGE develops differences in form to correspond to differences in thought: and the normal meaning of a given form is that form of thought for which it was developed. But in idiomatic use the form gets extended beyond its proper and primary sense This is possible without confusion when the context or the subject-matter sufficiently indicates the true meaning This very transference may be taken advantage of to deceive. for the form may be used with nothing to suggest that its use is not the normal one, the hearer so understands it and the speaker intends he shall, whereas he himself really means the sense which is not normal—a jest or a prevarication.

The normal meaning of 'if A is B, C is D' and of 'if any A exists, it is B' implies that it is uncertain whether A is B, or whether any A exists This is the true hypothetical statement[1]; the normal meaning of the so-called categorical statement, 'all A is B', is that A certainly exists

The latter, in a familiar idiom, may be extended to cases where the existence of A is uncertain. Nevertheless, the normal meaning of 'all A is B' involves the existence of A, and it is just because of this that the form 'if a thing is A, it is B' has been developed for the cases where this existence is uncertain.

A similar extension happens to the hypothetical form. It is not true, however, that 'if there is such a thing as A, it is B'; 'if A exists, it is B', 'if anything is A, it is B', are ever, at least naturally, used to mean 'all A is B', but rather the hypothetical form may seem (for a reservation must be made) to do duty for what may be properly called the conditional

[1] On the use of the word 'statement' instead of 'judgement', see Part II, particularly § 33 The hypothetical statement, when taken to be true and not an opinion, is a real judgement, because it is a decision on evidence, for, as shown in § 298, it is an inference

form. 'A's being B is $\left.\begin{matrix}\text{a}\\\text{the}\end{matrix}\right\}$ condition of C's being D' is a true conditional and indicates normally that A is really B. So also the conditional may have the form $\left.\begin{matrix}\text{where}\\\text{when}\end{matrix}\right\}$ A is B, C is D, and has in that form the same implication.

The hypothetical form appears sometimes to be used in the same sense. For instance, in giving the description of a scientific experiment and a direction how to perform it, or even in stating the 'construction' of a geometrical figure, we use the idiom: 'If an equilateral triangle be described, &c.' In such cases the possibility of the experiment, or of the construction, is not left uncertain nor supposed to be doubtful. Yet this is not a mere equivalence to the conditional form The hypothetical character is not lost. The question left open is not whether the construction can be realized but whether the reader or hearer will realize it and it is really on this account that the 'if' is used. The conditional form, on the other hand, may stand for a true hypothetical, a point to which we shall return [1]

§ 270. It is a common doctrine that the categorical can be reduced to the hypothetical form Here by the categorical is meant 'all A is B', as opposed to the hypothetical 'if A exists, it is B'. This is a mere mistake about the usage of speech, for, whereas 'all A is B' implies normally that A exists, 'if anything is A, it is B' normally implies that the existence of A is uncertain. Thus the proposed reduction is impossible. Again a distinction is usually made between the categorical and hypothetical statements as follows A categorical statement is one which attaches its predicate to its subject unconditionally: in 'all A is B', B is attached unconditionally to A. An hypothetical statement attaches its predicate to its subject only under a condition Thus in 'A is B, if A is C', A is made subject and B predicate (wrongly) But every statement must attach its predicate to its subject definitely and unconditionally · and the hypothetical statement is only a statement at all because it does this Just as in 'A is C', C is attached to A without condition, A itself being a sufficient condition of C, so, in 'if A is B, A is C', the connexion of the apodosis with the protasis

[1] §§ 271–2.

is affirmed without condition, the protasis itself being a sufficient condition of the apodosis. Or we may put it thus: a consequence is affirmed to follow immediately and unconditionally from the hypothetical connexion of A and B. What this hypothetical connexion is, is the main object of the present inquiry: it is enough, however, for our present purpose to describe the hypothetical statement as above; for this at all events is evident, that in it something is attached unconditionally to something else; something affirmed unconditionally of something else.

With the above definition, then, of 'categorical', every statement is categorical and 'categorical statement' = 'statement'. Thus 'categorical' can only serve to distinguish an expression which is a statement from one that is not, and its proper opposition is to 'interrogative', 'optative', 'imperative', &c., not to 'hypothetical'. Consequently, when we maintain that the hypothetical cannot be reduced to the categorical, categorical is used in the limited sense in which it only means the form 'all A is B' in opposition to 'if A exists, it is B'.

If, then, we make categorical expression = 'statement', what word shall we use for the form 'all A is B'? According to the ordinary use of the words 'real' and 'hypothetical' and their usual opposition to one another, we might call the one statement 'real', and the other 'hypothetical'. This, however, might be taken to imply that the hypothetical statement was not about reality whereas it will be seen hereafter that it is in a sense about reality and is indeed only possible through a conception of what certain realities are

The distinction of statements into 'hypothetical' and 'non-hypothetical' would avoid this difficulty. The classification, then, is

Statement in general = categorical statement

non-hypothetical statement hypothetical statement.[1]

If, however, we prefer to keep 'categorical' for 'all A is B', we must abandon the usual definition of the word categorical and make it suit the implication of 'all A is B' in its normal use.

§ 271. If we classify these statements as above, then the conditional statement proper comes under the head of the non-

[1] Disjunctive statements are considered in §§ 305–6.

hypothetical, as we have above described it. The causal proposition, as it may be called, has the form 'because A is B, C is D'. Now this not only affirms the relation of condition and consequence, but asserts the realization of the condition. How then does it differ from the conditional statement as above described? As a matter of verbal expression the causal puts expressly what the conditional form implies. This has for its consequence that the causal form cannot in language be extended to include the hypothetical while the conditional may. 'Wherever (or, whenever) A is B, C is D' may be used hypothetically, although, apart from exceptional circumstances, this is not natural. If a man were really uncertain whether any A is B, and wished to be accurate, he would hardly use the conditional form If, however, he was inclined to think A could be B, especially if strongly inclined, and did not aim at strict accuracy, he might choose the conditional form. It might seem easier for the conditional form 'A's being B is a condition of C's being D' to be used for a true hypothetical. But even here it would not be natural to say 'is', but rather 'A's being B would be a condition of (or, would condition) C's being D'.

This, however, is not the only difference between the causal and the conditional forms otherwise there would be no proper use for the conditional. 'Because A is B, C is D' means according to the matter or context either 'because A is always B' (or, 'because all A is B'), &c., *or* 'because A is B in this case', &c. The conditional form 'whenever A is B', however, does not imply that A is always B, or that all A is B, but, while normally implying that there are such cases, leaves it an open question whether A is always B. Thus once more the conditional cannot as such refer to the present case of A's being B.

The hypothetical statement is not the only form which arises from our attitude of uncertainty. We may not know whether B belongs to A, but we may know that either B or C belongs to it, and this is the disjunctive statement 'A is either B or C'. The disjunctive statement, however, does not always arise out of, or express, uncertainty. We shall return to this hereafter.[1]

§ 272. A statement, as we saw above,[2] may have the hypo-

[1] § 305. [2] § 269.

thetical form verbally while its matter is not hypothetical. But more than this, a statement may be hypothetical if taken in one sense, but not if taken in another.

Suppose that A is a universal known to have been realized. Then the statement 'if anything is A, it is B', when referring to time in general, is not hypothetical, because it means that all instances of A (in whatever part of time) are B. Or again, though referring to an unknown future or present, the speaker may really presume that there are or will be further realizations of A. In such cases the statement is not intended by him to be hypothetical, though if questioned he might admit that it ought to be put hypothetically.

In other circumstances A may indeed belong to reality as having been realized, yet it may be a matter of conscious uncertainty to the speaker whether A is realized for the time to which 'is' refers in his statement that 'if anything *is* A, it is B'; or where he says 'if anything will be A, it will be B', he may feel doubtful whether A will ever be realized in the future; and so far his proposition is hypothetical. For it is the essential character of the hypothetical statement that the matter of the clause introduced by 'if' should be uncertain, i.e. this is the normal meaning of the hypothetical form

Nevertheless, even here, there is a non-hypothetical statement involved, for in the given case, where A is real in the sense that it has at least been realized, the hypothetical statement 'if anything is A, it is B' depends on the implied assertion that the nature of A necessitates B.

'Whenever A is B, C is D' has a non-hypothetical form as compared with 'if A is B, C is D': and indeed normally implies that A's being B does belong to reality as being realized at some time or other. It may be non-hypothetical altogether, or again not intended as hypothetical; but it may be hypothetical, because any further realization of A's being B may be uncertain.

§ 273. There is a way of representing the statements 'if A is B, C is D', and 'whenever A is B, C is D' which is very misleading. This arises with logical reflection. Thus it would sometimes be said that 'if A is B, C is D' means that the proposition A is B has for its consequence the proposition or judgement C is D. This is done in order to analyse the hypothetical proposition

into subject and predicate. Accordingly it is said that the hypothetical judgement or proposition expresses a relation between two judgements or propositions.

Now this way of speaking is only the product of logical reflection: no one in ordinary life would express himself thus. Indeed it would rightly be answered that we do not mean anything about the relation of propositions as propositions; we mean that A and B and C and D are such in the nature of things or in reality, not in our minds merely, that if in reality A were B, in reality C would be D. The mode of expression is not merely artificial but fallacious. It does not even follow from the above that, while the true meaning of the proposition is what has been described, a consequence of it is the aforesaid relation between 'propositions' or 'judgements'.

A judgement is not a judgement unless it is judged. The same is true, *mutatis mutandis*, of a statement. Thus the words 'A is B', which may represent a statement, do not represent a statement in any given context, unless that context represents acts of thought in which the statement 'A is B' is made. But in 'if A is B, C is D', used to express a true hypothetical statement, we do not state ('judge') that A is B, for we are *ex hypothesi* uncertain whether A is B. Similarly we may make C's being D dependent on whether A is B; in that case 'C is D' is uncertain and we do not state that 'C is D'. Thus neither 'A is B' nor 'C is D' represents a statement in the hypothetical form of statement in question.

A similar criticism applies to translating 'whenever A is B, C is D' into 'whenever the proposition or judgement "A is B" is true, the proposition or judgement "C is D" is true', but here there is an additional fallacy. The implication is that a statement can be sometimes true and sometimes not true. But this is a fallacy. If a statement is true at all, it is always true. Truth is not something which can vary in time. It is upon this fallacy that Boole's whole calculus of inference as applied to the relations of propositions is based.[1]

An hypothetical statement then is never the statement of a relation between two statements.[a]

[1] § 384.

[a] The notion that the propositions are connected by *relation* came into common use from Kant. Cf. note to § 98.]

§ 274. In the true hypothetical statement there is a kind of paradox. It relates to something the reality of which is uncertain and yet something definite is said about it—definite and positive, not merely negative, for instance, not merely the definite statement that it is uncertain.

This uncertain something is the hypothetical element in the hypothetical statement, and we must investigate its precise nature. With the idea of the hypothetical we do not connect merely uncertainty. To be uncertain about a thing is not to treat it as an hypothesis, nor as the hypothetical part of an hypothetical statement. There is something apparently more positive than mere uncertainty. Whether rightly or not, we often think of the positive side as consisting in some fiction of ours. (Fiction here is taken in the sense of conscious fiction, and not of an unconscious mistake. A 'fictitious idea' sometimes means a mistaken idea.) Or again it may be said that the hypothetical element is a *mere* idea of ours, which really comes to the same thing.

Without analysing for the present (though it greatly needs analysis) the idea of fiction, we can see that the hypothetical element in a statement cannot be wholly fictitious and cannot be merely uncertain. It must contain something known to be real and something about which we are certain.

§ 275 If we examine any hypothetical propositions we make in ordinary life or in science, as distinguished from philosophic reflection, we find them to be of the type 'if A is B, C is D' where A, B, C, and D are none of them regarded as fictions but as realities. It would not even be correct to say that A, B, C, and D are 'conceptions', but, even if we do call them conceptions, they are supposed to be conceptions of realities and so not fictitious. If there is supposed to be any fiction in such statements, it would probably be said that 'if A is B' represents or is something fictitious, or *mere* idea, or that the hypothesis that A is B (or the hypothetical case of A's being B) is a fiction. But the important thing is that A, B, C, and D are not thought of as fictions.

It is, however, a current doctrine of popular philosophy that the mind has a power of constructing fictions which are not hypotheses in the above sense. It is supposed that the mind

cannot *make* simple ideas, but that it can arbitrarily *make* complex ones, because it can arbitrarily combine simple ones into complex. This doctrine quite ignores a consideration which should lead to a re-examination of it and, indeed, to its rejection, that ideas may just as much be impossible to combine as to make. We cannot, for instance, make the complex idea of a closed rectilinear figure which has three sides and only two interior angles. If the doctrine were true it would imply that we might have, in a statement, an element AB (as in 'all AB is C', or, 'if AB is C, it is D'), which was a fictitious idea or conception, besides such fiction as may be supposed to consist in an hypothesis If so, the form of an hypothetical statement would not necessarily be of the type 'if A is B, C is D', where not one of the elements A, B, C, and D is a fiction. We have therefore to consider whether there really can be fictitious complex ideas This again suggests the question, whether there can be any fictitious simple ideas

But the usual distinction between simple and complex ideas is not clear; indeed it is sometimes confused and contradictory. As a preliminary therefore we must first discuss the distinction itself.

§ 276. The *locus classicus* for the distinction of 'simple' and 'complex' ideas is Locke's *Essay*, Bk. II, especially Chaps. ii and xii.

The ideas peculiar to each of the five senses he calls simple, Tastes (sweet, sour), Sounds (loud, low), Smells (sweet, stinking), Colours (white), Touch (hard, soft, hot, cold). There are others which we shall return to presently, but those named are such as we should naturally take to be simple ideas, if any are simple.

But this common-sense view is threatened by a consideration which had not occurred to Locke and is familiar in modern philosophy. The apprehension of red as a distinct colour, different, say, from white and different from a sound, necessarily involves the distinction of it from other colours and other objects of the sensitive consciousness. The recognition or apprehension of '*red*' as a distinct and definite colour is only possible for a consciousness which has other objects to compare it with. Thus every such idea as red, hot, sweet, loud, necessarily involves a plurality to which it is related. Hence, if the involving thus

of a plurality made an idea complex, these simple ideas of Locke's, which correspond to the objects peculiar to the five senses, would all be complex.

Whether this is a real objection to the distinction or not (and we shall see that it is not), it does hold against Locke's presentation of it, and he can be shown to contradict it himself.

All ideas of relation he makes complex. So of course 'equal', 'greater', and 'less'. This, as his own language shows, is because a relation involves the different objects between which it obtains and thus involves a plurality 'The second ⟨of the acts of the mind wherein it exerts its power over its simple ideas⟩ is bringing two ideas, whether simple or complex, together and setting them by one another so as to take a view of them at once, without uniting them into one; by which way it gets all its ideas of relations.'[1] It is clearly the presence of the two ideas thus compared which makes Locke call the corresponding idea of relation complex. He expresses himself indeed rather confusedly: for the first of those acts of the mind is 'combining several simple ideas into one compound one and thus *all complex* ideas are made'; [2] whence of course the relations ought not to be complex! Yet elsewhere he says distinctly that relations are complex ideas.[3] His account of the formation of the idea of relation is obviously confused At all events he thus implicitly admits that an idea is complex if it involves a plurality of ideas compared. He therefore contradicts himself, for the ideas of red, &c., as we have seen, also involve a plurality.

The same thing results from his account of modes, and here he explicitly contradicts himself Thus he says that modes, which include both 'simple' and 'mixed' modes,[4] are complex ideas; they are 'such complex ideas which, however compounded, contain not in them the supposition of subsisting by themselves but are considered as dependencies on, or affections of, substances'[5] (e g. triangle). That is because a mode as an attribute of substance involves the reference to the substance, the 'complex' clearly being itself and the substance (according

[1] *Essay*, II. xii, § 1.
[2] ib
[3] ib II. xii, § 3
[4] ib. II. xii, §§ 3, 4
[5] ib. II xii, § 4, cf xiii, §§ 1 seq, 'mixed' or 'compounded' modes are a species of the complex ideas called modes.

to Locke's way of thinking, that is, for he probably doesn't think of its relation to its substance as an element in the complex). Here again the implying of a plurality makes the idea complex for Locke and he is involved in implicit contradiction. But the contradiction is also explicit because what he has called simple ideas must have precisely this kind of plurality. Properly speaking, colour must be a mode and therefore complex; for colour is the colour of an extended surface and so not 'subsisting by itself'. He would, however, probably answer that colour is not this but a mere sensation like heat At least he would have to admit, then, that no sensation can subsist of itself, but is an 'affection' of ourselves, i e affection of our spiritual substance.

He expressly makes solidity a simple idea; but it does not exist by itself. it is exactly an affection of a substance.[1] There are other explicit contradictions, for he makes 'succession' a simple idea, but 'succession' is obviously an idea of relation and therefore, according to himself, should be a complex idea. Again, 'power' is a 'simple idea';[2] yet obviously it involves relation and things related, as much as any of what Locke calls ideas of relation. It should therefore be an idea of relation and 'complex'. Moreover, power, as an 'affection of substance' and not subsisting by itself, should be a simple mode, if a simple anything, and therefore a complex idea 'Unity'[3] is also a simple idea, but unity is 'unity OF', and so implies plurality.

§ 277. The reason of Locke's confusion is that he didn't see the necessity of determining clearly what he meant by simplicity and complexity. He unconsciously used these distinctions sometimes in one sense, sometimes in another.

By a complex in general is meant something which consists in a unified manifold, or in a unity of a manifold or plurality. But everything depends on the kind of plurality. We do not as a rule mean that a thing is a complex because it necessitates, involves, or is connected with a plurality from which it can be distinguished, but because it itself consists of a plurality which is unified. Thus from complexity in a thing is excluded the necessitation of plurality other than and distinguishable from itself. (Examples will make this clear.) Locke's mistake is that

[1] *Essay*, II iv, § 1, where it is a mode and therefore a complex idea.
[2] ib, II vii, §§ 1–8 [3] ib., II vii, § 1.

he takes the latter for complexity, and hence relations and modes are with him complex.

But sometimes he thinks of complexity in the true sense and so makes *succession*, for example, though a relation, simple. Power again, though both relation and mode (it never occurs to Locke that a relation may be a mode), is also for him simple. An example will make the issue plain. Consider equality. Equality is a relation. It is complex, according to Locke, because it is a relation between two magnitudes. It is certainly true that in itself it necessitates two magnitudes and therefore a plurality. But if equality itself is a complex, it must be a unity of different elements in itself What makes it a complex or plurality must be the plurality of elements or members which constitute it. The two magnitudes, then, as making it a complex, must be elements in it and it must be their unity, in the sense of the whole system which they constitute. But the two magnitudes are not parts or elements of the relation of equality between them. A solid, we can say, consists as a complex of surface and volume; we cannot say equality 'consists' of the two magnitudes which are equals. The two magnitudes, as standing in the relation of equality to one another (or in any other relation), are thereby unified into a system, i.e. a complex. That is true enough. This system 'consists' of the two magnitudes; but this unification of the two, i.e. this system which they constitute, is obviously not their equality. The plurality of elements which makes a thing complex must be such that those elements in their unity—of whatever kind—*are* the thing in question.

Thus, if the plurality of the two magnitudes made their equality complex, the two magnitudes in their unity must be (or constitute) the relation of equality between them, which is obviously absurd. The relation of equality, then, unifies them but is not itself the unity of them, in the proper sense of 'unity of them', i.e. a unity of which they are elements, the magnitudes themselves as unified.

The phrase 'unity of' is ambiguous. That which gives a plurality unity and systematizes it produces a system which is a unity of the plurality, but the principle which gives it unity is not itself the unified system; it is distinguishable from it.

The children of the same parents are unified into a group, called brothers and sisters, by their relation to the same pair of parents. They form a unity of which they are the members; but that unity is not the relation which unified them. They are members of the family group, not members of their relation to their parents

A complex is not distinguishable from, and other than, the totality of its members So, any plurality from which it can be distinguished cannot be of members of itself, and so cannot make it a complex But equality is obviously distinguishable from and other than the magnitudes between which it obtains, and so *their* plurality does not constitute it a plurality. Hence equality is *not* made a complex for the reason which Locke gives, and this applies, generally, to any other 'idea of relation' whatever.

We have then to ask whether equality in *itself* as distinct from the plurality of the equal magnitudes is a unity of elements. If we take it 'in itself' it seems clear that we cannot make it a complex which is a unity of members; we could not assign any members the unity of which constitutes or makes up equality. It is in this sense, therefore, simple. A sign of this is that equality will not yield a complex attributive or a plurality of attributives, as every true complex idea should. We say of the magnitudes of two bodies that they are equal, and the attributive is simple and will not yield several attributives. But 'animal' yields both living organism and sentient organism

It is because succession shows no such complexity within itself, has no members, that Locke calls it simple. If consistent, he would have seen that relation as such is not necessarily complex.

§ 278. But there is another kind of plurality or manifoldness in 'ideas' which it is not natural to call 'complexity'.

Consider the particular red colour of this object. This is an individualization of the species red colour, which again is a differentiation of the genus colour. The genus is distinguishable from its differentia, and the species as universal from the individual. Hence there is plurality, whether red means the species red colour in general or this red colour. This plurality,

however, is not one of what could be called elements or 'members' of some colour, while 'colour' and 'red colour' are not elements of or members of *this* red colour.

Thus the equality of these two magnitudes is a particular case of equality, which is a species of relation of magnitude, which again is a species of the genus relation. Relation is then differentiated into relation of magnitude, and this again into relation of equality of magnitude.

Now in equality again, the elements, relation in general, relation of magnitude in general, and relation of equality of magnitude, are not naturally called members; the genus 'relation' is not a member of a relation of magnitude, nor are these (i.e. 'relation' and 'relation of magnitude') members of the relation of equality of magnitude.

It is therefore also not natural to call 'the relation of equality of magnitude' complex For the elements of a complex are 'members'. Members are normally understood not as different in degree of universality, but as co-ordinate either as universals, and of the same degree of universality, or as individuals.

Hence, while relation and relation of magnitude are not members of that relation of magnitude which is equality, a relation of magnitude in general and a relation of weight in general are, as classes, members of the genus relation. The species, equilateral, scalene, and isosceles triangle, are members of the genus, but, even so, not of the genus or abstract universal present in them, but members of the group of classes which constitute the whole of the species of the genus.

A complex is conceived as a unity of members which are co-ordinate in universality, so that each member is a part of the whole in the sense at least that the whole contains something besides itself.

Now colour is not a member of red colour, for red colour is not a whole of which colour is a part in the sense that red contains something besides colour. On the contrary, red contains nothing except colour. Colour comprises everything in it. Red is not colour together with something which is not colour. In equilateral triangular figure, figure is not a member of equilateral triangular figure as a complex. For everything in the equilateral triangle comes under figure: it is altogether figure,

and not figure together with something which is not figure. Nor is triangle a member of equilateral triangle, for equilateral triangle is altogether triangle, not triangle and something else besides triangle.

The plurality of manifoldness in red colour due to the distinction of genus and species in it is not such a manifold or plurality as can make it a complex.

In this way, then, i e. in the accurate sense of 'complex', equilateral three-sided figure is not a complex idea, and, if simple means that which is not complex, what is not a unified system of members, it is certainly simple If, however, we mean by simple that in the nature of which no plurality is present, and that which, if related to a plurality, is distinguishable therefrom, then red and triangle are not simple. For red in itself has colour and a differentiation 'red', and triangle in itself involves the plurality of figure and three-sided · 'in itself' because red in itself is colour and can't be distinguished from colour, as something not colour, any more than three from number, whereas surface can be distinguished from volume as something not volume.[1]

On the other hand, the universal has plurality inherent in itself, because it is its nature to be necessarily differentiated into species. It is not some one thing present in all the species along with something else, viz a differentia, constituting with it the species. The differentia is entirely included in the universal. But this plurality is nevertheless not complexity. A universal may have true complexity, if it comprises elements which are not differentiations of one another. In the general idea of a 'red and transparent body', redness and transparence are not related as genus and species. Thus Locke's examples of equality and succession are not complex but have this manifoldness of genus and species. 'Equal in magnitude and heavier in weight' would be a proper instance of a complex relation.

§ 279. There is a danger in some cases of putting complexity into an 'idea' (so called) which relates to a complex, when the

[1] We can assign to red, as red, no attribute which is not an attribute of it as *colour*. We can assign to colour, in an important sense, no attribute which is not true of red (colour) But we can state of volume attributes which are not true of its surface

complexity in question does not belong to the idea. Consider the number 'three', in Locke's language 'a complex of three units'.[1] [a]

Suppose we divide a multitude of units into sets of three each. Each set is a complex of three units. Each unit in the set is a member of that complex. But this does not cause the number three to be complex. Threeness does not consist of the three units: none of the units is a member of the number three but only of the group of three.

Their threeness also does not consist in these three units: they are not members of the threeness, but of the set of three. The threeness of the set doesn't consist in its three units; it consists in having three units. As such it shows no complexity: the plurality it does show is that of genus and species and particular, quantity and number and number three.

The same is true of triangle, viz. three-sided rectilinear figure. In any triangle the three sides are unified in a complex whole: and the triangle consists of these lines in their unity (which is their relation of contiguity), but the triangularity of the figure does not consist in these three lines, it consists in its having the three sides; they are not members of its triangularity.

These cases show that the members of the complex (set of three; the triangle) which, in their unity, constitute the complex (this set of three; this triangle) are not members of their property of having this particular complexity (not members of the property of being a set of three, threeness; not members of the property of triangularity) and therefore *their* plurality does not make it a complex.

This three, in fact, ABC, or this set of three, consists of A, B, and C in their whole nature: their threeness is only an aspect of their nature; it therefore cannot consist of them in their whole nature. Thus they cannot be members of their threeness.

Similarly for triangle. This triangle consists in the whole

[1] Locke, *Essay*, II. xvi, § 2; cf II xii, §§ 3 and 5.

[a] The author wrote, 'Locke's calling this a mixed mode amounts to making it complex'. See the passages in the foot-note. Locke calls numbers simple modes, but all modes *are* complex ideas. Three is called complex because it is reached by repeating the simple idea of unity (cf. § 282).]

nature of these three lines, as bounding this particular piece of plane surface. The fact that they form a three-sided figure is only an aspect of their nature, not the whole of it. They are therefore not members of their triangularity. The same is true for triangularity in general. Triangularity in general does not consist of three lines.

§ 280. The foregoing distinction is not that between a complex and the idea of a complex. Our idea of anything is our idea of what it is in itself: it is what we think of it considered as thought by us; and what we think of it is what we know or conjecture it to be. In the first case, then, our idea of a thing is simply our apprehension of its nature or a part of its nature. It is not an imaginary picture of the thing—'idea' in that sense —for that is not what we think the thing is, nor is it the thinking what the thing is. In the second case, our idea of the thing is our conjecture of what its nature really is. This, though helped by imagination, is neither the mental image nor the imagining of it.

We are here concerned with the first sense of idea of a thing as our apprehension of its nature or of a part of its nature. Our idea of a complex like the triangle or set of three is then simply our apprehension of it. Our apprehension of a complex involves apprehensions of the members of the complex and so may be, in this sense, called complex apprehension.

In the case of the triangle we have distinguished between the complex triangle and its attribute of triangularity. So we distinguish now between the idea of the complex (the triangle or set of three), and the idea of its attribute (triangularity or threeness); that is, between our apprehension (as subjective) of this set of three and our apprehension of its threeness.

The distinction then of this three from its threeness is an objective distinction in the object apprehended, and not a distinction between something subjective (our apprehension of it) and something objective (the object's nature), and is parallel to a subjective distinction in the corresponding apprehensions.

§ 281. We have excluded from complexity the plurality of genus and differentia in the species. This is not, however, because the species necessitates the genus of which it must be the determination. Elements in a manifold may necessitate

each other, not in the way of differentiation, and may constitute a complex. A line has both length and direction (direction is either constant, straight, or changing, curved or crooked); neither of these is a differentiation of the other, though each necessitates the other. Thus a line is complex.

A rectilinear boundary (i.e. of a finite plane space) must have a number of straight lines, not less than three. The attribute of triangularity involves the attributes of linearity and rectilinearity; but the threefoldness of the sides (though some number or other is necessitated by rectilinear boundary) is not a differentiation of boundary or of rectilinearity or of linearity, and conversely. In this sense the attribute of triangularity, whether universal or particular, is complex. We have seen already in what sense it is not complex.

Contrast threeness. Threeness has no such complexity. The plurality in it, number, odd number, number Three, is one of differentiation only.

§ 282. If A necessitates B, the system AB is complex, supposing A other than B. (This does not include differentiation; for the species necessitates the genus and is not something outside the genus, it *is* the genus in a certain form.) Now it may be said that it is a part of the nature of A to necessitate B. But this does not make the 'Aness' of A complex. For, by hypothesis, the quality of A as A—its 'Aness'—is distinguishable both from B and from the necessitation of B. B is not a member of the 'Aness' of A. The necessitation of B is not a member of the 'Aness' of A. For A doesn't consist, in its nature as A, in the necessitation of B, or in B and the necessitation of B, for that leaves out its quality of Aness. We should therefore have to add this and say that the Aness of A consisted in the necessitation of B together with Aness, which is absurd. Thus the extension of a line necessitates points as its boundaries or extremities. But the extension doesn't consist in the points or in necessitating the points, nor in both, nor in both together with extension.

The A quality in A, then, is not made complex because it necessitates B which is not A.

We may apply this to show that an attribute is not complex because it necessitates a subject: by substituting the attribute

for A in the above proof and its subject for B. Surface is an attribute of a solid body. It does not consist in being the body, or in body together with being an attribute of the body, or in the latter. Any one of these leaves out its quality of being a surface. And we cannot say surface consists in being an attribute of a body *and* in being a surface.

This may be shown otherwise. Belonging to a subject is undoubtedly of the nature of an attribute. Now 'belonging to a subject' in general is not made complex by the reference to a subject—the subject is not a member of 'belonging to a subject' or of 'being an attribute'—and all that remains for it is 'being an attribute', that is, an identity.

Thus 'being an attribute' is not complex because the plurality it involves is not of elements which can be members of it. But any particular attribute is merely a species of attribute in general. Thus its manifoldness is that of differentiation and so not complex. Surface, e. g., is a species of attribute.[a]

An attribute again may necessitate other attributes, but this does not make it complex. Surface necessitates another attribute of a body, viz. volume, but, as surface, doesn't consist in volume or in 'necessitating volume', or in both; and, as before, not in necessitating volume *and* being a surface. This is sufficient to disprove Locke's view that a mode is a complex idea.

§ 283.[b] It is important to distinguish two kinds of complex idea or complex conception For clearness we return to the statement that the conception or idea of a complex is the apprehension of a complex object, and that this is the only intelligible sense of complex idea or complex conception. Observe that apprehension does not here necessarily mean perception. What is apprehended *must* be present to thought but it is not necessarily present to perception. Ordinary accounts of memory tend to confuse this. What we remember of a thing is certainly not the memory image, it is something *in* the thing. Thus what we

[a A lacuna is marked here in the manuscript

b The language here, as often above, is unnatural because the author has mechanically substituted the words 'statement' and 'state' for 'judgement' and 'judge' in his old draft The act is surely not an act of statement which is 'performed' but a comparison which is made, together with an identification. I have sometimes had to reintroduce the words 'judge', &c]

apprehend in remembering is the nature of the thing which is not present to us in perception.

Suppose I remember that certain words were spoken by a certain man at a certain time. I may imagine myself hearing them, but the fact which I say I remember, which therefore I am thinking about and which I apprehend, is not these imagined sounds. But it is just the apprehension of the fact which is necessary to memory; otherwise the mental images or imaginations would occur as the images in a mere reverie. When I say of them, in the case of memory, that they are like the past experience, I am in some sense (other than having this image) apprehending the past experience; I could not otherwise perform this act of statement.

The complex conception, or idea, being then the apprehension of a complex object, the true conception, or the conception in the most proper sense, is, obviously, the apprehension both of the members or elements of the complex and of the nature of their unity. Now in the case of a given complex object we may in perception apprehend its members or elements, and we may apprehend the fact that they are a unity, but yet not apprehend the nature of their unity because, as we say, we do not understand how they are unified In a solid, for instance, we do apprehend both volume and surface, and their unity also, for we see how one necessitates the other. So in 'triangle' we understand the unity of the three sides with the three angles, for we see exactly how the possession of three sides necessitates the possession of three angles. Suppose, however, that we apprehend in perception the fact that in a crystal substance a certain chemical quality is combined with a certain geometrical form. This apprehension does not contain the reason why this chemical substance crystallizes in this particular shape, and accordingly we have not an apprehension of the precise nature of the unity. There is therefore an undetermined and indefinite element in our apprehension. Our attitude, then, is that we apprehend the elements of the complex and the fact that they are unified but are uncertain about the nature of the unity. If we call this whole attitude 'conception', then, as contrasted with the full and proper sense of conception, we may call it an indefinite conception, or a partly determinate, partly indeter-

minate, conception. Inasmuch as the nature of the unity is a problem to us, this 'conception' might be called 'problematic', though that word is more natural for a second case of indeterminateness.

In this second case certain elements are apprehended but not even the fact of their unity; on the contrary, we are uncertain whether they can be unified in a given way. Here, then, the possibility of the elements being unified at all in the given way is a problem to us. If, then, this is to be called 'conception', it may be called a problematic conception. Both kinds of problematic conception have the verbal form of a definite conception, i. e. conception in what seems the primary and proper sense of the word. Thus in the statement 'a talking canary would fetch an enormous price', 'talking canary', which may be a problematic conception of the second kind, has exactly the same kind of verbal form as 'tangent line' or 'right-angled triangle', which are definite conceptions.

It is true that in what we here call a definite conception (like triangle) there may be said to be an indeterminate element (and indeed this is true of all 'definite' conceptions), because we do not know many things which triangle involves. For instance, before we apprehend the proof, we do not know the magnitude of the interior angles. But this indeterminateness is not relevant to the present distinction. We do not indeed apprehend the full nature of the three lines unified in a given triangle, or of the corresponding universal, but we have apprehended something in them definitely, and we also apprehend definitely the unity of these elements which we *have* apprehended. We know, in short, how these elements are combined or put together.

This distinction of problematic conceptions from others is obscured by the verbal form, since this may be identical in both cases. It is of the greatest importance for understanding the nature of hypothetical thinking. The want of it causes serious confusion in logic, as, e. g., in the theory of Definition. A type of the ordinary confusions thus caused is Mill's account of Definition. It is the same confusion which is at the bottom of the somewhat naive illusion, which affects even distinguished mathematicians, that it is possible to conceive a space other

than three-dimensional.[1] These problematic conceptions, if conceptions at all, must be classed as complex conceptions.

§ 284. The foregoing investigation was necessitated by the common doctrine that simple ideas cannot be made at all by us, whereas complex can, and that the latter may be fictions, while the former cannot. Having discussed the meaning of the distinction of simple and complex as applied to ideas or conceptions, we can now begin discussing whether either kind can be fictions.

The common doctrine above referred to, whether in popular form or as held by philosophers such as Locke and Hume, implies that the simple idea (or conception) is some mental and subjective existence different from the thing to which it refers and existing apart from it. This is treated as if it were somehow a mental copy of the thing; and, if we press for an exact meaning, it reduces itself to the mental image in imagination, as it is explicitly in Hume.[2] This image, however, we have seen, is *not* our 'idea of' or 'conception of' the thing.

On this account the doctrine involves two things which it either implicitly or explicitly treats as different: first the existence in the mind of the mental copy or counterpart of the thing, and secondly its validity in relation to the object or the reality, 'correspondence' or whatever else it may be called. So when it is said that the simple ideas are not fictions,[3] or that we do not make them,[4] it is meant that their existence in our minds is not our arbitrary creation. On the other hand, the way in which they are caused in us, the source of what we passively receive and do not make, guarantees their validity—this source is experience. This is their only source, so that the same cause makes them both not fictions and 'valid'. Conversely, as the idea is taken to be a mental something quite apart from its object, or the real thing, so an idea arbitrarily made by us would be, for these empirical thinkers, entirely independent of any relation to an object in the way of 'validity' or 'correspondence', and so with them fiction and invalidity go

[1] §§ 317–19.

[2] 'By *ideas* I mean the faint images' Hume, *Treatise*, I. i. 1. In Locke, the primary qualities really come to this.

[3] e g Locke, *Essay*, IV. iv 4.

[4] ib., II. ii. 2.

together. This indeed is not the strict consequence of their view; it would only follow that such a fiction might not be 'valid', not that it could not be. As to the 'source in experience' there is much confusion. If pressed, the theory in its usual form involves the tenet that the object not present to consciousness causes in us the idea of it which is present to consciousness, whence arises the familiar difficulty that neither the validity of the idea could be guaranteed nor even the existence of any 'object' of it. Or, as in one aspect of Hume's philosophy, the object itself may be made mental, an 'impression' existing only in the mind like the idea: the idea, then, becomes its fainter copy; and certainly such fainter copy, the same in kind as the impression, is *not* our conception of the original or idea of the original (which latter is itself, according to this theory, directly apprehended). Even if we allow this 'idea' (i e of Hume's) to be true imagination and not a fainter mental state of the same kind as the object or experience (which cannot possibly be imagination) the result is the same, this imagination is *not* 'idea of' or 'conception of' the thing or object. Since, however, experience naturally means direct apprehension of the object, the above difficulties are often obscured or avoided by ambiguous language such as 'got in experience',[a] 'got from experience', 'given in experience', and the like: where advantage is unconsciously taken of this natural meaning of experience.

§ 285. The general statement of the doctrine is, then, that *experience* is somehow the origin of the existence of the simple ideas, as these are understood by empiricists (and generally in the confused way which we have found in Locke), and also gives them validity. Now whatever may be meant by this, whatever 'being given in experience' means, the facts which the Empiricist refers to in this doctrine are such as what we call the experiences of an actual colour, of an actual sound, of an actual shape (with some Empiricists). And they mean that everything of which we have an 'idea' is experienced, in the sense in which colour, sound, and (with some) shape are experienced (seen, heard, &c.). Now in this sense of 'origin in experience' there are 'ideas' which do not thus originate in experience, because that of which

[a 'This relation (causation) arises entirely from experience, when we find that any particular objects are constantly conjoined' Hume, *Enquiry*, iv. 1]

they are said to be 'idea' is not experienced in this sense—as not seen, felt, heard, smelt, or tasted; nor are these ideas combinations of such as are given in experience in this way. This being so, whether they are simple in themselves or not does not matter. Such ideas are, e. g., those of substance and attribute, cause and effect, necessity in general. The development of the empirical philosophy begun by Hume was due to a growing consciousness of this fact. The development, however, failed to explain the origin of such ideas, and only ended in self-contradiction. The association of ideas, for instance, the favourite resource, either does not give any new ideas beside those found in experience, which form its material (and this is indeed the true alternative), or if it is supposed to do so, the admission is involved that the mind can make new ideas which it has not found in experience, and thus the premisses of the theory are directly contradicted.

In view of the doctrine that 'experiencing' of the kind meant (and best described by reference to the facts as above) is a source of certain 'ideas' and guarantee of their validity (waiving for the moment the question whether origin and validity can be separated in the manner described), we have to ask how such ideas as Substance, Power, &c., are related to experience conceived in this way.

§ 286. But it is useful first to consider a set of ideas of which also it may be said that they are not 'given' in this experience although they are not in the same relation to that experience as substance, cause, necessity, &c. These latter (i. e. substance, &c.) are what caused the first difficulty and misgiving to Empiricists;[1] but there is an abundance of other ideas in ordinary thinking about which there is a similar great difficulty, though the Empiricists are either not conscious of it at all, or only very imperfectly. As Plato pointed out, we do not, for example, observe or perceive 'equality', because we do not perceive two perfectly equal magnitudes. That is, in the language of these Empiricists, we do not 'experience' equality, or perfect straightness, or perfect circularity. Some Empiricists[a] have indeed

[1] Cf Locke's controversy with Stillingfleet, Hume on Cause.

[[a] e. g. 'Since, then, neither in nature, nor in the human mind, do there exist any objects exactly corresponding to the definitions of geometry.' Mill,

so far realized this that they are driven to say, like Mill,[1] that there are no real objects possessed of the attributes studied in geometry, which must involve *for them* the conclusion that there are *no* ideas whatever corresponding to the words. This conclusion is actually admitted by Mill, but he conceals from himself the contradiction in which he is thus entangled by devices of language and restores again to the mind as 'hypothesis' what he had banished from it as 'idea'.

But the usual answer (and the very one to which Mill himself has recourse, implicitly, later on in his discussion) would be that though we do not perceive perfect straight lines, perfect equals, &c., we do perceive the unequal and approximation to equality, the curved and approximation to straightness, and the imperfect *suggests* to us the perfect, i.e. the equal and the straight. But now suggestion, understood as these Empiricists would have to understand it (they have never really thought out what they mean by it), could only mean that what was suggested had been already experienced, and this is the real ground of Plato's doctrine of Reminiscence, into which, or something essentially like it, the Empiricist would be driven

The true answer, which is impossible from the point of view of such empiricism, conducts us necessarily to the recognition of a kind of apprehension which is not perception and therefore not 'experience' as understood in the view we are examining. It is granted that we observe or experience unequals and inequality But, for this, we must apprehend the things so observed to be unequal. Now inequality is unintelligible without the idea of equality, and therefore in such statements we must both have and use an 'idea', i e equality, which by hypothesis we do not observe or experience, and in this sense do not find in experience, i e in what we experience. In this sense the idea is not given in experience. Now to have the idea of equality in this active way is to know what equality is. This again does not mean to have a mental picture or an imagination of equality as distinguished from apprehension of actual equality, for that

[1] *System of Logic*, II. 5

System of Logic, II. 5, § 1 'It is customary to say that the points . . . exist in our conceptions merely,' id. ib., and the note in the same section, 'this unreal exactitude might be called a fiction' (9th ed., vol. I, p. 261).]

is not equality itself. To know what equality is we must apprehend equality itself; and thus the idea of equality is simply the apprehension of equality and nothing else but this apprehension. Thus the experience or perception of inequality itself necessitates the apprehension of equality, which the experience does not present. Further, if the experience guarantees the validity of the 'idea of inequality' (using the language appropriate to the theory we are criticizing), it certainly guarantees the validity of the idea of equality since the former necessitates the latter. And so for other 'ideas', such as 'straightness'.

The relation of such ideas to experience may be put also in this way: the apprehension of the objects of these ideas (i. e. strictly the apprehension of them which constitutes what are called the 'ideas' of them) is a necessary part of the whole apprehension of which the apprehension called perception or experience is the other part. Just on this account it is not in any way necessary to assume that we have the ideas in question before all experience, or that we could have them. They are no more actively present to us without perception than are colour and heat. If we like to call the whole act of apprehension above referred to experience, then we experience the objects of these ideas of equality, straightness, &c, as much as the unequal, crooked, &c. But this is not only not a fair use of the word 'experience' but tends to obscure an important distinction. What, of course, is right in the view of these Empiricists is their feeling that we have none of these ideas without experience, but they entirely fail to understand their relation to experience.

§ 287. The ideas above considered, or rather their objects, have corresponding to them in experience contraries to them more or less remote, which may sometimes be accounted approximations to them. But others, also not 'given in experience' (such as cause), have no such counterparts in 'experience', and this is their distinction from the first set We do not, e g, experience something approximating to causation or contrary to it. If we keep the meaning of the experienced which is proper to the empirical theory before us, Time is as much an 'idea' of this kind as is Cause. The Empiricists are strangely unaware of this. For instance, in the empiricist sensational Idealism, Time is always presupposed and yet, on their account of the data of

experience, it could not be among these data and would have to be accounted for by the Association of Ideas. This difficulty, however, never occurs to them. Time is not seen, felt, heard, or experienced in general, in this sense of experience; it is apprehended along with the apprehension called experience, and as apprehended it is an apprehension of particular time. Here again, as in the case of the former set of 'ideas' or objects, we cannot have the one apprehension without the other. Properly, neither apprehension is more 'valid' than the other, in the sense that one is more certainly of a real object than the other. But if it is held that the apprehension of experience is 'valid', or guarantees the corresponding 'idea' as of a real object, then it guarantees Time. For that which is necessitated by what is taken to be real must itself be taken to be real. Now the objects experienced necessitate Time. We, on the other hand, apprehend this, and our apprehension is necessary as being of the necessary. This proper account is inverted when we represent it as a necessity of thinking as distinguished from the object, and so virtually as primarily a subjective necessity. In recognizing or apprehending Time we are simply apprehending side by side with experience a side or aspect of the object we are experiencing, which aspect is not experienced. Time, then, is not an 'idea' or 'conception' given in experience as simple ideas are supposed to be, but must be as 'valid' as that which is given in experience. Causality implies Time and it is not mere necessitation of events, or of their order, but implies the distinction of events which happen from objects to which they belong and which do not happen. The fact again of the existence of such objects, identical in change, could not be experienced in the sense in which we are now using 'experience'; and this contention is borne out by the failure of Locke to get the ideas, e. g. of substance and power, out of experience as he understands it. Some Empiricists might perhaps hold that we do experience permanence, or identity in change, when we apprehend the movement of a body. But, if this is denied to be experience, the events apprehended in experience at all events necessitate this belonging to an object, and this, we contend, we do apprehend even if we do not experience it.

Causality will ultimately be found to mean that the events

belonging to an object, or to a system of objects, have a definite order, that is, therefore, a necessary order. There is a reason why a certain event happens in the particular order it does in relation to the other events of the system. We apprehend this necessity as belonging to the order of events: we apprehend it as much as we do the events, though we do not apprehend it in the way of experiencing it. And as before, therefore, experience guarantees it, if it can guarantee anything. Here again the true relation is inverted and the apprehension, because it is held not to be experience, is represented as in the first place a necessity of thinking (and hence arises a quite false problem as to how reality has to agree with this necessity of thought).[1] There is in this case a special temptation to the fallacy, which we shall come to presently. We repeat that our thought about the object is necessary because it is the apprehension of something necessary. Necessity must lie in the object apprehended: for apprehension as distinguished from that has no content whatever. Whence necessary thought, as what we are obliged to think about an object, can only be the apprehension of a necessity in the object.

But, now, we neither apprehend in experience, nor do we apprehend otherwise, the particular way in which a particular event is caused: we apprehend a necessity in general, i. e. that the event must have some cause or other This is a universal and there is therefore a special temptation to make it subjective, because of the common mistaken tendency to regard universals as merely subjective existences. The example of the contrasted set of 'ideas', such as straightness and equality, shows that there is nothing strange in the statement that we can apprehend something in an object of experience, which 'something' we do not experience, and that we can thus apprehend a universal. For in the experience of two unequals we apprehend their relation to equality; this relation in them we do not, *ex hypothesi*, experience; we must also apprehend equality which we do not experience, and this is not a particular equality but a universal.

[1] A false problem is one in which we put as a question what cannot possibly be a matter of question It therefore always admits a *reductio ad absurdum*, which could easily be given in the present case.

If it be said that what we apprehend in the object of experience must be a part of our experience of it, then inevitably it would result that we experience time, the identity of a subject in its changes, or of a system of subjects, and that we experience the fact that events must be 'causally' connected. And if experience is of the real, these will all be experienced realities. But as the example of equality shows, we cannot properly call experience all that we apprehend in an object of which some aspects are 'experienced'.

So far, then, from it being difficult to relate time, causality, &c., to objects of experience or to account for their validity, they must either be considered to be as direct objects of experience as anything else, or to have their validity guaranteed by that of experience, if experience can guarantee anything

The real reason why people make experience the ground of knowledge is that they think we directly apprehend in experience real objects, and this, as already noticed, is the natural meaning of experience. But though this is the only thing which makes their appeal to experience intelligible, they often either obscure it, or are actually not entitled to hold it, because of their theories of perception.

Our conclusion, then, is that *if* experience is of real objects and their properties, or if the objects of experience are real, it inevitably follows that both these groups of ideas, equality, perfect straightness, time, cause, &c, are properties of real objects.

It must be remembered that in any case the object cannot apprehend itself: we must do that. Therefore such appeals to experience, and indeed any appeal to experience, imply that our apprehension of the object is final and authoritative, in some respect, whether it be of the quality or kind of quality of the object or at the lowest of the existence of the objects. It follows that our apprehension must also be authoritative in respect of that which we apprehend as necessary to the objects of experience.

§ 288. We return now once more to the question whether a simple idea can be made. A simple idea may, as we saw, necessitate a complex idea, so that the one question involves the other, whether, that is, the corresponding complex idea can be made. The sense of 'making' which seems most intelligible

is that of composing something out of pre-existing materials which are not made. But if ideas were so made they would be complex, and thus we are anyhow, apart from the theory to which we have been referring, led to consider whether we can make new simple ideas.

For our purpose, which is to find whether there is any element in an hypothetical statement which may properly be called a fiction, or fictitious, we might be content with the fact that no one believes it possible to make a simple idea, and that there is no record of any one ever having made one. At first sight it might seem enough to say that the absolute origination or making of an idea out of nothing is inconceivable and unintelligible. Yet it is worth while considering the question for its own sake. These are vague words. 'Inconceivable' may mean that we don't know how the thing could happen, or that we know it to be impossible as contradicting something known. 'Unintelligible' may mean that we do not understand it, or that it is contrary to reason, which again means contradictory to something we know to be true. There is always a danger of passing from one of these meanings to another. As to 'origination', it is important to remember that we are accustomed to attribute origination to ourselves, and yet the way in which this happens may be said to be inconceivable and unintelligible in the first of the above senses of these words We suppose ourselves to originate the movements of our limbs; but we do not understand how we do it, nor can we even form a conjecture. If this is immediate origination, then at all events immediate origination, 'out of nothing' as the phrase is, would be a fact. But what is more important and more to our purpose is that we certainly suppose ourselves to originate trains of imagination and trains of thinking, where, again, there is immediate origination or, if not, we at least do not understand at all how they are originated. It is decisive to say that a thing is inconceivable only if this means we know it to be impossible because we know it contradicts something that we know for certain. Thus in the case of this example, if we are to pronounce it inconceivable, we ought to know that the nature of the thing itself does not admit of making or origination.

If we consider some ideas that we have, we certainly do see

that they are incapable of being made or originated at all in time; this is the ordinary way of speaking; it is more accurate to say that their objects are incapable of it. Unity, plurality, the number three, &c., are universals which as such cannot have any kind of beginning in time. In the nature of the case they are unchangeable. And it might be contended that for any idea whatever, supposed to be made, there must be a universal which could not be made. But, for our present purpose, it is not necessary to insist upon this. The question cannot be properly discussed unless we get rid of the confusion caused by the false representation of 'idea' as a mental existence which we can have apart from any reference to or apprehension of an object. A definite idea or conception being nothing but the apprehension of a definite object, the question whether an idea can be made arbitrarily is the question whether the apprehension of an object can be made, and this, again, really means whether the object apprehended can be made by the apprehending mind. Object in the wider sense includes states of ourselves, feelings, emotions, which can be apprehended, and objects not states of ourselves, the latter, narrower, sense being the usual one. From these, both of which are called objects of experience, are distinguished *imaginations*; imagined sensations, and imagined objects[1] in the narrower sense. For clearness we may repeat that the definite idea or conception of an object is not the 'imagination' of it, but the apprehension of what it is itself

We may apprehend an imagination, or mental image, as an imagination, and this apprehension of its nature is the 'idea' or conception of it. To make an idea, then, we must make its object. Suppose we could make an object in the narrower sense, it would of course be real and the idea of it, i.e. the apprehension of it, the apprehension of something real. If we could originate a sensation, the same would be true of its 'idea' or conception. As to imagination we might here again be content with the universal admission that we cannot imagine anything simple except as the imagination of something already experienced. But suppose we could imagine something quite new, it would be something existing as our imagination; the idea of it would be the apprehension of it as something existing in our imagina-

[1] On the difference between imagination and experience, cf § 137.

tion. By 'fiction' is usually meant something mental, not corresponding to reality. In the first two cases the thing made (an 'object' or a sensation, e. g.) would be a reality and therefore not a fiction and the idea of it (i. e. the apprehension of it) would be in no sense a fiction. In the third case the imagination would not have any known reality (= 'object' or sensation, e. g.) corresponding to it and, in that sense, would be called a fiction; but it would certainly be an existence, and the idea *of* it, though *not* the apprehension of 'reality' in the above sense, would be the apprehension of an existence. Thus even here the 'idea' or 'conception' would not be either untrue or without relation to truth (that is associated with fiction and the fictitious), nor would it even be the conception or idea of something untrue

Now that we see what 'making' an idea would have to mean, it is clear that if a definite simple conception could be made—and in the nature of the case all simple conceptions must be definite—it could not be something false or even doubtful, or a pretence that something was real which was not. In short, it would have none of the usual associations of the word fiction and fictitious. This, then, is the important matter; not whether it can or cannot be made. Thus in an hypothetical statement, or indeed in any statement, there can be no simple idea which can be called false, or doubtful, or a fiction, in the sense of the pretence that something exists which does not exist.

§ 289. We come now to the complex ideas. It is both popularly and in some philosophical systems taken for granted that the mind can 'make' complex ideas, in the sense that it can arbitrarily conjoin simple ideas, irrespectively of whether the corresponding conjunction (to use the language of these systems) has appeared in experience or not. It would follow in this case that there could be uncertainty about any correspondence in reality to these complex ideas. Such ideas, then, might be called fictitious, as indeed they usually are.

We shall, however, find the truth to be that we have no arbitrary power at all of thus making complex ideas, and that, in the only sense in which we can be said to make them, the making is necessary and the result not only not fictitious but necessarily real. For if we really put together two or more simple con-

ceptions into a conception which is truly one, the conceptions must be put in organic connexion. But such organic connexion depends on their own nature, and we manifestly could not put them into a connexion of which their nature did not admit. Observe that the connexion cannot be merely their being somehow together; if we think the connexion at all (and we do not put them into connexion unless we think the connexion) we must think of it not as connexion in general, but as a particular form of connexion. When, for instance, we connect the possession of three angles with the possession of three sides, we see *how* the one fact is connected with the other. The connexion of them will be one which their nature necessitates and therefore as real as these elements themselves which are connected. This is illustrated in geometrical demonstration. For in this we really advance to new complex conceptions, not by experience but by our own reasoning processes; and, in this sense, viz. as not merely taking from experience but advancing to a new connexion of elements by our own thought, we may be said to *make*. Moreover, what we make is accepted without hesitation as true and as real; at all events, taken to be everyway as true and real as the elements from which we start.

We have obviously, on the other hand, no power to put elements together either in a way known to be contrary to their nature or in a way about which we are uncertain whether it is possible for them or not. For, in the first case, we should be thinking the unthinkable; and, in the second case, we have not really put them together. In the second case, at the best, we only know that we see nothing in the conception which prevents the connexion, but we know no more. Our frame of mind is not conception or statement, but interrogation or wonder. We merely wonder whether the connexion is possible. The second case, then, is clearly that of conceptions which we have called problematic: they have only the verbal form of a conception. The common error is to take them for conceptions proper, which involves considering them as fictions which we have made without reference to reality. But it is now obvious that it is a mistake to suppose them 'made'. they are not made at all and are therefore not fictions. Instead of their representing a connexion made between known conceptions, they merely represent the

state of mind in which the connexion is problematic to us; a state of mind in which, on the one hand, we know nothing against the connexion and, on the other, do not see its possibility (i. e. necessity). We do not here 'make', we are only in doubt whether the connexion can be made. Particular examples show this at once. Thus in no case is it really possible for the mind to produce a merely fictitious idea, whether simple or complex. In the sense in which a complex idea can be made, the idea cannot be a fiction but must be as real, or apply as much to reality, as any other idea.

§ 290. The preceding argument is expressed in the language usual about conceptions and, as such, is of polemical use,[a] because it does not involve an appeal to the doctrine about the true nature of an idea or conception, which we ourselves maintain. It requires to be restated in the terms of this doctrine, for the ordinary language is, in the end, likely to mislead by suggesting the common confusion about the mental existence of ideas and conceptions.

A complex conception or idea is nothing but the apprehension of a complex object in which 'simple' elements A, B, C, &c., are united or unified in some particular way. The apprehensions of A, B, C, &c., severally may be called simple ideas as apprehensions of simple elements whether universal or individual. Obviously, then, the complex 'idea' is not properly speaking a '*combination*' or 'complex' of the simple ideas corresponding to it, but rather the idea of the complex which the objects of these simple ideas form; not, that is, a complex of these apprehensions of the simple elements but the apprehension of the complex formed by these simple elements. At all events, to call the complex idea a complex or combination of the simple ones is, at best, an obscure and misleading way of saying that the object of the complex idea (or apprehension) is a certain kind of complex of the objects of the simple ideas (or apprehensions).

Suppose, now, certain simple ideas α, β, γ are given. These are respectively apprehensions of objects A, B, C, &c. The question, then, whether there can be a complex idea combined

[a In truth, I believe, an embodiment of his own earlier treatment of the subject. See p 814]

of these simple ones is only intelligible as being the question whether the objects A, B, and C form a complex in a certain way, i. e. with a certain kind of unity, and whether we can apprehend this as a definite conception, can, that is, both apprehend the elements and the nature of their unity. Can we see *how* they form a unity of the given kind?

But whether A, B, and C form a complex of the given kind, or can form it, depends entirely upon their own definite nature and not on any act of the apprehending mind. The nature of the elements themselves determines necessarily what unity they have or can have, and thus the mind has no power whatever arbitrarily to effect a combination of them (the necessary prerequisite of the complex idea), since that is only the apprehension of such combination. Thus the mind has no power whatever to make a complex idea, and therefore, in any intelligible sense of the words, no power whatever to *make* a complex idea out of simple ones

As to the sense in which we may be said to make a conception or a demonstration in the exact sciences,[1] this again is an inaccurate and misleading use of words In the reasoning process, though as such it is not experience, we are simply apprehending what the objects, with the apprehension of which we start, necessitate through their own nature and we no more 'make' what they necessitate than we make them. If we do not know the nature of the objects sufficiently well to know whether they can be unified in a complex of the given kind, the possibility of such unification of them is a problem to us and, if our attitude is to be called idea or conception at all, our conception must be what we have named problematic conception or question-conception.

[1] Compare § 289, p 522, beginning 'This is illustrated .

VI

HYPOTHETICAL STATEMENT AND HYPOTHETICAL ARGUMENT

§ 291. UNCERTAINTY, then, which is a condition of the hypothetical element, and not fictitiousness, in the sense explained, attaches to problematic conceptions. Nor can we possibly think them really valid, although we may incline to the belief that they are, or, as the phrase is, believe they are, or think it very probable that they are. Where the belief is not justified this *appears* to introduce a fictitious element into the 'conception' (as it is called). But this is a mistaken way of looking at it. The belief does not any the more enable us to effect a connexion, whose possibility for the elements we do not know; we have not thereby 'made' the conception and there is no fiction or error in it. The error lies in the belief.

Thus, if A and B be conceptions in the primary sense, that is, definite conceptions, whether simple or complex, and we mistakenly think of their connexion in a unity, it is the belief that A and B are compatible in a certain kind of unity which is erroneous. This we may symbolize by AxB, putting x (as a symbol of the unknown) for the connexion, or by A ? B. The conception AxB is not erroneous and therefore made wrongly and therefore a fiction; for the conception corresponding to AxB has not been made at all

The consideration therefore of the problematic conception reduces itself to the consideration of a judgement (so called) about probability, or rather of a *belief* (for we shall have once more to question the use of the word judgement for belief at all).[1] What we have to consider are such statements as 'all (some) A is probably B'; 'it is probable that A and B can be combined in a certain kind of unity'; 'I believe that A and B can be combined in a system C'; or, as will be seen, the true character of an hypothetical clause such as 'if A is B'.

[1] §§ 48 et seqq

§ 292. The hypothetical statement has for its hypothetical clause 'if A is B' or 'if A exists'. When A is a simple conception or a proper complex conception, as opposed to a problematic conception, the latter form (*i. e.* if A exists) is not admissible, because it cannot be a question whether an object of the conception exists and so the hypothetical form is merely verbal. When A, then, is a conception in the true sense, whether simple or complex, the only true form of the hypothetical clause is 'if A is B'. When, on the other hand, AB is a complex conception, where the connexion of A and B is problematic, 'if AB exists' is a correct form: yet not only is this equivalent to 'if A is B', but this latter form is the simplest and most natural verbal expression. If AB exists presupposes in fact 'if A is B', in cases where AB is not only a problematic conception but is *put* as a problematic conception. Thus there are two types of hypothetical clause involved, 'if A is B' and 'if AB is C'. The latter, again, is equivalent to 'if A is B and C', or 'if B is A and C'. Thus, in every case, the hypothetical clause can be expressed *without any problematic conceptions whatever*. Moreover, this expression is the most natural and adequate because the problematic conception, not being a 'conception' in the normal sense but only simulating a conception in its verbal form, ought to be replaced by the hypothetical form which is adequate to it.

It follows therefore that the hypothetical clause cannot refer to what is merely fictitious or even to that the existence of which is merely uncertain. It always relates ultimately to conceptions of whose validity we are certain, and the uncertainty is not as to whether the objects of these can exist, but as to whether they can be combined, or, in general, stand in a certain relation to one another [1]

§ 293. There may of course be a belief that a certain problematic conception is realized. Then we may have a statement of non-hypothetical form 'AB is C', where the speaker does not intend to put AB, though really problematic, as problematic, in accordance with his belief that AB is really existent. From this point of view, then, the statement would not be resolved

[1] We might equally well have taken for the general form of the hypothetical clause 'if A stands in the relation R to B' We should have arrived at the same result

into 'if A is B, it is also C'. Yet for strict accuracy and for the purpose of scientific thinking, the only true form is the hypothetical. Thus for 'AB is C' must be substituted in strictness 'if AB is possible, it is C', or still more accurately 'if A is B, it is also C', or 'if B is A, it is also C', or 'if A is B (in any case), that which is both A and B is also C'.

Similarly in the hypothetical clause 'if AB is C', when AB is really problematic, the speaker may believe that AB is realized or realizable, and so, from his point of view, this hypothetical does not presuppose 'if A is B'. But nevertheless, as before, it must in accuracy be resolved into 'if A is B and C', or 'if B is A and C', or 'if $\left.\begin{matrix}\text{B is A}\\ \text{A is B}\end{matrix}\right\}$, and that which is both A and B is C'.

In both cases, to express the full thought a statement should be added to express the belief that the conjunction of A and B is possible, or that A can stand in the given relation R to B.

§ 294.[a] Thus, in every case, the consideration of the meaning of the hypothetical clause in an hypothetical statement resolves itself into a consideration of the meaning of the form 'if A is B', where A and B are both conceptions proper, of whose validity, therefore, there is no doubt.

We must now try to determine precisely the nature of the thought, or element of thought, of which this form (if A is B) is the expression.

It is necessary to the hypothetical clause 'if A is B' that there should be uncertainty whether these two elements of reality are compatible, but it is clear that in the hypothetical clause we are not concerned merely with this; there is something more positive, and our question is what does this more positive something consist in?

Take 'if A is B, C is D' as the general form of hypothetical statement, the elements A B C D all being real. Let us ask first: 'What is it about which there is a definite statement?'

It may perhaps be said that we 'predicate' of the statement 'A is B' that the statement 'C is D' is a consequence of it. The point has already been settled, but we may add the following consideration. The inadequacy of such a representation will be

[a Re-written in 1906. It is termed by the author 'Aporematic discussion'

evident if we compare the statement 'because A is B, C is D', which implies the truth of 'A is B', or the realization of AB. Of this it might equally be said that it asserts that the statement 'C is D' is the consequence of the statement 'A is B'; and in this way the same explanation would be given of two very different forms of very divergent meaning.

But perhaps it may be said that this is so, only that the latter form with 'because', instead of 'if', implies that the statement 'A is B' is true. This involves what seems a paradox, that we can assert something of a statement quite apart from its being true or not; as though a statement could be a statement irrespective of considerations of truth or falsity. We have already seen that this paradox and mistake come from supposing that 'A is B' and 'C is D', in the form 'if A is B, C is D', &c., represent statements; and we may here add that even the causal form 'because A is B, C is D' does not really assert anything about the relations of statements as such.

In such a statement we apprehend, or recognize, or suppose ourselves to apprehend or recognize, a fact, the fact is not our recognition of it. Now in the form 'because A is B, C is D' we obviously assert that the fact that A is B (not our statement that A is B) necessitates another fact, that C is D. Thus the statement expresses a relation between the facts, A's being B, and C's being D, between two objective realities, and not between two statements which are our subjective acts—our subjective recognition of the facts. It does not even follow that the statement 'A is B', as our subjective act, necessitates that we should pronounce the statement 'C is D'; for though, if we pronounce the first statement, we ought to pronounce the other, it does not follow that we shall. The connexion may be a matter of demonstration which we have not got. It is the fact recognized in the statement 'A is B', which necessitates the fact of C's being D, whether we recognize the latter in a statement 'C is D' or not.

Again the statement 'if A is B, C is D' can appear as the ground of 'because A is B, C is D'. For if the hypothetical statement is true, and if in any case 'A is B' is true, it will follow that 'C is D' will be true. Thus 'if A is B, C is D; but A is B: therefore because A is B, C is D'. Since, then, a state-

ment which expresses a relation between facts can be deduced rightly from the hypothetical, it follows that the hypothetical must also somehow express a relation between facts. The same also follows directly from the statement itself. For its truth obviously depends upon and is entirely conditioned by the real nature of A, B, C, and D, and can only be arrived at by a consideration of what their real nature necessitates.

Nevertheless the statement does not mean that the *fact* of A's being B has a certain consequence, for it is implied that we do not know whether A is B. The 'if' relates somehow to our subjective point of view, as there is no possibility of alternatives in reality. Can we, then, say that in the hypothetical statement the two sides are so combined that the nature of the facts, as we know them, and the relations between them determine relations between our subjective statements or suppositions? For instance, does the statement mean that our supposition that A is B involves the supposition that C is D? In the ordinary meaning of 'suppose' we may suppose 'A is B', but this is not necessary to the hypothetical statement: for in pronouncing 'if A is B, C is D' we may not suppose A is B but incline neither way, or we may actually suppose A is not B, as often when we are hoping to find a *reductio ad absurdum* argument to confirm our own position. Does it, then, mean 'IF we suppose A is B, we must properly speaking also suppose C is D'. Here, however, we are repeating the IF in what should be an explanation of it. Again, though the statement would be true, if the hypothetical statement corresponding to it were true, it seems clear that it is not the primary meaning of the hypothetical statement but a deduction from it. The primary meaning, if we keep the IF which the proposed equivalent also keeps, is that 'IF A in *reality* is B, C in reality will be D', and it is on account of this primary meaning that if we also suppose A is B, we should also suppose C is D.

The hypothetical, then, does not seem primarily to express a relation between suppositions. Does it, then, express a relation between statements? This question has been answered in the negative, but we may add here that to obtain any relation of statements we should have to introduce the IF again, as before: 'If the statement "A is B" is pronounced, so must the

statement "C is D"', and the same criticism holds as in the former case. Notwithstanding the IF, then, the hypothetical statement 'if A is B, C is D' does not primarily, it would seem, express a relation between statements nor a relation between suppositions, understanding 'supposition' as above.

We may here repeat and apply a former consideration: If we compare again 'because A is B, C is D' with 'if A is B, C is D', the first expresses something which is the consequence of the real natures of A, B, C, and D, and is a statement about the nature of reality, not about our subjective attitude to reality; and the statement is. 'The fact that A is really B necessitates the fact that C is really D.' So also the second expresses something which is a consequence of the real natures of A, B, C, and D. It means in its simplest and most natural expression that A, B, C, and D are such in reality that, *if* A were really B, C would really be D. This, too, is a statement about the nature of reality.

The IF, as already said, seems to have a subjective reference, but so far we have not found a subjective reference that would suit the meaning which we know it to express. We have still to ascertain the meaning of the IF and to find what is the exact attitude of mind, corresponding to the hypothetical clause 'IF A is B'.

§ 295. 'If A *is* B' is obviously not the same as 'if A is *supposed*, or *assumed*, to be B' The idea of supposition or assumption (we are not identifying these two terms) must therefore be found in the IF, if anywhere. Consider the form 'if A is B, C is really D' or 'if A were B, C would really be D' Substitute for the hypothetical clause the words 'suppose that A were B' and we get 'suppose that A were B, C would *really* be D'. This is obviously not justified, but only 'supposing A were B, C would have to be supposed to be D'. Thus we cannot get the true form of the hypothetical statement by such a substitution for IF. The same happens if we substitute for the 'if' clause the words 'assuming A to be B'.

However, the words 'assume', 'assuming', and 'suppose', 'supposing' are ambiguous. They may mean 'if we suppose, &c.', and this would reintroduce the IF, instead of giving a substitute for it, and further 'if A is B' is clearly not the same as

'if we suppose A is B'. They may mean 'whenever we assume A is B', or 'in the cases in which we assume that A is B', and then the above criticism will hold.

The hypothetical may be correctly expressed thus: 'If A is really B, it follows that C is really D', it being uncertain whether A is really B.[1] Thus it expresses that something follows from something or that something necessitates something. Now in the statement 'if this triangle is rectilinear', &c., substitute for the hypothetical clause the words: 'from the supposition that this triangle is rectilinear', &c. This is clearly untrue: it would only be true that 'from the supposition, &c.', it follows that it must be *supposed* that the three angles of the triangle are equal to two right angles. For a supposition can only necessitate another supposition.

But it may be contended that the real meaning of the hypothetical is that one assumption necessitates the other thus: 'The assumption or supposition that this triangle is rectilinear necessitates the assumption that its three interior angles are equal to two right angles. But then we should have to explain this relation asserted between the two assumptions by adding 'for, if the triangle is rectilinear, its three internal angles are equal to two right angles, and therefore if we suppose the one we must suppose the other'. The assumption 'A is B' necessitates the assumption 'C is D', for if A is B, C must be D.

Thus the original form with IF appears to be ultimate. For, in order to explain the relation between our assumptions, we have to appeal to a relation between the facts to which they relate, and for this the IF is necessary.

Consider again what it is that is necessitated in the hypothetical statement, 'if A is B, C is D'. It is not that C is D. For then the statement would imply that C is D is true, which it does not, that is only true in the causal form, 'because A is B, C is D'. What the statement tells us is that C's being D *would* be *necessitated*, IF a certain condition were realized. What, then, is involved as absolutely necessary in the hypothetical statement? It might be replied: 'the following of the con-

[1] e g. 'If this triangle is really rectilinear it follows that its three angles are really equal to two right angles', it being uncertain whether the sides of the triangle are really straight.

sequence (C is D) upon the realization of the condition is asserted as necessary and unconditional in the hypothetical statement, that is to say, 'if A is B, C is D' asserts it to be necessary that the realization of the condition 'A is B' should be accompanied by the realization of the conditioned 'C is D'. The 'realization' of the condition, however, here means, '*if* the condition is realized', and once more we cannot really explain the manner in which necessity enters into the statement without using the word 'if' and presupposing its signification.

§ 296. To understand better what seems to be the attitude of thought in relation to the hypothetical clause 'if A is B', a clause which has a meaning only in relation to the hypothetical statement of which it is a part, let us consider the process of inference, in which the hypothetical statement has its main use and in which also we shall find its origin. We may in this way get some light upon the meaning and use of the words 'supposed' and 'assumed'. As already pointed out,[1] we have only to consider the case in which A and B are both known to be realizable, so that so far there is no uncertainty about them. The elements A and B being elements of reality we are uncertain whether they can be combined. Let us for simplicity take the case where the combination corresponds to our merely asserting B of A; where, that is, A and B are combined in the unity of every subject which is A. Let us also take again the case where the further process of thinking, after we have realized this uncertainty, is directed to removing the uncertainty, or deciding whether A can or cannot be B. The reasoning may have one of two forms, viz either: Regress through the conditions of A's being B, to find a condition known to be realized, from which therefore A is B would follow, as thus: 'C's being D is a condition from which A's being B would follow, E's being F is again a condition from which C's being D would follow; suppose it is known that E is F, A's being B follows'. The argument would have the form:

If C is D, A is B;
If E is F, C is D;
But, E is F:
Therefore C is D;
Therefore A is B.

[1] § 294.

Or, the reasoning may progress through the consequences of A's being B in order to find a consequence known to be impossible and thus decide that A is not B; thus:

If A is B, C is D, — If C is D, E is F;
But E is not F:
Therefore C is not D;
Therefore A is not B.

Let us examine the second of these cases We start with the uncertainty above described about A's and B's combination, whether, that is, A can be B. This uncertainty, being mere absence of knowledge, can give us nothing to start from; we can get no new statement and make no advance from that. The only possibility of such advance lies in our knowledge of the certain elements A and B, which, of course, as certain knowledge, is expressed in non-hypothetical statements. And thus the only advance in reasoning that is possible is by non-hypothetical statements into which A and B enter. Thus, suppose we know that C necessitates A and B necessitates D. This enables us to take the next step in the argument, 'If A is B, C must be D'. Here we observe the origin of the hypothetical form; it depends on and is conditioned by certainty, and it does not appear until we have some certainty. But how do we take the step in which the IF appears for the first time? We are uncertain whether A can be B, we possess the knowledge that C is A and B is D. Perhaps the usual account given would be that we proceed to *suppose* that A is B, or, to *assume* 'A is B', and then draw from this the inference that C is D; and in ordinary language A's being B would be said to be the supposed or assumed case. This, we may note, seems especially natural in the second instance, inasmuch as it turns out that A is not B.

But what is the meaning of 'suppose'? The meaning it most commonly has is 'to think something is probable'. Thus to 'suppose' A is B would mean to think it probable that A is B. But, as already said, this 'supposition' is not necessary for the statement 'if A is B, C is D', the validity of which is no way affected by the fact of our thinking 'A is B' to be probable or improbable. Moreover, this is so much so that, even if we do not believe that A is B (though *ex hypothesi* we don't *know* that

A is not B), we can still form the hypothetical statement, and actually do so, in order to arrive by a progress through consequents at a confirmation of our belief that A is not B.

However, it may be said there is another use of 'suppose' in which (whatever else it means) it does not mean to think something probable, and it is this meaning which is the only kind of meaning relevant to the present question.

We may begin the refutation of an assertion 'A is B' by saying 'let us *suppose* A is B', and continue 'then it follows that C is D, for C is A, and B is D'. That does not mean 'let us think that A is probably B', for then the inference could not be 'C is D', but at most 'C is probably D'. Nothing indeed seems to do short of 'let us think that A is really B'. But now we may suspect here some inaccurate or at least artificial use of language. The proposal to think that A is really B cannot possibly be carried out. We are, *ex hypothesi*, uncertain whether A is B. That is, we do not think or conceive the connexion or unification of them in one subject, and we cannot think A is B without thinking this connexion. Nor will it do to say that we 'imagine' that A is B. We may imagine A and B, if this means to present to ourselves images of individual realizations of A and B, but of course we cannot imagine the connexion of which we have, as explained, no conception and therefore no image. Nor indeed would such 'imagination' be of any use in inference, unless accompanied by statement. It may perhaps be said that we create the fiction that A is B. But our criticism of fiction has shown that there can be no such thing as a fictitious idea. It would have to mean, if it meant anything, the making of a mental image, i.e. the imagination just criticized, accompanied in some way by the statement that A is B, a statement that by hypothesis we do not make.

One way of putting this would be to say that it is a fiction of ours that A is B. But what can this mean? That we *pretend* A is B? We may pretend by a verbal statement to others; but we cannot pretend to ourselves. We cannot pretend to ourselves to think that A is B, inasmuch as we have proved we do not think A is B and cannot think it with our data. Language here appears to encourage the same illusion as in the case of the problematic conception, which has the verbal form

of a conception which is really thought, although in fact it is not so thought.

But perhaps it may seem that the idea of fiction or pretence is after all only an inaccurate way of putting something that is true about this form of reasoning (where we say 'let us *suppose* A is B'). It may be said we do not pretend 'A is B' or pretend that we have made the statement 'A is B', but we treat 'A is B' as if it were a statement, viz. by inferring from it. Just so we may treat a man as we should an honest man, without assuming that he is, or believing he is, or pretending he is; or, a chemist may give some unknown substance the treatment which would yield a certain kind of result in a certain kind of substance, without supposing it is of that kind, in order in fact to test it. But these analogies are fallacious.

How do we propose to treat as a statement what is not a statement? And more difficult still to answer—*What* is it that we propose to treat as a statement? The word-form 'A is B', when we say 'treat "A is B" as if *it* were a statement', seems to give but really does not give the 'it' some definiteness, for '*it*' is not represented by 'A is B', which properly represents a statement, whereas, as we have seen, 'A is B' does not, in the context 'if A is B, C is D', represent a statement. The words simply correspond to my questioning whether A is B, and to this we cannot apply the treatment applicable to a statement.

Again, let it be said that we treat 'A is B' as if it were a statement by inferring from it. This taken literally means that we infer from something which is not a statement. But it is only from a statement that we can infer, i e the proposed treatment is only possible for a statement. Just so there are ways in which we cannot treat a man believed to be dishonest like a man who is honest; we cannot respect the dishonest man. To avoid this it might perhaps be said that having the statement 'B is C' we infer as if we also really had the statement A is B.[1] But this could only mean that we inferred outright that A is C. We do not do that, for the uncertainty as to whether A is really B makes us also uncertain whether A is really C.

[1] The form implied here is the following 'If A is B, A is C because B is C', or the form given in the next section. 'If A is B, A is C, for all B is C'.

§ 297. Take the simple case, 'If A is B, A is C, for all B is C; A is not C, therefore A is not B'. We begin with uncertainty as to whether A is B, we have the certain non-hypothetical statement all B is C We consider whether A is B, and we have C as a mark of B, or a necessary, though not therefore sufficient, condition that a thing should be B. We look to see whether A has the mark, find it has not, and decide therefore our question in the negative, i.e. that A is not B. For the reasoning the following is sufficient—All B is C, A is not C: therefore A is not B. Here the statements are non-hypothetical; and as for the rest of the process there is no supposition, or assumption, that A is B, no conception of A as B, no fiction. This helps us to see that there is no 'supposition' or 'fiction' necessary: for the process, as above described, which precedes this reasoning also shows none.

But, nevertheless, this reasoning does not represent the whole mental process. Being uncertain whether A is B, we ask what B necessitates in order to see whether any of its consequences are found or not found in A; because, e.g, the absence of such consequences would decide that A is not B. The reason therefore why we look for consequences of B (a reflection therefore which must *precede* such search) is that we reflect that IF A is B, it must have the consequences of B. Thus this hypothetical statement starts the whole process of search or inquiry, and therefore instead of being superseded by the above statement of the non-hypothetical argument, it truly conditions the appearance of such argument.

§ 298 This brings us to an important characteristic of an hypothetical statement, viz. that it is *always* an *inference*, and an inference from what is non-hypothetical. It is an inference which results from our knowledge of reality and its form is due to the combination of the knowledge about something with an uncertainty about it. Knowing all B is C, questioning whether A is B, and therefore not possessing the premiss all A is B, we do not infer A is C, but only, if A is B, A is C. Thus and thus only do we get the hypothetical statement, which is essentially inference, and inference from knowledge of reality.[a]

[a cf § 102, and Croom Robertson's discussion of the *if* of doubt and of inference in *Mind*, vol ii (1877), pp 264–6]

The temptation perhaps is to suppose that 'if A is B, A is C' is merely the equivalent of 'all B is C', but it is not. B is C doesn't state a condition *to be* satisfied, but an actual quality of all the real things which are B. In 'if A is B, &c.', we are considering something which we do not know to be a B, and the possession of C is a condition, for it needs to be satisfied if A is to be a B, and it is only in relation to an uncertainty that there is a condition *to be* satisfied at all. Or, to put it otherwise, 'B is C' is a fact realized, and a realized fact is not a condition to be satisfied. The condition cannot be a fact, it is hypothetical, something to be satisfied IF &c., but there is no '*If*' about the fact. Hence, if we avoid the 'if', the condition has to be expressed not by the indicative mood but just by the conditional mood. That A *should* be C is the condition of its being B which is the mere equivalent to if A is B, A is C. The latter is the full and adequate statement in language of the condition as condition.

§ 299. The hypothetical expression 'if A is B, A is C, because B is C' when general is parallel to the non-hypothetical 'whenever A is B, A is C, because B is C'. But we cannot reduce the former to 'every case of AB is a case of AC' · for this is proper to the latter, the normal meaning of which is that there *are* cases of AB, whereas the normal meaning of the hypothetical is that this is uncertain and there may be no such cases. Thus, if we take the form 'all cases of A being B are cases of A being C', we are obliged for accuracy, in view of the possible addition in argument of 'but there are no cases of AC', to add this qualification, 'all cases of A being B, IF there are such cases', and thus the IF refuses to be eliminated. The two forms given above are parallel though not identical—and if our explanation is successful, it ought to make the parallelism appear natural.

To get at the meaning of the IF, we must reconsider the whole actual process of thinking in which it arises We ask the question whether A is B : we find that B is C ; we then ask the question whether A is C. This must be because we think the answer to this second question may throw light on that of the first, as being connected with it. Where the second is answered in the affirmative, we cannot decide the answer to the first, since B is

not necessarily coextensive with C.[1] But in those cases where the answer to the second form is negative, so is the answer to the first, and this is our reason for passing to the second.

Clearly we have now to consider the relation of the answers to the questions. Whether A is B, and whether A is C, are both of them problems to us. Now since B is C, the problem or question whether A is B is a case of the question whether A is C, since B is a case of C. If we ask the question whether A is C at all, if the species of C are, e. g., BC, XC, YC, then the question whether A is C, which may be for convenience symbolized by (AC?), has for its various cases (AB?), (AX?), (AY?). That is simply, the question (AC?) necessarily takes the forms (AB?), (AX?), (AY?). The hypothetical expression 'If A is B, A is C, because B is C', corresponds to the relation between the problems (AB?) and (AC?) that is, its meaning may be put thus. (α) 'The question whether A is B, is a case or form of the question whether A is C, because B is C'. We see now the reason of the parallelism in form between the two expressions 'if A is B, A is C' and '$\left\{\begin{matrix}\text{wherever}\\\text{whenever}\end{matrix}\right.$ A is B, A is C'. For the meaning of the latter is (β) 'The fact of A's being B is a case or form of the fact of A's being C, because B is C.'

Now (α) and (β) only differ in the substitution in (α) of the expression 'the question whether A is B', for the expression 'the fact of A's being B', which appears in (β). Observe that when we use the form (β), which is equivalent to 'wherever A, &c.', we expressly leave open the possibility that A might in some cases have another form of C; and in (α) we leave open the possibility that another form of the question whether A is C might be raised. The above relates to the *general* form of hypothetical reasoning. The particular, 'if this A is B, &c.', corresponds to 'the problem whether this A is B is a case of the problem whether this A is C', but the corresponding or parallel non-hypothetical is the particular statement 'This A's being B is a case of its being C'.

The above seems to represent everything there really is in the consciousness or mental attitude which corresponds to the hypothetical expression; and, if the preceding analysis is correct,

[1] Viz we can't argue 'If A is B, A is C, A is C ∴ A is B'.

and also the criticism of the ideas of supposing, assuming, imagining, &c., there would be no other alternative.

But now we must test its adequacy by examining the inferences based upon an hypothetical statement, and this we do by considering the relation between the answer to a question and the answers to its cases or forms. As to those questions which have a negative answer, it is clear that any case or form of them has a negative answer.[1] Thus, given that a question has a negative answer, we infer that any case of it has a negative answer. We analyse therefore the argument 'if A is B, A is C; but A is not C, therefore A is not B' as follows. The question whether A is B is a case of the question whether A is C. But the question whether A is C, has a negative answer; therefore its case, the question whether A is B, has a negative answer. Again, obviously when a case or form of a question has an affirmative answer, the question itself has this same answer. Hence, 'if A is B, A is C, but A *is* B, therefore A is C' is analysed as follows. The question whether A is B is a case of the question whether A is C. But the question whether A is B has an affirmative answer; therefore the question whether A is C has an affirmative answer.

So far, then, the hypothetical statement corresponds to the apprehension of a connexion between questions or problems based upon a knowledge of reality. This knowledge must not be overlooked. For we do base the connexion of our problems upon the nature of objective reality, i. e on the apprehension that B is C. And so we are not confined to mere subjective factors when we state the connexion of our problems. For we do not merely say that the question whether A is B is a case of the question whether A is C, but that, *because* B is C, it is a case of the question whether A is C. Here the clause introduced by 'because' definitely bases the connexion of the problems on reality. Nothing which is true in the matter and necessary to the reasoning seems to be left out.

§ 300. The foregoing analysis may not seem quite satisfactory, because we may feel that in the expression 'if A is B, A is C, &c.', we do something more than assert a connexion between our subjective problems. We may doubt, that is, whether the verbal expression is intended to put just this aspect of our thought;

[1] This answer is *not* an *hypothetical* statement.

since in fact it seems to do something more. In the clause 'if A is B' we don't in fact seem to be merely thinking of the question whether A is B or not and of its implications, but to have our attention somehow directed rather to the affirmative answer, though of course we should stop short of saying that we affirmed it. The difficulty must not be exaggerated. In a certain sense, when we say the question (AB?) is a case of (AC?), because B is C, we are alluding to what may be called at least the positive side of the interrogation, even if it is not the affirmative answer to it. For it is true that we are considering primarily the question (AB?) and not the question (A not B?). These two questions are not the same, though they involve one another.

That they are not the same may be shown as follows. If (AB?) and (A not B?) were the same question, they must admit of the same forms of answer. There is of course a form of answer which is the same for both; we may answer to (AB?), 'A is B', and then this is also the answer to (A not B?). But there is one form of answer which can't be the same. If the answer to (AB?) is 'Yes', the answer to (A not B?) must be No, that is, the answer must be negative.

Again, if (AB?) were the same as (A not B?), and therefore (AC?) the same as (A not C?), in the statement '(AB?) is a case of (AC?), because B is C', we ought to be able to substitute (A not C?) for (AC?); whence we have (AB?) is a case of (A not C?), whence, if the second question is denied, so is the first. That is to say, if A is C, A is not B. Which of course is absurd. For though it is possible that A may be C without A being B, there is nothing in the form of the given hypothetical statement to show that this is necessary.

Nor does it even follow that (AB?) being a case of (AC?), (A not B?), the equivalent of (AB?), is a case of (A not C?), the equivalent of (AC?) For, if it were, the negative of (A not C?) necessitates the negative of (A not B?); in other words, if A is C, A is B, which is obviously untrue. This may be shown as follows. If we substitute for (AB?) its equivalent (A not B?), and similarly for (AC?) its equivalent (A not C?), we shall get (A not B?) is a case of (A not C?). Now what is the con-

dition that the first question should be a case of the second? Clearly that not B should be a case of not C; i.e. that all not B is not C; i. e. that all C is B: whereas all we know is that all B is C.

The questions (AB?) and (A not B?) therefore are not the same; but the one is the complement or necessary concomitant of the other: and the answer to the one is not in every form the same as the answer to the other, but nevertheless necessitates the answer to the other. They are two different and inseparable aspects of the interrogative attitude; (AB?) is the positive side of it (avoid calling it 'affirmative') and, as such, it is the definite side, which alone gives any meaning to the negative.

Thus when we say the question (AB?) is a case of the question (AC?), it is the positive side of the interrogation which we have before us, and we affirm an implication of it, which could not be affirmed of the negative side. We relate the positive side of one interrogation to the positive side of another.

§ 301. But, nevertheless, the language, in the form 'if A is B, A is C, because B is C', seems to go further even than correspondence to the positive side of the question or problem, and in a sense to indicate a contemplation of the affirmative answer. It is in this reference to the affirmative answer that we may look to find the true account of the verbal form of the usual hypothetical statement. It will not, however, do to say that 'if A is B, A is C' means 'the answer or statement A is B necessitates the answer or statement A is C', because this in its form, as we know, properly implies the answer really given, i. e. the statement really made, whereas the answer is uncertain. Therefore the above will have to become, when fully expressed: 'the answer A is B, if it were given, necessitates, &c.'; where we have again introduced the IF which is to be explained.

Let us return to the mental process itself as previously described. Being uncertain whether A is B or not, we know, of course, that the decision of our uncertainty must be in a statement which must have one or other of the two forms A is B, or A is not B: the latter comprising under it, if we know $\left\{\begin{matrix}\text{B}\\\text{A}\end{matrix}\right\}$ sufficiently, A is C, or A is D, &c.; in other words, if we

know the species C and D of not B, which are relevant. We therefore have these two forms before us, in the sense that we contemplate them as the alternative forms which the answer must have, that is to say, we apprehend them. Here it is of the greatest importance to notice that the forms symbolized by A is B, A is not B, are *forms* of statement, and not statements; neither could be a statement unless we had stated it; but before decision this is impossible (Besides—as a *reductio ad absurdum*—if they were statements, they would be stated, and we should have made two conflicting statements.)

But now we know that B is C. Consequently, we see that the affirmative of the two *forms* of answer, namely, A is B, necessitates also the *form*, A is C; so that the complete expression of the first form of answer is 'A is B and therefore A is C'. Observe here that just as the statement that there *are* two forms of statement, one of which the answer must take, is not hypothetical but non-hypothetical, so also the statement that one of them, viz. 'A is B' necessitates in addition the form of statement 'A is C', is strictly non-hypothetical. It is not that the form of the answer 'A is B' *would* in this case necessitate the form 'A is C', but the form of answer 'A is B' *does* necessitate the form 'A is C'. 'Would' would bring back the IF, of course. Our attitude, then, is this: we know that the two forms of statement, one of which the answer must take, *are* (not *would be*) 'A is B' and 'A is not B'. We do not know which form it will take. We also know that of these forms the first, 'A is B', necessitates in this case (not *would* necessitate but does necessitate) the form A is C, because B is C Now this is what the verbal form 'IF A is B, A is C' seems intended to express. It indicates both our uncertainty as to which form of statement the answer will take and our certainty that the affirmative form has in it the form A is C as consequence. Here we notice that there is no fiction in the matter and nothing asserted of any fiction. Everything is apprehension of reality, together with no fiction as to what reality is, but merely the question. This property of the form of one (i e. the affirmative) of the two alternative forms of answer is simply an aspect of the fact that what we apprehend, in the hypothetical attitude,

is a relation between the two problems A is B and A is C, involves this necessarily and is involved by it.

§ 302. The apprehension of the forms of answer or forms of statement which the answer or statement must take is subserved by the imagination: the universal in fact is as usual apprehended somehow through our apprehension of the particular. In a certain sense, though in some cases only, it is allowable to say that we imagine the fact corresponding to the affirmative answer 'A is B'; that is, we imagine A being B and then imagine A being C. But, when and so far as this is possible, neither is the imagination, although subserving the statement, the statement, nor is the hypothetical statement about the imagination. There are, moreover, cases in which the imagination of the affirmative answer is impossible, as, for example, in the enunciation of Euclid, I. vii. We cannot possibly imagine two triangles with their sides equal, &c.[1] For if we imagine the sides terminated in one extremity of the base to be equal we cannot imagine those terminated in the other extremity also equal. But the most important consideration is that the description of the imagination is inaccurate or misleading. If we examine the supposed imagination, we shall find it is not properly of A's being B—a kind of objective possibility—at all, and that it actually contains the problematic element, only appearing to avoid it through a certain confusion of thinking. The imagination cannot contradict anything we apprehend as necessary to the object to which it refers. We necessarily apprehend the exterior angle of a triangle as greater than the interior and opposite angle, and we cannot imagine the contrary. We can imagine a greater and a lesser angle, but we cannot imagine them as respectively interior and exterior opposite angles of an imagined triangle. Consequently, if the connexion of A and C is a problem to us it remains so in the imagination; we may imagine A and C but not their connexion. An illusion about this may be caused by the fact that the imagination in a given case only refers to a part of the reality of the object. When

[1] 'On the same line and on the same side of it, there cannot be two triangles having their sides terminated at one extremity of the line equal to one another, and likewise those terminated at the other extremity of the line equal to one another'

we say we imagine a serpent breathing fire, all we imagine is the colour and exterior appearance of a serpent and the same for fire. We are not imagining the physical properties of fire, and the biological properties of the serpent which make 'breathing fire' impossible for the serpent.

§ 303. The inference 'if A is B, A is C; but A is not C, therefore A is not B' arises in the following way. Being uncertain whether the answer has the form 'A is B' which really, since B is C, is equivalent to the form 'A is B, and therefore A is C', or whether the form of the answer is 'A is not B', we afterwards find that a statement of the form A is C cannot be made; in other words, that A is not C. The answer therefore cannot have the form including or necessitating the form 'A is C'. It cannot therefore have the form 'A is B', which (as we have seen) includes or necessitates the form 'A is C'; in other words, it must be 'A is not B'.

This way of putting it explains most simply and clearly the verbal parallelism between the hypothetical and non-hypothetical arguments. We have simply to substitute, in fact, in the non-hypothetical form of argument, the expression 'the *form* of statement A is B', for 'the statement A is B'. Thus, e. g.:

Non-hypothetical argument.	*Hypothetical argument.*
the statement A is B necessitates the statement A is C,	the *form* of statement A is B necessitates the *form* of statement A is C,
the statement A is C necessitates the statement A is D,	the form of statement A is C necessitates the form of statement A is D,
therefore the statement A is B necessitates the statement A is D.	therefore the form of statement A is B necessitates the form of statement A is D.

The second (or hypothetical) argument is now rewritten, 'if A is B, A is C; if A is C, A is D; therefore if A is B, A is D'.

We may also put the parallelism as follows:

A's being B necessitates C's being D. (This may be either singular or universal in form) The affirmative form of answer to (AB ?), viz. the form A is B, necessitates the form C is D. In other words, 'if A is B, then C must be D'. If we put the form 'if A is B, C is D' in the following shape, 'the affirmative

form A is B is a case of the affirmative form C is D', we have language analogous to that used before about one problem being a case of another, and the inference may be put similarly. In the case where we know C is not D, the form C is D is impossible, and therefore any case of it, as 'A is B', is impossible. From this point of view then we may say that the hypothetical statement asserts something of one of those forms of statement, *one or other* of which is the form which the answer to our problem must take. The distinction on which everything turns in the foregoing is that of the *form* A is B from an actual statement of that general form.

In the hypothetical statement we assert nothing of a statement, but we do assert something of a form of statement.

§ 304. The 'if' clause in an hypothetical statement may, according to a familiar idiom, refer to something already decided and thus appear not to embody a question. We say, for example, 'If A had been (*or* were) B, as we know it was (*or* is) not, A would have been (*or* would be) C'. Such statements are, of course, of frequent occurrence, e g.: 'If he had jumped out of the carriage a minute later he would have been killed.' Observe that all such cases refer to the contradictory of what really happened, i. e, as would be said, to something which did not happen. It does not follow that in such cases, or indeed in any cases, the hypothetical statement is about a possibility which is objective. For, as we have seen, there is no objective possibility of anything which is not necessary.[1] The only other possibility, if it can be called one, is in our subjective uncertainty. In such examples as the above the reference is to something impossible and not to something objectively possible. Nor is it true that something is merely affirmed about an imaginary existence. Something true of reality is affirmed, as is easily seen when we reflect that the example gives the information that the man jumped before a certain thing, incompatible with the life of a man in the carriage, happened, e. g. before the carriage fell over a precipice. The solution of the difficulty may be got out of the case where there is a real question. Take a case where we start with a real uncertainty as to whether A is B but know that B is C; and suppose that we afterwards

[1] § 90.

find that A is not C. We say first, 'If A is B, A is C because B is C', i. e. (AB ?) is a case of (AC ?) since B is C. We add afterwards, 'But A is not C', i. e. (AC ?) is answered in the negative. 'Therefore A is not B', i. e. (AB ?) is negatived. But, when we have got so far, (AB ?) ceases to be a question, for our uncertainty no longer exists. The proper expression of our thought now is: 'if A were (*or* had been) B, A would be (*or* would have been) C; but A is (*or* was) not C, therefore A is (*or* was) not B.'

Now this is equivalent to the question (AB ?) was a case of the question (AC ?), and as (AC ?) was answered in the negative, so also (AB ?) was answered in the negative

Thus the hypothetical form we now have before us, 'if A were (*or* had been) B, &c.', expresses a relation between what were questions (AB ?) and (AC ?), as questions, and conveys also the information as to how they were answered, viz. negatively.

The carriage accident is quite parallel to the above. The question whether the man would jump in time or not was a real question before he jumped, and the question whether he would jump in time was a case of the question whether he would be killed.

In the case of an ordinary *reductio ad absurdum* proof, it may be said that the question (AB ?) is known from the first to be negatived; but this is not for the inquirer or learner, but only for the teacher when he communicates it So in the mouth of the teacher 'If A is B, A is C' means. 'the question for *you* (i. e. the person learning) whether A is B is a case of the question for you whether A is C' Of course for the teacher himself at one time (AB ?) will have been a problem.

§ 305. The disjunctive statement 'A is either B or C' often, not always, implies an uncertainty about some given subject A, and accordingly is akin to the hypothetical. It may mark a stage in our inquiring attitude; we may begin with uncertainty as to whether A is B, but advance sufficiently in knowledge to have the certainty that A is either B or C, or in general, either B or C or D, &c. Moreover, it then gives rise to hypothetical reasoning, A is either B or C, B is X, C is Y, ∴ if A is B, it is X, if A is C, it is Y. But the statement 'A is either B or C' nevertheless does not always imply an uncertainty, and is at

least not intended to express any. It may express the necessary differentiation of a universal and contain nothing hypothetical at all. For example, 'Lines are either straight or curved or crooked'; 'number is either odd or even'. 'A triangle is (or triangles are) either right-, obtuse-, or acute-angled.'

In reference, however, to a particular instance of the universal such a statement has the disjunctive form which expresses a limited uncertainty. For of a given triangle we may know it is a triangle because we know it is a figure consisting of straight lines joining three given points, each to each, without knowing whether it is right-angled, obtuse-angled, or acute-angled. Thus we know that the figure formed by joining any point in a circumference to the extremities of the diameter is a triangle, before we know its property.

But, in this case, we have the disjunctive statement 'the triangle X has either one right angle, or one obtuse angle, or three acute angles', which is based on the disjunctive non-hypothetical statement, 'a triangle must have either one right angle, or one obtuse angle, or three acute angles'. In fact the disjunctive statement of uncertainty 'A_1 is either B or C' (it is uncertain which) is always an inference from two statements, viz: (1) a disjunctive which is not problematic, or expressive of uncertainty, i.e. 'A (*or* all A) is either B or C', and (2) a statement neither hypothetical nor disjunctive 'A_1 is A'. In other words, in the problematic disjunctive statement 'A_1 is either B or C', A_1 has the alternative attributes or properties B and C only because it has the characteristic A which must differentiate into B or C.

In this characteristic the problematic disjunctive is obviously like the hypothetical, for this also must be an inference from a non-hypothetical statement. But the non-problematic disjunctive statement is not necessarily an inference from another statement. A disjunctive statement, however, of the form 'A is either not B or not C', suppose we start with A as subject, can only arise as an inference from A is not (BC), i.e. from 'A is either X or Y, and X is not B, and Y is not C', for obviously a universal A cannot be differentiated into negative classes immediately.

§ 306. The disjunctive statement, whether problematic or not,

is, like the hypothetical statement, as categorical as the statement to which the word categorical is usually restricted.

The statement 'A_1 is either B or C' (when not the differentiation of a universal) implies the problems ($A_1B?$) ($A_1C?$); it states that the problems are such that one of them has an affirmative answer. But its meaning cannot be more simply and directly expressed than by the use of the words 'either' and 'or', i.e. 'A_1 is either B or C', supplemented by 'but it is not known which'; for to say that one of two problems ($A_1B?$) and ($A_1C?$) has an affirmative answer is to say *either* ($A_1B?$) *or* ($A_1C?$) has an affirmative answer.

Or put it this way. the question 'what is A_1?' is such that the answer must take one of the two forms 'A_1 is B', or 'A_1 is C'; where, once more, 'A_1 is B' and 'A_1 is C' are not statements but forms of statement

Thus the problematic disjunctive statement does not affirm a relation between statements; it expresses a relation between two problems, as above described, *or* (another aspect of the same thing) it states a relation between two forms of statement 'A_1 is B' and 'A_1 is C', that is, the statement which answers the question about A_1 must take one or other of these forms.

§ 307. If we have the statement A_1 is either B or C, it follows that if A_1 is not B, A_1 is C, and if A_1 is not C, it is B. Thus the problematic disjunctive statement gives rise to hypothetical statements.

We may put the problematic disjunctive reasoning as follows: the question whether A_1 is not B is identical with the questions whether A_1 is not B and whether A_1 is C (A being either B or C); or the question whether A_1 is not B is a case of the question whether A_1 is C, i.e. the *form* of the answer 'A_1 is not B' necessarily includes the form A_1 is C. The original disjunction, as stated, is, however, compatible with A being both B and C. We ought to add the *caveat*, if A is B, it does not follow that A is not C; and where this is not the case, it ought to be distinctly stated.

§ 308. The problematic disjunctive statement may by combination with assertoric premisses lead to a conclusion which is not problematic. For instance:

A_1 is either B or C; B is D and C is D
$\therefore$ if A_1 is B, A_1 is D
if A_1 is C, A_1 is D
$\therefore$ A_1 is D.

In other words, an attribute which belongs to A_1, if in the group B and C, involves D.

Here it is not the disjunctive statement as such which leads to a non-disjunctive conclusion. B and C must involve D in consequence of something common to both, viz. A. We know that A_1 is B or C because A_1 is A, and A is either B or C. Thus we know already that A_1 has the condition of D, viz. A, and thus the argument accurately stated is A_1 is A; A is D; $\therefore$ A_1 is D. The disjunctive statement only arises from an imperfect insight into A and D, on account of which we have not been able to attach D directly to A. That is to say, the problematic statement A_1 is either B or C follows from the non-problematic disjunctive statements A is either B or C (where B and C are differentiations of the universal A), and A_1 is an A.

But B (= AB) is D, and C (= AC) is D,

And AB and AC make up A (which statement implies that AB is real and AC is real).

$\therefore$ all A is D, and A_1 is A, $\therefore$ A_1 is D.

§ 309. Aristotle showed a right instinct in not proposing a reduction of hypothetical argument to a non-hypothetical form. He does not condemn it and probably it had never occurred to him to consider its possibility. The ordinary method is to substitute for the if clauses, in, for example, if A is B, C is D, if C is D, E is F, therefore if A is B, E is F, the apparently non-hypothetical statements 'all cases of A being B are cases of C being D'. But, as we have abundantly seen, the substituted and non-hypothetical form properly means that it is *not* uncertain whether any A is B. In fact, to represent the reasoning in full, we should have to say all cases of A being B, if there are any, &c. Thus we have not eliminated the 'if' and the supposed reduction is impossible.

Consider again, if all A is B, C is D; but all A is B, $\therefore$ C is D. Here, if we substituted 'all cases of A's being B are cases of C's being D', we should imply that all A is B and 'but all A is B' would be superfluous. In fact it is really unintelligible, in regard

to such reasoning, to say all cases of A are cases of A being B, for 'if A is B' contradicts the idea that all cases of A are B. If it be said that reality necessitates A's being B, the first clause must be A's being B necessitates C's being D, and this implies, in ordinary usage, that A *is* B. The same difficulties occur in the ordinary reduction of the disjunctive argument. Take, for example,

either A is B or A is C,
A is not C ∴ A is B.

We are asked to represent this by the following reasoning:

Every case of A not C is a case of AB.
Every case of A is a case of A not C.
∴ Every case of A is a case of AB.

But here, in the first statement, we have implied the reality of the cases of A not C, whereas the object of our original statement was to make these cases problematic. Moreover, the preceding criticism applies to the way the second (so-called minor) premiss is stated. Similarly we cannot reduce the argument A is either DB or DC, ∴ A is D, to a non-disjunctive form.

§ 310. We may now inquire how hypothetical reasoning is best represented so as to bring out its true form. It is uncertain whether A is B, let C be A and B be D. Then if A is B, inasmuch as C is A and B is D, it follows that C is D. Further, we know that if C is D, E is F. Therefore if A is B, we know that E is F.

The accurate way of stating this appears to be this. An affirmative answer to the question whether A is B necessitates an affirmative answer to (CD?), an affirmative answer to the latter necessitates an affirmative answer to (EF?), therefore an affirmative answer to (AB?) necessitates an affirmative answer to (EF?).

But here we are in difficulties again, because we can say that the question (AB?) is the same as the question (is CA—BD?) but not that it is the same as (CD?). That is, we have to add the reason for the dependence of C's being D on A's being B, in order to express the hypothetical statement in terms of the identity of one question with another. If, then, we only state that C is D, if A is B, without the connecting reason, we cannot identify something relating solely to C and D with the question whether A can be B.

What is adequate, then, is to say that the form or answer to the question (AB?) is identical with the question whether A is B and C is D, owing to the real implication of the real elements A, B, C, and D. This exactly covers the hypothetical statement. For, if C is not D, the question whether A is B and C is D has a negative answer and therefore the question (AB?) has a negative answer, that is, 'if C is not D, A is not B'. But if the answer to the question (CD?) is affirmative, it does not follow that that to (whether A is B *and* C is D?) is affirmative, and therefore not that the answer to (AB?) is affirmative. This corresponds to the usual expression that if C is D, it does not follow that A is B. Thus the hypothetical argument, if A is B, C is D, and if C is D, E is F, &c., may be correctly put in the form of the corresponding questions. We may, however, assimilate this to the usual form more closely if for 'the question whether A is B is the same as the question whether A is B and C is D' we substitute 'the question whether A is B is the same as the question whether C is D and *something else* is true'. This exactly covers the usual hypothetical statements, and the conclusion will be 'the question whether A is B is true is identical with the question whether E is F and something else'. The argument, then, is a relative argument, that is, the relation of two terms to one another by means of their relation to a third. We can put it strictly as follows:

The problem whether A is B includes the problem whether C is D.

The problem whether C is D includes the problem whether E is F.

∴ The problem whether A is B includes the problem whether E is F.

And this form of argument cannot be reduced to a syllogism.

§ 311. We thus reach the conclusion that the assertion in any hypothetical statement is that one problem includes another or that one question is identical with another and that the grounds of this connexion or relation are based on non-problematic considerations. These latter are implied but not stated explicitly in the hypothetical statement. If A is B, C is D is grounded, for example, on the non-hypothetical implication that C is A and B is D, and means not that we imagine or suppose or assume

that A is B; not that the supposition C is D follows from the supposition A is B, but that the question whether A is B is identical with the question whether A, which presupposes C, is B which necessitates D, or whether AC is BD, inasmuch as C is A, and B is D. That is the full expression of our thought. If the thought is not fully expressed (is expressed, that is, without the ground of connexion, as 'if A is B, C is D'), the adequate expression is 'the question or problem whether A is B is identical with that whether C is D as well as A is B'. A relation, once more, of problems, not of statements.

VII

DIRECT AND INDIRECT ARGUMENT THE FICTIONS OF MODERN MATHEMATICS

§ 312. As the forms of language encourage certain mistakes about the nature of hypothetical thinking, the question arises how far they are to be observed and whether they are capable of modification. Hypothetical thinking is so common in ordinary life that there can be no idea of using a merely technical and 'artificial' set of expressions. But some of the misleading expressions are unnecessary and the ordinary language provides the right ones. Thus the causal form of the hypothetical which sometimes occurs in a *reductio ad absurdum* proof is unnecessary and can be discarded. We need not put 'if A is B, C is D' in the erroneous forms 'since A is B, C is D' or 'because A is B, C is D'.

The case is somewhat different with such expressions as 'assume', 'suppose', 'imagine'. We have already pointed out the natural meaning of 'suppose'.

It clearly must have passed to a different meaning, if usable at all, when applied to an hypothesis which we do not suppose to be true, the consequence of which we are investigating. We have not decided here either way. The same is true of 'assume'. There is a sense of 'assume' (as applied to a doubtful or hypothetical matter) in which it implies a decision; and we must be careful here to avoid a confusion. The same is true of 'suppose', but yet it is more usual with 'assume' than 'suppose'. A man is uncertain which of two possibilities for him is true and they concern a matter in which action has to be taken. He has to decide how to act, which means that he must act as if one or other were true. If he decides to act in one way, he acts and decides to act as if one or other of these alternatives were the true one. In that sense he decides for one alternative, and only in that sense. He does not decide that it is true; that is an impossible feat. He does not *think* that it is true;

that also we have seen to be an impossible feat. He may suppose it to be the more probable alternative.

There is another case of decision which may seem rather theoretical than practical. Of two alternatives a man may choose one to argue from and to develop its consequences. For example, in certain chemical or biological investigations where a man may have to choose between conducting one set of experiments and another, or between treating the living body in one way and another. Such decisions, however, are clearly practical, for theoretically he would conduct, if possible, alternative experiments. Here we may say that a man acts on the assumption that one alternative is true rather than another.

Putting aside these uses of 'assume' and 'suppose' as obviously inapplicable to the case where we say 'if A is B, C is D', without either deciding practically in favour of the alternative that A is B, or thinking it probable, we may ask what it is that corresponds to the expression 'let us suppose (or 'suppose') A is B and see what follows'. These words are nothing but a way of proposing a problem for consideration; they really mean 'let us fix our attention upon the question whether A is B, and ask what follows *if* A is really B'. We shall then see that 'if' intrudes itself and therefore can't possibly be replaced by 'suppose'.

This 'let us suppose' is, then, really a way of proposing some definite problem; in other words it is a kind of decision, viz. to consider one aspect of a problem. It is so much more convenient to use the short than the long expression which appears to be the legitimate one, and this in language is a scarcely resistible consideration, and usage will be too much for us. For ordinary purposes we may therefore retain such expressions as 'suppose', 'supposing that', 'let us suppose', 'let us assume', &c., remembering of course what they really stand for; but it will be imperative in a philosophical investigation, or where there is a possibility of confusion, to substitute the true equivalents. The urgent necessity of the latter will appear in the subject we are now approaching, the treatment of *reductio ad absurdum* arguments. The confusion caused by not doing so is mainly responsible for the modern pseudo-mathematical monstrosity of non-Euclidean space. There is no need in any scientific argument to use these somewhat misleading expressions 'assume' or

'suppose'. Their legitimate use is really for the decisions we have mentioned.

§ 313. The true view of hypothetical statement and reasoning is obscured by a certain misleading use of language, which is found in the ordinary scientific representations of a *reductio ad absurdum* proof.

The *reductio ad absurdum* proof, stated in the way we criticized above, usually begins thus, 'assume A is B', then follows 'because A is B and B is C, therefore A is C: but A is not C, therefore A is both C and not C'. Now we could not in fact advance to the conclusion usually subjoined, if the first premiss really corresponded to the verbal forms in which it is put. We have to restore the hypothetical form which we have verbally suppressed before we can make another step; we have to say 'therefore *if* A is B, A is both C and not C; which latter is impossible, therefore A is not B'. That is, we have to remember that there was no real premiss 'because A is B', and that what we really meant was 'if A is B, then *because* B is C, A will be C'. Now misleading expressions like 'assume A is B', which put quite a false complexion on thought, incline people to think they have 'supposed' A to be B, and hence that they somehow start with a 'conception' of A's being B. To avoid this danger the 'if' should be restored throughout the *reductio ad absurdum* argument.

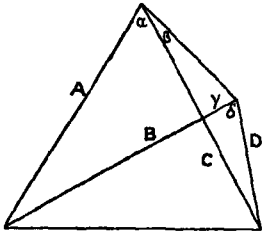
§ 314. It must not be supposed that the form of the *reductio ad absurdum* is peculiar to a particular kind of matter.

We can replace any *reductio ad absurdum* argument with its hypothetical premisses and hypothetical inference by an argument entirely non-hypothetical[1] We infer 'if A is B, then in general C is D', and 'C is D' is known to be false Then it follows unhypothetically from C is not D that A is not B, and the steps of the non-hypothetical proof can all be obtained by a sort of inversion of the *reductio ad absurdum* proof. This brings out its true characteristics. Starting with our question

[1] §§ 105, 259 [a]

[[a] Cf note to § 259 If the matter is so simple, it may well be asked why the indirect argument should be used. On this, see § 316. In the author's direct proof of Euclid 1 7, observe the last step, which certainly seems to be indirect]

whether A is B, we conduct an inquiry represented by a train of hypothetical inferences till we arrive at a criterion which decides the answer to our question. Now the *reductio ad absurdum* proof represents the process by the system of hypothetical inference till it terminates in the hypothetical contradiction to a known truth, which then serves as criterion. The non-hypothetical proof consists in applying the criterion simply, which of course is done in a system of non-hypothetical reasoning. We may illustrate by a particular instance, drawn from the Seventh Proposition of Euclid's *Elements*, Book I. The truth which it is desired to establish is that 'on the same base and on the same side of it, there cannot be two triangles having their sides which are terminated at one extremity of the base equal, and likewise those terminated at the other extremity, equal'. The *reductio ad absurdum* proof given in Euclid runs as follows:



If A = B, then $\alpha+\beta = \gamma$.
But $\alpha+\beta > \beta$.
Therefore $\gamma > \beta$.
Again, if C = D, $\gamma+\delta = \beta$.
Therefore, if A = B *and* C = D,
$\beta (= \gamma+\delta) < \gamma$.
Therefore $\gamma > \gamma+\delta$.
Thus the part would be greater than the whole

Therefore A cannot = B, and C at the same time = D.

The corresponding non-hypothetical proof runs as follows:

Make A = B, and join the extremities of AB, by C and D, to the other extremity of the base.

Take as before vertices external to each other's triangle.

Required to prove that C cannot be equal to D.

Then, if A = B, $\alpha+\beta = \gamma$.

But $\alpha+\beta > \beta$,

Therefore $\gamma > \beta$.

But $\gamma+\delta > \gamma$, and $\gamma > \beta$,

Therefore $\gamma+\delta > \beta$.

Therefore the sides of the triangle of which $\gamma+\delta$ and β are base angles are unequal. Q. E. D.

The last step is that unequal angles at the base necessitate

unequal sides. It is important to see that this last step is non-hypothetical and is mistakenly represented as resting upon a *reductio ad absurdum* proof.

It would be proved by a *reductio ad absurdum* proof as follows: If the sides were equal, then the angles would be equal. But the angles are not equal. Therefore if the sides were equal, the angles would be both equal and not equal. But by the principle of contradiction this is impossible. Therefore the sides are unequal. This is a mere illusory and unnecessary transformation of argument. It is always futile to appeal to the principle of contradiction as if it were a special premiss to be used on a special occasion. For, so far as any premiss depends on it, every premiss depends on it equally, and every premiss would fail, if the principle of contradiction were not observed.

The general form of the above argument is this. Let A be a condition of B. If B is absent A is absent, because if B were absent and A were present, B would be present and not present, which by the principle of contradiction is impossible. Now the fact is that the thing to be proved 'if a thing is absent, its condition A is absent', is absolutely self-evident. It is an inseparable aspect of the fact that A is the condition of B · and a false argument always arises when we try to prove the self-evident. We really only base it on itself in the negative form and so bring in the principle of contradiction. You can indeed only argue from the principle of contradiction as a premiss by using the principle of contradiction and so *ad infinitum.* In the present instance, since the angles at the base of an isosceles triangle are equal, it follows immediately that when the angles are not equal the sides are not equal. The latter follows directly: there is no hypothetical reasoning in the matter. Thus what we have done in the non-hypothetical argument is to use precisely the same material as we used before in the hypothetical form of proof; inasmuch as the proposition that unequal angles at the base necessitate unequal sides is simply a necessary aspect of the truth contained in the statement that equal sides necessitate equal angles.

§ 315. The general proof that a *reductio ad absurdum* argument may be always expressed without a *reductio ad absurdum* can

be put rigorously as follows. As a preliminary it is necessary to prove the following theorem.

Suppose that we have the argument:

(1) If A is B, C is D;

(2) But C is not D, therefore A is not B.

This argument is self-evident as it stands and is an ultimate form, not requiring a *reductio ad absurdum.* In the *reductio ad absurdum* form it would be

(3) If C is not D, and A is B, C is both D (from (1) above) and not D.

(4) But C is both D and not D is not true; therefore C is not D and A is B is not true.

Here (1) and (2) have the form, if P_1 is true P_2 is true; but P_2 is untrue, therefore P_1 is untrue. But this is precisely the form we have in (3) and (4) Thus, instead of proving the form represented in (1) and (2), we have merely repeated it in (3) and (4). Thus we can't help making this form ultimate and self-evident.

In any *reductio ad absurdum* proof the argument depends on a proposition or system of propositions non-hypothetical in form and given as true Let this proposition or system be represented by P_1. Let Q_1 be our question or problem. Let it follow from P_1 that if Q_1 is affirmed (or true), R_1 is true. Let R_1 necessitate P_2 as true. Then, if Q_1 is true, P_2 is true Let it be known that P_2 is untrue. Hence Q_1 is untrue. This may be reversed as follows: P_2 is untrue, therefore R_1 is untrue, therefore Q_1 is untrue. By the theorem we have just proved none of these reverse steps will involve a *reductio ad absurdum* proof. It is important to point out that Q_1 may itself be a system: and it would be better to repeat the proof for this case.

Let P_1 be true. Let our problem or question be whether Q_1, Q_2, Q_3, &c., are to be affirmed. Let Q_1, Q_2, Q_3, &c, in virtue of P_1, necessitate R_1, R_2, R_3, &c, i. e that if Q_1 be true it necessitates R_1, that Q_2 similarly necessitates R_2, &c. Further let R_1, R_2, R_3 together necessitate P_2. Let P_2 be untrue, and it follows that if Q_1, Q_2, Q_3 are affirmed together P_2 is true; but P_2 is not true; therefore Q_1, Q_2, Q_3 can't all be true or affirmed together.

This may now be put non-hypothetically. P_2 is untrue,

therefore R_1, R_2, R_3 can't all be true: therefore Q_1, Q_2, Q_3 can't all be true. Here as before the reverse steps don't imply a *reductio ad absurdum*.

We may apply this to Euclid's proposition, I. vii.

The angles at the base of an isosceles triangle are equal.

(Q_1) therefore if A = B, $\gamma = \alpha + \beta$ (R_1),

(R_2) and if C = D, $\beta = \gamma + \delta$ (Q_2).

Therefore, if A = B *and* C = D, $\alpha + \beta = \gamma$ and $\beta = \gamma + \delta$.

Therefore $\gamma > \gamma + \delta$.

Therefore if A = B *and* C = D, $\gamma > \gamma + \delta$.

But γ is not greater than $\gamma + \delta$.

Therefore A = B and C = D can't both be true.

To reverse this process we must begin by showing that from $\gamma + \delta > \gamma$ it follows that $\alpha + \beta = \gamma$ and $\gamma + \delta = \beta$ can't both be true.

Now $\gamma + \delta > \gamma$, hence, if $\gamma + \delta = x$, γ cannot be equal to a magnitude greater than x. Therefore it is impossible that $\gamma + \delta = \beta$ and at the same time $\gamma = \alpha + \beta$. I e. from $\gamma + \delta > \gamma$ follows immediately that $\gamma + \delta = \beta$ and $\gamma = \alpha + \beta$ cannot *both* be true. Therefore a condition of $\gamma + \delta$ being equal to β, and a condition of γ being equal to $\alpha + \beta$ cannot be simultaneously realized. Hence it follows that A = B and C = D cannot be true together.

Now this has exactly the form $\gamma + \delta > \gamma$, $\therefore$ $\alpha + \beta = \gamma$ and $\beta = \gamma + \delta$ are not *both* true, wherefore A = B and C = D are not both true; and thus it exactly reverses the steps of the hypothetical reasoning.

§ 316. We observe in the first stage of this last argument a certain artificiality, if we suppose ourselves to start absolutely with the non-hypothetical and not to have the hypothetical argument before us. The statement that if one magnitude is greater than a second, &c., is of course true; but the question is, why should it occur to us and why should we know it to be of any use? Secondly, even granted this statement, why should we give x the value β? For any magnitude whatever will do for the purposes of the statement itself. Again, why should we take $\alpha + \beta$ for the magnitude greater than x?

It is here important to remark that this kind of difficulty as to why we should select a particular premiss, the justification of which is not seen till we have got the conclusion, is not at

all peculiar to such a case as that before us. If it were so peculiar, we should be obliged to infer that the *reductio ad absurdum* was the natural proof in certain kinds of matter, and so that our difficulty had arisen through substituting a form not natural to that kind of matter. But if we look at any proposition with a non-hypothetical proof in Euclid, we find the same kind of difficulty. The construction is introduced without a word of explanation as to why it should be chosen and we don't see the reason until the problem is solved. In some simpler propositions this is not so evident because we see pretty quickly what the construction is going to lead to. But the same remark applies to all. For instance, the difficulty of accounting for the construction in *Elements*, I. 47, is notorious.

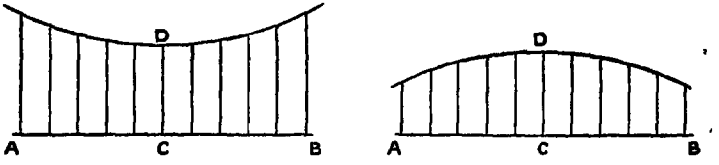
Hence it does not follow that the *reductio ad absurdum* mode of proof belongs to any peculiar matter. The truth is simply this. The step which appears so difficult to account for in the non-hypothetical argument is accounted for and arrived at quite naturally in an hypothetical argument. The hypothetical argument in fact represents the process by which the construction upon which a non-hypothetical proof depends is found. We begin simply with our problem and we ask what an affirmative answer would necessitate; these necessary results we derive from previous propositions, or from direct intuition in particular instances, and it is the carrying forward of this chain of results which suggests the construction.

The hypothetical process therefore combines in itself both the method of discovery and the proof, and is the proper scientific exposition. The non-hypothetical proof to which we are accustomed is a sort of scientific pedantry,[a] and it is consequently a great mistake first to give what is called analysis, which corresponds to the hypothetical process, and then to follow it by a synthesis, which is the non-hypothetical part, thus putting aside analysis as if it were a sort of accident. It is an error because it conceals the true process of thinking.[b]

[[a] From Lotze, *Logic*, ii 4, § 206 'Es ist logische Pedanterie . . . für einen Satz, der indirect sich mit zwei Worten schlagend beweisen lässt, eine directe Ableitung zu suchen . . . und die Einsicht in den inneren Grund ihres Vorhandenseins um nichts reicher macht'

[b] Indirect demonstration in geometry has often been said to show that a theorem *is* true rather than why it is]

§ 317.[a] The modern theory of the possibility and conceivability of two kinds of space, respectively called hyperbolic and elliptic, different from those of which we are said to have experience, is connected on one side with a mere misunderstanding of hypothetical reasoning in general and of the nature of *reductio ad absurdum* proof in particular. One form of it began not at all through an attempt to find whether another kind of space was possible or thinkable but was a quite unforeseen result of an attempt to prove Euclid's Twelfth Axiom. In Euclid this axiom is the premiss from which the equality of the three interior angles of any triangle to two right angles is proved; on which the immensely greater part of geometry depends. Possibly the need of some proof of this axiom would not have been felt or the question at least not raised so soon, if for Euclid's



axiom a somewhat simpler proposition had been substituted, from which it could have been derived and which is really self-evident. Such a simpler proposition is this 'If two straight lines have a third perpendicular to both of them, the first two are always at the same distance from one another'; that is, if a perpendicular be drawn from any point in the one to the other, this perpendicular is of constant length. Or, what comes to the same thing, given a straight line AB and a straight line DC perpendicular to it and of finite length, any line through D, such that the ordinates from it to AB are not equal but increase or decrease on both sides, cannot be a straight line but must be bent. That this is so, is absolutely self-evident. From it Euclid's axiom can easily be derived; and from it the refutation of these hyperbolic and elliptic systems.

[a This XIIth Axiom is called in Euclid's *Elements* the 5th Postulate. In old English books and still on the Continent it is called the XIth Axiom. For attempts to prove this in antiquity see Proclus in *Eucl* 1 (Friedlein), p. 364. I have left the author's words standing as this part is the cherished conclusion to which his discussion of hypothetical reasoning was directed.]

It would be denied that this was self-evident, but here mathematics would be involved in a contradiction. For the fundamental principle of the application of differential calculus to the treatment of curves is certainly far less self-evident than this. At all events it may even be doubted whether it is self-evident.

The proposition that the interior angles of a triangle are together equal to two right angles is further connected with Axiom XII in the sense that either can be derived from the other. The attempt to prove Euclid's axiom was made by the method of 'assuming', as it is called, that it was untrue, and then deducing conclusions in the hope of finding some contradiction, i. e. the attempt was made to prove it by a *reductio ad absurdum*. The contradiction to Euclid's axiom may take the form obviously that the three interior angles of a triangle are either greater or less than two right angles. Each of these contradictions was, as the phrase is, assumed, and a number of theorems deduced from each (merely hypothetical theorems of course). The result would be ordinarily described thus: the theorems deduced from the assumption that the interior angles of a triangle are less than two right angles never showed any contradiction. This strictly means that no proposition was hypothetically arrived at, the truth of whose contradictory seemed more self-evident than the contradictory of the original assumption Otherwise the theorems arrived at were in the figures corresponding to them constantly in contradiction to our faculty of representing straight lines—a faculty on whose correctness the whole of geometrical thinking depends; but this point will not be dwelt on for the moment.

In the case of the assumption that the interior angles are *greater* than two right angles, a quantity of coherent theorems can also be deduced showing no contradiction, in the above limited sense of contradiction. But one of them does lead to a contradiction [a] of the proposition that any two sides of a triangle are greater than the third, a proposition accurately proved in Euclid, without presupposing the Twelfth Axiom. But the failure to get a certain kind of contradiction in the *first* case reacted strangely on the mathematicians who first tried the method

[a] Wilson notes that this was discovered by Legendre, but independently by himself.]

described and who evidently were destitute of logical knowledge or any philosophy of the true procedure of geometry. The thought arose that the process had led to no contradiction because in some sense the hypothesis argued from was valid. The hypothesis that the interior angles of a triangle are less than two right angles may be replaced by its equivalent in terms of two lines and a common perpendicular. But this supposed property of straight lines contradicts the property on which 'Euclidean geometry' depends. The Euclidean system, however, not only doesn't lead to any contradiction of a self-evident truth but to no contradiction of our faculty of representing or, more accurately, *apprehending* geometrical figures. And thus if the one geometry were valid the Euclidean should be so *a fortiori*. There was therefore no attempt to deny the validity of the Euclidean system, but the really inevitable conclusion that if the latter is true the former is false was evaded, by supposing that the former (hyperbolic) system represented a different kind of space in which straight lines in a plane had these curious properties.

The fact is that to these nonsensical words corresponds no thought whatever · they are mere words.

Next, in order to make this more possible, the ordinary idea of straightness was replaced by a definition that a straight line was one such that if two of them coincided in two points they must coincide altogether. Then of straight lines so defined there were supposed to be varieties, one corresponding to Euclidean geometry, and others corresponding to spaces of different kinds. And mathematicians who believe in this actually suppose themselves to be able to conceive such a space Oddly enough also, the geometry which betrayed a contradiction was now retained.

§ 318. At the root of this there are several fallacies of an elementary kind. If we waive an objection which is in itself quite fatal to the system, that is, that the corresponding figures contradict our faculty of apprehension, there is first the elementary mistake of supposing that because a train of hypothetical argument (for it is purely hypothetical) has not as yet led to a contradiction of a certain kind it never will.

It ought to have been a corrective to this to notice that the so-called elliptic system also showed a quantity of coherent

theorems, without the aforesaid kind of contradiction, if developed in certain ways. But of course the mistake is too obvious to need such a corrective.

The supposed theorems are entirely hypothetical. The start, as we have seen, is hypothetical, and this of course never becomes non-hypothetical in the process of deducing hypothetically from it; it no more becomes non-hypothetical at the thousandth deduction than at the first. In fact the system has precisely the form of a *reductio ad absurdum* argument; that it has not led yet to a certain kind of absurdity makes not the slightest difference to its entirely hypothetical character.

As we have seen abundantly, it is not true that we begin by *conceiving* what we start with in the *reductio ad absurdum* (or in any hypothetical argument), and then find that it is not true; for if ever conceivable it would be always so. In that case the proof would be that it was not true though conceivable; whereas we have seen that the proof lies in showing it inconceivable and altogether impossible for thought. On the other hand, if the thing was conceivable, it would be true, for it would be seen to be necessary (as explained)[1] and there could be no thought of testing it by arguing from it hypothetically

The fallacy may be made explicit thus· The question is proposed whether a straight line (A) can have a property (B). We are so far from conceiving A to be B, that by no feat of the imagination can we represent the connexion of A and B; so that it is absurd to suppose that the initial step, which may be put in the deceptive form 'assume A to be B', means that we conceive A as B . for we cannot do it.

But we may waive even this form of objection. It is enough that at first we are confessedly at least uncertain whether A and B can be connected. And we have shown that this means we do not think the connexion but wonder whether it can exist and what its nature would be if it did. We think A and we think B· we do not think their connexion—it is a problem to us and remains so through all the hypothetical deductions.

'Assume A is B', then, only means that we ask whether A can be B and propose to see what would follow if A were B.

[1] viz necessary to thought in the sense of the apprehension of a necessity in the object

What difference, then, does the train of argument make which as yet has not led to a contradiction, that is, if it were really true (as it is not) that it had led to no contradiction? Merely that, as far as we have gone, we have not found anything which decides our original question by showing that A cannot be conceived as B; but it remains equally true that nothing whatever is found which makes an affirmative answer possible: and we have made absolutely no progress to or in the act of conceiving A as B.

But the supposed new conception of another kind of space means that we can conceive a space in which A is B, which is only a disguised form of saying we can conceive that A is B. Hence the view that the process of deduction described has made this a possible conception is an obvious fallacy; and it is a very naive mistake to suppose that the mathematicians who have conducted these kinds of deductive operations have discovered the conception of a new kind of space. This may be put as the mistaking of the problematic conception A ? B for a true conception, or rather the idea that the problematic conception has become a real conception.

We have seen, then, that a new apprehension of space could not originate by the method supposed: and, as this is the only method by which it is thought to have originated, this is a sufficient refutation.

§ 319. But there is another way of making this clear. Really the decisive thing is that all the proposed constructions are impossible for our faculties. We cannot imagine such lines straight, we cannot think of them existing as straight. But the advocates of the theory are in general inaccessible to such an argument because they are under an illusion about the function of the imagination in geometry, owing to their want of all critical investigation of the relation of thinking to imagining and of the thinking consciousness in general to its object. In short, they suppose that it does not matter that the so-called straight lines cannot be imagined straight, which is as nonsensical as to suppose that there could be a number 'two' which we had necessarily to imagine as 'three'.

But they may be approached on their own ground. It may seem that if a man affirms he has a conception, however con-

vinced we may be that he has not, we can do nothing with him because he must claim to know what he has in his own mind. And similarly, if he affirms he hasn't a conception which we know he has and yet is in some illusion about. But in both cases a refutation is possible. In the latter case, by showing that he uses the conception and so must have it; i. e. that certain of his thoughts and actions presuppose it. In the former case, by showing that no use whatever is made of the supposed conception and supplementing this by showing that the illusion that there is such a use arises from the mistake about an hypothetical or problematic conception

The truth is that no other apprehension of space is used except the Euclidean in these theories. As we have seen, the start in such hypothetical reasoning is from the *question* whether A can be B (or A and B can be in a relation R) where A and B (and R) are elements of which we have a certain amount of undoubted knowledge. Advance in thought beyond is only possible through non-hypothetical reasoning from what we know of A and B, from what we do and must apprehend them to be, to something further which this necessitates. Now, *ex hypothesi*, we start with no other conceptions than the Euclidean. A and B are notions we have from Euclidean space alone, and our next step must consist in reasoning in the only way we have as yet—the Euclidean—to the consequences of A and B. In fact, asking whether a Euclidean element A can have the Euclidean property B, we prove that A_1 involves A and B involves B_1, by a purely Euclidean method in each case, and the manner of the reasoning is such that our question whether A is B is seen to depend on the answer to the question whether A_1 can be B_1: or, putting it hypothetically, if A is B, A_1 is B_1.

Now not only must the process begin thus but it is the only way in which it can possibly be continued. This is evident from what is said above, that the process of deduction cannot lead to any new apprehension; every stage is as interrogative as the preceding. But it is also at once verifiable from the actual procedure in the theorems in this subject. In all cases the forward progress is made only by arguing positively or rather unhypothetically from the Euclidean elements to the Euclidean consequences.

The analysis is this:

I. (i) Can A be B (A and B being Euclidean)?
   (ii) Euclidean argument showing A_1 involves A, and B involves B_1.
   (iii) Hence, if A is B, A_1 is B_1.

II. (i) Can A_1 be B_1?
   (ii) Euclidean argument showing A_2 involves A_1, and B_1 involves B_2.
   (iii) Therefore if A_1 is B_1, A_2 is B_2, and if A is B, A_2 is B_2.

III. Then again, can A_2 be B_2? and so on.

So that unconsciously these mathematicians *use* no other conceptions of space than the Euclidean, none other of course being possible for thought, while they imagine themselves to be talking of another kind of space. The origin of the illusion is obvious.

§ 320. To corroborate the view that hyperbolic space is another kind of space, under the generic idea of space, it is usual to point out that the various theorems in hyperbolic geometry have their analogues in the Euclidean system, so that the latter can be derived from the former by certain suitable changes. This is an elementary mistake in elementary logic. For the relation in question may hold between a system of true theorems and a system known to be false.

Suppose we have a system of theorems derived from a theorem in which certain elements A, B, C are connected in a certain way Suppose we make some change in A so that it becomes A_1 and then let the connexion which the theorem would make between A, B, and C after this substitution be untrue. The change of A into A_1 will involve a corresponding change in all the derived theorems and we shall have a system of false theorems side by side with the original system of true theorems. Now in the first of these, by changing A_1 into A we shall recover the true theorem about A, B, and C. Similarly, by making the analogous change in each of the derived false theorems, we shall recover each of the true theorems.

§ 321.[a] There is a fallacy which has helped towards making the illusion of extra-dimensional space seem plausible. It is supposed we have 'space of one dimension' in the line, 'of two' in the plane surface, and 'of three' in the solid body.

[a] He is referring to an amusing little book by Edwin Abbott called 'Flatland'.]

It is falsely considered that these three dimensions are three different spaces and that it is a mere matter of adding one dimension to another. Thus the idea suggests itself that the addition can be continued beyond three (*ad lib.*). The dimensions are in fact treated like units independent of one another, each like the other and so capable of addition. This is a serious fallacy. 'Dimensions' cannot be treated as independent units. There is no space of one dimension and none of two; nor does space of three dimensions arise by addition of dimensions. It is not formed by addition and it cannot be increased by addition. The so-called two-dimensional space necessarily presupposes and is a part of space of three dimensions. The one-, two-, and three-dimensional space are in organic unity and cannot exist apart. They presuppose one another and form parts of an inseparable whole

To illustrate: the idea of a plane is quite impossible except with reference to space on both sides of it. Instead of a third dimension being added by erecting a perpendicular on the plane, the plane as such necessitates the perpendicular, and a plane as a plane necessitates three-dimensional space.

VIII

THE MATHEMATICAL EXPRESSION OF PROBABILITY AND ITS RELATION TO REALITY

§ 322.[a] We have spoken of our belief or feeling of confidence depending partly on what we call the *strength* of the evidence, and have said, what is indeed obvious, that what we call the probability of an occurrence does not express anything objective but the degree of confidence which we entertain, a purely subjective matter. We tend, however, to forget that strength of evidence is merely something for us, and this leads to a fallacy which is often illustrated in the treatment by mathematicians of the doctrine of probability.

Knowledge has no degrees, any more than reality. There is nothing in reality to intervene between being and not being, nothing in knowledge between truth and falsehood. But, though there are no degrees in knowledge, there is, in our minds, something between absolute ignorance, the refraining from pronouncing a judgement, and knowledge. This is the region of belief and here only differences of degree are possible. We express these degrees commonly by the words probable and possible and the expressions more or less probable. The lowest expression of belief is that the thing is possible, a higher degree is denoted by probable, and degrees of this are indicated by more and less. But though possible is applied in one way to the lowest degree of belief, the word improbable is used commonly of something lower still; not, however, usually of our own beliefs. We contrast an improbable opinion with another which we call possible or more probable, and this latter would be our own belief. An improbable belief, then, is one that is rejected relatively to another, it cannot be rejected absolutely, however improbable we may estimate it to be. It must be in the category of what is possible, and if we reject it we pronounce it impossible,

[a cf. § 51, 92 This and the next two sections are early lectures as delivered. §§ 325–7 are from his article in *Nature*, 12 xii 00.]

not improbable. Now, as we said above, what influences the mind in these degrees of belief has no corresponding influence over facts, and the terms possible and probable are therefore alien to the objective, to reality, and merely express a subjective attitude.

§ 323. We may here offer an outline of an analysis of this subjective attitude Consider first the probability of a universal statement, all S is P. This implies the necessary conditioning of S by P. Now from the side of the experience of particulars we may approximate to belief in this universal statement in the manner described under the head of *enumeratio simplex* or by the method of agreement. This is usually called arguing from particular instances of a rule to the universality of the rule. For this purpose the quality of the instances is really important, the quantity being of advantage only because it increases the probability that a sufficient variety of instances will be examined. The more instances we take, the more probable we think it is that they should be varied, and if S and P have remained permanent while the other elements varied, we think that they are necessarily connected. But now this is just the problem that we have to examine, for it is obvious that we cannot be sure that while we increased the number we have in the same proportion, or even at all, increased the variation. It is thus possible that the larger quantity has not in fact increased the variation, and we may therefore be altogether mistaken in giving more weight to the increased number of instances than to those we had to begin with. How, then, is it that the mind, notwithstanding this possibility, inclines to attach more value to the larger number of instances?

The general answer to all questions like this seems to be that the mind inclines to that side in favour of which there are at all events the larger number of possibilities, other things being equal, that is, when we can't or don't distinguish between the qualities of the possibilities. In the case before us, then, suppose that one instance is observed in which S is P. Suppose next a second in which S is also P. There is now a possibility that this instance may vary in its other elements. A third instance will give a further possibility, and thus we increase with each instance the number of possibilities, at all events, of a variation

of the different elements. Now the second case of S which we approach might, so far as we know beforehand, not be P. If, then, it turns out to be P, one possibility of the contradictory of all S is P is destroyed. The same is true of further instances, and as a matter of fact our mental inclination, our belief, strengthens as the number of instances which might have been contradictory but are not increases. Whether this is rational or not, at least it appears to be the fact. A simple instance will illustrate it. Suppose that we are entirely ignorant of the conditions under which a die is thrown, but know that it has six faces differently marked, one of which is the ace. The question might arise whether it is more probable that the ace or *any* one of the other numbers would turn up on a single throw. Now we can't distinguish between the value of the possibilities that the ace, the deuce, or any other particular number will be thrown. Thus we have one possibility in favour of the ace and five possibilities of something not the ace coming uppermost, and we should in fact say it was more likely that a number not the ace would be thrown than the ace. The mind, we see in this instance, chooses the side on which the larger number of possibilities are. In this there is at least a certain appearance of rationality, since from the point of view of our knowledge the appearance or non-appearance of the ace, both possible cases, are not on the same footing. If we have to choose, then, there is a difference, so to speak, in favour of one side, and we take that side, knowing nevertheless that the difference may be only apparent and that the favoured event may be the one that does not happen. It is a decision of the will, just as if a man should come to two roads quite unacquainted with their difference in respect of his destination. He must decide somehow, and he may, his mind being otherwise absolutely undetermined, choose the one in which he happens to see something consistent with the hypothesis that it is the right road. That indication, however slight, serves to put an end to his indeterminate attitude. We may, then, perhaps thus explain our attitude when we assign more probability to the proposition all S is P as the number of instances increases.

§ 324. Take next the singular statement, the answer to the question 'is this S a P?'. Either we know the conditions of the

appearance of P in S or not. Suppose we don't, the lowest point of knowledge is to know nothing against the appearance of P in S. Here we may say that the belief is for us a possible one. Secondly, though we do not know that the elements, say, *a*, *b*, of S condition P, yet from particular experience we may have reached a belief, more or less probable, that they do. This is obviously only an instance of our first case of all. Suppose, however, we know the conditions of the presence of P but do not know whether they are present. As before we have first the case in which we know nothing but assume that P is present on the grounds of mere possibility. Next we may know that one of the conditions, say *a*, is present in S. If, then, we have nothing to influence us from the side of the *absent* conditions, our belief in the probability of the presence of P will increase with the number of conditions we know to be actually present. Thus if *a*, *b*, *c*, *d* be the conditions, finding *a* and *b* we are more ready to believe than if we found *a* alone, and finding *a*, *b*, and *c* we are still more ready to believe We have, in fact, an application of our formula that our belief increases in degree with the increasing number of possibilities in favour of the statement in question, an increase which has an important negative side, the removal of one obstacle after another to the truth of the statement.

It remains to consider the case where we are influenced by the remaining conditions, those about whose presence we are at present uncertain The measure of probability we have arrived at will now be affected by the probability of the presence of these conditions. Clearly we may here have to take into account the probability of the existence of *their* conditions, if we know them, but this process must stop at last with the question of the probability of the association of the ultimate group, say, of those conditions with some other facts either in the present or the past. Here we return to the first case of all and the probability will be estimated in that way. If, however, we don't know the conditions upon which these conditions depend, we shall have again the question as to the probability of the association of these conditions with certain facts before us, and this again is the first case of all.

So far we have considered the opposition of a single fact to

its negation and not to a definite number of possible alternatives. There are cases, however, and these are the most ordinary ones in the mathematical theory of Probability, where to a given fact is opposed a limited number of alternatives, as in the instance of throwing the die. The question here arises of the comparative probability of any one element being realized, given that we are certain that one or other of the enumerated alternatives will be realized. We require some sort of common measure of the possibilities in favour of each element in order that we may understand how it is that we incline to one rather than to all the rest, or to one rather than another of a given pair, and so on. The mathematical theory of probability is an attempt to reach such a common measure, and though there are many complicated calculations in this part of mathematics, the principle of them all admits of simple statement.

§ 325. We may avoid certain confusions which often arise in the statement and application of the mathematical theory if we form a clear idea of what is meant exactly by the fraction which is said to represent the probability of an event.

A good statement of the ordinary account of it is given in Todhunter's *Algebra*: 'If an event may happen in a ways and fail in b ways, and all these ways are equally likely to occur, the probability of its happening is $\frac{a}{a+b}$, and the probability of its failing is $\frac{b}{a+b}$. This may be regarded as a definition of the meaning of the word probability in mathematical works.' A definition must not assume and use the notion to be defined. Here probability is defined through cases 'equally likely to occur'; but 'equally likely to occur' means equally probable, and so the definition assumes the very notion which causes difficulty, and of which we require the explanation.

The first thing to settle is the meaning of these 'equally likely' cases. Is the equal likelihood a quality in things themselves, or is it something in our minds only? If it is a quality in things it can only mean equal possibility of occurrence or realization. But if a number of cases, mutually exclusive as intended in the above definition, were in the nature of things equally possible, not one of them could happen. If the claim

of any one of them on reality were satisfied, so must the claim of any other, since these claims are equal, and therefore if one happens all must. But by hypothesis if one happens no other can; thus the only possible alternative is that none of them can happen. (It is precisely on this principle that we decide that the resultant at a point of two equal forces, whose directions include an angle, cannot be in any other direction than the bisector of the angle, and that there can be no resultant of two equal forces which act in opposite directions.)

The equal likelihood then intended cannot be anything in the nature of things, because it is assumed that one of the equally likely cases will happen. It is really only in our minds, where there is an equal balance of reasons for and against two or more events, and is due solely to our ignorance, since if we knew which was to happen there could be no such balance and indecision. This is clear if we consider what is the reason why we pronounce one event more likely or probable than another; it is because we think there is more evidence in favour of the one than in favour of the other, however the 'more' may happen to be measured Two events are equally likely for us when we know nothing more in favour of the one than we do of the other; when the state of our knowledge—and (it is important to add) of our ignorance—is the same for both contingencies. This view agrees with the actual procedure in mathematical examples. If a bag contains n balls, and one is to be drawn 'at random', there are said to be n equally likely cases; that is, each of the n balls is equally likely to be drawn. Clearly this only means that, as we don't know how the hand is going into the bag, we have no information in favour of the drawing of any one ball as compared with any other and no information against the drawing of any one as compared with any other.

'Equally likely' cases then being such that owing to our ignorance the evidence in favour of any one is no greater or less than the evidence in favour of any other, the meaning of the definition of probability criticized above is evident. It is not a definition of probability, but it is the definition of a certain way of measuring evidence. We are entitled to say that one event is more probable than another when, the evidence before us being decisive for neither, that in favour of the one seems

to us, according to some standard of measurement, greater than the evidence for the other.

§ 326. Now what the mathematical analysis does is not to alter the ordinary meaning of 'probability' at all, but to find a standard for the measurement of the more and less in evidence. The whole possibility before us in any given contingency is divided into a number of cases, 'equally likely' or 'equally possible', in the sense that they are equal from the point of view of the evidence in favour of each of them; then if one event has more of these equal possibilities in its favour than another, it has in this sense 'more' evidence in its favour, and so, in accordance with the usual meaning (as above described) of 'more probable', is more probable than the other. And here the 'more or less' in the evidence is not a mere 'more or less', but has a definite numerical measure. The evidence being, so to speak, divided into equal units, the strength of the evidence in favour of a contingency is measured by the number of these units in its favour. Thus if the total of equal possibilities, one of which must happen, for the events A, B, and C is n, of which a involve A, b involve B, and c involve C, the comparative strength of the evidence in favour of A, B, and C respectively is measured by the ratios $a : b : c$, while the strength of the evidence for A, B, and C respectively, as compared with the evidence for one or other of them happening (which is certain), is, on the same principle, measured by the ratios $\frac{a}{n}$, $\frac{b}{n}$, and $\frac{c}{n}$. If, then, we symbolize the strength of the evidence for A, B, and C by $\frac{a}{n}$, $\frac{b}{n}$, and $\frac{c}{n}$, and similarly that for one or other of them happening by $\frac{n}{n} = 1$, these quantities have to one another the ratios required. We then arrive at the true meaning of the fraction which is said in mathematics to be the 'probability' of a contingency; and much confusion might be avoided if we called the fraction, not the 'probability', but the '*modulus of the evidence*', and the so-called equally likely cases not 'equally likely' but '*equi-evidential*', or by some more convenient name conveying the same idea.

§ 327. It must, however, be insisted that the above is only

one way of measuring the evidence, and is not applicable to all cases. Indeed, the more important matters of daily life usually do not admit of it, for there are qualitative differences in strength of evidence which cannot really be measured quantitatively, and that is why the application of mathematical probability to the testimony of witnesses is so obviously futile.

The solution of every mathematical problem in probability is in the last resort only the finding of a modulus of evidence, in the *ratio* of the part of the whole number of equi-evidential cases which involve a given contingency, to the whole number of such cases; and with the finding of the modulus the strictly mathematical work ends. Mathematics, as such, has nothing to do with the inclination in our minds to expect the event for which the modulus of evidence is greatest (or 'the probability' greatest), or the inclination, when some practical step has to be taken, to act on the hypothesis that the event will happen for which the evidence to us seems strongest. Unfortunately, however, there is too often a tendency to confuse the mathematical measure of the mere state of our minds with the measure of something in reality. This is an insidious fallacy and we are not unlikely to fall into it in one form when we have escaped it in another. It is the origin of various mistakes—e. g. the inclination to expect that the actual proportion of the occurrences of the event will tend to conform to the proportion represented by the mathematical probability, in other words, conform to a formula of our ignorance. The mistake of supposing that the mathematical probability could be confirmed by actual observation belongs to this head. The attempt to regulate betting by mathematical probability is another instance of the fallacy of confusing the subjective with the objective. The truth is that an observed average may be made the basis of a mathematical 'probability' or modulus of evidence, by a process which could easily be explained; but, though a 'probability' may be based on an average, an average can never be based on a 'probability'.

PART IV

SPECIAL LOGIC

I

INDUCTION. THE GENERAL FORM OF THE EXPERIMENTAL METHODS

§ 328. The recognition of Induction as a method seems to come about in two ways. It arises first with the syllogistic logic itself, because the ultimate premisses of syllogistic proof cannot be given by the syllogism. Thus, if they are to be got by any process at all as opposed to direct intuition, that process (not being inference from a universal) can, it is supposed, only be some sort of inference from a particular. Secondly, it arises from certain facts of experience, because in scientific inquiry generalizations are met with which cannot be demonstrated in the manner of the exact sciences but are founded upon the observation of particular instances.

The general notion common to all ideas of induction seems at least to be this, that it is a process in which we arrive at a universal statement of the form 'all A is B' by the consideration of particular cases of A which are B; and this general definition can be fairly maintained. But the definition is commonly stated in a wider form, viz. that it is a process in which we start from singular statements and arrive at a universal conclusion (of which the singulars are instances) without presupposing a universal statement, that is, without deriving or *deducing* from that.

There is another way of looking at induction. Deduction is conceived as (1) inferring the particular from the universal, and (2) as inferring a consequence from its reason or ground or condition, where *condition* is used in its general sense and includes *cause*. In Aristotelian terminology, deduction is regarded as arguing from the universal to the particular, or from the condition or ground to that of which it is the condition[1] (and according to Aristotle the two should coincide in the perfect scientific demonstration)

[1] *καθ' ὅλου, καθ' ἕκαστον, αἴτιον, οὗ αἴτιον.*

Induction by contrast is considered[1] to be the inverse process to deduction, and accordingly as either arguing from the particular to the universal (the usual form of the conception of induction) or as arguing from the conditioned to its condition (of which a special case is arguing from effect to cause); the process, that is, of inferring from something what its condition is, whether that condition is cause or not. Thus it is assumed that *as* such an inverse process it is different in method from deduction. This is uncritical because it assumes without examination that the relation of condition and conditioned is not reciprocal but one-sided, and further that this involves in consequence a difference in method.

Now we have seen[2] that the relation of the true condition to what it conditions is reciprocal, in so far as each necessitates the existence of the other: though there are cases in which there is a priority of the one over the other in the order of our thinking.

This being so, it does not follow that the method or process of inferring the 'result' from its 'condition' must be different from that of inferring the 'condition' from its result. And we have seen that in geometry it is actually the case that the process is the same, even when there is a priority of one to the other in the order of our thought.

Where there is no such priority we prove the converse of the proposition without assuming the proposition itself and the method is as direct in the one case as in the other. The term inverse is not confined to one of them: each of the proofs is the inverse of the other.[3] But when there is the kind of priority referred to, so that the converse of a proposition is proved by assuming the proposition (whether with or without[4] *reductio ad absurdum* [a]), the proof of the proposition is the same in *method*

[1] As, for instance, by Jevons [See, e g, *The Principles of Science*, chs vi, vii, xi, and xii]

[2] § 259.

[3] e. g a direct proof of Euclid, *Elements*, i 6, or of the converse of *Elements*, iii. 31 (the angle in a semicircle is a right angle)

[4] With *red ad abs* as in *Elements*, i 7, or without, as in *Elements*, i. 48 (converse of Pythagoras's theorem).

[a Euclid, *Elements*, i. 40, can be proved either with (as in Euclid) or without *red. ad abs*. He also used his direct proof of the converse of i. 32; cf. § 259.]

as the proof of its converse. Both consist of geometrical intuitions or constructions.

In the first case neither proof can be called the inverse proof because, each being independent, each is the 'inverse' of the other, in the sense that the one is the converse of the other and the one may give the stages of the other in inverse order.

In the second case the proofs are not independent and so the first couldn't be called the inverse of the other, in so far as it is really contained in the other, nor does the dependent proof reverse the stages of the independent. Each is, however, still 'converse' in its conclusion as compared with the other. But the essential thing is that, while the 'inverse' method does *not* apply to the first case, the procedure in the second case is exactly the same in kind in the dependent process as in the independent, that is, whether the inverse method applies to it (is employed to demonstrate it) or not.

Thus the given definition of induction does not seem justified, and if the account already given of the nature of a mathematical demonstration is true, it is evident that the search for induction cannot properly originate in the unreal distinction between axioms and demonstration. So far as the demand for induction has arisen in this way, it is from a complete misunderstanding of the nature of mathematical demonstration and is part of a theory which supposes that the formal operation of the syllogism advances science by synthetical propositions beyond its original data. The secret of it all, i. e. the recognition of the universal and essential connexion in the particular instances, seems never to have occurred to these inductionists, and hence, in part, has arisen their misunderstanding of Aristotle's position when he speaks of the induction of principles or axioms.[1]

§ 329. From what has been said of mathematical demonstration it follows that the idea of induction ought not to be traced to a distinction of ultimate axioms and demonstrated propositions, since that distinction, as we saw, involves a mistake. So far as it has had such an origin, a serious misunderstanding of the nature of demonstration is implied. Moreover, this mistake has had a misleading effect upon the theory of induction itself; because the true secret of mathematical method, which

[1] See note to § 329.

is the possibility of recognizing the universal in the particular (i. e. a universal truth represented by a universal judgement), has never occurred to the inductive logicians. It has affected also the understanding of the Aristotelian account of 'epagoge',[1] which is often taken to imply the view that axioms are discovered by inductive inference from particular instances. The truth is that with Aristotle the axioms are self-evident, and the induction is simply the mental process in which we recognize the universal apart from the particular. When this recognition is of axioms there is added 'intelligence',[2] as the intuition of their universal validity. In modern induction two different forms are really recognized, about the relation of which to one another there is the greatest confusion. One is *enumeratio simplex*,[a] the other is often called 'induction' simply, and comprises what are called the experimental methods. This is supposed to be the essentially modern form and we may therefore term it modern induction. The former is characterized by Bacon and his followers as the loose uncertain method practised by the ancients; and they often profess to replace it by the new induction which they think of as a modern discovery: while they speak of the old as a fallacy to be forgotten.

But we find to our surprise that the despised *enumeratio simplex* is not only retained with a special function of its own which the 'new induction' cannot fulfil, but has an absolutely necessary place as a part of the new induction itself, and cannot be got rid of. That is, these logicians are not only driven to retain *enumeratio simplex* as their only possible account of the axioms of mathematics, but the more modern writers (as opposed to Bacon, who would not carry his analysis so far) are driven to recognize that the new induction itself depends entirely upon an axiom called the axiom of the uniformity of nature or of universal causation. And here again they have to resort to *enumeratio simplex* to establish the axiom.[b] But worse than this, we shall find that the reasoning of the new induction as

[1] ἐπαγωγή, Latin *inductio*

[2] νοῦς, intelligence [as distinguished from ἐπιστήμη, scientific understanding and science].

[a] Described shortly by Mill, *System of Logic*, iii. 3, § 2.

[b] Mill, ib., iii. 21, §§ 2 and 3.]

such is what these logicians are bound to call deduction or syllogism. Thus the only element which could be called inductive in the vaunted improvement upon the *enumeratio simplex*, is just this *enumeratio simplex* itself. There is a further confusion of analysis: we are told that the axiom of induction is itself the result of induction. Now no process can possibly prove its own presuppositions. Thus we have either this elementary fallacy or the *circulus in probando* must be avoided by supposing an ambiguity of terms. For the process called induction which supplies the axiom cannot be the same in kind as the process which works from the axiom. This seems to be the fair account of the matter. The induction which presupposes the axiom is the new induction. The induction which proves the axiom is the old *enumeratio simplex*. This necessitates the question, why should two processes apparently different in kind both be called induction? What have they in common? These logicians, however, are so unconscious of this difficulty that they don't even raise it.

NOTE. Aristotle's authority could probably be cited for the view that the axioms of the exact sciences are got by *induction*. In connexion with syllogism he says, 'those are first principles which do not depend on syllogism, they depend therefore on induction'.[1] But he nowhere gives an account of the induction of axioms by which they would appear, as in a modern inductionist, to be the conclusion of an inferring process from experience. Moreover, he says more than once [2] that the first principles of these sciences are self-evident—the exact opposite of what is understood by an inductive origin. Moreover, as we have said briefly above, what he describes as the *induction* of first principles [3] is quite obviously the gradual process by which the mind comes to recognize, in the abstract and apart from all particular instances, a principle which it has been using in the particular instances. This is not the inference of the validity of the

[1] ἡ μὲν δὴ ἐπαγωγὴ ἀρχή ἐστι καὶ τοῦ καθόλου, ὁ δὲ συλλογισμὸς ἐκ τῶν καθόλου. εἰσὶν ἄρα ἀρχαὶ ἐξ ὧν ὁ συλλογισμός, ὧν οὐκ ἔστι συλλογισμός ἐπαγωγὴ ἄρα. *E. N.* 1139^{b} 28–31

[2] *Top.* 100^{b} 18, *An. Pr* 64^{b} 34–6.

[3] *An. Po* II. 19, especially 100^{b} 3–15 νοῦς ἂν εἴη τῶν ἀρχῶν . . . . . ἐπιστήμης ἀρχή

principles from the validity of the particular instances as such. When he adds that the faculty of principles is intelligence (νοῦς) and that this is the most exact kind of knowledge, this is just what we should expect from a writer who grounded all knowledge on experience and at the same time made the principles of experience self-evident. Intelligence is the intuitive faculty of reason by which we recognize this self-evidence, when an *induction* from experience has brought us to think of the universal principles in abstraction from the particulars in which we first thought the universal.

§ 330. It is usual, perhaps universal, to regard as ultimate and irreducible the ordinary formulation of *enumeratio simplex*: and there seems to be no idea that its presuppositions have to be discovered or examined.

The general formula of *enumeratio simplex* may be fairly described as follows. This A is X, that A is X, &c., all observed A's are X. Whence the conclusion (as probable, not certain), all A is X. This is what has seemed to be ultimate, and it is here therefore that our inquiry must begin. The first thing to notice is that the instances of A and X are also instances of a good many other things. This A is X is really (e. g.) ABC is X. The important thing, then, is that in the conclusion all A is X, we have selected one element A out of the complex ABC rather than another as the subject of X in the universal statement all A is X The first question therefore is, What is the ground of our selection? Now suppose that the examples of A were all indistinguishable in quality and the corresponding premisses therefore each of the form 'this AB is X'; clearly a thousand such will be no more use than one for the selection of A: there would be no reason for preferring A to B. Thus we arrive at the next step. Our instances, to effect selection, must be different in quality. Our premisses must be this AB is X, this AC is X, this AD is X; and these incline us to select A as the subject of X. And here we strike upon the essential principle of the selection and have now to ask for the presuppositions of this rejection of the elements which differ, and selection of what is common. It will be noticed that the analysis now resembles the method of *agreement* in the new induction;

and the truth is that that method is but a particular case of *enumeratio simplex*, i. e. the application of it to cases of causation. The modern analysis of this method then is a step, though an entirely unconscious step, towards the formulation of the true process of *enumeratio simplex*. We shall find, too, that as this method of agreement is admitted by the inductionists to depend upon an axiom, so also does the *enumeratio simplex*, a result totally unexpected from the inductionist point of view and even totally contradictory to their theory of the experimental methods.

§ 331. The premisses of the *enumeratio simplex* argument being this A is X, &c., the interest of the conclusion, all A is X, clearly lies in the fact that it is not an enumeration of observed cases, but goes beyond them. This universality means that A *as such* is supposed to necessitate X; or the quality of A-ness necessitates X-ness. This necessitation has not been observed in any of the premisses; for, if it had been, a single instance would have been enough, as it is in geometry, and there would be no need of an appeal to any further instances of what is called the inductive process. The premisses, then, do not give the element of necessary connexion which makes the conclusion of importance. This of itself is enough to show that the process as usually analysed cannot be ultimate: there must be something behind it. This we may see by putting the matter in another way. The premisses are not merely related (as ordinarily supposed) to the conclusion as particular to universal; for the particular that corresponds to the real meaning of the conclusion, which meaning is that A as such necessitates X, must be a particular in which we see that A in that case necessitates X. That is the only kind of particular which would correspond to the given universal: whereas that is not the character of the statements which constitute the premisses, for if it were we should be able to draw the conclusion from a single instance.

The premisses, then, are not sufficient and require to be supplemented, but still it will be said that they *are* evidence in favour of the conclusion. Now how are they evidence? The act of selection, which we have described as the important thing, shows us what is presupposed and how the particulars can be regarded as evidence. Before we can undertake such a selection,

which is directed towards finding what element another element necessitates, we must already have the idea of necessary connexion, as opposed to the mere coexistence of two elements together at a given time in our consciousness. Thus we argue that if A conditions X, A and X must be always realized together in the particular instances of A: that follows from our idea of necessitating. In the case before us the facts of observation so far conform to this condition that X is always found with A as far as we know; and thus the mere coexistence of A and X which we observe becomes so much evidence in favour of their necessary connexion. But this is not enough. We argue further that any element not always found with A cannot be conditioned by A; consequently in AB is X, AC is X, if the analysis is correct, B cannot be conditioned by A, because it does not appear in AC is X; and similarly with C. Thus, as a result of elimination, there is left only X which could be conditioned by A. We may put this accurately thus · If in a complex ABX and a complex ACX, A conditions anything, that cannot be either B or C, and therefore if it conditions anything, that must be either X or a part of X. Now whether we represent these processes clearly to ourselves or not, they are obviously what the mind must go through in an argument by *enumeratio simplex*, however elementary. We do not proceed absolutely from the more particular to the more general: the particular premisses, instead of being the starting-points, are only of use when subsumed under this general premiss [a] of necessary connexion. The process itself indeed is what the inductive logicians would be obliged to call deductive, and purely deductive. And, for the matter of that, *enumeratio simplex* argument differs from strict demonstration not in its form but in its matter; the form is accurate, but we are not sure of our analysis of the objects of experience expressed in the individual premisses. If we were sure, two instances *might* be enough to eliminate all but X from what could be conditioned by A; '*might be*', because if we were aware that the two instances of A, which we have assumed so far to be identical, may be instances of a species to which other and different species correspond, we might require further instances before accepting our universal conclusion.

[a] It is an axiom rather than a premiss Cf § 330 and note to § 242.]

§ 332. The number of instances [a] is too often represented as if by their mere multitude they contributed to the universality of the conclusion This is the fallacy of supposing that any finite number, however large, can be an approximation to the infinite and is analogous to that other fallacy of treating the universal statement as if it represented the totality of the particulars. We have seen already that the mere *number* of instances as a number is of no value, because, if the quality of the instances is indistinguishable, however you increase them, they are of no more use than the first for selecting X as conditioned by A. To be of value they must differ in quality, and it is because of this that there must be a plurality.

Now if our analysis ABX, ACX, were certain we should require no further instances; but in practice it is quite impossible to be sure that our analysis of particular instances is complete. We are not sure that X is the only element beside A remaining invariable in the two instances; some we notice vary, but we are not sure of the extent of the variation. Now what we do in attaching importance to a large number of instances is to assume that the more instances we take having A and X in common, the greater the chance that we have varied all the other elements except A and X. This, then, is the true and only reason for attaching importance to the number of instances. Thus in a way their larger quantity makes up for their defect in quality, or, their quantity becomes a kind of substitute for two instances perfect in quality. This also explains how the ordinary presentation of the *enumeratio simplex* argument, i. e merely in the form—this A is X, that A is X, &c., can be justified. It is really because the plurality of instances tacitly implies the probable variation of all the elements except A and X. Otherwise the failure to call any attention

[a 'Now the precariousness of the method of simple enumeration is in an inverse ratio to the largeness of the generalization . . As the sphere widens, this unscientific method becomes less and less liable to mislead, and the most universal class of truths, the law of causation, for instance, and the principles of number and of geometry, are duly and satisfactorily proved by that method alone, nor are they susceptible of any other proof.' Mill, id., III. 21, § 3 But in III 2, § 1, he says, 'It is the number and the nature of the instances, &c' H. Spencer lays stress on this point; see Mill, id., II. 7, § 2.]

to the variation of quality would vitiate the procedure in the manner already described.

§ 333. In the foregoing inquiry we have used the idea of the conditioning as opposed to the conditioned. The argument turns on the assumption that A is the conditioning. As we have seen, it really comes to this as its result, that if A conditions anything it conditions X. But we cannot convert the probable statement all A is X and say all X is A, without some express stipulation about the reciprocity of the conditioning and the conditioned. Now the ordinary analysis of the *enumeratio simplex* inference gives the result all A is X, without ever inquiring why the result should not have been all X is A. Yet as the premisses give the mere conjunction of A and X in the same subject, both are on the same footing and the ordinary analysis is helpless before the question why the conclusion should be all A is X and not conversely. If we do select one form in preference to the other, it must be on some grounds extraneous to the premisses as ordinarily presented. In fact we require to add that, in getting the conclusion all A is X, we have assumed that A may condition something, and our conclusion is, accurately, only the statement that if A conditions anything, it (probably) conditions X. This ambiguity in the ordinary method would certainly appear in the method of agreement, if that were only an instance of the general form of *enumeratio simplex* without anything added about the relation of A and X But, first, there is a distinction between conditioning and conditioned made and presupposed in that argument. In the instances taken, ABC and $\alpha\beta\gamma$ are not on the same footing; ABC are conceived as prior in time and already conditioning $\alpha\beta\gamma$, the method only settles which element determines which. The question really is, What does A condition as subsequent to itself? (We must remember it is correct to ask, what does α condition as preceding itself?) Secondly, the ambiguity does appear to a certain extent and shows the necessity for considering whether we suppose the relation of conditioned and conditioning to be reciprocal. For in Mill's account it appears that an ambiguity is caused by his doctrine of plurality of causes, a difficulty which would not arise in that form at least (i. e. as he puts it) if he had believed the contradictory of the 'plurality of causes', i. e. the reciprocity of

conditioning and conditioned.[1] We shall as a matter of fact see that the validity of the method is not really altered, as Mill supposes, by the assumption of a plurality of causes.

§ 334. In the following investigation we are not trying to find an ideal of inductive method but first to understand the true nature of the methods actually used in science and ordinary life, methods which are called the experimental methods and usually considered to be inductive, and secondly, to discuss the account given of these methods by the inductive logicians.

The method of difference as described, e g. in Mill, deals with the relation of antecedents and consequents. We are supposed to have given us at least two cases in experience, symbolized as follows:

Antecedents, ABC, BC.
Consequents, $\alpha\beta\gamma$, $\beta\gamma$.

In such a process as Mill describes we assume in the argument first that every antecedent necessarily conditions some consequent and that every consequent is conditioned by some antecedent; and secondly, that we have been able with some probability to isolate ABC and $\alpha\beta\gamma$ from the remainder of the universe, so that we have only to look for the effects of ABC in $\alpha\beta\gamma$, and the causes of $\alpha\beta\gamma$ in ABC. We assume, lastly, that every antecedent necessarily conditions the same consequent, i. e. the same in kind whenever it occurs; we assume, in other words, that the same cause has the same effect.

From these assumptions we argue that in the second instances BC causes $\beta\gamma$; for, *ex hypothesi*, the effects of BC are to be found in $\beta\gamma$, and the cause of $\beta\gamma$ in BC. If, then, there were one effect in $\beta\gamma$ not conditioned by BC, we should have a consequent not conditioned by any antecedent, i. e. an event without a cause: similarly, if there were an element in BC which had no effect in $\beta\gamma$ there would be an event without an effect. Thus $\beta\gamma$ is the whole effect of BC, and BC the whole cause of $\beta\gamma$. But the same cause has always the same effect: therefore in the first instances also BC causes $\beta\gamma$; similarly BC does not cause α, for if it did it must have caused it in the second instances

[1] Add to this that the argument only enables us to say that if A conditions anything it is X or a part of X, not that it is the whole of X, hence conversion is impossible.

from which α is absent: therefore if A has an effect it is α; but every event has an effect: therefore A causes α.

Now whether we have a right to consider BC the same for this purpose both when alone and in company with A, or not, that is certainly the presupposition of the argument and the meaning of the symbolism. The inductive logic is here describing an ideal of reasoning, to which we endeavour to approximate, in the sense that the premisses which cover the particular instances are assumed to be correct.[1] Now clearly the argument is demonstrative, and the inductionists in their opposition of induction to deduction would be obliged to call it deduction. There is no uncertainty in the form of the argument: the uncertainty lies only in the matter. The premisses contain presuppositions comprised in what is called the law of the uniformity of Nature or the law of universal causation; and to these we must add the assumption of the isolation of instances.

§ 335. The argument presupposes that a given set of occurrences can be isolated from everything which is happening at the same time in the universe. This assumption is clearly of the last importance practically, and the inductionist ought to consider how it is that he can come to make it. Inductive logic such as *Mill's* can hardly be said to face the difficulty; indeed such a system as his gives no means whatever for making the distinction and for such a system the question is insoluble. For with him the elements related to one another and to be connected as cause and effect are treated as mere antecedent and consequent, i. e. as events. So far, then, their only bond is succession in time, which, properly speaking, is no bond at all. And, if this were all, it would be difficult to understand how any definite number of events could ever be grouped together so that we could possibly say, 'this antecedent belongs to this consequent', inasmuch as all events at a given time, if we think of them only under the form of time, are equally near to or remote from every other event, however remote in space; an event happening here and now is absolutely contemporaneous with another event on the other side of the world; and a process beginning here and now succeeds as immediately in time to

[1] i e. we are sure of what we observe and are sure that we have isolate ABC and $\alpha\beta\gamma$.

a process now ending on the other side of the world, as it succeeds to the processes and events which preceded here. As regards mere time it is quite as near to one set as to the other. And the difficulty is a real one for Mill, when we reflect that, apart from logic, in such a metaphysic as Mill's, the Real consists solely of events in time. The idea of any permanent substance or substances (which might serve to group together or connect events as somehow belonging to it) is pronounced a mere fiction of subjective thought and the result of 'the association of ideas'. It is true that in practice the difficulty is concealed because the events are by Mill himself (i. e. in the concrete examples) referred to subjects of them other than *events*. The truth is that the description of these methods in such books as Mill's is accompanied by an irretrievable confusion as to the meaning of such words as cause, effect, agent, patient, condition, and the like.

But, however that may be, when Mill comes to deal with the definite processes of inference, in these methods, cause and effect mean in his formulation respectively antecedent event and sequent event between which there is assumed to be a necessary connexion: for that is what Mill means in spite of what he says. He purposely avoids 'necessary' and 'necessary connexion'—and with good reason, for these terms are fatal to him—and speaks only of 'invariable unconditional sequence'.[1] We find, what he is indeed anxious to tell us, that he does not mean invariable sequence merely; and it emerges that by an odd perversion of language by 'unconditional' he means 'conditioned' sequence. He uses unconditional because he means that the effect *must* follow the cause and thus cannot be prevented by any condition. Thus he is committed to Necessity.

§ 336. In the method of agreement we have two instances symbolized as before, but the second instances contain A in the antecedent and α in the consequent Thus.

ABC	ADE
$\alpha\beta\gamma$	$\alpha\delta\epsilon$

The same assumptions as before being made, the argument would be: every cause while remaining identical has the same effect: therefore the effect of A, A being the same in both sets

[1] Mill, l c iii 21, § 1.

of antecedents, must be found in both sets of consequents. That which is common to both sets is α; hence the effect of A must be found in α. It may be shown as before that this argument is entirely deductive. Further, notice that we do not come upon any difficulty so far about the plurality of causes, which Mill supposes to be the peculiar drawback of this method. He supposes that since the event α might have different causes at different times, it might be caused the first time by B and the second time by D, and have nothing to do with A. This is another example of the weakness in logical analysis which is found in every part of Mill's account of inference. He argues from the general assumption that α might have different causes at different times, without noticing what is entirely obvious, that in the particular case before him the data make this quite impossible for the two times represented by the two sets of instances. He is prevented from seeing this because he happens to begin with the consequents. If he had only started with the antecedents, he could hardly have failed to see that α must be necessarily connected with A. Thus even if the doctrine of plurality of causes were true, as in the strict sense it is not, Mill's difficulty is unreal, and if we assume that the plurality of causes is true, the only difference it would make would be this, we should have proved that the effect of A must lie *in* α, and if α is something which admits of no further analysis, we should have proved that A causes all α. If, however, we suppose α resolvable into x and y, though x may be necessarily connected with A, it might be that the remainder y was really caused the first time by B, and the second time by D. There must be a residuum at least, identical in both cases, caused by A.

§ 337. In the method of concomitant variations we start with the same presuppositions as before, both as to isolation and as to the nature of causality. Here, however, we are supposed to have instances of an antecedent A, and of another element associated with it, whether contemporaneous or consequent is left somewhat obscure in Mill. We observe changes in A, and we observe changes in α associated with it, but no change in the other elements. We argue, then, that A causes α:

A_1BC	A_2BC
$\alpha_1\beta\gamma$	$\alpha_2\beta\gamma$

The ground of the argument is clearly as follows: We assume that if A conditioned some other element, a change in A would involve a change in that other element, and consequently those elements which do not change with A are not connected with it. Hence everything is eliminated except α. Or otherwise thus: the identical elements have identical effects, therefore they have not caused the changing effects; and thus the changing consequents are caused by the changing antecedents. Here again the argument is deductive; and here again we observe serious deficiencies of analysis in Mill. Mill ought to have considered what can be meant by the conception of change applied to an antecedent if that antecedent is an event. How are we to distinguish what we call changes or variations *of* something from events considered simply as new events? An antecedent, as Mill describes it, is an event. Now an event, properly speaking, cannot change, and on the other hand change cannot be reduced to the mere denomination of event. Clearly, then, change requires something not itself an event, something identical in time and in the change, in reference to which the events can be called changes, or more correctly phases of change. If we speak of a change in an event itself, this can only mean difference in the form which the universal takes: the universal, that is, corresponding to the event; so that strictly there would not be a change in an event but different events of the same kind but of different species. If again we assume with Mill that the same effect may have different causes, the fact that an element does not change with the change of A does not eliminate it as not having causal connexion with A. If the instances are

$$\begin{array}{cc} A_1BC & A_2BC \\ \alpha_1\beta\gamma & \alpha_2\beta\gamma \end{array}$$

it is only consistent with the plurality of causes to say that the element β which has not varied may have been caused the first time by A_1 and the second time by A_2. Further we may ask, in such instances as Mill gives, how we can distinguish which is cause and which is effect, if the variations are concomitant in the sense of being contemporaneously concomitant. As the pendulum moves towards the mountain the period of its vibration changes: therefore, he argues, its movement towards the

mountain causes the change in vibration. But on such an analysis as his it would equally follow that it was the change in the vibration that caused it to move nearer the mountain. The reason in fact why one of these elements is the condition, the other the conditioned, must lie in something extraneous to such data or premisses as Mill gives.

The same criticism is fatal to Bacon's *Tabula graduum* (which corresponds to concomitant variations), considered as a means of discovering the 'form'.

§ 338. It would usually be said of modern induction that it investigates the relation of cause and effect; i.e. not necessary connexion in general but a particular form of it. It is of course in the particular subject-matter to which these methods apply that we must look for their real significance and explanation. The first characteristic is that the necessity of their connexion refers usually to time. In pure mathematics, which also deals with necessary connexion, the connexion is as such not in time at all. But beside this there is another distinction which is really responsible for the difference in the methods employed in the two cases. In mathematics we *understand* the necessity of the connexion, or we know why the one element conditions the other. The expression 'understand the necessity' is the better, because the other ('know why') rather suggests something to mediate the connexion between the two elements and rather implies that the knowing why and how the two are connected is knowing something else which connects them, whereas in geometry we see the necessary connexion of the two elements directly and therefore see how one conditions the other. Whenever our knowledge of one element shows its necessary connexion with another, then we 'understand' the connexion fully. But there are other kinds of necessary connexion where our knowledge of one element does not show the necessary connexion of it with another, and where we have recourse to experiment and observation; and our knowledge about the connexion has to be (in an obvious sense) a mediated inference. Now the general form of this argumentation is as follows. We start with the assumption that there is a necessary connexion in a group of elements given in experience, and our problem is to find which is connected with which. It is just our want of intuition into

the nature of the connexion between the elements which causes our argument necessarily to take the threefold form corresponding to the methods of agreement, difference, and concomitant variation. The problem accurately stated seems to be this. Given a complex of elements ABC, and another $\alpha\beta\gamma$, and given that each element in the first is necessarily connected with some element in the second, which is connected with which? The elements are merely given in experience with some sort of juxtaposition in time; and in the experience we do not see the connexion. If we could, the single experience would be enough: we should not need the methods; we should be in the position of the mathematical sciences. Observe also that this general form of the problem is not restricted to the connexion of cause and effect as this is usually understood. That relation is not the essential thing, but solely this want of direct intuition into the connexion. This being so, what means have we of settling which elements are connected with which? We have seen (1) that one instance is not enough, because we do not observe the connexion; and (2) that the instances must vary in quality.

Now in the case of a given element A we have this exhaustive division:

(i) A present in different kinds of instances.
(ii) A absent ,, ,, ,,
(iii) A changing ,, ,, ,,

Consideration of these cases leads to the three following main distinctions of the experimental method.

In the *first* (presence) the method is such as to eliminate what cannot be conditioned by A, leaving what is conditioned as residue.

In the *second* (absence) the method is such as, by combining the presence and absence of A, to eliminate things present when it is absent.

In the *third* (concomitant variations) the method is such as to eliminate what does not change with A.

With nothing but experience to help us, this is an exhaustive principal division of the possible methods: the subordinate character of the so-called method of residues being obvious. We must not forget that the analysis given in each method is

an ideal, because we assume the elements of what is observed to be completely known, or at least with sufficient completeness for our purpose, an ideal which we know can never be realized. Further, if the ideal conditions were satisfied, one method would be enough, if applicable, and one instance of it; and it is only because we cannot be sure of our analysis of facts that we multiply the instances and may use all the methods on the same problem.

We see now what the characteristics of the subject-matter are which necessitate the use of such a method and that the general character of the method itself in all its forms is elimination.[1] As we start without knowing the nature of the necessary connexion, so we end without any further insight into it; for the methods as negative and eliminative can by themselves alone lead us only to the conclusion that A conditions a because A can condition nothing else. they do not tell us how A conditions a. Now this has a serious consequence; for we must admit that in the empirical sciences, so far as they depend on those eliminative methods, we never reach that understanding of the necessary connexion which we have got in mathematics: we have the fact but not the reason why.[2] It is true we seem at first to have advanced from the fact to a reason in so far as we have advanced from the particular observations of A and a together to the belief that A necessitates a, which seems the reason of the particular fact. Still the general principle to which we advance from these particular instances is itself but a general fact, the reason or ground of which we do not understand.

Lastly, our conclusions can never in the nature of the case be more than highly probable. Now of this kind of necessary connexion the causal connexion is a particular species. It is an example of a connexion as to the nature of which we have, in many cases at least, no direct intuition.

[1] Bacon's *per rejectiones*.

[2] Aristotle's ὅτι and διότι.

[It is just to note here that Wilson in a letter to a former pupil, Mr. D. S. MacColl (27.VI.XI), wrote: 'Mill was a great man, if rather confused as a logician and metaphysician, and the older I get the better I appreciate such men as he.']

II

DEFECTS OF THE MODERN THEORY OF INDUCTION. THE FORMAL CAUSE IN BACON

§ 339.[a] It seems, then, that the Eliminative method only disentangles out of a mass of mere conjunctions in time those which are necessary; finds, that is, which member of the complex belongs to which other in consequence of their own nature. But yet it does not advance beyond the *fact that* the first element does condition the other, it does not get within and enable us to see *how* precisely the nature of the first element conditions that of the other. We may fairly say we know *that* the thing is so, but we don't understand it fully.

This is characteristic of all the so-called Natural Sciences, but in one department, that of organic and animated life and matter, we observe a remarkable tendency to explain the parts of an organism by a teleological idea; so that there is something more than the mere knowledge that an element in the organism *is* so and so, through the reference of it to some function or *end* of the organism. This must be recognized as an attempt, whether successful or not, to get behind the mere mechanical fact. The position of it, however, in actual science is dubious and probably varies with the individual thinker. In the reform of modern science the teleological idea was regarded as a kind of superstition and certainly with some amount of truth. Science had been perverted by the assumption of what ought to be instead of patient inquiry into what *is*, and it was part of Bacon's service to protest against these teleological guesses. It is remarkable, however, that biological science, starting as it did with a mere mechanical theory of causation, has come to present the complex of a plant or animal as all ordered and arranged relatively to a plan or function which defines the organism, and

[a §§ 339–45. These sections were originally inserted at about this point. They were dropped out to save time, but the author was satisfied with his argument. Cf. §§ 520 and 527.]

it is important to note that while many still represent this as, so to speak, an accident of necessity working without a plan or end, yet the idea of the plan or design is not merely a sort of after-thought and without essential connexion with the frame of the science. On the contrary, it forms often the most valuable clue to direct the investigations of the biological student. We find in the writings of Darwin himself that the idea of what *ought* to be, on the assumption of the preservation of species and its reproduction, led him to important discoveries as to what is actually the fact. We may say generally that this notion is not a mere product of the imagination but an actual development of these supposedly mechanical sciences. But, as yet, the students of these sciences do not seem to have made up their minds as to the rank they shall assign to an idea which is so important to themselves and to the general student, an idea which appears at all events to give deeper insight into the meaning and working of Nature than the merely mechanical view.

§ 340. We have hitherto spoken of necessary connexion in general in connexion with the eliminative method; but, as a fact, the modern Inductive Logic seems to confine induction to the discovery of relations of causation, understanding thereby not the conditioning of one element in general by another but the conditioning of an order in time of events. This is not an accident apparently, for it would seem from Mill's general introduction to the theory of induction that it is the special merit of this modern method to introduce the notion of *cause* and that it is in this way that it has its great advantage over the ancient *enumeratio simplex*. So, too, Bacon's methods are professedly intended to discover what he calls the *formal cause*. However, the formal cause in Bacon is not the same kind of thing, as far as we can see, as the *cause* which is presupposed in the more modern accounts of induction. It must, however, be obvious that there is no need to restrict these inductive or eliminative methods to this one form of necessary connexion, and that it might be applied to the discovery of necessary connexions which, as such, have nothing to do with sequence in Time.

§ 341. We have seen that with Bacon and his followers the

conception of cause is the central conception of their so-called inductive method, and that this method, which we have chosen rather to call eliminative, consists first in assuming universal axioms about cause and effect and then applying them in an inferential or deductive process to the particular facts of experience.

The conception of cause in Bacon stands by itself and is peculiar to him in the Inductive philosophy.[1] The cause which his inductive methods have to discover is called by him the *Forma* or Formal Cause. His utterances about it are such as to have caused considerable difficulty in interpretation. He assumes the distinction of material, formal, efficient, and final causes from the Aristotelian philosophy without analysis or definition, though of course it is extremely important for him, as the inventor (as he thinks) of a new method, to give clear definitions of what he exactly means by these terms, and that more especially because he criticizes earlier systems severely for using abstract notions without any clear idea of their meaning. He can himself hardly be said to attempt a definition of any of them except the *forma*. And this absence of definition is the more deplorable because he makes it one of the chief merits of his Induction and of his scientific method in general that it inquires into the *formal* cause and not into the other three. Thus he repeats with approval the Aristotelian maxim, 'to know truly is to know by the causes',[2] and adds, also with approval,[3] the current distinction of the four causes. The final cause he rejects here and elsewhere, as tending to misdirect investigation. The efficient and material causes he thinks superficial and not able to give the real nature of the thing [4] and therefore not the true principles of action or production [5] The distinction of the formal from the material and efficient causes is further brought out in the next Aphorism. There he speaks of them as being variable [6] and, as it were, only the vehicle of the *form*. This vehicle in fact varies and, we should gather from his words, is

[1] Cf § 527. [2] Vere scire esse per causas scire, *N O* II 2.
[3] Etiam non male constituuntur causae quattuor, *N O* II. 2
[4] Scientiam veram, ib [5] Scientiam activam, ib.
[6] *fluxae*. 'At qui efficientem et materialem causam tantummodo novit (quae causae fluxae sunt et nihil aliud quam vehicula et causae formam deferentes in aliquibus)', *N O* II 3

only important as in some way containing the form or bringing it about, while the form itself is the invariable essence of the thing. The knowledge of it, moreover, should, according to this aphorism, not only give the true knowledge [1] of the thing but also power over nature,[2] the freest and most unlimited by the accidents of the material.

In all this it will be observed that Bacon gives no definitions of these efficient and material causes, but assumes them as sufficiently understood, although they belong to that part of the Aristotelian philosophy of which he is a pronounced enemy. If we follow the usage of the philosophy from which he borrowed the terms, we should make the *forma* simply the definition of the thing, the formal cause of a thing being in Aristotle its definition, its essence If we do this, a difficulty at once meets us. How will the knowledge of the essence of a thing (its definition) bring that power to produce it which is, according to Bacon, the very end and aim of the scientific method? That is, though we may get true knowledge and true contemplation from the form, how shall we reach the knowledge of principles of action and the free production? For production we expect indeed the efficient cause, and are entitled also to expect that, because he has not defined the efficient and material causes for his own purposes, but accepted the terms from ordinary usage.[3]

§ 342. The difficulty we here feel is only confirmed when we examine a particular instance of the application of Bacon's method. When he gets the form of heat,[4] it is interesting to notice whether it is really a precept for action, a formula by which the thing could be produced, or simply a definition of it, not containing the means of its production. In the last section but one of this aphorism, heat or the formal cause of heat is given merely in the form of a definition: 'Heat is a motion, expansive, restrained, and acting in its strife upon the smaller particles of bodies But the expansion is modified: while expanding all ways, still it inclines somewhat upwards. And the struggle in the particles is modified also; it is not entirely

[1] contemplatio vera 'Quare ex formarum inventione sequitur contemplatio vera et operatio libera,' *N O* ii 3
[2] operatio libera. [3] Cf note (3) to p 598, supra [4] *N. O.* ii. 20.

sluggish but hurried and with some violence.'[1] When further we come to the practical precept, we find what seems like trifling with the investigator: 'The direction is as follows: if in any natural body you can excite a dilating or expanding motion, and if you can repress and turn that motion upon itself in such a way that the dilatation does not proceed equably, but holds its own in part, in part is counteracted; without doubt you will generate heat'[2] Thus we are presented with a definition of heat and then told, what no one requires a philosopher to tell him, that, if you can produce the object so defined, you will produce the object of the definition.

Turning back to earlier parts of the *Novum Organum*, we find that the utterances of Bacon vary between those which describe the *form* as a mere definition by genus and difference and those which seem to relate it somehow or another to active production or causality. His phrases are so vague and general that we can hardly hope to discover what he means, except by looking at the examples he works out Some trouble has been spent in attempting to define the distinction between the *form* and the efficient cause, but the difficulty here is that he says so little about the efficient cause Clearly, then, the most important thing to do is to try to make out how the *form* differs from that of which it is the *form*, viz. the *simple nature*. According to Bacon the object of our investigation should be the simple quality, and we have to find out the *form* of the given quality or *nature*. This implies an important distinction between the two

§ 343. The material from which we must form our opinion is, (*a*) certain general statements[3] about the relation of the *form* to the *thing* (*simple nature*), and (*b*) the actual example of heat, in the *vindemiatio prima*.[4]

It will be at once obvious that, if the distinction of *form* and

[1] '*Calor est motus expansivus, cohibitus et nitens per partes minores* Modificatur autem expansio *ut expandendo in ambitum, nonnihil tamen inclinet versus superiora* Modificatur autem et nixus ille per partes, *ut non sit omnino segnis, sed incitatus et cum impetu nonnullo*' *N O* II 20, ad fin

[2] 'Nam designatio est talis *Si in aliquo corpore naturali poteris excitare motum ad se dilatandum aut expandendum; eumque motum ita reprimere et in se vertere ut dilatatio illa non procedat aequaliter sed partim obtineat, partim retrudatur, proculdubio generabis calidum*' ib II 20

[3] *N O*. II. 13 [4] ib II 20

nature is not clear, this will be a fatal difficulty in the practical application of the Baconian methods. These methods presuppose *forms* and *natures* somehow given together, and there is no note or mark to discriminate between them except the definitions of their being. They are not separated in the convenient way in which, for instance, antecedents and consequents are separated in Mill's methods. That is, we cannot distinguish them as occupying different moments of Time. They are given, in Bacon's methods, as entirely contemporaneous and, from some of the things he says about the *form*, it is necessary that they should be. The method therefore will utterly fail, unless we can tell in the group of facts before us which are *forms* and which *natures*.

According to the Thirteenth Aphorism, the form is absolutely identical with that of which it is the form: 'the form of the thing is the very thing itself.' The form only differs from the thing as the real existence from its appearance; as the inner from the outer; as that which is in reference to the universe from that which is in reference to man[1] These phrases are, however, very vague and may mean almost anything. We must turn therefore to the example in the Twentieth Aphorism. The opinion we should naturally form from it is clearly this: in the case of heat we may distinguish our own feeling from what in the atomistic theory is its external cause, material particles and their movement. And we find that Bacon's definition of heat is a definition of this external cause. We might therefore conclude that our feeling of heat was what he meant by the thing or simple nature. Thus his distinction of form and nature would be a recognition of the atomistic theory and his methods would really have for their object to discover the material or external causes for our sensations. One may say in advance that this is probably the view to which Bacon, in a confused way, is committed; but we shall endeavour to show that he was not clearly conscious of it: that he could not even describe it and that it contradicts not only his words but his general method of representing nature.

To begin with, it is clear that when Bacon represents a *body*

[1] 'in ordine ad universum', the πρῶτον φύσει, and 'in ordine ad hominem', the πρῶτον πρὸς ἡμᾶς of Aristotle

or individual thing as a complex or combination of simple natures (e. g. gold is a complex of such simple natures as yellow, heavy, malleable, &c.), he does not in the least imply that these simple natures are only in our consciousness He not only does not say this, though it would have been of the last importance to him to say it, if his idea of *form* and of the scope of physical investigation were such as we have described, but his language in general implies the contrary. He clearly thinks that the simple natures are in the real 'body' or nature, not merely something in us.

A further difficulty is this: the sensation of heat is the effect of the aforesaid external conditions and is different from them; so much so that, in this atomistic theory, the one is supposed to exist merely in the human consciousness and the other to be entirely independent of the human consciousness, to exist indeed apart from all consciousness.

§ 344. Now we have seen first that Bacon makes the *form* and the *nature* in some way identical and uses very strong language to express this,[1] and secondly that he expressly denies that the form of heat is the efficient cause. He says: 'not that heat generates motion or that motion generates heat (though both are true in certain cases), but that Heat itself, its essence and quiddity, is Motion and nothing else'; [2] and a little further on: 'nor again must the communication of Heat or its transitive nature, by means of which a body becomes hot when a hot body is applied to it, be confounded with the Form of Heat. For heat is one thing, heating another.' [3] This agrees with his reiterated statement that the *form* is the precise definition of the *nature* and also with the use of form in the philosophy from which the term is borrowed. This difficulty cannot be removed by assuming that he meant that the *nature* should be defined by its cause, that cause being different from it,[4] because of his express assertion of the identity of the two.

It would seem as though the only solution possible is to assume that Bacon, like ordinary people, did not recognize that certain of the attributes, as they are called, of things exist only

[1] 'ipsissima res' *N O* II 13 [2] *N O* II 20 [3] 1b

[4] As Aristotle, for instance, says that the ὅτι, or demonstrable property of a thing, should be defined by a διότι, which is in a sense different from it.

in our sensitive consciousness: that he thought, in the ordinary way, of the heat felt, as in the hot thing; just as we speak of a thing as coloured, as if we held the colour to be in the thing as much as the extension is. But even this is excluded, for he recognizes expressly that what he calls 'hot to the sense' is only an effect in us: 'Hot to the sense is a relative notion and has reference to man not to the universe; and is correctly defined as merely the effect of heat on the animal spirit. Moreover, in itself it is a variable thing, for the same body (as the sense is predisposed) induces a perception as well of cold as of hot.'[1] It seems then, that, if the form of heat is (as we know it is with him) this external cause of our sensation of heat, the sensation of heat cannot be the *nature*

What, then, can the nature be? There seems nothing left as an answer to this question. The confusion, as far as words and formulae go, is quite incurable, and it is made somewhat worse by another statement about the 'sensibly' hot. 'Now hot to the sense is the same thing: only ⟨it must be considered⟩ with reference, so far as it is fitted to sense.'[2] These phrases, had we not the rest of Aphorism 20, would certainly seem to suit the distinction, given in Aphorism 13, between the *form* and the *nature*, for corresponding to 'the same thing' we have 'the very thing itself', and to 'with reference', we have 'in reference to man' as distinguished from 'in reference to the universe' and the *apparent* as distinguished from the *existent*; and, further, in Aphorism 20, while the 'hot to the sense' is so distinguished from Heat proper, as the mere effect of it, it has the designation 'in reference to man, not to the universe'.

§ 345 In face of these contradictions, which are so explicit, it is futile to attempt a reconciliation; to try to present a definite conception of *form* as existing in his mind Indeed it is idle to suppose that such a confusion of ideas could be of any value either in philosophy or in science, to which Bacon professes to supply a new and certain method.

The only thing that remains is to attempt to account for these confused statements; we must try, if we think the problem

[1] *N O* II 20, § 5.

[2] 'Calidum autem ad sensum res eadem est sed cum analogia, qualis competit sensui' *N O* II 20 (concluding words)

worth solving at all, to trace the various influences that were crossing one another in his mind. An indication perhaps of the true state of things is to be found in his curious statement about the 'hot to the sense' already quoted.[1] He distinguishes it apparently from Heat proper, as merely a sort of result or effect of it in our sensation; he seems further to distinguish it from what is *properly* heat, by the remark that in itself it is a variable thing, and indeed so far distinguished from true heat that the same body may indifferently give the sensation of cold or of heat. ('Moreover, in itself it is a variable thing,' &c.) He himself refers to the forty-first instance given in his Third Table,[2] in the wording of which we find something curious and suggestive This 'hot to the sense' is called 'hot as far as regards the sense and touch of man', and therefore 'a variable and relative thing'. The instance of its variability given is that 'tepid water, if the hand be cold, feels hot, but cold if the hand be hot'. Now this 'hot to the sense' occurs in the last instance of the Table as though it had not occurred already, as though it were in fact something separate from the quality of heat observed in bodies and described in the forty preceding instances. This is extraordinary because there could be no other test of the existence of heat in the cases he has mentioned except the 'human touch' and 'sense'.

This seems enough of itself to prove a certain confusion about the relation of the quality of heat or simple nature (as first observed in bodies before we have discovered what he calls the *form*) to our sensation. We may therefore provisionally account for the difficulties we have found by supposing some such confusion as the following No doubt we ordinarily think of the heat we feel as though it were in the object. Now Bacon has no doubt got beyond that stage, especially through his studies of the Atomists; he recognizes the sensation of heat as in ourselves, an effect of the object, not in the object. But it is possible, even when this stage is reached, to fall back into the most natural form of our consciousness and to represent, or rather think of, what is really in our sensation as if it were in

[1] See note 2, p 603

[2] 'Calidum, quatenus ad sensum et tactum humanum, res varia est et respectiva. adeo ut aqua tepida, si manus frigore occupetur, sentiatur esse calida; si manus incaluerit, frigida' *N O* II 13, *Inst.* 41.

the thing. Thus we go so far as to think of sound as if it were in the sounding instrument. And this is not uncommon in our ordinary perception when we aren't reflecting on the distinction. The confusion is removed, of course, by reflection; but the truth about Bacon seems to be that his confusion is retained in his philosophy. For him the *simple nature* seems to be our sensation, the 'hot to the sense' confusedly or inaccurately referred to the object as its attribute. And thus this *nature* is sometimes half-identified with our sensation and sometimes distinguished from it.

Thus, for example, in those first forty Instances, he simply thinks of the *nature* as in the thing, without reflecting on its distinction from our sensation, just as any ordinary person will do. In the forty-first instance he has become conscious of the difference between our sensation and the objective fact. Again, when distinguishing the 'hot to the sense' from the *nature*,[1] he recognizes its difference from the objective fact and presents it as the effect of heat [2] and not as heat itself. We see why, when he thinks of the nature as in the thing, he makes the form its definition. On the other hand, when he is specially concerned to describe the *nature* itself, it is not surprising that he should slip into a phrase which belongs to our sensation, the 'hot to the sense', because the nature is nothing but that 'hot to the sense' confusedly conceived as if it were in the object.

[1] *N. O.* II 20

[2] *Calor* here, from the context, is the *simplex natura* We have three terms, 'form of heat', 'heat' (i q *simplex natura*), and 'hot to the sense' (an effect of the simple nature)

III

THE AXIOMS OF MODERN INDUCTION

§ 346 A COMMON characteristic of all the current definitions of Induction seems to be the implication that the conclusion must be wider than the premisses. It is easy to see, even from the admission of such an inductionist as Mill,[1] that this is not true of the so-called Experimental Methods. These imply certain major premisses[a] (clauses, so to speak, of the law of the uniformity of nature) far wider than the particular conclusions drawn from them by the help of the particular experiences used in the inferences. It may seem at first sight that an inductive element is left in the *enumeratio simplex* argument from which these major premisses are supposed to be derived. And if we take the point of view of the inductionists themselves, they would have on their own showing to admit that the only inductive part of the new methods is precisely that supposed antiquated form of Induction which these new methods are to supplant. But we must take away even this last claim. It is an illusion to describe the *enumeratio simplex* inference as a process in which the conclusion is an advance in generality upon the premisses. We have seen that such a statement is due to an analysis which is not ultimate, and that ultimate analysis involves a universal axiom which is parallel to the universal axioms used for the experimental methods; the latter axioms being only cases of the former. But, apart from this, we can show that the Inductive logic is not only involved in the contradiction above described, but also that it is involved in more serious contradiction by the manner in which the *enumeratio simplex* method is made to supplement the new methods. On this point our objections will not be merely formal in character. We shall be able to show that the new methods would have an uncertainty which the inductionists have not suspected, and that actually they could not, on their own showing,

[1] *System of Logic*, III. xxvii

[a] Note a p 585]

be so trustworthy as the old method which they profess to replace.

§ 347. Such inductionists as Mill admit that their induction depends entirely upon the Axiom of Universal Causation. The axiom being found by *enumeratio simplex*, we ask how a process described [1] as 'loose and uncertain' can prove the validity of an axiom which is the foundation of processes supposed to be methodical and satisfactory It is really clear that it could not give a satisfactory proof and could not account for our conviction that the necessary connexion of events is universal and knows no exception. And it is a sign of the truth of this that Mill is unable to maintain this part of his theory without contradiction. If we ask why a process which he condemns as fallible should be admitted to prove this most important axiom, his first reply is that the case is exceptional. This *enumeratio simplex* can be relied upon, because of the immense quantity of the premisses We should naturally suppose therefore that Mill believed both that the principle of causation was universally true and that this exceptional form of the ancient induction was enough to establish it. Indeed he says [2] that 'the law of causation, for instance, and the principles of number and of geometry, are duly and satisfactorily proved by that method alone, nor are they susceptible of any other proof'. Mill must mean by 'duly and satisfactorily proved' that the conclusion, the law of universal causation, is absolutely true as a matter of fact and that the proof of it is as valid as a proof in geometry. This agrees so far as it relates to the mathematical axioms with what he had said earlier: [3] 'These are rigorously true, even beyond any possible experiences of ours.' [a] And so of the axiom of causation (law of universal causation) he says, 'we may . . .

[1] Mill, l c , III xxi, § 2 [2] ib , § 3 [3] ib , II vi [see note a].

[a] This quotation, the reference for which is given in the author's typescript and in the various lecture notes as *Mill, System of Logic*, II vi, I have not been able to verify. It was sometimes cited as 'even beyond the limits of our possible experience'. The difficulty is that Mill's doctrine in this chapter is that axioms are derived from experience, and he is expressly speaking even in regard to *number* of 'all objects known to our experience' In III. v, § 1, it is true, he says, 'In the laws of number, then, and in those of space, we recognize . . the rigorous universality of which we are in quest'; but this is consistent with his arguments elsewhere as to their origin in experience]

regard the certainty of that great induction as not merely comparative but, for all practical purposes, complete'.[1] Yet at the end of the chapter, in the course of which the uncertainty of the method has been gradually growing upon him, he denies that we may rely upon the law beyond the possible range of our experience, saying 'In distant parts of the stellar regions, where the phenomena may be entirely unlike those with which we are acquainted, it would be folly to affirm confidently that this general law prevails'.[2] Observe the contrast to his earlier language in the same chapter. 'It is therefore an empirical law coextensive with all human experience; at which point the distinction between empirical laws and laws of nature vanishes.'[3]

Clearly, in the later passage, he has become aware of the untruth of what he said earlier, and realized that all human experience is not therefore coextensive with all nature. Again in the case of mathematics, the axioms of which he says are duly and satisfactorily proved by the same proof as the principle of causation, he has affirmed[4] that we can rely upon them just in those distant parts of the stellar regions of which we can have no experience.[a]

Finally, in a note at the end of this same chapter, he rejects his own doctrine in criticizing Taine. Against Taine he says that he can't see how in 'interpreting in general language the testimony of experience, the limitations of the testimony itself can be cast off'; though this is exactly what he himself did in the earlier part of the chapter. The contradiction is complete. The truth seems to be that when Mill has his attention fixed on the objective fact of causation he can't help feeling certain about it, and therefore is obliged to think that the only method which his philosophic system allows him for establishing it must be satisfactory. On the other hand, when his attention is fixed upon the inferential process, he sees that it is unsatisfactory

[1] Mill, l. c., III. xxi, § 4, para. 2.
[2] Ib., last para. of ch. xxi.
[3] Ib., § 3, para. 2, *sub fin.*
[4] 'That a straight line is the shortest distance between two points, we do not doubt to be true even in the region of the fixed stars.' l. c., III. iii, § 3, para. 3.

[a] Notice, however, Mill's own *caveat* in the note to this section.

and so now, to save his philosophy, he has to throw doubt on the conclusion of the process.

§ 348. To return to the exact form of the proof of the law of causation by *enumeratio simplex* inference. First, what are the premisses? In the nature of the case they must be of the form 'This A causes this α', 'This B causes this β', &c., with the generalizations 'all events have effects'; and similarly, 'all events have causes'. Now how can we get premisses of this form? They can't be got by the four methods: for these methods presuppose the axiom of all induction. They can't be given by direct observation: for, as Hume contended and as empiricists must and do admit, we can't observe causal connexion. Indeed if we could *observe* A causing α we should not need the methods: for the meaning of these methods is that they are to remedy this defect of observation, and the analysis of the experimental methods shows that the inductionists assume that we can observe antecedents and consequents alone.

Where, then, do the premisses come from? In this philosophy there can be only one answer, from *enumeratio simplex*. This is implicitly admitted [1] by Mill, though he seems straightway to forget the admission and its serious consequences. We notice first, then, that this *enumeratio simplex*, so far from being founded on the facts observed, is founded upon premisses; premisses which are themselves not observed facts, but each of them an inference from another *enumeratio simplex*. *Enumeratio simplex* proof, then, of the law of the uniformity of Nature, instead of being a peculiarly infallible form of *enumeratio simplex*, is a specially fallible one. For, besides the uncertainty which belongs to the final proof by *enumeratio simplex*, there is also the uncertainty which attaches to each of its numerous applications to prove the premisses. There is not one inference of this nature concerned, but a great number; and, unlike the ordinary *enumeratio simplex* proof, there must be a doubt attaching to each premiss, since it is got by an admittedly uncertain method. But we see the fallibility to be still greater when we look at the form of the various applications of this mode of proof to prove the premisses. How are we to prove that A_1 causes α_1? Not from the observation of A causing α at any time, but only from

[1] l c., III. xxi, § 2.

the observation of A *followed* by α. Thus the conclusion 'A causes α' involves two processes, each inconclusive: first, the inconclusive generalization from 'A preceded α on the occasions observed' to 'A is always followed by α'; and secondly, by the fallacious introduction of a new conception not contained in the premisses, i e the conception of causality. For whereas the premisses must be 'A *is followed* by α', &c., the conclusion is 'A *causes* α'. The fallibility of the ordinary proof by *enumeratio simplex*, instead of being removed or mitigated, is, so to speak, multiplied by itself.

§ 349. We can now tabulate the whole process:

(i) There is an experience of the type 'A preceded α, as often as observed'.

(ii) There is an *enumeratio simplex* inference from many such instances to the conclusion 'A causes α'; and similarly 'B causes β', &c, &c

(iii) The results of this unscientific induction give the premisses for another induction, also an *enumeratio simplex* inference, which has for its conclusion the axiom of the uniformity of nature or the law of universal causation.

(iv) After this come the processes of the four experimental methods, based upon the results of the second induction; themselves, though deductive in form, called inductions; and these constitute the supposed scientific Induction. Now these methods yield conclusions of exactly the same form as those arrived at in the second stage by the unscientific Induction. Modern Induction, then, claims that the conclusions in this fourth stage are more trustworthy than those in the second stage. Thus 'C causes γ' in stage (iv) is more trustworthy than 'A causes α' in stage (ii)

Now obviously the conclusion cannot be more certain than its premisses; and if the method is uncertain, the conclusion must be less certain than its premisses But the inference in the experimental methods is not certain, because we cannot be certain of the facts of observation. Thus the conclusions of the new induction are less certain than the axiom of Uniformity. This axiom again cannot be more certain than its premisses and, the method of its derivation being admittedly not rigorous, must

be less certain than its premisses; but these premisses are just the results of the unscientific induction. Thus *a fortiori* the results of the scientific induction are less certain than these last premisses; are less certain, that is, than the unscientific induction. Thus the conclusions of the new induction are two degrees less reliable than those of the old. It follows that the only rational procedure would be to abandon the methods, go straight to experience, and use the unscientific induction.

§ 350. The question of the plurality of causes is part of a larger question, the relation of the condition, whether temporal or not, to what it conditions. We tend to assume a certain priority of condition over consequent and to look on the former as active and the latter as passive. The condition, then, seems to have a complete hold over what it conditions, so that the result must follow, whereas the result is not thought of as having the same hold over the condition which precedes. A fact is not conceived as compelling the existence of its own condition in a particular case, though apparently it is conceived as necessitating some condition or other, an admission of the last importance. The view is erroneous and probably due mainly to this idea of superior activity in the condition which specially suggests itself when the condition is temporal; for, as such, it has apparently a prerogative and superiority of existence over an as yet non-existent result. Thus, when the relation is of sequence, it seems a kind of paradox to represent the not yet existing result as the condition of what is existing There is a further corroboration of the erroneous view in the doctrine of the plurality of causes, which means that the same effect may at different times be produced by different causes. Moreover, what is very important, the view thus suggested by experience gets support from a general *a priori* opinion about the relation of the condition to what it conditions; an opinion based on the fact that the hypothetical proposition, as such, cannot be converted. From 'if A, then B' we cannot infer 'if B, then A', thus A seems to control the existence of B but the converse does not seem to be true. Even in this abstract form the conditioning clause seems to have some superior force.

§ 351. We shall try to show first that the relation of the condition to the conditioned is not one-sided; and that the

conditioned has the same hold over the condition as the condition is usually thought to have over it; and, secondly, that the illusion of a plurality of causes is due to imperfect observation.

In such a question we must begin with something admitted to be necessary. We shall therefore start from the fact that, while it is thought that an effect may have different causes, the ordinary observer is sure that the same cause cannot have different effects, or that the same condition, as essence say, will necessitate the same result, say as property. In this lies the key to our problem.

Suppose A conditions α: A does that in virtue of its own nature,[1] and the reason why B conditions β lies just in the difference of the nature of B from the nature of A. It is self-evident that the definite nature of A must, as definite, condition not something indeterminate, but a perfectly definite nature such as α. We may get the same result negatively. If A conditions now α, now β, then there must be a reason for the difference and that reason would constitute a difference between the two cases of A. Thus these two cases were wrongly supposed to be identical. Now this is doubtless the ground for the certainty we assign to the proposition that the same cause must have the same effect. But exactly the same argument must necessitate that a definite R determines for itself a definite C.[a] It is admitted that α must have some condition, i. e. its nature necessitates some condition, but necessity cannot possibly be indeterminate and thus the determinate nature of α must necessitate a determinate condition. α as identical with itself and different from β determines from its own nature a connexion with A, as distinguished from any other element such as B: and so with β. Again, negatively, if this were not so, we should have to suppose that the nature of α at least determined that it must have one or other of a certain kind of conditions; and, if we assume that it does not determine a particular one, there will be nothing at all to decide which shall condition it in a particular instance. Further, if there were anything which did so decide, that would constitute a difference between the two cases of α. Or to put it otherwise; if the given conditions were all equally indeterminate, α could not come about at all. We see in fact that,

[1] *καθ' αὑτό.*

[a] R = result, C = cause.]

just as the definiteness of the condition necessarily determines a definite nature in the conditioned, so the definite nature of the conditioned must determine a definite nature in the conditioning. If the argument connecting the definite thing conditioned with the definite condition is correct, so must the converse be also, for the reasoning is exactly the same. Consequently, when the contrary seems true, there must be some difference in the conditioned or the difference in the condition is only apparent. The instance of a conditioned by x cannot be the same as the instance of a conditioned by y; for otherwise it would be true that the difference between x and y would have nothing to do with the identical part of a, and that which really had to do with it must be identical in the apparently different conditions x and y. Thus, either the conditioned is not really the same, or the condition was not different in the two cases, so far as it was a condition in each. The argument is self-evident and the difficulty would never have occurred but for certain appearances which we must now consider.

§ 352.[a] Consider first the 'condition' in an hypothetical statement. We have already seen that in geometrical science a property of a figure is derived from its full essence or definition and that, if the proof has its completest form, the property can always be shown to condition the essence.[1] In other words, the hypothetical proposition in these cases is convertible. This is important because in geometry we see into the necessity of the connexion of the conditioning and the conditioned. Thus, in a case where we *understand* the connexion, there is no plurality but the relation of condition and conditioned is reciprocal. It is significant that the case which causes our main difficulty relates to that species of necessity where we *do not* understand the nature of the connexion but only establish that there is a connexion. So too for the 'condition' of an hypothetical statement; we can easily show that this is compatible with the doctrine we have laid down and indeed follows from it, and that the possibility of conversion depends merely on sufficient knowledge. Suppose A to be the true condition of C: then C conditions A, as in the case of a reciprocal relation in geometry. Now

[1] § 258.

[a 'In re-writing this section, the sense of cause as not merely event (as in Hume and Mill) but as condition must be discussed.' *MS. note.*]

suppose that we have A and B connected together in experience and let AB = x. Then it would be true that if x exists, C is existing; since x contains A, which conditions C; but it will not be true that, if C exists, x exists. The reason is obvious. C only necessitates A and not B, which is also in x. This is why the statement of the connexion is not convertible, i. e. because the protasis of the conditional statement is not the true and accurate condition of the apodosis, but something more This is just what happens in a mathematical proof when a proposition is not convertible, more than the true condition has been admitted into the protasis. Thus, when the conditional proposition expresses in its protasis the true condition of the apodosis and no more, the proposition is always convertible.[1]

§ 353. It is clear that the foregoing investigation explains many cases of the apparent plurality of causes: that so far as these various causes are causes of something identical, so far they themselves are identical. They differ only in what is irrelevant to the effect, and it is the business of science to discover the identity by eliminating the irrelevant, just as mathematics discovers the true condition by eliminating the irrelevant parts of the protasis

There are, however, other ways in which the same false appearance is produced. Suppose the condition of x (state, process or event) is *abcd*, where every element is necessary to x. Suppose only a portion of *abcd* is realized, say *bcd*: then x will not be there. Suppose now a is added · then x will be there and it will seem as though a was the cause of x. Similarly, if *acd* is realized, x appears when b is added, and thus x will *seem* to have a or b or c or d as its causes. In fact the condition was really always the same, i. e. *abcd*.

There is another case which is more important, because more illusory. Suppose x is destroyed, not originated · now suppose the condition of x is *abcd*, each element being necessary; then, if any one, e. g a, is removed, x disappears and similarly with *bcd*; thus the disappearance of x may *seem* to have a plurality of causes. Now, obviously, while the cause of the existence of x is always one and the same, the cause of its disappearance

[1] In this section the term hypothetical statement has been employed in accordance with ordinary usage But the more correct term here is *conditional* statement Cf § 293

is one and the same; it is always the destruction of *abcd* as a combination. The completion of *abcd* the cause of *x*, by adding *a* to *bcd* or *b* to *acd*, &c., is exactly parallel to the process of destroying the cause by taking away *a* or *b* or *c* or *d*. It may seem, however, that, whereas in the first case the addition of *a* is not the cause, because *a* by itself is not sufficient and *x* only results because of the existence of the complete complex of conditions *abcd*, in the second case—the negative case—the destruction of *x*, the removal is rightly accounted the cause, inasmuch as the removal of *a* is sufficient to destroy *x*, quite irrespectively of the absence or presence of the other conditions *bcd* But this is untrue. If the other conditions, *bcd*, were not all present, *x* could not be in existence and so the destruction of *x* would not take place as a result of the removal or destruction of *a*. Thus the removal of *a* can only result in the destruction of *x*, because of *a*'s relation to the remaining conditions *bcd*, and therefore is not a sufficient cause in itself, irrespective of their existence They must be in existence if the removal of *a* is to result in the removal of *x*. The destruction of *x*, when *a* is removed, only takes place on the dissolution of the relation between *a* and *bcd*, just as the production of *x*, when *a* is added, only takes place through the establishment of the relation between *a* and *bcd*. Thus, as the true cause of the production of *x* is always the organic complex *abcd* and not any element of the complex, so the destruction of *x* has always for its cause the destruction of the organic complex *abcd*, as an organic complex. This is the solution of the stock instance of the different causes of death. Life depends on the co-operation of certain elements organically connected, each necessary, none sufficient. The complex being necessary, the destruction of the organic connexion causes death and is necessary as the condition of death. The removal of any one of the elements of the complex from its connexion with the rest is enough, obviously, to destroy the organization of the complex. The account which assigns a plurality of causes to death, then, is a confusion because there is no consciousness of the distinction between the negative and positive cases. For the kind of mistaken consideration, due to imperfect analysis, which gives rise to the illusion of a plurality of causes in the negative instance, the destruction of something, cannot affect the positive instance, the production of something.

IV[a]

FAILURE OF EMPIRICISM TO EXPLAIN THE LAWS OF THOUGHT

§ 354. THE weakness of the inductive theory in regard to such notions as cause, substance, &c., gave rise to the doctrine of the Association of ideas, which dealt more especially with the question of causation. Hume's mistake, in this doctrine, was strange, for, according to his own theory, ideas and impressions are equally facts of consciousness. They differ only in vividness. If, then, the notion of causation does not appear in the vivid series, obviously it cannot appear in the weaker *replica* of that series, the ideas Moreover, Hume, contrary to his own criticism of Locke and his own theory, was here allowing to the mind a power of originating an idea, which did not arise from any impression. The self-contradictoriness of the association doctrine, in its variety of forms, has been exposed sufficiently by many writers.

The empirical theory has made a last attempt to save itself by taking into account the modern doctrines of evolutionist biology, and in doing so most naively assumes in the biological doctrine from which it starts the very principle which it proposes to establish. Further than this, it defeats its own purpose by ending in a complete scepticism. That this is so will appear from an examination of the evolutionist theory of axioms proposed by Herbert Spencer.

§ 355. The inquiry into the origin of what are called axiomatic truths, or first principles, is, as is well known, one of those fundamental questions which divide philosophers into opposing schools.

I propose to consider the contribution made to it by the Evolutionist school of philosophy.

[a This is a composite chapter put together for completeness (see Chronological notes of composition) § 366 is one of the last things the author wrote, and was written with deliberate reference to § 365, which is very early.]

In the discussion which will follow, I use the words 'evolution' and 'evolutionist' in reference to evolutionist philosophy, and not to evolutionist science. Why I am anxious to make this distinction will appear hereafter.[1]

The Evolutionist claims to have solved the vexed problem, and to have reconciled all previous schools of philosophy. The principal representative of this philosophy says: 'Already I have pointed out that the hypothesis of Evolution "supplies a reconciliation between the experience-hypothesis as commonly interpreted and the hypothesis which the transcendentalists oppose to it"; [2] and here we see how complete the reconciliation is.' [3] This claim is a high one and made, as we see, with some conviction. The theory, though distinctly empirical, recognizes that induction is insufficient for axioms. It is well known, but its essential features must be recalled.

§ 356. The sufficient and the only guarantee, it is said, of a universal proposition is that we cannot conceive its contradictory. This is the ground of our belief in axioms; and thus they are *a priori* for the individual, being due to the constitution of his mind and not to his experience. But this constitution of his mind has been itself produced by experience, not his own, but that of the race.

The effect of experience is to modify the nervous structures: the modifications have been accumulated by inheritance from generation to generation, so that the mind is at last, in the case of axioms, completely adapted to the object. Thus, in the well-known formula, axiomatic beliefs are said to be '*a priori* for the individual, but *a posteriori* for that entire series of individuals of which he forms the last term'.[4]

I will read a passage [5] which describes how all this happens: 'Little more need be said to make it clear how certain primary space-relations are presented to consciousness under the form of necessary relations. If a segment of a circle be looked at, the image of it cast on the retina is necessarily such that the arc covers a greater number of retinal elements than the chord; and since each of these retinal elements yields its separate

[1] § 364. [2] Spencer, *The Principles of Psychology*[2], vol i, § 208.
[3] ib., vol ii, § 332. [4] Spencer, ib., vol. ii, p. 195 (§ 332 fin.).
[5] Spencer, ib., § 332.

impression to consciousness, the series of impressions produced by the arc is felt as larger than the series produced by the chord. This continues to hold however much the arc is flattened: so long as it has any perceptible curvature at all, it is felt to be longer than the chord uniting its extremes. Parallel experiences are derived from the ocular muscles. Carrying the eye along the line of the curve yields to consciousness a greater quantity of sensation than carrying the eye along the chord does. As the curve is flattened this difference diminishes; but some of it continues as long as the curve continues appreciable. Thus the truth that a straight line is the shortest line between two points, lies latent in the structures of the eyes and the nervous centres which receive and co-ordinate visual impressions. We cannot think otherwise because, during that adjustment between the organism and the environment which evolution has established, the inner relations have been so moulded upon the outer relations that they cannot by any effort be made not to fit them. Just in the same way that an infant's hand, constructed so as to grasp by bending the fingers inward, implies ancestral hands which have thus grasped, and implies objects in the environment to be thus grasped by this infantine hand when it is developed, so the various structures fitting the infant for apprehensions of space-relations, imply such apprehensions in the past by its ancestors and in the future by itself. And just as it has become impossible for the hand to grasp by bending the fingers outwards instead of inwards; so has it become impossible for those nervous actions by which we apprehend primary space-relations to be reversed so as to enable us to think of these relations otherwise than we do.'

This theory, it may be remarked, does not necessarily imply that the mind is identical with physical organs, but it does imply that the mind not only perceives by them, but can only think and imagine by their means, and is therefore restricted by the functions of which they are capable. Divested of technicalities unnecessary for our present purpose, it comes to this. In its early experiences the mind was not obliged to think the arc greater than its chord. But every arc presented to the experience of every generation has been greater than its chord: and the constant perception of this relation gradually modified

the perceiving and thinking organs, so that it became more and more difficult to think arc and chord in any other relation. The difficulty transmitted by inheritance became stronger by accumulation in successive generations, till it became altogether impossible to think the contradictory of the axiom. This is how the individual's belief in the axiom is formed, and the process secures that the subjective belief shall correspond to an objective fact.

This has an appearance of clearness and simplicity which recommends it to many.

§ 357. Before asking if it is true, we may ask whether we should like it to be true. Such a question may seem unscientific, but in philosophy at least we should clear our reasonings a good deal if we confessed to ourselves what we wanted to believe and what we hoped was untrue.

Now at first sight the theory may well have attractions. Many will feel that induction does not account for the conviction they have had, as far back as they can remember, in the same strength about axioms · and yet they will feel on the other hand the force of Mill's objection—'I must protest', he says, 'against adducing, as evidence of the truth of a fact in external nature, the disposition, however strong or however general, of the human mind to believe it.'

The evolutionist view seems to bring a welcome help and to show how the desired correspondence between subjective belief and objective fact has been brought about by the operation of known causes.

No doubt, then, many may think it would at least be a good thing if it were true. The evolutionist philosopher probably believes that it is a beneficent result of the great law of Evolution; and there may be scientific evolutionists who feel at least a prejudice in its favour. But let us look a little nearer to see whether we should have cause to congratulate ourselves if the evolutionist philosopher were right.

The theory that the mind has *become* modified by its environment, and that its functions have *become* fixed, implies that at first it was not modified and its functions not thus fixed. If 'Just as it has become impossible for the hand to grasp by bending the fingers outwards instead of inwards; so has it

become impossible for those nervous actions by which we apprehend primary space-relations to be reversed so as to enable us to think of these relations otherwise than we do', then it follows that once it was possible that these nervous actions should be reversed. And what does this mean? It means that the mind and its organs were so constituted that they were capable of thinking the contradictory of axioms, for instance, of conceiving a chord equal to or greater than its arc, or two straight lines which enclosed a space, only that this capacity was never developed.

To some this will appear a sufficient *reductio ad absurdum*, but I do not propose to take that line of argument. Let us rather imagine that such a condition of the mind is possible. It will follow that our minds have been deprived of half their powers beyond recovery. The geometrician, for instance, is cut off from a field of thought as large as the present science—a field in which among many other things he would have developed the properties of those straight lines which are not the shortest distance between any two points upon themselves.

In fact, in respect of all that is axiomatic, that is, of all that appears self-evident, the whole human race is in the position of that part of it which has lost or never had the use of a particular sense—the deaf, the blind, or the colour-blind, or those who have no ear for music. We are like the Proteus which in dark caves has gradually lost the eyes which would be useless to it: except that we are worse off, for though an object for our lost faculties may never come into our experience, we should obviously have plenty of employment for them. The same process has woven a falsehood into our nervous tissues and so into our minds: for the confidence we have in axioms is made to come from our conviction that their contradictories are not even thinkable, and yet it is shown at the same time that this is an utter mistake, inasmuch as thought in itself, though not as developed in us, is capable of thinking these contradictories. The individual, then, at best has got a true belief at the expense of entertaining a false belief, and at the expense of the loss of half his mental powers.

Is this a result to be contemplated with satisfaction?

May one not look for sympathy if one confesses to the hope

that it is not true? Surely it would be one of those things for which we could not honestly say we were thankful. At best it would be a matter for resignation and not for gratitude. We may bow to it as an inevitable decree of nature; we deceive ourselves if we say we can see it is good and desirable. The pessimist would find another instance of Nature's unkindness. Even where she seems to confer a benefit, she has done us a deep injury. She has mutilated our minds with a refinement of cruelty. For instead of mercifully concealing a loss which could only be deplored and never repaired, she has evolved the philosophy which has betrayed the secret.

And is there really any compensation?

If we have gained a belief which happens to be true, can we be said to understand it if we ground it on a falsehood. Can we be said to 'know' at all in the proper sense of the word? Can a belief be even intelligent which is simply caused by the want of a power to think otherwise, a power which might have been developed and is lost to us by a process as unconscious as that which deprives us of taste or smell when we have a cold? In ordinary life, when we are not philosophizing, we have a great contempt for beliefs which are merely the result of imperfect mental development; we expect them among savage tribes, the prejudiced, and the uneducated.

§ 358. An attempt has been made to remove a bias in favour of the evolution theory. If the attempt were successful, it must be admitted that it would create a bias against the theory: and there is all the more reason to try to take a purely scientific attitude and ask whether we have to believe whether we like it or not.

In the first place attention must be expressly called to the fact that this is a theory of knowledge. It does not explain merely how we come by a belief, but how that belief is true; that is, how we have knowledge.

I will read a passage to make this clear: [1] 'Hence the inconceivableness of its negation is that which shows a cognition to possess the highest rank—is the criterion by which its unsurpassable validity is known. If the negation of a cognition is conceivable, the discovery of this amounts to the discovery that

[1] Spencer, op cit., § 426

we may or may not accept it. If its negation is inconceivable, the discovery of this is the discovery that we are obliged to accept it. And a cognition, which we are thus obliged to accept, is one which we class as having the highest possible certainty. To assert the inconceivableness of its negation is at the same time to assert the psychological necessity we are under of thinking it, and to give our logical justification for holding it to be unquestionable.'

Other passages to the same effect might be quoted.

§ 359. I begin with an objection, relating to a matter already referred to, which perhaps concerns the form of the theory rather than its essence. The evolutionist does not seem sufficiently to notice that, while seeming to make inconceivability an ultimate test or criterion, he is really throwing entire discredit on the mind's power to conceive or not conceive as such

Men do indeed believe firmly when they think the contradictory of their belief inconceivable; not as being merely inconceivable for them, but as being inconceivable for thought as such. For the moment a man is convinced that some one else can conceive what he cannot, he ceases to suppose that his own state of mind can determine the truth in the particular case. Thus a man without a musical ear nevertheless may believe that there is such a thing as a musical order. But the supposed history of the mind's modification implies that, though we are not able to conceive the contradictory of an axiom, we might have been able, and our ancestors perhaps were able. and thus the mind's limitation in respect of the conceivable becomes as such entirely indifferent.

Any reliance placed on the inconceivableness of the contradictory as a criterion must lie, and indeed according to this theory does lie, in the way in which it has been produced, and this is the essential thing to be considered The criterion has its value because it is the effect, as supposed, of an experience infinitely greater than that of one individual: and thus when Mill objected that if the test got its value from experience, we had better appeal directly to experience,[1] it was replied that the large experience represented by the test was inaccessible to the individual. It is easy to see that the maximum logical

[1] Mill, *System of Logic*, II vii, § 2.

worth of the test would be that of an induction drawn from all the instances which have occurred in the experience of the race, supposing these could be presented to one mind which would draw the inference. The induction has registered its conclusion automatically in the physical organism, and, through that, in the mind. The race has come to believe that all A must be B, because the constant experience through its history of instances of A which were B has so modified the organs that they cannot be used for thinking A except as B.

§ 360. It has been inadvertently assumed that in axioms such a belief has been produced in one way only. From the very assumptions about biological processes which this theory makes, it is clear that the sufficient and necessary condition that the belief 'all A must be B' should be produced, is not that every A should be B in the nature of things, but only that those instances of A which have acted on the organs of perception should have been B. And thus the conviction that A must be B could be perfectly well produced in the automatic way supposed, in a case where A was not necessarily B. The limitation of our experience to a certain species of A which would be necessary for this, would correspond to known facts. For instance, generation after generation of a particular race had never seen swans which were not white. Countless generations living in certain parts of the earth have never seen water in a solid form.

Thus from the very manner of its formation our belief that the arc must be greater than its chord is compatible with the existence of arcs which are not greater than their chords. There are ways enough in which our limitation to one kind of arc may have come about. For instance, naturalists expect to find new forms of flora and fauna in regions which are being explored for the first time. How do we know—not to speak of what may be found in other planets and stars—that the polar regions have not an abundance of that other kind of arc and chord? And was it not as important scientifically that the naturalists in the *Challenger* should have dredged the deep seas for them as for those objects in which they were immediately interested?

Or again, allowing, what could not be known if the theory we are examining were true, that all the arcs in the world now

are greater than their chords, how can the evolutionist know that in early geological periods there were even any arcs at all which were greater than their chords? For aught he can say the arcs not greater than their chords may have been contemporary with the 'dragons of the prime' and have perished with them. Why should he expect us to allow arbitrary assumptions here, which are allowed in no other empirical science?

It is quite remarkable to find the evolutionist confidently making such statements as the following, without even asking himself how he could be entitled to them. 'Space-relations have been the same not only for all ancestral men, all ancestral primates, all ancestral orders of mammalia, but for all simpler orders of creatures.'

But there is another and more serious form of the difficulty. It is not even necessary that the experience of the race should have been uniform. Arcs may have been seen equal to or less than their chords, but if they only came seldom, or if in the course of time their number much diminished, like that of an expiring race of animals, then according to the biological principles presupposed, the infinitely greater accumulation of contrary experiences would in time wipe away all trace of them from our organism.

Worse than this—owing to the way in which Nature has fixed our functions, we could not perceive arcs which were not greater than their chords, even if there were plenty of them about us. It is therefore useless to dredge the deep seas or to go to the Poles, and indeed we do not know whether in our own parts of the world these arcs have not begun to exist. We could no more see them than a blind man could. And thus the evolutionist, professing to have established the validity of the test of inconceivableness, has unwittingly shown it to be consistent with the contradictory of what he supposes it to guarantee absolutely. It follows irresistibly that all that seems to us simplest, clearest, most self-evident, and more certain, according to the evolutionist himself, than anything else we can believe, may be an illusion.

The theory which was to have reconciled great philosophies has destroyed itself, and has ended in a scepticism which has not even the merit of being self-conscious.

§ 361. The contradiction cannot be avoided by dogmatically affirming that the experience which has fixed our functions does, as a matter of fact, correspond to a universal truth which has no exceptions: for this involves, according as we look at it, a new inconsistency or an argument in a circle.

(i) We are told that the ultimate and only criterion for the individual who forms the last term of the series is the test of inconceivableness, and yet on the other hand that the criterion is trustworthy because an invariable experience has produced it. To vindicate this position the evolutionist must know the latter proposition to be true. But if he does he has a higher knowledge than the criterion itself, because it is knowledge from which the criterion derives its value, and thus the criterion is not the ultimate criterion for him. The evolutionist has fallen into this contradiction apparently because he has forgotten that he himself is one of the last terms of the series.

(ii) Or the difficulty may be put thus. The inconceivableness of the contradictory of an axiom has been produced, it is said, by a constant experience in the race, which again corresponds to a universal truth without exception. How does the evolutionist know that the experience of the race has been such, for he cannot have had it; and *a fortiori* how can he know the universality of the corresponding fact in nature? Apparently from what has been quoted he thinks he knows. Being the last term of the series he can only know the fact by the inconceivability which he supposes it has produced in himself: and thus the invariable experience and the corresponding fact give the criterion its value, and they themselves are only known by the criterion: which is arguing in a circle.

This may be shortly put by saying that the evolutionist philosopher has cut himself off from the possibility of giving the necessary evidence in favour of his own theory.

But it may be answered that though satisfactory evidence for the theory cannot be given, it is a possible hypothesis, and though it may have to surrender all claims to establish the validity of our beliefs, it may be true, for constant experience might produce such an effect on our organism. This of course would be giving up a great part of the evolutionist position.

In the first place, it must be answered that according to

biological laws the loss of a function or of an organ may not only be caused by want of a use for them, but also by the operation of various processes, among which, for instance, are diseases, and how can it be known that some such process has not destroyed our capacity for thinking the contradictory of axioms?

But the most important difficulty is this. The objector can hardly have reflected on the real consequence of his hypothesis. It would throw a doubt, as we have seen, on every principle however simple, self-evident, and certain it may appear, and therefore it would leave nothing even to found itself upon. It would throw doubt also upon experience, which is supposed to be our most direct access to reality, for owing to the limitation of the nervous structures to one kind of function, our minds may be misrepresenting the object.

§ 362. How deep this unconscious scepticism has gone will appear still better from another aspect of the theory.

'For logical intuitions', it is said, 'there is no warrant assignable other than that assignable for all intuitions accepted as certain. namely, the impossibility of thinking the opposite. Unless it be alleged that the consciousness of logical necessity has a different origin and a higher origin, it must be admitted that the consciousness of logical necessity is just as much a product of past experiences as is every other consciousness of necessity.'

It follows, then, from what we have seen, that these logical intuitions must share the uncertainty of axioms. But among them we find included the principle of the syllogism, the law of excluded middle, the principle of contradiction. They are in fact those simple 'laws' or forms of thought to which thought must conform to be thought at all. Thought therefore cannot throw any doubt on them without committing suicide. As Aristotle has said.

ὁ δ' ἀναιρῶν ταύτην τὴν πίστιν οὐ πάνυ πιστότερα ἐρεῖ.[a]

But there is one short general criticism which seems enough, and would have explained beforehand without such an examination of details that the evolutionist theory must end in self-

[[a] Now he who destroys this belief cannot expect his own words to be believed *Eth Nic* 1173[a] 1]

contradiction. There is an elementary principle on which we should expect all philosophies to agree, which is that thought cannot question the validity of its own presuppositions or even try to establish them without self-contradiction. Now it is evident that the evolutionist theory violates this principle, whether we consider the account given in it of the supposed criterion of all truth, inconceivableness of the opposite, or the account of those primary laws of thought which have just been spoken of.

§ 363. The mistake is of the more elementary character when the attempt to establish the laws of thought, or a general criterion, is made by help of the reasonings of a special science, for that of course must presuppose the general laws and the criterion. But this is precisely the use here made of biology. The reasonings of biology would collapse if it did not assume, for instance, the principle of contradiction; it is futile therefore to prove its objective validity by biology.

As to the criterion itself, which is to show that a cognition is of the highest rank (its 'unsurpassable validity', &c.), the principles of biology cannot depend on it since biology has to establish it. Hence, either they have not the highest rank, which involves the absurdity that the criterion of cognitions of the highest rank is shown to be a valid criterion by appealing to cognitions which are not of the highest rank: or else the principles of biology have the highest rank, and then it turns out that there are some absolute truths which are not derived from the criterion, and, what is still worse, that evolution has altogether forgotten to explain their origin, thus leaving the old problem so far from being solved that it is not even attempted. For instance, among these principles so entirely unaccounted for is the axiom of universal causation. for of course biology presupposes this. It is true that in one place where Mill is criticized it seems as if the belief in the uniformity of nature (on which biology depends) would be accounted for by the same biological argument as the other axioms. If so, confusion would be confounded.

§ 364. May I say, in conclusion, that I venture to think there is all the difference between evolution in science, and what is known as the evolutionist philosophy? It seems reasonable to

believe that there are many scientific evolutionists who will not be led by a mistaken idea of the solidarity of the subject to suppose themselves committed to the conclusions of the evolutionist philosophers.[1] It is not the first time in history that enthusiasts for a new and successful development of science have tried to carry the science beyond its proper limits, and have aspired to make it universal by making it what science never can be—that is, philosophy.

§ 365. The futility of the inductive theory was seen by Descartes long before the development of English empiricism, and with his name and that of Leibniz is connected the theory of Innate Ideas. This theory has often been misrepresented, but the gist of it is that the truths in question are the mind's own possession, developed somehow by its own activity and not derived from experience—in the narrow sense—of the particular facts.

The well-known criticisms of Locke on this theory are with scarcely an exception an *ignoratio elenchi*. The answer to him is obvious: Innate Ideas are intended to account for knowledge where it *is*, not where it is not. It is as useful to allege against it that plants have not these ideas as that children, idiots, and savages have not. Locke was also under an illusion about the presence of an idea which is implicit and not explicit. The time at which an innate idea appears and the extent to which it is defined are entirely irrelevant to the real issue. Locke was answered by Leibniz in his *Nouveaux Essais*.

The real difficulty for the Cartesian school was to account from their point of view for the validity of these beliefs which the mind has on its own account. Why should this necessity for thought be true of the objects of thinking? The theory of the inconceivability of the opposite as the criterion of truth is closely allied to that of Innate Ideas and may fairly be called only a particular form of it. We may say that it is a test of truth in this sense that, when the contradictory of a statement

[1] The speculations of Weissmann have made a difference in the beliefs of evolutionists about the possibility of inheriting experience; but neither biology nor any other special science can deal with the question of axioms or of the criterion of truth—*περὶ δὲ τῶν ἀρχῶν λόγον οὐχ ὑφεκτέον τῷ γεωμέτρῃ ἢ γεωμέτρης*. [Of the first principles of his science we must not require an account from the man of science, *qua* man of science. *An. Po.* 77^{b} 5.]

is inconceivable, the statement must be true. No exception can be found to this rule; such exceptions as are alleged by Mill and others are mere confusions depending upon ambiguity in the members of the statement. For example, when it seems as if an assertion that was inconceivable has become conceivable, there are really two assertions, not one, concerned. These two are confusedly represented as one through an ambiguity in the logical subject, which appears to be the same in both statements but is not. The reason why inconceivability cannot be an ultimate criterion (in the limited sense in which we can allow such an expression) is that we cannot begin with negation; what a thing cannot be we can learn only from our apprehension of what it must be, that is, from itself as apprehended.[1]

The true difficulty with which the Cartesians were faced depends in the last resort upon a representation of the mind as distinct from the object, and it is one of Kant's merits that he seems to have been the first to realize clearly the nature of the difficulty, even though his own solution may not be satisfactory. If, he says in substance, instead of the mind obeying the object, the object has to obey the mind, we could understand at once our possession of universally valid truth. This is Kant's Copernican idea and is obvious enough when once stated. The important addition is that he believes that in no other way could we have universal knowledge such as appears in the demonstrative sciences. The required correspondence of object and subject is secured by the simple fact that the object is the object of a subject and the two are in inseparable relation. However we try to represent the object as independent, to be the object of experience it must obey entirely the law of being experienced, which is only the other side of the law of the experiencing mind. Thus if we present the mathematician with a particular case in experience of two lines which we affirm to be straight and which yet enclose a space, he will without hesitation pronounce that they are not straight, thus legislating (in Kant's words) without hesitation for the object.

The distinction then, for Kant, of subjective and objective is not a distinction of thought from something alien to it; the objective is within thought. Nor has the time-order of our

[1] See the remarks about immediate inference, §§ 216–17

thoughts anything to do with their content or meaning. The laws of this order (memory, association of ideas, &c.) are not laws of what our thoughts mean, nor does the whole of what we know stand in any time relation to anything. We do not, for instance, suppose that the truth about things did not exist till we discovered it.

§ 366. This point of view must, however, be supplemented by what it has been the object of the preceding lectures to develop. Kant's standpoint is what follows if we start from the subjective side of knowledge only. Even starting thus, we cannot represent reality as other than what 'agrees' with the necessity of thinking and knowing, but the standpoint is indirect and incomplete. The true account of the necessity of thought explains why there must be the 'agreement', and this shortly is that the necessity of thinking is the necessity of apprehension. Necessity of apprehension, then, turns out to be unintelligible save as apprehension of a necessity, that is, apprehension of objective necessity or apprehension of what reality must be. Thus it is plain that *agreement* is not a proper term to employ and that the question *why* reality must correspond to the necessity of thinking only arises by a false severance of the apprehension from what is apprehended. Out of this false severance comes all our difficulty as to how the two should agree or correspond. The fact is that there is no necessity in the thought or apprehension save the apprehension of an objective necessity. When once this is appreciated the question of agreement or correspondence, which will still arise even when we have satisfied ourselves by the indirect method of the above (apparent) adaptation of Kantian principles that we cannot intelligibly represent them as *not* corresponding, disappears. It still arises because we are, so to say, only negatively forced into the conclusion and don't see the positive reason. We see that it must be a fact but not quite how it must be or the reason of it. In the light of the positive development the question disappears as altogether unreal. There has never been any severance and so there is no need to explain the correspondence The necessity of thought (apprehension) can only be the necessity of thinking, as the thinking or apprehending of the necessity in the object with which we have been uselessly trying to reconcile it.

V[a]

THE METHOD OF PHYSICS AND ITS RELATION TO PURE MATHEMATICS

§ 367. The sciences called applied mathematics are sometimes represented as having an intermediate position between the inductive and deductive sciences. They are not pure mathematics but yet mathematics is necessary to them. This kind of position for them is in a way recognized by Aristotle. He distinguishes (*a*) the knowledge of the fact, e g the account of the observations of the nature of the rainbow; (*b*) optical science which is, as he supposes, the explanation of the rainbow by mathematics. This is for him the science of the fact and the reason together.[1] Pure mathematics, in this case geometry, he regards as (*c*) the science of the reason alone. This last is a strange phrase. 'The reason without the fact' means that the reason is found in mathematics but that the fact does not appear there. He speaks as if mathematics supplied the complete reason. But, if it did, how could the fact fail to appear as a deduction in mathematics? Aristotle has never asked himself this question.

The distinctions he makes are untenable. Mathematical

[1] ὅτι + διότι.

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[a] This chapter was until 1899 followed by sections, the main purport of which appears elsewhere in the book Of the rest I am not sure how far the writer would have maintained their tenets in later years Their contents were:

§ The conception of force in physics Further development of the relation of pure mathematics to physics We do not understand what Force means

§ On the axioms of physics Some great generalizations (e g. the indestructibility of matter) are self-evident and *à priori*, the experiments employed being suggested by our conviction of their truth

§ On the objective validity of our thought. The problem originates in difficulties felt about the axioms of formal logic [*Dictum de omni*, &c].

§ The inductionist theory of the origin of axioms, Mill's theory of parallels; association of ideas explanation, H. Spencer's theory of inherited experience

§ The Cartesian answer. The inconceivability of the opposite as a criterion of truth Kant's solution [the last accepted broadly in 1899]]

physics depends ultimately on physical laws, which pure mathematics cannot give. Thus in optics, as Aristotle knew them, there were the laws of reflection and refraction, and it is odd he did not see that in mathematical optics, as he knew them, these were accepted as facts given by observation, together with certain geometrical relations which they involved. Thus all geometry did was to deduce certain geometrical consequences from the geometrical part of the given premisses.

§ 368. As may be seen in the instance of optics, mathematical physics requires physical laws, which mathematics cannot supply. About this we find in modern times a confusion like Aristotle's, though not quite the same. The physical laws themselves involve certain mathematical relations both geometrical and of pure quantity, and all mathematics can do is to develop the consequences of these mathematical data. How, for example, does mathematics deal with motion? Certain laws of movement are given, i e. laws of the way in which a body changes its distance from certain fixed points. Given such changes of geometrical relations, geometry simply calculates certain other geometrical relations which follow from them. But it can say nothing whatever of the reason for the given laws of the changes. To put it otherwise, if we think of force as the cause of movement, it is with the effects of force, as changes in the geometrical relations of bodies, that mathematics alone can deal. Given, for instance, the force of attraction and given its operation or effect, in the sense that a body revolving round the sun tends to shorten its distance from the sun at a regular rate,[1] and given that it has at the same time a tendency to move parallel to a certain fixed line at a certain rate, we find the body describes an ellipse. This is only one geometrical fact deduced from another. Given that a planet moves in an ellipse with a certain varying velocity, we can prove that its distance from the sun tends to shorten in the way attributed to attraction, and that its tendency to move in the parallel direction is constant.

What is not supplied by geometry is assumed, i. e. the parallelogram of forces.

§ 369. This (i e the mathematical work) is entirely misunderstood by Mill in his chapter on the Deductive method. He

[1] The inverse square of the distance.

supposes that if the effects of the forces, operating singly, are given, or the law of each force operating singly is given, the combined effect, when the forces act together, can be found by ratiocination, a mere name for the syllogism rejected in Book II. He is thinking of course of mathematical physics and especially of the example we gave in the last section. He supposes, given the forces acting, given the operation of the law of attraction on a body, and the operation of the original projection, that the result, the path of the moving body, is got simply by deduction. This is impossible and, given the laws operating singly, mathematics as such is quite unable to discover their combined effect. It cannot move a step without assuming a physical law of combination, that is, how these forces will act in combination.

In the given problem mathematics solves a purely mathematical question: it does not touch the conception of Force as such at all. It deals only with the changes of geometrical relations, assumed as the effects of the forces; and it assumes what it is supposed to prove, namely, certain laws of the combined operation of physical forces. It is the easier for Mill to make this mistake because he has so entirely mistaken the nature of the geometrical method itself. He thinks of it in general as a method of deduction. If he had realized that its method is construction, he might have seen that the construction, being solely of mathematical quantities or relations, could not possibly deal with the physical conception of force, which is not identifiable with such relations.

§ 370. In one department of these sciences, i. e. in dynamics and statics, motions are considered without relation to their effects on consciousness. But there is another department in which motions are treated as causes of what are taken to be sensations of ours—heat, light, and sound. Here we are concerned with another set of causal laws, i. e. the effects in feeling-consciousness of some action (in the way of movement) upon our sense organs. Now here the mathematical part is concerned solely with the movements of the material bodies. It cannot deal with them as causes of sensation. Violet may be the colour corresponding to a certain wave-length and the ordinary theory is that it is a sensation of ours, like heat or cold, caused by the impact of ether waves of a given wave-length on our retina.

But mathematics can only deal with the undulations: it cannot say why violet corresponds to, or is the effect of, the undulatory movement. That is learnt *a posteriori* from certain facts of consciousness taken in connexion with the hypothesis of the undulatory theory. Sometimes, again, it is supposed that the concord between two musical notes is accounted for by the fact that a certain number of vibrations of the air corresponding to the one take place in the time occupied by an integral number of the vibrations corresponding to the other. But this is an illusion. It is merely an interesting fact that such commensurateness corresponds to our musical sense of concord. The science fails to establish any connexion between the two. We cannot possibly show why that numerical connexion should have as its effect a musical concord The apprehension of the sensation, if it be such, is in no sense an apprehension of the vibrations which are its physical cause.

VI [a]

SYMBOLIC LOGIC

§ 371. The term Symbolic Logic is taken in the following sections in its current meaning. It denotes, that is, a theory which, whether rightly called logic or not, proposes to conduct operations of purely formal inference by the aid of an algebraic calculus. Dodgson,[b] in calling his book *Symbolic Logic*,[1] departed from what seems to be the common usage, for he did not employ a mathematical calculus; and how far he was from a mathematical treatment is still more evident from the unpublished second part, of which he, with his usual kindness, sent me the most important of the proof sheets. I like to think that my main contention would probably have pleased him a good deal.

A common objection to Symbolic Logic, as thus defined, is that it is mathematics and not logic, and so is not necessary to the logician. A line of defence taken by symbolic logicians is that the subject is not strictly mathematical, the signs of multiplication and division have not the mathematical meaning. But while this could hardly be alleged in the case of the signs + and −, there must be something essential, common to the use in mathematics and symbolic logic of the signs of multiplication and division, for otherwise the employment of these signs in the latter would be ridiculous. The truth is, that in symbolic logic these formulae follow the same laws of development as the corresponding formulae (or operations, rather) in mathematics, notably in the matter of the multiplication of the signs + and −.[2] Besides, the idea of an equation, which is

[1] *Symbolic Logic*, Part I, Elementary, by Lewis Carroll London (Macmillan), 1896.

[2] See p 641, § 379, infra

[a] Redrafted from two manuscript lectures of Hilary Term, 1898, and manuscript connected with the same subject I have left the 'propositional' terminology

[b] The Rev C L Dodgson, the author of *Alice in Wonderland*, was lately dead when the lectures were delivered. There is a kind of mathematical calculus suggested in Dodgson's book as an alternative to the counter system, which makes the easy part and the bulk of the treatise.]

fundamental to the subject, is obviously a mathematical idea. Thus Boole entitled his earlier investigation 'The mathematical analysis of Logic', and gave as a sub-title to his better-known work[1] 'The mathematical theories of logic and probabilities'.

§ 372. A stronger line of defence may be suggested. Suppose it *is* mathematics; provided only it is sound mathematics, it is futile to object. We may not like mathematics, but that is not an objection to the subject. And we may not like the extra trouble; but astronomers might as well agree to ignore the planet Neptune, as a member of the Solar System, because it gives them extra trouble. On the other hand it would be a most interesting fact that mathematics could conduct logical processes (as Boole's titles imply), and the logician would be bound to study it as throwing light both on logic and mathematics. Nor is it really enough to object that the inferences can be conducted by ordinary methods, so that a calculus is not necessary, or even that these ordinary methods are simpler, quicker, and so better. The symbolic logicians would probably deny the truth of both objections and certainly they would deny the latter with entire confidence. The answer should rather be that, even if the calculus were more cumbrous, we should have to study it or we should be cutting ourselves off from a most interesting piece of knowledge. The objections clearly are only relevant to any claims of superiority made for the calculus and not to the questions of its right to exist and of its validity.

§ 373. The original objection, however, has a deeper basis than mere prejudice and rests on a sound notion of logic, often insufficiently realized, especially in Deductive Logic, but tending to become conscious of itself, when such investigations as these are presented seriously as logic. This sound notion is that, with regard to inference, the methods of all the special sciences are the object-matter of logic; it neither makes them nor conducts them, but studies them. It is thus, in a sense, more general than any of them. This being so, it is incredible that any particular scientific method, for example the mathematical, should be the method of logic itself. We suspect something wrong and that the subject cannot be logic at all. That is an

[1] *An Investigation of the Laws of Thought, on which are founded the Mathematical Theories of Logic and Probabilities*, George Boole. 1854.

objection which is never even dreamed of by the symbolic logicians, but the greatest danger usually comes from the quarter which we least suspect. Indeed it would be a shock to their feelings to have their title to be logicians disputed, for their language is in no sense self-depreciatory and they openly profess to make great improvements on the common or 'ordinary' logic,[a] as they call it.

§ 374. The foreboding, however, of the 'common' logician is justified, for he will find that the symbolic logic as such consists of the solution of particular problems, which are on the same plane as the solution of geometrical or algebraic problems and, though concerned with the abstract forms of subject and predicate, as specially scientific as these mathematical processes—no more logic than they are, and related to logic precisely as they are. Incidentally there is a little elementary logic involved, but the real and serious problems of logic proper do not appear, nor is the symbolic logic able to touch them. In comparison with the serious business of logic proper, the occupations of the symbolic logician are merely trivial. I do not want, however, at present to dwell either upon the comparative importance of the 'logical' calculus or on its relation to true logic, but to take it rather at its own valuation, to inquire what its problem is, and with what success the solution is attempted.

§ 375. The problem stated in most general terms is to find what various inferences can be drawn from premisses stated in the abstract symbolic form of the traditional syllogistic logic, with no other quantitative notions than 'all', 'some', and 'none'. It might seem that in this comparatively humble sphere there would be no difficulty, as every step of the argument is either immediate inference or needs no more than the first figure and mood of the syllogism, than which nothing could be simpler. But it should be remembered that, in the solution of a difficult and complicated mathematical problem, every single step, when the reasoning is fully expressed, may be very simple. The difficulty consists in finding the right steps to take and the right order of them. And so, in formal thinking, it is possible to devise problems of such difficulty as would never be

[a] The phrase goes back to Bacon. Here Wilson defends *logica communis* polemically though he continually elsewhere speaks disrespectfully of it.]

anticipated by those who are only accustomed to such specimens of formal argument as are given in the traditional logic.

If the premisses have compound subjects and predicates, as all ABC is D, or DE, and if they are further perplexed by a judicious sprinkling of negatives (e. g. all A, not-B, C is not-D), if further a considerable number of premisses be taken, a problem may be invented which may take hours to solve, unless we have some definite rule and method besides the mere possession of the first syllogistic figure.

The following is an instance of a formal problem devised by Dodgson, who was clever in the construction of difficult problems.

Given that—

(1) all active old Jews are healthy,
(2) all indolent magistrates are unpopular,
(3) all rich snuff-takers are unhealthy,
(4) all sarcastic magistrates are Jews,
(5) all young snuff-takers are pale,
(6) all rich old men, who are unhealthy, are sarcastic,
(7) all magistrates who are not poets are studious,
(8) all rosy magistrates are talented,
(9) all talented and popular students are rich,
(10) all pale snuff-takers are unpopular,
(11) all unpopular magistrates are abstainers from snuff,
(12) all talented poets who are active are rich;

to prove that no magistrates are snuff-takers.

To reach this conclusion by a series of ordinary syllogistic arguments would involve a very long process. By Boole's methods I have calculated that 2,048 factors are required, but by an improved calculus of my own the result may be gained in a very short time

A simpler example is the following.

Given that No AC is B and that no D which is not-B is A,
To prove that all A is either not-C or not-D.

A third example might be one given by Boole.[1]

Given that—

(1) If A and C are absent, E is found with B or D, but not with both.

[1] *Laws of Thought*, p 146.

(2) If A and D are present and E is missing, either both B and C will be found or will both be absent.

(3) If A is present with either B or E or both, then C or D will be present, but not both, and conversely.

What conclusions can be drawn ?

This is said to be ' the most intricate of any given by Boole '.[1] It took two and three-quarter hours to work out by ordinary methods, but only twenty minutes by my own calculus. The solution Venn gives could not have taken very long.

One soon discovers that some definite rule and method is required here as much as in mathematics, and on this point Dodgson, in the preface to his book, rightly insists [a]

§ 376. Method, then, being needed, the subject really being a particular science, any method deserves to be examined, provided always the thing is worth doing at all. Hence, *prima facie*, there is a case for symbolic logic, though it can hardly be said to have originated in a consciousness of the difficulty of such problems as I have been alluding to. It was probably felt by those who originated it that formal reasoning was like mathematics, in its *a priori* character, in its necessity and absolute accuracy, and like algebra, in the use of abstract symbols. The science of pure quantity had received an enormous extension by the invention of the symbolic method of algebra, and the thought naturally arose that something similar might be done for formal thinking

Now when the perception of the aforesaid analogies has suggested symbolism, two courses are possible. We may either study the peculiarities of the actual problems which have arisen in the science and consider what kind of symbolism they seem naturally to suggest as likely to facilitate their solution, or we may try to apply so far as possible the already existing symbolism of algebra, as it stands, and thus invent a calculus on strict algebraic analogy and then see what we can get out of it. This second seems really to be the way in which symbolic logic came into being. The first method is clearly the sound one, for,

[1] *Symbolic Logic*, by John Venn, London (Macmillan), p 351.

[a] In the original there is here a reference to the great loss to philosophy by the untimely death of Mr Adamson, Fellow of St John's College, ' a good mathematician and a promising metaphysician ']

though it is right enough to take the suggestion of symbolism in general from algebra, it is a mistake to suppose that the symbolism adapted to one science is likely to suit another. The lesson which modern logic has learned and has to teach is that the method of the particular sciences depends on their object-matter, the various forms of the method of science are discovered by scientific men themselves in the study of the special problems peculiar to their respective sciences, and they cannot be discovered in any other way.

§ 377. The presumption is therefore that if the reasonings in a given science can be conducted by a symbolic method, the same symbolism will not suit another. The case of analytical (i. e. Cartesian) geometry (which is perhaps too easily misrepresented) is not an exception. There geometrical relations give rise to relations of quantity, not sham quantities, but real quantities which can enter into equations; they thus give rise to true algebraic problems which algebra solves in real, not sham, equations, with real multiplication and division. Consequently, if the symbolism of algebra is transferred to the science of formal thinking, we (the common logicians) cannot help entertaining certain anxieties. We should fear that mathematical analogy, the symbolism being the same, might sometimes mislead, and a relation of logical terms be confounded with a relation of literal quantity. Again, we should fear that the algebraical processes would only fit partially, and that the calculus, instead of being a potent instrument as in true algebra, might be rather narrowed in its use; the attempt to carry it out consistently as a general calculus might even lead to contradiction and positive error. We might again expect that much ingenuity would be expended in solving problems the difficulties of which were due to the artificial form into which the premisses were forced by the calculus itself. Lastly, while skilful enough in overcoming unnecessary difficulties of its own making, the calculus might possibly not facilitate the solution of the difficult problems which arise naturally in formal thinking itself.

§ 378. To judge from its contents, the main ideas of the logical calculus seem to have been developed in some such way as this. Formal logic from Aristotle downwards has used symbols, viz. letters of the alphabet, like algebra, for class conceptions;

for instance, all A is B. Operations with these symbols it recognizes in symbolic form only in the case of the association of two class conceptions to form a species. Thus AB means that which is both A and B, and thus a narrower class than either. This symbol AB resembles the symbol in algebra of the multiplication together of two quantities. These symbols, then, form the starting-point. Equations being the important instrument of algebra and its applications, an equation is sought for between the given symbols. 'A = B' suggests itself, and the question arises how this should be interpreted. It is seen that the identity of the extension of two classes could be thus represented, and this is the interpretation given; that is to say, A = B means that the whole of the individuals in the class A constitute the whole of those in the class B.

A+B, then, would mean the united extension of A and B, or the class formed by uniting A and B. If A' and B', then, symbolize not-A and not-B respectively, we get A+B = AB'+AB +AB+A'B. Here the interpretation of AB+AB is not what it would be in algebra, because if the classes A and B overlap, the class AB is not counted twice over.

Thus AB+AB = AB,

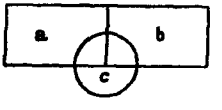
Similarly, A+A = A and not 2A.

If B is part of A, the usual symbol for subtraction may be used, so that A−B = AB'. Here certain difficulties arise if B is not wholly contained in A. They need not, however, be discussed here.

§ 379. The ordinary law of algebraic multiplication is said to hold;

viz. if $a+b = x$,

$$cx = c(a+b) = ca+cb$$



for what is common to the extension of c and x must consist of what is common to c and the parts a and b of x. Supposing c has nothing in common with b, then the class cb is non-existent and so the equation remains true because $cb = o$. This last is, as we shall see, a serious assumption. It may, then, easily be verified that the multiplication rule obtains, if for c a polynomial be introduced. Thus

$$(a+b)(c+d) = ac+ad+bc+bd;$$

also that the algebraic rules about the signs + and − hold in this sort of multiplication. Algebraic powers, however, cannot enter since $aa = a$, obviously. In fact for Boole $x^2 = x$ is the expression of the fundamental rule of thought. The operation of division appears to a certain limited extent, and such a symbol as $\frac{x}{y}$, of which the symbolic logicians are rather proud, though it is really a snare spread for their feet.

§ 380. The symbol *ab*, if compared with the corresponding algebraic symbol, has nothing to do with multiplication in the sense of the repetition of a unit, but serves to indicate an operation following some of the general laws which are followed by the symbolism of multiplication. In *ab*, *a* may be taken as the symbol of an operation, viz multiplication, performed upon *b* as a given quantity, but *b* may just as well be a symbol of operation, and the mathematical theory of symbols of operation seeks to make such symbols always represent the operation of multiplication. Thus the symbol *a* standing alone represents the operation of multiplication performed on the unit of number, 1, *a* thus is equivalent to $1 \times a$ and $ab = (1 \times a) \times b$. *a* and *b* then are always symbols of operation, that which is operated on being the unit whatever it may be. Unity, then, is understood always and not expressed with the symbols

In the symbolic logic, however, *ab* denotes an operation of *a* upon *b*, which is the operation of taking the part of *b* common to it and *a*. On the analogy of mathematics we seek for a way of representing *a* itself as an operation. What, then, must be the class operated upon by *a* to produce *a*, the ultimate unit, so to speak, operated on? That is to say, what is that class such that when we take out of it what is common to it and any class *a* we get the class *a* itself? The answer is obviously 'reality'. As reality, then, is to be understood with every symbol like *ab*, just as 1 is in algebra, the symbol 1 is used in this calculus as the symbol of reality.

This may seem ingenious, but it is really unfortunate. It would have been much better to have taken a symbol which originally had no numerical significance to represent reality, e. g. *r*; then *ab* would be understood to mean *abr*. The negative of *r*, that is the unreal or impossible, would be represented by

r', like any other negative class. A serious mistake would thus have been avoided; for, as we shall see presently, the symbol 0 or zero, besides its significance as a true zero, has to do duty in the calculus as the symbol of the impossible with grave consequences.

[For the present we are concerned only with the normal meaning of zero, in the logical calculus. A non-existent class is regarded as a compartment of reality which is empty and is represented by 0. This appears to follow in a mathematical manner from what has been already laid down, for ab denotes the operation of taking the part of the class b common to it and a. Thus oa $(= o)$ will stand for the things which are both nothing and a, that is, for any empty class.]

§ 381. On the use of 1 to symbolize reality is founded the equation fundamental to the calculus $x+x' = 1$, which expresses (or is supposed to express) the fact that everything must be x or not-x. The equation expresses this in the form that the whole of reality $(= 1)$ is made up of the class x and not-x, i. e. what is not in the class x.

Similarly $a = ax+ax'$, which we may get directly, or derive from the above by writing $(x+x')$ for 1 thus

$$a = a \times$$
$$= a(x+x') = ax+ax'$$

These equations we shall call for convenience equations of dichotomy, and shall criticize them presently.

§ 382. We will now turn back to the equation A = B. It expresses primarily a relation between the extensions, not the intensions, of A and B. That is, it does not mean that the nature of A necessitates that of B and conversely, but that the two classes coincide. This leads, for example in Venn, to the crudest and naivest nominalism. Now, in the case of truly universal notions, the extension is infinite and there is absolutely no way of identifying their extensions except through the conceptions. We cannot know that the class A coincides with B, except by knowing that the conception of A necessitates that of B, and conversely. That is, we can only know it through the two propositions, all A is B and all B is A. This is, then, not so much the interpretation of a *given* equation, but the knowledge we must have before we can form the equation at

all. Similarly the proposition A = B + C implies all A is either B or C, all B is A, and all C is A. Since, then, the relation between the extension of two or more conceptions can only be got at through the relation between the conceptions themselves, as expressed in the aforesaid propositions, one may be curious to know beforehand how far any reasoning from the equation can be carried on without referring back to these propositions.

§ 383. It is obvious that the equation A = B will not do for the ordinary proposition 'all A is B', it only represents the case where 'all A is B' is convertible simply, that is, it represents the two propositions 'all A is B' and 'all B is A' taken together. Now the important problem of the calculus is to find an equation for the ordinary proposition 'all A is B'. The relation or equation between the extension of A and B is clearly only A = some B. This is represented by A = vB, where *v* has the special significance of 'some' This is merely the pretence of an equation, a form of equation without the reality. For an equation something definable must be identified with something else definable, but here a definite A is equated to an indefinite, which seems to contradict the idea of an equation. The symbol v does not improve matters, especially when we find that the calculus, for other reasons, is obliged to give it the entirely indeterminate meaning 'some'; it may be none, or all, or a part. Further, A = vB does not even tell us that any A is B.

§ 384. There is, however, another form of equation for the proposition all *x* is *y*, due apparently to Boole, which seems to have the exactness necessary for an equation and to secure another advantage. In this form of calculus the proposition all X is Y is treated as hypothetical, not so much from a theory that the universal affirmative categorical is necessarily hypothetical (if anything is X, it is Y), but from the special needs of the calculus itself. In algebra when we set a problem and have to determine the value of a so-called 'unknown' *x*, which suits the problem, we may be asking that certain conditions shall be satisfied which cannot be, and then our calculation shows this by representing finally the unknown *x* under the form of an impossible operation. Thus we cannot say that the variables in our equations always represent real quantities. This logical calculus proposes its problems with the same generality,

and rightly. A subject X is given as satisfying certain conditions in the form of premisses, and the combination of these may result in the conclusion that X has contradictory attributes (say X is Y and X is Y'). We are in such a case not to pronounce the premisses false, but X impossible. The important case will be when X is compound = AB; then the conclusion AB is impossible yields the judgement 'no A is B'. Consequently in a premiss all X is Y, we cannot say that X is possible, it may be impossible, and so it must be read, 'X, if possible, is Y', or 'X, whether real or unreal, is Y'. On this account propositions which are universal and affirmative are said not to be existential, that is, not to convey definite statements about existence. They are regarded, whether rightly or not, as in a sense ambiguous. However, it is said, the proposition all X is Y does involve a definite statement about existence, viz. that there are no really existing things which are both X and not-Y, or XY'. This is taken, then, as the equivalent of the proposition all X is Y and expressed in the form there are no members in the class XY'. This translated into equational form becomes XY' = 0. This seems at first most ingenious, for we now have an equation of the requisite accurate and definite mathematical form, and we need not insert any hypothesis about the existence of the terms. This equation is possibly the most important and fundamental feature of the calculus. I shall try to show that it is an illusion, by proving —first, that the proposition which it symbolizes, no XY' exists, though an inference from all X is Y, is not the true equivalent of it; secondly, that given the proposition 'No XY' exists' (or XY' is impossible) it cannot be rightly symbolized by the equation XY' = 0, and that the symbolism involves a grave error from the point of view of a mathematical calculus, or theory of symbols of operation in general. Finally, that it is impossible to give a true equational form at all to the universal proposition 'all X is Y'.

§ 385. We are led to suspect some error in the fundamental equation, if we consider a curious theorem in the doctrine of elimination [a] in the calculus which depends on this equation. Logical elimination so called means merely this. Given pro-

[a Some of this was suggested, I think, by Venn's *Symbolic Logic*, chs xiv and xv.]

positions involving conceptions such as x, y, and z, a given element is said to be eliminated when a proposition is found from the data which does not involve that element. Thus in the syllogism the middle term is eliminated. If all X is YZ, the proposition All X is Y is true and is said to eliminate Z. In the symbolic logic elimination is made very easy in the case of particular propositions by the principle that such propositions always imply the existence of their subjects, while universals do not. Thus, while All X is Y cannot guarantee the existence of any X, Some X is Y implies the real existence of some X and therefore of some Y also. Thus in Some X is YZ we can eliminate each element singly and each group of two thus.

some X exists,
some Y exists,
some XY exists, and so on

Now, at first sight, it seems odd that the merely indefinite subject, some X, should ensure the existence of X, while the definite, all X, cannot do this[a] And, if the universal X is Y must, for the reason stated, mean, If any X exists it is Y, then for exactly the same reason the particular, Some X is Y, should mean Some X, if it exists, is Y; or, Some X, if it were to exist, would be Y. Formal problems lead to such an hypothetical *some* just as they do to an hypothetical *all*. Take, for instance, All A is Y, all A is X: therefore, Some X, viz. AX, is Y. Here the some X meant is clearly hypothetical, since A is hypothetical It must mean: Some X, if it existed, would be Y. There must, then, be some strong reason for a doctrine which presents such obvious difficulties, and the fact is that the symbolists are committed to it as the necessary consequence of their view of the equation $XY' = 0$ The argument is.

Some X is not Y is the contradictory of the proposition All X is Y,

All X is Y is equivalent to No XY′ exists.

Therefore Some X is not-Y is the contradictory of No XY′ exists. But the contradictory of No XY′ exists is Some XY′ exists. Therefore Some X is not-Y is equivalent to Some XY′

[a In Dodgson's system All X is Y is considered to be equivalent to Some X (at least) is Y, and No X is Y′ The resolution is, however assumed, never justified]

exists, that is, Some really existent X is not-Y. For the affirmative we have to use the negative universal, which in this theory is No XY′ exists (= No X is Y′) or XY′ = 0. The contradictory of No XY′ exists is correctly stated; thus the argument must turn on the equivalence of No XY′ exists to All X is Y; and so if we suspect the conclusion, we must suspect this equivalence. Indeed we may reverse the argument and say the two propositions cannot be equivalent because their contradictories are not. For clearly, while the contradictory of No XY′ exists is Some XY′ exists, i e. Some really existent X is not Y, the contradictory of All X is Y (understood as equivalent to Any X, if it exists (or existed), is (or would be) Y) is Some X, if it existed, would not be Y. Or, to put it in another way, All X is Y is equivalent to All X, whether real or not, is Y, and the contradictory of this must be Some X, whether real or not, is not Y.

§ 386. Again the equivalence of the two propositions is disproved by the fact that the existential meaning which it necessitates for the particular as against the universal proposition can be shown to involve the theory in a contradiction. It may be premised that the existential proposition, No XY′ exists, must be understood in the wide sense, No XY′ can exist at any time; for, otherwise, it would be compatible with the contradictory of the proposition, all X is Y, which it is supposed to represent.

All A is B = If a thing is A, it is B . . . . (1)

No A is B = If a thing is A, it is not B . . . (2)

Therefore (1), if true, necessitates that (2) is untrue.

But (2), according to these logicians, = No AB exists, or No AB is possible, ... (3)

Therefore (1) necessitates the untruth of (3).

But the untruth of (3) necessitates the truth of its contradictory.

And the contradictory of (3) is 'some AB exists'.

Hence, 'all A is B' necessitates the proposition 'some AB exists', i. e. some A exists and is B.

Thus the universal affirmative necessarily implies that its subject is real, which is contrary to hypothesis This is a sufficient *reductio ad absurdum* The equation, then, XY′ = 0, if representing No XY′ can exist, is not the true equivalent of All X is Y.

§ 387. We shall now inquire what is true of XY'; whether any equation for All X is Y can be got from it; and the meaning and consequences of equating XY' to zero. If all X is Y, then XY' is YY'; that is, XY' is impossible, and has the particular form of impossibility YY'. If we take 'XY' = 0' merely to mean 'XY' is impossible' we certainly do not take the full inference from All X is Y, because we omit the particular sort of impossibility to which 'all X is Y' leads; accordingly we cannot have got a true equivalent for All X is Y. This is verified by the contradiction test. For, as in the case already considered, the contradictory of the two propositions is not the same. Indeed the proposition XY' is impossible is really in the same position as No XY can exist.

The want of equivalence is otherwise evident; for, given XY' is impossible, what follows is not that all X (*whether real or not*) is Y (the true equivalent of All X is Y), but the disjunctive, Either X is unreal, or Y' is unreal, or X, if real, is Y.

That the full inference All XY' is YY', is a true equivalent of All X is Y, can be verified by inference, or by taking the contradictory Thus:

(1) X is Y or Y', that is, is Y or XY',
but XY' is Y,
therefore all X (whether real or unreal) is Y

(2) The contradictory of 'XY' is YY'' is Some XY' is not Y'Y,
but all XY' must be Y'.
Therefore some XY' is not Y.

But this would be impossible if all X were Y, therefore some X is not Y; which is the contradictory of All X is Y.

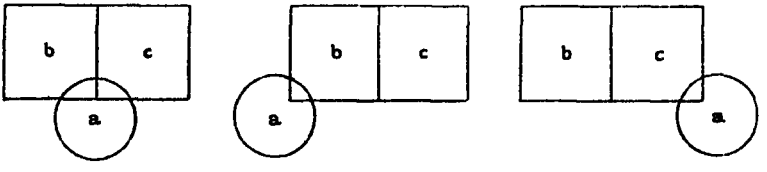
But this does not yield an equation. We cannot put XY' = YY' because XY' is not coincident with every form of YY'. The nearest we can get to an equational form is XY' = some YY', or XY' = vYY'. Thus, instead of improving on this and getting a more accurate form and one which corresponds to the determinate character of a true equation, we have only come back to it and with no improvement, for X = vY makes XY' = vYY' unnecessary. In view of these considerations it is of importance to observe that Venn,[1] after a sneering reference

[1] Venn, l. c, p. 403

to the venerable structure of the syllogism, continues: 'the distinction between universal and particular propositions which to it is unimportant is to us vital.'

§ 388. So far we have examined the *logic* which leads to the equation $XY' = 0$, and found that it contains serious errors. We will now consider the mathematical side. XY' represents, we have seen, an impossible class, and it is as such that it is equated to zero; the idea is that XY' being an impossible class there are no members in it, and so its extension is 0. Thus $XY' = 0$. Similarly, if A is B, $AB' = 0$. And clearly these equations will be very poor equations, indeed a mere sham, if it does not follow that $XY' = AB'$, whatever propositions XY and AB represent. The question therefore arises, what consequences have such equations? But strangely enough these equations, though the absolutely necessary result of an equational calculus, do not seem to have been formed or considered. The equation of impossibles as such to zero does not arise merely from the attempt to get an equation for the universal affirmative All X is Y, but is necessary everywhere to this calculus.

The distributive law in the association of class conceptions, or rather the limiting of one class by another, or taking the extension common to two classes, $a(b+c) = ab+ac$, corresponding to the distributive law of the operation of multiplication in algebra, depends entirely on the equation of impossibles to zero.



§ 389. The common part of the class *a* and the class formed by putting together *b* and *c*, supposing there is any common part, will certainly include *either ab or ac*, and it may include both, that is, it will have a common part with *b* or *c*, or both, but it *may* have no common part with one of them. It is possible, for instance, that there may be nothing common to *a* and *c* and, therefore, no class *ac* at all. Then *c* is *a'*. Suppose

$c = a'd$, then $ac = aa'd$. But the formula for $a(b+c)$, although this is intended for a real class, gives always $a(b+c) = ab+ac$; therefore $a(b+c) = ab+aa'd$, even when ac is an impossible class. Thus the equation can only be maintained by equating the impossible class ac or $aa'd$ to zero; then and then only the right result, $a(b+c) = ab$, is obtained. The same is true of all the equational forms which attempt to represent dichotomy. The fundamental one is $x+x' = 1$ (= all reality). Now, if x is real, x' (or not-x) includes not only the part of reality which is not-x, but all unrealities or all impossible classes Thus the equation would falsely make reality include unrealities, and this can only be prevented by equating the unreal part of x' to zero.

Similarly the equation $a = ax+ax'$, which may be regarded as an application of the above, is given as always true But if a is real (and even though x and x' are restricted to realities), one of the classes on the right-hand side of the equation may be unreal For instance, if all a is x, then ax' is xx' and ax' is impossible. The equation, then, is only saved by equating ax' to zero, when we get $a = ax$, which is true.

All impossibilities, then, are equated to zero. Thus, if A is B and X is Y,

$$AB' = 0,\ XY' = 0,\ AA' = 0,\ BB' = 0,\ XX' = 0,\ YY' = 0,$$

and so on.

In this equating of the impossible to zero, two points are to be noticed and distinguished. First, all impossibilities are equated to the same, which has the effect of equating them to one another a result which equally follows if they are not equated to zero, provided they are equated to the same thing. Secondly, each impossibility is equated to zero

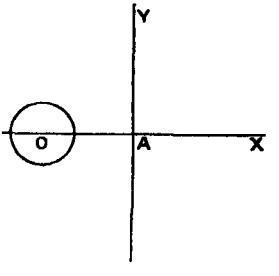
§ 390 In algebra both these statements would be considered fallacies and elementary fallacies. Familiar examples, which illustrate the principle involved in the equation of all impossibles to zero, are such as the following. Every person in the next room can prove that the diagonal of the square is commensurable with the side. This statement is supposed by some to be fully justified because there are no persons in the next room. Again, every person who can marry a person who lived a thousand years ago can prove the diagonal of a square to be commensurate

with its side. This is justified by saying that there are no persons in either category.

The justification is a fallacy, and it is partly on this fallacy, we may remark incidentally, that the modern form of the doctrine of the reduction of the categorical to the hypothetical proposition depends.[1] Moreover, the most brilliant and striking part of modern mathematics, the treatment of so-called imaginary quantities, depends on a principle which makes it a fallacy. In algebra the proof that impossibilities can neither be equated to one another nor to zero is easily given. Probably most, if not all, of us, when we first arrived at a result of the form $x^2+1=0$, therefore $x^2=-1$, if we avoided the mistake of putting $x=-1$, and saw x to be impossible because the equation makes x equal to the square root of -1, rightly said that no quantity would satisfy the equation and then wrongly translated this into $x=0$. The mistake is corrected by observing that $x=0$ will not satisfy the given equation For, put $x=0$ and we get the absurdity $0=1$.

Again the equation of impossibles to one another is easily disproved For suppose a and b real quantities and unequal. If $\sqrt{-1}\,a=\sqrt{-1}\,b$, because both are impossibles, then if we operate on each side by the inverse operation of the root, that is, if we square them, we get $-a=-b$, and therefore a and b are equal. Thus the equation of impossibles or imaginaries to one another involves the equation of all real quantities to one another.

§ 391.[a] If at first this simple operation of squaring seems suspicious, we have a better illustration in a more concrete form in the application of algebra to geometry. Take a point O in a straight line and, at a given distance a, draw another straight line at right angles to the line From the given point as centre describe a circle of radius less than a and therefore not cutting the straight line AY. If we combine the equation of



[1] Cf § 400.

[a] Cf § 116]

the circle with that of the straight line we find, for the distances of the intersection of the two from A, two impossible quantities, that is, reals affected with the sign $\sqrt{-1}$. If we describe another concentric circle also not cutting AY, we get for its intersections with AY two other impossible quantities, different from the first in form, depending on the difference of radius. Two different sets of impossibilities thus correspond to the different equations of the two circles, which really means that the impossibility of one's cutting the line is different from the impossibility of the other's cutting the line. If now we identify the two impossibilities, we identify the equations of the two circles and get the absurd result that all circles with the same centre O which do not meet AY coincide with one another.

That the equation of impossibles to one another would imply the equation of unequal realities to one another, and indeed the equation of all realities to one another, is a thing we should anticipate on quite general grounds. For an impossibility is only the other side of a necessity; every different necessity gives rise to a different impossibility And so there is nothing mysterious in the treatment by mathematicians of impossibilities as definite, not as mere nothings merged in an indistinguishable zero. Thus, if we identify different impossibilities with one another, we shall expect to find that we have thereby identified the corresponding necessities with one another.[1]

§ 392.[a] Professional, and indeed distinguished, mathematicians, no mere amateurs, have invented this system of symbolic logic, and so it may seem strange that they should have allowed the equation of each impossible to zero; but perhaps there were these reasons for it. First, the equational form cannot be maintained without it. Secondly, by the help of it, an equational system is produced which is coherent within its own limits, just as in algebra, if we equate the unreal roots of equations to zero, we shall find the real roots correctly enough, and may avoid the contradiction resulting from the equation of unreals to zero by refusing to consider unreals any further. In this way, however, algebra would be cut down to very narrow

[1] Cf § 400.

[a] On Symbolism in this connexion see Venn, l c, ch. iv]

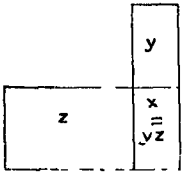
limits, and we should lose the most important part of it. Thirdly, it was probably thought that the equation of impossibles to zero would have no further consequences like those in algebra and therefore nothing would be lost and no contradiction caused. And that for two reasons. First, the equations which usually occur in this calculus do not present forms in which the equation of an impossible to zero would cause the equation of a real to zero, as in the algebraical equation $x^2 + 1 = 0$. Secondly, it probably seemed, if the experiment of equating two impossibles to one another was tried at all, that nothing but nugatory or identical predication could result.

§ 393. However, the experiment may be tried of constructing an equation, according to the rules of the calculus, which is parallel to $x^2 + 1 = 0$ and leads to the same sort of contradiction, if impossibility is made identical with zero. Thus we might show that, in this calculus too, the equation of impossibles to one another results in equating all real classes to one another. We might also attempt to show the same contradiction by a proof analogous to the geometrical *reductio ad absurdum* in regard to the concentric circles, none of which cut a given line. To take first a proof of the second kind. Consider the equation $aa' = bb'$. According to the rules of the calculus this necessitates all aa' is b, which at first seems to yield nothing but identities. We shall get something else out of it if we inquire how impossibles such as aa' can get predicates at all. It must be remembered that in this calculus they do have predicates assigned to them—in proofs by *reductio ad absurdum*—and it is in consequence of these predicates leading to a combination of contradictories that we discover that they *are* impossibles.

In such indirect proofs in geometry, in order to prove that A is not B, we assume that AB is a possible combination, AB being really impossible, though the separate elements in it are possible. We then argue further, not from the combination of impossibles as a combination, for that we cannot do, but by finding (or having given) predicates of the real elements in it. We then predicate of the problematical whole AB all the predicates which belong to its parts, and the argument ends when, among the predicates combined, contradictories are found. Thus the impossible combination AB only acquires predicates

through the fact that they attach to elements in AB. Assuming this, it can be shown that if we have an impossible, as in the present case, with the form aa' and a positive predicate b, b must be due to a alone. Of several proofs which may be given, take the following. The conception a' as merely negative is wholly indeterminate; it includes all kinds of conceptions entirely irrelevant to one another, the infinity of what is not-a. Thus its content cannot be unified by any positive conception, its only unity is the negation of a. a' therefore cannot necessitate b, for then it would be unified by a positive conception. Similarly a' cannot be resolved into the association of two positives (e. g. $a' = xy$), nor of a positive and a negative (e. g $a' = xy'$), so that it might include a positive element from which it could be derived, or from which, in conjunction with a, b could be derived. Thus, if aa' is b, all a must be b, and b be due to the positive element a alone Now whatever a and b may be, $aa' = 0 = bb'$ according to this calculus; thus aa' is b, and therefore, by the above, all a is b, whatever a and b may be Thus the equation of all impossibles to one another necessitates the equation of all reals to one another.

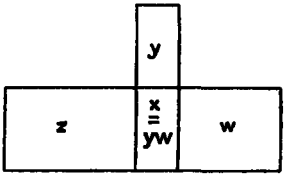
§ 394. The proof parallel to that derived from $x^2 + 1 = 0$, that the equation of an impossibility to zero necessitates the equation of a reality to zero, requires the use of the symbol $\frac{x}{y}$, which must now be explained. $\frac{x}{y}$ (according to mathematical analogy) represents the inverse operation of xy. It is explained most simply thus:



Let $zy = x$, then z is a class such that when restricted by y it reduces to x. If, then, we write z is $\frac{x}{y}$, we get this meaning for $\frac{x}{y}$, that it represents any class which being restricted by y reduces to x.

Thus, if we have given that z is $\frac{x}{y}$, it follows that $zy = x$, in fact we may multiply as in algebra. Observe, however, that such an equation may be satisfied by more classes than a single

given one as z, e. g. by w as in the figure. Thus from $zy = x$, we cannot infer $z = \frac{x}{y}$ but that z is one of the solutions of $\frac{x}{y}$ (w, e. g., being another). Thus, though we can multiply, we cannot divide. We can, however, infer from $zy = x$,



that $$z = v\,\frac{x}{y}.$$

Now suppose the equation given

$$cx = \frac{a}{b} \quad . \quad . \quad . \quad . \quad (1)$$

Where a, b, c are real classes and x is to be determined by the equation. Then we have $bcx = a$ . . . . . (2)

and $$x = v\,\frac{a}{bc} \quad . \quad . \quad . \quad . \quad (3)$$

Suppose also the equation given

$$c = b' \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Then $$b'x = \frac{a}{b} \quad . \quad . \quad . \text{ from } (1)$$

and $$bb'x = a \quad . \quad . \quad . \text{ from } (2)$$

where bb' is an impossible class.

In the equation $x^2 + 1 = 0$, or $x^2 = -1$, though x is unreal we have not the equation of a real quantity to an unreal, for the square of x is not unreal.

$$((-1)^{\frac{1}{2}})^2 = -1$$

The impossible operation on -1, viz. $(-1)^{\frac{1}{2}}$, is removed by its inverse $(\ )^2$, thus leaving -1 where it was, that is, as -1. The same is true of $bb'x = a$; we have not got the equation of an impossible to a real, for (by the above)

$$x = v\,\frac{a}{bb'}$$

so that the equation is $bb'v\,\frac{a}{bb'} = a$

where the impossible operation $\frac{1}{bb'}$ is cancelled by its inverse bb', leaving a on the left-hand side of the equation. Now therefore the equation $bb'x = a$ arises necessarily out of the operation

of this calculus just as $x^2+1=0$ arises out of the operation of algebra.

x is an impossible, equate it to zero by the rule of this calculus, $\therefore 0 = a$. A real class a is part of unreality. And we have the required contradiction.

§ 395. Again, with the help of this calculus, we can prove that all classes are predicable of one another. Suppose A and C to be two classes outside B.

Then A is B′ and C is B′.

The combination of A with B produces the impossible class ABB′.

Conversely also, the class which, by combination with B, produces the impossible class ABB′ must be AB′ (since A is not contained in B).

But ABB′ = 0 = CBB′.

Thus, whatever is necessary to produce CBB′ by combination with B is necessary to produce ABB′.

But CB′ is necessary to produce CBB′ in combination with B, therefore AB′ and CB′ must be the same class, but AB′ = A and CB′ = C.

Therefore A and C must be the same class.

Thus all A is C and all C is A.

Hence, all classes outside B coincide with one another, and as B may be any class whatever, it follows that all classes are predicable of one another.

§ 396. Now that the symbol $\frac{x}{y}$ has been explained, one can show why the simple proof in mathematics, that the identification of impossibilities is an identification of realities, from the equation $\sqrt{-1}\,a = \sqrt{-1}\,b$, cannot be repeated simply in the calculus.

The analogue would be $aa'b = aa'c = 0$.

If we could perform the inverse operation $(aa')^{-1}$ on both sides of the equation, as we do in the above algebraic equation, we could prove that $a = b$.

But we can't divide by aa', all we can say is b is $\frac{aa'c}{aa'}$. That is to say, b is *one* solution of $\frac{aa'c}{aa'}$, and c is obviously another

solution, but we can't say that they coincide and therefore that $b = c$.

This is perhaps a further explanation of the fact that it was not seen that the equation of impossibles to one another would have similar effects to those produced in algebra. But of course the difficulty will suggest itself, that the calculus must have, nevertheless, led to right results; for, if it only led to contradictions, it could not have made its way at all, or have imposed on any logician or mathematician. Those who have studied it, all know that it does get *something* right.

It may be replied that one department of it at least, which is put forward as of undoubted truth, has been shown, if the above arguments are correct, to be entirely wrong, i. e. the theory of elimination from particular propositions and all that depends upon that theory. What, then, of the mistake in the operations analogous to those on what mathematicians call imaginary quantities? In algebra a certain amount of truth would be got, even if we treated all such quantities as zero. So generally, in reasoning, we may have right conclusions from wrong premisses. One case of the kind arises where a premiss includes a truth and a falsehood, and we argue only from the truth, e. g. A is BC and C is D. Suppose it to be true that A is C but not BC, then the argument all C is D, A is BC, therefore A is D, is sound. I shall try to show that the same holds in this equational logic.

§ 397. Consider the treatment of the ordinary syllogism all x is B, all A is x,

therefore, all A is B.

All A is x, or $Ax' = 0$ . . . (i); all x is B, or $xB' = 0$ . . . (ii),

therefore, $AB'x' = 0$ and $AB'x = 0$,

therefore, $AB'x' + AB'x = 0$

$$AB'(x' + x) = 0,$$

therefore, $AB' = 0$, therefore, all A is B.

As compared with the syllogism, the process is recommended neither by shortness nor simplicity. But it is also erroneous, only the mistakes happen not to influence the conclusion. We must see how this is. First of all, the reasoning, which corresponds to the above process and is incorrectly symbolized by the equation, is as follows:

Since all A is x,

Ax' is impossible.

Similarly xB' is impossible;

Since Ax' is impossible,

$AB'x'$ is impossible;

Since $B'x$ is impossible,

$AB'x$ is impossible;

Now everything is either x or x',

therefore, AB' is either $AB'x$ or $AB'x'$.

But, by the above, in either case AB' is impossible. Therefore AB' is impossible, therefore A (if possible) is B Even in the reasoning as thus put there is a defect (with which, however, we are not so much concerned at this stage), for the full conclusion is 'A, whether possible or not, is B'.

Take the steps of the calculus which correspond to this reasoning. The equation $Ax' = 0$ is really founded on Ax' is impossible, a proposition involving predication. If now $Ax' = 0$ and $xB' = 0$ are taken accurately, as they should be in the calculus, then from the other operations and equations we get $AB'x + AB'x' = AB' = 0$. From these the only true inference is that either A or B', or both, are zero; and so in the original equations $Ax' = 0$ and $xB' = 0$, A or x' must be zero. Thus A or B' would be neither an impossible class nor any real class (unless 0 is formally real). The only possible escape is to take zero, not as it should be in an accurate calculus but as a symbol of impossibility. Then the equation $Ax' = 0$ involves first the true statement, Ax' is impossible, a mere predication, together with the false statement Ax' coincides with every form of impossibility. Of these only the first, the true one, is used in the argument; the second, the untrue one, is not used and so causes no error.

§ 398. Next consider the step involved in the addition of the equation $AB'x + AB'x' = 0$ and the all-important equation $AB'x + AB'x' = AB'$ associated with it. The equation of dichotomy, $a = ax + ax'$, involves, in general, the equation of the impossible to zero, to justify it, and is so far incorrect. The truth which corresponds to it is the disjunctive statement, a is either x or x', and there are cases where only the true part of the equation affects the conclusion and so the conclusion is correct.

Suppose that, whether ax and ax' are imaginary (impossible) or not, we have given (or can prove) that they have a common predicate, ax is y and ax' is y.

The disjunctive statement above gives at once the conclusion that all a is y. The equation gives

$$a = ax' + ax,$$

$$\text{therefore, } a = (ax' + ax)y,$$

and, if we take only the result, $a = vy$ (i. e. a = some y), which is included in and is a part of the above equation, we get a right conclusion.

That is as much as to say that we use only the hypothetical or disjunctive statement which the equation includes and neglect the erroneous addition to the statement which is necessary to make the equation. This is exactly what is done in the present case. The equation $AB' = AB'x + AB'x'$ is incorrect, but as we can attach a common predicate to $AB'x + AB'x'$ and only infer this of AB', the result is correct. The common predicate is 'impossible', and corresponds to y in the above.

§ 399. The result, then, is this:

All the equations, both those which represent the premisses and the equation of dichotomy, involve a simple predicational form. To this they add another element through which they become equations. The argument we have just been through is sound and depends solely upon the predicational element. Thus the equational element is not employed at all; that element is not only surplusage but incorrect and, if developed, leads to contradiction and to the destruction of the calculus altogether The predicational element is correct, and the argument is sound because it depends upon this alone and the erroneous part, the equational, is in no way involved. The equational form, then, is a mere sham; to get at the truth we must strip it away and fall back on the simple form of predication, and what we strip away is not only useless but erroneous.

§ 400. Two remarks may be made by way of conclusion. Modern logicians (in the true sense) often suppose that, because in the categorical universal statement, all A is B, we may not be sure of A's existence, all A is B may be better expressed by its supposed equivalent, if anything is A, it is B.[1] But this

[1] Cf § 390. [Part II, ch. 11; Part III, ch 6]

latter form does not include the statement, if anything unreal is A, it is B, whereas all A is B does include this. In mathematics the importance of the difference between the categorical and hypothetical statements is essential and is obvious, and the identification of the two is seen to be merely an error.

Finally, the statement made above,[1] that the equation of impossibilities is connected with the equation of the necessities, of which they are but the other side, may have led to an expectation that I should have proved that from the reduction of all x is y to $xy' = 0$, and all a is b to $ab' = 0$, it could be shown that the equation of the propositions x is y and a is b to one another is involved. This is true no doubt so far, that it involves the result that, if x is y, then a is b; and, if a is b, then x is y; that is, generally, that every true statement would involve every other. But for reasons obvious to every metaphysician this would not supply an incontrovertible objection to the theory, or at least not one to which no one could object.

[NOTE. Wilson left eight manuscript volumes of close examination of the symbolic logics, viz. those of Boole and Venn, which he had studied. Naturally a part of these volumes is devoted to a criticism of Boole's inquiry into the logic of Probability. The main points which are supplemental to the preceding sections are these:

The treatment of the principal equation of symbolic logic,

$$z = x + y$$

is even more useless than that of the equation of dichotomy; for complicated processes are applied to it which give as their result the propositions z is either x or y, x is z, y is z. But these must be known *before* we can form the equation in question and are the sole means by which in actual thinking we form the equation at all.

Again, certain famous elimination and expansion theorems of Boole[2] involve quite fatal mistakes from either a mathematical or a symbolical standpoint. Such a theorem is, that if any class, however formed, be represented by $f(x)$,

then $$f(x) = f(1)x + f(0)\,(1 - x).$$[3]

[1] § 391. [2] See *Laws of Thought*, ch 5

[3] In Boole's system $(1-x)$ is equivalent to the symbol x', used above

Thus this formula gives, if applied to $z = \frac{x}{y}$,

$$\frac{1}{1}xy + \frac{0}{1}x'y + \frac{1}{0}xy' + \frac{0}{0}x'y'.$$

But if $z = \frac{x}{y}$, $z = x + vy'$, $z = x(y) + vy'(x')$.

Thus $\frac{0}{0} = v$, if the calculus is right. This important discovery was supposed by a strange confusion of ideas to be confirmed by the mathematical meaning of the symbol. The principle adopted is that these coefficients as they stand represent logical classes, just as $\frac{x}{y}$ is supposed to do. If $\frac{x}{y}$ represents a class $= z$, we have $zy = x$,

therefore, if $z = \frac{0}{0}$, $z \cdot 0 = 0$.

z, then, is such a class that, if operated on by zero, it yields zero. Now any class will do this Thus z may be any class. The inference Venn drew from this is, '$\frac{0}{0}$ stands for any logical class whatever; that is, *it is perfectly indefinite*'. To this Wilson of course replied that it does not stand doubtfully for some or other, we do not know which, it stands for every one indifferently and so for all. So he made great fun of Venn's symbol $\frac{1}{0}$, which attempts symbolically to represent a class such that, when we take none of it, we yet obtain all. As usual he tried to penetrate to the true cause of these contradictions. They arise not because the coefficients are wrong but from a wrong view of the coefficients themselves. The confusion arises, he believed, from forgetting a principle in the substitution of definite values for variables in a given function. In $\phi(xy)$ we can substitute at will, if x and y are independent; but if there is a law connecting x and y, or limiting the range of their independence, we cannot so substitute and still retain the meaning of the function. Thus in $\frac{x}{y}$, the form of the function prescribes that all x is y. Then $\phi(01)$, or $\phi(10)$, is an impossible substitution, if 01 or 10 is to be a class. Thus, if such a meaning

is to be given to the coefficient, the expansion formula cannot be applied.

Much care was devoted by Wilson to showing that the calculus is not in the least adapted to the solution of such difficult formal problems as are mentioned above. On the contrary it is painfully clumsy and long, even in the solution of fairly simple problems. It solves problems of its own creation, but even those can be done quicker and better without any calculus at all, by a method involving only Barbara and immediate inference. Wilson spent much time on the invention of such a simple method, as well as on a philosophic toy of his own, as he called it, a fractional method, and it was characteristic of him that he should have spent so much time trying to beat the symbolists at their own game. His little book *On the Traversing of Geometrical Figures* arose out of notions suggested by these studies, and so did his arduous attempts to prove a theorem, which is connected with disjunctive reasoning, the famous four-colour theorem]

PART V

TENTATIVE INVESTIGATIONS AND PHILOSOPHICAL CORRESPONDENCE

I

THE NATURE OF A 'THING'

§ 401. The 'thing' we treat as an identity in the multiplicity of its attributes and in the variety of the attributes which belong to it at different times. We say that it is hard, heavy, round, implying that the same thing is these attributes; we say that it is now something and at another time something different, and treat it as the same in the differences. The events in time which thus belong to one thing are unified because one identical thing is present in them all. They do not seem to belong to the thing as a mere unity of themselves. Nor can we say that they belong to their unity with one another, they cannot be thought save as belonging to something other than themselves. This is obvious in the case of the various kinds of being which a thing has at different times. But is it true also of the thing in relation to the attributes it has at one time, or with regard to those which it always has, its permanent as opposed to its variable being? Is the thing merely the unity of its permanent attributes, and is that the unity to which the variable attributes are to be assigned? If this body is hard and heavy, we cannot say that the unity of extension, hardness and heaviness (the body being extended, hard, heavy), is the hardness or has the hardness, that would be absurd. In our statements the thing is said to be something that is, to have the kind of being to which the attribute refers. The thing is hard, not hardness. But we can't say that this hardness and extension and heaviness

[§§ 401–59. These sections are a very imperfect attempt of mine to present various important investigations of Wilson's as fairly and as clearly as I could on the basis of manuscript notes. §§ 406–21 and 451–9 are part of an essay which he was working upon for the British Academy, called 'A Grammar of Universals.' §§ 422–32 are critical of Mr. Bertrand Russell's logical views. The criticism is given otherwise, in some respects, in §§ 477–518. §§ 433–7 are a criticism of a chapter in F. H. Bradley's *Appearance and Reality*. §§ 438–50 are condensed from Wilson's manuscript and give his views on an interesting and vexed question in the history of logic.]

is hard, nor that the unity of them is hard, nor that they in their unity are hard. Further, we say the hardness is hardness of the thing, the extension is extension of the thing. But hardness is not hardness of weight and extension and hardness, nor the hardness of the unity of these three.

It may be objected that if we say the thing is hard, &c., the words mean simply that hardness is an element in a unity of itself and other elements, or that the unity of (this) extension and (this) weight is also a unity of them with hardness. This has been disproved above. Moreover, the language we use is not naturally interpreted so. It is chosen just to express that there is something definite which is extended and heavy and also hard; not that there are three abstract terms, or something which has those terms (hardness, &c.), unified. Nor is it a mere matter of language. The thing is not a unity of the three terms as general abstract terms; so far as it is a unity of them it is of *this* hardness, &c Now this hardness is hardness of something; thus each attribute necessarily implies something of which it is attribute, and this cannot be another attribute nor the mere unity of itself with other attributes. The attributes are not beings, like the parts of a machine, which by their systematic unity form another being. They have no being apart from the thing to which they are attributed. Hardness only exists as this thing's being hard. On the other hand they are ways in which the thing exists and the thing only *is* in these ways. If we exclude its temporal variations for simplicity, everything we can say that it is implies *it* as somehow different from what that, which is attributed to it, is. What, then, is that identity in the thing which is said to have these forms of being, hardness, &c, and which is the same in their difference? If we call the identity x, any definite answer of the form x is a is excluded; for 'a' will be an attribute, something not the identity which nevertheless the identity is said to be. We thus have not merely a problem we don't know how to solve, but the attempt to solve it necessarily involves a contradiction. We seem in fact to ask for an answer in a certain form, whereas the form cannot be a form of such answer.

§ 402. Observe that the view which we arrived at about the identity of the thing was not the result of any logical or meta-

physical theory, it was the necessary result of reflection on the presuppositions of our ordinary language and thought about the thing. That is why the identity (sometimes called substance) appears so early in philosophy, in Aristotle in fact. We may get help by observing what those, who suppose substance to be unintelligible, find to be intelligible and not at all difficult, in contrast with it No difficulty is felt about the attributes. So much so that an answer given to the present difficulty is that there is no such thing as a substance apart from attributes and that the so-called substance is nothing but the aggregate of its attributes But the attribute is quite unintelligible save as referred to the thing or identity or substance, which has it. The reference is essential to its nature. Hardness is hardness of something. Thus the intelligibility of the attribute presupposes that of the substance; though it is equally true that substance is only intelligible in relation to the attributes, as that which has them

§ 403. So far, then, it seems that we cannot speak of the being of substance as apart from that of the attributes, nor conversely. Being must include these two sides in their unity, is only intelligible as including them both We can't say without contradiction that substance or attribute is or has being *simpliciter*, we must say substance is something, the something being the attribute. Being, then, is not a simple notion, substance and attribute are inseparable elements of it and neither of them is *a* being; just as surface and volume are neither of them a solid, though they are elements which constitute a solid Now this general view of being may seem to suit a self or individual mind, but presents difficulties in regard to material things. The latter are what seem to ordinary reflection the most obvious instances of things, as individual and independent unities.

Take for clearness a thing A which is a continuous solid. Its extension would have, according to the above view, to belong to some indivisible, identical unity and could not be separated from it, or belong to a different unity, any more than one person's feeling can belong to another person Now suppose the solid broken into two pieces, B and C. Each is as much a thing as the original unbroken solid. Thus the extensions of B and C would belong to two different unities, to two different

substances, whereas those extensions belonged at first to the one substance A, to its unity and identity. This seems impossible. Are we, then, to suppose B and C to be still extensions of one and the same substance? If so, all extended bodies would be extensions of one and the same substance. Now there is a sense in which all extended bodies are unified by some common reality, as appears in considering the presupposition of their mutual relations, action, interaction, &c. Yet each has its separate unity, and the separate unity of B would be that to which the extension (extended matter) of B belongs, and similarly for C. The difficulty might be avoided if we could maintain that the really continuous solid could not be broken, any more than one self (notwithstanding the nonsense talked about multiple personality) can be divided into two or more selves. Breaking, then, would mean separation of the contiguous, not the continuous, and all fracture would imply the atomic constitution of bodies, the atoms themselves being unbreakable. (Notice that if we say *a* thing is extended, it must have parts outside one another; unity therefore cannot be arrived at by mere diminution of extension) Suppose now that there are perfectly continuous solids which can be broken. Such solids, as unbroken, are one by their continuity throughout, and such absolute continuity, not mere contiguity, is presupposed in any account of matter. Those theories which reduce an apparently continuous solid to a system of atoms assume the absolute continuity of the small volume of the atom, and the idea of extended matter itself necessitates this assumption. For, if possible, suppose a piece of matter continually divided until pieces are arrived at which are not continuous; this would mean that no part of matter has a finite extension though it has position. That would be the exploded absurdity of making matter consist of points and giving a point a separate existence. A point can only exist as a boundary of a continuous line, a line of a surface, and a surface of a continuous solid or of continuous matter. A continuous piece of matter, then, is one by its absolute continuity; as Aristotle would express it, it is one thing in actuality and, as capable of being broken, potentially several things.

Consider now a part of the thing which might be broken off but is not. Is this as little a unity with the remainder as if it

were broken off actually? The answer must be, No! because it exists only as a part of a continuous whole. It is unified with any other part because each part exists, not separately, but only as a part of the continuous whole of which the other is a part. Imagine a geometrical plane within such an absolutely continuous solid, dividing it into two parts geometrically. The matter on either side of the plane is not contiguous or adjacent to, but continuous with the matter on the other side Thus the unity of the one thing, which is one piece of solid matter, is the unity of the continuity of its parts, the parts having no separate existence Thus the extended thing is not mere extension, something is extended, and that something we call matter; the continuously extended piece of matter is one thing.

The question, then, what this is which is extended and not mere extension, which we call matter, is one more of those unreal questions which, in philosophy, have no answer just because they are not real questions That which we name matter is *sui generis*, and the question asked about it is false because it presupposes, probably unconsciously, that an explanation is wanted of it in terms of something else But of course it is not anything else; we might just as profitably ask about sensation what it is that is felt as sensation!

§ 404 Given now one continuous solid A, consider how as one it is said to be so and so, to have attributes Of A we certainly say it is heavy, the pronoun it referring to a unity. Are we justified in saying that as one it is heavy? The only possible objection would seem to be that it has not one weight as one but that that which we call its weight is the sum of the weights of its parts, each of which has weight. But the same would apply to each of its parts, and so the weight of any one part would not be one of the ultimate constituents of the aggregate Thus no finite extended matter, however small, could have *a* weight and the ultimate units would have to be the weight of points. That the weight of the various parts of the continuous solid are unified in one weight, as the parts of a divided aggregate are not, is therefore obvious. The weight of a part can never act independently of the weight of any other part. Its effect can never appear except as a part of the effect of the whole weight, owing to the continuous connexion of the parts.

Thus, what makes the piece of matter one thing makes it have one weight. On the other hand, it is not true that the weight of a piece of matter, which is part of an aggregate or heap of pieces, can only have an effect which is a part of the whole weight of the aggregate. The one solid, one as continuous, has one weight in the truest sense, because the weights of the parts only exist as weights of the parts of the whole and therefore only as parts of the weight of the whole. The same argument holds for other attributes of a continuous solid.

§ 405. The case of a fluid causes considerable difficulties. In mathematical physics a fluid is treated as a system of particles or atoms. If these atoms are considered as solid the argument of the previous section would apply to them. But the theory of fluids is not clear and definitely thought out. The continuity of the fluid is repeatedly spoken of, yet on such an atomistic theory continuity could at most mean contiguity, and not even that literally, because the atoms are not supposed to be of a shape to fill space continuously. Thus nothing can be made of the mathematical theory as it stands, because the equations of hydro-mechanics are not compatible with any atomistic hypothesis but presuppose that the fluid is absolutely continuous Now such continuity must either mean that the fluid has no parts actually separate and merely contiguous, or that it has parts separate and actually contiguous, these parts themselves having parts not separate, so that these parts are really extended continuously in three dimensions, or thirdly that there is only contiguity of separate parts, all parts being really separate from all others. The hypothesis of a perfect fluid can be shown to imply necessarily that no force is necessary to separate the parts. The third alternative is therefore the only possible one, and that is absurd. Thus a perfect fluid as continuous is impossible, and yet the mathematical equations of fluid motion are compatible only with perfect continuity.

II

THE UNIVERSAL AND ITS DIFFERENTIATIONS

§ 406. ACCORDING to the natural use of words, universal would mean that which is identical in a number of particulars and is thereby distinguished from the diversity in which it is identical. Nor is it merely identical but something definite which is identical in them I have called this distinctive character the quality or the characteristic being of the universal. The universal so understood as genus takes of its own nature specific forms or species; linearity is either rectilinearity or curvilinearity, colour is blue, red, &c. It is identical in the universals which are its species, they are nothing outside it but belong to its own nature, oddness and evenness are included in the nature of number The aggregate of the species belongs to the nature of the genus and might seem to constitute its full nature, so Aristotle says the genus is nothing but the species [a] This is, however, impossible, for the genus is identical in its forms and their aggregate cannot be something identical in each of them. We cannot say the genus is each of its specific forms, or any or all of them together, because *is* in such a statement naturally means 'is identical with'

Again the universal cannot be except as an identity in the particulars Of its own nature it necessitates the particulars as that in which it must be identical The particulars belong to its own nature and are nothing apart from it. Yet here again we cannot, any more than in the case of the universals which are species, identify any one of them or their aggregate with the universal. The complete reality of the system of particulars has distinguishable within it the universal and its particularization. The complete system of particulars may be infinite and

[a This seems to refer to a tentative remark at *Met* 998b 28 For Aristotle's real view see *Physics*, 210a 17–19, and 'Further the members of the definition which is explanatory of the individual are parts of a whole Thus the genus is part of the species and in another sense the species of the genus' *Met.* 1023b 23]

may not be realized all at one time, but in any particular whatever the universal and its particularization are united.

§ 407. Thus the class is not accurately called the complete reality of the universality, but rather the complete reality which the nature of the universal necessitates. The saying that there is nothing in the species but the genus is not accurate in form. It is only true so far as the species is necessitated by the genus's own nature A truer statement would be that there is nothing in the specific universal (e. g evenness and oddness) except the differentiation which belongs to the nature of the generic universal (e. g number)

Observe, however, that if the nature of A necessitates B, A and B are by implication different. A cannot be identified with B, and it may be that B cannot be called a part of A, though necessitating B may be called a part of A's nature. For example, though surfaceness necessitates volume, we cannot say that there is nothing in volume but surfaceness The latter, however, does not offer an adequate analogy to the relation we are considering. The necessitation through which the particular appears as having its nature comprised in that of the universal has no analogy, it is a unique relation between necessitating and necessitated. We cannot say anything of the particular (individual) which is not universal, except the mere 'this' and 'that'. On the other hand, not to rest the matter on an appeal to language (important though such an appeal is), what we apprehend in the particulars is the universal which is their distinctive kind of being and which is in individuals of absolutely the same universal, besides that universal we apprehend only the one and the other of the same kind.

§ 408. But to return to the unity of the genus in the species. The difference here between the species cannot be reduced to a mere this and that. Straightness is a kind of being, not a mere this linearity; straightness is a form of linearity, but linearity is not a form of straightness. They are inseparably united in the same real individual, but not in the way in which, for instance, surface and volume are united. These two are equally determinate, linearity and straightness are not, the latter being more determinate. Thus the unique idea of determination is necessary to characterize what is meant. The genus is identical

in its determinations, but not identical with them. We understand this by examples and can say nothing truer or better of the specific universals than that they are determinations of the genus, for to say they are the forms it takes is to employ a mere metaphor If, now, we include under determination differentiation of species and particularization in individuals, the determination of the universal is completed in individuals, which themselves involve its determination in species.

§ 409. The term necessitation is not adequate to express the relation of the universal to the manifold of species and of individuals. For while it is true that linearity necessitates its kinds, it is also true that straightness presupposes and so necessitates linearity, and through the latter necessitates curvilinearity So a particular line necessitates linearity and through that all particular lines. On the other hand, the whole being of linearity is not to be a kind of surfaceness, though these two necessitate one another Thus the complete reality relative to any universal is necessitated either by that universal or by any one of its species or of its individuals What has to be taken into account is the particular kind of necessitation The whole being of curvilinearity is, in one sense, comprised in that of the universal linearity, in the sense that its whole being is to be a kind of linearity; but, in another, is not, in the sense of being a part of it or an element in it, otherwise as linearity is identical in its two species, so must the latter be Part of, element of, do not express the relation, whereas kind of, species of, differentiation of, do express it, for it is a relation *sui generis*. So the whole being of a particular line is to be a particular, not a part, of linearity. Again the term 'comprise' is itself too general, for the being of linearity can be said to be comprised in that of surfaceness. Nor can we use *include* in its natural sense, for though the genus seems to include the species and the particulars, what is included in its nature seems necessarily to be in it wherever it is, and so, if we said the genus 'includes' them, we should again have one species in another species and one individual in another. The point is that the species have not only got something identical in them but that whatever else they have somehow belongs to this identical something. Redness, in fact, agrees with blueness in that in which it differs

from blueness. It has colourness in common with it, but it also differs from it in colour and in nothing else. Colourness inevitably covers the whole nature of redness and of blueness.

We must therefore examine the language in which we express this relation, to see what it is which makes us feel that somehow the nature of the generic universal (for instance, colour) covers and includes the whole nature of the species.

§ 410. In ordinary unphilosophical language we say a line must be either straight or curved. This really expresses a connexion of universals. Can we put this technically in the disjunctive form, linearity is either straightness or curvilinearity? Now if we say A is either B or C, when A is not a universal, we mean that A is B or that A is C. This again would be true if A is B and not C, or C and not B, or if A is B and A is C. But we cannot substitute linearity for A and the specific universals for B and C in these statements. We cannot, for instance, say linearity is rectilinearity and linearity is curvilinearity. The disjunctive statement, then, is either quite wrong or at least misleading, for it cannot be interpreted as the verbal formula of which it is an example is usually interpreted. It is, however, true that any particular linearity, the linearity of any given line, is either rectilinearity or curvilinearity, where these two terms do not stand for universals, so the sentence means that any particularization of linearity is a particularization of rectilinearity or of curvilinearity. Here the disjunctive has a normal signification. But we can't say that linearity as universal (linearity in general as the phrase goes) is rectilinearity. How, then, can we accurately express that there is nothing but colourness in blueness, blue is all of it colour, there is nothing but linearity in rectilinearity? A particular linearity (this line) is a particular rectilinearity and conversely. This line, then, has nothing in it but linearity in the sense that it is entirely a particularization of linearity and has its whole being comprised in and constituted by that, yet not every particularization of linearity is a particularization of rectilinearity. But the relation of the particularizations depends upon the relation between the universals themselves, so the real question is about this relation. It is not enough to say the particularization of one is a particularization of the other, for this does not show

which is the generic universal, and is compatible with neither being genus, as in the case of trilateralness and triangularity.

§ 411. The relation of course is that of differentiation. The whole nature of blueness is constituted by its being a species of colourness, not in being colourness. Colourness as such, viz of itself, must have for its differentiations blueness, &c. It is the peculiar nature of differentiation, unique and only intelligible in examples, which makes us say that there is nothing but colour in blueness or that colour takes the forms of blueness, redness, &c. We may solve the difficulty by reference to the analogy of a body which preserves its unity, though it is now in one place, now in another. The body as existent at different places has a variable being and yet is identical Being something here and something there are not something different from its being though associated with it, they belong to its being Remaining completely identical the body *is* thus different at different times Yet by an argument similar to the above about genus and species we could prove that if B is in B's being here and in B's being there, B's being here must be in B's being there, or that B in being there must also be here As we have seen, its complete being is the complete series of what it is at all different times, what it was, is, and will be; which includes its identical permanent being In this identical being is included its quality of having the series of phases of variable being, so that, in this sense, the whole of its variable being is comprised in the identical being Supposing it to remain, for the sake of simplicity, unchanged in volume and shape, its identical being, what it is in all times and places, is the universal volume and shape together with the law according to which it moves The variable being is not the identical being nor a part of it, yet is a part of the being of the body and is, while not a part or constituent element of the identical being, still comprised in the above sense in the identical being. The diverse forms of being are not something necessitated by the thing's being, as line is by surface (where line is not what surface *is*), but are included in its being; they are true parts of its being though not of its identical being, wholly comprised in the being of the identical thing, for they are what it is, and there is nothing in them except the being of B.

§ 412. It is important to observe that we do not identify the thing which is identical with its complete being. For the thing exists now but has not yet its future being. If we did this, we should make the past and future present. Potentiality is not able to express this relation, because that refers to the future, and we seem to have no word for the relation of the present actuality to the past actuality. So if we ask how the universal which is an identity in present particulars can also be an identity in particulars which have ceased to be and which have yet to be, we have an exact parallel in the existence of the individual thing. A moving body is *now* something which, remaining the same, will be hereafter in a position not yet reached, a state which has not yet come into existence. Similarly it is the same universal which is in these present particulars and will be in others not yet existent.

§ 413. How, then, this unity in diversity is possible we understand by the illustration, and we can only understand it in this way. Using language parallel to that about body, we can say that the generic universal (colourness, linearity) is an identity in diverse forms of being (blueness, &c , straightness, &c.), so that we are right to say linearity is rectilinearity or curvilinearity but not simply linearity is rectilinearity. The former expression is permissible because the *or* shows what is meant We cannot say 'linearity is rectilinearity', for that would express either identity or that rectilinearity was wider than linearity. 'Rectilinearity is linearity' is also unnatural. 'Rectilinearity is a form of linearity' is too metaphorical, but we may say 'is a kind of', for this expresses the true relation and, though kind may have been in its origin metaphorical, it has ceased to be so. The species, then, are parts of the whole being of the genus but not parts or elements of its identical being,—that is, what it is in the species identically. They are wholly comprised in the being of the genus, for they are what it is and must be; they are also comprised in its identical being in the special sense that the identical being is such as to necessitate these kinds of being, these differentiations, for itself; not comprised, however, as constituent elements of that identical being Further, the genus universal is not to be identified with the totality of its being as the species, for it is that which is identical in all of them; nor

is it the potentiality of its realized species but only of those not yet realized. It is also true that there is nothing in the specific universals except the being of the genus Colourness covers the whole nature of blueness and redness, blueness differs from redness only in colourness

§ 414. A similar account is to be given of the individual. Its being is entirely comprised in the being of the universal; it is a part of that whole being, not a part of its identical being, and the sum of the individuals, with the universal which is identical in them, is the complete being of the universal but not to be identified with the identical being of that universal. In any part of the whole set of particulars the being of the universal is incomplete but its identical being is not incomplete. This we can only understand from instances, just as it was only by instances that we understood the identical being of body in its varying positions in space. It is important to observe what the idea of this identical being leads to This being, which it is, is not any temporal being and cannot be identified with any temporal being As we saw in the case of the broken body, the parts as existent at various times in the past and as existent now must have the same non-temporal character And it is the same with the identity of the universal.

III

MODERN FALLACIES ABOUT THE UNIVERSAL

§ 415. We will now examine certain modern fallacies which have arisen from a failure to understand the true nature of the universal. And first, that particularity is itself a universal.

Universals are distinguished from one another by their distinctive characteristics or characteristic being. But this, which is their quality, also distinguishes their particulars from the particulars of other universals Besides being this or that particular, besides mere particularity, two particulars of the same universal have no other distinction, unless the universal is diversely differentiated in them, so that they are of different species Universals, then, cannot differ from one another in the manner in which particulars do, i. e. as particulars. If two universals, Mness and Nness, be particulars of Lness, their distinctive quality is Lness and Lness only. Now if they are not related as different kinds of Lness, they are identical in quality, viz. Lness, and only differ as this or that Lness. They are identical in everything universal in them, differ in nothing which is universal. And they are also presumed to be universals. But two universals which differ in nothing which is universal do not differ at all They are universals not differing in characteristic being and so not differing in the only way in which universals can differ. If we particularize Mness and Nness we shall simply get individual cases of Lness. For Mness is this Lness and Nness is that Lness, and the only universal to be particularized is Lness. The class of M's could only appear as so many L's, and the same would be true of the class of N's. Thus the distinctive character of a universal cannot be the distinctive character or quality of another universal, unless that other universal is a species of itself (or the genus which includes itself). If, then, Mness and Nness do not so differ but are different only as this Lness and that Lness, they differ in their particularity, that is, by something which cannot be identical

in different things and therefore cannot be a universal. We may express this obvious truth by the formula 'particularity is not a universal'.

§ 416. The fallacy may be illustrated very simply by applying it to individuals Let A_1, A_2, A_3, &c., be each an A. Then Aness is common to them and each is a particular of Aness. Now since each is a particular of Aness, being particulars of Aness would appear to be a new universal of which they are individual instances But that this new universal is common to them is only intelligible as meaning that Aness is common to them as the universal of which they are particulars. What is common to them, then, so far from being 'particularity of Aness' reduces to Aness, and the false universal, which is merely verbal, disappears Moreover, if the process by which the false universal was obtained were continued we should clearly have an infinite series of such universals In fact, when we say particularity is common to this particular and to certain others, we mean that having a universal common to it and others is common to certain others and to it

However, the universal common to all particulars might be said to be their being particulars of some universal or other, not necessarily the same. Now if we say every A is either B or C, we might infer that it is common to every A to be either B or C and so 'being either B or C' might be represented verbally as a universal common to every A. But this is absurd. Each of these men is either a good man or a criminal gives a common verbal statement for each, but it does not assign anything in common to them, nor is it intended to do so. A is either B or C only gives something in common to them, if D is the universal of B and C, and then the proper statement is that every A is D. Consequently the statement that each particular is a particular of some universal or other and therefore shares in particularity does not state anything common to particulars If, however, it is said that every universal is a species of some common universal, and we then infer from this that all particulars are particulars of one universal, this is exposed to the previous criticism of the representation of the particularity of a given universal as itself a universal.

§ 417. A similar inquiry will show that universality is not

a true universal. A universal (redness) is a definite kind of being identical in particulars. A universal is a particular kind of universal being, and this is what is meant by saying that redness is a universal. It is a species of universal being, which is the genus of all other universals, unifying them as its species, not as a universal in particulars. A false universal can be constructed as before and be framed in the words 'being a species of universal beingness'. 'Redness is a universal' is not equivalent indeed to 'redness is a universal being'; redness is a kind of universal being, a differentiation, not a particularization of universal being. This last sentence is in fact a concise refutation of the whole fallacy

§ 418. Similarly speciesness is a false universal. This would be to say that because AXness, BXness, &c, are species of Xness, the quality of being a species of Xness, viz (species of Xness)-ness, is the universal of each of the species. Here the distinctive character of the universal being AXness, there is added to this the relation to Xness, viz. being a species of Xness. This, then, is treated as part of the whole quality, which is now AXness and being a species of Xness But the addition is superfluous since it is already contained in the form A added to Xness, which precisely means a differentiation of Xness. Blue colour means the blue species of colourness. AXness is not merely *a* differentiation of Xness, it is the special differentiation which is AXness. Thus its being as specification of Xness is equivalent to its being as the A specification of Xness, and this is exactly AXness In 'AXness is *a* species of Xness', the indefinite article is fallaciously treated as indicating particularization whereas it indicates the differentiation of a universal. We may see this in another way. Being a species of Xness is common to AXness, BXness, &c. But what is common to them is Xness, as their genus. The fallacy therefore arises by substituting for the genus which is common to them, their having the genus common to them. Xness is common to AXness, &c., therefore having Xness as genus common to them is common to them. The form is artificial and merely verbal, and only intelligible as meaning Xness is their common genus.

§ 419. The general nature of the fallacies which have been examined is now evident. They arise from the assumption that

the following statement is true, whatever A and X may be: 'If it can be truly said that A is an X, then A is a particular of the universal Xness.' This, however, is not true for every meaning of A and X. In every universal, on the contrary, what may be called the character of the universal must be distinguished from its universality or its community (of a special kind) with what is subordinated to it. The fallacies in question arise from an unguarded generalization. In regard to ordinary universals with a definite character common to their particulars which are individuals, it can truly be said that if A is an X, A is a particular of Xness. If now we substitute for X not the character of the universal but its universality (or what may be called, in a wide sense of the word relation, the relation of the character to the particulars), or again the fact of its particularization or some substitution of the relation of differentiation (specification) for the character differentiated, we get the false universals already examined. Thus A is an X, therefore A is a 'particular of Xness'.

Now the second statement is itself of the same form as the first; it follows, then, that A is a particular of 'being a particular of Xness', whence there results an infinite regress, for we can obviously continue the unreal process of substitution Again, since A is a particular of Xness and A is a particular of 'being a particular of Xness', we have either that 'being a particular of Xness' is a species of Xness, which is impossible, or Xness is identified with 'being a particular of Xness', which is nonsense. Indeed Xness is identified with each of an infinite series of universals

§ 420 It is important, in view of what has been said, to formulate the different ways in which one universal may necessitate another, for otherwise the phrase 'necessitate' may be used as if it had one and only one meaning

In the first place, the relation properly understood is always reciprocal One universal may so necessitate another that all the being of one not only involves but *is* the being of the other. Thus every trilateral closed figure is triangular and every triangular closed figure is trilateral. If Xness thus involves Yness, every X is a Y and every Y an X. Here it is not quite correct to say that every form of the one is a form of the other. Equilateral

trilateralness (X_1) is not identical with a form of triangularity (Yness), but any particularization of the form trilateralness (e. g. X_1, equiangular trilateralness) is a particularization of the form triangularity (e. g. Y_1, equiangular triangularity).

Thus it is not enough to say that the existence of one universal necessitates that of the other (as the existence of surface necessitates that of line), but we must say that the existence of one universal so necessitates the existence of the other that every particularization of the one must be a particularization of the other. The necessitation being of this nature, all the X's are particulars of Yness, and the members of Yness are individuals and not classes such as the X class. The universals are coextensive, but one is not the universal of something of which the particular of the other must be an attribute; the universals are not related like linearity and surfaceness. This is the second kind of necessitation which we are now to consider Here Aness necessitates Bness in such a way that the identical being of Aness has Bness inseparable from it, in its identical being. For example, surfaceness and linearity necessitate one another in their identical nature, but every particularization of surface is not a part of linearity in the sense above explained. Aness and Bness reciprocally condition one another, but each is not on that account an identity in all that in which the other is an identity. All being of surface involves being of line, but all being of surface is not all being of line. If we consider a particular line, this line is an attribute of this surface (as its boundary). In this way the particulars x, &c (given lines), of one, Xness (linearity), are attributes or elements in the particulars y, &c. (given surfaces), of the other, Yness (surfaceness), and in this way X is attribute to Y.

Here the total of the particulars of Xness do not form one particular or class of Yness, but are elements or attributes in the particulars of Yness, each in each Thus Yness cannot have the class of X's as its particular.

Thirdly, two universals may necessitate one another as attributes of the same subject, for example, the weight and solidity of a body, but neither weight nor solidity is an attribute of the other. Here the particulars of neither are particulars of the other, nor are the total of the particulars of either a particular

of the other. The universals, then, of such subjects with attributes (the subjects being heavy solid bodies) have the *same* particulars. Thus Yness (bodiness) cannot have the class of heavinesses or the class of solidities as its particulars.

[Fourthly, of two universals one may necessitate the other as the genus universal necessitates the species universal. Here the class of X's are as one a member of the class of which Y is the quality Thus any particular X has its nature determined by the nature of Yness, and as Yness is not identical with Xness, the nature of Yness must necessitate that of Xness in the well-understood relation of genus to species. Thus the x's are all particulars of Yness, and therefore Yness cannot have for a member the class of Xness itself Xness in fact is not a particular member of the genus class as its own members are members of it, but Xness is a part of the genus class; its members, *not itself*, being members of the given class Yness] [a]

§ 421. Lastly we may consider universals which are presumed to have no common genus, that is, universals which are themselves *summa genera* An attempt might be made to unify them under their common predicate universality. Now we have seen that any universal to be unified must be a universal whose particulars are individuals. If, then, universality be supposed to unify these *summa genera*, the particulars of universality must be individuals, since we have seen that the particulars of the species are particulars of the genus But the only particulars universality can have must be universals. We therefore reach the contradiction that the particulars of universality, which are by hypothesis universals, are in fact the individuals which compose the whole of the *summa genera* in question. Thus universality cannot be related to universals as their genus.

These considerations seem to point to the conclusions that no universal can be a particular of another universal, that the quality of being a species (speciesness) is not a true universal, and that particularity or particularness is also not a true universal.

[a This paragraph is supplied for completeness, cf § 423]

IV

CLASSIFICATION OF CLASSES

§ 422. THESE considerations may now be applied to the questions and puzzles about the classification of classes to which they provide the key. We shall show that in the proper acceptation of the word *class*, there is no class of which the members are themselves classes This statement corresponds to the formula that there is no universal of which the particulars are themselves universals.

In ordinary practice, as opposed to the artificialities which we have before us, universals are never unified under any universal except their genus. As we have seen, the ordinary practice is correct. So also classes are ordinarily only unified as species of a genus , that is, each class is not made a member of any class as its own members are members of it but only as a kind of the generic class, its members not itself being members of the generic class This we shall also find to be the only correct, as it is the natural, method.

§ 423 We must first define a class. The primary sense of a class is that it consists of the totality of the particulars of a universal Particulars of a universal, we have maintained, can only be individuals These individuals are unified and form one class *in consequence of the identity of the universal in them.* We shall endeavour to show that such a class cannot as a whole be a member (i. e. one member) of another class, in the same sense, cannot, that is, be a particular of a universal.

It is clear that a class in this primary sense is nothing but the complete being of the corresponding universal. Suppose two classes, that of AX's and that of BX's, species of the class X. Since these two are respectively the complete reality of AXness and BXness, we know from the foregoing argument that they can only be unified by their genus. Xness is identical in them as it is in every member of them, and there is nothing in them which is not comprised in the nature of Xness. Xness,

then, is not a universal of which each of them as a whole is one member but a universal of which their members are members. Each is a species but not a member of the genus class. A class cannot, then, be a member of any other class. Suppose that the class of W's could be as one a member of the class of which Y is the quality or characteristic. Since the full being of the universal Wness is a particular of Yness, Wness must be necessitated by the nature of Yness; or since the manifold of Wness as its manifold is to be a particular of Yness, Yness must necessitate the nature of Wness Then any individual W has its nature due to that of Yness. But it is also due to Wness. But Yness is not identical with Wness. Therefore either the nature of Yness necessitates that of Wness as genus and species respectively, in which case the W's are all particulars of Yness and Yness has not the class of Wness itself for a member, or Yness and Wness reciprocally necessitate one another. Of this there are three cases. The first where Y and W are related not as solidity and surface but as triangularity is related to three-sidedness Each necessitates the other in such a way that a particular of the one is a particular of the other.

The second is the case where a particular of the one (a given surface, for instance) is an element in a particular of the other (this solidity of which this surface is a boundary). If W is in this way attributive to Y, a particular of Wness (this surface) is an attribute of or an element in the reality of a particular of Yness. Hence the total of W's does not form one particular of Yness, but the W's are elements in the particulars of Yness, each in each, or attributes of the particulars of Yness, each of each.

The third case is where Yness and Wness necessitate one another as attributes of one and the same subject Zness, yet neither is an attribute of the other Weight and solidity, for example, are attributes of corporeality (being of body) but neither an attribute of the other. The particulars, then, of neither are particulars of the other, nor is the total of W's a particular of Yness. The universals of such subjects with attributes have the same particulars. Thus in no case can Yness have the class or total of W's as its particular.

§ 424. But it may be objected that individuals are sometimes

classed by something identical in them which does not cover their whole nature. Suppose, then, that a set of X's have an attribute Y, which is an element in their nature, as weight is in bodies. The X's which are Y might be said to form a class unified by the fact that each member of it is Y. If Yness, then, were the corresponding universal, its particulars would be not the particular X's but their particular Ynesses, and these would so far form a class in the sense already considered. The fact is after all that the universal which unifies particulars in themselves members of a class must have them for its own particulars, and the X's which are Y are themselves, and not only their attributes, members of a class through their Yness, because they are unified by the universal of being subjects of Yness, not as being Ynesses but as having Ynesses. This universal is (subject of Yness)-ness This, instead of excluding the rest of the nature of an X besides its Yness, expressly relates to it, though not expressing what that other nature consists in beyond being subject of attributes. In this sense it covers the whole of each of the given X's, and that is the necessary condition of the whole of each X being called a member of the class. If, for instance, we classify a man with Tories, on account of his political opinions, we don't say a part of him is a member of that class, we say '*he* is a Tory'. He is classed therefore as a man who is a Tory, that is, under a universal which covers his whole nature

§ 425. If, however, it be objected that the class is not taken in its unity, whereas it is as one that it is a member of the class unified by the universal of being a species class of Xness in general, the answer is that the class (i. e. all its members) can only be one as the manifoldness of the unity of its universal; it is one as the completed being of its universal. It is, then, as equivalent to the being of the universal that its members can form a 'one'. If, then, this class is one member of a class, it is a member as equivalent to the being of the universal AXness. The universal of the latter class must be identical in the beings of the universals AXness and BXness and must comprise the whole being of each. But this universal is Xness, and as it is the genus universal it is not the universal of a class of which the whole being of AXness or of BXness is a member but of a class

of which each particular in the whole being of AXness and BXness is a member This is the old fallacy of taking X-speciesness as a universal, as if the way in which Xness is present in its species were some other quality common to them. Moreover, if we accept the fallacious conclusion, we shall be committed again to an infinite regress

§ 426. The fallacies we are examining arise from neglecting the special character of the unity of the manifold in the case of the universal and its particulars. This special character is the reason why the manifold of individuals forming a true class cannot as one be one member of another class. This is best illustrated by taking a manifold not so unified which can be one member of a class

An army is composed of individual men, who in ordinary usage may be called its members An army is an organism and each member of it is unified with the rest by the character which defines the organism. But this unifying element is not a universal of which he is a particular. Defining the army by the universal, a particular kind of organism, the individual soldier has not the quality of the universal, he is not such and such an organism As a member of an army he is defined only through his relation to the whole and to the other members, and the attribute assigned to him in that definition is not something which he has irrespective of the others. It is not a universal identical in him and in those others, which he has whether they have it or not, whereas the blue colour is blue in itself, as we say, and not by relation to the whole of blue colour or to any other colour An organized whole is not a class. Verbally we may make its members members of a class by stating of each that he is a member of this particular army; but the fallacy is transparent, for what we state of each assigns as their unity something which is not a universal of which each is a particular. The organism, as one, is a member of a class whose universal is the universal of which the given organism is a particular

§ 427. We have shown that a manifold of individuals forming a species class cannot as one class be one member of another class. We have also seen in an example that a manifold which forms a unity and is one member of a class does not itself form

a class. It remains to show when a set of individuals can form such a unity that this unity can be a member of a class. This will make the nature of the fallacy here investigated clearer by showing at the same time why a manifold which does form a class cannot as one class be a member of another class.

If a set of individuals can form such a unity, the quality of the class universal must be able to be attributed to them as constituting this unity but not to each of them separately. Now this happens when the individuals form an organic whole and can happen only then. The attribute which they have as unified is one which they have only as a whole and not each separately. If, however, the set is only unified as having an attribute identical in each, as being true particulars of a universal, they are not an organic whole. The attribute which they have as belonging to the whole of the particulars, so far as it can be called a whole, is one which they have each separately and not as a set or manifold. We may indeed give them as a set a verbal attribute and say they together make up the class of X's. But here is no unity to which a quality is assigned; on the contrary the only quality assigned is Xness, which each of the given set has separately It is said that we say not of one of them but of all of them together that they make the class of all X's and so the attempt is made to make 'constituting the class of all X's' a sort of quality attributed to the whole, as somehow one whole. But consider what the expression exactly means. How does it differ from the statement 'each of these is X'? If it does not differ, a quality X is merely assigned to each separately and no quality is assigned to the whole set as constituting a unity of which the quality is an attribute But it may be said that the difference is this, that we do not only say that each is X but that all of them together make up the class of X's. But now the class of X's accurately means all the individuals that are each of them X. Hence this last form means that all of the set together are all of the individuals, each of which is X But now we must be able to define the subject of which we say it constitutes all the individuals. We can't determine it as merely this set of individuals, we must say what set, and obviously it can only be defined as all the individuals which are X. Thus the statement which is supposed to assign

a quality to a whole as one is the mere tautology, all the individuals which are X constitute all the individuals which are X. Individuals or things therefore associated only as particulars of the same universal do not form a whole of which as one there is one universal applying to it and not to the individuals. This obviously destroys the fallacy about the class of classes.

§ 428. The fallacy consists in classifying classes by their property of being classes To classify a group we must be able to state something of it as a unity, as a 'one', the attribute stated forming the classifying universal. Classness can never be such an attribute, because to state of a group that it is a class is only to state that the individuals form a certain kind of unity but not to state anything of them as forming that unity. Even if, in the case of an organism, I state of these pieces of metal that they are a unity because they belong to a clock, I have stated nothing of them as a unity I state something of them as a unity only when I state something of the clock. But I cannot similarly go on, after saying that this set is a class, to state anything of this class as one, because it is not the kind of unity which makes it a one, whereof something can be stated in contrast with stating something of each member individually.

§ 429 We may put this generally as follows. If possible, let the class of X's as one be a member of the class of Y's, of which class Y is the universal.

Now the class of X's, as the total particularization of Xness, is only what it is as a particularization of Xness, as having Xness identical in it. It is only a member, as a unity, of the class Y, i. e. it is only one particular of Yness, because its particulars have Xness identical in them. That is, the particulars of the universal Xness, conceived as forming a whole which is a particularization of Y, a system in which Y is identical with what it is in other systems, have Y in them only because Xness is in each of them identically. This necessitates that Y is identical in each of them Y, then, must either be convertible with X, which is contrary to hypothesis; or Y is in X as an element in X's identical being, which is also contrary to hypothesis, or, finally, Y is in X as genus in species, but this again is contrary to hypothesis. Therefore the original hypothesis is impossible.

PREDICATION PREDICATED OF ITSELF

§ 430. THE fallacy that predication can be predicated of itself is easily disposed of. Let A is B be a given statement. It is said that this is equivalent to Bness is predicated of A. From this is similarly derived the statement that predication of A is predicated of Bness, and from this that predication is predicated, which means that predication is predicated of predication, that is, of itself.

Observe that A is B means something objective, not the act of predicating B of A. B is predicated of A is a new statement relating to the subjective act of stating B of A. I may reflect after stating that A is B that I have stated this, or (as it is said) that I have predicated B. Thus the statement Bness is predicated of A does not mean that predication is predicated of Bness, it is a new statement about the statement from which it is derived

But allowing, for the sake of argument, the derivation proposed, the interest of the statement 'predication is predicated of itself' is that it makes predication a case of itself.

We must therefore determine more precisely what the form A is B means As we have seen, in our previous inquiries, B represents normally an adjective, so that when A is B is said to predicate Bness of A, Bness is the universal of an attribute of A, named B. Thus in 'this orange is round', we predicate roundness of this orange.

If, however, B is a noun form preceded by the indefinite article, as in A is a B (This is an orange), Bness is the universal of which the whole subject A is the individual. This is true even of such statements as this orange is a round object. Thus to say that Bness is predicated of any subject A means either that Bness is the universal of an attribute of A or that it is the universal of the whole of A.

Hence, if anything is predicated of itself, it is either the

universal of an attribute of itself or the universal of itself, and, as we have seen before, neither of these is possible.

§ 431. The fallacy may, then, be exposed as follows.

(1) From A is B we get Bness is predicated of A (by hypothesis). Let predicated of A be here an attributive. Then (predicated of A)-ness is the universal of which an attribute of Bness, viz predicated of A, is the particular This may be put generally as

(predicated of A)-ness is a universal of an attribute,

and this is of the form X is a Y, which means that Yness is a universal of which X is a particular or case.

Thus (predicated of A)-ness is a particular of (universal of an attribute)-ness.

Thus a universal is a particular, which is absurd.

(2) From A is B we get Bness is predicated of A (by hypothesis). Let 'predicated of A' be here equivalent to 'a predicate of A'.

This is of the form X is a Y, which means that Yness is a universal of which X is a particular.

Hence, as before, Bness is a particular of being a predicate of A.

But Bness is a universal, which is absurd.

The fact is, that the process we are criticizing which interprets Bness is predicated of A to mean 'predication is predicated of predication' does not yield the statement that predication in general is predicated but, at best, only that predication of A (a subject) is predicated of Bness (an attribute regarded in abstraction) or that being a predicate of a subject is a predicate of B, an attributive.

§ 432 The truth is that predication should be taken in its strict and accurate sense. Then A is B does not mean that B or Bness is predicated of A. Its meaning is objective and it means nothing about statement or predication. But if Jones states that A is B, he may be said correctly to predicate Bness of A and to predicate that A is B Now this is not the A is B from which we started but a new A is B, nor does B is predicated by Jones mean that predication is predicated of B, but that B is predicated or stated of something. We require, then, a new statement by Brown, which is that B is stated of A by Jones.

We thus get the series 'A is B', stated by Jones; 'B is stated of A by Jones', stated by Brown; 'being stated of A by Jones is stated of B by Brown', stated by Robinson, and so on. Thus A is B is the predication by Jones; B is predicated by Jones is the predication by Brown of Jones; predication by Brown of predication by Jones is the predication by Robinson of Brown. In other words, Robinson asserts that Brown says that Jones states something. Here of course nothing is predicated of itself, because the three different uses of predication belong to three different persons.

VI

RELATION AND QUALITY

§ 433. THE verbal fallacy just exposed is one more of a kind which forms the staple of certain modern writers. They arise from the habit of using abstract terms without asking what they would mean in a given context or bringing them to the touch by examples. There is a serious confusion of this kind in a more distinguished writer, in the chapter entitled 'Relation and Quality' in Mr. F. H. Bradley's *Appearance and Reality*. Throughout this chapter there is not one single illustration, though it is of the last importance that there should be The writer indeed takes the fallacy so seriously as to regard it as evidence of the necessarily illusive character of our experience. It is in fact a mistake of his own of a very inexcusable character which he thus naively thrusts upon the 'appearance' of the object and treats as evidence of the unsoundness of phenomena. The fallacy is a supposed infinite regress which results from relating the relation of two terms to the terms of the relation themselves The puzzle may be put briefly thus · let A and B be the terms of a relation, r the relation between A and B. A and r are different and, being different, stand in a relation, say r_1, to one another Similarly let r_2 be the relation between B and r. Thus we have two new relations, r_1 and r_2, and r has itself become one of the terms of a relation. Now r_1 being different from A and r we similarly get two new relations, and the process is infinite, producing an infinite series with terms all different from one another

The presupposition of this fallacy is that if two somethings are different from one another they must stand to one another in a relation which is different from either, not identical with nor included in the separate nature of either. In other words, r is not identical with A or B nor a part of what is already understood in A or B. So far from being always true, this

presupposition can be shown to be never true where A and B are in fact properly described as related.

Take first the case in which A and B, being different from each other, there really is a relation r between them different from either. For instance, let r be equality in 'A equals B'. We have to ask whether there is indeed a relation between A and r and what it is; for the fallacy clearly lies in the first step, and it is presumed by the writer that we do not go beyond the original A and B and r by any new process of apprehension.

Now, if there really be such a thing as r_1, it is necessary that it should be new and that the statement 'A stands in the relation r_1 to r' should be new and not a part of the original statement which gave r as the relation between A and B. The question, then, what r_1 is or what is the relation of A to r must be real and intelligible. We shall find that there is, in fact, no new statement and that the question is an unreal one, because the idea of a relation cannot be applied, as is proposed, to a relation and a term of that relation itself.

§ 434. The ordinary statement of a relation does not contain the word relation itself. A is B's father So we say ordinarily A is equal to B (where the relation between A and B is one of equality of magnitude), not A stands to B in a relation of equality. The latter form, however, gives a convenient general formula. A stands to B in a relation R, and if we define the kind of relation, r, this is the particular of which R is the universal. Our question, then, is what r_1 is which is the presumed relation of A to r, where r is the relation of equality. Results can be obtained only by considering particular instances, not by a symbolic method. Two replies, differing in completeness, are open to us. We may say that the relation of equality to A is that it is the relation of A to B, or more accurately the kind of relation which A has to B. Here equality is not the equality of A to B, viz. r, but R the universal, that is, equality in general. The answer, then, is simply a statement of what kind (viz. R) the relation r is. Thus we have not gone outside the nature of r itself nor reached a new relation r_1. If, then, we are to give the full answer and to use all the information which the question supplies, we may reply: the relation of A to B is not R but the particular instance r, which is the equality of A to B.

If, then, with this in view we go on to ask what is the relation to A of r, we can only reply that the relation to A of A's equality to B (r) is that it is A's equality to B. Here, instead of giving the genus R for our answer, we have r, the full being of R itself, yet we have not got beyond the original relation r, nor in any sense beyond the original statement that A stands to B in a relation r.

§ 435 The question, then, what the relation of A is to A's standing in the relation r to B is obviously unreal, for it contains everything necessary to its own answer; it puts as a question what is no question to the person asking it. There is a contradiction in the very asking, a contradiction between verbal form and the matter to which it is applied. This is enough to show that to suppose a relation (r_1) of A to r is absurd; or (since the very question what that relation may be involves an absurdity) it must be an absurdity to suppose such a relation.

§ 436 If now we try to avoid this by saying that r_1 is the difference between A and r, we can get the solution simply by asking what the difference is between A and r. To this question the only possible answer is that A is A and not the relation of A to B, and that the relation of A to B is the relation of A to B and not A. Thus the so-called difference is a way of restating the same facts as before and therefore a mere verbal difference. Moreover, it would be easy to show the futility of inquiring what the difference is between A and the last difference, as well as to show that the only possible answer gives nothing not already contained in A and r. The puzzle here is equally foolish with that puzzle which produces an infinite series by taking the difference between A and B as something different from each of them and then taking the difference between that difference and either A or B, and so on.

§ 437. The formula, then, which the apparent puzzle presupposes being false in this particular case, we may next consider cases where it is equally false for other things than a relation and one of its terms. Here again we must proceed by particular concrete instances, not by a symbolic method. Consider, for example, the surface of a volume and the volume. These are clearly distinguishable, but what is the definition of a surface? It is the boundary of a volume. What, then, is the relation of

the surface to the volume? We can only reply that it is that the surface is its boundary. If the word relation applies at all, clearly this must be the accurate account of the relation. Hence in the case of the two terms A, the surface, and B, the solid, the relation *r* (if there is any relation at all of A to B), instead of being, as the presupposition of the fallacy demands, neither identical with A nor included in what is understood in A, is either identical with A, inasmuch as the description of *r* (being a boundary of the solid) and the definition of A's being (being a boundary of the solid) are the same, or at least something always included necessarily in what is already understood by A. This must always be the case when A has no being except as an element in the being of B. The same may easily be seen to be true of the so-called relation of a subject to its attributes, the relation, for instance, between a body and its weight or its motion. Thus, besides the particular case where one something is a relation and the other a term of the relation, there are other cases where the general formula is not true. There may be two somethings different from one another, not being a relation and one of its terms, which yet do not stand in a relation different from either

VII

CATEGORIES IN ARISTOTLE AND IN KANT

§ 438 If our reasoning is correct, the universalities of the various species of a genus are not particulars of the universality of the genus but kinds (or parts) of it. Suppose now an abstraction beginning from individual things till we come to so-called *summa genera*, or, as they would be more correctly termed, *summae species*. The universalities of all other universals would be comprised in the universalities of these, not as their particulars but as kinds, forms, parts, or aspects of them If, then, we considered these *summae species* as kinds which Being or Reality must take, where Being is more accurately the Being of Things (that is, the universal of which Things are the particulars), the Being of Things is in the position of *genus* to these *summae species* and their several universalities are all comprised in the universality of this genus In regard to this we must avoid the fallacy of creating a universal of universalities, with an infinite regress That fallacy has been already sufficiently exposed. There is no universal of universalityness of which the universalities of universals would be particulars, for the universality of one genus universal as distinguished from that of another lies not in its being a unity which is particularized but in being particularized in these precise individuals, in being the particularization of its own peculiar quality or characteristic; but this is precisely itself, and its universality is indistinguishable from itself in the fullness of its being. Thus the classification of universalities can only be, if possible at all, the classification of universals, there is no universal of universalityness of which the universalities of universals would be particulars

§ 439 This genus, then, of the *summae species* is not mere Being but the universal of the being of Things or Substances, and is therefore the generalization of Substance, Substanceness, or Substance-in-general. Now in this system every universal has things for its particulars, and thus the universals of attribute

and relation will not appear in the system. Nevertheless, every variety of being, attribute, and relation as well as substance, must be comprised in the system, because all these are comprised in the existence of things. The nature of substance involves in itself attributes and relations; its universal therefore involves the universal of possession of attributes and the universal of relatedness. We have now to consider the relation of such a universal as attributeness in general or the universal of a particular attribute such as colour(edness) to the universal of substance in general and to the universals of substances. We may note in passing that we can understand why Aristotle said that Being was not a genus if we remember that the genera *par excellence* (in the *Categories*, for example) are in fact universals whose particulars are substances.[1]

Attributeness means being an attribute of a substance, hence the universal of substance involves in its own being the being of attributeness Nevertheless, the latter is not an element in the universal substanceness, the corresponding element is 'having-attributes'-ness Moreover, the latter is not a differentiation of substanceness The question, then, is whether these universals are capable of being unified by any unity beside the unity that one involves the existence of the other. They cannot be differentiations of one and the same universal because differentiations of the same universal must have identical particulars with that universal. Nor can they be particulars of the same universal, for if two universals are particulars of the same universal, they must be differentiations of a common universal though not of that universal Thus, though we can state of attributeness and substanceness that they *are*, Being is not a universal of which they are differentiations, nor is Being a universal of which they are particulars

§ 440. Thus the form 'S is', which as opposed to 'S is P' does not occur in ordinary linguistic usage whether in ancient Greek or in modern languages, does not represent in its subject particulars of a universal which is Being or Beingness. If, then, Aristotle had carried out fully the thought which appears to be implicit in Chapter 5 of the *Categories*, with the distinction there made between 'said of a particular subject' and 'existing in

[1] *Categories*, 2^a 11 seq.

a particular subject', the result would have been a system of universals classified as in the section above and based on his view that the only true independent reality is the individual thing (or person). The *summum genus* would be Thingness, and for this the only word in his terminology that appears to be suitable is Being (*οὐσία*), a word which both in the *Categories* and in *Metaphysics*, Book Z, is sometimes the name for Thing as such, or First (Primary) Being. This, however, is nowhere unmistakably stated, he appears indeed to have virtually stopped in his classification at certain highest genera and his own expression, 'the highest of the genera',[1] favours this interpretation.

§ 441 For his view that Being is not a genus, the important passages are three in the *Metaphysics* with which one in the Topics agrees, a passage in the *De Interpretatione*, and one in the *Posterior Analytics*.[2]

The first of the passages from the *Metaphysics* does not turn on his view that the genera *par excellence* are secondary essences and is a little difficult to interpret It runs as follows 'But it is not possible for One or Being to be a genus For each difference of a genus must have one and being said of it (sc. when we speak of it), whereas it is impossible that either the species of the genus or the genus without the species should be said of the differences. Thus, if One or Being were a genus, none of their differences could have one or being stated of it' By difference he here means not the differentiated universal (e. g. rectilinearity) but the *differentia* (straight, &c).[3] This suggests that he had not realized that, according to his own theory, Being would be the proper name for the highest genus. The passage from the *Posterior Analytics*, 'to exist is not the being (essence) of anything, since the being existent (that which exists) is not a genus', does seem to be connected with his doctrine that the individual is the only real existent and that the true genera have 'beings' for their particulars. Finally, the passage in the *De Interpretatione* runs as follows: 'for not even "to be" or "not to be" is a sign of a thing, not even if you mention

[1] *Met.* 998^{b} 18

[2] *Met.* 998^{b} 22, 1053^{b} 23, 1059^{b} 30, *Top* 121^{a} 16, *De Int* 16^{b} 22; *An. Po* 92^{b} 13

[3] Cf his language in the *Topics*, 144^{a} 37 seq

"being" by itself merely. In itself it is nothing, but it signifies in addition a kind of compounding which cannot be understood without the things so compounded.' This is most important for the linguistic point involved in our problem.

It is singular that Aristotle, who in the *Metaphysics* is attacking those who made Being and Unity the essence of things, does not adopt the seemingly clear and decisive argument based upon his own theory of true being as the individual and the true genus as that of which the individuals are the particulars. His argument, however, and this is important, is directed to showing that Being as a universal cannot be differentiated, though he has not realized what seems to be the direct proof required (he gives instead a single *reductio ad absurdum*), viz. that if being were the genus or class universal of the universals which we say 'are', those universals, as we have shown, would necessarily have the same kind of particulars as it and consequently the same kind as one another. But, obviously, the universals of a substance, an attribute, a relation, cannot have the same particulars. To reach this positive point of view Aristotle would have required to have had before him the point of view from which a universal could be represented as a particularization, not a differentiation, of another universal

§ 442 We miss, then, two things in Aristotle's discussion. The line of thought which led him to say 'being is not a genus', at least when it took the shape it seems to have in the *Posterior Analytics*, might have made him recognize that though the universal 'being' was not a highest genus, yet in his own terminology such a genus was exactly 'essence' as the being of things or, more accurately, 'the being a thing'. How far he was from this may be seen when he says,[1] 'Unity cannot be a genus for the same reasons that neither being (existent) *nor essence* can be.' Secondly, he might have been led to see that though Being is not a classifying genus which unified everything in that way, yet there is a unifying principle in all reality. Instead he rested content with the negative statement that Being is not such a principle, where being is that 'is' which is universally predicable. He may have been prejudiced by his justifiable criticism of Plato who had sought this unifying

[1] *Met* 1053^{b} 23.

principle in the Idea of goodness. Had he followed up his thought he might have reflected that just as a genus demands its own differentiation into species and individuals, so by the same inward necessity the unity of reality demands the kinds and things into which reality actually is differentiated.

§ 443. Taking his own categories, they are obviously unified in the reality of (primary) Being or Substance, and he does elsewhere recognize that the other categories depend on the first. But he never put this as the unity and the real unity which corresponds to what is common to the statement of 'being' In fact he never cleared up his mind about it, or he could not, so to speak, have so degraded 'being', as he does in the passage translated from the *De Interpretatione*, and have merely left it at that What he has said there is true and important, but it is misleading, as it stands.

Moreover, the formula 'Being is not a genus', although it shows from one point of view an accurate insight into the nature of classification, is extraordinary and misleading when considered in relation to his own terminology. He must speak of the categories as categories of being, this being cannot be merely the common predicate of everything, if we are to take categories literally as predicates. For we cannot state of this 'being-in-general' that it is a substance. On the other hand, if it does mean 'being-in-general', categories would surely have to mean species or kinds, so that being would indeed be a genus and the formula be contradicted. It is most natural to leave category its proper meaning and then 'being' will stand for 'that which is', not for being in general. This again, if all the categories are asserted of it, as the formula 'categories of that which is' naturally implies, could only be complete being, that which is in the fullest sense. Now that with Aristotle is the individual thing This agrees with the fact that the categories are given not as abstract universals in the noun form (whiteness, as an example of quality) but in the adjectival form (white, double, &c). If so, what is meant by the categories of being (that which is) is that each of them is an attribute of the true and real being of the individual thing, the primary being of this treatise. It is the thing in fact of which we can properly state that it is a substance, two cubits long, in the market-place, &c.

The only word form which causes any difficulty in this interpretation is that of the adverb of time (e. g. yesterday). This, however, is again not put as an abstract universal but as it would occur in a statement, and it is true (and the truth) that as every happening belongs to a substance, so the temporal qualification ultimately also belongs to a substance (e. g. this substance was in the market-place yesterday). This appears, then, to be the meaning, for it is difficult to see how anything else could be meant.

§ 444. But now, if this is so, in the categories of being, of 'that which is', the latter expression is used in the general sense, and 'that which is' represents the universal of all particular 'beings' and so is the universal of substanceness. It is just in relation to the distinction of the categories that the meaning of 'being is not a genus' becomes of great importance. On the above view 'that which is' is equivalent to substance (being) when the categories are termed categories of that which is. Thus 'that which is' is not only a genus of which real complete things are the particulars (complete, that is, in the Aristotelian sense as equivalent to things) but the highest and most comprehensive genus, though *not* the genus of the categories, including all reality whatever. This discussion seems to confirm the hypothesis that Aristotle did not pursue the train of thought which his view that 'being' taken in one sense is not a genus might have suggested to him.

§ 445. What, then, is the fallacy in Aristotle's proof that being is not a genus? He contemplates two ways of stating the genus X in regard to a given subject S. Either a species AX is stated in S is an AX and therefore mediately the genus S is an X, or the genus is stated 'without its species', and we say immediately S is an X. In neither way, he says, can we state the genus of the difference A. He seems to mean that odd being the differentia of odd number, we can't say that odd is an odd number and so a number, nor directly that odd is a number.

But is the argument free from verbal fallacy? When we state that S is a species, we mean that S is *an* AX, and when we say that the genus belongs to the species, we state X, the genus universal of a *member* of the species AX, we mean *an* AX is an X. If, then, we state the species of the differentia we ought

to mean that we state that an A is an AX. Now if the differentia necessarily presupposes X the genus, the statement must be true. We do not say that odd numberness is numberness, but that an odd number is a number. So if we state odd number of odd, we mean that an odd thing is an odd number, which is necessarily true It looks as though Aristotle meant that linearity cannot be predicated of straightness because straightness is not a line, but neither is rectilinearity a line, and thus he would appear to have fallen into a verbal fallacy.

§ 446. There is a danger that in appreciating the insight shown by Aristotle in his dictum 'being is not a genus', we may ourselves fall into the same one-sidedness. There are certain characteristics of Being which, if not identical with those of a true genus, are parallel to them in a remarkable way and must therefore not be neglected These characteristics are not destroyed by the discovery that being is not a true genus; only a certain way of regarding them, i e. as particularizations or differentiations of a universal, is destroyed. An attribute or a relation may rightly be said to have being as well as a substance, and this being is not identical with that of substance. We can study the attribute in abstraction from the subject, as we do in the mathematical sciences, and this proves that it is distinguishable in being So of relation, it is essential to relation that it should be 'between' things which have some other nature than that of standing in a given relation. Hence it is natural at first sight to say that Being is a genus with its species 'being of attribute', &c Again we think that Being in its own nature necessitates these forms of itself, and this is parallel to the self-determination of a universal in its differentiations. If, then, the forms of Being are not species nor kinds as ordinarily understood, nor of course particulars of a universal, how should they be described accurately? We have to revise one of our usual conceptions and either to refuse to call these universals or to admit universals which have no particulars and no differentiation, the universality of one quality (attribute) being identical in kind with the universality of every other. In this difficulty we may provisionally term them common principles. They are live universals because they are a unity in a manifold; universality in each universal, particularity in each particular,

and so on, but different from true universals because the manifold is not a particularization or differentiation of the common principle. This will meet the difficulty we found in regard to classes. We say of every class that it is a class, yet we saw that classness is not a true universal of which classes are particulars. Class has no differentiation and no particulars We have a unity in a manifold, but the manifold is not a particularization of the unity. Instead, then, of our ordinary view we have to recognize that there are some universals which admit of differentiation and particularization but not of individualization, and that some (which we have called common principles) do not admit of either particularization or differentiation

§ 447. In Being, then, called provisionally a common principle, we recognize a unity in a manifold which is *sui generis*, just as much as the unity of a true universal in its particulars and its species is *sui generis*. Being, like a true universal, also determines its own manifoldness but in a different manner. If we are to seek an illustration or analogy we may refer to the self, which is an absolute unity in its different thoughts, or a body, which is identical in its different positions and aspects, but neither the self nor the body is the universal of those differences nor they its particulars

§ 448. If we now consider the forms of Being, the manifold which it must assume and which simulates the differentiation of a universal, we may perhaps find the true significance of the philosophical classifications called systems of categories. These categories are obviously of a very special kind and are philosophical and not scientific. They are, that is, though comprehensive, not a classification and could not be reached by abstracting successively from the whole field of individual substances. In this way we should attain to the classifications of the natural sciences but not to categories. Now Kant's criticism of Aristotle's categories has shown that while there may be agreement about some of the main categories, there may be the greatest disagreement about the real meaning and object of them as a whole. Kant believes that Aristotle was led to his grouping of the categories without realizing its true character, and that in consequence he did not carry it out consistently. Whether, then, Kant's own view of the categories was right or

wrong (and surely it was not right) this suggests that there may be a certain instinctive impulse to search for categories, without full consciousness of its nature. The impulse will lead to an arrangement of a quite peculiar kind, and reflection must then supervene in order to understand the impulse and to correct its imperfect work. Kant naturally makes the categories forms of the unifying understanding, because of his dominant confusion of the apprehension and the apprehended. Aristotle's tendency is far sounder, for necessity of apprehension can, after all, only mean apprehension of a necessity in the object.

The explanation, then, of systems of categories may well be that we come in time, by reflection on the use of the verb 'to be', to recognize a corresponding unity of being, that the totality of particulars in all their variety is a unity. Long before we have recognized this unity in particular sets or varieties of being. Then comes the philosophic impulse to determine the forms which *being in general* must take, suggested by the analogous determination of such a unity as the section of a cone, and so to cover exhaustively the differentiations of being in general in the whole of existence This impulse need not be fully aware of itself. Aristotle, for example, doesn't consciously go to work in this way or, if he did so begin, he probably gave it up when through his clear idea of differentiation and classification he realized that Being is not a genus Still it may have been the fact that we state of everything that it *is*, which suggested the idea of an absolutely unified system of being.

§ 449. Thus the characteristic of Aristotle's system is that it is a sort of exhaustive attempt to cover the whole variety of reality. 'Everything is either substance or an affection of substance' is its implicit meaning These are in fact his two main categories. But he gets into difficulties about substance, so that his thought in the end is that everything is either substance, or quality of substance, or quantity of substance (or of what belongs to substance), or relation of substance (or of what belongs to it), and so on. Now it is clear from our analysis above that the impulse to determine a sort of differentiation of Being must produce an altogether unique system. For the general forms of Being are not true differentiations of it, nor are the individuals, which 'are', particulars of Being but of other

universals. This explains the fact that the classification (as it first seems to be) of all being could not be attained by any abstraction from individuals or any classification of them; because this, the natural form which classification first takes, proceeds to universals of which the original particulars are those very individuals, and the successive universals are differentiations of one another in succession. This systematization is by means of differentiation and particularization, the other (the system of the categories) is achieved by neither. Hence the latter acquires its uniqueness; for the ordinary scientific method, even when carried to the utmost unity, will not bring to light one single category except substance itself. If continued ideally upwards, it cannot even bring to light the universal 'mere being' of which the categories are the unfolding The same is of course true of any classification proper which starts from the individual, whether thing or individual attribute Attributeness, for instance, if regarded as the universal of individual attributes, is neither a differentiation nor a particularization of being; the most general universal which could have individual attributes for its particulars is just attributeness Now substanceness, attributeness, relationness, &c, cannot be treated as *summa genera* because they would then be members of the same classification; this they can never be, for such genera must have the same sort of particulars, and here the particulars of the one would be substances, the particulars of the other attributes.

§ 450. If, then, this is the true account of the philosophic impulse under discussion, we see that it is a most serious and fatal mistake to regard it as a classification, when once we understand what a classification truly is. (That leads to the further error of vainly attempting to adapt the system of categories to the ordinary classifications and to make them departments of it.) Properly understood, it is simply the just endeavour to determine the manifoldness in which Being in general must unfold itself, and Aristotle proceeded correctly when he exhibited as categories such general forms as those of Substance, Attribute, Relation, &c. Contenting himself, however, with pointing out that Being is not a genus and therefore could not constitute the essence of things, he seems by his merely negative attitude to have missed the true significance of his own list of categories.

§§ 438–50. *Translation of principal passages referred to in the above investigation.*

[Arist., *Cat* 2^a 11. 'Substance most properly and primarily and especially so called is that which is neither said of a particular subject nor is in a particular subject, for instance, a particular human being or a particular horse. Secondary substances are what the species are called, in which the substances primarily so called exist, and besides them the genera of these species; for instance, while a particular human being exists in the species human being, the genus of the species is animal These, then, are called secondary substances, for example, both human being and animal'

De Int 16^b 22 'For not even "to be" or "not to be" is a sign of a thing, and not even if you mention "being" by itself merely. In itself it is nothing but it signifies, in addition, a kind of compounding which cannot be understood without the things compounded.'

Post An 92^b 13 'Again we say that it is necessary that everything be proved to exist by demonstration, unless it be essence But to exist is not the essence of anything, since the being existent (that which exists) is not a genus. That a thing exists therefore will be (subject of) demonstration. This is what the sciences in fact do The geometrician assumes what triangle means, but proves that it exists'

Met 998^b 22 'But it is not possible for either being one or being existent to be a genus of existents. For while it is necessary that the differentiae of each genus should each both exist and be one, it is impossible either for the species of a genus to be said of the appropriate differentiae or the genus ⟨to be said of them⟩ without its species Therefore if we assume being one or being existent to be a genus, no differentia will either be or be one'[1]

The same argument is used in *Met* 1059^b 30 and *Topics*, 121^a 16. In *Met* 1053^b 23 he says, 'being one cannot be a genus for the same reasons that neither being existent nor essence can be.'

Met. 1017^a 22 'Whatever the forms of predication signify are said to be essentially. For to be has as many significations as there are forms Inasmuch, then, as the predicates signify what the subject is, others its quality, &c., . . to be has a signification equivalent to each of these (for there is no difference between "the man is walking" and "the man walks").']

[1] [J C W translated 'It is not possible for One or Being to be a genus. For each differentia of a genus must have one and being said of it (in statements), whereas it is impossible that either the species of the genus should be said of its differentiae or the genus without the species So if One or Being were a genus, none of their differentiae could have one, or being, stated of it']

VIII

UNIVERSALS. LAST WORDS

§ 451. We will now return to a difficulty presented by the view we have taken of organisms in relation to their universals. We have seen that a whole organic system is one particular of a universal and that its members are not particulars of this universal. We have also seen reason to think that if a class is classed, that is made a member of another class, the totality of the members of the class must form an organism or system.

It is essential to a system to have as a whole a quality which the members of it taken singly have not. Now a work of art may seem to cause a difficulty. As a whole it is beautiful but each part of it is also beautiful. Thus each part appears to be a particular of the same universal as the whole system. There must then either be some ambiguity in the assignment of the common quality, or else the quality assigned is not a universal of which the system as a system is the true particular. We shall find that beauty is an attribute of the whole which is of the same general kind as one attribute of each member of it, and it would be difficult, perhaps impossible, to find a system in which this was not true. But the attribute as attribute of the member and of the system as one is not the same in species and in this sense the universals are not the same. Moreover, the universal of the attribute, beauty, is as such the universal not of the whole system but of an element in it. This system then is not its particular. Thus the supposed exception will prove to be no exception but confirmatory of the view already adopted.

§ 452. Consider, for example, a musical melody. It has a beauty as one, as a system of its notes. The single notes may be beautiful either considered apart from the whole or as in it. A clear note quite by itself is beautiful and that same note in a melody may naturally be called remarkably beautiful coming where it does. The single note by itself is not merely beautiful, it has a definite quality as a sound and it is to that that we attribute

the beauty. Thus beauty is a universal not of the sound but of an element in the nature of the sound. It is the universal of an attribute of the sound. So of the melody. Its universal is a definite number of notes in a certain order, relation of pitch, and with fixed relations of duration. It is an attribute of this that it is beautiful, and the universal of that is a universal of which only a part of the whole nature of the melody is a particular, whereas the universal of the system is its whole nature of which the individual note cannot be a particular. Finally, the single notes as parts of the melody are beautiful because of the quality, duration, &c., of the sound as related to the quality, &c., of each of the other notes and as having a certain place in the whole. The universal of the single note as thus regarded is clearly an element in the universal of the system and not the universal of the system itself Again, though this is not necessary for our present contention, we cannot accurately treat the note so considered and the melody as two beautiful things and then compare their beauties as being homogeneous. For the beauty of the note in the system involves the beauty of the whole system. More precisely the description of it is the description of the beauty of the whole system as contributed to by its relations to all the notes, not as contributed to by the beauty of the note *per se*, and this involves exactly the same thing for every other note of the whole. In fact we only call the note beautiful as being an element in a system which is beautiful as a system. It is therefore equivocal to speak of a beauty of its own which can be distinguished from that of the whole. So if a clock keeps time, we may say equivocally that any part of it keeps time too because it contributes by its quality and relations to the other parts and the whole keeping time. And here the artificiality is obvious. The timekeeping of the clock is not a system composed of the timekeepings of its several members, nor is the beauty of a whole work of art composed of the beauties of its parts as members. The beauty is not a system but the beauty of a system.

§ 453. The same may be said of a difficulty raised about 'the good'. The difficulty is not quite clear, but appears to be that the universal good consists of the individual goods which are members of the system together with the system itself as one

good. Before considering whether we have here an exception to the above theory of universals, we may question the validity of the idea itself. First let the good as universal be one which has a limited number of differentiations each existing in but one particular. The context of the argument we are criticizing suggests that it is assumed that such a universal is contemplated and that it must have a finite number of members. Yet we see no limit to the differentiations of good which may be evolved in history, and therefore, if the differentiations are assumed to be finite, the hypothesis seems idle. As the good again is actualized in infinite time, we can see no limit to the particulars which are good. We must therefore assume that the hypothesis applies to particulars supposed to be, possibly at least, infinite and yet somehow composing a system. Are then the particulars to be called good only as members of a system? This seems clearly untrue; for there are goods, one would think, which are good without any such reference. For instance, the love of truth is such a good. The love of truth and hatred of falsehood are unaffected by relation to any other kind of good. Suppose it to be said that the good meant is real and that we, if our knowledge were complete, would see that every instance of the love of truth is good in relation to all other particulars of good. That would be another kind of good which it has and not the good, the really important practical good, of which we have spoken. This good is not determined to have its quality by any such relation. The particulars then would be good in themselves, though they would also be good as members of the system. This leads to the other general alternative that the particular goods are each good apart from the system but members also of a good system. The treatment of this will obviously be the same in kind as that already given in the case of a work of art and its relation to beauty as a universal. It would follow that the good system could not possibly be the true particular of the same universal as the goods which are members of the system. Goodness cannot be assigned to any particular except as attaching to a quality in that particular other than good. Thus the good particular cannot have its nature covered by mere goodness. As Aristotle would say, 'being something else it is good'. Indeed Aristotle in the sixth chapter of the First Book of the *Nicomachean Ethics* reaches in his

criticism of Plato a point of view which is correct and which he does not succeed in sustaining everywhere else.

Goodness is not the true universal of any good thing, it is the universal not of the thing but of that thing's particular goodness. The true universal of such a particular will include its goodness as something necessitated by the quality expressed in the universal. Similarly truth is not, accurately speaking, the universal of particular truths

§ 454 The idea that the members of a class may be limited in number suggests the question whether there may not be a limiting case, to use mathematical phraseology, of a class with one member or a universal with one particular. Those who speak of such a case as possible or actual probably have not considered whether the account they have given or implied is consistent with anything less than a plurality of members in a class or of particulars of a universal. Certainly one ordinary way of thinking of a universal as an identity in a manifold is not consistent with the idea

If an instance could certainly be given of such a universal, the nature of the universal must show us *a priori*, as we say, that it could have only one particular What universal is there about which we have this knowledge? One may indeed doubt whether a definition of God could be given in which a universal element should necessitate there being but one particular, but for the present purpose we need not ask whether God, or the totality of reality, is one particular of a universal which has no other particular, we must rather consider whether the nature of a universal admits or excludes restriction to one particular. If we allow, as seems reasonable, that the universal can be confined to a limited number, what reason is there for saying the number cannot be less than two? For no universal, not even the universal of a relation, is a relation between particulars which could necessitate at least two of them. The formula that causes our difficulty about the universal having one particular only is that the universal is an identity in its various particulars, so that but for a manifold—at least two—it would appear as if the universal could not be. Yet if it is an identity in a strict sense, it must be in any given particular taken by itself and yet not identified with it If then it were in one particular only, it would still be

'universal' as at least distinguishable from the particularity. It is in this particular what it is and is not constituted what it is by being in any other particular besides the given one. On the contrary it is because it is not identified with particularity that it can be in more than one particular, where it does realize itself in a manifold. We could not indeed recognize it as the same universal in several particulars and identical in them unless we could in a singular instance already so distinguish it. Hence, if it were only in one particular, its universal nature would not be affected thereby. We may illustrate this from the identity of a body in its several positions or from our recognition a second time of a person whom we have seen but once before. In short, what we call the universal is something of such a nature that because of this nature it can be in a variety of particulars. It is not necessary that it should be in more than one, and its nature is not constituted by being in a plurality. It is what it is not because it is in a plurality; it can be in a plurality because it is what it is.

§ 455. Universal then is not the most adequate term for what we have endeavoured to elucidate, for universal suggests identity in plurality and plurality is not involved in its essential nature. The term we inherit from Aristotle is seriously misleading; in his own language it is a definition of a thing not by its essence but by an attribute which is not a property of a *given* universal. If we admit universals with only one particular, the designation 'universal' does not even apply to all universals. In Plato the terminology used does not imply this presence in a manifold. Plato's attitude is often that of one who cannot understand how this unity can be in a manifold and who supposes that this apparent immanence is an illusion.[1] This negative characteristic of his terminology has advantages, but the positive side is only conveyed by help of the metaphorical terms *form* and *Idea*. Again the Greek term 'word', like such modern terms as conception and Begriff, is useless, because it is subjective in suggestion and therefore misleading. Nor is the analogy drawn from the individual thing or person as distinguishable from being

[1] In the *Parmenides* he indicates vaguely a procedure for the solution of the difficulty which is so far right that, had he clearly realized it for what it was, he would have discovered the solution

here or there to be transferred literally to the character of the universal. This latter is not merely to be distinguished from particularity as though particularity were merely spatial, for then its identity would not be distinguishable from the identity of the individual It is distinguished from all particularity. To treat particularity as merely spatial or temporal is indeed a fallacy to which primitive reflection seems to incline.

§ 456. What then is a proper designation? It must be technical and not explanatory, for the distinction being *sui generis* we require it always to understand or explain anything whatever. This is too often forgotten in discussions of the matter now before us. Just so the terms 'particular' and 'individual' are none the less technical because familiar. They are adequate and useful because we know what is meant, we forget their etymology and are not misled by it. But as soon as we treat them as explanatory designations, we are obliged to recur to their etymology and they become misleading. Aristotle has recourse sometimes to the 'this' and 'that' of everyday language, but the practice though convenient is not adequate for several reasons. 'This' and 'that', though they often refer to a particular thing, do not mean particularity; moreover, they are applied to the so-called universals, although incorrectly, and by a transference from their proper use, this colour standing for this kind of colour. Now a universal can always be designated otherwise than by this or that, whereas any particular cannot; even if we appear to do otherwise, we find, on scrutiny, that the particular in question is really designated by a relation to something only designated by 'this' or 'that'. Again this use is so far really subjective because it means the individual which I am pointing to now

In this difficulty the only important thing is to be able to distinguish something, the so-called universal, from its particularization, that is from its being this thing. Whatever we call it we can explain that it is everything that the individual can be said to be besides being merely a this or a that. If an individual is this X, then X is an instance of what we mean by the technical term we are in search of. Or, to put it objectively, X is all that the individual is besides being merely particular.

§ 457. It is repugnant to create a technical term out of all

relation to ordinary language. If we took the term 'a this' or 'being a this', as Aristotle does so often, we might take for the universal 'being somewhat' or 'being such', a use also found in Aristotle. This again corresponds to the Latin term 'qualitas', but it is difficult in English to make 'quality' a technical term for the universal, since then it would have to embrace quantity also. Yet, with this proviso, *quality* seems the most convenient of terms already in use. In distinguishing, however, the particularity from the quality we must guard against making the particularity something which has the quality, for then the quality will inevitably become the particular quality and appear to be the attribute of the particular. Moreover, attributes are themselves particular, the whiteness of a surface being a particular whiteness. Thus our distinction would not give us the true universal, which is 'whiteness'. In this way Aristotle represents the particularity as matter which has the form. That form is the particular quality of the thing and not the universal, it is the particular definiteness of the thing. Yet in the *Metaphysics* the form is often treated as if it were the universal.

Now the particular is not something which has the quality, it is the particularized quality This animal is particularized animality. Particularization belongs, as much as differentiation, to the nature of the quality.

§ 458. We might substitute for individual, or particular, the term existence without danger of being misunderstood. Existence does not suggest an explanation as 'Dasein' tends to do, thus carrying with it a wrong implication. Every existence combines being as this and being as such, and the inseparableness of the this from the such might be represented by 'a this such', or in better English 'a this such and such', which brings us back to Aristotle's language. This avoids the confusions caused by matter and form, where matter is inevitably regarded as having again a form of its own; and if Aristotle had made 'this such' his formula of the existent individual (i e of his composite being), he might, with the help of the conclusion of the *Parmenides*, have given the true solution of the difficulties into which thought had got in his day, as it still does in modern discussions of the universal. The advantage of this formula is that it makes no pretence to be an explanation of what can be

recognized but not explained and that it does not employ a metaphor, which is sure to mislead It indicates directly what is meant, being simply a generalization of the language we use in any particular case, it contains both sides of the existence in inseparable union and has separate words for each side. It has finally the advantage of not defining the quality, the 'such', through its presence in a diversity of particulars.

§ 459 Of late years it has become customary to speak of a concrete and an abstract universal. These are terms to be avoided Concrete was originally merely opposed to abstract and should mean a particular existence Nothing is gained by calling an existence *concrete*, and the term has the danger of seeming to give an explanation. Concrete universal would be a veritable contradiction in terms Again we hear the words 'more concrete' or 'less concrete' used loosely and erroneously Nothing is more or less individual or particular This language arises from a confusion of the differentiation of a universal with its particularization

IX

TRICHOTOMY

LETTERS TO PROFESSOR H. GOUDY

§ 460. ⟨*From Dr. H. Goudy, Regius Professor of Civil Law in Oxford*⟩

All Souls College, Oxford.
7.XII 04.

My dear Cook Wilson,

May I venture to bother you, as a great Aristotelian, with a question? Aristotle was, I think, a believer in the symbolic value of the No 3. Can you say that this belief has affected his classifications of his subject-matter, so as to make them strained and illogical? Or if not in 'il maestro', perhaps you may have found the influence in some of his followers This notion of symbolism seems to me to have curiously affected some of the Stoical writers and, *inter alios*, the Roman jurists.

Perhaps you can put me on a good track for my inquiry, and oblige

Yours very truly,
HENRY GOUDY.

[Oxford, 9th Dec. 1904.]

§ 461. I should be glad to know in what form and from what source the view that Aristotle was a *believer* in the symbolic value of the No. 3 has reached you. In general there is scarcely anything one would think more alien to the sound and sober sense of Aristotle. He had such superstitions before him in the traditions of Pythagoreanism and the vagaries of the later Platonists, and in his criticism of them he shows his usual sanity. He clearly has a contempt for mysterious symbolic meanings attached to number · and in all of his principal works there is no trace of his taking any of them seriously.

If he had attributed any value to a threefold division as such,

one would expect it in his account of Division, or in his remarks upon it in his logical treatises. There is nothing of the kind and no advocacy of trichotomy. On the contrary he gives the sound doctrine that while dichotomy was always possible, it was useless in so far as the negative side not-B admitted no further division from its own notion. True division, he rightly says,[1] must proceed *ταῖς ἀντιδιῃρημέναις διαφοραῖς* (e. g. as when we divide triangle into equilateral, isosceles, and scalene). Not only is there nothing in Aristotle's theory of Division about the value of any trichotomy, but his own divisions, in his writings, do not, as far as I am aware, show any special inclination to it.

§ 462. On the other hand there is in the *De Caelo*, a treatise accounted genuine, a single passage, which as far as I know stands alone in the works of Aristotle about the number Three.[2] He is talking about the three dimensions and is very naturally led to speak of the completeness of threefoldness here 'Magnitude in one dimension (*ἐφ' ἕν*) is the line, in two (*ἐπὶ δύο*) the plane, in three (*ἐπὶ τρία*) the solid body, and there is no other magnitude (i. e as the fourth dimension, e. g.), because 'the three dimensions are all and the threefold division is complete'.[3] For as the Pythagoreans say, the All and all things are defined or determined by the number three. For Beginning, Middle, and End comprise the Number of the All and that is the number of the triad (let three comprise the number of the triad) Therefore accepting this as a law of nature we make use of the number in religious ritual' He continues (showing the real use he makes [at all] of the idea) 'Solid body is the only complete magnitude, because it alone is defined by three (dimensions), and that is all that is possible (in the way of dimension) And being divided in a threefold way it is divisible in all the ways (possible for magnitude).' The last is further explained in what follows. The one dimension is only divisible in one way (i e into length): the two dimensions in two ways . . but the solid only is divisible in *all* ways, for it is divisible in the *three*, which are all, viz. length, breadth, and thickness.

§ 463. I have written out the whole thing (with an exception remedied lower down about 'all three') in order that you may

[1] [Arist., *Top* 143[a] 36 seq.]

[2] *De Caelo*, i 1, init 268[a] 7

[3] *διὰ τὸ τὰ τρια πάντα εἶναι καὶ τὸ τρὶς πάντῃ.*

see how the land really lies. It was the bad luck of Aristotle that certain isolated passages in his works, because they showed some apparent affinity to the fancies of the decadent succeeding epochs in philosophy, were made great texts and their meaning perverted or exaggerated. An instance is the name δευτέραι οὐσίαι ⟨secondary essences⟩ in the *Categories*. This in medieval times was associated (I am told, I haven't verified it) with the view that Universals were Substances, than which nothing was more remote from Aristotle's meaning and beliefs. So also it is possible that the passage before us was misunderstood and made too much of.

§ 464. It is worth while looking attentively into the passage. In the first place we notice that there is *no attempt* to assign a symbolic value in serious philosophical thought to the number Three. Aristotle mentions the sober fact that in *space* three dimensions comprise all possible dimensions and constitute completeness, and that (*in time*) a process is distinguished (defined) exhaustively and completely by its beginning, middle, and end. This also is a mere fact and neither statement has any symbolism about it.

The completeness of the threefold distinction in the case of spatial dimension naturally reminds him of the Pythagorean view that the number three defined the All (the whole process of the world) because it was the number of the exhaustive distinction into beginning, middle, and end. And it is noticeable that even here he does not quote any of the fantasies of Pythagorean symbolism (e. g. the mysteries of the τετρακτύς, but something which, as far as it went, was right. One might paraphrase him thus: 'there is a sort of completeness about the number three in regard to space, for three is the whole of all possible dimensions. So also the Pythagoreans noticed a similar completeness about Three in respect of the process of time, for that is all comprised in beginning, middle, and end.'

§ 465. But Aristotle does not argue from a general idea of the perfectness or completeness of *three*—he neither says 'because it is the complete number, it is the number of the dimensions', &c., nor does he propose to argue from its completeness in general to conclusions in other spheres. And there is *no trace* of a view that every division should be trichotomic. For

symbolism he only refers to religion and clearly only in the way of showing how the observed completeness of the number three in time and space distinctions naturally suggested a mysterious use of it in religion—which any modern philosopher might have said

So also in the context immediately following what he says about the religious use as depending upon the observing of certain striking cases in Nature, where every possible alternative was comprised in three, he goes on (less happily) to connect it with an ordinary use or idiom of language (Greek, ⟨so⟩ it is in English but not ⟨in⟩ German or French) whereby we do not attach *all* to *two* and say *all two*, whereas we do say *all three*.

I dare say the passage was purchase (sic) enough for medieval fancy and may be the whole source of what I believe to be a mere illusion—the view that Aristotle assigned symbolic value to the number three, and I should be glad to know where you find this view

§ 466 I may repeat that I believe this passage to be the only one upon which such a view could be founded, and I may shortly put the gist of it once more shortly thus: 'The solid is magnitude, in the complete or perfect sense, for it alone has every kind of dimension, for it has *three* dimensions and *three* here comprises *all* This reminds us of the importance attached by the Pythagoreans to the number three, for they emphasized the fact that three is [1] the complete number of the distinctions of temporal process (cosmic) It is this observed completeness of the threefold division, in certain great natural facts, that has made the number three a natural religious symbol: and it is perhaps due to the same thing that we find it natural to say 'all three', whereas we never say 'all two'.

I may repeat that I know of no trace in Aristotle's philosophy of an affection for a threefold division as such.

§ 467. It is significant that in Simplicius' comments on the place (Simplicius mentions Ptolemy, VII 33 (Kunster's reference), and is assigned to the sixth century A D) two misunderstandings ⟨occur⟩, just of the kind one might expect in a late writer. He interprets διὰ τὸ τὰ τρία πάντα εἶναι καὶ τὸ τρὶς πάντῃ as if it meant complete magnitude had three dimensions 'because three in

[1] MS 'was'

itself was a *complete* number'[1] (the meaning is really what I have given above), and so gets himself into a difficulty and asks whether a man ought not to have three fingers if three was a perfect number![2] He erroneously supposes Aristotle is arguing from Pythagorean principles: whereas, as I have shown, he does nothing of the kind. Here again Simplicius gets himself into a difficulty, since he knew it was not Aristotle's way to argue from Pythagorean principles, and so he says 'it is worthy of notice that Aristotle here, contrary to his wont,[3] is using a Pythagorean proof!' The interpretation of the words misunderstood by Simplicius is put beyond a doubt by a comparison of 268[a] 8 above[4] and 268[a] 24 seq. below.[5] As he explains in the latter place, linear magnitude in one dimension is divisible only in one way (i. e. into lengths) and the surface, on the other hand, is divisible only in two ways (i. e so many units in its length and so many in its breadth), whereas body is divisible in three ways (*τριχῇ*) and so is the only one divisible in all ways, since three ways is all possible ways. Thus in 268[a] 10 *καὶ τὸ τρὶς πάντῃ* has exactly the same meaning, the division of magnitude in respect of three different ways (length, breadth, and thickness) is a division in all possible ways

[Undated.]

§ 468. Dichotomy[6] seems everywhere possible, because we can divide a class into the members which have a certain quality and those which have not (I shall raise the question of the value of this presently). There seems no such general ground for trichotomy, so as to make it applicable everywhere. But can we find any general reason for it or principle of it where it is applicable, and so determine certain general characteristics which the subject-matter to which it applies must have?

Suppose two species of a genus are related as 'contraries' and are the species of a given genus furthest removed from one another according to the *same* principle of order.[7] If there is

[1] 6[b] 35. [2] *παρὰ τὸ σύνηθες.* [3] *τέλειος ἀριθμός*, 7[b] 19 seq. [4] *σῶμα δὲ τὸ πάντῃ διαιρετόν.* [5] *τριχῇ δὲ ὂν διαιρετόν, πάντῃ διαιρετόν.*

[6] [Dr H Goudy had sent Wilson his article 'Trichotomy in Roman Law', with a request for criticism.]

[7] Aristotle defines *ἐναντία* as *τὰ πλεῖστον ἀλλήλων διεστηκότα ἐν τῷ αὐτῷ γένει.*

one class 'between' them and with affinities to both, we get a tripartite division as of triangles into

those which have *all* their sides equal (equilateral),
those which have *none* equal (scalene), and
those which have *two* sides only equal (isosceles)—the latter species being intermediate between the other two. But this does not necessarily apply when there are two 'extreme' species, for it is not necessary that there should be only one species intervening. E. g. we may apparently divide four-sided figures into three:

(1) those with *all* sides equal,
(2) those with *no* sides equal, and
(3) those with *some* sides equal.

But the third species divides into those with

(1) 2 sides equal and the remaining two unequal,
(2) 2 sides equal and the remaining two equal to one another but not equal to the first two,
(3) 3 sides equal.

And the co-ordinate division is therefore into 5, there being three species intermediate to the extreme species.

Such tripartite division therefore, which is based on there being two 'extreme' species, depends entirely on the subject-matter and is not of universal application even to the cases which present 'extreme' species.

§ 469. Another trichotomy which has the look of generality is that of which masculine, feminine, and neuter, ⟨in our⟩ [1] classification of species of certain animals (e. g. bees and ants), is an example. Here the members of a genus are classified according to the presence or absence of a quality (e. g. sex), and the case of presence shows two species (e. g male and female) only, which are opposed to one another. Clearly this is not of general application to the cases where a class can be divided according to the presence or absence of a quality, for it is not necessary that the presence of the quality should give rise to two species only, like male and female. E. g. conics are divided into those which have a centre and those which have not. If we confine ourselves to real curves (excluding, i. e., straight lines as limits of conics) there is only one species without a centre, the parabola.

[1] MS 'or'

But there are three which have a centre, the circle, the ellipse and the hyperbola.

§ 470. Thus the possibility of trichotomy depends on the subject-matter entirely and (1) there is no general principle of trichotomy applicable to all subjects, like dichotomy, (2) there is no general reason or principle of it for all cases where it *is* applicable: no general characteristics which all the subject-matter to which it is applicable must have and which always determine that the division must be tripartite.

§ 471. But we may go further. In dichotomy there is apparently a general principle of division into *two*: and one applicable to all matter. This is an illusion. If we divide X into A and not-A, the negative (mere) designation is indeterminate, cannot determine a class. There 's nothing to prevent the negative not-A from having sometimes several true classes corresponding to it, which are co-ordinate with A, so that the division of X into A and not-A should really be, e. g., into A and B and C.

The negative *may* have one class corresponding. Thus number is either even or not-even, and not-even = odd. But this depends again on the subject-matter The mere negative *not-even* cannot determine a class, that is due to the positive character of number dividing itself into even and odd, both positive.

The mere negation, without thus corresponding to a positive designation, may seem to determine a class in such an example as the division of cats into those which have tails and those which have not. The general account of this is that X being divided into A and not-A; XA really = B+A and X not-A = B. Thus X not-A is really defined by the quality B and X is divided into that which is B and A, and that which is B only.

Thus there is not even a *general* principle which would yield a true dichotomy or division of a genus into two co-ordinate species. And there is no general principle which would yield a division into any definite number of species, whether 2 or 3 or more. The number must depend solely on the nature of the special subject-matter.

§ 472. This brings me to Hegel, for Hegel's principle does seem to lead to a threefold division, and that would appear to

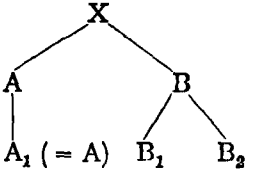
conflict with my statement above. But the truth is that Hegel's threefold division is not a division of a genus into species at all. The Hegelian triads, in fact, are *not* an example of trichotomy. He supposes that if you take a conception and then its contradiction, there arises a third conception in which they are united or reconciled E. g. unity and plurality are thus combined or reconciled in totality. Now this is not a classification of members of a genus into 3 mutually exclusive species One and the same thing shows the unity of a plurality of parts or elements in a totality. There is here no question of the distinction of a genus into species. So also with the combination of Being and Not-Being in Becoming Not only is Hegel not properly then a trichotomist, but I doubt whether it is at all fair to Hegel to say that he was affected by any merely numerical influence or superstition His method is not founded on any respect for the number three or for a threefold division. But his principle of the unity of contradictories, as above explained, necessarily produces these triads of conceptions I am not here defending his principle but only pointing out that it is founded on no consideration of number as such, though it gives rise to these groups of three

§ 473 Kant would be under more suspicion than Hegel, for each of the 4 classes into which he divides judgements (as to quantity, quality, relation, and modality) contains three members. In three of these he might be said to find a threefold division (*Quantity* into Universal, Particular, and Singular; *Relation* into Categorical, Hypothetical, and Disjunctive; and *Modality* into Problematical, Assertorical, and Apodeictical). But in *Quality* he will only find Affirmative and Negative. So one suspects his addition of the Infinite, which is clearly artificial, was due to the wish to make a threefold division of the class, corresponding to the threefold divisions in the other three classes. But even this would not be due to a respect for the number 3 but to a respect for symmetry. When Kant had got, very artificially, his 12 Categories out of his logical distinction of propositions, he discovered that, in some of the 4 classes, the 3rd Category was the union of the 1st and 2nd. He again, artificially and affected by symmetry, supposed this true of the 3 categories in all the four groups, though it clearly is not the

case. It was this unity of two in a third which suggested Hegel's principle. But while Kant does not arrive at his categories from it, but observes the peculiarity in them when found, Hegel finds his system of categories by means of the principle.

§ 474.[1] You think there is here 'the most patent disregard of logic'. I must here join issue with you on the logical point. Your statement as it stands would condemn a procedure which is perfectly logical and may only appear not to be so through a difficulty which is mainly verbal.

Suppose we have a class X, which is divided into two, A and B, and the class B is divided into two, c. g. B_1 and B_2

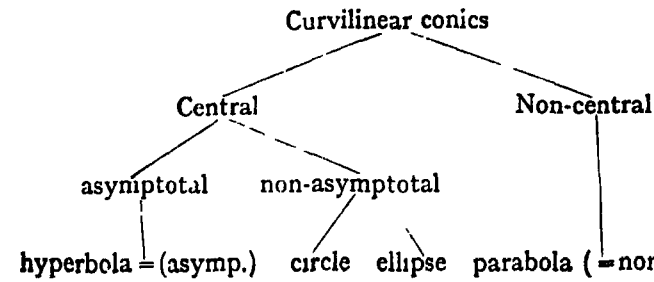


B appears as a *genus* in relation to its divisions B_1 and B_2 as *species*, and A, being co-ordinated in the first instance with B, may seem also a genus in contrast with the species B_1 and B_2 and therefore improperly co-ordinated with them as species of X. But there is a case in which this is but a verbal fallacy and the co-ordination is quite right, and it looks, too, as if this were the case before you Suppose while B is thus capable of further differentiation, A is not. A, then, is as much differentiated as B_1 and B_2; or, if you like, X is as much differentiated in the direction of A, in A itself, as X is differentiated in the direction of B, in B_1 and B_2. And, if you ask the question what are the *infimae species* of X, the right answer is A, B_1, and B_2.

It is in fact the case where there is only one species of A, which coincides therefore with A itself In the above figure A_1, the species of A, quite co-ordinate with B_1 and B_2, is equi-

[1] [*A note to Dr H Goudy on an extract from the 'Digest'* 'Conventionum autem tres sunt species, aut enim ex publica causa fiunt, aut ex privata ⟨privata⟩ aut legitima aut iuris gentium' Ulpian, libro IV, *ad Edictum. Dig* II XIV 5]

valent to A. There are plenty of instances in the sciences where A_1 (= A), B_1, and B_2 are thus co-ordinated, e. g.:



Here the point is illustrated twice over. In the division of central conics into two, asymptotal and non-asymptotal, the latter being *genus* to the *species*, circle and ellipse, asymptotal would (as you put it) seem like a genus in contrast with them because co-ordinated with their genus But its one species is the hyperbola, which therefore coincides with it, and the hyperbola is always co-ordinated in a division of conics with the ellipse and the circle, and rightly. Further, non-central curvilinear conic has only one species, the parabola, and this again, though non-central as co-ordinate with central (which is *genus* to the *species* circle and ellipse), seems like a *genus* in comparison with these latter But having but one *species*, with which it coincides, it is rightly co-ordinated with hyperbola, circle, and ellipse These forms are called *the* conic sections (curvilinear), and quite rightly, because each represents the functional differentiation of the *genus* conic section which is relevant to geometry

The consequence is that in the division of conventions into *ex publica causa* and *ex privata causa*, if there are no differentiations further of the former parallel to those of the latter (and from the silence in your text one would certainly infer there were not), then the former is rightly co-ordinated with *ex privata legitima* and *ex privata iuris gentium*, and the logic ⟨of Ulpian⟩ is quite flawless

§ 475 The example of the conic sections is a true example of the classification in which one member is co-ordinated with a true alternative member of division and yet co-

ordinated as *infima species* with the *species* of that alternative member.

There is an incorrect way of producing the same thing in appearance. We may take a species X_1, which is co-ordinated with others, X_2, &c., in an immediate differentiation of one element, say X.

Then divide this X into X_1 and not-X_1, and not-X_1 into X_2, X_3, &c. So that X_1 is co-ordinated first with the *genus* not-X_1 and then with the species of it, X_2, X_3, &c.

But this is obviously a merely verbal device and the class not-X_1, defined by the mere negative of X_1, is not a true class at all.

The fallacy here would not lie in co-ordinating X_1 as a *genus* with X_2 and X_3 as *species*, but in co-ordinating X_1 with not-X_1 as both *genera*. An instance would be dividing triangle into right-angled and oblique-angled (= not having a right angle) and then dividing oblique-angled into obtuse-angled and acute-angled, so as to co-ordinate right-angled first with oblique-angled and then with the species of the latter.

§ 476. ⟨The following is a full account of the true case illustrated by the conic sections [1]

A universal A has, e g, two elements, X and Y. A = XY.

The differentiation of A according to X produces A_1 and A_2, which are of the form X_1 and X_2 and are truly co-ordinated, because the differentiation of X can go no further in either of them.

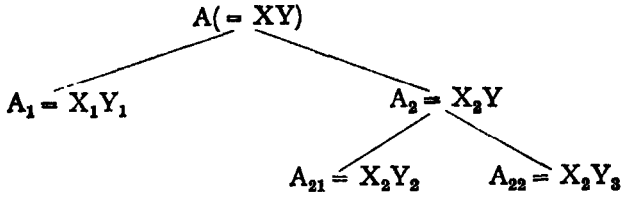
But the connexion of X and Y in A is such that the differentiation of A into X_1 involves necessarily the differentiation of Y at the same time into Y_1, so that $A_1 = X_1Y_1$.

Here A_1 is necessarily an *infima species*, both elements in the universal X being differentiated.

But the connexion of X and Y in A may also be such that the differentiation of X into X_2 does not carry with it the differentiation of Y, so that $A_2 = X_2Y$.

Then A_2 is not an *infima species* and is divided into such by the differentiation of Y. Thus

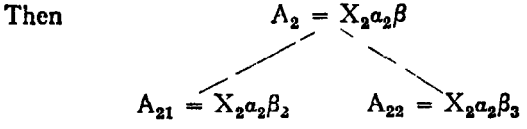
[1] [This was added later, I think, by the author to his copy of his letter to Goudy He has marked it as an addition to the logic lectures (Part II, ch 16) and noted that they are, where necessary, to be corrected accordingly]



Thus $A_1 = X_1Y_1$ is rightly co-ordinated in the differentiation of X with $A_2 = X_2Y$ and rightly co-ordinated as an ultimate differentiation of A, or as an *infima species* with A_{21} and A_{22}.

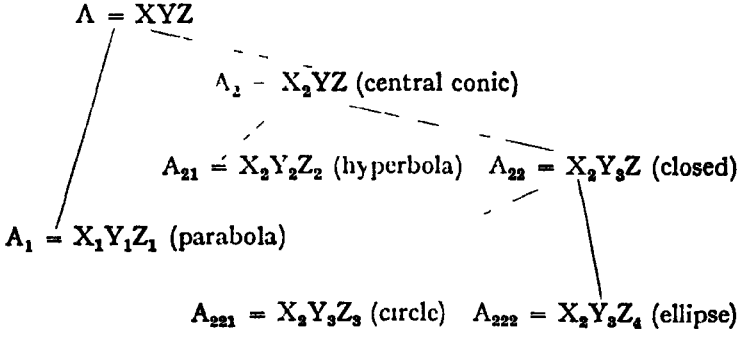
The same kind of thing happens if X_2 involves the differentiation of some element or elements in Y, but not of all.

Let $Y = \alpha\beta$, and let $A_2 = X_2\alpha_2\beta$, because X_2 involves the differentiation of α into α_2



We may get more steps, if there are more elements in A.

Thus if $A = XYZ$, we may get steps corresponding to the above classification of conic sections For suppose that X_1 necessitates Y_1Z_1, and X_2 leaves Y undifferentiated, while Y_2 necessitates Z_2, and Y_3 leaves Z undifferentiated:



So that A_1, A_{21}, A_{221}, and A_{222} are co-ordinate as *infimae species*.

Note also that, in strict accuracy, the language used above (pp. 723–4) is not admissible. A ought not to be represented

as a *genus* with only one *species* (A_1 which coincides with it) and similarly for the further concrete instances. In the fuller symbolism A_1 ($= X_1Y_1Z_1$) is a species of A ($= XYZ$), just as much as A_{221} ($= X_2Y_2Z_1$). It looks like a genus, only because A_2, with which it is co-ordinated through the division of X_1, is a *genus* with regard to the *infimae species* A_{21}, A_{221}, A_{222}.

Strictly, a genus with one species is a contradiction in terms and it is only allowable if it means a genus which *now* has only one existing species, though it had others once For example, if all species of a kind of plant have died out except one. But this case can't occur in the mathematical examples)

Note to § 462 [*καθάπερ γάρ φασι καὶ οἱ Πυθαγόρειοι, τὸ πᾶν καὶ τὰ πάντα τοῖς τρισὶν ὥρισται· τελευτὴ γὰρ καὶ μέσον καὶ ἀρχὴ τὸν ἀριθμὸν ἔχει τὸν τοῦ παντός, ταῦτα δὲ τὸν τῆς τριάδος. διὸ παρὰ τῆς φύσεως ἐιληφότες ὥσπερ νόμους ἐκείνης καὶ πρὸς τὰς ἀγιστείας χρώμεθα τῶν θεῶν τῷ ἀριθμῷ τούτῳ . . . τὸ σῶμα μόνον ἂν εἴη τῶν μεγεθῶν τέλειον· μόνον γὰρ ὥρισται τοῖς τρισίν.* (Aristotle, *de Caelo*, 268[a] 10 seq.)

'For as the followers of Pythagoras also say, the whole and the all is determined by "threes". For, they argue, end and middle and beginning embrace the number of the whole, and that is the number "triad". Accordingly we receive from Nature, as in a sense her laws, this number and use it for the worship of the gods . . Body alone of magnitudes would appear to be complete, for it alone is determined by three.'

St. Thomas Aq. remarks on this passage: 'Quod ergo Aristoteles dicit per hunc numerum adhibuimus nos ipsos magnificare Deum unum, eminentem proprietatibus eorum quae sunt creata, non est sic intelligendum quod ipse poneret ternarium numerum in diuinis Sed uult dicere quod antiqui utebantur ternario numero in sacrificiis et orationibus propter quandam ternarii numeri perfectionem.' *S. T.* I, qu xxxii, art. 1, ad primum. This shows that the passage had been used in controversy in the thirteenth century. There is a curious reference to Aristotle's supposed doctrine in the *Port Royal Logic*, Part III, ch. xix, § 3 (Non causa pro causa)

(It was usual in Greece to invoke the gods in threes, not in pairs; thus in the *Clouds*, Socrates swears by Void, Clouds, and Tongue (424), and again by Breath, Void, and Air (627), but the conscientious usurer by Zeus, Hermes, and Poseidon (1234).)

Cicero criticizes Epicurus for a mistaken trichotomy, *De Fin.* ii. 9. Hegel ascribes to Kant the credit of drawing attention to the predominance of trichotomy in the sphere of the mind. *Logic*, (Encyc.) § 230. Cf. Kant, *K. der Urth.*, Introduction, § 9 (note).]

X

CORRESPONDENCE WITH B. BOSANQUET

§ 477.

12 Fyfield Road,
7th July, 1903.

My dear Bosanquet,

I am returning your 1st six pages and am sending also some new notes on the subject, as I do not think you quite realized my points. If I have got wrong, I trust you will be as frank as you wished me to be Error in these childish and verbal (for that 's all they are) subtleties is exasperating, for it tends to make these abject trifles seem important—as doubtless they seem to the writer you wot of so I shall only be too glad to have any mistake I have made put as clearly and *pointedly* as possible. Though I don't pretend to think I have made any serious error, I know one 's never safe

What I feel is that these are simply difficulties for minds that do not realize accurately the nature of grammatical distinctions, and it makes me very angry that they should masquerade as philosophy.

There are difficulties coming from a want of realization of the fact, on the part of the logician, that he is dealing mainly with a linguistic (grammatical) question with which one has more sympathy—e g the whole confusion of the stock distinction (and its treatment) of the connotation and denotation of terms.

But *this* stuff makes one feel why Plato and Aristotle hated the quibbling eristic with such a holy hatred. They must have felt it so cheap and been so vexed that it could attract attention and be mistaken for clever thinking.

As to the papers on Royce (by the way I daresay you would like them back. How long may I keep them? You needn't return what I am sending you this time as I have taken a copy of the essentials—just as you like) I would rather write later on *that*, when you have had time to consider what I am now sending.

I will just say that though not quite ready yet to take the field upon the whole 'ketten' business on which R.'s puzzle seems to depend, I have just a little to say on the fallacies to which distinguished mathematicians are committing themselves in our day, which may interest you.

I feel sure we shall get on, because I am attracted by what I venture to think the grasp of such matters as 'infinite number', &c., shown in your *Logic*.

Will you however in the meantime when you next write tell me what you yourself understand by a *true* and a *false* infinity, and what you suppose Royce to mean by them. I observe that reflective mathematicians are beginning to speak of *true* and *false* infinity: but I don't know where they get it from nor am ⟨I⟩ altogether sure I know what they mean by it. It can hardly be Hegelian (can it?), for would Hegel's idea of the 'infinite', as the non-finite and as therefore something whole and unlimited by anything else and therefore properly only *the* whole, be applicable at all within *quantity* (mere)?—and of course it 's only of quantity that these mathematicians are speaking (except, as Caird points out to me, that the formula which covers the whole infinite series might be allowed by Hegel as the truest approach to infinite in quantity).

May I add I like controversy with a single person? In a general talk of several I find I am too inclined to be afraid of being shown to be in any way wrong, and to fight for my hand. But that sort of feeling hardly exists, if at all, when discussing with no 3rd person to hear, and I like the clearest and most unsparing statement

Yours truly,
J. Cook Wilson.

§ 478. The first three pages[1] are a little difficult to deal with because the objection does not seem clear. It mentions something about the difference between the class of classes and ordinary classes which is on the whole true but seems irrelevant as an objection. Besides you interpose a criticism on it yourself in which you abandon an important statement, so that it is not perhaps easy to see what really remains.

[1] [Notes appended to letter of 7 vii 03]

I venture to imagine that I see what the matter is because something similar happened when I read a paper like that sent to you to our small society Isn't it that you began a little too hastily—you observed I was talking of a class the 'members' of which were not, or not necessarily, themselves groups or classes; whereas it is distinctive of the class of classes to have groups unified by a common element (or notion) as its members.

When I read my paper and had completed the account of the fallacy applied to *any* class whatever, and its refutation, one of my hearers at once objected that the 'class of classes' had the above difference from an ordinary class. I simply begged him to observe that I of course knew the supposed characteristic of the class of classes, and asked him to be content at present to judge whether I had proved my first point or not, viz. that the paradox enunciated of the class of classes, that it was a member of itself, could be established, on the line of the paradox, of any class whatever and was not a privilege of the class of classes—and also that I had stated its refutation

Then he admitted this (as every one else did), and then I asked him to hear me out for I had provided for the class of classes. He admitted when he had heard the whole thing to the end that I was right. And I really think you began just as he did, whether you will end as he did is another matter

I had really written a longish piece upon membership of a class and the special character of membership of class of classes, which I spared them and you, because it seemed to me one could shortly and neatly refute the fallacy in the way I proposed. If I had sent that, you would have seen I was quite awake to your consideration.

I had better state exactly what my method is in order to show the irrelevance of the objection (or half objection).

§ 479. The paradox about the class of classes is that being like the classes subordinate to it, in so far as it is *a* class and *they* are each *a* class, it has a common element with them (being a class) in virtue of which it is *classed* with them, i e —a member of a class of which they are also members

Now I undertake to show that it is a mistake to suppose such a procedure applies only to the class of classes, and therefore

the paradox or supposed bit of metaphysics (I I) was not due to the wonderful subtlety of making a class of classes, but belonged to any class, however ordinary, common, or unclean.

1. I proved I think rigorously that, by the *method* of the paradox, [that] the paradox could be shown true of any class—taking the quite general symbol X for any class of things which are X.

Now on page 4 you say apparently that you doubt whether without *much forcing* I could apply the paradox to any class. This puzzles me extremely—unless I may suppose that in hastiness you merely thought of the aforesaid difference of the class of classes and my X class, without first going through my demonstration. It seems to me if you would look at that, you wouldn't find the slightest forcing about it. It seems easy and simple and I certainly think I have shown that to any class the paradox could be applied and you haven't anywhere indicated any flaw in the demonstration.

§ 480. 2. (*a*) I showed that, without making any special provision for the 'class of classes', what I said of a class in general could be applied immediately to the class of classes—syllogistically. For *that* it was only necessary to make a certain substitution. For Xness substitute simply 'being member of a class'. I didn't begin this way, but showed *independently* how the paradox was developed in the class of classes and what was its refutation But it struck me, after I found the *general* application of the paradox, that it was quite easy to represent the 'class of classes' case as a particular case of it: and it was important to do so as the full nature of the fallacy is the better elucidated. Besides it helps to show that it *is* a mistake to suppose the whole thing a unique characteristic of the class of classes.

(*b*) I gave, what I put rather shortly at the end of the paper sent to you, a treatment of the class of classes as such, and without bringing it under the general theory as in (*a*).

In this way I satisfied my objector (who is one of the acutest people here) because I not only treated the class of classes on its own merits—with special attention to its nature as having *classes* for its members, which was what he wanted, but I also

gave him a new view of it which hadn't occurred to him (or to any of my hearers) by showing the analogous thing in *any* class whatever. I attach importance to the last, because clearly the people who advertise the paradox imagine it due to their clever 'class of classes'.

(By the way it is a mistake to suppose—if anybody does—that the class of classes is the only class which has classes for its 'members'. I shall come to that presently.)

§ 481. Let me now return to the first part of my argument. Perhaps I put it too shortly (a habit coming from my smattering of mathematics) but I stated essentials, and thought it could be easily worked out.

I will now detail it fully—or as fully as necessary. *First* we may show that, if W is the class of classes, [that] it is rightly represented as 'the totality of things which are members of classes', and that the general characteristic of the class W is rightly put as 'being member of a class'. (Of course I do not deny that we should naturally at first put 'classness' in this general notion, and indeed that is right enough, but it doesn't suit my present purpose And the notion I have chosen is also right from another point of view, and is the point of view which suits my purpose.)

W being the 'class of classes'.

Let the classes subordinate to it be A, B, C, &c.

Now each of these classes has its own members unified by a universal, A by α (say), B by β, C by γ, &c —so that A consists of all things which are α; B of all that are β, &c, &c.; but for the purpose of the 'class of classes' we abstract α, β, γ, &c., and regard A, B, and C, &c, &c, only as groups unified by some principle or another (= classes)

Thus the members of each group A, B, C, &c., &c., have the common designation of being 'members of a class'.

W is the totality of the groups A, B, C, &c

∴ W is also the totality of the members of these groups.

∴ W is the totality of things which are *members of classes* and the generic conception corresponding to W is 'being a member of a class'.

The relation of the general case to the class of classes may now be shown in parallel columns:

Let X be any class whatever, of which the class conception is x.

Let W be the 'class of classes'. Then, as above, the class conception is 'being a member of a class'.

X then consists of things each of which is an x.

W then consists of things each of which is a 'member of a class'.

Let A, B, C, &c., be classes subordinate to X.

Let A, B, C, &c, be classes subordinate to W.

A is a group of things each of which is an x (as well as being an α—α being characteristic of the sub-class A).

A is a group of things each of which is a 'member of a class' (as well as being an α, α characterizing the class A).

So B is a group of things each of which is an x, &c., &c.

&c., &c.

So B is a group of things each of which is a 'member of a class', &c., &c.

&c., &c.

X is the totality of the groups A, B, C, &c.

W is the totality of the groups A, B, C, &c.

∴ X is the totality of the members of A, B, C, &c.

∴ W is the totality of the members of A, B, C, &c.

X then is *the* totality of things which are each x

W then is *the* totality of things which are 'members of classes'.

This latter statement is then erroneously put in the form ·

The latter is erroneously stated thus .

X is *a* totality of things which are x.

W is *a* totality of things which are 'members of classes'.

The mistake of substituting the indefinite for the definite article being made, the fallacy proceeds thus .

X being 'a group of things which are x' is exactly like, in this way, each of the groups A, B, C, &c , for each of them is 'a group of things which are x'.

W being a totality (or group) of things which are 'members of classes', is thus exactly like each of the groups A, B, C, &c , for each of these is '*a* group of things which are members of classes'.

It is therefore like them a *member* of the class defined by the general quality com-

W is therefore like them a *member* of the class defined by the general quality com-

mon to A, B, C, &c., of 'being a group of things which are x's'.	mon to A, B, C, &c., of 'being a group of things which are members of a class'.
But the class so defined, being the totality of groups which are 'groups of things which are x's', is exactly the class X itself.	But the class so defined, being the totality of groups which are 'groups of things which are members of a class', is exactly the class W itself.
∴ X is a member of itself.	∴ W, the class of classes, is a member of itself.

This I think proves the exactness of the parallelism. I suppose I need not add the refutation in parallel columns; it may be easily shown that the refutation I gave for the X class applies with the same exact parallelism to the W class.

I will come to the treatment of the 'class of classes' by itself—taking 'classness' as its general conception instead of 'being member of a class'—presently.

§ 482. Now I want to add the proof that *any* class whatever may be treated as *a* class of classes, i. e. as a class of which the members are themselves classes. This I think has the additional interest of making clearer the meaning of membership of a class.

Consider the class of *rectilinear polygons*. Take as its general characteristic 'polygonal' The species are triangle, quadrilateral, pentagon, hexagon, &c An individual triangle, as triangular, is a member of the class triangle.

As polygonal it is a member of the class 'polygonal', then the members of the species 'triangle' as 'triangular' are the same as the members of the genus 'polygonal', and the members of the genus are the same as the members of the various species.

The *class* 'triangular' is *not* a member of the genus 'polygonal' simply because *it must be a definite individual figure which is polygonal*. The class triangle cannot *as* a totality have the predicate 'polygonal'. Thus the species 'triangular', 'quadrilateral', &c, are *not* members of the class polygonal.

Consider the class triangle as the total of all triangles, it is thus *the* class consisting of all triangular figures and it is also *a* class consisting of polygonal figures. Similarly the class

pentagon is *a* class consisting of polygonal figures. Taken in this way each of the species, triangle, quadl., pentagon, &c., is *a* class consisting of polygonal figures.

As having the general predicate 'a class consisting, &c.', they come under the genus 'class consisting of polygonal figures' of which they are true members. Thus if we look at the class of rectilinear polygons, as we may, as the class whose class *predicate* is '*a* class consisting of polygonal figures' and ∴ as the class consisting ⟨of⟩ the various classes of polygonal figures, i. e. as the totality of classes consisting of polygonal figures, it has for its *members* now *not* individual figures (for no individual figure is a class, i. e. can have the *predicate* 'a class of polygonal figure') but the species or classes 'consisting of triangles', 'consisting of quadls.', &c.

Class X with *generic* predicate '*a* class consisting of polygonal figures'.

class triangle.	*class quadrilateral.*	*class pentagon.*
specific predicate, 'the class consisting of triangles', generic predicate, '*a* class consisting of polygonal figures' &c.	specific predicate, 'the class consisting of quadrilaterals'; generic predicate, 'a class consisting of polygonal figures' &c.	specific predicate, 'the class consisting of pentagons'; generic predicate, 'a class consisting of polygonal figures' &c.

Thus *any* class *whatever* may be exhibited as *a* class of classes, i. e. as a class of which the members are classes and not individuals (and you can easily repeat the fallacy for this class, i. e. *the* class consisting of polygonal figures).

§ 483. I venture to hope you will find that the above completely answers the difficulty you have raised on page 4, not that the addition of detail I am now sending is necessary; for it is contained in what I sent you at first. You say in short you don't see how, in an *ordinary* class, groups can be members of the class—and how ∴ the paradox could be applied to any class. Well, if you had looked carefully into my demonstration about the X class—which I have repeated in parallel columns above—you would have seen I had proved that the paradox *could* be applied to any class, and so that your objection was

either irrelevant or incorrect in itself. But if you had then examined the implication of the argument you would have seen that it *does* show any class whatever can be so represented that its species appear as its *members* (and *not* the members of the species themselves). For the classes A, B, and C appear there as each '*a* group of things which are *x*', which makes them true members, and the only possible members, of the general class determined by the generic predicate 'a group of things which are *x*'. And the general class of which they are the true members is obviously coincident with X.

What I have added now is simply the illustration of the polygons to make the thing clear, and exhibit the general principle apart from its appearance in my other argument.

I thought after all I needn't take the separate case of the general class of all classes over again by itself, because you seem to agree with me—for you say your criticism (middle of your third page) comes to the same thing.

§ 484. But now frankly—you wish me to be frank—I do not think your treatment is clear or satisfactory (merely, I think, because you were too hasty over it) (i) It is encumbered by what seems to me untenable, viz that the species cannot be represented as members of the genus, i e as classes members of a class I have tried to show how this is really true after all. (ii) I think it important to keep these questions about classes and their members quite apart from the relation of a 'concrete' whole like a rhomboid to its parts or of a square to its component squares Such comparisons are liable to lead to much confusion, and so I think it a pity you should work your criticism (p 2, bottom, 3, top), which you think comes to mine, through these dangerous analogies More especially as it seems to involve you in something untenable (p 2, bottom): for the whole class is in *no* sense co-ordinate with its members: and this leads to my third difficulty in this part of your paper. (iii) Page 3, middle, you seem to me, frankly again, to slip back into the fallacy yourself. 'The class of classes *could* be looked at, if it interested one to do so, as "a class" and so co-ordinate with all other classes.' On the contrary I should say this can't be done, and indeed that the simple account of the fallacy is that it consists in representing *the* class as *a* class—a mere

fallacy of confusion of the two articles. The essence of the argument of mine with which you profess agreement is that we can*not* regard the class of classes as *a* class: 'a class' is an adjectival predicate, and as I have said, we have to see what such adjectival predication exactly means.

§ 485. In these linguistic puzzles I think we must not only hold strictly to the normal meaning of the language, but substitute *symbols* for the words likely to deceive and then interpret our result back again.

The class of classes is *a* class, is of the form 'Y is *a* Z'.

'Y is *a* Z' strictly means that Y is *one* of the things in which Z is realized or realizable; and it does *not* mean that Y is the only thing in which Z is realized, i. e. does *not* mean that Y is the total realization of Z. It means, then, that Z is realizable in things *other* than Y, in the strict sense that they are not *included* in Y.

These *other* realizations of Z are said to be co-ordinate with Y, and this is the proper meaning of co-ordination: they are members of the class as each is *one* realization *among* others, and that is the meaning of membership of a class, which must not be considered as merely *identical* with the possession of a predicate (this answer to one of your questions I will return to further on).

Now to translate back: make Z = class, then Y is a Z = Y is a class.

By the above this properly can only mean that Y is *not* the total realization of 'classness': that classness is realizable in classes other than Y which are not *included* in Y.

Thus in the proposition 'Y is a class', Y cannot possibly be the 'class of classes'; for Y as the totality of classes *is* the total realization of 'classness', and 'classness' is *not* realizable in anything not included in Y.

Thus the expression the 'class of classes is *a* class' is simply false. If it were true it would necessitate the co-ordination (as the above shows) of the 'class of classes' (Y) with classes *not* included in it.

Thus the paradox is shown to be a mere verbal paralogism, for it all depends on the false proposition, 'The class of classes is *a* class'.

The above while directed to a remark of yours is also the promised separate treatment of the class of classes making X = 'classness'.

§ 486. You ask (p. 1), after a correcting sentence, 'What *is* membership, as distinct from possessing the predicate?' To all questions of the form 'what is A except B?', or to statements of the form 'A *only* means B', I should answer that their insufficiency is shown by taking them strictly. If A *only* means B, then we need never mention A at all again: substitute B for it everywhere, and strike out A. In any concrete instance the result of the substitution is enough to convince, e. g. 'A straight line is only (means only) the shortest distance between two points'. Very well—never use the word 'straight' again and see what happens How, for instance, should we feel when we came to the word curved? In fact the statement 'A *only* means B', is only of any interest because A *is* somehow distinct from B, not as a mere word, but as a meaning A and B are different, but yet they are inseparably connected, and this latter is confused with their identification.

Thus in the present instance 'possession of a predicate' (adjectival predicate) *necessitates* in the way above shown membership of a certain class, but is not to be merely *identified* with it.

Obviously, when in experience we observe that a particular Y has a property Z, in general we only think of the particular Y having the particular property—we do not think of it as a member of a class, or think of other objects as possessing the same property. Of course our judgement necessitates that Y is so classed, and on reflection we may realize this.

But the answer to your question doesn't affect the other issues

§ 487. The puzzle you give on p 6—which was apparently taken *seriously* on the authority of your I C S. man—is only a corollary of the general fallacy explained above, and may be easily refuted by help of the foregoing consideration. The statement of alternatives 'everything is either a member of itself or is not a member of itself' is altogether incorrect, and in it lies the fallacy. The correct statement is.

Every class is either.—

(1) *a* class which is a member of itself,
or (2) *the* totality of all such classes,
or (3) *a* class which is not a member of itself,
or (4) *the* totality of all such classes.[1]

The first and third alternatives may be destroyed as in the argument which you reproduce on p. 6. The second and fourth alternatives are untouched by that argument.

The second alternative is destroyed because the class in question is the totality of classes *not* members of themselves. The fourth alternative remains and is the true one. The truest destruction of the first alternative is that *no* class is a member of itself: and the truest destruction of the third is by the truth of the fourth. (It 's the fallacy of the indefinite article again.)

By the way, I don't understand your solution of the fallacy. You have put it too shortly for me to understand. Perhaps you will expand it for me.

I am afraid I am obliged to think that a man is conceited as well as silly to think such puerilities are worthy to be put in print: and it 's simply exasperating to think that he finds a publisher (where was the publisher's reader?), and that in this way such contemptible stuff can even find its way into examinations.

J. C. W.

⟨PS.⟩[2] Perhaps formally one should add a fifth alternative class, consisting partly of classes members of themselves and partly of classes not members of themselves: if so it would be easily disposed of, for the class consisting of total of non-members of themselves couldn't belong to it. But on a proper division this fifth mixed member would not appear.

The division being.

Class of all classes

class of all classes members of self	class of all classes non-members of self
A, B, C, &c.	α, β, γ, &c.

J. Cook Wilson.

[1] [See PS. infra]

[2] [On a postcard, 9th July 1903]

18 July 1903.
§ 488. 12 Fyfield Road.

My dear Bosanquet,

In reply to your courteous expression about sending such decided objections to my views with 'trembling, &c.', let me assure you that, in controversy with *one* other man at least, I do like the points urged against me put as clearly as possible and with all their proper force. If a man from motives of consideration doesn't quite express his full convictions, there is a danger one may not fully appreciate the objection, and either, if the thing is right, one remains in error, or more controversy is needed to get to a right mind. It is true that a strong expression of difference, or unqualified accusation of error, does in the first instance rouse the fighting instinct (in its irrational form) and that obstructs clear vision, but this has this kind of drawback rather in conversation, where one is tempted to hit quickly back, but even so, it is the least of the two dangers: and in a written objection the danger is almost *nil*, and *the* thing is to be absolutely clear. This is no affectation, at my age one wants to save time wherever controversy is necessary, and one is more aware of the limitations of one's own mind and more anxious for help from others.

I would say this to any one who was good enough to discuss with me. But to *you* I would say especially, you cannot possibly offend me by the most downright and uncompromising statement that I am in error in some particular. Because I am quite sure that even if you detected me in a pretty bad mistake, you do not and would not think I was rather hopeless. I should not feel abased, for I doubt whether any one has given such generous commendation to some of my theories as you have, and it has, I assure you, been a great encouragement to me that you, especially, have so much belief in me.

I have indeed very flattering 'testimonials' from time to time from younger men here; but then they have been my pupils, or attended my lectures, and have got into my ways of speech and thinking; and besides are under obligations for mere trouble taken with them.

But you are perhaps a trifle my senior, and are a 'Master in Israel', and so I have been greatly delighted and helped by your

very unstinted expressions of agreement on certain important matters.

I am sending you a discussion of one of your objections, and without any tremors. I am only making clear a point I believe of importance and you needn't answer at all, and if you wish to, seeing something obviously wrong, you can delay as long as you please. We are off actually to-morrow (Sunday): perhaps, however, I may send in another paper on another of your points. My address will be *Kurhotel, Schonwald, bei Triberg, Baden.*

Your very kind letter (July 17) about Dedekind and Royce came while I was writing the paper. I am so glad that what I said was of some use.

§ 489. There seem to be two main faults in my argument from your point of view.[1]

(i) That whereas I suppose that arguing on the *same* principle as is shown in the argument by which the paradox that 'the class of classes' is a member of itself is proved, it could be shown in the same way that any class whatever may be a member of itself. the truth is that my argument is *not* on the *same principle.*

(ii) That in the argument itself there is a *non sequitur*, viz. the last statement but one in the left hand parallel column, 'the class so defined as being the totality of groups which are "groups of things which are *x*'s" is exactly the class X itself'. You say it is not this class at all.

It seems to me really on examination that there is no serious difference after all: it is almost a mere question of statement.

§ 490. Let me take the first point first. Then of course we hypothetically consider the argument in itself sound and not affected by the second objection. We are only asking whether it falls (if sound) under the *principle of the paradox.* I think you only differ because I mean only the general and essential principle of the argument in the paradox, while you mean something more detailed, some closer agreement in detail, perhaps.

The argument of the paradox is this:

The class of classes is *a* 'class',

∴ it is a member of the class 'class',

[1] Notes appended to letter of 18.VII 03.

∴ it is a member of itself.

The first two propositions here being of the form

Y is *a* Z,

∴ Y is a member of the class Z.

As to the first proposition: I suppose myself to have shown that the way it is got is really this:

The class of classes is the complex or total of all the classes, which are themselves totals or unified manifolds. Then it is argued really that it shares with its subordinate classes the property of being *a* totality, or a unified manifold, and so comes[1] under the general idea. That's how the first proposition 'class of classes is a class' is got, whether rightly or not doesn't for the present purpose concern us. In the case of the class X, assuming for the moment of course that it is rightly represented as the totality of groups of y things which are x, arguing in *exactly* the same way as above we should get that X shared with its subordinate classes the property of being *a* group of things which are x, and thus get by a parallel method an exact parallel to the 1st proposition in the paradox of the class of classes. Then the argument continues with the same parallelism.

The class of classes is *a* class,	X is *a* group of things which are x,
∴ it is a member of the class class	∴ X is a member of the class 'group of things which are x'
∴ it is a member of itself for the class class is the class of classes.	∴ X is a member of itself, for it is the whole group of things which are x.

This is what I mean by saying that I 'prove' the paradox about the X class on the essential principle by which the paradox is proved for the class of classes—or, if you like, on the principle which really underlies the proof of the paradox, however it may be stated. I think the above way of putting the parallelism may very likely satisfy you on the first point, for I have avoided what you object to, viz. that resolution of the class of classes (which by no means 'destroys' its character as

[1] MS. 'some comes'.

you think) in which one is able to represent its 'class conception' as 'being a member of a class' instead of classness.

§ 491. But supposing this difficulty (No. i) to disappear, there remains (ii), and this is the important one. Consider the class X of which the class predicate is x, i. e. every member of the class X is an x and so from this point of view its 'members' are certainly not classes. I say the same class X can be represented so that its members are classes. You reply this is [is] not true. It seems to me there is no more difference between us than between two people one of whom should say a curve was convex, and the other should object 'no it is concave'. Only the parallel is not accurate, for I suppose my position to be that of the man who says it is both concave and convex.

I will make a suggestion which I think will conduce to your appreciating the lines upon which I was thinking. You say the class which is the totality of *groups of things* which are x, is *not* X, i. e. it is not the class which ⟨is⟩ the totality of *things* which are x.

Now I should suggest you should not stop at the mere negation, but ask yourself the question, if the first class (call it X′) is *not* X, what is the positive relation between the 1st class and X? 'X′ is not X, how is X′ related to X?' I imagine that if you had asked yourself the question, you would have revised your criticism and possibly have quite agreed with me.

Now my point is this: 'The Class X as the class X means a certain part of reality, or say a certain total of reality—it consists of all the things which are x.

'But the very same class X, as this total of these realities, is divided into groups A, B, and C, each of which is ⟨a⟩ group of x's (αx's, βx's, γx's), and so it is a total of such groups—a total of groups each of which is *an* x group (in contrast with which it may be called *the* x-group).'

Or to put it otherwise:

'One and the same part of reality—or total of realities if you like—is (i) a total of individuals (say) all of which are x, and so is a manifold unified by x-ness, the members or elements of the manifold *not* being classes, and (ii) it also is divided into groups of individuals: groups A, B, C. These groups are each unified by a kind of x-ness (*αx-ness*, *βx-ness*, *γx-ness*) within

themselves, which make them groups and distinct groups. Now these groups, taken as units, themselves constitute a manifold: and they are unified by the common property of 'being a group of x's'. Thus the totality now appears as the totality of these groups—its members from this point of view are classes, and the class predicate is no longer x but 'being a group of x's'.

Surely this is all right, it is but an instance of the concave and the convex—one reality with two inseparably united aspects.

Supposing you agree to this, the parallel argument [to] which you object to can be easily put in a form (for it 's I feel a mere matter of statement) which would satisfy you.

I might say 'any unified manifold constituting as such a '"class", even if it, the manifold, is presented as individuals, 'not classes, *is* also or *consists* also of a manifold of classes 'unified, &c., &c.' And then I should continue, 'If any manifold 'of individuals constituting an ordinary class is considered from 'this other point of view as the unification of a manifold of 'classes, the argument of the paradox can be applied to it in 'exact parallelism.'

§ 492. Now in truth I had taken to myself some credit for observing two things which hadn't been observed, and hoped (may I still hope?) to be patted on the back by you at least for it.

(1) I think I have shown how a class of individuals *is* also (not merely 'may be represented as') *a* class of classes (Against this the 'class of classes' is simply *the* class of classes, but that doesn't matter even if you don't like that way of putting it).

(2) I have also shown how 'the class of classes' is also the class of individuals defined as 'members of classes' so that, as in the case of an ordinary class the class conception is altered, when represented as a class of classes, so in the case of the class of classes, when its members are represented as individuals, its class conception becomes as I showed in my previous paper 'being member' of a class.

The parallel argument I first sent you, you will see if you look at it again, combines this double aspect of each class. It isn't *necessary* it should, for we can simply express the X class

in the form corresponding to the class of classes, without any reduction of the class of classes, and represent the parallelism as I have done it on p. 3 of the paper. It interested me, however, to do it that way too in order to make the *class of classes* appear as a *case*.

§ 493. Now in conclusion I do venture to think that the theory of the class of classes, &c., is not complete until the double aspect of an ordinary class and of the class of classes is recognized: and it is not till this is done that one realizes the full likeness and difference between *the* 'class of classes' and what we call an ordinary class. (You will note that what you called *destruction* of the characteristic of the class of classes, is to me the recognition of something essential [1] to it.)

The Heath Cottage,
Oxshott, Surrey.
Aug. 2, 03.

§ 494.

My dear Wilson,

Many thanks for your kind letter of July 18th. I noticed what you point out in ——'s review of Adam, but did not know the inner history. Is he not also wrong about the shadows in the cave (Adam, ii. 95)?

I almost wish I had gone in for the Logic Prof*ship*, it seems to me that all the real battles are on Logical ground, and these discussions attract me tremendously. I will say a word or two, to state how I think we stand. I do *not* think what I say worthy of the discussion you have initiated. It must be taken as conversational.

I don't think we are far apart as to Y is a Z. You say, if I understand right, that it means Y is one among Z's, and so, if applied to The Class of classes is a class, becomes *false*. I say it doesn't *merely* mean 'one among Z's', and ∴ if applied to prove 'class of classes is one among other classes', it doesn't prove it. But I am still interested in the point that Class of classes shares definition of a class. I think it a peculiarity which necessarily suggests a possible abstraction from the character of the Class of classes as a totality. The abstraction would not give a truth, but it would be inevitable if we were

[1] ['essential' is illegible in the original]

discussing the nature of the Class of classes as a class, or as 'class' if you like. I am sure it remains an exceptional case that the class-predicate can be attached to the class as such, and I know of no natural form for this assertion but 'Y is a Z'.

§ 495. In your analysis of an ordinary class X, you construct, as it seems to me, a new and artificial class GX, G equalling definition of 'group'. Of this, the sub-classes gx's are fair and regular members. But of this class GX, x's are not members, and GX is not a gx. You cannot escape from the law of membership expressed in the class-predicate. X *is* a gx, but then gx's are not members of X, so this does not help to show X as a member of itself. This can only be done if X = GX. But surely there is no ground for this. Just because x's were *not* groups, you have introduced the reflective notion of a group explicitly into the class predicate in order to be able to make groups its members. *It* is your whole instrument for putting the class apparently on a level with its members, and you cannot possibly identify a class determined by this elaborate notion with one in which it does not appear. It is like saying that you and I are members of the class of municipalities, because particular municipalities contain us. Of course, X *is* a class; but this does not mean that the idea of class determines its connotation, which in GX it does. Is it not 'a material use of a formal principle'?

Well; I must break off. I shall try not to write again, and I know you will excuse me. This got between me and my Inaugural, and I had to try and ease my mind. I must try to give up these things to Stout now; they will be his business!

I should rather like to write something about this problem some time. I think there is more behind in the question of membership.

I hope you are enjoying Germany, and that Mrs. Wilson is well.

Yours very truly,

B. BOSANQUET.

§ 496. Sunday. [August 1903.]

My dear Bosanquet,

We leave in 2 hours so that will excuse the extra bad writing of accompanying paper. But I thought I had better get this off my mind before I started. For same reason it isn't as detailed as I should like it to be for clearness, but, if you've time, I really think you will make it all out especially with my previous paper before you.

Yours truly,
J. Cook Wilson.

PS. I hadn't time to notice other details in your paper. Will look at it again when I return in September, but these are the essentials.

§ 497. *On the meaning of the general form Y is Z.*

Though it is necessary, for the reason I gave in the previous note, sometimes to substitute a symbol for a significant word, a symbolic statement like the above is inadequate and may mislead because the only word with significant grammatical form in it is the verb 'is'. The other symbols have nothing to represent the grammatical terminations, which are absolutely essential for determining the meaning of a linguistic form. There would be less chance of error if one could by some barbarism give a kind of adjectival form to the predicate symbol, ⟨thus .⟩

Y is Z-ical, Y is Z-y, Y is Z-ible,

for the meaning of the predication is quite indeterminate without the precise grammatical form which follows the word 'is'. (From this point of view the form Y is *a* Z has advantages—we will return to it presently.)

Of course it is true that language is elastic and that not only are certain artificial forms introduced for technical purposes like Aristotle's A ὑπάρχει τῷ B, and 'whiteness is inherent in snow' or 'whiteness is in snow'—but also linguistic forms get away from their original meaning otherwise.

But now I must in this reference hope to gain assent to a point which is to me of the last importance. It is the business of the student of Logic to determine the *normal* use of an idiom or

linguistic expression. Everything depends on that. If this is once done, one gets a hold over the 'implications', of which you speak, and derived and extended meanings. It not only gives the key to fallacies which come from the confusion of different meanings under one form of expression: but it prevents certain mistakes in the logician himself. This is not sufficiently appreciated by very able logicians · they do not humble themselves sufficiently to the banausia of grammar, and pay for it. For instance, in Bradley's *Logic* there are very serious consequences of this kind · much that he has to say on some topics is merely worthless in consequence.

§ 498. 1. The form Y is Z may represent what I call equational predication, where Y is the total realization of the universal in Z. But whether it is so or not can only be learnt from the word-form of the particular judgement itself. The symbolic representation tells nothing.

2. The form may also represent what I call adjectival predication, when Z has the form of an adjective, or some grammatical phrase equivalent to an adjective. 'This blotting-paper is red'; 'this letter is heavy' About the normal meaning of the adjectival predication, I should have thought there could be no manner of doubt Language has developed certain different forms for special and different purposes, and that is their *normal* use as opposed to shifting and change. For instance, ⟨it⟩ is not for nothing that language has developed the two forms 'all AB is C', and '*if* A is B it is C'.

Now I venture to say that the adjectival form of predication has been developed for the precise purpose of expressing the case in which the universal referred to by the predicate is either known not to have the subject Y for its complete realization, i. e. only realization, or, where this is a matter of uncertainty, to leave it an open question, and not *state* more than that Y is at least *a* realization of Z.

Thus when we say Y is Z-ical (or Z-ible or Z-ial, &c.) the reason for choosing this form of expression rather than any other—the reason why language has developed it—is that we do not mean to state more than that Z is realized in Y, that Y is at least *a* realization of it, whether we are sure there are more or simply don't know whether there are.

I hope that this way of putting it may satisfy you that it is quite an irrelevant objection to say that often in such a judgement as Y is Z we don't know that Z has more realizations than in Y. Just because we don't know we choose a form of expression which commits us to no more than we know to be true or think true, viz. that Y is at least *one* of the realizations of Z. And I think it a conclusive answer also to the objection, that if we do intend to affirm that Y is the whole and sole realization of Z, we should never naturally choose the adjectival form of predication. Not only so, but I suppose it literally true that we *never* do, in ordinary speech (of course artificial or technical expressions invented arbitrarily to support a theory do not concern us).

§ 499. The above applies without change to the form Y is *a* Z. There is a reason why we say 'Socrates is *a* man' on the one hand and 'Socrates is white', without the indefinite article, on the other, which does not here concern us, and I won't dwell upon it; but its normal use *always* indicates that we do not state more than that Y is *one* of the realizations of Z, and the normal implication is that we at least leave open the possibility that there are more. This form again draws attention through the indefinite article to the fact that the subject Y is being represented not only as having Z realized in it but as *a member* of a group of similar things in each of which Z is realized. I venture to think that the soundness of this view is brought out by the fact that it is upon the well-understood normal meaning and normal implication of a given phrase that prevarication often depends. It is this that makes certain kinds of this form of deceit possible. Suppose a man described to a foreigner really every part of the British Empire, and after finishing the list said, 'All that is *a* territory now in possession of the British'. The foreigner would of course suppose it was *not* the only British territory and would expect to hear about some other. The speaker might take advantage of this by then enumerating certain parts, not before mentioned by name, which fell within the whole he had mentioned and calling them British territory. Some practical end might turn on this. When it was secured, and the deceived man rounded on the prevaricator, the latter might claim he told the truth: that his first statement

'all that is *a* British territory' was true. Whereas the fact is he lied: for he knew his statement would be taken in its normal sense. If he appealed to Logic to defend him, his opponent might well conceive that Logic was some contrivance for justifying knavery.

§ 500. This, I must maintain, must be applied without any abatement to the statement that the class of classes is *a* class. It is only because the idea of the class of classes is new to us or unfamiliar that it seems strange not to be able to say that ⟨it⟩ is *a* class. We don't realize in so speaking what the 'class of classes' means. To say that it is *a* class is as mistaken as to say that Space is *a* space. Just because Space is thought of as a totality we *never* speak of it as *a* space.

Just as you call Space 'Space' without any article, so you may if you please say 'Class' as equivalent to the class of classes or the totality of classes, but you cannot without doing violence to the normal and understood meaning of language call it *a* class.

But now people *do* violate the normal meaning of language in order to take advantage of it—and hence, as I have said, prevarication or paradox. And this statement that the class of classes is a member of itself because it 's *a* class is a mere instance of unconscious prevarication. The statement is only of any interest because it is paradoxical. If membership was so construed as to mean the whole of the members of the class, then that the class of classes is a member of itself only means that it is identical with itself or with the totality of its members in the proper sense of members. But then it would be a silly tautology and no one would take ⟨the⟩ slightest interest in it. It is interesting because a member is understood naturally to be a *part* of a *whole* group, and so the sentence implies that somehow the whole is identical with one of its parts. That 's the whole story. Now such a result comes from the prevarication: to call a thing *a* Z normally implies that it is one among other Z's, or at least that we don't state it 's the only Z, and normally leaves open the *possibility* that there are other Z's. But if you do more, and say that Y is *a* Z *and therefore* that Y is *a* member of the class Z, you go further than merely leaving the possibility of more members open, or of other realizations: the normal implication is that there are other members.

Thus (i) if we say the $\underset{\text{Class-of-classes}}{Y}$ is *a* $\underset{\text{Class}}{Z}$ and go no further, we normally imply that there is at least a possibility that there are classes outside the class of classes, but even the possibility is false, and so our statement is really false.

(*ii*) If we add 'because the class of classes is a class, it is a member of itself' we commit ourselves to a slightly worse falsehood, for the normal implication of this is not only that there may possibly be classes outside the class of classes, but ⟨that⟩ there are actually such classes.

Similarly I think there is no difficulty whatever in showing the trivial fallacy of language involved in making any class a member of itself.

J. C. W.

XI

ON THE NOTIONS OF A CLASS AND OF CLASSES[1]

[Oxford, 23 September 1903.]

§ 501. *The distinction of the 'class as one' from the 'class as many'.*

The accurate account of '*a* class' is that it is a unified manifold; but it is not any unified manifold that is a class. For instance, an animal organism is a unified manifold of organic parts, but it is not a class.

A class is a particular kind of unified manifold and its distinctive character lies in this that its unity consists in the realization of one and the same universal in each of its constituent members. This is the same as saying that 'the unity is constituted by the truth of a certain assertion concerning each of its members severally'; though I prefer the objective way of putting it

§ 502. We can therefore distinguish between the unity and the manifoldness of the class, and this is a valid distinction, for the unity of the class is *not* its manifoldness, though the two are inseparable from each other.

Now, of course, the class as *one* (or as *a* unity) is not identical with the unity of the class, and the class as *manifold* is not identical with the manifoldness of the class. We therefore cannot argue from the above true statement 'the unity of the class is not its manifoldness' to 'the class as one (or as a unity) is not the class as a manifold'. Indeed the latter statement is *not* true; on the contrary, '*the* class as one' *is* 'the class as manifold'.

It must be remembered that we have to do with the distinction of the class as one from the class as many, as it appears in the

[1] Letter to Dr. G F Stout, F B A, Professor of Logic in St Andrews University.

statement that the class *as one* may be a member of (stands therefore in a certain relation to) the same class *as many*.

§ 503. Suppose A is X and A is Y; suppose also that X and Y necessitate one another and are inseparable.

Then A *as* X is Y and A *as* Y is X;

Suppose A as X is B, then A as Y is X, therefore A as Y is B; and A as X and A as Y have exactly the same predicate and can't be distinguished by difference of predicate.

Further, let A as X stand in a relation R to C: then A as Y must as X stand in this relation R to C.

Let C = A as Y.

Then, by the above, if A as X stands in a relation R to A as Y, A as Y stands in the relation R to A as Y, which is nugatory.

And if it has been asserted that in consequence of the *distinction* of A as X from A as Y, the relation R is possible between A as X and A as Y, this statement will be nullified, for A as Y will stand in the relation R to A as Y, where there is no distinction of the terms related.

This will be still clearer if we suppose a relation R, which is not convertible. If we suppose, that is, that M is in a relation R to N but that it doesn't follow that N is in the relation R to M, but in a relation r to M. For instance, if M is greater than N, N is *not* greater than M.

Suppose then R to be such a relation that if M is in the relation R to N, N is in the relation r to M, R and r not being coincident.〉[1]

Now 'A as X stands in a relation R to A as Y' means that A, because it is AX, stands in the relation R to A, because A is AY; but, since Y necessitates X, AY is AX; or A, because it is AY, is AX.

And since X necessitates Y, A is AY, because A is AX. Therefore it follows from 'A because it is AX stands in the relation R to A, because it is AY' that A because it is Y (and is therefore AX) stands in the relation R to A because it is X (and is therefore AY), i. e. A as Y is in the relation R to A as X.

Now it has been shown that A as Y is in the relation r to

[1] The part in brackets is lost from the original letter and is supplied from Wilson's manuscript notes.

A as X, but this is impossible on account of the relation between R and *r*.

If we substitute in the above general theorems for A, 'the class'; for X, 'a unity'; and for Y, 'a manifold'; and for R, 'the relation of member of', the above refutation will immediately apply to the statement that a class as one can be a member of itself as many.

For the unity and the manifoldness of the class involve one another in the way necessary for X and Y in the above theorems. The unity of the class only exists as a unity OF the manifold in it, and thus the class itself—not the unity of the class, but the class—is 'one' only as a unified manifold. That is, the unity of the class is such that the class *as* one is a unified *manifold*. Also it is only a manifold as the pluralization or manifoldness of a unity. That is, the manifoldness of the class is such that the class *as* manifold is a *manifold* unity. Thus *as* one it is manifold, as manifold it is one; i. e. the nature of its unity necessitates its manifoldness, the nature of its manifoldness necessitates its unity. This corresponds to the inseparable relation of X and Y, and it would then follow that if the class 'as one' was a member of itself 'as many' (rather 'as a manifold'), that the class *as one* was a member of itself as *one* and that the class as *many* was a member of itself as *one*.

§ 504 Or again we may put it otherwise thus:

The expression 'A as B' implies that the statement 'A is B' is true. The expression therefore 'the class as many' should imply that the statement 'the class is many' is true. But this is not a proper use of language. The class as class is not many, it is one: it is the members of the class that are many, and it is of them that 'many' is properly predicable, and in such a statement. The best chance for making possible the distinction of the class as many from the class as one is to substitute for 'the class as many', 'the class as manifold'.

The class has something to do with 'manyness', with 'many', with '*a* many': but what is it exactly? The class has only to do with 'manyness' or 'a many' as being the unification of *a* many. The expression then 'the class as many' if allowable at all should mean 'the class as having the aforesaid relation to many-ness', that is, 'the class as unification of a many'.

Thus the expression 'class as many' must be capable of having substituted for it 'class as unification of a many', and conversely. But the class is one as unification of a many: therefore the class as one (*a*) is the class as unification of a many (*b*). Substituting, in accordance with the above, for the second phrase (*b*) in this last sentence, we get 'the class as one is the class as many'. Whence to say that the class as one is a member of itself as many is to say that the class as many is a member of the class as many; and we should also easily derive the other statements already given above (§ 503 fin.).

§ 505. But however this may be, I should have thought the idea of a class as a unified manifold made it easy to disprove, shortly and simply, that any class can be a member of itself We have but to consider what membership of a class means.

A class being a unified manifold, it is the elements of the manifold which are the members of the class. That is the whole story. Consequently it is a contradiction to say that a class can be a member of itself, for that would mean that the unified manifold of elements was one of the elements unified. Hence it would follow that any belief that a class could be a member of itself must depend on some fallacy of language. It doesn't matter what class it is—the 'class of classes' included. This would be quite certain *a priori*, before examining any particular statement: and I have tried to show, in previous papers, what the linguistic fallacies exactly are. I should have thought the above refutation, short as it is, sufficient and decisive, and that the only matter of dispute could be, not whether there was a verbal fallacy, but what the exact nature of the fallacy was.

§ 506. It follows that I am ⟨in⟩ entire disagreement with your two papers, so far as these presuppose (1) the distinction of the class as one from the class as many, in what seems to me an untenable form, and especially (2) that a class as one can be a member of itself as many.

Nevertheless, as criticisms are asked for ⟨by you⟩ on these papers, I may venture to discuss certain parts of them in detail.

§ 507. Distinction of

(*a*) class taken *simpliciter*,

(*b*) class quâ class,

(*c*) class taken as many.

Consider what would be the natural interpretation of a distinction so stated, before considering the interpretation given of it and the use made of it.

If the words are to be taken strictly, the distinction is of the general form

(*a*) A taken *simpliciter*,
(*b*) A quâ A,
(*c*) A taken as B = A quâ B.

A term like '*simpliciter*' does not seem very suitable, when clear analysis is aimed at, for it may mean various things and itself requires interpretation. We ask how can A *simpliciter* differ from A quâ A? 'A *simpliciter*' seems naturally to mean 'A pure and simple' (ἁπλῶς), as distinct, e. g., from any analogous or metaphorical uses of the term A; but that is just what is meant by A quâ A, and so (*a*) and (*b*) would be indistinguishable.

But perhaps the formula is not to be taken strictly, and we should read each time for 'class', '*a* class': and then the general form will be

(*a*) *an* A *simpliciter* = a *particular* A *simpliciter*,
(*b*) *an* A quâ A = a *particular* A quâ A,
(*c*) *an* A quâ B = a *particular* A quâ B.

And this turns out to be the case: for the application of the formula shows that 'class *quâ* class' means *a* particular class considered as *class* only in general, and not as the particular class which it really is; and from the same context one gathers that by 'class *simpliciter*' is actually meant 'a class as being the particular class which it is', or 'a class quâ the class it is', or 'quâ itself'. The distinction, then, between (*a*) and (*b*) is merely the distinction of the particular or concrete (the particular class) from the universal (class in general).

§ 508. This being so I cannot understand what is gained by describing the distinction so artificially, not only by the odd use of '*simpliciter*', but in the formal explanation given of it. In this latter explanation, instead of the simple statement that by 'a class *simpliciter*' is meant a particular class as particular, we find an account which seems to me to be indirect and confusing. One doesn't gather what it means till further on. But, besides this, the form of this explanation seems to me open to criticism. The procedure is to explain 'class *simpliciter*' by

appeal to certain linguistic forms: the meaning of words in certain statements. This may have seemed simple in itself and clear, but I confess it does not seem to me to be so. Is there any particular reason for taking such an example as *men*? No reason is given and yet there is one, for it would not do to substitute, e. g. 'the class "classes"' for 'the class "men"'. What is exactly meant by the 'complete meaning' of the word 'men' in a certain statement? The phrase may have seemed so clear that the question may be held captious, but I cannot think it clear. To what kind of meaning of the word 'men' is the 'complete meaning' opposed. The next sentence, 'it is equivalent, &c.', suggests that *equivalence* is the test. But in such an appeal to verbal forms, the equivalent should be capable of being substituted for that of which it is the equivalent. The context requires, then, that the 'class man *simpliciter*' should be equivalent to 'every man' in such a proposition as 'every man is mortal' (= every man is a mortal), or to 'all men' in such a proposition as 'all men are mortal'. But this seems to fail, for we can't substitute 'the class men' for 'every man' in the above proposition; for 'the class men is a mortal' would not be true, nor indeed is 'every man' equivalent to 'all men', as the context implies: e. g we cannot in the sentence 'all men should love one another' substitute 'every man' for 'all men', nor can we in 'all men form a community'.

§ 509. Now compare your page 2, where a similar procedure is adopted to define (*a*) 'class taken as many'. It will be found that this, if taken literally, can be made to coincide with the account given of (*a*) just discussed—'we refer to the class "men" as many, whenever we speak of *a* man, or of "some men" or of "any man"'. But as 'a man' is here meant, the statement '*a* man is mortal' is equivalent to 'every man is mortal'. So again 'any man must die' is equivalent to 'every man must die' or to 'every man is mortal'. So that the above amounts really to explaining 'the class "men" as many' by the way 'we speak of' *every man*, in 'every man is mortal'; whereas 'the class "men" *simpliciter*' has been explained in the self-same way.

It seems to me that this sort of thing is always liable to happen to us, when we are trying to define something quite

simple and well understood. Because it is perfectly well known what object we are defining, what we really mean, we may overlook the fact that our *formulae* are not adequate, or are indirect. We must ask whether any one who didn't know what was meant beforehand would discover it from our *formulae*. For instance, in the case before us, would a foreigner who did not know the meaning of the English word 'class', or of its cognates in other languages, really find out from the description what 'class *simpliciter*' meant? And would he, from page 2, understand there was any difference between 'class *simpliciter*' and 'class as many'?

I am obliged, then, to think that the method adopted for explaining the distinction really intended between (*a*) and (*b*)—not to speak of (*c*), 'the class as many'—is curiously artificial and indirect, and that it does not really explain the distinction. Even if it will not be admitted that it could be misunderstood, it is at least assailable, whereas on the one hand such definitions are bound to be accurate and must submit to the test of being taken literally and strictly, and on the other hand the distinction can be stated simply and without possibility of misunderstanding.

§ 510. Further, it seems to me that whereas the account which I offered was rejected, the present account reproduces the essentials of a distinction which I gave—as something merely obvious to anybody—between the 'class of classes' and the classes subordinate to it (or of which it is composed), but associates this distinction with an artificial formulation which tends to obscure it.

I may be allowed to recall, summarily, the distinction as I gave it. A class is a unified manifold, in which the elements of the manifold are unified by the appearance in each of a common characteristic. This account, as common to every class, constitutes their 'classness'. On the other hand, any particular class is unified by some special common characteristic, and not by a common characteristic in general. And it is this special or particular nature of the common characteristic which makes any given class the special or particular class that it is, e. g. such special characteristics as 'humanity', 'redness', 'triangularity'. What is called the 'class of classes' is obviously therefore the totality of such special unifications of manifolds.

Just as triangularity is the common characteristic of each member of the class triangle, so unification of a manifold by a common characteristic is the common characteristic of the members (themselves classes) of the class of classes.

§ 511. Not only so, but an important conclusion which I arrived at reappears, though in a somewhat different dress, viz. that classness cannot be predicated of the class of classes. This was one of my principal points, and one I laboured to elucidate. I do not suppose this is a conscious acceptance of, or coming round to, my view, because you are so ready always and so frank in acknowledgement, and it would be difficult to account for your opening expressions of regret at disagreement. If this is only carried out consistently, it results, as I think I have shown, in the refutation of the fallacy that the class of classes can be a member of itself. But though there is this important agreement in the conclusion, is the method of arriving at the conclusion successful? Would it really convince a doubter? Is it not open to an objection for which no provision is made? 'If you call "the class 'classes'", the *class* 'classes', how can you refuse to predicate classness of it?' Would it not also be inevitably said that in your statement there is a mere assertion of what has to be proved, that the question is begged, and no answer given to the above objection? For my own part I am convinced the objection cannot be answered except by an analysis of the meaning of idioms of predication, 'A is B (adjective)', 'A is *a* B', &c., and their relation to class membership. It is a matter of linguistic analysis almost solely. I adopted this method in the accounts submitted, and the account now under discussion only confirms my conviction that this is the only method by which these paradoxes can be dealt with (inasmuch as they arise entirely out of a misuse of language). And I venture to think the use of this method makes the whole matter transparent; shows clearly that the idea of any class being a member of itself is a mere verbal fallacy. It seems to me also to settle the supposed contradiction in two ways, (1) because the 'contradiction' is grounded on the mistake about a class being ⟨a⟩ member of itself; and (2), in the way which I have already detailed before.

§ 512. With respect to certain developments which I added

to the criticism of the fallacies—though not necessary to it—these depend in part at least on a characteristic of classes of which account had not been taken, which may be put shortly as follows: Consider a *manifold of elements unified as a class.* The elements are members of the class. This manifold of elements may be divided into sets, each set itself a manifold unified by some common characteristic. These unities (i. e. the sets) constitute a manifold, and the unity of the whole may be presented as the unification of *this* manifold, the members of it being the sets, and themselves unified manifolds.

[One section omitted]

§ 513. 'A class is essentially a distributive unity.'

'A *class collection* is essentially a collective unity.'

This distinction seems to me ⟨to be⟩ put in an untenable form. I think I understand what is aimed at, but the *formulae* do not seem to me to express it; the distinction being not really due to the difference between two kinds of unity between the members of a class.

(i) What would be naturally understood by a 'distributive unity' in such a connexion? That a certain unity (universal) manifested itself in a plurality of particulars. This agrees with the definition given of it—'constituted by the truth of a certain assertion concerning each of the members severally'. The common predicate represents the unity manifested in each particular. Shortly then the 'distributive unity' means a pluralized unity.

(ii) Secondly, what would be naturally understood by a 'collective unity' of a class? That a certain number—or an indefinite number—of particulars, though many, were collected into a unity, or unified, by something common to them: that is to say, a unified plurality. Thus the distributive unity would be a pluralized unity (manifold unity) and the collective unity would be a unified plurality (unified manifold). But these are merely two verbal methods of describing the same thing.

§ 514. You say, however, that for the collective unity 'it is essential that its constituents should be connected by some other form of connexion than that which simply consists in the truth of a certain assertion concerning each of them severally'. (One

observes here that both unities are conceived as having constituents; that is, both as pluralized unities or unified pluralities.) *Prima facie* one must wonder what the 'other form of combination' could possibly be; for one would suppose the unity in question must be manifested in each particular, and inevitably give rise to a common predication. Unfortunately no illustration is given: and the general formula, 'any collective unity having for its constituents all members of a class', is (I should have thought) exactly applicable to a class and to what has been called a distributive unity.

§ 515. But one may judge, by help of the other paper and also by apparent implications in this one, that the collective unity has somehow corresponding to it a predicate *not* predicable of each constituent severally and that the real intention is to distinguish between the two forms of predication illustrated in these two examples:

(*a*) Every man is a vertebrate animal.

(*b*) The class man is a species of the class vertebrate, or, is a member of the class of vertebrate classes.

For we cannot predicate the predicate of (*a*) of the subject of (*b*), the class man is not a vertebrate animal; nor can we predicate the predicate of (*b*) of the subject of (*a*). The distinction between (*a*) and (*b*) seems to me certainly not due to a distinction between two different kinds of unity of constituents at all.

§ 516. But before coming to this, I think it may be shown that, if the distinction of (*a*) and (*b*) is, as I imagine, the fact on which the distinction of the 'distributive unity' and 'collective unity' is founded—⟨then⟩ the characteristics above assigned as distinguishing these two unities can hardly be maintained· viz. that in the distributive unity the constituents have severally the same predicate, but that in the collective unity there is some other bond of connexion. For if the collective unity is that represented by the class 'man' in (*b*), subject of a singular verb and with one predicate, and similarly that represented by the class 'vertebrate' in the statement 'the class vertebrate has the class man for a species', then such 'collective unity' owes its collective unity precisely to the bond of union expressed by the predication of the same thing of its constituents, and to nothing else.

The 'collective unity' or 'class collection' represented by the 'class vertebrate' in the above statement, with its singular verb, is unified solely by the common principle which gives each of its members the common predicate 'vertebrate'. That is to say, its unity is made in exactly the same way as that assigned to the distributive unity, and it has not 'some other bond of connexion' besides this. So also 'the class man' representing a 'collective unity' or 'class collection' in (*b*) owes its unity precisely and only to the fact represented by the predication of one and the same universal (vertebrateness and whatever else is common to men as men) of its members. Supposing, then, that the right kind of example has been given of what is intended by a collective unity, the *prima facie* impression above stated that the 'collective unity' could have no other bond but that which makes the 'distributive unity', would be confirmed.

§ 517. Now as to the difference between (*a*) and (*b*).

The class being a unified manifold, as described ⟨by you⟩ is, as a unified manifold, *a* unity, and so can have a predicate attached to it as *a* unity, a predicate which cannot be attached to any of its members individually. This corresponds to the instances of the form (*b*). But more than this, there is no other form of predication of the class as class possible. As a unified plurality or pluralized unity, it *is* a unity and all its predicates as *its* predicates are of it as a unity. Thus (*b*) is the only real form of predication about a class as a class. But we may also predicate something not of the class—the unified manifold as such—but of one of its manifold members, or of some severally, or of each severally. Even in the latter case there is no predication of the class as class, the predicate is attached only to each member as *individual*, e. g. 'each triangle is a figure with interior angles, &c.', but we can't say 'the class triangle is a figure, &c.'. This predication of each of the members, and not of the class as such, is represented by such instances as (*a*). Apparently, however, (*a*) is regarded as a case where something is predicated of a class 'as many'. As I have contended before, the class is not many, it is its members which are many, and to speak of the class as many seems to me a loose way of speaking, which leads to serious confusions when we introduce it into strict analysis.

§ 518. However—and this is what probably leads to what I have ventured to deprecate as a confusion—from such predication as (*a*), which is not a predication of the class as class, a true predication of the class can be derived: e. g. 'the class triangle is such as to have each of its members a figure whose interior angles', &c., &c.; 'the class man is such that each of its members is a vertebrate animal.' And there is a further relation which may similarly promote confusion. Though the predication (*a*) is one in which the predicate can only attach to each member of the class, and not to the class as class, that is, not to the class as the unity of such members, nevertheless the fact that this very predicate, which attaches to each severally and cannot attach to the class as class, is present in *all*, makes a unified manifold of them, that is, makes a class as class. E g. the property predicated of each triangle unifies them all.

The above seems to me the true analysis; the explanation not being by what I venture to think an untenable distinction between a distributive unity and a collective unity. And the distinction between (*a*) and (*b*), which I imagine a simple matter notwithstanding the length at which I have treated it for the sake of clearness, seems to me merely this:

In (*a*) we have a predication in which the predicate can only attach to each member of the class as a separate member, and not to the class as class; in (*b*) a predication in which the predicate attaches to the class as class

To avoid misunderstanding of the use in the above of the expression 'class as class', I may say there is no mystery about it; it simply stands for class, but is put for clearness as against the representation of a class which seems to violate the idea of a class as such. And so by 'class as class' I only mean 'a class, if it is to be a class at all'.

J. C. W.

XII

PRIMARY AND SECONDARY QUALITIES

Letter in criticism of a paper on Primary and Secondary Qualities

July, 1904.

§ 519. *Some preliminary points.*

According to the formulation of Locke (e. g.) the secondary qualities, which are qualities of the external object, are strictly speaking not the sensations, as heat and colour, produced in us by the object, for these are not qualities of the object, but the powers in the object to produce the sensations. Still the sensations themselves—inaccurately I think—get called secondary qualities I observe this in the present article. It doesn't matter, but it is well to remember it or it tends to confusion in thinking.

For clearness in a discussion like the present it is well to confine attention to an example like heat and to exclude colour. For in the case of heat every one easily recognizes, without philosophy, that he himself has a definite sensation of heat—we know this to be a sensation inasmuch as we say we feel hot. This is not so with colour People do not easily recognize that colour is a sensation of their own On the contrary, the belief that it is a sensation is an inference from a theory scientific or philosophic. We all of us, both the plain and the philosophic, think of colour as in the thing and put it there in a way we never put our sensation of heat. Thus, too, ordinary people say they feel hot, but never say they feel coloured, e g. feel red. Moreover, whereas they speak of a sensation of heat, they *never* speak of a 'sensation of yellow'. It is only the scientific or philosophic who speak thus in consequence of a theory which they hold, and that only because they think they have *proved* to themselves it is a sensation, not because they recognize it directly as such: for it is not natural to do so. A connexion with sensation becomes apparent when the colour dazzles us: but even then we think of the colour as in the thing and of the dazzling as its effect upon us. That colours are only seen in

the light makes no difference (and here Locke's argument is wrong), for we simply think of their colour as a property *they* have in the light, just as we think of floating as a property cork has in the water.

We may therefore for the present purpose when speaking of *secondary* qualities confine ourselves to the sensation of heat and its correlative in the thing.

§ 520. *Consciousness of the plain man.*

I don't think you give a quite accurate representation of the theory of the plain man's consciousness implied in the view you are attacking. It needn't be assumed that he definitely thinks his sensation as in the hot iron, so as, e. g., to be liable to what is called a 'flagrant absurdity'. Rather it would be supposed that in feeling a hot surface he tends to think that besides having a sensation of burning or warmth, he is feeling something in the thing which causes it, just as when he feels a rough surface he not only has a special kind of sensation but recognizes something different from it, viz. an uneven surface. Only that in the case of heat the only positive nature he assigns to the supposed quality felt in the thing is derived from his own sensation. So it might be said that by a confusion he assimilates the character of the quality attributed to the thing to the character of his own sensation—a confusion in which he is not fully conscious of what he is really doing.

(Oddly enough, this seems to be exactly what your own theory comes to, as I shall try to show presently, so that you don't really differ from the theory you are attacking.)

I shouldn't mind calling this an unconscious transference of the *quality* of his own sensation to the thing; but he certainly distinguishes from it his own feeling of being burned (and pained possibly) or of being warmed (and pleased possibly). The quality of his sensation thus ⟨does⟩ double duty; and I am prepared to show that this is done in Bacon, and that it is the key to the standing puzzle about the *causa formalis* or *forma*.

I should be quite prepared to find that some people who hold the theory that in heat there is really nothing in the body but a state of its primary qualities (dynamical)—especially scientific people who think crudely on these things—have said the plain man simply thinks the heat he feels (his sensation) is in the hot

object; but that is merely an awkward, perhaps unintelligent, presentation of the consequences of their theory, and the philosophic view would be, I think, what I have represented.

I think it is important to notice that we have not to deal with the plain man here as opposed to the philosopher. We are all 'plain men' in this sense. It represents the operation of *our* ordinary unreflective consciousness: and it is because we who are reflective and philosophic have this consciousness that we are able to deal with it. Thus it is true that the *only* course we can take is to ask the plain man (in this sense) what he means. As philosophers we interrogate ourselves as plain men

§ 521. *On the 'secondary quality' as a 'mere possibility'.*

Unless I have mistaken your drift, you seem to treat the theory you are criticizing as if it implied ⟨that⟩ the Secondary quality in the thing was a *mere* power to produce a sensation in us, and you seem to be controverting that and trying at least to show that our 'plain consciousness'—'ordinary unreflecting thought'—considers the quality as having some actual nature beyond being a mere power (= mere possibility). But the moderns who hold Locke's doctrine (and I should say, with doubt, Locke himself) do not hold this at all. They suppose that corresponding to our sensation of heat there is an actual, not potential, activity of the particles themselves, a vibratory movement—a dynamical state which is entirely comprised within what Primary qualities mean for them. This exists whether there is any sensation or not and it is a potentiality only with regard to sensation. If there is a sensitive surface upon which this vibratory movement can act, then it produces a sensation of heat. Your argument certainly gives the impression that you suppose yourself correcting a point in the Lockean view, and insisting on some actual or positive quality in the body said to be hot, other than the mere possibility of producing a sensation in us. If this is what you really mean, it is a misconception. The thing you are trying to establish against the theory is an integral part of the theory itself, i. e. so far as you are merely trying to prove that the quality in the thing is not merely potentiality of sensation

It seems to me that there is not only this false issue in the paragraph before us, but that it has helped to put you off

the scent, and to prevent you from realizing the true significance of the 'fact' to which you appeal. I shall return to the last point presently.

Perhaps you do *not* intend your contention as to the actual and positive nature of the quality in the body to be a criticism of the Lockean theory; but I feel sure every reader would suppose you did, and therefore I should suggest in re-writing to make your meaning plain. But in any case I venture to think that it is your attention to this particular point as following from ('shown by') the fact to which you appeal, that has obscured for you the main significance of that very fact.

§ 522. *Sensation without representative value.*

There is another point I should like to clear off before coming to grips with the main issue. You suppose that the gradual diminution of heat sensations as the sensitive organism is withdrawn from the hot object is without representative value. Here you have overlooked the fact that, according to your own meaning of representative value, this change has a very definite and very important representative value. It obviously corresponds to the objective physical fact that the heat in a body has effects at a distance from itself, and that these effects diminish in intensity as the distance increases: this is as objective as the diminution of attractive force with the distance. It shows itself not only in our sensations but in the fact that a body gets warmer from the heat of the warming body as it gets nearer to it

This suggests another point which you don't notice and should, I think, be kept in mind in the discussion of secondary qualities. Sometimes we feel the warmth or the cold of a body by touching it and when we touch it, then it is easier to confuse the quality which belongs to our sensation with a quality *in* the body. But how about the case where we feel warmth 'in the hand', when the hand is *advanced* to a certain distance from the object? Here we do not naturally say we feel anything *in* the object, but I suppose we think of ourselves as feeling heat conceived as *something* in the space between us and the object, and 'coming out' from it (think, e. g., of warming the hands at a good hot blazing fire). And in this case we clearly and definitely locate a sensation of warmth in the hand. And here I would call your

attention as a psychologist to a very striking difference between heat as a sensation and colour (if it is to be called a sensation); though the perceiving organ is always at a distance from the perceived coloured object, we always locate the colour in the perceived object and *never* in the perceiving organ. In the case of heat felt at a distance from a hot object, on the other hand, we locate the sensation of heat in the hand (e. g) and the 'heat' (as objective) which we suppose ourselves to feel (through the sensation) we do not locate *in* the object, though we may connect it with the object.

§ 523. *The theory criticized and the theory opposed to it.*

The theory criticized seems properly to be this: Heat *in* the hot body is only a vibratory movement of its material particles · and this is confined entirely to its 'primary qualities'. Symbolize *this* heat by Hm. (Heat as a mode of motion.) There is nothing else *in* the body except the power of this Hm to produce a sensation in us. Call the sensation Hs. In the case of heat felt by us, then, the whole reality consists of Hm, the power of Hm to produce Hs, and of Hs.

This is the theory of the physicists and philosophers referred to. (By the way, the theory began with the Greek Atomists, who, though philosophers by nature, had surely rather the scientific[1] than the philosophic spirit) They would usually admit that we ordinarily think of heat as something in the body like our sensation, and not as a mechanical condition of its particles (which indeed we only know by scientific investigation). And this latter tendency of ours they regard as an illusion though they may differ in the way in which they describe it.

The theory you oppose to this I cannot think clear, and I doubt whether you have got yourself clear as to your own meaning. The one certain thing by which one has to try to interpret you is that you deny ⟨that⟩ the plain man's view is an illusion This would seem to necessitate that he is right in thinking there is some quality in the body which, whatever else it is, is neither his sensation of heat (though apparently somehow near akin to it) nor the vibratory movement of the particles. It must be different from the latter or you would agree with the theory you criticize. This then is neither Hs nor Hm; let

[1] In the MS 'scientific' and 'philosophic' are inverted.

us call it Hx. The distinction, however, is obscured, from your tendency to represent the 'primary quality' theory of heat as though it *only* meant a power to produce a sensation. Thus where one would expect Hx (the secondary quality as ascribed by the 'plain man' to the *thing*) to be distinguished from the primary quality as a vibratory movement of particles, it is only distinguished from 'their power to produce a certain sensation in us'. One would expect you to say 'he does not merely mean the secondary qualities as vibrations of particles together with the power of this dynamic state to produce sensations in us'. Thus you seem committed to recognizing Hs and Hx and Hn, which latter is not Hm.

The question arises: do you really mean to recognize Hm at all? And, if so, ⟨in⟩ what relation does it stand to Hx, since both Hx and Hm are supposed to be in the body? This last question is a serious one, and the fact that you haven't asked it seems to me to indicate that you have not fully thought out your position. Nor on the other hand do you deny Hm.

The first criticism I should therefore pass is that you have not made clear what it is exactly that you do hold. But, assuming that what you don't deny you retain, the reasonable supposition seems to be that you recognize as real and as distinct from one another Hs, Hm, and Hx.

§ 524. *Sensations as representative of secondary qualities and the general idea of such representation.*

You begin an important section of your argument by assuming the idea of sensations being *representative.*

⟨They⟩ { represent / express / stand for } something other than themselves

without any critical elucidation of the idea.

Now, I venture to think that the idea of such *representation* in philosophy, or psychology rather, is very loose and treacherous and, if used at all, should be preceded by a 'critique' of such *representative* character, and an explanation of the exact sense in which the word representative is used.

If you had undertaken such a 'critique' I think you might have written very differently in the sequel. It seems to me that in what follows you not only assume this idea of repre-

sentation, but assume its application to Hs and Hx, thus assuming the existence of Hx, the very thing which you have to establish.

Let us consider the idea of 'represent', which you make equivalent to 'express', and 'stand for'. I am convinced that we are all liable to fall into serious confusion by an uncritical use of such words. For example, I am convinced that Bradley's theory of judgement, and with it therefore a great deal of his Logic, is hopelessly shipwrecked in consequence of a great confusion in his mind as to the meaning of 'significance'—'meaning'—'sign'—'symbol', which have to do with representation. (I do not publish my criticism of Bradley; but this criticism I have lately been encouraged to circulate in MS. among Oxford students of philosophy. You are welcome to see it if you like: though I shouldn't care for you to talk much about it—except perhaps to me—especially as among its consequences (not developed in my circulation copy), there is involved an uncompromising rejection of the general account of inference which Bosanquet follows. For I have an uncommon regard for Bosanquet, begotten greatly by his own generous treatment of myself.)

In the strict meaning of 'represent' and 'stand for'. nothing in the world in *itself* ever 'represents' or 'stands for' anything else. In *itself* it can only be said to 'represent' itself and 'stand for' itself; though even this is an abuse of terms. Representation is our subjective act. Nothing has a *meaning* in itself. It is *we* who 'mean'. It is *we* who make something represent something else, something other than itself. (This point I have worked out in detail in the aforesaid criticism) Thus it is we who decide that x shall represent the problematic quantity, and a, b, c, &c., determinate quantities. It is we who make a weeping willow a symbol of sorrow. There may of course be something in the object which prompts us to give it a meaning, e. g. the resemblance of the weeping willow to a human figure bowed over in the attitude of grief. But the willow in itself can neither 'mean' grief, nor 'represent' nor 'stand for' nor 'express' grief. *We* do all that. The fatal thing in Bradley is that he thinks an 'idea' can in *itself* have 'meaning', be 'significant', or 'symbolical'.

The thing we make representative, as opposed to mere symbol, is, in the strict use of representative, always somehow *like* the thing it represents. But there is an extended use. If the existence of A necessitates that of B, we may, taking the existence of A as evidence of the existence of B, speak of A as a *sign* of B—i. e. a sign to us because we infer B from A; but when we are fully conscious that this *is* the relation between A and B, I think we stick at saying A is 'representative' of B, though by a confusion we may perhaps do so, when we are not clearly aware of the true relation between A and B, or not accurately remembering it But we should never say A 'expresses' B.

Now, in the present case, if I understand you rightly, your text seems necessarily to mean that Hs is a sensation which 'mediates our knowledge of' Hx, because Hs 'represents' Hx, or 'stands for' Hx or 'expresses' Hx. Now I would venture to cut out at once the word 'express'—a sensation cannot in any proper sense of the word 'express' anything different from itself. Let us then merely retain 'represent' and 'stand for'. How is it we come to speak of a sensation as 'representative', for I suppose we all tend to do it, not you only? Before criticizing the application to secondary quantities, which themselves (in the Hx form) are *sub judice*, I should like to consider the case where the existence of the quality (the primary quality) is undoubted, and where we are liable to speak of 'representative' sensations.

I think the best case to take is that of *felt extension*, for that gives the most plausible instance, and besides I do not agree with what you say of *visual extension*. Here (in felt extension) we seem to recognize in feeling round the surfaces and edges of a die (e. g) that we have certain feelings quite different from the extension, and yet that it is somehow *through* these feelings that we become aware of the extension. Thus we may get to call these sensations 'representative', without any distinct envisagement of what we mean—and the word 'representative', instead of being an explanation, is itself a problem, and indeed is itself due to a confusion in our ordinary thinking. Now the very description of these sensations (tactual and the like), as *mediating* our knowledge of the extension, implies that they are

different from extension and have no extension in themselves. They are not therefore 'representative' by reason of likeness. How, then, are they representative? As already contended, in no kind of way can they of themselves 'represent' anything other than themselves. If we consider them as representative it must be either on the ground of likeness (excluded in this instance) or because of their association (in their qualitative character and their given temporal order) with extension. We must therefore be already acquainted with extension either to recognize the likeness of the representative sensations to it or to know of the association. But this is impossible if the mediation of our knowledge of extension by the sensations (together with their order) means, as it usually does, that we depend entirely on the sensations for our knowledge of extension at all.

But suppose, for the sake of argument, we know already that a certain order and quality in our tactual sensations and (say) 'sensations of resistance' is caused by the fact that our fingers are moving in contact with the surfaces of an extended object. If we know this in general already, we can apply it to a particular instance, and we might say that this order of tactual and other sensations is 'representative' of extension here, the phrase being really inaccurate (and misleading); our true meaning, then, being that the so-called 'representative' sensations are known to be associated with such and such an extension: and that they therefore can be taken as evidence of the existence of the extension. This waives the question as to how we could have got our knowledge of the correlation of the sensations and extension, and is merely given to show how little can be made out of representation, in even what is perhaps the most favourable case.

As to this knowledge, it seems clear that we could not get it at all *if our datum was the mere sensations, given without any* relation to extension. If we really get both together we might as well call the extension representative of the sensations as the sensations representative of the extension.

The idea of representation, then, in fine, seems to me not only useless in philosophy but misleading as tending to obscure the solution of a difficult problem. In any case observe that here the 'represented' idea (extension) is as clear to us as the

supposed 'representative' ideas, indeed clearer and clearly distinguished from them. Thus there is a science of the so-called represented idea—geometry, which takes no account whatever of the 'representative' ideas.

§ 525. *Sensation (Hs) as 'representative' of a quality (Hx) in the body.*

To apply the above general considerations to the case before us: Is Hs representative of Hx as being like it? No. for then Hx as like a sensation in quality would itself have to be a sensation: and that, according to your own view, would be a 'flagrant absurdity'. It would seem, then, by the above, to remain that Hs would be something unlike Hx, but correlated with it, and that we could only make Hs 'representative' of Hx because we had found both associated (i. e. if we allow the word 'representative' at all). Hx, then, ought for us to have a positive content different—quite different—from Hs, and clearly distinguishable from it: and according to your theory clearly distinct from Hm. Clearly also there ought to be a science of Hx. These seem to be necessary presuppositions of the only intelligible sense in which Hs could be said to represent Hx.

§ 526. *Evidence of the existence of Hx.*

If Hx exists it ought, according to what seem the implications of your representative theory, to satisfy the above conditions. But (1) it seems evident that there is nothing which does satisfy them. As distinct from our sensation Hs, there is nothing in the body with any definite quality clearly distinguished from Hs and from Hm, except only the power of Hm, or, if you like, of the body in general, to produce Hs: precisely the contention of the theory you attack. If there were, there should be a science of it, and there is none. In fact there ought to be a science (empirical at least) of the relation of Hx to Hs and to Hm, but there is no science of its relation to either, the only science of such relation is the science of the relation of Hm to Hs.

But (2) your own argument undesignedly leads straight to the same conclusion: and here I come to the main significance of the fact to which you appeal. 'The fact that in ascribing secondary qualities to things we *normally* think of the things as if they were actually producing the sensations in an hypothetical percipient.' This proves that if we try to find any positive

quality of heat *in* the body which we may suppose ourselves to feel (just as we *feel* extension in it), we are always thrown back on the sensation itself for positive content, and can only give a derivative positiveness to the property the body has of causing it, by thinking of it as producing this definite quality of effect. This means that we are able to say nothing of the body's quality so far, except that it is the cause of this definite kind of sensation, though we may, by a confusion, be misled into transferring what can only be a quality of the sensation into the body itself.

All that you say in this connexion confirms the theory you are attacking, and is indeed precisely the kind of argument which the advocates of that theory would and do employ. They would say that if any one thinks there is a positive quality perceived in the body (allowing the case most favourable to you, when the hot body is *actually touched*) other than the sensation and the mere extension of the body, he may be challenged to assign any positive predicate to this quality in itself (= Hx), and it will be found he can give none—and you have given none whatever—, and a further proof that he is under an illusion and can really give none is that if he tries, he can only describe its positive character by reference to the positive character of the *sensation* which it causes, and can never do anything else; and this either means that there is no quality assignable to the thing except the power of producing the sensation in us, discoverable or implied in our perception (and he therefore agrees with them), or that he is confusedly transferring the quality of his sensation to the body. Now this is just true of your own account, i.e. you really say that no positive character can be found in the thing except by this reference to the quality of the sensation. Now it follows, from the above analysis of such meaning as 'representative' can have, that the quality of a representative sensation cannot possibly enter 'in consequence of its representative function' into the 'essential constitution of the corresponding secondary attributes of matter'. Just because of 'its representative function' it can do nothing of the kind, and the statement is a mere contradiction in terms. This, I think, you will see if you ask yourself now what could be intelligibly meant by 'representative function'. In fact I hold that in this sentence

you have exactly illustrated the unconscious transference of the quality of the sensation into the object. I may here add that I think this explains why some quite acute people among your audience at Oxford were puzzled by your paper. I think they felt they were not quite sure what you were driving at: and some of them were inclined to suspect some confusion, if not in your own thoughts, yet in your presentation of them.

You will see, then, that I cannot help thinking you have proved the contradictory of what you originally intended, or at least that your arguments are on that side and confirm the position you meant to shake. The right formulation of the theory seems to me what I have given above (in § 520).

§ 527. *On the relation of the supposed Hx to Hm.*

As I have said, I consider it a sign that you haven't fully cleared yourself up that you do not ever raise the important question of the relation of Hx to Hm If Hx is really the quality of the body itself which is correlative to our sensation of heat (Hs) why is Hm needed? One might expect you would have to say that Hm is itself an illusion arising from people's not recognizing the existence of Hx. If, however, in deference to science you retain Hm, how is it to be related to Hx? Is it the cause of Hx and Hx again the cause of Hs? If so, the important thing for science would be a theory of the relation of Hm and Hx in the body. Thus, in any case, you would have to propose a new science. If you reject Hm, the new science would recognize Hx and study its causal relation to Hs. If you do not reject Hm, you would introduce a new science to study the relation of Hm to Hx.

But, of course, you know I should maintain there could be no such science, because no positive content at all can be given to Hx. I will here add, what I have already indicated in conversation, that I am fairly sure the secret of the whole puzzle about Bacon's '*forma*' lies in the fact that he himself got into complete confusion between Hs, Hx, and Hm. The real question to ask, I am sure, is not the usual one, 'how does the "formal" cause differ from the "efficient" cause in Bacon?' which no one has been able to answer satisfactorily (or even to give a coherent account of what *forma* itself means)—but we should ask what is the relation of the *forma* to the *simplex natura* of which it

is the *forma*. No one seems to have undertaken this inquiry. If it is done, the result is a mass of hopeless contradictions of statement. Some time ago I thought I detected the source of it all in the fact that the *simplex natura* as conceived in the *body* was precisely Hx, and the *forma* of this is Hm. Bacon sometimes seems to recognize that heat in the body is only Hm, and besides this there is only the effect in our sensory ⟨organism⟩—a sensation—and this accounts for a certain class of passage. But sometimes he unwittingly transfers the sensation into the thing; thus appears the *simplex natura* of heat as in the *thing*, which, nevertheless, derives all its positive character from the sensation with which it is confused This accounts for another set of passages, and the two sets are thus in irreconcilable contradiction to one another.

As the Baconian difficulty is a recognized problem of philosophy, and I believe it capable of being quite cleared up, I had thought of giving a public lecture on it next term. Your paper has itself made me undertake the rethinking-over process which I should have had to undertake, and so I may as well execute my project. The lecture itself was written long ago, I should say fourteen years ago, and I have occasionally delivered it in my ordinary logic course. There is perhaps nothing in it I want to alter, at least as far as I can see. I dare say it will interest you to know that I have given my course on 'hypothetical thinking' again this year, and I think I have improved it by developing it and making it quite definite in certain directions.

§ 528. *The case of the Primary Qualities.*

(A) For the main issue, which must be settled one way or another by the preceding, it would not be necessary to go on to what you say upon the primary qualities.

With regard to your words 'This analysis of the secondary attributes of matter holds good in all essential respects for the primary also', it doesn't follow, because, as I think, the analysis is not valid for the secondary, that something like it may not be valid for the primary; and so I propose to consider the primary. Moreover, I don't find I can agree with your account of sensible extension, &c., and want to talk about it because I think your account of the secondary has infected your view of the primary qualities. The facts in the case of the secondary qualities seem

to be (taking heat) that we perceive only our own sensation (Hs) and possibly the extension of the hot body, *if we touch it*: we infer a special power in the body to cause it, and we arrive at our idea of the state of the body which has the power (Hm) by inference and there is nothing else in reality. Hs, then, is perceived, Hm is not perceived but inferred by a scientific theory. Add that there is the illusion Hx, letting Hx now stand for our tendency to transfer confusedly the quality of our sensation (Hs) to the body.

What is there to correspond in our knowledge of extension? Take *felt* extension first. There are certain tactual and muscular sensations: call these Es; quite distinct from these is the extension felt and which we say we feel in the body: call this Eb.

Eb is conceived as distinct and different in its nature from Es.
Hm „ „ „ „ „ Hs.

So far the relation of Eb to Es is like that of Hm to Hs. But there is a fundamental difference.

Hm is the mere result of scientific inference, it is not the object of perception, we neither perceive it actually, nor suppose we perceive it. Eb we certainly suppose we *perceive*. We say, not we feel the tactual feelings, but we feel the extension itself. A corroboration of this is that we are able to describe the extension we say we feel, as it appears to us in perception or in what we call 'feeling' it, we cannot possibly describe Hm from the experience in which we have Hs.

If it is objected—we don't really perceive Eb but infer it, and are under an illusion in thinking we perceive it, it may be answered: (1) If so, how is it we do come to think we perceive Eb, and never come to think we perceive Hm? Why can we from mere perception describe the one and not the other? (2) There would still remain the fundamental difference that we at least *suppose* we perceive (feel) the one and do not suppose we feel or perceive the other. (3) Eb, or extension in a body in general, cannot possibly be something originally unperceived and only inferred from what is perceived: for, as shown in the discussion of the idea of representation, if what we perceived were only tactual, &c., sensations, without the perception in them of any relations of extension, extension could never be inferred from them.

Thus again, while there is this correspondence that Hm is conceived as the cause of Hs, and Eb also as the cause of our having certain sensations (Es) in a particular order, there is this fundamental difference that our idea of Hm is *only* acquired by our searching for a cause of the given Hs, but our idea of Eb is not acquired merely from a search for a cause for Es. . .

Is there anything to correspond to Hx, not as you conceive Hx, but as an illusion, as I conceive it? There probably usually is. We not only think of the hardness of the extended substance in association with our feeling when we endeavour to compress it, but confusedly transfer the quality which belongs to our feeling to the thing itself.

Therefore in the case of the sensations relative to *felt* extension (not seen), I should say that these sensations and their order, though of course correlated to the extension in the body, most certainly do not 'enter into the constitution of it', and do not 'represent' something other than themselves, i. e the extension. As to the former phrase, the context shows that you hardly mean it seriously yourself, for 'only so far as', &c, practically cancels anything naturally meant by such a strong phrase as 'enter into the constitution of it', and as to the second phrase I needn't repeat what I have said about 'representation'.

§ 529. To come now to visual extension.

It is here I specially find the 'infection' of your view of secondary qualities. You hold here that we perceive a visual (coloured) extension, apparently, *which is as much our subjective state as the sensation of heat*, and is merely representative of the real extension which is other than itself. The words in italics seem absolutely necessary to your view and to the analogy you institute with the secondary qualities.

I confess I cannot understand how you could have persuaded yourself that the distinctions you make in this part of your argument were tenable, if it had not been that you were affected by the view you had developed about the secondary qualities. Let us take first your instance of the extension (visual) perceived with closed eyes.

This is nothing but an instance of the following. Suppose we are looking at a flat surface—say the wall of a room. We see the rectangular shape of the wall when the eye is at a sufficient

distance. This surface is a part of 'real' space, of the space 'in which attraction varies', &c. As we move the eye nearer the wall, its limits which constitute its shape get out of the range of distinct vision. When the eye is near enough to the wall we lose sight of them altogether; when it is close to the wall we see but a comparatively small part of it: the centre of this distinctly, and parts at an increasing radius less distinctly, fading off into nothing visible at all by imperceptible gradations. And so, as in your case, we cannot say what shape it has, but nevertheless what we see is a part of actual extension and *has a position in actual space*, for it is on the wall Now in the case of the closed eye—if you really mean we perceive or see 'visual extension', as opposed to merely imagining—the only difference is that the surface has come into actual contact with the eye, for it 's nothing but the eyelid itself which is seen by transmitted light, and so what we see is actual extension, it certainly has a *position in real space*. As the case of the wall shows, the fact that we can't tell what shape limits what we see is no proof whatever that it is merely some subjective extension that we see. I feel I cannot be too emphatic about this, and I feel you have been seriously misled. You say again it is an 'intrinsic impossibility to express its magnitude in feet and inches'. The magnitude of any part of it, on the contrary, can just as much be expressed in feet and inches as the magnitude of any part of the visible part of the wall, whose limits we can't perceive on account of the nearness of the eye. The only way in which it is impossible is irrelevant, i. e. it may be impossible for me to use a measure so as to measure a given bit of the extension, but nevertheless it *has* a dimension expressible in inches.

§ 530. (B) Leaving this subject for the moment, let us consider the perception of a piece of coloured surface, as of a blot of colour on a piece of white paper. This is with you 'visual extension'; you also speak of it *totidem verbis* as 'sensation' and 'extended sensation'. This 'visual extension' again with you is not in the thing (and if seriously meant to be extended *sensation*, could indeed not be) but has a 'representative function', i. e. represents something in the thing.

With you 'representative' means 'representing something

other than itself'; it would be important to say what 'other than itself' exactly means, whether 'other' as one smarting pain is other than another such pain or different as the smart is different from the thorn which causes it—'different in kind'. You don't expressly say, but, as I have shown above, your theory necessitates the latter kind of difference, for if Hs, which represents Hx in the body, were *like* Hx, what you call the flagrant absurdity would be committed of supposing the body had a sensation.

In several ways it may be shown that this view of the relation is quite untenable.

For simplicity, suppose the blot to be bounded by rectilinear edges, and to be a parallelogram.

(1) The quality of the colour—supposed to be a sensation—is irrelevant as, e. g. green, or red, or blue: it is the extension only that matters. This extension I describe as a parallelogram, and say it is bounded by straight lines. By hypothesis this is to represent something in the body different in kind from itself. This extension in the body is different in kind from anything of the nature of a parallelogram and cannot be bounded by straight lines. What is that? Clearly we could say nothing whatever of it, and you would be committed to something worse even than 'a thing in itself', viz. to 'an attribute in itself'. (In our conversation in the quadrangle, in order to reply to a remark of mine, you committed yourself to the view that the real extension of the thing was for aught we knew as different from the visual as is Hm from Hs. I build no argument on a statement in a hasty conversation; but it confirmed me in thinking you had not worked out your position clearly and realized its implications.)

But (2) we always assume that the extension in the thing is exactly the same in kind as the visual extension, and the science of geometry depends entirely upon this. (This is one of the reasons why your criticism of Kant is invalid.) There could be no science of the 'extension in itself' any more than of the thing in itself. Moreover, science assumes *not* that the extension of the blot that I see is *like* the extension in the thing, or *represents* the extension, but that it *is* the actual extension in the body. It is visual extension simply as real extension *seen*.

(I mean of course science as science. If the scientific *man* was asked, he would very likely give a ridiculous answer. They usually do, for they are more incapable of reflecting on their own presuppositions than the ordinary 'plain man'.)

(3) Let us analyse this 'visual extension' and inquire what parallel it presents to the secondary qualities. If we attend to the colour alone and distinguish Cs, Cx, Cm, then Cm as the movement of particles of ether actuated somehow by the body corresponds to Hm, for it is not perceived and is solely an inference in scientific theory. But the relation of Cs to Cx is very different from that of Hs to Hx. As already indicated, whereas we can and do locate Hs in our sensory organ, even though at the same time we may erroneously locate something like it (Hx) in the body, colour is clearly and distinctly located in the body perceived and we no more locate it in the perceiving organ (the eye) than we locate there the extension of the body itself. We have no choice: we must locate the colour in the surface: we cannot imagine yellow save as on a surface. Nor will all our scientific theory, that it is not there but our sensation, enable us to 'locate' it in the eye or represent it otherwise than as at a distance from the eye and on the surface. The colour cannot be made in any sense 'representative' of the extension, simply because we can't perceive it except as 'extended', i. e. on an extended surface, nor even imagine it without extension. Thus if we 'perceive' colour, we must at the same time 'perceive' extension.

§ 531. (4) But the important thing now is the extension element in this; because it is as *extended* sensation, or visual *extension*, that you make it '*representative*' of something in the body—apparent and not real, and, as it would seem, something merely subjective.

As remarked in the discussion of the foregoing point (the comparison of secondary qualities with extension), the perception of colour is impossible without the perception of extension, not at all as an inference but as a necessary part or aspect of the same perception, and such extension again is absolutely unintelligible save as the extension of a *surface*—i. e. a real surface. If two colours are juxtaposed which do not shade off imperceptibly into one another, i. e. two colours homogeneous

in themselves—as the coloured blot on white paper, we necessarily perceive their common boundary as linear, that is really the common boundary of the surfaces to which we refer the colour. (N.B. If two sensations different from one another, other than colour (granting it to be a sensation), succeed, no such idea of a boundary arises, e. g. a taste and a smell, one toothache and another pain, one sound and another.)

We 'perceive' the limit or boundary just as directly as we do the colours.

The aim of this detailed analysis (in which for clearness I have put the same thing in different ways at the risk of wearying by repetition) ⟨is to show⟩ that in no sense does the perception of what you call visual extension (as a kind of what you call sensible extension) involve any extension in sensations as sensations. When we analyse it, we find that though the extension ⟨is⟩ inseparably associated with the colours, it is not assigned to sensations but to the surfaces, and to these *alone*, to which the colour (if this is to be sensation) is referred. It is the coloured surfaces which are extended, the coloured surfaces which have a boundary. It is either a confusion or a mere *façon de parler* to speak of the colour *sensation* as extended, and as a *façon de parler* is only excusable as meaning that the surface to which the colour is referred is extended. Therefore, strictly speaking, in a philosophic discussion such language should be avoided and the accurate expression substituted Accurately, then, the sensations, whether tactual or of colour, are *not* extended, and the idea of extension is *absolutely inapplicable* to them

Now the view that visual extension (and sensible extension in general) is merely subjective, is nothing in the thing, is an appearance as opposed to reality; an appearance which somehow represents a reality, which as unreal and merely subjective has no *position* in real space, depends wholly upon the view that sensations can be in themselves extended, and is therefore, I am obliged to think, entirely untenable. This seems in itself enough to me, but I will consider some of your principal statements in support of your view in detail.

§ 532. (5) I should say it was impossible to develop such a theory into details without *contradiction* or some *verbal con-*

fusion. I seem to find contradiction in what you say of the apparent size as given by the visual extension in contrast with the real size.

The visual extension as consisting, according to you, of representative sensations *must*, I have contended, according to you, be different in kind from that which it represents. You never say this quite clearly, though it is a point on which you ought to have pronounced definitely, and I can't help thinking that it was a sort of instinct which prevented you. You use, for example, the very strong expression 'extension as a characteristic of visual sensation is *quite distinct* from the extension of things in space', of which the natural interpretation is that the two things differ in kind. But your remarks on real and apparent size entirely contradict this The contrast of the apparent size of a thing with its real size 'as measured in feet and inches' means simply, e g., that it looks 2 inches long whereas it is 3. This means again that the apparent length is a *line* 2 inches long and the real length a line 3 inches long. (With this kind of difference between a real and apparent distance one is very familiar in the exercise of 'judging distance'.) This is only the comparison of one line with another, of two things (two extensions) exactly the same in kind.

Verbal confusion I seem often to find in what you say of this curious 'sensible extension' as opposed to real extension. First you say sensations are extended, then you seek to take away the paradox, by adding an explanation of 'extension', in the case of sensation, which takes all that 'extension' means out of it; ⟨thus you say⟩ 'they are not extended in the *same sense* as corporeal things', 'bodies are extended in space', &c. But what other sense could there be if it was extension at all? This to my mind is as bad as saying 'our sensations are numbered and counted, but then it is not the same kind of numbering or counting as applies to corporeal things'. 'Bodies are extended in space', and it is therefore implied that sensations are extended but *not* in space. This to my mind is sheerest contradiction. What is extended *must* be in space or it is not extended. For instance, if it is extended as a surface (and it is really this case we have before us), that necessarily presupposes space on both sides of it. It will not do to meet the difficulty by allowing

the 'extended sensation' to be in a space of its own, quite different from and not included in 'objective space', or the 'single homogeneous infinite space', as you describe it, for this would involve a terrible mess which you will realize of course well enough.

I seem to find a good deal of verbal confusion. This 'sensible extension', notwithstanding the difference you make between it and the 'real' extension, you speak of as a *manifestation* or *expression* of *spatial extension*. Here I note the expression '*spatial extension*' which would be paralleled by 'numerical number' and implies a non-spatial extension (= a non-extended extension) just as the other would imply a non-numerical number. But anyhow, what could these somewhat loose and popular phrases 'manifestation' and 'expression' mean philosophically? How could the sensible extension be the 'expression' of the spatial extension which is quite different from it? It seems connected with the fallacious use of 'representation'—'expression' here is only to be tolerated as a metaphor, as such it is of no use whatever as an explanation; one has to explain what the metaphor means. 'Manifestation' is used legitimately enough in philosophy for the particular as a 'manifestation' of the universal, but then you do not mean to relate 'spatial extension' to 'sensible extension' as universal to particular. Possibly, as the context with the word 'appearance' in it suggests, 'manifestation' means the manifestation of spatial extension to *us* in perception. But then obviously it must be spatial extension its very self which is before us—spatial extension must as manifested be the object of our perception: and *not* an extension 'quite different' from it. If the latter presented itself as a 'manifestation' of spatial extension it would be a mere fraud: it would be only a 'manifestation' of the 'sensible extension' and not of 'spatial', for clearly what was really manifested would be simply the 'sensible extension' before us.

If you want to introduce Jones to me, it won't do to introduce to me somebody 'quite different' from him; that wouldn't be 'manifesting' *Jones* to me, but that other person.

§ 533. (6) *Apparent and real size.*

The supposed difference of apparent size is what seems to have affected you greatly in your view of the difference between

sensible and real or spatial extension. Here again I think you have assumed what is little better than a popular distinction without criticism. Just as 'representative and represented' so 'real and apparent' seem to me regular trap words, which cannot be safely used in any particular case without a most patient examination of their meaning and justification in the particular instance. The distinction of real and apparent size seems to me very slippery and to require more than usual care—all the more so because at first it may seem familiar and not to require any discussion at all. There are two ways of treating the theory of 'sensible visual extension' *versus* 'spatial extension'. The first and the easier for mere negative purposes is to show the untenable consequences of adopting it. The second and harder, but the one required to complete one's estimate of the true facts, is to analyse what is really meant by and involved in the conception of apparent *versus* real size.

(*a*) Assume the theory to be true. Suppose I see a chair before me upon the lawn. The strict meaning of the theory absolutely necessitates the view that the 'visual extension' or the extension which I see is my own 'extended sensation' (your phrase); that it is not the real extension of anything, for such extension is in space, while sensible extension (*ex hypothesi*) cannot be. The same holds of the lawn itself and of all the objects (extended) which I see about the chair. These 'appear' to me to be 'in space', *there seems to be empty space* between them—indeed this is absolutely necessary to and involved in my thinking that the chair has any extension at all. (*This*, I think, you have hardly realized.) Now the space, which indubitably thus 'appears' to me and in which the apparent objects have their extension, is non-real; for, if it were real, the objects I appear to see would have their extension in real space, whereas by hypothesis they have not. (We thus come in a different way to a previous thought.) Is it necessary to develop the consequences of this further? May I not assume that it leads to an uncompromising subjective solipsistic Idealism? (N.B. that this space (apparent) couldn't possibly be called 'extended sensation', and so has no place in your non-real extension.)

(*b*) No doubt we are accustomed to say that as an object is removed farther from the eye it '*looks smaller*'. We believe it

has not become smaller, and so people come to speak of the variation as 'mere appearance' and so to call the size itself which seems to vary 'apparent size' as against 'real size'. Again the apparent variation of size is said to correspond to a real variation of the distance from the eye, and is often said to be 'representative' of this real variation.

All these phrases are most misleading and tend to pernicious consequences in ordinary psychology In your own investigation I think you omitted to ask yourself at the start what could be meant by real size. You conceal the difficulty from yourself by describing the real size as the size measured by *feet and inches*. 'Thus we contrast the apparent size of a thing . . . with its real size as measured in feet and inches.' This of course suggests two important questions :

(*a*) How is the *apparent* size measured if *not* by feet and inches?—a question which doesn't seem to have occurred to you. It must be measurable or it wouldn't be a 'size' at all, and we couldn't speak of its becoming smaller or larger. Moreover (part of the same question), since the apparent size is a size and the real size a size they must stand in a relation of measure. So any apparent size must stand in a definite relation of magnitude to the real size Moreover, we can increase and diminish the apparent size *ad indefinitum* by removing the object far enough or bringing it near enough to the eye. For this purpose the magnifying glass is merely an instrument which enables us to see the object, when brought much nearer the eye than is consistent with clear vision without it Consequently some one of the apparent sizes must coincide or be equal to the real size (*and be measured in feet and inches*). There must therefore be some distance from my eye at which the apparent size of the object is its real size. What is this distance? and how can it be determined? Clearly this result is absurd, for there could be no reason why one distance should have the prerogative over another. I expect you will readily agree in the absurdity of this necessary consequence. But it shows the untenableness of the ordinary distinction of apparent and real size. You come partly into this train of thought, but you have not followed it to its consequences, nor realized that it should have made you revise all that you had stated previously. E. g. you say 'still

less can we select this or that apparent extension and identify it with the real. For each of them has in principle just as much and just as little logical title to be so regarded as any of the others.' This makes me say you will agree with me about the foregoing absurdity; but you have not seen that the ordinary distinction of real and apparent size, which you yourself use, makes it necessary that there *must* be one of the apparent sizes or extensions which is equal to the real.

(*b*) Virtually defining real size as that measured by feet and inches, you have, as I said, disguised the difficulty. For now the second question arises, 'What are feet and inches?' To say the real size of our object is two inches is to say its extension is twice a *given* extension which is called an *inch*. But what is the real size of this extension? What is the real size of the extension which you call an inch? The question shows that we are no 'forrarder' in the definition of real size by this reference to feet and inches

The distinction of real size and apparent size, as above described, I must then hold to result in hopeless contradiction. Your theory depends very largely upon the distinction, so conceived, and so for this reason again I must think your theory untenable.

§ 534. We must therefore undertake anew the investigation of the facts upon which the misleading language employed above [§ 533 (6) (*b*)] is based

We may introduce this by asking the question, 'How should we go about to show that the real extension of a given line was two inches, in reply to the inquiry as to what it was, or as against a conjecture that it was, say, an inch and a half?'

Suppose we are given a six-inch rule. We should define an inch as the sixth part of the extension of this rule, which we may again simply define as the rule we are holding in our hand. We want to compare the real extension of this rule with the real extension of the given line, e. g. by applying the one close to the other.

But how can this be done, since, by hypothesis, in experience only the sensible (take here the *visual*) and merely apparent extension of the given line and the sensible or apparent extension of the ruler are given?

The answer would doubtless be given readily enough. 'We simply *apply* the visual extension of the ruler to that of the given line or edge, and if the line's visual extension coincides with two-sixths of the visual extension of the ruler the real length of the line is two-sixths of the real extension of the ruler, i. e. two inches. If we ask 'Why?' the answer will be '*because then the real extensions are in the same ratio to one another as the apparent extensions*'. These answers, probably given with some confidence, involve a pretty coil; even if we omit to criticize the application of one visual extension, as equal to one extended sensation, to another extended sensation.

(1) This implies that the apparent extensions stand in a ratio to one another, and so are measurable. Nor can the implication be avoided that the apparent stand in a ratio to the real. And from this of course it follows, as before, that some apparent extension must coincide with the real extension, with the consequent absurdity

(2) How can we possibly know that the real extensions ('spatial extensions' you call them) are in the same proportion as the apparent? For the former are never objects of our experience—by hypothesis—but only the latter, i. e. *all* extension we see is visual extension This is an unanswerable difficulty.

(3) The idea of the *application* of the visible extension of the ruler to that of the given line has also serious implications.

In mere visible extension we can make the visual extension of the given line appear to coincide with any fraction of the ruler. It will be readily replied, 'But that is because the ruler and object are not at the same distance from the eye, and though we may see no space between them, they are not in contact; the true measure is got when they are at the same distance from the eye.' But this refers to actual real (spatial) distance from the eye. That we can never get at, our perception being only of the 'sensible extension', and in such a case we can only compare what we can perceive. Consequently we could not know when the real distance from the eye was the same for the ruler as for the given object.

(There is a point about the use of '*representative*' which I ought to have mentioned explicitly. I have continually been

presupposing it. If two things different from one another are correlated, why, in this use of 'representative', do we make the one (A) 'representative' of the other (B) and not the latter (B) representative of the former (A)? The one (B) is said to be 'represented' because it is supposed not to be itself present to our consciousness; the representative (A) is that which is supposed present to consciousness. This is vital to the distinction of 'representative' and 'represented', and perhaps tends to be forgotten at important points.)

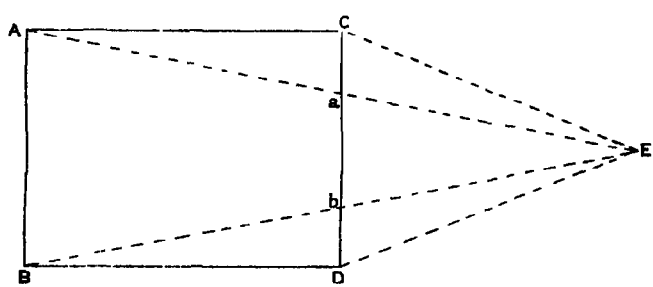
Or, to put it otherwise: the apparent sizes which correspond to the real sizes, when the latter are at the same distance from the eye, are to be compared, as these are in the ratio of the real sizes. But how are we to know which apparent sizes correspond to the same distance of the objects? For it is only the apparent sizes which are present to consciousness, neither the real sizes nor the real distances from the eye are, by hypothesis, present to consciousness, nor indeed could they be. The question is quite unanswerable by the theory before us. For instance, we couldn't say they (the apparent sizes) had the same apparent distance from the eye, for that would not only put them in a space in general of their own (of which we have already seen the difficulty), but since it would be a distance from the eye, this subjective space would, at the eye, have a common point with real space—an additional absurdity—but perhaps it can be dodged by the doubtful substitute of an *apparent* eye for the real! Or suppose, to avoid such a difficulty about the position of the eye, we say the visual or sensible extensions of the ruler and the object, which (i. e. object and ruler) are at the same real distance from the eye, are observed to be conterminous (at least the tactual extensions, as there are more obvious difficulties about supposing we observe conterminousness in visual extensions by eye only). This conterminousness inevitably means proximity in a space including both the conterminous extensions. (Besides, though this is not necessary for our immediate purpose, there could be no test for such conterminousness even in the case of touch—an interval of time between the apprehension of the sensations constituting one tactual extension corresponding to one surface, and the apprehension of those corresponding to another, does not necessarily

correspond to their discontinuity or non-conterminousness. This can be proved, but we needn't stop for that now perhaps.) If we try to do away with such space by reducing the sensible extension, whether visual or tactual, to a sort of analogue to a surface with no space on either side, then sensible extensions representing surfaces of objects at very *different* distances from the eye would, in such 'extension', be conterminous with one another.

§ 535. This may serve to introduce the positive analysis of the facts: for the explanations which are inevitably given to account for the variation of 'apparent' magnitude, and which cannot themselves be accounted for by the theory now criticized or reconciled with it, are the result of a very different view of our perception of extension and show that this view is the ground of all our thought and perception of the spatial.

I shall try to show that it is precisely because we reason as if we saw the *real* (spatial) extension of the real object that we are able to explain what are (as I hope to show) misleadingly called 'apparent variations in its seen size', as due to a variation of distance from the eye. That is, our mental attitude presupposes that we see the real extension in real space and not some *simulacrum* of it which is not in any space at all.

The explanation which would always be given of variations of apparent magnitude while the real magnitude remains the same is of the following kind.



Let A B D C be the tops of four vertical posts rising to the same height above the same horizontal plane. Let E be the position of the eye of the observer and let this be in the same horizontal plane as A B D C, outside the rectangular quadri-

lateral A B D C, as shown in the figure. Let the straight line AE cut CD in *a* and the straight line BE cut CD in *b*. Let AB = CD and be parallel to it. Then the observer will see the points A and B between the points C and D, which more accurately expressed means that he will see the point A immediately behind, or in alignment with a point *a* between C and D upon CD, and B behind, or in alignment with, a point *b* upon CD between C and D. The two points *a* and *b* are 'between' C and D. Similarly every point between A and B will be seen behind or in alignment with a point in *ab*. The observer then will see A, B, C, and D in this kind of relative position C *a b* D, measured along CD.

This is then described (erroneously) by saying that AB 'looks smaller' than CD · and the above figure is supposed to explain why AB being equal to CD 'looks smaller' than CD to the observer at E.

We are only concerned to show what this theory—universally accepted—presupposes

It presupposes a fact about the manner of our visual perception as given and known—it does not attempt to explain this fact.

This fact is that the observer sees a point in the real extension of a real object in the direction (real) of the line drawn from his eye in real space to the given point in the real object.

It would doubtless be said—as another way of describing this fact—that the point seen 'appears' to the observer in the direction of the line from it to his eye. The expression may seem harmless, but it is likely to lead to a fallacy, for 'appearance' tends to be opposed to reality. So it may seem as if the point seen *merely* appeared in the direction from it to the eye; but it really *is* in this direction.

Now the important thing in this presupposition is that the observer is supposed to be looking at the real extension of the object itself, and also that what he *sees* in so looking is definitely a part of the real extension. He is looking at the point A as the end of AB in the figure, and what he *sees* is the end of this line AB. It is most important here to keep out of one's mind all physiological and psychological theories to *account* for the way we come to do this; our attention must

be fixed solely on the conception of the fact as implied and presupposed in this theory with its figure, i. e. on the conception of what we do apart from any theory of how we come to do it. Since the lines EC and EA are apart, or in different directions, and the points C and A are in these different directions and are seen in the direction in which they are, C and A will be seen apart, i. e. A will not be behind C. Similarly B will not be behind D. This is a result of geometry and of the geometry of the space as the real space in which A, B, C, D, and E are. The geometry further shows that A and B will be seen 'behind' points *a* and *b* between C and D, and that in general the whole line AB will be seen as behind a line *ab* less than CD But this relation of AB to *ab* is no illusion, no mere 'appearance': it is an objective fact. When we say we *see* A behind *a*, this means that we *see* A in the same direction as *a* from E.

This means that we see A upon the same straight line from E as that upon which *a* is. But A *is* actually on the same straight ⟨line⟩. So, what is really true, we see a true relation between A and *a*, and so in general a true relation between B and *b*; and not a mere illusion or a mere appearance. Moreover, and this is of the last importance, it is just because we are seeing the real extension of AB and seeing the real A and the real B, that we have this particular visual perception (which we call perspective). Or, to put it otherwise, because we are actually seeing the real extension of AB and not some apparent extension different from it, [that] we see AB and *ab* in this relation, for it is the real AB and no other length that stands in this relation to *ab* and E.

In short, instead of perspective being an illusion or at best a mere subjective appearance, the whole geometrical theory of it implies that we see the actual extension of things in actual space and the 'perspective' itself is nothing but the recognition of true objective spatial relations, determined by the real relations, of the real extensions seen, to the position of the eye in the same real space. Now to see a real extension or size is not the same as being able to measure it and say what size it is. Measuring size means assigning the proportion of the extension to be measured to a given extension In this accurate sense we do not in seeing a real extension see also its size, simply because

we do not see its relation to the given measuring extension. We only do it exceptionally if the length of the measuring extension and of that to be measured are in actual contact, and then only in one of two cases, either (1) when the two extensions are exactly equal and their extremities are seen to coincide, or (2) when the measuring extension is graduated like a ruler and the extremities of the extension exactly coincide with ⟨two of the⟩ graduations marked. The general case, then, in perception, whether visual or tactual, is that we certainly *do not see size or perceive size.*

§ 536. This consideration enables us to see the misleading tendency of the customary expression that the 'apparent size' diminishes as the object recedes. What really diminishes is the part *ab* of CD 'behind' which AB is seen, and the diminution is no mere appearance, it is really an objective fact—it appears but it is also real—a reality here, *as* reality, appears.

To say the apparent size diminishes is a mere *façon de parler* and, as often with such *façons*, a very treacherous one. Now that we realize what size means, it is obvious that in 'seeing' as such we do not see size *at all*, and so *a fortiori* we cannot be said to see an *apparent* size, and so cannot be said to be seeing an apparent size getting smaller.

We do indeed in practice speak of the apparent size of an object, but we do not mean the above by it. We see an object in the distance and we try to guess its size—an operation which presupposes that we do not *see* size. We guess that a distant object (the side of a building) is ten yards long. This is our estimate of its real size, but we call it 'apparent', meaning simply and solely that it is our *guess*, a guess depending often on a guessed distance. So we record our guess, if necessary, as the real *size* guessed. If now we see a horse walk up close to the building we know the real size of the average horse and take its length as a measuring unit, and then can see that the side of the house is more like twenty yards than ten. We may express this by saying that the apparent size was 10—i. e. we guessed the real size to be 10—whereas the real size was nearer 20: the latter is also merely the result of a guess, but a guess with better data.

Now if we move, say, 100 yards farther off, the horse remaining still, we don't say the apparent size diminishes—in the discussion of size—on the contrary the apparent size (our guess) depends on a comparison of the extensions of the house and the horse, the result of which may seem to us exactly what it was before . . .

It is true that the further objects are away, the more difficult does it become to judge their real size. In the case, e. g., of the sun, it is impossible to form any approximately plausible conjecture. But it is not always true that our estimate of the size of the object is less for a greater distance than a nearer, in practice it is sometimes the opposite. And in any case this is not the meaning of the expression that the apparent size of the object diminishes as its distance increases.

We have now attempted to explain the true meaning which corresponds to this misleading language, and tried to show how sorely such language needs analysis and how important it is that it should be undertaken In fine, then, the theory appealed to, and universally admitted, to explain what is called the diminution of apparent size with increase of distance implies that we *see* actual extension, and not any subjective *simulacrum* of it, the diminution or variation to which it refers and which it explains is not a merely 'apparent' variation, but a true objective variation which 'appears' to us, and is not on that account a mere appearance.

Further, we do not usually see size at all, and *certainly not* in the experience called variation of the apparent size with the distance.

§ 537. *Refraction and reflection.*

The theory and its figure imply normal conditions of vision. We have to consider the visual 'image' in refraction and reflection. For there we have to modify the presupposition to the form (which includes the previous) that the object, or point in it, is seen in the direction in which the ray of light from it enters the eye. In the case of reflection it would be said ordinarily that the position in which the object appears is not its real position, it is an illusion and therefore a *mere appearance.* Further, the reflection is called an 'image' of the object, and

thus again distinguished from it as mere appearance. But now notice again the absolutely necessary presupposition of the explanatory theory itself. The very distinction of the 'image' from the object implies that what we see 'directly' *is* the real object and not an 'appearance'. But if this is so, clearly what we see as image is also the real object—it's the same object 'seen round a corner', so to say. If it is assumed that the real object is seen by means of the rays of light proceeding from it (no matter how they mediate the result), it follows strictly, just as much in the case of the reflected image, that what we are looking at is the real object and what we *see* is the real object. And of course the *geometrical optics* of the explanatory theory expressly assumes this, though still speaking of the 'image'.

As to the *apparent* position of the reflected object, note again that the explanatory theory implies not that it is some mere appearance that we look at and see, but that we locate what we see, i. e. the real object, in a wrong position in *real* space. That is the important point, that 'the appearance' is our error in locating the object not in any *subjective* space or any 'apparent' space, but in a wrong position in *real space*: implying therefore that the space we suppose ourselves to be perceiving, or to be aware of, is *real* space.

In short, according to the explanatory optical theory, what we see is *not* a mere appearance, nor is the space in which we see it an apparent or subjective space we see the *real* parts of the real object and locate them in the *real* space though in a wrong part of it The theory would entirely fail if these assumptions were not made.

Finally, the theory enables us to substitute for the wrong judgement of position of the object the recognition even in this of an *objective fact*, viz. the real position in real space of the rays of light from the real object which enter our eye.

It might be said that, owing to the hypothetical form of the argument at the beginning of this section, the alternatives really are that either both reflection and object are real objects seen or that they are both images. But the latter alternative is excluded by the geometric optics of the explanatory theory, which (as shown) demands that the directly seen object should be real.

All this applies, *mutatis mutandis*, to refraction. If we see an object through an uneven pane of glass, the theory implies that what we see are parts of the real object, just as much as if the pane were not there, &c.

There is much more to be considered in relation to these last subjects. I have confined myself to what seemed relevant as against the view that the extension we perceive in seeing is something merely subjective and an extension merely in and of our sensations in no space at all. My argument is that the very theory by which the statement that 'apparent size' varies with the distance is justified, and from which it is therefore inferred that 'apparent size' so varying proves what we see to be an extension not of any object at all but of our sensations, actually implies as a necessary presupposition that we see the real extension in the real space.

§ 538 *Note upon a certain confusion to which we are liable in regard to the conception of appearance.*

If we perceive some property of an object, there is presupposed on the one hand the property of the object as existing on its own account whether we perceive it or not; and as distinct from this, our act of perceiving or recognizing the nature of this property.

This latter, the subjective act of ours, is sometimes spoken of from the side of the object as the *appearance* of the object to us This 'appearance' then gets distinguished from the object, and that in itself is justified in so far as our subjective act of recognition of the object's nature is not the same as that nature. But next the *appearance*, though properly the appear*ing* of the object, gets to be looked on as itself an object and the immediate object of our consciousness, and being already, as we have seen, distinguished from the object and related to our subjectivity, becomes, so to say, a merely subjective 'object'—'appearance' in that sense. And so, as *appearance* of the object, it has now to be represented not as the object but as some phenomenon caused in our consciousness by the object. Thus for the true appearance (= appearing) to us of the *object* is substituted, through the 'objectification' of the appearing as appearance, the appearing to us of an *appearance*, the appearing of a phenomenon caused in us by the object. (The thing to emphasize on the

contrary is that the so-called appearance is the appearing of the *object*, that is, we have the nature of the object before us and not only some affection of our consciousness produced by it.)

It must be observed that the result of this is that there could be no direct perception or consciousness of Reality under any circumstances or any condition of knowing or perceiving: for the whole view is developed entirely from the fact that the object is distinct from our act of knowing it or recognizing it, which distinction must exist in any kind of knowing it or perceiving it. From this error would necessarily result a mere subjective idealism. Reality would become an absolutely unknowable 'Thing in Itself', and finally disappear altogether (as with Berkeley) as an hypothesis which we couldn't possibly justify.

I venture to think that this insidious and scarcely 'conscious' dialectic has done much mischief in modern metaphysics and theories of perception, and that people, who would disclaim subjective idealism entirely, unconsciously fall into it.

§ 539. *Modern accounts of the theory of vision.*

It is extremely important to distinguish the nature of our acts of consciousness from a theory of the manner in which we come to perform these acts. But the two seem to me to be often confused. The theory which psychologists give of the origin of our acts of perception unconsciously affects and corrupts the account of these latter. The facts assumed are partly at least deductions from the theory, and do not represent the actual facts of our perceiving consciousness.

Thus, since Berkeley's time, there seems to be current an account of visual perception which is absolutely false to experience. The psychologist's account of perception commits him to a view as to what the data are which are produced in the consciousness of the perceiving subject, from which the subject is somehow to advance to the perception of the object. These data are really presented as something which the subject has to *interpret*. But the account given is entirely false, for on examination it will be found that the subject, in order to effect the interpretation, must be already equipped with all the ideas

about the nature of reality which he is supposed to acquire through perception and from the data.

It is among other things generally assumed that we do not 'see distance' and that somehow the original visual data are all in space of two dimensions.

This is an absolute falsification of the facts. The object cannot appear to us as in 'space of two dimensions' strictly understood, because there is no such thing as 'space' of merely two dimensions. It is absolutely inconceivable to us and therefore to us objects could not be presented in such space. The only intelligible sense of 'space of two dimensions' would be *a* space of two dimensions, as a *surface*, and, strictly speaking, a plane surface. If we had any such perception, we could not represent the surface as a surface at all save as having a position in space, and having space on both sides of it. (We cannot think of any extension at all, except as in space of three dimensions.) And clearly no position could be assigned to such a surface, to which our perceptions are supposed ⟨to be⟩ confined. Further, we most certainly do not refer our 'visual data' to any such surface.

The only chance for such reference would be if there could be present to our perceiving consciousness space which consisted *only* of a surface. But this, as we have seen, is absolutely impossible and a contradiction to the idea of space itself. Again, as the apprehension of the supposed surface necessarily involves that of 3-dimensional space in which it is, the presupposition of the theory, put in the form that we do not see distance, is falsified. In fact such an account of visual perception—whatever form it takes—and it perhaps is generally put confusedly without any full consciousness of what it involves in the way of mere surface space, &c.—is a mere untrue figment of the psychologist to which he is compelled by his theory, instead of being a representation of the facts which his theory is to explain. His theory therefore itself is untenable, and there will be no progress till the nature of the facts of consciousness to be explained is accurately recognized.

§ 540. You would perhaps reply to the argument in §§ 528–9, that the *visible extension* of the wall was what was here concerned, and the visible extension was not in space but in our

sensation. I have delayed therefore coming to this point till I should have discussed this view of '*visible extension*'. But the purpose of the argument in §§ 528–9 is to show ⟨that⟩ the issue is somewhat confused by appealing to the visual extension before the closed eye, inasmuch as, if you had taken the example of the wall, every one would say that in what could be seen of it there was measureableness in feet and inches and that it was in real space. But we also know that under normal conditions the actual limit of the visible part of the wall surface, when from the nearness of the eye the whole is not visible, is a circle, though we may not be able to determine its radius, because of the insensible gradation of the vividness of the perception of colour down to zero. Just so in a surface whose limits we do see there may be a colour insensibly getting fainter down to the white of the rest of the surface. There is in reality a boundary of zero points for the colour, within which, however near, there is colour increasing insensibly and continuously from zero. We cannot distinguish the boundary, but we do not think that the extended coloured surface we are looking at (contained in the surface whose definite limits we see) is, on this account, not a real extension but a mere appearance to us. The subjective element is merely our inability to say where in the general surface the colour ends.

Again you don't seem to me to have asked yourself what the exact presuppositions of your line of argument here are. You seem to argue that a real extended surface or a real extension must have limits, and ⟨that⟩ therefore a perceived extension which appears to have no limits is not real extension.

But how can it be known that real extended surface must have limits except from the notion of extension itself, for of course it is erroneous to abstract extension from reality and then say it has particular modifications when it is the extension of something real? But if it 's true of the notion of extension in itself, then it must be true of 'sensible extension', and, if not, the latter is no extension at all. And as already stated the 'sensible extension', which you think has no shape or limits, has shape and limits, though *we* are unable to give an accurate estimate of them. Compare what I have said about the visual estimate of size.

PS.—I don't want to go into your criticism of Kant, which I cannot think very happy. People like ourselves who try to think out everything for ourselves, not fearing any authority, are not likely to acquiesce either in Kant's account of reality, nor in his theory of knowledge; though we learn so much by the way from him. But this criticism of yours depends on your making his 'sensibility' equivalent to your 'sensible extension', which I do not think will do. And there are other things to be said.

XIII

LETTERS TO MR. H. A. PRICHARD, FELLOW OF TRINITY COLLEGE, OXFORD

53 Cambridge Terrace,
Hyde Park, W.
6 Jan. 1904.

§ 541.

MY DEAR PRICHARD,

Your letter is very welcome, for though I am not at all in a fit condition to answer it, it accords with a strong feeling I have had for some little time that I ought to get into shape something on the pure 'theory of knowledge' in distinction from my Logic lecture. There are places in the lecture itself which are expressed in a particular way, rather than in another, because of certain views I have on the nature of knowledge and reality which are not developed in the lecture itself. And I have of late been feeling that I had got my own views and doubts and difficulties into a much clearer shape. I have prepared the way by a determined attempt to analyse hypothetical thinking and especially the exact meaning of the thought we express by the attachment of an apodosis to an hypothetical protasis In the lectures I gave last summer I arrived at a definite opinion, at least, on the problems in it which most perplexed me. . . . It is not a complete solution (or attempt at one) of the proper presentment of the whole theory of knowledge, but is an indispensable preliminary, in my opinion: and I do not think that any one can be supposed to have anything really clear to say about it, or even to know his own mind, unless he has got himself clear on the precise character of hypothetical thinking, where our activity seems most to distinguish itself from reality and where, even, we assert a necessary nexus, while in a sense there may be no reality to correspond. . . . We are both therefore rather wrecks. But tho' so unfitted, I will say something on one question, and that may be some help for the moment,

though I hope when more free to address myself to it in the precise form in which you put it.

I think it is probably an instance of what I suppose to be a certain illusion we are liable ⟨to⟩ in all investigation of knowing and willing. Whereas we have to do with the relation of subject and object, we try to express and explain various aspects of this relation in our ordinary categories which are all of the relation of object and object, easily seen in crude materialism where thoughts are treated like solid objects inside the mind like another such object—a box, but not so easily detected in more modern forms. The only remedy is to look into the nature of the thing before us *where we are certain of it*, and see if it really admits of such categories If we do that, we shall find these functions or activities of the thinking subject often cannot admit of such categories. If we think of knowing as an activity, as doing something, then, as if we had to do with relations of objects, we require a something to which something is done and a something in it which is done something ⟨to⟩—in fact, as one object in causal activity produces a change in another object, we think that the knowing subject must, in knowing, do something to the object it knows and ⟨that⟩ that object must suffer something. Or if we don't envisage this to ourselves as clearly as you seem to do—for to put it so clearly is, I suppose, half the answer, we tend to think on this principle. Now we must know something about knowledge, and we know when we reflect that the very idea of it is incompatible with any such *action* upon, or *suffering* in, the object known. You can no more act upon the object by knowing it than you can 'please the Dean and Chapter by stroking the dome of St Paul's'. The man who first discovered that equable curvature meant equidistance from a point didn't suppose he had 'produced' the truth—that absolutely contradicts the idea of truth—nor that he had changed the nature of the circle or curvature, or of the straight line, or of anything spatial. Nor does any one else suppose so. Obviously if we 'do anything to' anything in knowing, it is not done to the object known, to what we know, for that simply contradicts the presuppositions of the act of knowledge itself. If we persist in trying to find something done to the object, we are simply using a category applicable to the relation of object to object

and not applicable to the relation of subject to object, and must fall into all manner of fallacies.

Now representation is only another form of the same fallacy. We want to explain knowing an object and we explain it solely in terms of the object known, and that by giving the mind not the object but some idea of it which is said to be like it—an image (however the fact may be disguised). The chief fallacy of this is not so much the impossibility of knowing such image is like the object, or that there is any object at all, but that it assumes the very thing it is intended to explain. The image itself has still to be *apprehended* and the difficulty is only repeated. We still distinguish the image and the knowing, or perceiving, or apprehending it. The theory which is to explain subjective apprehension of the object cannot, as one could predict, do anything but presuppose the absolute ultimate fact of apprehension of an object, and so explain apprehension of the object (unconsciously) as apprehending another object like it. Obviously neither can apprehension be explained in terms of the object apprehended, nor the object in terms of apprehension. In a way the distinction is not only ultimate but of extreme simplicity—nothing can make it clearer than itself. It is 'simple' because we absolutely must always presuppose it to know anything, or doubt anything, or think about our knowing anything. Perhaps most fallacies in the theory of knowledge are reduced to the primary one of trying to *explain* the nature of knowing or apprehending. We cannot *construct knowing*—the act of apprehending—out of any elements. I remember quite early in my philosophic reflection having an instinctive aversion to the very expression '*theory* of knowledge'. I felt the words themselves suggested a fallacy—an utterly fallacious inquiry, though I was not anxious to proclaim ⟨it⟩. I felt that if we don't ⟨know⟩ what knowledge is, we know nothing; and there could be no help for us. I feel sure many most respectable theories commit the fallacy of supposing the presupposition of all explanation can be explained. What on earth is gained by 'construction' or 'reconstruction' over 'representation'? When you have made your construction you still have to *apprehend* it! It 's no good—knowledge and apprehension can only be described in terms which already mean knowledge and appre-

hension. Is it not true that just as those who consciously or disguisedly hold a representative theory are leaving out apprehension altogether and substituting another object for it, so the χαριέντες, the idealist constructors or reconstructors, are either leaving out the object and substituting for it the activity of perceiving it—this I think is their general tendency, or merely like the others constructing something which is an object but still requires apprehension: object on the one hand without apprehension, apprehension on the other hand without object?

§ 542. As you know it already, I won't repeat what I have said about the fallacy of explaining *judgement* as any kind of 'putting together'. As to your present special difficulty, it apparently does not take you in, but you see it has to be dealt with. 'Doing to' is a very general phrase. If we ask what it specially means in the case of knowing or apprehending the object, i. e. what it is we are doing to it, the only sane answer must be we are knowing it, apprehending it. Knowing can't be explained by doing to, doing to has to be explained by knowing, and the knowing as I have said is itself utterly incompatible with any 'doing to' in the sense of effecting any change in the thing known. You want to know what *is* changed; the answer is *ourselves*, our subjective attitude. It is quite right to say our conception of the object is changed in the advance of knowledge, and I should always stick to that phrase as the simplest. I admit it requires elucidation because of the mistakes people make about what 'our conception of an object' means. It is, however, the right and natural expression and in accordance with the use of ordinary speech; it requires vindication, not the substitution of something else for it. I will take your own case, probably chosen advisedly, where the progress of knowledge about a geometrical figure is due to my own thinking. Without believing it right, you don't see how the following reasoning can be met: 'in knowing, it is the thing which must be directly before me, *as my content*, and, in coming to know, more and more of the being of the thing must come before me . . . but it is my thinking which brings this about. This must be taken to mean that I have somehow *produced* what is before me and *ex hyp.* what is before me is the (thing or) facts.'

The last sentence seems an almost artificial *non sequitur*. Why must it be taken to mean, &c.? If I wonder what is the use of a certain part of a machine about which I know of course something already and, by examining the other parts and thinking and inferring, finally arrive at a knowledge of what the part is for, what its function is, must that be taken to mean that I produced that function of the machine? And so if I wonder whether there is any rule about the magnitude of the angles of the triangle and then, by thinking about the triangle and its relation to a line parallel to one of its sides, discover the equality to two right angles, why have I any more produced the addition to the content than I have produced the function in the machine?

I wonder whether the words I have underlined ('as my content') have betrayed you. If you have unconsciously identified the object *as my content* with some subjective state of mine (which contradicts the very idea of it), then the fallacy may be caused ⟨in that way⟩: for my subject⟨ive⟩ state has changed; I apprehend more of the object than I did before; my conception of it has changed, and by the activity of my thinking. Identify then my subjective state with the object and we have your conclusion. Any idealism which led to such a conclusion would have to maintain I made the function of the part of the machine. When I apprehend the proposition about the triangle, I apprehend something universal, which can't therefore be any state of my subjective consciousness. Suppose you repeat to yourself the argument I have quoted from your letter, substituting in it the universal proposition about the triangle for the general 'fact' or 'thing', and see how it sounds to you. (Just let me say, while I think of it, you seem to have overlooked a serious defect in a distinction you quote—as between

(*a*) the thing about which we think,

(*b*) idea as = what is before the mind when we think (Locke),

(*c*) the idea as = the activity of thinking.

There seems to me here to be much confusion. Idea can never be the *activity of thinking*; a more confusing use of it I can hardly imagine. Idea is surely always 'idea *of*', always implies something apprehended, and not merely apprehension. And when people use it in distinction from reality, they always mean

some mental image together with the judgement that the reality is more or less like it.

And again, how distinguish (*a*) and (*b*) in the case of this universal proposition about the triangle? 'The thing about which we *think*' is the equality of the angles of every triangle to two right angles, and that is precisely 'what is before the mind when we think' The only distinction that can be made here is between the *part* of the thing which is before us when we think [as opposed to] ⟨and⟩ the parts of it which are not before us. E. g. the equality to two right angles involves many other properties which are not 'before us' when we have completed the proof of this property itself.)

You must infer, as you apparently do, that according to me judging is 'putting thoughts together'. I repudiate all manner of 'putting together', and I don't see how 'putting together' is ever an intelligible expression, at best it would be a clumsy *façon de parler* requiring a good deal of explanation. Suppose M in the syllogism of mine to which you refer is triangularity. I may apprehend 'triangularity', i. e. understand a closed figure can be triangular. I may apprehend that three angles in a more or less complicated figure before ⟨me⟩ are the three angles of one and the same triangle. I may not yet apprehend the property of triangularity as involving equality of three interior angles to two right angles, and so haven't yet got the premisses necessary to complete the syllogism and prove that the three given angles = two right angles. When I do, M has suffered a change, that is, my conception of triangularity has suffered a change: which means simply this, that, whereas I knew something about triangularity, I now know something more about it, which implies no change in the thing but a change in me. I not only know M is *a*, as I did before, but that M is also *b*. This doesn't mean putting thoughts together, but that I have passed from thinking M is *a* (and also contains an unknown margin *x*) to thinking or apprehending M as *a* and as *b*. All language about putting thoughts together or things together is simply misleading I haven't here put any thoughts together or any things together; I pass from the apprehension of M as *a*, and the apprehension of M as both *a* and *b*, to the apprehension that *a* and *b* are together in M. This is the simple account and

nothing can make it clearer than it is. I think S is M, apprehending only the *a* in M; afterwards I apprehend *b* in this M, which I recognize as the same M as I attributed *a* to, and so my thought that S is M*a* is *replaced* by the thought that S is M*ab*, and, if *b* = P, I have the conclusion of the syllogism. I have the same M before me each time, the second time I apprehend more in it than I did before. In some ways it would be more accurate to say not that my conception of M has changed but that *I* have changed from one conception of M to a different one. That is, I have changed from one judgement about M to another judgement about M. If you like to use the language about my content, you may say that more of M has become my content, that is simply, more of it is revealed to me or apprehended by me; but M is in no way itself changed by this, because, as I have contended, my apprehension produces no change in the object, in the nature of the object as it is known: such categories do not apply at all.

§ 543. Now let us consider what we mean by our 'idea of a thing', or 'conception of a thing'. Suppose an object of perception—Cologne cathedral. If asked what was my idea of it, I should at once state certain judgements of mine that the church (a reality) had certain real attributes when I saw it. Such judgements are accompanied by a mental image, but that is not my conception or idea *of* the church, nor do I say it is. On the contrary I think the image is more or less inadequate, and I say the church was something like the image I am forming. The judgement is obviously not the image and can be reduced to no kind of terms of images. The image is as distinct from the judgement as the object (the church) of which I say I have an idea. In the judgement about an object not present to perception, what is the fact before me? It is not the church but the fact that the church looked so and so when I saw ⟨it⟩; it is a fact *now* that this was so in the past. It is the fact which is before me, the fact, i. e. which I apprehend in the judgement now. There is nothing between me and it, no intervening image. I may forget details but I remember a general character, such as two spires, very much crocketed. And this which I do not forget, viz. that I apprehended then the general character, is what I now apprehend. Besides this there is an uncertainty about

details not certainly remembered; this is not an image, it is a state of my thinking consciousness which can be expressed in no terms except its unique self, uncertainty. If now we take a universal reality like triangularity, my conception of this, my idea of this, when analysed, is not my image, but a direct apprehension of a part of the reality, viz. the enclosure of space and the *three* angles as necessitated by the three sides. I may, indeed must, have either 'perceived triangles' before me, to think it at all, or mental images, but they are not what I think; I think the reality which I directly apprehend. It seems to me that even the most advanced people one reads are under the dominion of the view that judgement is somehow idea as = these representative *simulacra*.

§ 544. The judgement of knowledge is apprehension of reality or fact, it is not the fact, it is not the reality; but neither is it any image of the reality nor the apprehension of any such image. Nor is it mere apprehension as a subjective state—mere apprehension is impossible—it is (1) apprehension (2) of the reality.

The ideal element we are looking for, and always (all of us) tend to misrepresent as an image of the reality, is the apprehending side as our act: the fact that we apprehend the reality. It is not here the least necessary to say *how* we come to be able to apprehend reality; the whole point is that the very conception of knowledge as such must necessitate this and nothing short of that will do. . . .

You have really done me a great service. It is one of the most useful things possible to me that I have the good fortune to know a number of earnest thinkers . . . who will put difficult questions to me and not let me off. The cleverest man in the world gets into his own grooves of thinking, and that can only be cured by the independent activity of other people's minds, and I consider myself specially fortunate in having so much access to it. . . .

I went to the Watts Exhibition on Saturday (this letter has flowed over to the Sunday). It is splendid and probably a unique opportunity.

New College, Oxford.
23 April 1906.

§ 545. *Confidential.*

MY DEAR PRICHARD,

Do you see any answer to the short criticism of Joachim's book overleaf?[1] If there is none, it is quite enough to nullify his whole book. If you think there *is*, do not trouble to write, for it might be a long business, but just say you think there *is* an answer and we can talk about it some time. It seems to me shortly that his book turns entirely on two fallacies: (i) the identification of what is apprehended with the apprehension of it, which produces the statement I criticize; (ii) the fallacy of making truth some sort of *correspondence between*. He can't shake himself free of this. It reappears in the form of 'coherence between' and leads to the fatal nonsense I refer to ⟨below⟩.

§ 10, p. 28, 'I answer "Euclidean space" is itself a system of such judgements.' But a judgement is a judgement *about* something else. That something else is not the judgement itself No judgement is a judgement about itself. The judgements Joachim refers to are of the type 'the internal angles of a triangle are equal to two right angles' (p. 27, bottom).

(i) If the system of such judgements *is* space, then, by the above, since what they are *about* is not themselves, what they are about is not space: *quod est absurdum.*

(ii) To ⟨meet a reply based upon⟩ the difference between a *single* judgement and the *system*:

(*a*) A system of judgements, as a system, is about something else not themselves. The system of judgements is not about itself nor about any one judgement in the system. Therefore the system of judgements which is *space* is about something other than space. Same absurdity as before.

(*b*) To ⟨avoid⟩ the absurdity in (i), reply that a single one of these judgements *is* about space. Then it is about the system of judgements which = space (which includes itself). But it is too obviously not about any such system and, if it were, it would be about itself.

[1] *The Nature of Truth*, by H. H. Joachim (Oxford), 1906. Cf § 240.

'The truth of each of them ⟨i. e. the judgements⟩', says Joachim, 'is "constituted" by its coherence with all the others.'[1] This I consider absolute nonsense into which he is driven by his theory or rather by the idea of *correspondence between two somethings* which he can't get rid of. I think I demonstrated in informal instruction this year that the attempt to represent 'truth' as any kind of 'correspondence or agreement between' is suicidal.

Yours,
J. C. W.

New College, Oxford.
Thursday (? 1906).

§ 546. As to truth of single judgements (I just see the title of your paper), I have been accustomed to contend, either as undictated comment to what I say of syllogism in lecture (or in informal), that the syllogistic form itself gives the directest contradiction to the view that something cannot be known as true in itself without its coherence with everything else. On the contrary, the syllogism implies that we can know a single truth and that the truth of it cannot *possibly be altered* by *any* new knowledge It means that when we say S is M (all A is M) this cannot possibly be altered by any new information about P. This is what makes the syllogism SM, MP, SP valid, otherwise when we got MP we should have to reconsider the validity of the major.[2] Of course S is M means just S is M and whatever, known or unknown, M again necessitates. Also I maintain (in informal instruction) that, if the truth of a single judgement depended on coherence with others, its truth could *never* be known, since (e. g geometry) the number of possible judgements in the subject is infinite. It 's only *because* we know *it is true* that we can know it coheres with ⟨the⟩ infinite system. In fact it is a theory of *pure Scepticism*,[3] not of knowledge. Again it 's absurdly contradictory to the facts of science. That three straight lines are enough to enclose a space and that they, as so enclosing, necessitate *three* angles is known by itself, and can't be modified by or depend upon any other proposition for its validity. Again, if you have a set A of judgements all 'cohering'

[1] Cf, however, ib § 37 [2] A slip for 'minor'. [3] Cf ib pp. 199–80

with one another and a set B of judgements all cohering, if A and B don't cohere how the devil are you going to say which set is true? Scepticism again and of a naïve character (other considerations in this line too). Again, ask any one who holds such a doctrine whether he thinks this is true:

If A is B
and if B is C
then A is C

and, if so, if he knows and how he knows it coheres with every other truth. Of course one sees that idealism of a certain kind is *driven* to this theory of truth, which is enough to refute it.

J. C. W.

By the way I have to go to my doctor (Osler) at 4 30—not well yet!

Bad Kreuth, Bavaria
13 July [1906].

§ 547. . .

As regards your interesting letter it is very welcome indeed because I have been reflecting that in preparing my lecture for publication I must probably expand or put in a different form the introductory portion. It contains in the para[s] on the def[n] of Logic the essentials from which started my whole criticism of the use of 'idea'. I have only been developing more clearly what I committed myself to in the para. about the 'content' of thought being the object's own nature and there being nothing else in the thought except the apprehension (or as I have more imperfectly said sometimes in the lecture 'presentation' of the object's nature—presentation won't do, it's too metaphorical and causes difficulty when we come to perception). I have been thinking e. g. that this important position ought perhaps not to come in as incidental to the discussion of the definition of Logic; but perhaps I ought to start with it. It comes in again in a clear manner in judgement when I maintain that, when I am thinking of Jones (or Williams, was it?) in the next room, *the* object of my thought is just Jones and not a picture of him, &c., &c. . . . You have only expressed in your own language (and I think *not* in the right kind of language) exactly what

I meant. . . . I think I can convince you of this in the present case.

(1) I did not mean to say . . . that 'idealism (of any kind) upset the view that logic studied thought and science studied things'. What I said, and I feel sure I said it accurately, was that we couldn't retain the statement *simpliciter* now that forms of idealism had developed themselves which in their respective ways *equate* thought and thing—whose very business it is just to deny somehow very emphatically the opposition of thought and thing. I pointed out that in consequence of the apparent emph[c] denial of the very distinction upon which the above differentiation of Logic from Science depends we were obliged to take notice of these idealisms and we might naturally too suppose we had to decide for or against them But then that it was obvious on examination that idealism in whatever form involved the direct presence of the object to consciousness and so merely the distinction of the present[a] or apprehension of the object from the object.

§ 548. Similarly that the materialism opposed to it made ultimately (in its theory of perception) the object with which we are concerned something directly present to consciousness or thought. Thus we after all hadn't to decide for the purpose before us between these opposing theories, for they agreed in this important point that *the* object of thought was not something 'outside' thought but 'inside' it. Now this of course implies that subjective idealism is for this purpose in exactly the same position as the materialism which distinguishes between *thought* and *thing* (I am sure I made this clear): for both of them there is something distinguished from the *thinker* as *object* and in both of them this is supposed to be *directly* apprehended. This I said was enough for Logic, i. e. the distinction of the apprehension (*subjective* side) from what is apprehended (*objective* side). This of course exactly means that idealism does *not* destroy the distinction between the *subjective* and *objective* side, when we come to examine it, but leaves it as the distinction upon which the difference between logic and science is grounded just as much as the ordinary materialism leaves it.

§ 549. Now this seems to coincide with what is the real truth in your supposed objection, but I think you do not express

it in a form which [it] is at all advisable, and I think I can make you see why I avoided that expression.

If *you like to call the object Thing and the apprehension of it Thought*, then the above doctrine may of course be put thus: in subjective idealism there is a distinction between the object thought and the apprehending of it. Therefore, if we call the object 'thing' and the apprehension of it 'thought', then subj. id^{m} distinguishes *thought* and *thing*; and as I go on to make *logic of the apprehension and science of the object* (shortly), so I make logic of the *thought* and science of the *thing*, even if I adopt subjective idealism—which may be put in your language that 'subj. id^{m} does not upset the statement that science studies things and logic studies thought'. But in choosing this sort of language you seem to me to be obscuring again what I had taken some trouble to get clear. It does *not* do to retain *simpliciter* the statement that Logic studies *thought* and that Science studies *things*, for the very vital reason that the *formula* usually conveys the implication that things are something outside thought altogether. It most certainly did, and does, in the common sense realism mean that. The *thought* not being at all supposed to be the apprehension of the *thing*, but something self-contained, something entirely mental and only apprehension of the thing as being an apprehension of some sort of a replica of the thing (i. e. when you push them to say what *knowledge of* or *thought about* the *thing* is). In fact by using this sort of language, which I carefully avoided, you obscure the very point on which you agree with me, and think, as I do, of the last importance to keep clear. Remember the sort of *locus classicus* [1] for this wrong way of putting the object of logic, *De Interp.*, ch. i, where language is said to be a sign of *thought* and *thought* a sign of the thing. Clearly the writer makes the distinction in the interest of what we call logic (but I won't say any more about the passage). I am getting a bit tired and so will stop—but if I haven't made it clear even yet please write to say so.

[1] *Καὶ ὥσπερ οὐδὲ γράμματα πᾶσι τὰ αὐτά, οὐδὲ φωναὶ ταὶ αὐταί· ὧν μέντοι ταῦτα σημεῖα πρώτων, ταὐτὰ πᾶσι παθήματα τῆς ψυχῆς, καὶ ὧν ταῦτα ὁμοιώματα, πράγματα ἤδη ταὐτά* (*de Int* 16^{a} 5–8) Whereas all men do not use the same letters nor utter the same vocal sounds, the affections of the mind, which these are signs of, are the same for all and, further, the things of which these affections are *simulacra* are the same.

I will only add now that I am sure there is no difference but a verbal one, and I think *this time* your verbal form not good but positively misleading. . . .

The very essence of what is contended in those passages is that *thought about* the thing is inseparable from the *thing*: that the thing is an 'element' *in* the complete act of thought, the significance of the latter coming out when we put it negatively—[1] viz. that if we take the thing's nature away, the thought is gone too, for without the thing it has no 'content'.

§ 550. (As to my lecture on complex and simple ideas) On consideration of things akin to this I thought it important for polemical purposes to express the same thing two ways, 1st in the ordinary (which is really conceptualist) language, and then in what would be the accurate language acc. to my own view of relⁿ of thought and thing, and I have done that in one important place[1] in my revised hyp[l] lectures as you will discover.

Oxford.
18 Dec. 1906.

§ 551. . . . What you have practically asked me to do is to bring what I say about the relation of Thought to its object in the Introduction to my Logic lectures . . . into clearer and more definite connexion with what I have communicated to you on my views about the mistakes in all forms of idealism. Or, in short, you ask me to adjust my Logic to my Metaphysics. . . . So in introducing my new and more detailed consideration of the subject to my present class . . . I said it was due to a colleague who had made me feel the necessity of putting this part of my logic into clearer and more satisfactory relation to my views on the theory of knowledge—metaphysics indeed.

For some years past I always felt a certain uneasiness about this part of my lecture, and have each time thought about it again. But I always found myself pulled up by a passage in the § entitled 'On the object as within consciousness'.[2] After contending that what is supposed to be the very 'content' of thought, i. e. what gives it actuality and life as opposed to a mere potentiality, is the nature of the Thing (directly and immediately—nothing between), that ⟨what⟩ we think is the

[1] i e §§ 289–90 [2] i e § 26

Thing's own nature, not some thought different from the Thing, I go on to the very objection which is the real ground of your present difficulty and the ground of whatever difficulty I have felt myself about the preceding part of the §—a difficulty therefore which I have had before me from the beginning. I find it e. g. in Brade's notes taken in 1896; but I find a better version of it much earlier in W. A. Craigie's notes, which are of hoar antiquity. 'Again we may try to separate the object from our thought, by saying that our thought is not the nature of the thing but the recognition of it (i. e. of that nature). But we shall find again that the content of the act of recognition, which makes it more than a mere form, lies in that very nature which we suppose thought to recognize. Or, otherwise, if we abstract from the act of recognition what is recognized, we shall find it empty' (Brade's version adds 'and meaningless'). Every time I came back to this I felt I was not going to give it up, that it was the essential—and that the main drift of these §§ was therefore right whatever it might lead to.

Now I always was careful to put this by itself, on its own merits, without assigning it to, e. g, any form of idealism (for from the first I would not commit myself even to the most attractive form of idealism, tho' greatly attracted by it). It is here that you misunderstood my attitude. . . . Certainly I was clear enough as to what I meant. I say in effect 'I am not going to decide about these realisms and idealisms, I don't need to, for such and such is my view about the relation of "what is thought" to thinking, and these theories themselves involve what in that view is essential to my purpose. . . .'

It is the inseparableness *in the act of apprehension* of the thinking from the object thought, which is not the least got rid of because the object can exist as unapprehended, which forces itself on one as the most important fact. . . .

However partly your criticism and partly my resolve to begin to prepare these things for publication made me decide to put the whole thing more plainly, and as I have of late years become much clearer as to my attitude to any form of idealism it was easier to do so. I devoted myself therefore to trying to find the exact character of the 'inseparableness' and what could be fixed as its necessary presupposition and consequences. And,

keeping the essential statement exactly as it was, I have recast and greatly added to my previous discussion. . . . And when I had finished . . . I found it advisable—again with a view to publication—to develop more fully a good deal more of the lectures beginning with the various senses of the word 'subject', and this kept me at work all the term. Among other things I added, after the negative criticism of Bradley, a positive attempt to represent the real genesis of his views, so as to show how he gets into the statements which appear so queer and eccentric. . . . I feel not only that what I have written in the new lectures is the best discussion I could give of the points you raise, but it anticipates some of your points in the very form in which you have put them, e. g. . . . I find that I have added a passage making still clearer than before how I use 'within'. But this was only natural because your difficulty . . . was that this representation of the inseparableness of Thing from Thought *in the act of apprehension* (with therefore *somehow* the necessary inclusion of the being of either within that of the other) might be supposed to really identify the Thing with state of consciousness, especially when one uses the word 'within'. I was therefore led as naturally as you were to make the 'within' clear, though I contend it was clear enough in my somewhat carefully phrased statements already. . . .

I may remark that I think I have made an improvement in terminology. I used to employ the words *recognize, recognized, recognition*, but for a year or two (perhaps more) I have steadily used the words *apprehend, apprehended, apprehension*, as being the simplest and truest expression of what is meant. It is partly the feeling of ⟨the⟩ necessity of some general word which introduced the barbarous *cognize* and *cognition*, which nothing would induce me to use. Such technical terms always tend to make a sort of meaning for themselves—as if they were some new metaphysical discovery—and so produce no end of confusion. . . . I have re-written my account of Judgement accordingly.

[? Oxford.]

§ 552. Monday, 9 Nov. 1908.

MY DEAR PRICHARD,

I am greatly pleased by your very liberal acknowledgements and am touched by the kind words which accompany them. . . .

It isn't quite enough to make the distinction between 'of' and 'between': because, if you add 'to' after 'of', you get the same result as if you wrote 'between'. Thus it must be 'relation of universal *and* particular' not 'relation of universal *to* particular'. You might say 'marked in language by the use of the prepositions "between" or "to"—the relation between A and B and the relation of A to B in the one case—and by the use of ⟨the word⟩ *of* only in the other, "the relation of universal and particular"'. . . . Observe also that one uses this OF idiom of things related which have a nature other than what is constituted by the relation, when we are only characterizing them as related—thus 'the relation of father and son'. . . .

I don't agree with this, for I think it misleading. You don't give it as merely Kant's ⟨view⟩ but approve it as *valid*. I have no doubt you would agree that our judgements in Geometry are *in a sense* (you say *no* sense) based on experience because we can't possibly have them without experience. We must experience particular space relations before we can get the universal geometrical judgement. The method of Geometry is nothing but apprehending the conjunction of universals in the particular, and, though we don't base them inductively *a posteriori* on the particulars, we must have the experience of the particular, &c., &c. If you meant that it was the best argument Kant had or that it was valid with a change of phraseology (i. e. omitting the statement ⟨that⟩ they were *in no sense* based on experience) you should say so. . . .

I am overjoyed at the completion of your difficult task, and much honoured by the handsome acknowledgement to myself. Both are a *great* comfort to me at a time when my own power of work is so unfortunately limited, and just when I need it most. With very kind regards and very hearty thanks,

Yrs. truly,

J. C. WILSON.

XIV

LATER CORRESPONDENCE WITH B BOSANQUET

The Heath Cottage,
Oxshott.
March 2, 1909.

§ 553.

MY DEAR WILSON,

The point on which I differ from Joseph's account of Induction is the relation of disproof of hypotheses to the establishment of an hypothesis. I understand him to hold that an hypothesis can only be established by the disproof of competing hypotheses. The view is formally very convenient, and not easy to refute in detail, which makes me all the more regret that a man of great ability should not have assisted in stating and illustrating the more difficult doctrine, which I discussed under the heading of 'Fusion of data and Hypotheses'.

His view (practically Jevons' also) seems to me a piece of what I call 'the negative approach' which is fashionable to-day —a sort of shirking the establishment of rational connexions, deeply akin to many philosophic phenomena of the day. It is, at root, I suggest, the same as saying that A is A is secondary to and rests on A is not not A. One recalls Bradley's account which takes the opposite view.

Of course, we have been warned that cause need not be 'similar' to effect. But none the less it seems clear that there must be a positive continuity of nature; and that, where this can really be shown, no operation with suggested other connexions is necessary, and where it is not shown, no such operation can supply its place.

Stout, I think, agrees with me, but only on my *ex parte* statement, about J.'s view. I have no real right to cite him in my support, therefore.

It seems to me that the disproof of alternative hypotheses may, strictly speaking, tell you nothing at all about the hypo-

thesis which is not disproved. And it is very difficult to see how the truth of any conclusion can be established without analysis of its positive continuity with its premisses. Somebody —it may have been Hoernlé but *les absents ont toujours tort*— gave me the impression that you were with Joseph on this point. I hope it is a wrong impression.

Yours truly,
B. BOSANQUET.

The Heath Cottage,
Oxshott.
March 21, 1909.

§ 554.

DEAR WILSON,

I will answer quite straight without trying to guard myself; it is best among experts, I think.

I do not know the mathematical proofs to which your first question refers But I think that in strict logical parlance the proof of a ὅτι without the διότι is a contradiction in terms. Proof I understand to mean the establishment of such a connexion between a proposition and the whole of experience that the two stand or fall together. Explanation I understand to be the establishment of a connexion between an alleged fact and the whole of experience. I have no doubt I could deal with any case the detail of which was within my comprehension. But it would be waste of time to deal with cases hypothetically. Besides, you can predict exactly what I should say. I attribute weight, of course, to what you say about a mathematical procedure, I attribute none to a mathematician's logical description of his procedure.

As to the second question, I used often to talk it over with old Faulkner at Univ. He said that those proofs could only be drawn directly from the consideration of the nature of whole and part, or, as I should say, from the nature of quantitative comparison in which a plus or minus can be established without being determinative. As to the case you give, I am wholly unfamiliar with the language and technique; but what it looks like to me is this. Within the formula the diminution of θ without limit is sufficient to explain the conclusion that the magnitude is L. If you were applying it to any fact, say, in

statistics or actual magnitude, I do not think the proof would hold without an explanation of what you mean by the diminution of θ without limit, apart from considerations showing L to be the value. In many cases of this type the mathematical proof so-called is simply invalid when applied. Piazzi Smyth applied a method of eliminating error ('least squares' I think) to different measurements of a rough box of stone found in Egypt, and brought out a result which he applied to determine the unit of measurement used in constructing the box. *The box had no dimension*; it was rough granite, and you could not say where the surface was intended to be. The whole calculation was a sheer figment.

This is a distant illustration, and perhaps irrelevant; yet I think it goes to ⟨prove⟩ this point; I will never accept any mathematical proof of any concrete fact, unless it is explained, or shown to be explicable, how the proof connects the characteristics of that fact with the whole of experience.

It would be waste of time to write more, till one knows what further point you take.

Yours v. truly,
B. BOSANQUET.

[7 April 1909.
[*To B. Bosanquet.*] Oxford.]

§ 555. . . . The case I referred to is this.

Suppose an attribute α is proved of a subject S whose *whole* definition is $A_1B_1C_1$, where A_1 is a differentiation of a universal A. Suppose A is *enough to necessitate* α, *being* what mathematicians call the 'sufficient and necessary condition' of α. A then is the true διότι. If we prove α of S by deriving it from A_1 we haven't then got the true διότι. As e. g. if we prove of an *isosceles* triangle that its interior angles = two right angles, arguing partly from the equality of the sides. This corresponds to Aristotle's dictum, of course, that the property must be proved of the πρῶτον καθόλου e. g. of triangle (A) and not isosceles triangle (A_1). (Correct as this is, he gives no criterion for knowing *when* the proof is from the πρ. καθ.; nor would his syllogistic theory of scientific proof allow him to.)

If then we prove α from A_1, we haven't got the true διότι,

if we prove of the isosceles triangle (using equality of sides as we may) that it has the given property, we don't know the real reason, which is not that it 's an isosceles triangle, but that it 's a triangle.

Still further are we from the διότι if our proof should be from the *whole* definition $A_1B_1C_1$.

Now it might seem that in the demonstrative sciences where α is really proved of S that generally (i) we should prove α from its true condition A. And as a matter of fact this is so nearly always in Euclid. We always, with some rather unimportant exceptions, do argue from the πρῶτον καθόλου. But (ii) supposing it happened in some case this was not so and we derived e.g. from A_1, we should expect that we should recognize A_1 as a differentiation of A, and recognizing A we might either (1) observe in our proof that nothing really depended upon its differentiation into A_1, and we had only *used* A; or (2) could observe that we had proved α in the special form α_1, and see that α depended on A, or (3), at least, recognizing A, we might test our proof by omitting its differentiation, and try whether we couldn't derive α from A.

But suppose, what at first might seem an impossible hypothesis, that we derived a property α of S from the whole definition of S, not from the element A alone in S on which it depended; not knowing that it depended upon A alone; not being able to see in our proof that the connexion was with A alone; and not even recognizing A as A at all—would it not be accurate to say, if such a thing were possible, that we had proved the fact, the ὅτι, that S has α, but had *not* shown its reason—τὸ διότι—we had not proved it from its true reason; for indeed we do not even know its true reason or condition—we neither know its condition *as* its condition, nor do we know of the existence of its condition, for its condition has not even appeared in our proof? Consequently though sure of the existence of the fact we can't be said to *understand* it.

§ 556. I imagine that when this statement is made the first answer in general would be that this would undoubtedly be the state of our knowledge: i. e.

(1) that we had proved the ὅτι not from the διότι;

(2) that we don't even know the διότι;

(3) that therefore while being sure of the fact we don't properly understand it: *if* the state of things described could occur.

But it would probably be said such a state of things cannot occur in a true demonstration issuing in certain knowledge of the fact.

It might seem the only thing at all corresponding would be empirical—experimental, inductive argument (i. e. eliminative) where we have not eliminated the true condition, but perhaps more than the true condition, e. g. have eliminated AB as true condition where B is superfluous; or we may even suppose a substance X to be the condition, or part of the condition, and yet not know an element A in its nature to exist, which element is the true condition (And here too the *knowledge* of the fact so far as we have it is not derived from the condition supposed) But now the thing *does* happen in *exact* demonstration, as we find when we advance beyond Euclid to Conic sections.[1] Among the properties common to all conics as conics, there are some, notably those connected with tangents, which it is usual in the elementary books to prove of each conic. Such proofs start from the *full* definition of each conic and use the full definition. The properties in question are not connected with an element in the definition common to all conics, but depend upon the *whole* definition: nor even does the element common to all conics appear either in the definition itself or in the proof.

For instance, the harmonic property of pole and polar is proved in the case of the circle by a proof which depends upon and uses the whole definition of the circle as the definition is given in Euclid (I can send you references to books if you like). Nevertheless the property proved is not peculiar to the circle and does not belong to it as circle but as 'conic section'.

But though circle *is* a differentiation of a universal 'conic section', this does not appear in the aforesaid definition of a circle at all.

We might, out of the definition, get the genus 'plane figure included by one line', but this will not do. It is too wide and is *not* the definition of conic section Thus, you observe, the

[1] Cf § 104.

universal on which the property depends cannot be abstracted from the definition used: and the definition has to be used as an inseparable unity. Moreover, this universal cannot be discovered in the proof, nor can a generic character of the proof be abstracted from the specific proof before us, which would fit any conic section.

The student first reading the proof sees in it no reason whatever for supposing the property attaches to anything but the circle. He is not the least conscious of any difference from the ordinary procedure of Euclid which (as I have said) correctly deduces a property from its true condition and so is *not* applicable beyond the subject of which it is proved.

This, by the way, raises the question how it is we know that the Euclid proofs are thus correct. To which I think the answer is that in general we cannot see it from the mere form of the proof any more than we can see that the aforesaid proof of polar properties is *not* 'correct' (in the given sense). We can only test it by seeing whether the *converse* is true, for if the property is derived from its true condition (=sufficient and necessary condition) the converse is always true. Now this can always be done in the ordinary case in Euclid and is done *explicitly*, e. g. propositions 5 and 6 of Book I, propositions 47 and 48 of Book I, unless the converse is felt too obvious to need proof. However, Euclid is a little deficient here because the converse of I. 32 ought to be proved. The fact that e g. the converse[1] has to be proved in Euclid, I. 48,[2] shows that the proof of I. 47 does *not in itself* reveal the fact that the property is proved from its sufficient and necessary condition. (The kind of unimportant exception to reciprocating proofs is Euclid, III. 2, which is true of the ellipse and parabola as well as the circle.)

§ 557. In the instance then which I have quoted the student in proving the property (1) does not prove it from its true διότι, (2) he does not know the true διότι, (3) he does not know even of the existence of the true διότι: he doesn't know the circle is a 'conic section', (4) even an examination of the proof will not at all reveal the generic element.

[1] Viz the converse *of* the enunciation of Euclid, I 47 (Pythagoras' theorem).
[2] MS I 47

On this account it seems right to say such a proof is a proof of the ὅτι—a real proof—so that we are certain of the ὅτι, but it is emphatically not a proof by the διότι; and it is right, I think, to say that the fact is not properly 'understood' and not properly 'explained'.

In order to bring out this better, I may draw attention to the fact that even when the properties in question were seen to be common to the various conics, it was some time before a definition of the element common to the conics was obtained from which the given tangential properties could be proved in one proof.

'Conic section' is no help, because the cone has itself to be defined as the surface formed as the locus of a line which passes through a fixed point, i. e the vertex of the cone and through every point on a *circle* (an ellipse also, &c., would do). We can, it is true, then, save [1] separate proofs for each conic section, by proving for the circle only, and extending by 'projection' (i. e. perspective) to the other conic sections. But we begin with the proof from the definition of the circle and so are no 'forrarder', for that proof uses the *whole* definition of the circle.

The generic element which has been discovered at last, which forms a general definition of which every conic is a species, is this: 'A conic section is the locus of a point which moves so that the anharmonic ratio of the pencil of lines drawn from it to four fixed points is constant' [figure drawn]. And from this definition the pole and polar properties (of tangents) common to all conics have been proved. You will observe that this late product of geometry is not to be found as an element in the definition of the circle, and certainly is not discoverable in the proof—such as I am referring to—of the tangential properties of the circle common to it and other 'conic sections'.

§ 558. I may add that the case is different in analytical geometry. The definition of the circle gives us its equation $x^2+y^2=r^2$ where r is the radius. If we form the equations of the tangents from a point, we easily establish the properties in question which belong to other conics also. But in this case we *can* discern the generic element, for we see that the variables have certain definite coefficients, viz. +1 for each of them. The

[1] Viz (apparently) escape the necessity of . .

equation then is seen at once to be a species of the genus $ax^2 + by^2 = c^2$, where a and b may have any value and any sign (c doesn't matter—I needn't explain why). This is the equation to a curve of the second degree.

This may suggest to us that it is as a curve of the second degree that the circle has the given properties. We then see that we can similarly generalize the equations to the tangents; and then we find we can demonstrate the properties universally.

It was then really comparatively easy to find the generic element in Analytical geometry, and it is true that the special proof suggests the generic one. But in pure geometry this was not so—'conic section' has a verbal generality parallel to 'curve of the second degree', but, as you have seen, isn't at all in the same position.

§ 559. Mathematicians are, of course, aware of the necessity of getting generic proofs of properties which belong to every species of a genus; but they are not, as far as I know, at all alive to the significance of the facts to which I have drawn attention, and the kind of curious paradox they involve which we may put as either (1) proving the fact not from its 'reason' and not knowing its 'reason', or (2) as somehow using the 'reason' in our proof without knowing we are doing so, or even knowing what the reason is.

Nor do I know that any logician has called attention to this, much less discussed it. (But I must grumble that logicians in general seem to me to have too little studied *actual processes* of demonstration in the sciences) I should say also that tho' mathematicians know in general they should get generic proofs, owing to the difficulty in non-analytic geometry of formulating the common element sometimes, they will present a proof which should be generic adulterated with specific proofs. I have before me (Drew's *Conic Sections*, Appendix, prop. VI) a proof of tangent properties which professes to be generic, and yet in one place uses a proposition which has only been got by proving it of each conic separately!

Of course, I am aware that there's a certain possibility of misunderstanding if one speaks of *one* reason; but if C is the sufficient and necessary condition of E, everything such that it and C mutually necessitate each other is in the same position

as C as διότι, and we don't know how many such equivalents there may be: the point remains that in the proofs I mean we *don't* prove by any form of true διότι.

Yours truly,
J. COOK WILSON.

The Heath Cottage,
Oxshott.
April 10, 1909.

§ 560.

MY DEAR WILSON,

I am sorry indeed to hear of all your worries. Anything affecting your wife's health must be so much worse than anything that could happen to yourself. I hope the whole thing will settle down before term begins: it is awful to have to work if you are not sure that the household is running smoothly.

It is extraordinarily kind of you to find time to write all this to me. I must put your letters together so that when your biographer's time comes, not in my day, they will be accessible to him. Your correspondence will be a large and valuable philosophical asset.

I had better recapitulate the points as I understand them from your letter. Of course I cannot here, as perhaps I could in some matters, hope to give an original analysis and solution of the difficulty. So I won't ask for books or attempt to criticize the proofs in question. I must take them in the light of what you say about them

I certainly seem to myself to find a contradiction in the conception of the proofs of which you speak. I think it can be accounted for only in one way, which is that suggested in your (2) of p. 12.[1] (I return your letter, as probably you have no copy; please let me have it back, whether you write further or not; it is my property!)

In the case you put I understand it to be admitted that: The whole definition of S is $A_1B_1C_1$, while the property to be proved a depends on A *only*

If, therefore, we prove a true of S from a_1,[2] or *a fortiori* from $A_1B_1C_1$, we haven't got the true διότι. (I remark on this that I hold it is a matter of degree, and that we may have *a* διότι.)

[1] § 559

[2] A slip of the pen for A_1.

I understand then that α is *not* dependent on the difference between A and A_1, still less on B_1 or C_1.

But in the case put, I understand that you do prove α of S from $A_1B_1C_1$, and moreover you do this for each conic separately and independently, i. e. you prove α of the circle from $A_1B_1C_1$, of the ellipse from $A_2B_2C_2$, or for all I know from A_2DE, and of other conics so on in order. And we are assured of two things:

(1) In accordance with the initial statement about dependence of α on A solely, that the property really depends on the nature of the conic section, but not of the circle, ellipse, &c., as such (N.B. words 'the universal on which the property depends', p. 6, upper half [1]).

(2) This universal nature of the conic section, on which the property depends, appears nowhere in the several definitions or in the proofs derived from them, note sentence, upper half of 5.[2] 'The properties in question are not connected with . . . but depend upon the *whole* definition.' This must refer to the actual procedure of the proof? If not, it surely is in flat contradiction with the assurance which I summarized in (1). I take it, then, to mean that in reaching the proof the student takes it to be so, viz. 'the properties in question to be not connected' (though, surely, it is not so). It seems to me that a proof of which (1) and (2) are true, cannot be accepted as a scientific demonstration of the ὅτι.

§ 561. (*a*) We have it *totidem verbis* that the conclusion is drawn from a whole from which are inseparable elements on which the property concluded to is not logically dependent, and that the element on which it is logically dependent does not appear in the premisses or in the proof. (Or would one say that the property α *is logically* dependent on the whole of the elements in the definition as a whole, though in some other way, e. g causally, not dependent on them but on A only? I don't think you mean this.) I understand, in a word, that α is taken to be proved from properties with which it has really no logical connexion.

(*b*) This is supported if we look at the arguments all together, when each of them appears analogous to the case of imperfect elimination which you suggested. You demonstrate α by, say,

[1] § 556, p 823, line 1 [2] § 556, para 2, 'Among the properties', &c.

four (is it?) independent arguments, presenting no feature of generic connexion, of four independent S's, using in each case the whole definition of the S in question. It follows at once that the nexus of α and S is not in any one of the cases reciprocal (cf. your test of a 'correct' proof), and therefore that there is irrelevance in the grounds of proof; and irrelevance in a ground of proof is a false affirmation of nexus, and this seems to me to be the case you put. It is a proof conducted according to the convention which fails to demand reciprocity. The true reason is no doubt operative somewhere within the argument; but there[1] is hidden in it an irrelevancy from which we have failed to disentangle it.

§ 562. No doubt it would commonly be held that (your p. 4) such an argument gives you the ὅτι without the διότι. But I do not think a logician should hold this.

As to the ὅτι.

If part of your ground of proof is, certainly, not logically connected with the point to be proved, and, as is the gist of the statement, you do not know which part; I do not see that you can be assured that the proof establishes α, even in the current isolated sense in which you would describe α for index purposes

And it seems to me irrefragable that the conclusion sets α in wholly a wrong light and leaves the student in a state of ignorance about it, which is not compatible with the establishment of 'it'.

As to the διότι.

But if you insist that this imperfect proof establishes its existence, then I think you must say that it establishes a relative διότι. Take your description of the student's state of knowledge after he has mastered the single proof of α about S from the whole definition of the circle and nothing more. It is obvious that the ὅτι is not established for him as being what it really is—only a part of the fact and not the whole is before him It is a property of a conic as such, and this principal detail of it he does not know. On the other hand, so far as it is established for him at all, it is established as somehow derivable from the nature of a circle, he does not, I gather, know exactly how,

[1] MS, it.

because the true nexus has never been before him at all. But in so far as he henceforth considers himself entitled to anticipate α as a property of S, so far he can only anticipate it in virtue of some connexion hidden within the nature of a circle; and this is so far a διότι.

§ 563. This is really adopting your suggestion (2) of p. 12;[1] and there is nothing in what I have said that does not come within suggestions that you have made, although in the hostility to non-reciprocal connexions I am recurring to the views of my own *Logic*.

I will speak more popularly in concluding. I feel strongly that your description of the student's state of mind makes it plain that the proof which you allege as dispensing with a διότι is a singularly bad type of proof. Our argument to begin with was about continuity, I really can't think that any one would agree that these cases make against the need of continuity for a true logical proof I think any one would say, 'Why, what the alleged proof without continuity fails to establish is the centre of the whole fact; without it—the nexus with the nature of the conic section—the fact is *Hamlet* without the Prince.' However, I must stop

Yours truly,
B. BOSANQUET.

[1] § 559

XV

DIVINE AND HUMAN CONSCIOUSNESS

[Oxford.]
1 Aug. 1904.

§ 564. 'There may be, &c.' [1] The proper reply is not that there *may* be such things in themselves but that the statement that there are is unintelligible. At all events, if 'out of reference to a thinking mind' means a thing not bound by the laws of our reason, as it often does, then if any one maintains the existence of what is unthinkable he is professing to think about the unthinkable, and so contradicts himself. His statement is merely verbal and no thought corresponds to it.

'The great presupposition' is neither a venture nor an act of faith. A principle or theory can only be a venture or an act of faith if the contradictory of it is thinkable. The great presupposition, however, is the necessary presupposition of any attempt whatever to think the world; and so it is merely artificial to represent it as in any sense a venture or act of faith as opposed to knowledge for it is the condition of knowledge and thought, and therefore indisputable Now the indisputable cannot be a venture or an act of faith; cf. Aristotle, *οὐκ ἔστι δ' ὑπόθεσις οὐδ' αἴτημα ὃ ἀνάγκη εἶναι δι' αὑτὸ καὶ δοκεῖν ἀνάγκη, κτλ.*[2]

The statement that the whole universe is a rationally coherent unity is *not* identical with the statement that it exists only as the object of consciousness. If the former does necessitate the latter, the connexion ought to be shown and, if it could be successfully done, it would be an irrefragable argument in favour of idealism.

Relations and things related are correlative and inseparable. The popular fallacy is to suppose them separable, so that the thing is something apart from all relations. The opposite fallacy

[1] Notes for a letter in criticism of a philosophical paper

[2] 'that which must be and be thought to be because of its own nature is neither hypothesis nor postulate' *An Po* $76^{b}23$

is to mistake the inseparableness for identity, an exaggeration not unusual in metaphysics. I am afraid Green falls into this fallacy. We may satisfy ourselves that any argument from the fact that relations belong to the reality of things to the conclusion that a thing was only relations would inevitably apply to each man's self, and this would reduce to a 'congeries of relations', and *that* no advocate of this idealism could admit.

There is in such a theory as Green's a far more serious difficulty in the relation of the individual's self and his consciousness to the eternal self and the eternal consciousness, which tends to be disguised by ambiguous phrases in which the transition from one to the other is concealed. This seems to me the weak point of the relation of morality to the individual self, and also of the account of the freedom of the will in Green's philosophy

. . . [he now turns to the paper under criticism.] To derive *human* consciousness by evolution from physical elements is ⟨said to be⟩ a ὕστερον πρότερον [a '*hysteron proteron*'] because the material elements presuppose consciousness as something for which they exist This is only '*hysteron proteron*' if the human consciousness to be derived from the material elements is that consciousness which the material elements presuppose and that for which they exist Whence one would suppose that it is the human consciousness for which matter exists. None but a subjective idealist believes this. Scientific knowledge presupposes that things, objects of my consciousness, existed before they became objects of my consciousness. Now the mischief is that, though this is not intended, the logic of the '*hysteron proteron*' argument demands it. Thus the consciousness which is the condition of the material world must be the human, and according therefore to one statement made must be eternal and infinite, and according to another must be universal. This certainly is the conclusion a man reading such an argument for the first time would draw. He would be surprised when he came to the protest: 'I never said my consciousness made the world.' He would feel rather taken in and might reply: 'No, indeed you didn't, but everything you *did* say naturally bore this interpretation and, if it didn't, you should have said so at once.' Clearly, if a distinction is to be made between *human* conscious-

ness and the *eternal universal* consciousness, it ought to be most plainly shown that the argument about *human* consciousness can be grounded on that other consciousness The fact that this is not done seems to me to show that . . the difficulty is really concealed by the identity of the word *consciousness* in both cases That is why the thing seems to me intrinsically unsound and to profit by an ambiguity. How could we refute a man who chose to answer in this way? 'I grant that the material elements exist only in and for an eternal consciousness. But your protest means they do not exist for and in human consciousness solely, and that they exist *before* a given human consciousness. Very well, the material evolution of human consciousness would now mean that this consciousness is developed out of elements which at all events existed before it and do not exist merely as objects for *it*, in *it*, and relative to *it*. Moreover, the brain and other organs, which I maintain are necessary to *human consciousness*, *never* are objects of the human consciousness which they condition. Thus my theory of the evolution of human consciousness is in no whit altered. Relative to *human* consciousness these material elements remain as material as anything need be. They are eternally existent matter and it makes no difference if they are said to have their eternal existence in an eternal consciousness. It is hardly more than a verbal change since the eternal consciousness is not human consciousness And, in fact, is not the eternal consciousness so completely other than a human consciousness that it makes precious little difference what we do call it? And at least there is no '*hysteron proteron*' These difficulties are rather concealed than answered by such representations as that 'consciousness awakens in me', where the consciousness is apparently the universal eternal consciousness which has been pronounced *like* my consciousness This sort of identification is of course important to help out the '*hysteron proteron*' argument. And yet for some purposes, as the protest ('I never said my consciousness') shows, it is absolutely necessary *not* to identify the two consciousnesses But consider the above phrase, 'consciousness awakens in me'. If we here use consciousness as a *universal*, in the ordinary sense of a universal, this ⟨is⟩ quite simple and intelligible, and only means that my consciousness is a particular

case of consciousness in general—such a general consciousness only exists in each particular consciousness.

But this is not the kind of universal consciousness in which material objects *exist*. For, if they are *in* consciousness at all, it clearly must be in an *existent* consciousness. If such a really existent consciousness is meant by the eternal universal consciousness, such could not 'awake in me' Actual consciousness belongs to an actual subject of consciousness, must be in *it*, be *its* consciousness, and cannot be the consciousness of any other subject whatever Therefore the existent eternal universal consciousness, as belonging to the eternal subject, cannot be in me or 'awake' in me or in any other person ; that would be possible only if ⟨I, as⟩ subject, were identical with the eternal subject. Indeed the consciousness of no subject can be identical with that of another. My learning and apprehending a theorem cannot be the same as your learning and apprehending it.

⟨You yourself say⟩ that the consciousness which does make the world is *like* my consciousness Thus it can't be mere consciousness in general, but particular and real as mine is, and have a definite subject for it, as mine has. One might indeed say shortly · a consciousness *like* mine cannot possibly be identical with mine, as it certainly must be if it awakes in *me*. By *me* is meant an actual individual subject with an actual individual consciousness. If this is thought of as a subject existent but not yet conscious, it is impossible to represent the consciousness of another already conscious subject as transferred to this subject

There tends to be an equivocation between the *subject* of consciousness and consciousness itself Consciousness is said to 'awake', which makes it *not* independent of time. In the same passage it is made independent of time as something present to both past and present. Now it is the *subject* which is present to the elements of the temporal series and so independent of time, but the subject's consciousness is an activity the phases of which take place in time. In relation to this independence of time there is a great difficulty for any theory. Each individual's life has a temporal beginning and we think of the subject himself as having a definite beginning in time. The idealism before us is specially bound to meet the difficulty

because of what it says of the eternal consciousness and the tendency in it to seem to equivocate between that and the human consciousness.

The organic theory of society does not depend on this idealism and can be stated without it. It depends solely on the metaphysic which recognizes that no individual has a reality complete, separate, and apart from that of others, and that the only complete reality is the complex of individuals, &c.

Morality ⟨is⟩ not properly affected by any theory of how consciousness originates, material⟨istic⟩ or otherwise. The intrinsic value of the moral consciousness is independent of any theory of how consciousness in general arises, as the value of a geometrical judgement is independent of any theory of the brain processes which make thought possible. On the other hand, the derivation of moral elements from *mental* elements which contain no morality is dangerous to moral ideas and affects them. This should therefore be refuted.

XVI

RATIONAL GROUNDS OF BELIEF IN GOD

§ 565. THERE are[1] subjects which do not appear at first in scientific form, art, morality, religion. In approaching such subjects for the first time it is natural to begin by trying to make proofs, or by criticizing given proofs, of moral, religious, or aesthetic principles, without any preliminary consideration of the meaning of proof or of the possible limitation of its province. But such a process may be endless. We may construct and reject proof after proof and yet make no real progress, because we may have presupposed, without distinctly recognizing it, that proof must have a certain form, a form which it happens cannot fit the matter to which we are applying it. The defect therefore will not lie in the particular proofs as such, but in the form of proof in general, and till we recognize this we shall labour in vain.

Or we may have something in our minds which would, whether right or not, prevent our accepting any proof of whatever form.

Or it may and does happen that being already convinced of the conclusion, moral or aesthetic, we admit a faulty proof of it: our interest in the conclusion blinding us to the flaw. This happens even in purely speculative proofs, e. g. even in mathematics.

Or a proof may seem correct and yet it does not quite satisfy us because it does not touch our feelings. Perhaps we are not aware, in our search for scientific completeness of demonstration, that this is the fact and, if we are at all ⟨aware⟩, may be afraid to own it, because it seems unscientific. But the demand of our nature remains, and we shall not be satisfied unless we raise the question of the relations of feelings or emotion to truth.

§ 566. The attempt to vindicate our religious beliefs and, it

[1] Rough notes for an address read at Canon W Sanday's Discussion Society in Christ Church in Michaelmas Term, 1897, subsequently slightly revised and expanded. The title announced was 'The Ontological Proof for God's Existence'.

may be, our moral and aesthetic decisions is often conceived as the attempt to find a 'rational basis' for them.

Now the inquiry after a rational basis for a belief has in its form certain important (if unconscious) presuppositions. We do not seek for something for a thing which it has got already, if we know it: and so also to undertake such a search we must be able to think of the thing, and must have thought of it, as existing without what we seek for it.

Thus it seems as if our question to have any meaning—unless it is to be entirely idle words—must imply that the belief exists somehow already, without the so-called 'rational' basis, and that the search for such a basis is an after-thought or later development. This is a serious implication It might be considered a concession which imperilled the worth of the belief, and so perhaps be pressed by one side and rejected by the other.

On the other hand, it might be thought ⟨to be⟩ in the interest of the dignity of the religious consciousness to maintain it: to insist indeed that the belief has a higher basis than reason, or what is usually meant by 'rational', though (it would be said) it cannot disagree with reason, which must confirm it.

We do not ask for a 'rational basis' for a proposition in the exact sciences—e g for the Pythagorean discovery of the relation between the sides of a right-angled triangle—because such a proposition has no meaning for us except as ⟨resting⟩ on a rational basis. Even when a theorem in the sciences is uncertain, e. g. a guess about some cause, it is not natural to put our inquiry into its validity in the form of asking for a 'rational basis'. On the contrary, so far as it exists at all, it has some sort of rational basis, it has been formed somehow on evidence, though the evidence is ⟨recognized to be⟩ insufficient and the proof but probable. In the case of facts, again, in nature which we know in some way, but of which we do not know the explanation, when we ask about them, it would be quite unnatural to put our question in the form of asking for a rational basis; we assume as self-evident and without reflection that they have such ⟨basis⟩ and are members of a rational system.

When we do come to speak in this way, it is because some explanation of the fact has been given which would take it out of a rational system

For instance, we do not ask for a 'rational' explanation of the cure of diseases by ordinary medicines or surgery, we do not seek to 'rationalize' these. But in the case of faith-healing (in modern times), if scientific people suggest as a true explanation of them that many diseases are really due to hysteria, and are in a certain sense apparent rather than real, depend therefore on certain states of the patient's mind, which again can be influenced and changed by the will: this may well be called rationalizing or finding a rational basis for the phenomena. And this would be done in conscious opposition to the 'miraculous' explanation.

It is clear then that ⟨when⟩ we come to think about the religious consciousness and its beliefs, we have some different attitude from that which we have towards nature and in the sciences, in respect of rationality. But it is not here alone, for in this particular the moral consciousness and, to some extent, the aesthetic or artistic consciousness are in the same position.

From one point of view we unhesitatingly speak of all these as rational and even *par excellence* rational We think that it is only a rational (thinking) consciousness which can be moral, and, more than that, we think that we show our rationality especially in morality. So also we think that it is only a rational consciousness which can be artistic. But in all of them we discern a certain distinction from reason or rationality and even a certain opposition to it. For instance, it is said that Greek philosophers, especially Socrates and Plato, endeavoured to put morality on a rational basis This implies that morality with its conceptions, judgements, rules, and practices, had been existing independently of such basis.

We must not make this exactly like the relation of our ordinary experience of Nature to the development of the same principle in scientific thinking. For instance, the principles of counting and thinking about distances in space, methodically carried out, become the mathematical sciences. But we don't look at them as the putting of our old experience on a rational basis. It is rather that we know more of the same kind of thing which we knew before. We do not regard the preceding experience as somehow unreasoning and requiring to be justified (if

it can stand) by a rational basis; on the contrary, we look upon it as giving the true basis to science itself.

§ 567. The difference and even opposition shows itself in the study of the subject when the rationalizing attempt has begun. Philosophers assume readily enough that morality is rational just as the ordinary consciousness does when it comes to reflect on the subject. Yet when they come to try to fix the nature of the reason or rationality, to find the place of reason and rational thinking in morality, the attempt has proved to be extremely difficult and there has been no sure unhindered progress from experience to science as ⟨there has been⟩ in geometry and the natural sciences.

Aristotle struggles vainly with the problem and ends in putting the moral life actually below the speculative on the ground of a certain irrational element in it; a difficulty not to be glossed over by calling this element imperfectly rational rather than irrational

The living actual moral consciousness, however, protests against such a theoretical position as absurd. We feel indeed that what is noble and dignified and most worth having in life is, on the contrary, above the mere scientific and speculative reason; and so (in modern times) when this consciousness has become more fully aware of its own value we find a different kind of reaction against, or resistance to, 'reason', yet still some reaction.

Thus, in modern times, in our English philosophy, after the reason in morality had been represented from the point of view of the knowledge in morality, with great emphasis, as if ⟨it were⟩ exactly like the scientific and speculative reason and, like that, producing a system of eternal truths, there is a reaction of which the characteristic is a certain repugnance to putting everything in morality under the reason. So much so that the very faculty which supplies moral judgements, i e. moral truth, is not called Reason but Moral sense, with a deliberate and expressed distinction from Reason,[1] and assimilated to a corresponding faculty in the aesthetic consciousness. And this faculty is considered more valuable to us than our reason, giving us directly

[1] Hutcheson, *An Inquiry*, Preface, pp. xiv–xv, 'The weakness of our Reason, &c'.

what Reason apparently, if perfect, could discover (but only as the result of a long train of argumentation), while such reason as *we* have, from its weakness, will not make the discovery.[1] Besides Reason,[2] as a principle of action, is apparently conceived as only calculating and contriving means to promote our private interest, while the Moral sense is directed to the good of others.

Whatever criticism might be passed on the passages which contain such statements, whatever difficulties or inconsistencies they may involve their author in, the important thing for the present purpose is that the faculty supposed to give us moral judgements is not only distinguished from reason but preferred to it. The same thing holds of the popular associations of the word conscience and finds expression in the philosopher of conscience, Butler.

Without professing to identify Butler's theory with the Moral sense ⟨theory⟩, it is enough to notice that, for our purpose and the point of view from which we are regarding moral systems, the conscience theory and the moral sense theory are in essential agreement; there is the same reluctance to make the faculty of moral principles simply reason; the same representation of the moral faculty, i. e. that which gives us moral judgements, as giving us *directly* what our reason, if it could give it at all, could only give as the result of a laborious process: with the further belief that *our* reason cannot give them and that by conscience we feel the force of certain obligations which our reason, conceived as trying to argue from ideas of the good of the world or the happiness of mankind, is not able to recognize. There is the same tendency to make Conscience the higher faculty, it is the 'voice of God in us'.[3]

Another school of thought affirms deliberately that reason can only calculate means for ends given to it, thus admitting that the most important thing in moral conduct, the end and motive, is not an affair of reason at all.

About the aesthetic consciousness one or two things seem

[1] Cf Butler especially, ⟨*Serm* v, xii, and xv⟩.

[2] Hutcheson, *An Inquiry*, ii, § 1, compared with ii, Introduction

[3] See Butler, *Sermon* v, p 49 'Reason alone, whatever any one may wish, is not in reality a sufficient motive of virtue in such a creature as man' [Oxford, 1896, vol. ii, p 98] Cf last note to *Sermon* xii. [Id., p. 226, note c]

clear. No artist, and probably no theorist, is likely to admit that mere thinking or reasoning without feeling or emotion could give an aesthetic judgement. There is no science, in the proper sense, of the fine arts A body of rules like the theory of Music is no exception, there *is* reasoning there, but such as would be admitted in a moral sense or conscience theory of Ethics, the application, that is, of given rules; but the rules themselves are the important thing.

It is worthy of note that a writer like Hutcheson while speaking sometimes at least in a manner which, if the words were pressed, might imply that in the case of the moral consciousness an absolutely perfect reason could give the moral rules, in the case of the aesthetic consciousness refers its judgement to Taste and Feeling or Sense, and never suggests that *here* a perfect reason might reach the same results; the idea never seems even to occur to him Further, in his later treatise, *Illustrations upon the Moral Sense*, he maintains outright that Reason does not give us moral judgements, that it cannot originate the moral ends, or any end,[1] in such a way that he seems to deny the possibility for any kind of reason, not merely for ours, ⟨this interpretation is⟩ confirmed by this that later, speaking of the Divine Consciousness, he says 'Why may not the Deity have something of a superior Kind, analogous to our *moral Sense* ⟨, essential to him⟩?'

§ 568. In the foregoing remarks two objects have been in view. The first is to remove an unfair prejudice which represents the religious consciousness as if in a unique position of antagonism to reason. The charge against it might be put in the most unfavourable way thus: 'Our reason has a right to call every belief in question, to ask for and criticize the evidence for them. Religious beliefs will not really stand this test To conceal the weakness Theology tries to avoid the attacks of reason by taking refuge behind mysteries. It assumes a mysterious source of religious truth, Revelation, and a mysterious faculty

[1] Sect 1, '"does not every *Spectator approve* the Pursuit of publick good more than private?" The Answer is obvious, that he does but not for any *Reason* or *Truth*, but from a *moral Sense* . ' 'Does a *Conformity to any Truth* make us *approve* an *ultimate End*, previously to any *moral Sense*?' (and Hutcheson maintains that it does not) [Hutcheson, *Illustrations upon the Moral Sense*. See Selby Bigge, *British Moralists*, vol 1, §§ 454, 459]

of religious truth, Faith; and, the more effectually to put reason out of court, it insists that these are above reason, superior to it and with an authority beyond the canons of reason.'

I have been trying to show that if we look at the thing fairly, we find a very parallel tendency in the moral and aesthetic consciousness. Shortly one may say that in the case of the moral consciousness also we find a faculty assumed to give truths, which is distinguished from reason and considered as even superior to reason. Nor can we say this is a merely popular and unscientific view, the deliverance of the untrained moral consciousness On the contrary, we find it in the very philosophic treatment of these subjects itself. undertaken by those who may fairly claim to try to take a thoroughly philosophic view. Nor can it be said that such doctrines belong only to unimportant people not really able thinkers. It is true that philosophy suffers from the fact that it is in a sense every man's subject, and one has to read some queer stuff under that title, but the thinkers in question belong to the strongest and ablest who have written on morals in this or any other country.

The students of philosophy therefore can hardly presume to take the superior attitude towards theology implied in the charge above formulated. We must rather first see that we set our own house in order.

If it be replied that, after all, these faculties of Conscience and Moral sense are aspects of Reason and misunderstood by their advocates and that the kind of reason to which they were opposed was really a kind irrelevant to moral principles, then *prima facie* the same kind of excuse may be made for the religious consciousness

The second object is to show that the opposition implied in the phrase 'a rational basis for belief in God' was no mere verbal accident, but corresponded to a real opposition, which has made itself felt in other departments of consciousness besides the religious, and that, too, an opposition so far from being destroyed or eliminated by a scientific investigation undertaken by reason in the interests of pure knowledge, that it actually sometimes appears as the result of such an investigation.

§ 569. To return now to the opposition itself.

It seems that where we can really ask for a rational basis for a thing and the language is really appropriate, we assume that the thing has already a basis which somehow is not rational, or is thought to be something not rational.

Then comes a twofold division.

We may suppose that in truth the thing has only a rational basis, that the other basis given to it is an illusion, and ⟨that⟩ in seeking for *the* rational basis we are trying to remove this illusion. The Greeks sought, for example, a rational basis for the phenomenon of thunder and lightning, which had been assigned to Jupiter, and supposed they found it in the quenching of fire, and this does away with the non-rational basis

If the thing which we treat in this way is a belief itself, we essentially alter the character of it, we are explaining the belief to be an illusion and assigning the basis of fact from which it sprang. Thus a rationalistic explanation of the belief in ghosts has been offered that men in a primitive state of half-developed consciousness sometimes confused appearances in dreams with real visions. It must be allowed, however, that in this case we use the phrase 'rational basis' for the phenomenon or fact which is misunderstood rather than for the belief.

The second case would be that in which the thing is supposed to have a basis already; but then it becomes a serious question whether it could have another basis, viz. the rational, at all; especially as we observe that where in moral theory a belief is supposed to have a basis which is not rational in some sense, this view is held on grounds which would make it impossible to have a rational one.

And anyhow, our first business must be to examine into the nature of the basis which the thing or belief is supposed to have already and before our investigation · for that it can have such a basis must be essential to it and, therefore, essential to our understanding of it In fact the possibility of a 'non-rational' basis will be exactly characteristic of such a belief as compared with those which have a rational basis only; and it is obviously to the distinguishing characteristic that scientific or philosophic investigation must be directed.

§ 570. What would be the state of mind of one who was really

unbiased and looked on it really as an open question, a thing to be proved or disproved ?

At all events he will not have any immediate knowledge or conviction, such as his belief that he is warm or cold or uncomfortable, or the question would not be an open one to him. He will regard the opinion so far as it exists as based on some sort of evidence, or on some desire, or ⟨on⟩ both together; perhaps in some such way as this:

In the earlier stage of the attempt to 'explain' natural phenomena, men naturally prefer what seems most intelligible to themselves and, in a first crude reflection, they think they understand best their own actions; or, rather, this is so natural to them that they don't ever reflect on it, and so their explanation of natural phenomena naturally takes the form of attributing them to a personal agent like themselves.

Inclination as well as reflection contributed to this result, because in any case men would prefer the superior force in nature to be like themselves and especially because, if it were so, they would have a better hope of a comfortable life, and that for two reasons · Such an agent as being a person might be sympathetic with them and inclined to help them, or, if not such but selfish or even malevolent (as they knew persons could be), yet at least accessible and amenable to influence from them if they could contrive to please him, and so better than mere unconscious nature.

Later, even when scientific knowledge has advanced very far and mere superstition ⟨become⟩ recognized as such, the same influences both in thought and in feeling will operate still, though in a higher form When the ordinary phenomena of nature have been reduced, with more or less success, to mechanical or non-personal causes, the laws of this mechanical nexus will appear as ultimate facts. We do not understand how these ultimate facts came to be there, and the mind tends always to be dissatisfied, whether rightly or not, with the recognition of a mere fact. It will tend therefore to be dissatisfied with mechanical causes.

There will also be a tendency to assign to the highest cause the highest quality; and the more our intelligence grows, the more the mind becomes aware that reason and consciousness

have a paramount worth as compared with the unconscious: and so we have a bias towards representing the highest cause as spiritual (especially as it is to be the cause of our own existence as well).

With increasing knowledge men begin to reflect on the adaptation of their own organism to obtain certain ends, which are desirable to them, and the more they know of organic life in general, the more marvellous appears to them the elaborate apparatus by which the ends of organic life are attained; and even to a very superficial knowledge this is seen to be far beyond what men could have thought out for themselves. Such adaptation of means to ends is most easily understood, by the analogy of human action, as the work of a self-conscious spirit conceiving ends and designing and planning accordingly, just as in the more primitive stage the so-called activities of nature are most easily understood on the analogy of human action.

But this time the argument has come to stay, for not only is this the easier explanation but it is difficult to believe in any other and the difficulty seems to increase and not to diminish with increasing knowledge We have the celebrated argument from Design which has influenced every period of thought.

Once conceive the highest Cause as spiritual and it is natural to consider him as the perfection or ideal of Spirit, and hence perfect knowledge and goodness are assigned to him. He must then be conceived as author of the moral as well as of the natural order and thus the belief in God has now enlisted on its side all the interest of the moral consciousness.

This might be taken as a sketch of the principal arguments and influences which could and have brought about such a belief in mankind in general apart from a special revelation, whether a fact or not. There are special philosophical and metaphysical arguments as well, but the important fact for us is that the belief is held by the masses of mankind, and it is to this that we have above all to direct our attention: and the mass of mankind is not metaphysical.

The unbiased judge, whom we are imagining, is bound to put the case strongly against the belief, to be very critical about the evidence, and we may suppose he would treat it in some such way as this.

Two different forces contribute to produce the belief. First, there is a rational element in the form of reasoning from evidence, the evidence being from acknowledged fact. Secondly, there is an element which some at least would, when viewed as the origin of a belief, call an irrational element; and, if not so to be called, it is not reasoning from evidence at any rate, it is our *desire*, and this appears as much in the later stage of human culture as in the earlier. We are dissatisfied with mechanism as an ultimate fact because we want the spiritual and conscious to be supreme. The interest of the moral consciousness biases us also, for we are too glad to give it the highest possible sanction. As to the 'rational part':

It has not the completeness of demonstration. There is some probability, perhaps much, in the argument that the existence of spirits depends on a spiritual cause. The argument from design is also questioned

(1) as not more than a high probability; (2) the existence of evil and imperfection in general is so much evidence the other way; (3) in modern times the theory of development and evolution explains the apparent adaptation of means to the ends of organic life, without any need of a designing conscious intelligence: at least some scientific men hold this view, and it is the popular conception of evolution.

⟨The impartial judge⟩ might ask therefore whether the great hold the belief has on men's minds is not really due to the strong interest they have in its being true; Wish and Desire, not Reasoning in the sense of reasoning from evidence. He might add that strong desire and hope that a thing should exist, however good it might be in its intention, was certainly no guarantee.

§ 571. One may venture to think there may be something beyond and more satisfactory than the defence of or attack upon these arguments, but I want to say a word, before going further, on the argument from Design.

I suspect we don't do it justice, that it has more hold on the mind of those who reject it than they are willing to confess or are perhaps aware of. Probably, when we are first introduced to it, it impresses us very powerfully. When we are more

educated and get some idea of science and exact knowledge, it doesn't satisfy the standard of such demonstration as we are familiar with. Besides it has been subjected to severe criticism from the scientific ⟨point of⟩ view, and so we tend to be afraid of it and regard it rather as something natural to immature thinking. Perhaps our difficulties are summed up in this, that the very effort to know Nature scientifically presupposes the necessity of everything in nature, and this seems, at first at least, the contradictory of design. But it makes all the difference whether we think in this general abstract way about the argument from design, or whether we are considering some of the actual facts on which the argument itself is founded. We may imagine we have got rid of it as a prejudice or illusion; but if, forgetting our general theories, we are absorbed in the study of the adaptation and harmony of the various parts of a plant or animal to the maintenance of its life and function, is there any one so adamantine that he can maintain an unemotional contemplative or 'scientific' attitude? Can he help feeling admiration and wonder? Whether it be an illusion or not, the idea of plan and design and choice of means comes on us with irresistible force; we cannot shut it out.

This impression is strengthened and not weakened by the new knowledge gained by modern biology, the gradual development of an organism in successive generations We see the gradual 'perfecting' of the organism for its functions; it isn't now as though we saw the marvellous adaptation in a given organism and inferred, or could not help thinking of, a designer, it is as if we saw the design or plan in process of being worked out.

But this is not merely the impression of the outsider. This idea of design is actually the clue by which scientific men are constantly working, not only in describing the parts of an animal, but especially the evolution of the organs; for the meaning of the elements in the lower stages is found by them in the higher. Sometimes this comes home to them and they admit its significance [1]

Secondly, ⟨as to⟩ the objection derived from evil and imperfection.

First notice that the impression above described is not one

[1] See Aubrey Moore, *Science and the Faith.*

which we have before the thought of evil and imperfection occurred to us; it comes over us strongly although we are aware of these things. They are not evidence against design in general. The want of adaptation and fitness in one department does not affect the evidence for it in another, so far as it is evidence, but leaves it untouched. The inference we should rather draw if we confined ourselves to the evidence is one to which our modern habit of thought disinclines us. Helmholtz[a] in his account of the eye takes note of certain optical defects in the crystalline portion, and adds significantly that if such an instrument had been sent to us by an optician we should return it with the request that such defects should be remedied One understands the innuendo. It is directed against the theory of design. But, if so, the inference was not the one which an accurate scientific man should draw. His own description of the structure of the eye gives an instance of that wonderful adaptation, in choice of material and form of organism, which eventually makes us think of a designing intelligence. If we are to argue strictly from such evidence alone with nothing else to go upon, we should infer the plan itself, as much as ever and as clearly and forcibly as ever, but we should infer something which hindered its execution, destroyed it altogether, or made it imperfect. But we have a scruple in drawing such a conclusion, because we think of the designer as also God, and mean an all-wise and all-powerful Spirit who could not make a mistake in plan or be hampered by the necessity in His material in carrying out His plan. But if it is mere design we are thinking of, the natural tendency would be to a dualism, and the tendency is realized in ancient authors like Plato and Aristotle.

⟨*Note on the argument from Design.*[b] In the foregoing I have not been concerned to offer an opinion on the value of the argument from Design because I wanted to keep clear what I have represented as the main issue. But I may here add a development of what has been suggested. If we put aside such scruples as I have referred to, the evidence is in favour

[a Helmholtz, *Popular Lectures on Scientific Subjects* (Eng translation), London (Longmans), 1873, Lecture VI, 1, 'The Eye as an Optical Instrument' Wilson's criticism is hasty, see especially p 228

b Added after receiving a copy from Professor C C J Webb of his book, *Problems, &c.*, in 1911. See § 582]

of supposing that there is an agency in Nature working with design, but neither omniscient nor omnipotent, and learning gradually how to fashion to its ends a material which it has not created.

But such agency would no more need to be identified with God than human agency needs to be, even from the theologian's point of view. The theologian has to admit the existence of human beings, imperfect in will and knowledge and power. He admits it is God's will that such beings should exist and that they should endeavour to work out their own progress both in their spiritual life and in fashioning the material world to their comfort and good. If so, there can be no difficulty in assuming a very similar agency in Nature which God also has willed to exist.)

As to the objection derived from Evolution.

I doubt whether Evolution as such has added to the objections against Design; on the contrary, as I have indicated, if the facts as imperfectly known suggested it, as more perfectly known, they suggest it more strongly. But it tends to suggest the picture of a designing mind working with methods curiously like (those of) the human mind It is as if nature made experiments and rejected the less satisfactory, retained the better and even advanced in knowledge and skill. (Many instances, e. g. Chrysalis. The outer casing of the chrysalis at one period shows the outlines of limbs and a definite organism, but the interior has nothing to correspond. The case is filled merely with a fluid substance apparently homogeneous The inference is that a stage between the caterpillar and butterfly has been omitted)

But the idea of necessity is inherent in Evolution as in all science; the material must be supposed to go through its changes according to fixed laws. What difference does this make? Assuming evidence of plan at all, the difference it would make would be to represent the plan and the gradual working out of it as inhering in the necessity of nature itself and constituting its essence. If we were told that it is a law of all conscious beings to desire to maintain their life, a law in their nature to fear death, we should not think of this as destroying our planning and designing activity in endeavouring to compass such ends.

§ 572. I have tried to imagine what might be an impartial and unbiased view. But there are many who are not in this condition; what is their attitude to the problem?

Some firmly believe in a divine Spirit and in such a way that the success and failure of what I may call a mere external argument would not really affect their belief, if they would only come to terms with themselves and recognize that this is so. We cannot in a scientific or philosophic interest simply put such out of consideration. They are not men whom we think otherwise irrational, they are to be found among the very wisest. With this kind of belief often goes a dignity and nobleness of character, and, in other spheres of thought, a finer perception of what we admit to be higher, and so there is at least a *prima facie* presumption that their judgement is not wrong in a sphere for which the highest qualities seem necessary. Now if these ⟨thinkers⟩ engage in a mere speculative argument, it is with little profit either to themselves or others, because they are not really talking of the conception of God as it is for themselves; of the living reality of it in their own minds In its true reality for them it cannot be seriously considered as a matter of argument. What they ought to do is to try to come to as clear a consciousness as possible of the nature of their conviction · that is the important thing and vital to science and philosophy; this is what they ought to do if they would help in a discussion like this. Perhaps many are deterred from this by a fear, which they have not fully confessed to themselves, that they may appear irrational. Well, they know well enough that they are not otherwise accounted irrational, perhaps indeed the very opposite. They know there is nothing petty or contemptible in the belief, but that it is confessedly associated at least with what is highest in human nature, and they should not fear to look into their own souls here.

Now some of those who seriously engage in argument are in this case; they are not aware of their true position and that it is their conviction which produces the argument and not the argument their conviction. ⟨One observes that sometimes practical theologians will gladly take up some supposed establishment of theism by a philosophical argument. But where do they suppose they themselves were, before they heard of the

theory, and what was the meaning of their lives? They possessed something far stronger to them than any such theory and, if they should find a flaw in the philosophical argument after all, they wouldn't admit they now had no ground for the belief.

It is this, whatever it may be, which precedes all such mere theory, which is the valuable thing for them and has been the moving principle of their lives. This is the thing for them and for us to try to understand Till this is done everything else is lost labour.〉[a] One sees this sometimes in the greatest philosophers. The arguments by which they seek to recommend some of their most important principles to others are clearly not the process by which they arrived at them themselves, for they are sometimes not only unsatisfactory but fallacious, and if they had not been so convinced of the truth of their conclusion they would have seen that themselves. 〈This is true where it is perhaps little suspected in theories which concern our conviction that we are perceiving 'real objects'. It is a fact demonstrable by the examination of theories of Perception, even the latest and supposedly most advanced, that there is no such thing as a theory of perception, in the sense of a theory able to account by argument for the conviction in question—an absolute conviction as is shown in the most convincing way by our actions. None of the theories extant from Aristotle till now succeed in the least in accounting for the conviction, and they do not merely fail in proving that we do perceive external objects, but they entirely fail to prove how we get this (firmest) conviction that we do perceive them, or even how we could get the idea of such objects at all. Some of them have actually resulted in explaining away the fact that they can't explain and reducing perception to an illusion. A certain kind of psychology of perception must indeed end in subjective idealism. The interest and significance of this for the subject before us will be evident in the sequel. We have in our ordinary perception a conviction antecedent to everything offered in the way of proof.〉[b]

Thus Plato and especially Aristotle try to present us with proofs that the rational life is the highest and best: mistaking the nature of proof and not perceiving that what they would prove is the absolute presupposition of any argumentation what-

[a Much later addition by author. b Much later addition by author.]

ever about the good. The argument as presented in the *Ethics* should take no modern student in. One's first thought when trying to prove anything about God or morality should be—did I really get this conviction myself in this way? Then and then alone do we find the meaning and value of our own thoughts.

§ 573. I have ventured to suggest above[1] that there might be something better than the attack or defence of the arguments I referred to. It might be inferred that I was prepared at once to suggest some other arguments, perhaps metaphysical ones. But I may repeat that I am concerned with the arguments and considerations that may be supposed to have affected the masses of mankind; for here must lie the secret, and it is here we must look for the inmost reality of the conception or belief. The true business of philosophy seems to be to bring the belief to a consciousness of itself.

It is no use producing philosophic arguments external to this; we should never satisfy ourselves, for we should shirk the thing absolutely necessary for understanding and it would return again upon us as *the* problem That would be to commit the error so often repeated in so-called 'Moral Philosophy'; to suppose that its function is to find a criterion of goodness, as a guide to moral action. If this is taken seriously, men couldn't have known what was good before the criterion had been discovered; they couldn't have lived moral lives. Besides, there is the further difficulty that the 'philosophers' have disagreed about the criterion.

When we do get a philosophic account, then, we must show that it harmonizes with the workings of the unphilosophic consciousness.

§ 574. About the popular arguments (which I don't believe to be a true expression of the popular consciousness),—we see something can be said for and against, which is enough to show they are not cogent. A simple proof in geometry is cogent and it can't be a matter of debate at all in the proper sense of the term.

Now I want to propose two questions connected with the preliminary considerations about proof with which I began.

Could any proof of such a form be perfect and cogent?

[1] § 571 *init.*

If we had a perfect speculative proof, should we really be satisfied (remembering what the object of the proof is), and would there be nothing wanting?

§ 575. First, on the form of proof. The form is 'empirical', starting from facts supposed to have the best evidence, viz. direct experience. The special form is 'analogical' It is manifest that in such ⟨a proof⟩ we can only have probability and not necessity To 'empirical' we oppose a proof in which we see the absolute necessity both of the conceptions and ⟨of⟩ the chain of argument into which they enter.

It may be asked 'why, in this account of the popular reasoning, ⟨should we⟩ assume it hasn't argued in the necessary and non-empirical form?'

Perhaps the truth is that such a question will not be seriously asked. If there had been reasoning of that kind we should hardly have had such stress laid on the 'design argument', considering the difficulties with which that argument is encumbered. We should certainly have heard much more of the other; and who has heard of it at all? For the moment I will only say that it has not been consciously present to the popular reasoning.

As to the other form of empirical proof, the matured form of it in empirical science; this moves only under certain presuppositions, and it is really the nature of the belief in God to try to get behind these It is no accident that this method has not been applied; it couldn't be. Could any proof satisfy us, as proof is usually understood—the type of geometrical proof and the proofs in natural science? Perhaps some one may think, IF they could get a proof of the empirical form found in natural science, they would be satisfied But would they? It is characteristic of the natural sciences, however careful the experiments and observations, that they never see into the nature of the necessity, and this is why they never attain mathematical clearness and certainty.[1]

But what of a proof in which we see the necessity? One must question whether a demonstration of such quasi-mathematical form could satisfy us, if we try to realize what it is we want to prove. In some subjects demonstration of a fact is more valuable knowledge than the direct experience of it. (E. g.

[1] See for this Part IV, ch. 2, on Induction and Lotze.

eclipse of moon at a certain date; the property of a geometrical figure.) In the case of the geometrical figure we know even more certainly than we could by experience. But in some subjects we have an impulse, which cannot be stilled, for what is called direct knowledge.

If we think of the existence of our friends; it is the 'direct knowledge' which we want: merely inferential knowledge seems a poor affair.[1] To most men it would be as surprising as unwelcome to hear it could not be directly known whether there were such existences as their friends, and that it was only a matter of (probable) empirical argument and inference from facts which are directly known And even if we convince ourselves on reflection that this is really the case, our actions prove that we have a confidence in the existence of our friends which can't be derived from an empirical argument (which can never be certain) for a man will risk his life for his friend. We don't want merely inferred friends. Could we possibly be satisfied with an inferred God?

It may be objected: You are only, if right, stating what we earnestly desire. It doesn't follow we shall get it. It doesn't even follow the desire is a rational one.

It may be answered. The demand for this kind of knowledge is not something of ours outside the conception of God, it lies *in the conception itself* [*] We can't think the content of the conception otherwise; if we do, then we haven't really the conception before us. To substantiate this ⟨reply⟩ we must think as fully as we can what the content is for the religious consciousness. We can only realize it in certain actual experiences of our own, which we may call religious experiences. Times of great emotion, when, as the saying is, we seem lifted beyond ourselves. Times, it may be, of great trouble; or of great joy. This can't be described in abstract language, any more than we can describe our feelings and attitude in hearing, for instance, a moving piece of music. We have to appeal to the experience of those who have had such feelings. So it is here. In order to understand what the conception of God is for us we must

[1] On this question, however, see Part III, §§ 263–8. [Cf Butler, *Sermon XIV*]

[* This is where the author touches upon the Ontological proof.]

recall such experiences and they must be experiences of our own, not any account of the experience of others.

It might here be objected: The sum is shortly this: (1) You have not rehabilitated after all the argument from design. (2) You admit that an empirical argument such as is used in the natural sciences is not applicable to such matter and, even if it were, it couldn't be complete in a direction in which we demand completeness. (3) As to a 'demonstrative argument', in which the necessity is understood, you rather imply it doesn't exist yet, and you don't say whether we could have it. But you say that probably even this, if we could have it, wouldn't satisfy us (4) You indicate that the conception itself demands some direct knowledge—'experience' or something like it.

Now (it may be said) (*a*) this direct knowledge of God, somehow directly in our consciousness, most certainly cannot be had. People know at least what is in their consciousness, and they are not conscious of this; and as you have said of the demonstrative argument, if it existed we should have heard about it, —if this direct consciousness were a fact, the same would be true.

Besides, (*b*) people wouldn't look for a demonstration if they had the direct knowledge, they don't look for a proof of what they are immediately conscious of.

So (it might be urged) the conclusion of the whole matter seems to be this: If the suggestions were right about the different forms of argument, &c., they point to one avenue of knowledge as alone satisfactory, and this avenue is closed. The belief in God, then, would be reduced to a mere hypothesis and an hypothesis which on the one hand your argument cuts off even from the possibility of verification, while, in fact, it has serious difficulties to contend with in our experience. It may so far be possibly a true hypothesis, though the demand for direct knowledge is not realizable

But can even this be allowed? If you put the demand in the conception of God itself, and don't treat it as a mere subjective attitude, is not the case still worse? If the conception demands an impossibility, is it even valid as a conception? If not, it is no use considering whether a real object corresponds to it.

The above objections depend upon the two propositions (*a*) and (*b*).

§ 576. Let us first notice some facts in the religious consciousness, whether connected with a definite form of religion or not.

We find in the religious consciousness as expressed in its highest and purest form—i. e. not mere rhetoric, for instance—that the direct knowledge of God (or ' ')[a] is regarded not only as a consummation to be desired but certainly to be expected. However, this is not looked on (in such a mood) as a present fact—that we now have this directly in consciousness, but rather as if necessarily in the future. Compare 'but *then* face to face'.[b]

Definitely as this seems to give up any direct knowledge at present, yet there is another tone quite as real. Men speak of God as being everywhere, all-pervading and so necessarily in themselves. They speak accordingly of being in the 'presence of God', and clearly not as a metaphor.

In the religious consciousness, when not so definitely associated with religious forms and expressions, we find the same antithesis. In one frame of mind even to those who have the belief, God seems a long way off—an infinite way, as they would say. He obviously can't be among the objects of the material world. We speak of him as 'behind' it—but as behind an impenetrable veil. In a way it would be a comfort if he only were a long way off in space, but he is 'behind' the infinity of space itself. Even if we say he is *in* the objects which are near enough to us, he seems concealed in them and, necessarily, even more concealed (it seems) than the forces of nature, which in a way are themselves not objects of a direct experience either.

On the other hand we find expressions which imply a consciousness of the nearness of God, of his most absolute closeness. Every one will recall the lines of Tennyson.[c] Nor can we object ⟨that⟩ it is poetry and *mere* poetry—for we feel these lines of his

[a This lacuna was illegible in the first draft and is left as a lacuna by the author. ? 'Ultimate reality' as in the Stoics

b 'For now we see through a glass, darkly, but then face to face. now I know in part, but then shall I know even as also I am known'—St Paul, 1 Cor xiii 12

c 'Speak to him thou, for he hears, and spirit with spirit can meet,
Closer is he than breathing, and nearer than hands and feet'
The Higher Pantheism.

The lines are quoted by T H. Green (*Works*, vol. iii, p 273) in a passage worth comparing]

express the strongest personal conviction—not a hope, not an inference.

These things are so far no proof—indeed they are antithetical —but they are most important and real elements in experience and may possibly help to a solution. It seems as if our solution ought to find a place for them. They at any rate make us hesitate about accepting the above propositions [1] as so cogent as they claim to be

§ 577 Let us now try to face these propositions

Is it true that we must be aware of what is in our consciousness and actively operating in it? No. Even in the acts of knowing and perceiving there may be something really existing and operating in our minds of which we may not be explicitly aware

(1) Take first a humble example—the syllogism—'Barbara'. This is a form under which we necessarily think, if we think at all, and all must have it; but it is long before we recognize it. Yet it is real in us because a law according to which our real thought operates But yet for a time we are unconscious of it. Mill's good village matron, with her daughter Lucy, even believes she hasn't got it and has unfortunately persuaded the philosopher that she hasn't [a] A little conversation with her, however, would easily bring out the implicit form of the syllogism

(2) More important for our purpose Take the familiar topic of Causality When we speak of what we know directly, of that of which we are conscious, we tend to think of objects of sensuous experience only, feelings of our own, or objects perceived—that is given (as the phrase is) as sensuous perceptions. If we think of these as all the content of consciousness, we shall get into immense difficulty, because there are elements necessary to our experience which can never be objects of perception in the above sense—the ordinary sense. Now we do all tend to make this limitation at first about the nature of that of which we are actually conscious; and it is a principle, too, in philosophy as well as in the untutored consciousness. Thus there was no place in Locke's philosophy for the conception of cause; causality never being an object of such experience as he is thinking of.

[1] p 854 (*a*) and (*b*).

[a] Mill, *System of Logic*, II. iii, § 3]

But he fails to notice that, because he appreciates the working of the conception and thinks really only of that. Hume's criticism detects this fault. Hume holds the same view of the object in experience and is driven by his logic, in effect, to a denial of the conception of causality altogether; though really he doesn't get this clear, because the conception *is* there and can't be denied away, and consequently he gets into great confusion, sometimes seeming to eliminate the idea altogether, and finally substituting another idea for it, something which it certainly is not.

The fact is, the conception is there and has conditioned the very experience in question. Here it is not merely that we have not become aware of a necessary element in our thinking, but we have actually denied that we have it at all.

(3) And then the most important illustration of all is the notion of our own Self. Following such presuppositions as Hume's, we can predict with certainty what must happen to it. That which itself experiences—experiences these feelings, these perceived objects—cannot possibly itself be one of such objects of experience It is in a far worse condition than the conception of Causality. And the Self in this philosophy had to go. The very presupposition of experience is condemned by the test of that experience to be non-existent, simply because we can't be aware of ourselves as objects of sensuous experience. Yet that we are conscious of ourselves—though, of course, not in the way of such experience—is the most absolutely certain thing of all. Every one will recall the words of Descartes.[a]

As to the second proposition,[1] if people have really laboured to disprove that which is most directly present to consciousness, we shall feel no surprise that they should have tried to prove what they have got already. The attempt to derive the law of uniformity of nature by an empirical argument from experience is a notable instance. We have the paradox that a philosophy, whose metaphysic has destroyed the notion of causality altogether, really tried to establish its validity (for science must have it) in the world of experience by an inductive proof. The proof is obviously an utter failure. The thing to

[1] p 854 (*b*)

[a] *Cogito ergo sum, Je pense, donc je suis*, I am conscious, therefore I am. *De la Méthode*, 4me Partie.

be proved is 'tumbling under our feet' [a] all the time, being the very condition of examining scientifically that experience from the examination of which we hope to derive it by scientific examination.

The fact, then, that people have tried to find a proof of God's existence is so far compatible with His direct presence in their consciousness; and the fact that they think, or some think, they certainly have no direct experience or knowledge of God is compatible with the same hypothesis. And now the facts quoted from the religious experience, oscillating between two extremes—the representation of God as immensely far and unspeakably near—get a new significance and we can hope to explain both sides of the antithesis.

* * * [b]

§ 578. Is there any evidence of such a direct experience which we thus ignore?

Let us try to consider again some peculiarities of the religious consciousness. As already said, we ought to try to keep it before us in the most definite and concrete form, recalling in ourselves definite experience and feelings. Though of course we can't attempt to describe it exhaustively, we *can* call attention to some striking features of it The conception has a certain setting in consciousness and peculiarities which present an analogy to the moral and aesthetic consciousness I do not now refer to doctrines of revelation and faith as modes of knowledge, because they come from reflection on the conception or belief, but to the active presence and working of the conception in what we may call, all of us, our 'religious experience'. We cannot maintain towards the idea the mere unemotional attitude of the scientific or speculative understanding. We do not think of God, to use popular language, *merely* as the cause of the material world and the power which is always sustaining it; or in more philosophic phrase as the *primum movens*, or, say, as the unity of the material world—i. e. the one reality of which it is all the varied expression, and the true substance, so to say, of which they are the attributes. Nor do we think of God as

[a Πάλαι φαίνεται πρὸ ποδῶν ἡμῖν ἐξ ἀρχῆς κυλινδεῖσθαι καὶ οὐχ ἑωρῶμεν ἄρ' αὐτό. Plato, *Rep* 432 D, cf Bacon, *N O* 1 110

[b Lacuna marked in original]

the basis of the spiritual world, *merely* in the same way as we think of the abstraction of our own selves, or our own self-identity, as the basis of our life manifested in time. But the conception is inseparably connected with certain feelings or emotions.

The one to which I want to draw principal attention is the emotion of reverence. This is not to be confounded with the fear of the superstitious. It is not confined to the lowest or least intellectual, but reaches its completest development in the highest and the most cultivated.

Yet, possibly some one might object, this is an utterly wrong method. We want the dry light of science and philosophy; we know that our judgement can be obscured and prevented by feeling, whatever notice the anthropologist must take of such things, it is the very business of the scientific or philosophic investigator to free himself resolutely from their influence. On the contrary, if we imitate scientific procedure, this is exactly what we must not do. With the precept of Bacon and the example of Darwin we must omit none of the facts which we know of, and this fact, that the conception of God can only be realized by us with certain emotions, is not only a very interesting fact but it is an essential characteristic of the conception, for it distinguishes it in a very striking way from any scientific conception. If it belongs to the nature of the conception as such that it should produce or be accompanied by such feelings, we must, if we are to understand it, attend to these feelings whether any illusion is connected with them or not

§ 579. Let us change the argument to another sphere.

Consider such emotions as are proper to the moral consciousness. Take (e. g.) the emotion of *gratitude*. We have feelings which, as we say, are due to physical causes; they do not originate from any previous thinking of ours, are not excited by conceptions. But there are others which are only possible to a thinking rational consciousness as such. In this sense they are rational, and it is a want of appreciation of the inseparableness here of reason and feeling which constitutes the difficulty in moral philosophy of which I spoke at the beginning.

Gratitude belongs to this class of feelings. And it presupposes a good deal of rational activity in consciousness. We are grateful to some one for some good (as we think) done to us.

Now what does this presuppose? (1) Not merely our enjoyment of a pleasure, but reflection upon it, distinct thought of it as some good to us. (2) It implies also that we refer it to another as an effect of his causality. Which means again that we have, in some form, conceptions of ourselves as distinct from other selves, also that we have the category of Causality and more especially of the activity of a person as a cause. (3) It implies also that the person, whom we have thought of as a person and a cause, willed and intended the action; otherwise we are not grateful. And further (4) that he is benevolent to us, wishes us well, has done it to please us, or certainly to give us good The appreciation of these relations between persons perhaps may seem to require considerable rationality, but whether it does or not it must be present or there could be no gratitude.

It may be asked whether this can be true of the consciousness of children. Obviously if they are susceptible of gratitude these elements must be present, though not abstracted from action and reflected on, or the child could not be grateful; and in actual life we think they are grateful and we certainly treat them as if they regarded the person to whom they are grateful in the way described.

Now here is a feeling whose whole meaning is relative to conceptions and acts of reason, impossible without them; and for it to be possible these relations must be somehow appreciated. But more than this, the feeling itself is inseparable from the act of judgement in which the recipient of the benefit appreciates his true relation to his benefactor. The judgement is not there unless the feeling is there too. We cannot separate the judgement from the act as something in itself speculative and in itself without the emotion. We cannot judge here except emotionally. This is true also of all moral and aesthetic judgement Reason in them can only manifest itself emotionally.

Now the feeling of gratitude would be admitted as a fact given in experience, and I want now to argue from the facts of experience In the first place the feeling is obviously only possible for us through the conceptions, if it exists, then, it guarantees the possibility of the conceptions for our thought at least. We must be able to think of ourselves as something more than and different from our feelings and as having the feelings; we must

be able to think of another self than our own; we must be able to think of such self as willing, and willing us good: and therefore to think of a certain relation between selves. So much for the possibility of the conception.

It may be asked, What of the reality of the object corresponding?

This means now for us: Are they objects of experience? Do we suppose we could communicate by *a priori* teaching to any one the meaning of the feeling of gratitude? This is no more possible than to explain the meaning of music to one who has not experienced it. He must have really experienced it. He must have an actual self and ⟨have⟩ distinguished himself from his feelings. He must have had an experience by which he represented a person in such a relation to himself. But now even suppose he was mistaken in attributing goodwill to another person; for that, it may be objected, is but an inference; or that he is mistaken even in supposing another person besides himself, for this again (it may be contended) is but inference; he hasn't direct experience of the person ⟨it might be said⟩, he really infers him from certain elements in his own experience.

Well, the idea ⟨of the⟩ goodwill of another person again, even if here mistakenly applied, can only come through some experience of this attitude in ourselves. Goodwill of a person, then, must here be a real experience.

But it may still be pressed ⟨that⟩ the other person is somebody not in our experience and never can be, and that experience has only guaranteed one person. Let us suppose that is so, though it seems to me a serious over-statement. Then at least the reality in experience of gratitude guarantees the reality in matter of experience of every element we have said to be presupposed except the last. But as to the last, I at least can't have goodwill towards a person whom I suppose revealed to me in an experience unless I certainly believe he is there.

§ 580. Let us return to the feeling of reverence with its solemnity and awe. What conception does this presuppose?

In itself it is not fear, love, admiration, respect, but something quite *sui generis*; it is a feeling directed to a spiritual being and presupposes the conception of a spirit.

Can it be directed to a human being? We speak of love and

respect and admiration of a human person, and it is true that we speak of a person sometimes of exceptional goodness as revered. But one must contend that this is not right and that we shall detect, on severer analysis, that it is a metaphor of exaggeration, and strictly only implies a high degree of respect and admiration and regard; but we cannot really say we have a feeling of reverence in the presence of such a person; and, if we add the feeling of solemnity which belongs to reverence, it seems still clearer that it is not so.

The feeling, then, seems directed to one spirit and one alone, and only possible for spirit conceived as God.[1]

Again, it is true that we speak of reverence for the moral law; but again I believe no such feeling possible for a mere formula and that, so far as it exists, it is only possible because we think of the moral law as a manifestation of the nature of the Eternal Spirit. Here again the feeling of solemnity seems to help. For those who have such a real experience of reverence and solemnity the existence of this emotion guarantees the possibility of the conceptions, and we retrace the same argument for the reality of the experience as in the case of gratitude. We must affirm, here as there, that we cannot have the conceptions without real experience to correspond. There we admitted, *verbi causa*, the possibility that the assumption of the existence of another person, though grounded on our own experience, might be only an imagination grounded on an experience of ourselves. But here the feeling *presupposes* the conception of another spirit, whom we must think as not human or like ourselves, or the conception would never give rise to the feeling. Can we resist the conclusion that we must have had experience of the reality of such a being somehow within us?

〈This[a] argument should have a special claim on a set of thinkers who, from their intellectual bias, may seem to be the most unlikely to admit that there is any sound evidence for a belief in God. These are the adepts in the methods of the empirical sciences and the adherents of empirical systems of philosophy. For it is just these who maintain and emphasize the doctrine

[1] See further the last paragraph of this section, p 864 (top of page)

[a] An interpolation subsequent to 1903 and probably much later]

that we can make no 'idea' ourselves, that the ideas we have are 'ideas' of objects actually experienced, or arbitrary combinations of them by us. In the latter kind, only the combination is supposed to be ours; the elements combined must come from experience.

Have we not here, in what we may call the religious experience, an emotion which absolutely guarantees the existence in us of the idea of a spiritual being transcending everything human, ⟨a being⟩ which is no combination, arbitrary or otherwise, of the 'idea' of other experienced objects? In the actual emotional experience itself there can be no thought of any arbitrary combination by us of ideas of what is already familiar in experience. Rather something we could not have imagined beforehand, something spiritual which we utterly fail to describe, seems to be present to us without our will or co-operation and, without our will, to fill us with a unique emotion. Is it not certain that the solemn reverent attitude is only possible because we are convinced of the presence of something entirely transcending everything human and all other experience? No one will think it worth while to contend that such feelings could be awakened in us by the contemplation of an hypothetical and problematic combination of our own. When ⟨we are⟩ not in the actual experience we may have recourse to combinations of ideas from ordinary experience and to analogies from such material. These attempts fail and their failure confirms what has been just maintained, for no combination of objects of ordinary experience, however magnified, can produce the conception of a thing which could affect us with such emotion.

It would seem, then, that the idea of God is one of those ideas which, in accordance with the empirical principle quoted, must be the ideas of something actually experienced. But the principle itself is not confined to empiricists Rather the essentials of it (for empiricists understand 'what is given in experience' in too narrow a sense) and all that is necessary for the present application seem necessarily admitted in all philosophy.⟩

But, at the very lowest, we are entitled to argue that just as the person who has a real experience of gratitude *must* believe that there is another actual person to whom he is so related that he is grateful to him, so the actual feeling of solemn

reverence is only possible because we actually *do* believe in God. So, in such feelings, in the actual experience of reverence and solemnity we are believing (not fancying, not imagining); we *are* believing in God, for it is impossible to have them at all actually except through that belief.

〈Those[a] who have had such an experience may, at times when it is absent, search for a proof of God's existence and may think they require one. But they are putting a question which could not possibly be a question to them *when* they have such an experience—an experience in which they have been absolutely convinced of the reality which they now seek to establish otherwise. The corrective is to recall such experiences if we can. But that is not easy. There is an analogy in our aesthetic consciousness; for often we can recall but imperfectly the effect of a piece of music though we are certain the effect was there. Besides, these 'religious' experiences are often, perhaps oftenest, such as have touched us so deeply that we instinctively shrink from trying to reproduce them They may have been at times of unspeakable sorrow, such as the loss of a friend. Is it not here that the great poet can help us? For he can awaken the mood in us again, and in his imaginary world bring home to us the living experience itself. Poetry does us here a far higher service than philosophy can; yet it needs philosophy to vindicate and complete it For to turn to poetry in such issues we tend to think of as abandoning the difficulty of reasoning to take refuge in the realm of fancy. This is a great mistake which it is the privilege of philosophy to correct

But to avoid misunderstanding, something must be added. It is not meant that in what we have called the religious experience a man must say to himself 'Lo, God is here!' or use any ordinary religious formula. It is enough that in such an emotional state he should be convinced of something infinitely beyond himself and all humanity, inspiring him with an emotion which nothing human could inspire—reverent awe. One recalls the fine saying of Tacitus about the religion of the Germans, 'secretum illud quod sola reverentia vident'.[b]〉

[a Another interpolation to end of § 580, of same date as the last
b 'That mystery which they behold with the eye of veneration alone.' —Tacitus, *Germania*, 9]

§ 581. The feeling, then, points to the reality of the experience. Such a view would throw light upon the antithesis we mentioned. We can see why we can fail to recognize what is in us, and how this which works within us is the original conviction which, under the influence of science, we misunderstand; and that it is really this misunderstanding which made us cast about for arguments such as that from design. But no argument from design, illustrated by examples however wonderful—and there are wonderful ones—can have anything like the effect of a single experience of the emotions of reverence and awe. The one is but an argument; the other is a sense of the reality itself and is the only thing that can satisfy us. This need not interfere in the least with any one's belief in the edifying effect of religious services. These are essentially for those who have already a belief in God. Even the religious exhortation intended for 'conversion' (or whatever it may be called) as a rule assumes that the hearer believes in God. If it is really a question of convincing the hearer of the reality of God, the best chance of the preacher lies in the possibility of awakening in the hearer what has been implicit in some of his own experience. And does not this depend on what is essentially a poetic faculty?

XVI (Appendix)

§ 582.

Nov. 3, 1911.

My dear Webb,

I was very surprised and pleased, and indeed touched by your kindness in honouring me with the present of your book.[1] What can I do in return? Send you a copy of 'Traverses'[2] or of 'Interpretation of Timaeus'[3]—both much at your service! My stock is (alas!) so small. I can't pretend to have read yet so far in the book as to discover your kind and conscientious reference to myself on p. 269. It was pointed out to me by Williams. I rather wish I had known you thought of bringing me in, for I should like to have talked to you about it. It doesn't put the thing quite in the way or in the setting in which

[1] *Problems in the Relations of God and Man*, by C. C. J. Webb, Fellow of St Mary Magdalen College, Oxford (London, Nisbet), 1911.

[2] *On the Traversing of Geometrical Figures* (Oxford), 1905.

[3] *On the Interpretation of Plato's Timaeus* (London, Nutt), 1889.

I gave it. But the essential point I suggested as important is given or implied, p. 270, ll. 8–13, viz. that whatever we might suppose we had to think of God's omnipotence, we had the solid fact that in the world *there existed imperfect beings*—human beings (whose willing and designing &c. we must distinguish from God's) · and that therefore there was *no difficulty* in supposing that the agency in nature might be of a similar kind (or, as you say, no *new* difficulty) and no more God's direct agency than our own agency was

You have put it a little differently . but this was my essential contention, i e. it saved one more *a priori* difficulties about dualism, because we have to admit and do admit such a difference from God's will and action as our own, and the supposition of an imperfect development in nature would therefore be no *new* difficulty (i. e. it no more commits us to dualism to suppose God not immediately operative in nature than our belief that He is not immediately operative in us—our will not His, our imperfect knowledge not His imperfect knowledge, &c) We explain our own wrong or imperfect doing by ascribing it to our own causality and not to God's. Similarly we may explain the imperfection, which so troubles the argument from design, as due to this other agency in Nature—not God. And I maintained it might be a mistake therefore to do what was usually done . . . attribute all that happened in nature directly to God as though it was His special province, forgetting that we have already made our own action our special province and not His. This is in effect just what your statement 'assumption we are nowadays too apt to make . attributed wholly . . . to God' comes to

Curiously enough, I have been led myself lately to the consideration of this very subject. The MS. of my own effort some years ago (14 ! I fear) is so imperfect and unsatisfactory that I have with one exception refused to let any one see it. . . . I undertook what I feared an almost hopeless task of reconstructing the fragments, although when I got to it I found it not so hopeless as I expected. And though I am dissatisfied at not being able to reproduce what I said and (worse !) what I felt, I have succeeded in making the thing intelligible. . . . I have said in it something pretty decidedly about the argument

from design and evolution, which leads most naturally to the thoughts I have just been referring to. I didn't develop it because my great object was not to vindicate the 'design' argument, but to make something quite different the essential. . . . I observe that you have not added what I should have thought the quite obvious correction of Zeller, viz. that Plato in the *Timaeus* commits himself to the *most decided dualism*. I explained this point in my reviews [a] of Archer-Hind, though not in the pamphlet. It is really to my mind rather absurd to make such an objection as Zeller's in ⟨the⟩ face of this. Once more let me thank you very heartily for the honour of the present of your book—a thing I could never have expected.

Yours truly,

J Cook Wilson

[a One of the main doctrines of the *Timaeus* ' relates to the existence of evil all cosmogonies which attribute the world to some divine activity find a difficulty here Some assume another spirit, an evil one, though partly subordinate to the good one , others, to avoid making an evil spiritual principle, assume an unintelligent matter, or in general some form of Necessity beside the Good Spirit We should suppose that Plato, if not monist, would incline to the latter and should have thought he clearly adopted it in the *Timaeus* Thus Plato says there are two causes, *νοῦς* and *ἀνάγκη*, each with its own effects and therefore the origin of the world is mixed . the necessary causes are irrational and disorderly (46 e, 48 a, &c) Again, Plato does not represent *νοῦς* as entirely creating matter, but only as modifying it God is never represented as making it entirely good but as only doing his best with it (30, 47, 53) '—Review in the *Oxford Magazine*, March 13, 1889, p 275]

POSTSCRIPT

SOURCES AND DATES OF THE SECTIONS OF THE LECTURES

POSTSCRIPT

WILSON, as is clear from many places in his familiar correspondence, made no pretence to be writing a systematic treatise on logic Thus he says, 'in the meantime I have been yearly improving (?) my logic lectures and have come to be pretty clear on certain departments—*that* may be publishable sometime, and yet here again I feel no inclination to write a general treatise on logic but only to discuss those parts to which I think I can make real additions'.[1] He was also most averse to premature publication. The revision of his lectures begun in his last years, tentative and imperfect as he knew it to be, was cut off in the middle by his death. Yet this has seemed no valid reason for withholding from a larger audience work of unusual, if unequal, distinction, perpetually reconsidered by its author and of proved service to many generations of Oxford youth The edition now submitted to the public will, I hope, in spite of many imperfections, be of profit to more than the group of Oxford tutors who desired its achievement and are already familiar with its general purport The merits of the author will perhaps cover blemishes in the execution of a task which seemed not indeed a trust expressly committed to my fellow executor and myself but still a debt we owed to Oxford and to the memory of our distinguished friend

That the author hoped some day to appeal to a wider audience seems certain Certainly also the lectures as they now appear are not what he would have made them or have desired them to be. This is due as much almost to his own literary procrastination as to his conscientious desire for truth. Painfully industrious in his solitary reflection, reckless spendthrift of his time upon subsidiary themes, profusely generous of his leisure to pupils and younger colleagues, in actual composition he resembled Dr. Johnson, he worked 'dilatorily and hastily, unwilling to write and writing with vigour and haste'. His many manuscript remains are not suited to publication; they

[1] Letter to S C. P., 13 vol.

are the dialogues of a man with himself, where the thread is often hard to discover. This habit of liberal self-colloquy gained upon him with advancing years, and he cared less and less to concentrate upon and reconsider the *form* of what he had thus composed.

His own apology for the delay to commit himself to the public is contained in a preface confidentially circulated to a few friends with his last printed volume of *Dictata*, that corresponding generally to Part I of the present book.

'In printing from a MS. which contains many corrections and many interpolations, representing, as they often do, gradual changes of opinion, it was natural to find much that at least required adjustment and much that had to be rewritten altogether. . . . My first reason for printing such extracts was that, as my views developed, I found I could not cover enough of the subject in the time available for lecturing But I have found the process of passing from MS. to print so salutary that I hope gradually to print the whole, adding the separate courses on Classification ⟨and Definition⟩ and on Hypothetical Thinking. These extracts however are neither to be published nor to be treated as published Those of my colleagues who were kindly anxious that I should publish my lectures years ago . . . will be, I am certain, well content now that I did not comply with their wishes. I really felt that there was too much which required a more thorough treatment and this uneasiness has had time to develop into (I think) a clearer consciousness and to produce considerable changes in the old matter as well as additions to it.

'There is a greater danger of fixing one's thought by publication and arresting one's own progress than is generally recognized. I have often noticed that quite able thinkers have the greatest reluctance in retracting anything to which they have committed themselves by publication, though the mistake may be perfectly obvious to the critic (whose work is incomparably the easier) and the author could only gain by admitting it. But the (printed) letter killeth and it is extraordinary how it will prevent the acutest from exercising their wonted clearness of vision. . . .

'I hope, by my present method, to gain that greater clearness

which is usually the result of printing . . . and at the same time to preserve the comparative freedom one enjoys as long as one's thoughts are only in manuscript. I hope also it will enable me at least (for I dare not count on more) to remain nearly as amenable to reason as if I had printed nothing. Of one thing I am fairly sure and that is that I shall want to revise both this pamphlet and the others which I have printed; while it is fairly probable and certainly desirable that several stages yet may precede publication, if I ever publish at all.

'The reader may observe in some of these lectures and in those on inference which I hope to print later a similarity, and sometimes a close similarity, to passages in the work of contemporary writers. I should like to explain that I have been lecturing a long time and the passages I have in view, or their equivalent, were delivered years before the appearance of the books aforesaid. In this connexion I may be permitted to add something which concerns a popular criticism of Oxford, as a University where philosophic systems are taken up when they have been worn out on the Continent. Years ago when, after the excellent instruction I had received from most distinguished philosophers in England and Germany, I began to try to think things out again for myself from the beginning (a tendency characteristic, I think, of Oxford in those days), I was gradually led in a direction very different (unless I am much mistaken) to the tendencies of the schools in which I had been educated. Perhaps this showed itself most in the subjects usually comprised under the title of the theory of knowledge, subjects but little represented in what I am now printing[1] For a long time I heard no sign of any such tendency as my own on the Continent, but, after the lapse of some years, one of my hearers told me a book had just appeared in Germany which had important affinities with my own views. This was in 1908, and the title of the book was *Das* sogenannte[2] *Erkenntnissproblem*. The main thought in the book seemed to me that indicated by its significant title, and that thought had been familiar in Oxford teaching and discussions long before the appearance of the German book. I am wondering whether I shall also in time hear that people

[1] Cf *Testimonia*, p lxxiv, from Mr H A Prichard's preface.

[2] 'sogenannte' is underlined by Wilson in his original manuscript

have begun to realize the failure of every theory of perception which has yet been proposed and to attribute this to a fallacy in the very idea of a theory of perception as ordinarily understood.

'Lastly, I find in looking at some contemporary works that certain passages in my lectures might well give the impression of having been motived by a polemic against statements in those works. The passages however had no such origin but arose entirely from independent thinking about the facts and about ordinary current logical doctrines. Of course some lectures are directed against contemporary authors, and I need not name them as the polemic is obvious, but the appearance of polemic in a certain number of other passages is quite accidental.'

To bring the work as it stood to a form more tolerable to the critical reader was the task I proposed to myself, and I have endeavoured to keep the mean between too great divergence from the original and too scrupulous an observance of strict fidelity. We must regret that Wilson allowed his work to accumulate in pupils' note-books, in manuscript and typescript in the disorderly way I shall shortly describe, a way fatal to consecutiveness, conciseness or complete clarity, whether of matter or form, in a subject where these virtues are peculiarly necessary and indeed incumbent upon an author. In the midst of maturer passages, dashed off in great haste, he left standing immature and inconsistent fragments of earlier date, themselves of varying degrees of elegance and precision. The only compensation is the light thrown upon the difficulties we may all encounter in treading the same paths. My own work, then, has been this. I first established a definitive text, by collation or comparison of the various sources enumerated below. This gave a sounder basis for this edition than any that existed before. This text I then edited for the press, endeavouring to bring out a result which might have been acceptable to the author had he superintended my anxious labours Disorderly sections I have rearranged, have corrected manifest slips, and excluded minor inaccuracies of expression while restoring in very many minute points the sense and words of the genuine original. The whole I have arranged in chapters, with a continuous series of sections, generally shorter than those which Wilson devised. I have

never consciously amended the matter nor have I destroyed, by misapplied industry, the flavour of the spoken word. The author, when committing his thoughts to paper, always anticipated delivery, and to give his sentences another turn would often have obscured the meaning. I have therefore generally left colloquialisms and characteristic abruptnesses; asyndeta, even anacolutha sometimes; and preferred a racy or vehement turn to a less striking alternative, where that existed. Arrian's *Epictetus* was my model, and the reader will, so far as he can, please to imagine, as he turns the pages, that he is listening to the eager and confident, the hurried tones of the man I have endeavoured to present in my memoir. I may not have succeeded, but I believe that I have not erred too much on the side of familiar speech. Wilson had good models in English for a colloquial style, and made the admirable effort to write in plain, honest, even homely language, avoiding cant terms and eschewing jargon, though he lacked the felicity of masters like Locke and Berkeley. His own view in this matter recurs repeatedly in the lectures, but nowhere so clearly as in the following words[1]: 'The authority of language is too often forgotten in philosophy, with serious results. Distinctions made or applied in ordinary language are more likely to be right than wrong. Developed, as they have been, in what may be called the natural course of thinking, under the influence of experience and in the apprehension of particular truths, whether of everyday life or of science, they are not due to any preconceived theory. In this way the grammatical forms themselves have arisen; they are not the issue of any system, they were not invented by any one They have been developed unconsciously in accordance with distinctions which we come to apprehend in our experience.

'On the other hand, the actual fact is that a philosophical distinction is *prima facie* more likely to be wrong than what is called a popular distinction, because it is based on a philosophic theory which may be wrong in its ultimate principles. This is so far from being appreciated that the reverse opinion is held and there is a tendency to regard the linguistic distinction as the less trustworthy because it is popular and not due to

[1] Omitted from the text at § 127.

reflective thought. The truth is the other way. Reflective thought tends to be too abstract, while the experience which has developed the popular distinctions recorded in language is always in contact with the particular facts.

'Now it is not uncommon in philosophic criticism that some popular term, when reflected upon, presents great difficulties to the philosopher; difficulties which are often due to some false theory of his which is presupposed. The criticism sometimes ends in pronouncing that really the term *only* means what is intended by some other term, so that it is in a manner explained away and any distinctive use of it is supposed to be an illusion, or the meaning of the term may be pronounced to be altogether an illusion. When the philosopher arrives at such a conclusion it too often happens that he is satisfied with this negative result and considers his work done. But the truth is that an investigation cannot be finished in this way. We ought under such circumstances to inquire how it is, if the given term *only* means something else, that language ever developed it and still so obstinately holds to it, and, when we suppose that we have explained a term away or shown that it is a mere unnecessary way of disguising some other meaning, we ought to put our result to the test by trying to do without the word criticized and seeing what would happen if we everywhere substituted for it what we suppose to be the truer expression.'

His conviction of the value of the unsophisticated distinctions which underlie popular usage led the author to those linguistic inquiries which are among the most remarkable in his work: the patient analysis of grammatical forms and the elucidation of the implications which are contained in ordinary speech. This may be seen in the chapter on Denotation and Connotation,[1] in that on the Meaning of grammatical forms,[2] and often elsewhere, as, for example, in the passage where he claims that 'ordinary language reflects faithfully a true metaphysic of universals'.[3]

I think, however, that in his use of speech as witness to logic and in his grammatical analysis he supports his argument upon evidence drawn from too narrow a field. Thus his treatment is coloured by an almost exclusive consideration of Graeco-Latin

[1] Part II, ch. xviii. [2] id. ch. viii. [3] § 82.

forms of expression, some of which are not altogether in agreement even with our own cognate tongue. This limitation may be seen in his decision to adopt the term 'nominative to the verb' for what we ordinarily call, in accordance with time-honoured usage, the subject of the sentence. This narrowness of survey has also perhaps prejudiced his treatment of the copula and of modality.[1] Yet even if this be true, his examination of language in this and other connexions appears to be of great value and interest to philosophy.

His belief in the efficacy of natural expression as an unsophisticated instrument of thought further led him to write down his reflections in the words which came most naturally to his pen. This is responsible for a certain looseness and, sometimes, inconsistency of expression. This fault was exaggerated by the manner in which the work as he left it had gradually grown up.

A few details will make this plainer. The lecturer opened with a very hurried survey of one or more sections which were to come; these sections were then dictated word for word. Thus he inverted the plan followed by Hegel In Wilson's case the commentary preceded the text and at least from an early period did not leave the text very far. He was diffuse, difficult to hear, and used geometrical examples The present text is thus based on a written original, the substance of which varied but little in oral delivery. The notes of pupils rarely, if at all, supply matter which was lacking in the text. The early drafts had careful transitions as a rule between the sections, each part ending with a characteristic aphorism The development, too, following closely upon the text-books, was approximately methodical in its progress In very early days there is some trace of actual improvisation from manuscript drafts, and the author would omit a point, sometimes add, as oral delivery suggested, consequences which had escaped him in the study. But as early as 1892 he had begun to dictate from notes taken by a pupil in some earlier year One object was no doubt to limit himself, another to have a fair copy before him. His own drafts were hurried and abbreviated, difficult even for himself to read These pupils' notes he would keep by him and work upon them by way of correction and addition. Some years

[1] Part II, ch x

passed and he took a later note-book to read from and to annotate in the same way. Thus the course grew and altered; some parts dropped out, inaccuracies crept in, and the whole was never seriously revised until a very late date and even then in a great hurry and without due consideration. His standpoint altered. The change is marked by two principal dates. Between 1907 and 1909 he worked over a large part of an unknown hearer's notes of 1904–5–6, inserting on spare leaves, on the verso, and on additional pages many subsidiary or substituted sections, as well as making minor additions to and verbal alterations of the original text.

Parts of this volume were copied very skilfully by a typist, the notes of two other pupils were similarly copied, and from these typescripts, with minor manuscript corrections and some additions, three volumes of *Dictata* were printed in 1913–14. The printing, however, went on in the reverse order, first the middle of Part II, then its beginning, and finally Part I. In the process Part I was very much altered and enlarged from its original lecture form, and matter belonging to Part II was heaped inconsiderately into Part I.

Moreover, the original words of the author suffered in these continual copyings, less indeed than if the lectures had not been precisely and verbally dictated, but sometimes seriously. The ends of sections suffered most, omissions having been made either by the lecturer to save time or by the hearer in his hurry. Many minor errors crept in, sometimes ludicrous mistakes. Moreover, the changes in verbal terminology were seldom exactly carried through, and when they were, the alteration was often, especially in regard to the words judge and judgement, merely mechanical.

None of this would have been of grave moment, where matter rather than manner is concerned, had Wilson really corrected his typescripts or his printed proofs with the sedulous care they deserved. There is clear evidence that he did not, obvious blunders are left standing, he added after-thoughts lightly, often corrected a faulty sentence with uncommon carelessness, not seldom accepted a pupil's variant for his own words.[1] Thus

[1] Since writing this I have come upon a letter from him in which he himself describes the origin of a mistake in the printed text which arose very much in the way I have indicated

the printed pamphlets or *dictata* tended to stereotype a text corrupted by the author himself, by his pupils, by the typist, and by the printer. Moreover, the matter consists partly of entirely new sections or paragraphs, dashed off with mature conviction but very hurriedly in 1912 or 1913, partly of ancient paragraphs remodelled to suit the changed point of view. Most strange of all, certain parts represent word for word his own earlier lectures but have, by a slight turn, been made to appear as the arguments of some imaginary disputant and are then confuted. New patches have thus been oddly mounted on old garments and sections moved about with some disregard for methodical procedure.

In the circumstances an editor's task was perplexing and a compromise inevitable. I have endeavoured to indicate by my tables the main character of the changes made. By dating, at least approximately, the various sections or parts of sections and by some rearrangement I have tried to remedy obvious blemishes and at the same time to satisfy the more curious or careful reader. To have marked every change precisely and produced a critical text would have been out of proportion to the expense and might have distracted the reader from the general burden of the argument, itself at times sufficiently perplexed and intricate. I have not endeavoured, any more than Wilson himself did, to make the terminology consistent throughout. This will be especially clear in Part III. Where, however, late manuscript evidence existed I have cautiously preserved the expression nearly exactly, though the style of it is more unstudied and less elegant than his earlier work. Lost in the matter and having long ceased to read, except at rare intervals, the great English models, he neglected too much the clearness and distinctness which cost care in composition; his sense of form declined and his word order often became German rather than English. This last fault I have endeavoured generally to remove.

In regard to the lectures as a whole, there is one further cause for a certain limitation. Wilson never broke loose from the structure dictated by their origin. He combined, from the first, two original courses: one a lecture in Formal Logic, where he followed the order of the text-books, the other an historical and critical inquiry into the Theory of Knowledge, principally the

English psychological school from Locke to Herbert Spencer. This will explain why two different motives so constantly meet and cross one another in this book. He is combining two different, if cognate, inquiries, embraced in Oxford under the traditional title Logic. A third current comes from the historical study of Plato and Aristotle, particularly the *Organon*. Thus some of the interpolated sections arose from a preoccupation at a certain date (1908–9) with the origin and process of knowing. Again the first half of Part II, which was in origin an exposition of the modern theory of Judgement, was gradually altered into a criticism of contemporary idealistic logic, and again Part III, originally a criticism, under Kant's inspiration, of the syllogism as the form of mathematical demonstration, developed into a general view of inference which was to justify Wilson's attitude to mathematical reasoning. Similarly the two discussions of Conception seem hardly logical in the strict sense, turning instead round a criticism of the ideology of the eighteenth century and its modern descendants Finally, the whole discussion of Induction appears in the place occupied by the treatment of Method in text-books subsequent to the Port Royal logic, and the author never had time to unify an original criticism of Mill with his own treatment of Inference. These are the causes which make the book uneven in texture and which have inevitably led to some inconsistency. The dates will, I hope, help to explain them.

I have been asked, and the question is natural, whether it can be determined what thinkers most influenced Wilson's progress. In the prefatory words which I have cited above he himself speaks of the advantages of avoiding publication which may stereotype an author's views At the end, however, he claims to have reached important opinions many years before the date of the preface Scattered about his papers are many notes of dates, showing his anxiety to assert independence of the contemporary progress of philosophy. Both things appear true. From a very early date the germs of what has come to comparative maturity in this book are present in his writings and lectures. This is certainly true of his dissatisfaction with the idealism current in his early manhood in Oxford. It appears also to be true that he originated his characteristic tenets for himself, if there can be said to be any strict originality in the

matter. That is to say, his whole effort was directed, as we shall see, to maintaining the objectivity of mathematics and the mathematical sciences. He was at first content with the Kantian solution to which he was no doubt guided by Green, and so far as mathematical method is concerned he accepted this solution to the end. But he was dissatisfied very early with the excessive subjectivity—as he considered it—of Kant's general position and his own conviction of the reality of the mathematical object was reinforced by his growing conviction of the immediacy and certainty of perceptual experience. These two things played into one another and were strengthened by his continuous absorption in problems of interpretation of Greek philosophy;[1] he fell more and more under the influence of the frank objectivism of the typical Greek scientific view of the world. This also helped to shape his doctrine of the universal.

If now we ask what modern influences, besides his reaction from Kant, led him in this direction, the answer is not Lotze. Lotze's presentationism is fundamentally opposed to Wilson's views, though with characteristic loyalty he did not go out of his way to say so The influence of Lotze is plain in much of his detailed work as well as in the fundamental conviction that man has, in that philosopher's words, 'an immediate certainty regarding what is universally valid, upon which all conviction rests, a certainty which, whether called intuition or by some other name, must be admitted to exist, though its origin is and will, in all likelihood, remain unexplained'.[2]

The determining influence was, I think, that of Ueberweg, whose *Logic* and *History of Philosophy* were familiar companions of Wilson in his formative period. In the preface to the second edition of his *Logic*, that philosopher recommends his work to the reader 'as a thorough attempt to reach a relatively objective theory of knowledge as against Kant's subjective criticism'.

No doubt the tendency had long been in the air, especially in Germany. At Göttingen Lotze was the successor of Herbart. But it is Ueberweg who, in his Logic, continually insists on

[1] Apart altogether from philosophical debate, we can see this, I think, in his paper of June 1890, 'On some apparent anomalies in the use of μή'. See *Transactions of Oxford Philological Society*, 1889–90, pp. 27–8.

[2] Cf Lotze, *Logic*, §§ 356–7.

a view which is, in essentials, identical with Wilson's. Ueberweg, too, rests his theory upon the certainty of mathematical truth and upon a conviction of the objectivity of our apprehensions. Not that Wilson followed him slavishly or literally, but he read him early and adopted his method, partly historical, partly critical, in the treatment of logical problems. Thus his general attitude to Kant was probably affected by Ueberweg as well as by his own English predilection for the scientific rather than the philosophic attitude to Nature. What, however, weighed with him more than anything else was his firm conviction of our immediate apprehension of the truths of Euclidean geometry, of the pure science of quantity and of mathematical physics, especially optics and acoustics, and of their objective validity. With this conviction went that other, that the method of mathematics is not by process of inferring but by direct intuition. These convictions explain his comparative neglect of Descartes, Spinoza, and Leibniz. Spinoza's method of exposition he thought fundamentally false, and he disagreed of course with Leibniz's views of mathematical method. Thus there remain for predominant influences Plato, Aristotle, and Kant, especially the *Prolegomena* of the latter, with the author's conviction of the certainty of our direct intuition of the truths of mathematics as they were conceived by the Greeks.

This is not the place to review his philosophy, even were I competent for the task. Excellent, if brief, summaries of Wilson's doctrine have been given elsewhere,[1] but it is best to allow him to speak for himself. He certainly does not disguise his teaching. Two clues to his thought, however, I have found useful in studying him. One is stated by himself in almost the last words which he wrote. They will be found in § 370, which I have placed immediately after a passage of his very early lectures to which he was referring. His objectivity is there declared to be the corrective of a one-sided subjectivism from which, under Kant's influence, he had started.[2]

Wilson, like his master T H. Green,[3] spent much toil over the ordinary problem of perception and the theory of knowledge

[1] *Testimonia*, p lxxv.

[2] See Hegel, *Logic* (the Encyclopaedia), 'Third attitude of thought to objectivity', ch v.

[3] Green's *Works*, Preface to vol. i (Nettleship) and vol ii.

connected therewith. In the endeavour to refute the scepticism of the English school of Locke's successors his argument recurs perpetually to the presuppositions of Locke, Berkeley, Hume, and Kant. His own view of reality comes out most clearly in the letters on *Primary and Secondary Qualities.*[1] I do not think he ever wavered from the faith which is there freely and fully set out. His conviction stands there in a form which cannot be doubted and is nowhere else so patent. He believes that we have direct apprehension of the physical world as it was philosophically conceived by Newton and his successors. Wilson, however, appreciated in a way in which Green, not being a mathematician, could not, the problem of knowledge in regard to pure mathematics This is the second clue to his logical inquiries His central problem was not the question of the validity of our thought in regard to things—the objects of the senses. He came to think of a theory of Perception as philosophically preposterous. His logical inquiries were largely guided by the attempt to justify our knowledge of the world—the world of three-dimensional space and of time as conceived by the physicists of the eighteenth and early nineteenth centuries. To this end his arguments on the subordinate question of what he calls modern mathematico-philosophical fiction really lead. The elaborate criticism of part of Mr. B. Russell's work[2] is an attack upon the opposed school of thinking and his dissatisfaction with the current Idealism really sprang as much from these convictions of his as from his views of the presuppositions of everyday experience and his teaching as to Perception and theories thereof.

This brings me to a last point. Wilson was in extreme earnest in his search for truth This it is which gives his writings a tone of almost religious zeal in regard to questions where enthusiasm is uncommon. This made his teaching so impressive to his listeners. He supported his convictions in a region from which emotion is supposed to be excluded with an ardour which never flagged and which fretted at every obstacle. This aspect of his mind and character comes out very clearly in the splendid fragment of a reconstruction of the Ontological proof of the existence of God.[3] There the old rational argument gets a new

[1] Part V, §§ 519–40 [2] §§ 422–32, 477–500, 501–18. [3] §§ 565 seqq.

turn from the introduction of the emotional element as part and parcel of man's religious experience. The same unexpected intrusion of human emotion into what appears at first sight a purely intellectual problem may be found in a striking passage in the inaugural lecture with which he entered upon the duties of his chair. 'Before asking if it is true, we may ask whether we should like it to be true. Such a question may seem unscientific, but in philosophy at least we should clear our reasonings a good deal if we confessed to ourselves what we wanted to believe and what we hoped was untrue.'[1] This is surely a daring saying for a professor of Logic and is made more striking by its place in the midst of a merciless exposure of a superficial philosophy.

Wilson had a moral repugnance to Scepticism in any form. Here, especially, he reminded one of Socrates. In the *Meno*, Plato puts into Socrates' mouth the uncompromising reply to a sophistical dilemma that, even if the eristic difficulty is verbally unanswerable, he is sure that we shall be better men if we undertake our inquiries in the faith that they can be solved. Thus behind the Athenian's acute dialectic lay a background of natural belief which gave even to his abstract speculations a moral fervour. So was it with Wilson. Acute though his intellect was, it is always the servant of his conviction It was the same in his moral philosophy, as, for example, in his belief in the retributory basis of punishment. He is like a preacher with a text to expound, he knows the answer and exhausts himself in the search for proofs This makes his negative and critical work so powerful, renders his positive utterances somewhat dogmatic. Where he traces an error or a mistaken conviction to its sources, as in his brilliant treatment of Negation or of Symbolic Logic, he is unrivalled Even where he seems to fail, he throws more light on the question than thinkers who rest satisfied with the mere detection of error and contradiction.

But it is time to say farewell to a task in which I have been supported by the conviction of the advantage of these lectures at least to earnest beginners and of the value of the rest of the book to more advanced students. There is something very bracing in the way in which Wilson attacks his problems and

[1] § 357.

judges even the immortals 'with all the freedom of an equal'. I close my labours with a heightened regard for that 'warmth and determination of spirit', to translate Francis Bacon's words, 'which aged not in the pursuit of its purpose and grew not cold in so long a space of years'.

A. S. L. F.

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OXFORD.
20 September 1925.

NOTE

In 1907 Wilson began a systematic rewriting of his lectures on what is usually called the theory of the judgement. His objects were partly to substitute a more correct terminology for his old phrases, especially to get rid of the erroneous use of the terms subject and predicate, partly to eliminate language which might suggest a subjective view of the nature of apprehension and the meaning of statement, and partly to work out more fully the grammatical problems which he believed to underlie the mistakes of the current logical doctrines. Part II, Chapters 1–9, were thus largely rehandled.

In 1911–13 he reconsidered this work and the whole of his lectures Parts I and II as far as the end of ch. 15. He was working on chs 13–15 up to the end.

The result of these two revisions was the issue in printed form of his *Dictata* (that is substantially the present book to the end of § 123) The work was very much hurried, and he intended, as he says, to reconsider and revise it all.

These printed pamphlets are of the following dates:

§§ 72–3, §§ 89–93, §§ 122–3 . .	December 1912
§§ 78–88 and §§ 94–121 . . .	May 1913
§§ 37–8, §§ 50–71, §§ 74–7 . . .	December 1913
§§ 1–13, §§ 22–36, §§ 39–42 and 49 .	January 1914

The remainder of the sections included in this group are §§ 14–21, which were intended to be recast and were never printed, and certain sections which have been remodelled by the editor, as is shown below

During the same period he made many small verbal changes and some additions to Part III, Chapters 1–3, but he never reconsidered ch. 4, and in chs 5–7 inclusive the alterations were purely verbal and often very insufficiently weighed.

To represent exactly the date of the various elements would have involved great expense in printing and confusion to the reader. I have therefore shown by tables the principal dates as accurately as I can.

By 1904, I indicate sections which, except for slight editing, had reached their present form in 1904–5.

By 1907, I indicate sections which appear to have been written between 1907–9

By 1904 and 07, I indicate sections completed in 1904–5, with additions of the later date.

By 1910, &c., I indicate sections which seem to have been composed in those years.

By square brackets I show sections which I have reconstructed. It must be understood that all the sections contain re-handled matter, often of very much earlier date; the dates show the period of their final revision.

In the text itself I have marked §§ 14–21 and §§ 70 and 71 to show additions (〈 〉) made much later. Those sections will give an idea of the manner in which the work grew and was altered. In the first group the additions are usually mere additions, in the second they indicate a change of philosophical outlook A few sections (e. g § 42), as I have explained in my notes, are of special interest as containing views which the author himself had held and later abandoned.

SOURCES AND DATES OF THE SECTIONS OF THE LOGIC LECTURES

SOURCES CONSULTED AND COLLATED FOR THE TEXT

1. A Wilson MS, &c ·

(i)	Early lectures, summary notes.	*c.* 1874–5
(ii)	2 vols, Logic lectures, revision of an earlier course in MS	1880–1
(iii)	MS note-books and folios.	*c.* 1900–4
(iv)	Later MS note-books	*c* 1907–14
(v)	MS folios for typist or printer	1911–13
(vi)	Typescript of Part I, and Part II–§ 123, based on I. F (i) and MS of a pupil (not available) of ? earlier date	? 1910–12
(vii)	Typescript of Part III (excluding chs 5–8), from MS of a pupil (not available) taken from I D in substance	? 1909
(viii)	Typescript of Part IV (excluding ch. 5)— same source as (vii).	? 1904–6
(ix)	Printed Pamphlets (*Dictata*)	1912–14

Pupil's note-books, representing developments, between dates shown, of Part I, Part II, 1–15, Part III, 1–4, Part IV, 1–5.

B. (i) G F. Hill, Merton (annotated by J. C. W.). 1889–95
(ii) J. Pedder, Oriel
(iii) W. A Craigie, Oriel (annotated by J. C W.).
(iv) A. S. L Farquharson, University.
(v) C C Edgar, Oriel

C. (i) W. R V Brade, B.N C (annotated by J C W *c* 1898). 1896–1902
(ii) T F Cattley, Hertford Inference and Special Logic only.
(iii) G B Cobb, New College. Inference and Special Logic only
(iv) H J Creedy, St John's (annotated by J C. W.). Inference and Special Logic only
(v) P Neville Prior, University (annotated *c* 1903–4)

D Anon (large MS corrections and additions, *c* 1907–8) 1904–6

E (i) K Lyon, Merton 1907–10
(ii) Anon (1908–9)
(iii) C D Carew Robinson (lender's notes typed)

F. (i) J Flynn, Pope's Hall 1912–14
(ii) O H T Rishbeth, Merton
(iii) A M Tristram, Worcester (last lecture taken 14 June, 1914)

II. Part II, chs 16–17

A H R P Gamon, Exeter (annotated by J C W) 1902

B (i) G F Hill, Merton 1889–90
(ii) W A Craigie, Oriel 1891–2
(iii) A S L Farquharson, University. 1892–3

III. Part II, ch 18 MS note-book not later than 1901–2.

IV Part III, chs 5–7

A Author's MS note-books §§ 305–8 were never delivered, probably for want of time

B (i) A J Jenkinson, Fellow of B N C. 1904
(ii) W D Ross, Fellow of Oriel 1904

V. Part III, ch 8 Mainly from II, B (i)

Of the above sources :—

I A and D, II A and B (ii), IV A and B (i), have all been collated.

Remainder compared and where necessary collated.

The authority for the rest of the text is shown in the notes below The Table of Contents is the Editor's work

CHRONOLOGICAL TABLE

Part I. Ch. 1. §§ 1–5, 1904; [§ 6 to 'provisional' 1904; 1912] § 7, 1904; [§ 8, 1904].
Ch. 2. §§ 9–10, 1912; §§ 11–13, 1913.
Ch. 3. Except [§ 14] and §§ 22–3, 1912, all is of 1904 and ⟨1907⟩.
Ch. 4 [§ 24, 1912] [§ 25, 1907 and 12] §§ 26–8, 1907, but last para. 1913. § 29, 1907 and 13, §§ 30–3, 1913

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Ch. 7 § 69, 1911, §§ 70–1, 1910 and ⟨1913⟩, § 72, 1907, § 73, 1911.
Ch. 8 [§ 74, 1907, 1912] §§ 75–7, 1911 (and 13), § 78, 1910.
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Ch. 10 [§§ 84–5, 1911] §§ 86–8, 1907, §§ 89–93, 1904, Note 1907, § 94, 1910, except 'In the above' to end, 1912
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Ch 14 [§ 126–'meaning them'] Rem §§ 126–37 and §§ 140–1, revised and largely rewritten 1914, but § 141, 1907 §§ 138–9, MS *c* 1904
Ch 15 § 142 and §§ 144–7, 1907, § 143 and §§ 148–50, March 1908.
Chs. 16–17 1902, see II A and B, above
[Ch 18] *c* 1900, see III, above, §§ 206–7, 1906

Part III. Ch 1 §§ 208–9, 1907 [§ 210, 1907, 1913] § 211, 1907 'But imagination'–end, 1904 §§ 212–15, 1904 and 08, §§ 216–18, 1904, except 'In the facts . . . equal to B, the same holds', 1912. §§ 219–27, 1907

Ch. 2. § 228, 1904, § 229, 1904 except foot-note, 1912. §§ 230–3, 1904, § 234, 1907, § 235, 1904, § 236, 1904, except ' If it were abstracted ' to end of section, 1907. [§ 237, 1904, 07, and 14] [§ 238, 1914] § 239, 1904, § 240, 1904, except ' It would usually . . . clearly then ' and note, 1912, and ' The foregoing '–end, 1914, § 241, 1904.

Ch. 3. §§ 242–3, 1904 and 07, § 244, 1907, § 245, 1904, §§ 246–7, 1907, § 248, 1908, to ' confined to itself '. Rem. 1914. §§ 249–50, 1907, §§ 251–61, 1904.

Ch. 4 § 262, 1904, § 263–end, 1907 All Part III, chs. 1–4, was worked over very slightly in 1913–14.

Chs 5–7. 1904, see IV, above

Ch 8 1890, see V, above, §§ 325–7, from *Nature*, vol 63, No 1624, 13 Dec 1900

Part IV. Ch. 1 § 328, 1910 [last para 1899] §§ 329–31, 1904, note to § 329, 1899 (from I C 11). § 332, 1904, except ' This also explains '–end, 1909 § 333, 1904, foot-note 1909, § 334, 1904 and 09, §§ 335–7, 1904, § 338, 1904, except ' We have seen that '–' change with A', ' and in consequence '–' connexion ', and ' It is true '–' understand ', 1909.

Ch 2. §§ 339–40, 1890–1, §§ 341–5, 1890–1, collated with 1898, I C (11).

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Ch 4 § 354, 1890–1, §§ 355–64, 1889, § 365, 1891, § 366, 1914.

Ch 5. §§ 367–9, 1904, § 370, 1904, except ' and the ordinary theory . . retina ', and last para , 1909

Ch 6 §§ 371–400 MS of two public lectures of 1898

Part V. [§§ 401–5, §§ 406–14, §§ 438–50, MS of relatively early date.

[§§ 415–21, §§ 422–9, §§ 430–2, MS 1912–14, intended for British Academy.]

§§ 433–7 MS *c* 1904, collated with two pupils' MS of later date.

§§ 451–9 MS. *c* 1913–14.

§§ 460–564 MS correspondence, dated in text.

§§ 565–81. Typed notes for address, 1899, with later additions, dated in text, collated with notes (of a portion) by S C. P , derived from original MS

§ 582. Letter, 1911

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