

WHAT CAN LOGIC DO FOR PHILOSOPHY ?

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THE aim of this paper is to show, with the help of a few examples, that certain very simple logical considerations can throw some light upon philosophical problems, including some of the traditional problems of metaphysics and theology. One of my main points will be to establish by way of these examples, that there are such things as philosophical problems—not only pseudo-problems. Another point will be that we may apply to them simple methods of logical analysis which have little or nothing to do with an analysis in terms of our elementary experiences (sense-data, perceptions, or what not) or with an analysis of the meaning of words. The methods I have in mind are, rather, those of constructing, or analysing, or criticizing, arguments, and ways of approaching the problem.

One of my main difficulties in preparing this paper was that of selecting my examples, that is, of selecting philosophical problems ; especially in view of the fact that the most obvious and important source of examples—the philosophy of language—is one which is to be discussed in another symposium, and therefore better left aside. I have tried hard to select examples which I think are both representative and interesting ; but I fear that I have not quite succeeded. I thought, further, that since in a short paper like this it is impossible to go very deeply into the analysis of any one problem, it will be better to select a number from various representative fields, including what is usually called metaphysics and ethics, and to be frankly sketchy. On the other hand, I have tried to introduce at least some slight degree of coherence into my somewhat mixed collection of

problems which ranges from the problem of the existence of philosophy and the problems of causality and determinism to those of the role of experience in ethics.

1.

Let us begin with a brief discussion of an extremely general philosophical problem—the much discussed question whether there is anything which may be called “philosophy.”

I have always felt much sympathy with Kant, the positivists and all others who, repelled by the extravagant claims of some philosophical system builders, began to doubt whether there was anything at all in philosophy. I have only admiration for those who reacted against apriorism—the attitude of possessing, if not all fundamental knowledge, at least the key to it—and against empty verbalism. But it is interesting to observe the fate of these brave fighters against apriorism and against verbalism.

The positivists who were mainly anti-apriorists, that is to say, those who believed that there is no room for a third realm of studies besides the empirical sciences on the one hand and knowledge of logic and mathematics on the other, found themselves nearly immediately in difficulties when asked to give a criterion of empirical knowledge. Their answers were, very often, naive and mistaken. But this is not my main point here. What was so striking was what I may call a strong aprioristic character of their answers. Their attempts to characterise empirical knowledge led them to the construction of fairly complicated philosophical systems, such as the sense-data theories or phenomenalism—systems which were perhaps not so very different from those against which they originally reacted. And these systems, in spite of paying lip-service to anti-apriorism, took up more and more the strange character of the old aprioristic systems; one felt quite clearly that their defenders had an axe to grind, and that they were much more interested in this philosophical axe-grinding than in learning from experience.

A similar fate befell those positivists who reacted not so much against philosophical apriorism but rather against

philosophical verbalism. To them, philosophy was “mere words”—meaningless verbiage. But when confronted with the task to explain the criterion of meaningful language, as opposed to meaningless verbiage, they got into very serious difficulties, proposing, for example, criteria in terms which turned out to be themselves meaningless. They discovered that they had started from a naive philosophy of language ; and they were soon surrounded by difficulties which they found practically unsurmountable. To these difficulties they reacted by giving up arguing about problems ; instead, these philosophers, who had started by denouncing philosophy as merely verbal, and who had demanded that, instead of attempting to solve them, we should turn away from the verbal problems to those which are real and empirical, found themselves bogged up in the thankless and apparently endless task of analysing and unmasking verbal pseudo problems.

This is how I see the recent history of a movement with which I thoroughly sympathise, as far as its starting points are concerned—the revolt against apriorism and philosophical verbiage. It has shown, I believe, that there exist, at the very least, two kinds of philosophical problems—the philosophy of the empirical sciences, which tries to analyse what makes the empirical sciences empirical, and the philosophy of language, including the theory of meaning.

But where does logic come in here ?

The parallelism in the fate of the two revolts, against apriorism and against verbiage, does not seem to be accidental. And I believe that the development could have been, to a certain extent, foreseen. The reason is that an assertion like “there cannot exist statements besides those of the natural sciences and of logic,” is very similar to the paradox of the liar, since it is certainly no statement of the natural sciences, and hardly one of logic (since it is about logic). Thus one way in which logic, and especially the analysis of paradoxes, might help us is by warning us against such sweeping aprioristic assertions and positions, and by making us a little more modest.

2.

But does this mean that there must be a "science of philosophy" or a philosophical system? I do not know, and I think rather not.

I believe that most of us think too much in terms of subject-matters or disciplines—physics, chemistry, biology, etc. Admittedly, we have in some of these unified theoretical systems. But these may or may not be found. In any case, we find them in our attempts, not so much to build up a coherent "body of knowledge" (a particularly silly expression), but to solve certain definite problems. Our subject matters or disciplines or "bodies of knowledge" are, I think, largely didactic devices designed to help in the organisation of teaching. The scientist—the man who does not only teach but adds to our knowledge—is, I believe, fundamentally a *student of problems*, not of subject matters.

Now problems often cut through all these subject matters. A problem of neuro-physiology, for example, may need, for its solution, bits from practically all the sciences known to us. And the fact that it needs mathematics, for its solution does not make it a mathematical problem or one of physics.

From this point of view, there is no particular difficulty in admitting the existence of philosophical problems. They may turn up in all sorts of contexts, and may need all sorts of considerations—empirical, logical, mathematical—for their solution. They can be called "philosophical" either because of certain historical associations, or because of the fact that they are of a *second-storey character*—connected with questions *about* science, or *about* mathematics, or *about* art, etc.

There seems to be *one* great difficulty to this view, from the point of view of those anti-aprioristic tendencies which I share: how do we *test* an answer to a philosophical problem? An answer to a scientific problem, it seems, can be tested by experience; but what about the status of an answer which is neither purely logical nor testable by empirical science?

The proper reply to this is, I suppose, on the following lines : philosophical answers must always remain tentative. There is no reason, it seems, why we should reach agreement on them. But this is no reason to deny the existence of philosophical problems. It is an aprioristic dogma (held by some positivists) that only such problems are real problems to which we can ("in principle") find a definite, established answer. This dogma must be given up. Even in the empirical sciences, our answers are, as a rule, tentative. They have in the past often changed, and we cannot know whether in future they will not continue to do so. Admittedly, the situation in philosophy is worse, owing to the absence of empirical tests, and those who find this situation distressing should better turn to some other field. Nevertheless, we sometimes make some progress—for example, we may discover that some proposed theory does not really answer the question which it is supposed to answer. This may not be much of a success, but it is something ; and it is the kind of thing which is achieved, mainly, with the help of logic.

3.

I shall turn to the philosophy of philosophy at the end of this paper. Meanwhile I intended to discuss a series of slightly more concrete problems ; and I take as my first example the problem of *causality*, because it is an example of a problem where logical analysis can help us even in a mildly constructive way.

In this section, I shall sketch¹ an analysis of what may be called the logical mechanism of causal explanation ; and I shall apply the analysis to some questions raised by Hume, and by some theists.

A scientific explanation of a certain singular event *E* (i.e. an event that happens in a certain place at a certain time) always consists of a number of statements from which a

¹ Cp my *Logik der Forschung*, section 12, pp. 26ff ; *The Poverty of Historicism III* (*Economica*, N.S. XII), section 28, pp. 75f, and *The Open Society II*, note 9 to ch. 25.

singular statement e , describing the event E , can be deduced. These premises or explanatory statements are of two kinds, universal statements u (or laws), and singular statements i which state what may be called the initial conditions.

In other words, an explanation of an event E consists of a deductive inference,

$$\begin{array}{c} u \\ i \\ \hline e \end{array}$$

in which from universal laws u and initial conditions i the statement e describing the event E (which is to be explained) is deduced.

Trivial premises are, of course, often taken for granted, or "suppressed."

In order that the explanation should be acceptable or satisfactory, the statements u and i must be well tested (*independently* of the event E in question; see below).

In the natural sciences, we usually do not use any longer the vague terms "cause" and "effect"; but I shall now show that the logical mechanism just analysed can be interpreted as covering what may be called a "causal explanation," and that what is usually called "cause" is described, in a causal explanation, by the initial conditions i , and the "effect" by e .

Take, as an example, that the event E which we wish to explain causally is the death of Mr. X. Somebody may suggest that the cause of his death is that he took a spoonful of potassium cyanide; and if we can find evidence that he did, we shall accept this as "cause" of his death. But why? We may also find that he ate, immediately before, a bar of chocolate. Why do we say that his taking potassium cyanide caused his death rather than his taking chocolate? Obviously because we assume the truth of the universal law that everybody who takes a spoonful of potassium cyanide dies at once, while we do not believe that a corresponding law holds for bars of chocolate. In other words, we accept the suggested cause only because we believe in the truth of a certain universal law u ("Everybody who takes a spoonful

of potassium cyanide dies at once”), which, together with the description of the cause, i.e., with the initial condition *i* (“Mr. X took a spoonful of potassium cyanide”), allows us to deduce the statement *e* (“Mr. X. died”) which describes the effect which is to be explained.

A fairly important point in our analysis is that we must have good evidence in favour of *u* and *i*, *independently* of the fact that *e* is true ; that is to say, *u* and *i* must be well tested, and we must not count the fact that X died after taking potassium cyanide as evidence in favour of *u*, nor the fact that *u* and *e* are established as evidence in favour of *i*.

I do not, of course, believe that this simple analysis is exhaustive. Undoubtedly there are cases which conform to our analysis but which we should hesitate to call causal explanations. For example, the famous syllogism “All men are mortal. Socrates is a man. Socrates is mortal” conforms to our scheme. Nevertheless, it is certainly a bit awkward to say that the fact that Socrates is a man is the cause of his being mortal. And we should be even more reluctant to call a certain day the cause of the following night, even though we believe in the truth of the universal law which allows us to deduce (or predict) the arrival of the particular night in question from the statement that it was daytime. I believe that it is possible to augment our analysis in such a way as to allow for the difference between such cases of deductive explanations or predictions and the other cases which we may feel inclined to accept as truly causal explanations. But I shall not go into this matter here. For our present purposes it is sufficient to note that all causal explanations fall under our scheme, even though other things may fall under our scheme as well. In fact, all that we need at present is this :

If anybody says that a certain singular event *I* is the cause of a certain singular event *E*, then he tacitly assumes that there is an independently testable universal law *u* such that from *u* and *i* (i.e. the statement describing *I*) we can deduce *e* (the statement describing *E*). Or more briefly, to say that *I* is the cause of *E* is to assume the truth of a universal law *u* such that, in its presence, *e* follows from *i*.

To be sure, in most cases of ordinary experience, u is "suppressed," that is to say, u is taken to be so trivial that we do not mention it. For example, if we say that my holding a match to it was the cause of this fire's beginning to burn, or when we say that the cause of the death of Charles I was that his head had been cut off, then we are, as a rule, not conscious of the fact that we assumed, in each case, the truth of a universal law. But if we had reason to believe that people whose heads had been cut off usually are the better for this operation, then we certainly should not accept the explanation which historians offer for the death of Charles I.

All this is very trivial ; but it can throw some light on well known philosophical problems.

Let us first take Hume's problem whether there is a *necessary* connection between a cause and the effect which it produces. Hume's answer is negative. Ours, I think, must be affirmative.

It must be affirmative because whenever we consider I to be the cause of E , we do so in view of a (usually suppressed) law u in the presence of which e follows from i ; and since we may take it that the relationship of deducibility may be described as a "necessary" one, we may say that the connection between I and E is a necessary one (although not "absolutely necessary," but only "necessary relative to u ."

Hume sees only I and E , and overlooking the suppressed u , he thinks that there is no connection between them - nothing beyond the fact that events similar to I have been, as a rule, followed by events similar to E . He does not notice that, if we formulate this fact in form of a universal law, the dependence of E upon I becomes, relative to this law, logically necessary. And he does not see that, even if we introduce the universal law in question merely as a tentative hypothesis, this means that we assume—tentatively and hypothetically—that the relationship between I and E is a necessary one, in the sense described.

I shall not here discuss Hume's attempt causally to explain a belief in a regularity or law by habit, although I think that this particular attempt at causally explaining away

causality can be easily shown to be completely mistaken ; I only wish to point out that he overlooked that a belief in a universal law u —whether or not causally explicable in terms of habits or associations—is, rationally, identical with a belief in a *necessary* connection between the corresponding I 's and E 's. We need not believe in the necessity or even in the truth of u in order to see that, given u , e can be logically obtained from i . Accordingly, we may describe the situation in this way : in the same degree in which we believe in u or doubt u or disbelieve in u , in the same degree do we believe or doubt or disbelieve that there holds a kind of *necessary* connection between I and E —a connection such that, given I , E must follow.

Our simple and somewhat trivial logical analysis thus allows us to explain certain psychological attitudes, and it does so perhaps better than the psychological analysis employed by Hume.

A second application of our analysis is to the cosmological proof of the existence of God (Aquina's "Second Way.") This argument has often been criticised, from many points of view, and it has recently been reformulated by Whittaker. It seems to be based on the intuitive idea that, if we can ask for the cause of an event, we can also ask for the cause of this cause. In this way we obtain a regressive chain of causes, and, if the regress is to be finite, a first cause, which we may call "God" ; or more precisely (since a cause is not a person or thing but an event or fact), the fact of the existence of God.

I shall not discuss that aspect of this argument which I consider the only one which is philosophically relevant (viz., that this argument, even if successful, could at best prove the existence of a powerful Creator, while what interests us is the existence of a God who is good). I shall only point out that the regress from one "cause" to a preceding one is always relative to one or another *universal law*, and that, accordingly, the argument from causation assumes the universal laws of nature to be given. It therefore cannot conceive God as the creator of universal laws, or of order in nature, and must clash with the design argument (St.

Thomas's "Fifth Way"), especially in the form proffered by Jeans and Whittaker (in which the fact that some natural laws can be conveniently formulated in a mathematical language is taken to indicate that God must be a mathematician).

4.

I now turn to the problem of determinism. It is easy enough to visualise the world in the way the determinist sees it—as a kind of clockwork or planetary system or as an electro-chemical machine. It is more difficult to analyse in words the determinist's faith.

I think the following formulation may be satisfactory.

Every future event, the determinist may say, can be predicted with any desired degree of precision, provided we can measure all the relevant initial conditions (with an appropriate degree of precision), and provided we have completed the discovery of the relevant natural laws.

According to quantum mechanics, this statement is either not relevant or not true; but I shall neglect this aspect. Even without quantum mechanics, we can see that the statement is very unsatisfactory. Any more complicated and more distant event will defeat us; we simply cannot obtain the knowledge of the initial conditions which we would need; and we even cannot, as a rule, find out, from the formulation of the problem—the event to be predicted—what the initial conditions are which will be relevant to the problem, and to which degree of exactness they must be known. (The only exception to this seems to be that misleading case, the planetary system—a simple mechanism which is as well insulated as a clockwork, and not at all characteristic of the physical world in general). Thus we shall probably never be able to predict the weather in London with any precision even for a month ahead.

But apart from the very important and insurmountable difficulties which are connected with the initial conditions, there is no reason to believe that we shall ever have a complete knowledge of the universal laws of nature. We operate with hypotheses, and we find again and again that

we have to improve upon them. And even if this process would come to an end, we could not know that it has come to an end.

Thus the determinist's programme is, at best, a pious wish, for a kind of divine omniscience ; but it may be something worse—a completely misleading idea. (This is suggested by two aspects of scientific method which, it appears, is always one of bold oversimplifications. We have reason to believe, first that most of our so-called natural laws are lucky oversimplifying guesses ; secondly that our experimental method involves interference with the things we study : we construct artificial, oversimplified cases—cases for study. One may even put it, perhaps, like this : the natural sciences do not deal so much with hard facts as with *interpretations* of facts, in the light of our theories, guesses, prejudices).

This discussion of determinism is completely independent of any problem of ethics. But it may clear the ground for one simple logical consideration in the field of ethics.

I shall discuss the question :

When do people consider human behaviour as praiseworthy or blameworthy action and when do they consider it not so ?

I suggest a rough and very simple answer :

If people believe that under the same initial conditions, *as far as they can be independently ascertained or tested*, all or most people would act in this way, in other words, if they think that the behaviour can be satisfactorily (i.e., without the help of *ad hoc* hypotheses) *causally explained* with the help of independently established initial conditions and universal laws (“All men—or most men—in such circumstances act in this way”), then they do not think that it is either praiseworthy or blameworthy. Or in other words, they think it praiseworthy or blameworthy or, as we may say, “morally free,” to the degree in which it is not causally explicable, on independently ascertainable initial conditions. They may then say, if they are determinists, that the action flows from the personality or from early influences, etc. ; that is to say, they postulate hidden initial conditions. Or they

may say if they are indeterminists, that the action was due to the free will of the individual. But both agree, roughly, that if ascertainable initial conditions—those for which we can obtain independent evidence—can be considered as sufficient “causes,” and to the degree to which they can be so considered, the action is not one to be morally judged.

This is just another suggestion about the way in which our very simple scheme of causal explanation may contribute a little towards certain philosophical problems.

5.

Our analysis of causal explanation can be applied to other and, in my opinion, more important problems—to problems of the philosophy of society, and of history. I have more especially, one problem in mind—the problem whether there are what may be called “natural laws of social life” or “sociological laws”; that is to say, laws which describe regularities of social life which are not produced by legislation, or by religious or moral custom.

The problem mentioned is, of course, of fundamental interest for the student of the methods of the social sciences. Nevertheless, it is not merely a methodological problem. It is of great significance for our whole attitude towards society and politics. We enter, as it were, into a new world—the world of purposes, of rational actions which pursue ends, and, of course, of irrational actions also. Has this world a similar structure as the world of physics and, say, physiology? The question is certainly of philosophical interest, even if it turns out to be a simple question of empirical fact. But it hardly is a question of empirical fact: at least it is not one on which social scientists have reached agreement. (Even in the natural sciences, we are constantly dealing with *interpretations*, rather than with hard facts. In the social sciences, this seems to be so to a higher degree).

The question whether there exist sociological laws has often been answered, both in the affirmative and in the negative, in a way which I consider mistaken. People have

asserted, for example, that there are laws of social evolution. I do not think that there are good reasons to believe that such laws exist, either in the field studied by biology or in that of sociology. Others have denied the existence of sociological laws altogether, the field of economics included. (This would make practically all rational political action impossible, since it does make impossible to predict the consequences of changed conditions). Many people seem still to believe in the view of Comte and Mill—that there are two kinds of laws, “laws of succession” (or of evolution) and “laws of co-existence.”

I think that nearly all these views are mistaken and mainly because of misunderstandings of a logical character.

It is impossible for me here to go into these interesting questions of social philosophy in detail, and unnecessary because I have done so elsewhere². I shall confine myself to a story : A friend of mine, an economist, recently expressed his scepticism concerning his science. In his opinion, economic laws did not exist. Economics was only a system of empty definitions, without empirical content. He illustrated this by an example. “If asked by the Government what policy they should adopt in order to have full employment without inflation, I could not answer ; indeed, I suspect, that there is no answer.” I pointed out to him that he had, in order to illustrate the absence of economic laws, just formulated one : The statement “There does not exist a policy which allows us to have full employment without inflation” (whether this is true is another question) is indeed a model of a sociological law. In order to see this clearly, we have only to apply some of the simplest logical rules to it—the equivalence of universal statements to negated existential ones. On the basis of this equivalence, all universal laws can be expressed in “There-does-not-exist” form. For example, the second law of thermodynamics by “There does not exist a machine which is one hundred per cent efficient.” The similarity with the economic hypothesis mentioned above is obvious.”

² Cp. especially my *Poverty of Historicism* (*Economica* N.S. XI and XIII).

6.

Another example of the significance of logic for ethics.

Perhaps the simplest and the most important point about ethics is purely logical. I mean the impossibility to derive non-tautological ethical rules—imperatives: principles of policy; aims; or however we may describe them—from statements of facts.

Only if this fundamental logical position is realised can we begin to formulate the real problems of moral philosophy, and to appreciate their difficulty.

As one of the most central problems of the theory of ethics, I consider the following: If ethical rules (aims, principles of policy, etc.) cannot be derived from facts—how then can we explain that we can learn about these matters from experience?

We can also put the question in this way: if aims cannot be derived from facts, can we do more than see that our system of aims is coherent? And if it is, can we do more than try to alter the facts, to “reform” them—in such a way that they conform to our aims?

The simple answer is, I believe, that not all the facts which can be altered can be altered in conformity with every preconceived and internally coherent system of aims. To take the example mentioned above. We may know that certain facts—such as unemployment, or inflation—can be altered. We may aim, on moral grounds, to avoid both. But we may learn from our attempt to do so that our system of aims, although internally coherent, does not cohere with some of the laws of economics, previously unknown to us.

7.

To close with a general remark.

A number of philosophical problems can be shown, it appears, to be composed of an empirical and of a logical component. The analysis into these components, together with the claim that there is no further problem left, do not, if successful, establish that the original problem was a pseudo problem; on the contrary, it shows that there is a problem, and the way in which it can be solved.

By WILLIAM KNEALE.

(1)

I AGREE with nearly all that Dr. Popper has said, but I think a hostile critic might object that he has not explained what he means by "logic" or tried to say in general terms how logic can be of service to philosophy. No doubt he can make a good reply to this charge. For he has stated his views on the nature of logic very clearly in a number of recent articles, and he may argue that it is more important for his present purpose to give a number of examples of the use of logic from which we may learn the "feel" of certain kinds of problems. It seems to me, nevertheless, that it may be useful to approach the subject of our symposium in a more direct way, namely, by considering the attitude which philosophers have adopted towards logic and the attitude which they should adopt.

When Aristotle introduced Logic into the household of Philosophy, he did not suggest that she should be received as a member of the family, and she was not treated as such, in spite of a plea from her admirer Chrysippus. On the contrary, until last century she remained a mere maid of all work, required to do everything for the family, including some tasks that were beyond her strength. But at the end of last century she obtained a position of greater independence and dignity as housekeeper to Mathematics, and since that time her relation to Philosophy has been obscure. Mr. Russell maintains that, if she is asked, she will still visit the house of Philosophy from time to time in order to do for her old mistress as a kind of intellectual charwoman. On the other hand, some of her new friends, including Professor Ayer, say she is so angry about her treatment in the past by Metaphysics, the eldest son of the family, that she has already done for him in another sense of that phrase. Dr. Popper refuses to believe this story, saying that she could never behave so violently. Whatever the truth of the

matter may be, Metaphysics has not been seen about lately, and Philosophy is still wondering what she may expect from Logic. Now I want to suggest that, if Philosophy is wise, she will admit Logic as a full member of her family, not so much in the hope of getting some odd favours for the other members as in order to set right an old wrong and increase her own reputation with persons of good judgement. For, in spite of the obscurity of her status, Logic has won for herself the respect of the scientific world.

In plain English, my suggestion is that philosophers should not spend so much time debating the boundaries of their subject and the propriety of their methods, but recognize that in logic they have at least one genuine field of study where intensive work may yield rich rewards. In recent years a great deal of the most valuable work on logic has been done by mathematicians. To put things on the lowest level, it is a pity that all the kudos should go to persons of another profession. But if that were the only consideration, I should not be much concerned. For demarcation disputes between scientific trade unions are of no great importance. If mathematicians are ready to take over some work that philosophers persist in neglecting, good luck to them ! In the long run all who are interested in the work will call themselves mathematicians, and that name will come to have a slightly wider application than it does at present. My chief reason for hoping that logic will be taken more seriously in philosophical studies is a belief that some of the most important questions of logic are philosophical rather than mathematical according to the present usage of these words and that persons with a philosophical type of mind should find them interesting. I do not want to enter here on a discussion of the meaning of the word "philosophical", and so I shall try to explain what I have in mind by means of two examples. If my belief is correct, you will recognize why I have chosen them.

Let us consider the foundations first. Logic has been defined in various ways, but it seems most satisfactory to begin by saying that it is the theory of entailment, *i.e.*, the study of what follows from what and why. There are

reasons for holding that this definition is too wide, but at first sight it is more likely to seem too narrow. Since we often say that one proposition follows from another in virtue of the logical forms of the two propositions and sometimes talk about logical form without explicit reference to entailment, it may be thought that logic should be defined by reference to form rather than to entailment. But how are we to understand the phrase "logical form"? If we talk about the shape of negative, conjunctive, disjunctive, conditional, universal and existential statements in a particular language, *e.g.*, ordinary English or Peano's symbolism, we seem to be tying ourselves to the study of that language in a way which is not proper for logicians. For logic is surely not concerned with English to the exclusion of French, or with Peano's symbolism to the exclusion of Hilbert's. Nor is it enough to say that a negative statement is any statement which is equivalent to an English statement containing "not". For that still leaves us tied to English as the standard language. The only way out of this difficulty is to define the logical forms by reference to entailment. This is what Dr. Popper has done recently in detail.¹ We may say, for example, that a statement *a* has the logical force of a conjunction of *b* and *c* if, and only if, *b* and *c* together entail *a* and *a* entails *b* and *a* entails *c*. I do not mean that the use of the word "entails" or any synonym is essential. But other methods have the same effect. Thus, if we define a non-general truth-function by means of a truth-table such as the following :

b	c	conj. (b, c)
T	T	T
T	F	F
F	T	F
F	F	F

we are really specifying certain relations between the conditions for the truth of the truth-function and the

¹ Cf. his article "New Functions for Logic" in *Mind*, vol. LVI, 1947.

conditions for the truth of some other statements, called its arguments. The table printed here is just another way of saying what I have said above by the use of "entails".

Now, as Dr. Popper has shown, the only *technical* terms we need for formulating logic in this fashion (at least up to and including the restricted calculus of propositional functions) are "entails" and a phrase such as "the result of substituting x for y in z ". But this is not to say that we need no other terms whatsoever. On the contrary, the presentation of logic as a meta-linguistic theory involves the use of a number of words that are sometimes called logical (*e.g.*, "and", "if", "all") or symbolic devices that play the same role; my example shows this clearly enough. There is no vicious circularity, however, in such use. For when we propose to speak about the logic of a certain language or class of languages (*i.e.*, about the rules of entailment holding for that language or those languages), we are entitled to talk in a language that shares some features with the languages we talk about. But some interesting questions arise. What is the minimum apparatus required in a meta-language for talking about the logic of a given language? And if we cannot talk about the logic of a language L_1 , without using a language L_2 , which itself has a logic formulable in another language L_3 , what bearing has this on the thesis of those who call themselves conventionalists? Since our deliberate conventions must always be formulated in some natural language, can we ever by convention escape to anything radically new? Finally, what sort of truth are we enunciating when we talk about the need for a hierarchy of languages? I do not think that all these questions have been answered satisfactorily so far, and they seem to me to be questions that should interest philosophers for their own sake. But if they have been answered satisfactorily, there remains at least a lot to be done before the answers are as widely appreciated as they should be, and this appears to be work for philosophers.

For our second example let us consider the essential incompleteness of any deductive system of more than a certain richness in types of variable. Gödel has shown that,

if a formalized system like that of *Principia Mathematica* is at once self-consistent and rich enough to contain the natural numbers or to allow for their introduction by definition, it is possible to construct in the symbolism of the system formulæ which cannot be demonstrated or refuted within the system, although they are necessarily true or necessarily false, as the case may be.² In order to decide whether such a formula is true or false, it is necessary to use a system that includes not only the symbolic apparatus of that with which we started, but also variables of a higher type than any found in the original system. And when we have succeeded in reaching a decision in this way, we find that it is possible to construct formulæ which cannot be decided in our new system. And so on *ad infinitum*. In short, the realm of logically necessary truths cannot be exhausted by any axiomatized system, however rich. Starting from a different point and working independently, Tarski has proved a similar conclusion, namely, that we shall inevitably fall into self-contradiction if we try to define within a formalized language what it is for any formula of that language to be true.³

These results seem surprising because we find it difficult to reconcile the story of the inexhaustible wealth of logic with the simplicity of its beginnings. Like Locke, we are all inclined to suppose that logic is trivial. And so in a sense it is ; for the most complicated demonstration is only a sequence of obvious steps. But we fall into error because we overlook the increase of complexity comes with ever higher types of variables. Logic is not all like the so-called algebra of logic, that is to say, the algebra which can be interpreted either as a calculus of classes or as a calculus of propositions. For there is no rule of thumb by which all formulæ constructed with logical symbolism alone can be resolved into patent truisms or patent contradictions. Here again we have something that should be of the greatest

² "Über formal unentscheidbare Sätze der Principia Mathematica und verwandter Systeme" in *Monatshefte für Mathematik und Physik*, vol. XXXVIII, 1931.

³ "Der Wahrheits-begriff in dem formalisierten Sprachen" in *Studia Philosophica*, vol. I, 1936.

interest to philosophers. I do not say that it is our duty to try to turn ourselves into mathematicians in order that we may take part in the progress of mathematics. But it is surely our business to try to understand what happens during the advance. For one at least of the marks of a philosophical mind is a desire to make the truth seem plausible, and the work that philosophers can do by following up this interest of theirs may be of great importance to civilization.

(2)

Although I have been arguing that philosophers should cultivate logic for its own sake, I wish also to maintain that the rest of their work will profit from this study, and that in an obvious way.

Whatever we may say about the nature of philosophy, we must admit that during our philosophizing we often have occasion to use technical terms of logic. When, for example, we discuss phenomenalism, we talk a lot about hypothetical propositions. And in our inquiries about induction we try to distinguish different kinds of universal propositions. This frequent use of logical terminology is not surprising. For it is obvious that the testing of philosophical suggestions involves saying "What would follow if this were true?" Socrates could do this without using much technical terminology, and so, no doubt, could we if we tried hard. But when once we have come to understand what we are about, it is natural and time-saving to use the language of logic. There is also another and more important reason for the use of such language by philosophers. We are interested in the classification of the various claims men make to knowledge, and before we have gone very far in this enterprise we discover that we must take account of the logical forms of the assertions in which they express these claims. It is of great importance, for example, that the statement "Iron is magnetic" is universal. If a man really knows that iron is magnetic, his knowledge of this must be something very different from his knowledge that the canister holding his tobacco is made of iron. Now confusion and frustration may result if philosophers use logical

terminology without paying much attention to the development of logic. Natural scientists often accuse us of being a generation or more out of date in our references to science, and it does sometimes seem that metaphysics is the heaven to which good hypotheses go when they die. But it is just as serious if philosophers persist in using logical doctrines that are discredited. I shall try to illustrate this by means of an example.

There are still some philosophers who try to work with Kant's definition of an analytic judgment as one in which the predicate concept is contained in the subject concept, although not all judgments have subjects and predicates and none of those that do are of a kind we want to call analytic. But this confusion is now well known and requires no further comment. It is less widely recognized, however, that it is unprofitable to define an analytic proposition as one guaranteed by the law of non-contradiction alone. According to the generally received tradition all logical truisms are to be accounted analytic, but it is not the case that all logical truisms can be shown to be such simply by presentation in the form "Not both p and not p ", *i.e.*, without appeal to any other principle than the law of non-contradiction. Let us consider, for example, the conditional statement "If all animals are mortal and all men are animals, then all men are mortal". This is a statement such as Aristotle might have used for illustrating the principle of the syllogism in *Barbara*, and it is a logical truism. But it is not obviously of the form "Not both p and not p ". It is true that we can derive a self-contradiction from its negative, but to do so we must use the principle of the syllogism in *Barbara* as a rule of inference. Now those who accept the definition of "analytic" mentioned above and hold at the same time that all logical truisms are analytic seem to be saying in effect that all logical truisms can be derived from "Not both p and not p " by substitution of other expressions for " p ", or, to put the matter in a more striking way, that with substitution as our sole procedure of inference and "Not both p and not p " as our sole axiom we can obtain the whole of logic. And this is false.

If we wish to use the word "analytic" in such a way that all logical truisms are analytic, it seems best to define the word by reference to logic. This is what Mr. Russell does in the preface to the second edition of his *Philosophy of Leibniz*, published in 1937. He writes there: "The important distinction is between propositions deducible from logic and propositions not so deducible; the former may advantageously be defined as *analytic*, the latter as *synthetic*". This is undoubtedly an improvement on most earlier definitions, but it should be noticed that we cannot use this definition unless we are already able to recognize propositions deducible from logic. And so we explain nothing if we now say that all logical truisms are analytic; for we only assert the triviality that they follow from themselves. In order to characterize logic we must proceed in some other way. Now it is often said that logical truisms are statements whose truth is guaranteed by the rules of usage of the symbols they contain. This description undoubtedly includes all truisms that belong to logic, but it may conceivably include others; for it is equivalent to the old phrase "*a priori*", and some philosophers have maintained that there are *a priori* truths other than those commonly assigned to logic. If in order to be more precise we suggest that logic is concerned only with truisms whose truth is guaranteed by the rules of usage for the *formative* signs alone (*i.e.*, the signs definable by reference to entailment in the way adopted by Dr. Popper), then there are undoubtedly *a priori* truths other than those of logic, *e.g.*, "Cats are animals". And if we combine this view of logic with the usage of "analytic" suggested by Mr. Russell in the passage I have quoted, we come to the conclusion that there are synthetic *a priori* truths. Why does this conclusion appear shocking? Is it not because the words "analytic" and "synthetic" have come to have overtones of meaning (dare I say "emotive meaning") which are relatively independent of any precise definitions we may offer? If my diagnosis is correct, it is time that these words were banished from philosophical discussions. When we are no longer worried by associations with Kantianism on the one

hand and Positivism on the other, we may be able to think about these matters more clearly.

(3)

I shall not try to add any more to the detailed illustrations that Dr. Popper has given in support of his thesis that logic can help in the solution of philosophical problems, because I do not want to side-track the discussion by starting a lot of disconnected controversies. But the points of detail he has raised are interesting in themselves, and it is proper that there should be some reference to them. I shall therefore say something about two of his suggestions that seem to me doubtful. His main contention about the relevance of logic to philosophy does not depend, of course, on the correctness of each of his analyses.

I wish, then, to consider first his remarks about Hume and necessary connexion. In the third section of his paper he argues that a particular event E is explained when a statement e recording it is shown to be derivable from two independently established premisses, namely, from u , a universal statement or law, and i , a statement about certain initial conditions I . He thinks that the necessity by which E is said to be connected with I is really the necessity of the conjunction of E with I in relation to the law u , and that Hume's mistake consisted in neglecting u . I agree that Hume did not say enough about the explicit formulation of laws, and that he talked psychology of a rather dubious kind when he should have talked logic. But I do not think that Dr. Popper's account of the matter does justice to our ordinary usage of the phrase "necessary connexion". When we say that E is necessarily connected with I , we do not mean merely that e follows logically from i and some universal statement u . We wish also to convey that u states a necessary connexion between kinds of events, and the fundamental problem is to explain the usage of "necessary connexion" in this latter context.

Many philosophers hold that a law of nature can be no more than a universal material implication, *i.e.*, something which could be expressed in a sentence of the form "For all

χ , it is not the case that χ is ϕ and χ is not ψ ". But this seems unsatisfactory ; for we suppose that we can derive contrary-to-fact conditionals from laws of nature, and we certainly cannot derive them from universal material implications. From the premiss that all the men in the next room are playing poker we cannot conclude that if the Archbishop of Canterbury *were* in the next room (which he is not) he *would be* playing poker. We can infer only that if the Archbishop *is* in the next room he *is* playing poker—a proposition which has no interest for us when we already know that he is not in the next room. It may perhaps be said in reply that laws of nature differ from propositions about all the men in the next room as not involving any restriction to a finite region of space or a finite period of time. No doubt this is true, but it makes a difference to the argument. For if the logical form of a law of nature is supposed to be otherwise the same as that of a proposition about all the men in the next room, its consequences must be supposed to be same *mutatis mutandis*. Our statements of natural law purport, then, to be something more than universal material implications, and I suggest we should say boldly that they are statements of necessary connexions which we cannot hope to know *a priori*. I know that it is very unfashionable to speak of necessary connexions in this way, but I can see no other way of doing justice to the ordinary thought of plain men and scientists.

Secondly, I wish to say something about Dr. Popper's remarks on freedom and responsibility. In the fourth section of his paper, which is concerned mainly with determinism, he says that we do not consider human behaviour either praiseworthy or blameworthy when it can be explained in the sense already mentioned without the help of *ad hoc* hypotheses, *i.e.*, when it can be brought under generalizations about the behaviour of all (or most) men in certain circumstances. And from this he concludes that conduct is morally free in so far as it is not causally explicable on independently ascertainable initial conditions. I agree that we do not praise or blame men for behaviour which can be brought under generalizations about what all (or most)

men do in certain circumstances. If a man gives away the names of his friends under excruciating torture, we say that he cannot be held responsible. But it seems strange to say that a man's conduct is free only when it is unpredictable. I may be sure that a friend will behave rightly in certain circumstances and yet think that his conduct will be free and deserving of praise. Does Dr. Popper want to deny this? If not, how are we to interpret his remark about freedom and causality? Does he mean that prediction based on knowledge of a man's character is fundamentally different from causal prediction?

(4)

In all that I have said so far I have deliberately avoided making any generalizations about philosophical problems, partly because their nature is the subject of another symposium, but partly also because I doubt whether it is possible to provide a simple formula which will cover them all. I certainly do not wish to maintain that they are all problems of logic in a narrow sense of that word, or even that we shall cease to be perplexed about them when we are as well versed in logic as we should be. On the contrary, it seems to me that some of the most interesting problems, *e.g.*, that of the relation of mind and body, have little, if anything, to do with logic. I admit, of course, that we know fairly well what problems are to be called philosophical; and I suppose that these must have some features in common, since a man who is interested in one of the group is usually interested also in the others. If I am pressed to say what this common element is, then I am inclined to agree with the view that they all have to do with the ways in which we use words. But I think this way of characterizing philosophical problems should be subject to two reservations.

In the first place, philosophical problems do not arise in the void. They arise during our use of language for non-philosophical purposes, and they can rarely, if ever, be solved without some study of subjects other than logic and linguistics. Dr. Popper has drawn attention to the fact that they often contain an empirical element, and it would be easy to

pile up examples in illustration of this. I shall refer only to the problem of the relation of body and mind which I have already mentioned. Anyone who thinks he can solve, or dissolve, this problem without paying any attention to the findings of neurologists or the inquiries of psychological researchers, must be very simple-minded or very doctrinaire. I do not say that neurology itself will provide an answer, not yet that we should accept any of the claims made by psychological researchers, but that the problem arises in such connexions as these and cannot be considered profitably without reference to them. If we professed to solve it from the logic book and the dictionary alone, we should make ourselves as ridiculous as any dogmatic metaphysicians of the past. It seems to me conceivable that the problem may be philosophical and yet insoluble in practice until empirical science has made much more progress. This does not mean that a philosophical solution would be of the same kind as a scientific hypothesis, but that our difficulty in getting the issue clear may be due to some limitation of our non-philosophical knowledge. The history of philosophical thinking about space provides an illustration of what I have in mind.

Secondly, when we say that philosophical problems have to do with the ways in which we use words, we do not prescribe a method for solving any single philosophical problem. For each problem must be solved in its own way. That is what we mean by calling it a problem. And so, if we accept this view of philosophy, we are not committed to any set of philosophical views. In particular we are not committed to saying that philosophical problems are pseudo-questions which disappear when we adopt a certain manner of speaking. For this latter thesis is not merely a general characterization of philosophy, but a claim to have solved all the problems of philosophy, and it must stand or fall by the success or failure of its defenders in dealing with all the particular problems commonly called philosophical. In my opinion it falls.

III.—By A. J. AYER.

I AGREE with Dr. Popper that there are philosophical problems. But merely to say this does not carry us very far. We want to know what it is about a problem that makes it philosophical ; how, for example, the method of philosophy differs from the method of a natural science ; and to this question neither Dr. Popper nor Mr. Kneale gives at all a clear answer. Dr. Popper speaks favourably of logical analysis, but he does not say what he takes logical analysis to be ; and while he implies that there are some philosophical problems that are not soluble, or not wholly soluble, by logical analysis, he does not say what these problems are or what other methods are required to solve them. Mr. Kneale makes the point that formal logic is itself a subject for philosophers to study, but neither he nor Dr. Popper makes any serious attempt to show how the study of formal logic will help philosophers to answer questions in other fields. Again, they both allow that philosophical problems may contain an empirical element, but they do not give any account of the way in which empirical questions enter into philosophy, nor do they show how the philosopher's treatment of them differs, say, from that of the natural scientist. Dr. Popper does indeed make one promising suggestion : that philosophical problems have what he calls " a second-storey character ", but he does not develop it. No doubt these problems are, as he says, " connected with questions *about* science, or *about* mathematics, or *about* art ", but as he does not go on to say how they are so connected, his statement does not tell us very much. Neither is it very helpful to be told that " philosophical answers must always remain tentative ". Dr. Popper makes this remark as the " proper reply " to the question how philosophical statements are tested ; but so far from its being the proper reply to this question, it is not a reply to it at all. If the answers must remain tentative, then presumably the tests, whatever they may

be, are inconclusive ; but to say that the tests are inconclusive is not to tell us what they are. And why is Dr. Popper so sure that there cannot be a "definite, established answer" to any philosophical problem? If, as I think he holds, no scientific theory should ever be regarded as finally established, it is because it may at any time be falsified by further observation. But then he tells us that the situation in philosophy is worse because of "the absence of empirical tests." Why is it worse? I suppose because a philosophical theory cannot in that case be confirmed by any observation. But if it cannot be confirmed then neither can it in this way be refuted. And if philosophical theories are not subject to empirical tests, what sort of theories are they? One possible answer would be that they were logical ; but this Dr. Popper hesitates to give. Nor would it square with his view that philosophical answers must always remain tentative. For I suppose he would allow that on logical issues a more positive decision was theoretically attainable.

At this point I think that Dr. Popper might reply that we all know well enough what a philosophical problem is, at least in the sense that we are able to recognize one when we come across it, and that it is a mistake to try to give a general definition of philosophy, or to specify the character of philosophical method. I think he might say that to attempt anything of this sort would be to take up the "aprioristic" attitude with which he reproaches the positivists. But what is wrong with this aprioristic attitude? In so far as philosophical questions are not empirical, what other attitude towards them is possible? Dr. Popper is very hard upon the positivists, but not, so far as I can see, with any very good reason. Thus, he repeats the old objection that any attempt to formulate their criterion of meaning must be self-defeating ; on the ground that a proposition to the effect that all significant propositions must either be empirical or else be, in some sense, propositions of logic, is itself neither empirical nor a proposition of logic. But I do not admit this. It seems to me that such a proposition can perfectly well be taken

either as an empirical statement about what people mean by "meaning"; or else, what seems to me preferable as a prescriptive definition; and in that case it may be held to belong to logic. No doubt it then legislates for itself, but I do not see that this is necessarily vicious. Dr. Popper refers darkly to the paradox of the liar, but he does not show that in this case any paradox arises. And if he wishes to make it a rule that no proposition can significantly refer to itself, how does this differ from the type of aprioristic assertion to which he objects?

Apart from an unsubstantiated charge of verbalism, the only other reason that Dr. Popper gives for condemning the positivists is that some of them have been addicted to phenomenalism. For he looks upon phenomenalism, and indeed upon sense-datum theories in general, as "aprioristic systems". This is a change from the usual accusation that the introduction of sense-data is a piece of dubious psychology, and to my mind a change for the better. For, as I have argued elsewhere, those who maintain that we directly observe sense-data, as opposed to physical objects, are not putting forward an empirical hypothesis; they are laying down a convention. They are proposing to describe certain features of our experience in a different way from that in which they are ordinarily described. And the point of doing this is that the fact, if it is a fact, that our ordinary perceptual statements can be interpreted as statements about sense data throws light upon their meaning. Similarly, the fact, if it is a fact, that it can be described in the terminology of sense-data tells us something about the character of our experience. Thus the sense-datum theory is aprioristic only in the sense in which any choice of concepts is aprioristic. And it does not follow from this, as Dr. Popper seems to think, that it is in any way dogmatic, or even that it is arbitrary.

I dwell upon this example of phenomenalism because the consideration of it may help us to see a little more clearly what it is that we are doing when we philosophize. I take it that the philosophical problem of perception is the problem of giving a logical analysis of perceptual state-

ments. Now the phenomenalist's way of dealing with this problem is to invent an artificial language and to try to show that the perceptual statements, which it is his purpose to analyse, are translatable into it. Whether he is successful or not is a point that I am not now discussing. To this extent, his method is aprioristic, and his answer to the problem takes the form of a logical statement. His analysis results in saying that two sets of statements are logically equivalent ; or, if he does not go so far as this, it results in saying that certain statements of the one class are entailed by statements of the other. There is, however, an important sense in which his method is not aprioristic. For the language of sense-data is not constructed arbitrarily. It is intended to describe the facts by which our ordinary perceptual statements are verified. The position is not that we first invent the language of sense-data and then look round to see what it can be used for. Sense-data are brought in as a consequence of our reflecting upon what we mean by perceptual statements ; that is, as a consequence of our reflecting upon the nature of the facts which verify them. These facts are discovered to be complex in a way that is not very clearly brought out by our usual manner of describing them ; and the language of sense-data is brought in to do justice to this complexity. But what is this procedure of "reflecting upon the facts" ? I suggest that it takes the form of considering what are the situations that would make a given proposition true. Now since any answer to this question must consist in a description of these situations, what we get by these means is the replacement of one form of description by another. And so it may look as if we never leave the field of logic. Yet there is a sense in which we do leave it. It is not as if in order to discover the correct re-description we merely looked up an agreed table of linguistic rules. It is rather that we put ourselves imaginatively into some situation in which the statement we are analysing would be true, and try to make it out in detail. This process of making it out in detail is indeed a process of re-describing it ; but there is a sense, I think, in which our new description may give us a clearer insight

into the facts. If you like, this is only another way of saying that it may enlighten us about the meaning of the statement we are analysing. But that it is another way of saying this seems to me important.

But how is the validity of such an analysis to be tested? Principally, I think, by looking for counter-examples. We try to find a case in which one of the statements we are comparing would be true and the other false; and if we do find such a case we conclude that they are not equivalent. Thus those who reject phenomenalism sometimes try to show that there is no proper equivalent in the sense-datum language for a statement to the effect that unobserved physical objects are causally related. They have maintained that the sensory statement which is supposed to be such an equivalent may be true in cases where the statement about the physical objects is false. I do not myself think that they are right in this contention; but if they were they would have refuted the phenomenalsists' analysis. The method is logical in the sense that finding a counter-example brings out a difference in the logical relations of the statements in question; the conclusion reached is perhaps that some statement which is entailed by one of them is not entailed by the other, or possibly that some statements which are evidence for the one are not in the same degree evidence for the other. But these conclusions are not reached by *a priori* calculation. We do not come to them simply by applying a known set of transformation rules. It is rather that by means of them we may hope to discover what the transformation rules of our language are.

So long as we cannot find a counter-example, we may hold that our analysis is valid; but I suppose it is always possible that some counter-example may be discovered. To that extent Dr. Popper is justified in his remark that "philosophical answers must always remain tentative". But what if the answer in question is negative? Even if we are never entitled to say of a philosophical theory that it is definitely established, are there not some philosophical theories of which we may say that they can be definitely refuted? An instance which comes to my mind is the

causal theory of perception. I think it can be shown that if the causal theory were true we should have no good reason to believe in the truth of any statement about a physical object ; and since it is a fact that we do have very good reason to believe in the truth of a great many statements about physical objects, it follows that the causal theory of perception is false. Put more formally, my argument is that statements about our sensory experiences which are evidence for the truth of ordinary perceptual statements are not in the same way evidence for the truth of the causal statements to which according to the causal theory, these perceptual statements are supposed to be equivalent. That is my counter-example. It may not be satisfactory ; many philosophers would deny that it was ; but if it is satisfactory, then it definitely refutes the causal theory. My point here is that if a philosophical theory is false, there is a way of disproving it ; and I see no reason why such disproof should not be allowed to be conclusive. What is not clear is that there is any way of definitely proving a philosophical theory to be true.

A better illustration of these points may perhaps be drawn from one of Dr. Popper's examples. Towards the close of his paper he puts it forward, as an empirical fact, that in so far as people believe that someone's behaviour can be "causally explained", they "do not think that it is either praiseworthy or blameworthy" ; and from this he apparently draws the conclusion that to say that a person has acted freely is equivalent to saying that we cannot assign any cause to his action. That is, it sometimes happens that our knowledge of the "initial conditions" which obtain in the given situation, together, presumably, with our knowledge of the laws which govern human behaviour in general, is not such that we can derive from it a satisfactory causal explanation of the action in question ; and to say that this is so is supposed to be equivalent to saying that the agent acted freely, and was therefore morally responsible for what he did. Now it seems to me, as it does to Mr. Kneale, that this conclusion is incorrect. And what convinces me that it is incorrect is that I can fairly easily conceive of

counter-examples. Thus a lunatic may act unpredictably, but we do not for that reason conclude that he is morally responsible. And conversely, there are many cases in which we have no difficulty at all in accounting for the way that a person has behaved, and yet are still prepared to say that he was a free agent. The fact is, as I see it, that the actions for which people are praised or blamed are those in which some choice of the agent's is a causal factor. Thus the decisive question is not *whether* we can explain the action or not, but *how* we explain it. So long as the explanation is in terms of the agent's own character and choices we are inclined to say that he has acted freely. The cases in which we are inclined to say that he has not acted freely, and so is not morally responsible, are those in which the agent's choice is either not a causal factor at all, or else an insignificant factor. In such cases it is said that the agent could not help himself, either because it is thought that he would have acted in the same way no matter what he had decided, or else because the circumstances were such that no reasonable man would have chosen otherwise. Thus, if someone points a pistol at my head with the result that I surrender to his wish, there is a sense in which I could have disobeyed him; but if what he demands of me is such that no reasonable man would sacrifice his life rather than grant it, then even though what I do is something that would have been considered wrong if I had done it deliberately, I am not held to blame for it. I am acquitted on the ground that I acted under duress. The mistake which Dr. Popper and many others have made is that of confusing causation with compulsion. No doubt in any case in which I act under duress my action is causally explicable; but the converse does not hold. It is not true that whenever my action is causally explicable I act under duress.

This analysis of moral freedom would need a great deal more elaboration for it to do justice to the facts; but at present I am more interested in the question of method. It is to be noted that Dr. Popper starts out with an empirical statement, and that from it he derives a logical rule. Mr. Kneale and I reject his logical rule, but our grounds for

rejecting it seem to be empirical. We say that it does not correspond to the way in which people actually use the word "freedom"; that people are in fact held morally responsible for actions for which they would not be responsible if Popper's rule held good. So it looks as if we are all engaged in a sociological investigation. But if we are engaged in a sociological investigation, our manner of proceeding is most unscientific. For we do not seriously set about collecting evidence. We seem content rather to take our own usage as standard. We imagine various situations and consider how we would describe them. This gives us a list of examples from which we extract a logical rule; and then we test the rule by trying it on further examples. The argument is thus to a certain extent *ad hominem*. A philosopher makes what seems to be a logical statement; he says that such and such an expression is equivalent to such and such another. You then ask him how he would describe a certain situation, assuming that he will describe it in the same way as you do yourself. And then you point out to him that, as he uses them in this instance, the two expressions are not equivalent. There is also the underlying assumption that the usage upon which you both agree is a standard usage. But this seems always to be taken for granted in philosophical discussions. It is a point that is never seriously investigated.

There is, however, another way of looking at the matter. In the case of free will, for example, a philosopher may come to think that the distinction which we draw between actions for which the agent is held responsible, and those for which he is not held responsible is unduly artificial. Why should we attach so much importance to the fact that the agent's choice is in some cases itself a causal factor, seeing that it may be possible to give a causal explanation of his choice in terms of some previous set of initial conditions, and that this process may eventually carry us beyond the series of his choices altogether? Now the critic who argues in this way wishes to emphasize the resemblance between the cases in which people are held accountable for their actions and those in which they are not. He does not deny that

there is a difference between the two types of cases, but he wishes to suggest that this difference is unimportant. It does not seem to him a sufficient ground for making moral judgments. Accordingly, he shifts the ground. He may, like Dr. Popper, make what seems to be the false empirical statement that we praise or blame people only in the cases where we are unable to explain why they act as they do. But this is not to be taken as a straightforward empirical statement. It is an encouragement to us to give up making moral judgments of this sort. The suggestion is that we should not be inclined to make them if we knew more of the facts ; and that therefore it is unreasonable for us to make them as it is. In short, the analysis is not descriptive but persuasive. I do not say that this is what Dr. Popper himself is doing, though it is a plausible interpretation of what he says. If it is his procedure, then I may point out that he too is an "apriorist", though not, so far as this goes, in any vicious sense.

Much the same questions arise in regard to the example which occupies the main part of Dr. Popper's paper, his analysis of causation. As he himself admits, this analysis is very sketchy ; but if he claims no more than that there are *some* cases in which it is correct to say of two events which are related in the manner he describes that one of them is the cause of the other, I think that he is right. The conditions that he mentions are not sufficient even in these cases ; but I dare say that he would not find it difficult to make the necessary amplifications. I think also that he has given a correct account of what is very often meant by "explanation". On the other hand I do not think that he is right in his view that explanation always takes the form that he describes. It seems to me that in history, and indeed in the field of human action generally, giving an explanation is very often not a matter of appealing to universal laws but rather a matter of telling more of the story. We are satisfied when the story takes on a familiar pattern ; and here Dr. Popper might reply that the reason why we are so satisfied is that it then comes to exemplify some universal law. But I do not think that this is true.

It is sufficient for us if the account that we are given describes one of the ways in which we should expect such things to happen, and we do not need to believe that they always happen so. I do not say that in such cases no universal laws are discoverable, but only that the knowledge of them is not essential to the process of explanation. Accordingly, if Dr. Popper means to describe our actual usage, I think that he is wrong to take the sort of explanation that occurs in physics as his only model. But here again it is not clear to me whether he means to describe how we do use a term or to prescribe how we should.

I disagree also with what he says about necessity. He claims that his analysis of causation enables him to solve "Hume's problem", but he seems to have an inaccurate conception of what Hume's problem was. To begin with it is a travesty of Hume's position to say that he made "an attempt at causally explaining away causality". What Hume was concerned to show was first, that from a proposition describing the occurrence of a particular event, considered by itself, it was not possible to deduce anything about the occurrence of any other event; secondly, that general propositions which affirmed the connection of two distinct events were not logically necessary; and thirdly, that such terms as "power" and "force", as applied to the relations between particular events, did not stand for anything observable. And with all these propositions I assume that Dr. Popper would agree. He would in any case be wrong if he did not. At the same time Hume thought that he had an idea of "necessary connexion", and since he believed that every idea must correspond to a previous impression, he set about looking for an impression from which this idea could be derived. He found it, as we all know, in the propensity of our minds to associate the ideas of objects which had frequently been experienced in conjunction. Now I do not think that Hume's account of the way in which we come by our alleged idea of "necessary connexion" is very convincing. The question is psychological; and I believe that a psychological investigation would show that the idea that people had of causal necessity

was very confused, and that it was partly derived from primitive experiences of pushing and pulling, and partly perhaps a relic of animism. But I do not press these suggestions. What I wish to point out is that Dr. Popper does not go into this question at all. For all the harsh things that he says about "apriorism", his method here is thoroughly aprioristic. What he does is to lay down a usage for the term "necessary connexion". He proposes that we shall say that E is necessarily connected with I when there is some well established law u , from which, in conjunction with the proposition i affirming the existence of I , a proposition e , affirming the existence of E , is formally deducible.

Now for my part I do not much care for this proposal. It seems to me to have the great demerit of reviving the confusion between logical and causal necessity which it was Hume's great achievement to have exposed. For the point is that e is not entailed by i . It is entailed by it only in conjunction with u . And u itself is not necessary. Dr. Popper says "we need not believe in the necessity, or even in the truth, of u to see that given u , e can be logically obtained from i "; and this is true. But if all that were required for E and I to be necessarily connected were that there was some premiss which in conjunction with i entailed e , then every event would be necessarily connected with every other. For it is always possible to find some proposition which will fulfil this purely formal condition. If there is to be a causal connection between I and E it is necessary not only that ui should entail e but that u should be true. If Dr. Popper is right, it is also requisite that we should have good reason to believe that u is true. Thus, as he himself recognizes, to assert that E and I are necessarily connected will be a way of expressing the strength of our belief in u . But we already express this by saying that E and I are causally connected. What do we gain then, on Dr. Popper's scheme, by saying that this connexion is necessary? In my opinion, only confusion can result from it.

On much the same grounds I object also to Mr. Kneale's proposal that we should regard u itself as the expression of a

necessary connexion. I am inclined to agree with him that the relation of material implication does not reflect our normal use of "if . . . then" as it occurs in variable hypotheticals. I find this use of "if . . . then" very difficult to analyse, and I can see that Mr. Kneale might wish to introduce some technical term to mark its peculiarity. But for obvious historical reasons, I do not think that "necessary connexion" is at all suitable for the purpose.

I have concentrated mainly on Dr. Popper's paper, and have not left myself time to deal with the many interesting points that are raised by Mr. Kneale's. But I should like just to refer to his proof that there are synthetic *a priori* propositions. As he defines his terms, the proof is valid; but for my taste his definition of "analytic" is too narrow. I should prefer to keep the term "analytic" for propositions "whose truth is guaranteed by the rules of usage of the symbols they contain", and use some other term, perhaps "tautological" to refer to the sub-class of analytic propositions which consists of those that are demonstrable within a given system. But I recognize that this notion of "being guaranteed by a rule of usage" needs rather more explanation than it has hitherto received.