

HUME AND THE PROBLEM OF PHILOSOPHICAL METHOD

by

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I

It is often said, especially in text-books of philosophy, that there was no difference between philosophy and science at first and that particular sciences got separated or even achieved their independence from philosophy one by one, as each could carve out for itself a given field of inquiry from the common body of philosophical problems.* In any case, it seems to me that if not properly interpreted, this contention can be more misleading than illuminating. First of all, it is not quite clear what is meant by getting *separated*. It would surely be wrong to construe this in the same sense as when within a given discipline more specific branches are formed, since this would imply that philosophy has no *specific* problems and methods for dealing with them. Although it is true that philosophers sometimes dealt with what we would *now* consider as *scientific* problems and also used scientific methods to answer them, (this is one of the reasons why we say that the word 'philosophy' is vague) it is also true that from the beginning philosophers discussed problems which it would be rather farfetched to characterize as scientific, at least in the sense in which the word 'scientific' is being used today. As a matter of fact, it has now become a common-place in the philosophy of science to say that a problem cannot be called "scientific" unless a universally accepted method for its solution is already at hand. And this seems to me to be the *main*, if not the only, reason why some problems are called "philosophical". Of course, there must be some reasons also why the so-called "philosophical" problems cannot be dealt with by uniform methods as in science, but it is a

* Russell's famous remark to the effect that science is what we know whereas philosophy is what we do not (yet) know can perhaps be interpreted in the same way.

fact that some problems yield to scientific methods whereas some others seem to be recalcitrant to a treatment by commonly agreed upon scientific procedures. This does not imply that there are no "unsolved" problems in science, surely not; we even know that there are some "undecidable theories" in mathematics.* But still it is commonly accepted by the scientific community that we at least know how to go about solving even the most abstruse problems, whereas for most philosophical problems it has not been possible to devise universally acceptable methods.

On the other hand, the "separation" of sciences from philosophy can also be understood as meaning a process of mere *specialization* inside philosophy. This would mean that no *difference of nature* existed between science and philosophy, but that the philosopher who was a universal scientist previously (the word 'philosophia' originally meant "love of knowledge" and this latter is a characteristic of any scientist) could no longer devote himself to all aspects of human curiosity, since even the greatest intelligence could no longer cope with all the innumerable problems it would be confronted with. So this second interpretation might have occurred to some, merely because no verbal distinction was actually made between the scientist and the philosopher during **the** greater part of our Western intellectual history, and it was perhaps made more plausible by the fact that both philosophical and what we would now call typically "scientific" problems were usually dealt with by men commonly called "philosophers".

Now, even though all this might have led some to assume that "scientific" problems are but a *sub-class* of philosophical problems, there being no difference of nature between them, such an assumption seems to be mistaken, since if it were true, it would be difficult to account for the unmistakable historical fact that, whereas science *progresses* at an ever increasing pace, philosophy seems to be going round the same perennial problems without being

* There are indeed many mathematical theorems to which the answers are not known, (such as Fermat's last theorem or Goldbach's conjecture) yet we know, when an answer is proposed, how to set about deciding whether it is acceptable. To be acceptable it must be proved by well-known deductive procedures used by mathematicians. We have also some well-defined empirical methods in accordance with which the acceptability of a hypothesis or theory can be decided. Driesch's theory of *entelechy*, for example, which he put forward to explain such biological processes as regeneration and reproduction is rejected by most scientists and philosophers on the ground that it cannot be ascertained by accepted scientific decision-procedures. Of course, there are also in science (especially in mathematics and logic) the so-called *undecidable theories* which bring another complication into the matter.

able to offer any uniform solutions that can be accepted as satisfactory (if not definitive) by the majority of enlightened people. Instead of uniform solutions we have only theories, doctrines and conceptions which are eternally at war with each other.

As a matter of fact, the ambiguity of the word 'philosophy' might have been responsible to some degree for the above assumption, but if we look closely at the history of human knowledge we will see that there have always been what we may now call *pure scientists* (men such as Euclides, Aristarchus and others in antiquity, and Copernicus, Huygens, etc. in modern times) as there were also *philosopher-scientists* (men such as Democritus and Aristotle in ancient times, Descartes, Leibniz, etc. in modern times) and *pure philosophers* (such as Parmenides, Socrates, Spinoza, etc. to mention but a few of them), even though it would still be true to say that up to the 17th century neither philosophers nor scientists seem to have given any serious thought to the nature of their problems or to the general question whether all problems can be solved by the same methods. In any case, the possibility of there being fundamentally different *kinds* of problems, each requiring a different method of solution does not seem to have occurred to most of them. This again was perhaps due to their being unaware of the fact that the apparently univocal term 'philosophy' may be hiding in itself some rather unsuspected ambiguities.

Some awareness of this possibility was surely (if dimly) felt by some modern philosophers like Descartes even though they might not have made a radical distinction between scientific and philosophical problems. Descartes himself was a philosopher-scientist, and it is most probable that he knew when he was being a philosopher, when a scientist. There is no doubt, for instance, that he was well aware of the fact that he wrote "Discours de la Methode" and "Meditations" as a philosopher, while he was being a pure scientist when writing his books on analytic geometry, etc. On the other hand, men like Bacon and Hobbes certainly knew that they were not scientists as a Huygens or a Newton was a scientist, and the great Newton himself knew quite well that he wrote his "Principia Mathematica philosophiae naturalis" as a contribution to *science*, even though the word 'philosophy' occurs in the title. As a matter of fact, the qualifying adjective 'natural' annexed to 'philosophy' was surely meant to make it explicit that the work was about what we nowadays call "physics". The distinction between philosophy and science was made by using the term 'natural philosophy' to denote the latter, whereas the term 'moral philosophy' was used for the former. So we see that a distinction was

actually made between the two, even though it was not considered as a difference of nature; between *natural* and *moral* philosophy only a difference of subject-matter was believed to exist. That philosophy might treat of problems *sui generis* and hence have to use quite different *methods* than those used to deal with scientific ones did not even occur to Hume, who considered philosophy as another branch of science.

II

After these introductory remarks, I wish to defend the thesis that there are *specific* philosophical problems and I think that perhaps the most efficient way of supporting such a thesis would be to show that no philosopher, *qua* philosopher, can consistently hold that there cannot be "philosophical" problems besides "scientific" ones. And for this purpose I intend to take Hume as the main target of my criticisms, since he seems to be the first and the most outspoken protagonist of such a view, as his famous (and for some "infamous") invective against traditional metaphysics makes quite clear. In fact the idea that all knowledge must be "scientific" so that there can be no such a thing as "philosophical" knowledge finds its most categoric expression in the last paragraph of Hume's *Enquiry concerning Human Understanding*:

"When we run over libraries, persuaded of these principles, what havoc must we make? If we take in our hand any volume, of divinity or school-metaphysics, for instance; let us ask, Does it contain any *abstract reasoning* concerning quantity or number? No. Does it contain any *experimental reasoning* concerning matter of fact or existence? No. Commit it then to the flames: for it can contain nothing but *sophistry* and *illusion*."* (Italics mine.)

A modern disciple of Hume's, Prof. Ayer, comments on the above passage in his cryptic way as follows:

"What is this but a rhetorical version of our own thesis that a sentence that does not express either a formally true proposition or an empirical hypothesis is devoid of significance." **

Now, even though Hume is against what he disparagingly calls "school-metaphysics", he is far from equating "philosophy" with metaphysics. Un-

* David Hume, *An Enquiry concerning Human Understanding*, Selby-Bigge edition, p. 165.

** A. J. Ayer, *Language, Truth and Logic*, 2d Ed., 1950, p. 54.

like Ayer, he is not even accusing traditional metaphysics of being non-scientific, but rather of using the "wrong" scientific method to solve philosophical problems. According to him, these also ought to be dealt with by "experimental" methods just like other empirical problems. The sub-title of his *Treatise* is quite revelatory on this point: "An Attempt to introduce the *experimental Method of Reasoning* into Moral Subjects", the word 'moral' here meaning "philosophical". As a matter of fact, for Hume philosophy is an *empirical* study of "human understanding". Representing his Own work in philosophy as a "science of man", he undertakes to chart the "mental geography" of man, or to "anatomize human nature in a regular manner" on the basis of "experience and observation." According to this view, philosophy is but a sub-class of *empirical science*, differing from other branches of science in its subject-matter and also in having the "peculiar disadvantage" of not being subject to *experimental control*, as compared with "physical" sciences, and this fact can obviously be said to constitute the fundamental characteristics distinguishing "philosophical" (or "moral") problems from among the other "empirical" problems. But the point is that for Hume philosophy is (or ought to be considered as) another branch of "empirical" science. As we mentioned above, Hume's main objection to traditional metaphysics was its rationalistic illusion that one can achieve knowledge concerning matters of fact by *a priori* theorizing. And his own constructive purpose was to replace this wrongheaded metaphysics by a "true metaphysics."*

Hume's conviction that philosophy can be a legitimate and worth-while activity if only conducted with *empirical* methods seems to me to be just the reverse of the mistake fallen into by his rationalist opponents since it shares with them the illusion that philosophy is just another *science*, his own disagreement with them concerning the *proper* scientific method to be used when dealing with "moral" matters. He was surely right in holding that it was a hopeless undertaking to try to achieve knowledge about the world by demonstration from *a priori* principles (just think of Spinoza's futile attempts to derive truths about matters of fact *more geometrico!*), but does this fact by itself justify Hume in introducing his "experimental method" into philosophical investigations? He would be justified in this if and only if it was generally agreed that the subject matter of philosophy is the world of facts and nothing else, but is this really so? Even assuming for a moment that it is so, is it also

* cf. *An Enquiry*, Sect. I, 7; p. 12 (Selby-Biggy ed. Oxford, 1961)

the case that Hume's own philosophizing is faithful to his own methodological principles? I don't think it is; on the contrary, I believe that his typically philosophical arguments are neither *a priori* nor *a posteriori* (empirical) and that his greatest and most important contributions to philosophy are of a wholly different nature than what we would call "scientific". Hume is in fact a *typical* philosopher and he is not in the least to be blamed for that, nor ought anyone to think that his own works should be committed to the flames simply because they contain neither abstract (*a priori*) nor experimental (*a posteriori*) reasoning. They all deserve our deepest admiration for some very important *philosophical* insights they contain.*

The tentative conclusion from the above discussion is that Hume, as so many other philosophers before and after him, *misconceived* the nature of philosophical inquiries, at least as he himself conducted them. To substantiate this contention I will submit to a critical examination some of his philosophical theses and then try to show whether he himself was using empirical methods to support them, as is usual in other branches of empirical science.

To take first a rather simple example, consider what he says about *meaningfulness*. Well, to put it roughly he says that to be meaningful, a term *must* have an idea annexed to it in the mind, otherwise it is meaningless.** Now, is this assertion of Hume's a *scientific* statement or not? To be scientific it must be decidable by scientific methods, as he himself enjoins. But can this statement be so decided to be true or false? Certainly not, at least not as it stands, since the *modal* (deontic) term 'must' occurs in it. So let us try to give it an extensional turn and then see whether in this new form it can be so decided: 'There is no meaningful term in language which is not accompanied by a mental image of some sort'. Is this latter statement scientifically decidable?

* Although they also contain some scientific remarks, what arouses our admiration is certainly their "philosophical" insights.

** In "*An Abstract of a Treatise of Human Nature*" Hume expresses this principle in a more explicit form:

"When he (our author) suspects that any philosophical term has no idea annexed to it (as is too common), he always asks *from what impression that idea is derived?* And if no impression can be produced, he concludes that the term is altogether insignificant".

One of the most impressive applications of this principle is what Hume says about the idea of causal necessity. He searched his mind for an impression or set of impressions from which this idea could be derived, but without success. So he concluded that our idea of causal necessity was not a genuine idea derived from any sensory impression, but from a spurious inner feeling caused by custom and habit.

Now, to be so decidable, it must either be provable or else confirmable by empirical methods. Is this, then, an *analytic* statement, a statement, that is, whose truth can be ascertained by purely logical means? Certainly not, and Hume himself would vigorously object to its being construed as such, since analytically true statements are trivial. There remains the other alternative: according to his own conception of philosophy, it can only be an *empirical* statement. But since it is a universal proposition, it must either be an empirical generalization, or else a theory. And in either case it must be possible to use it to make predictions which will either come out true or false. But can this statement be so used?

Well, before answering this question, let us see how people generally react towards an empirical law or theory. Suppose a physicist puts forward a new theory, according to which every piece of iron exposed to a bombardment of gamma-rays for longer than 10 seconds will contract 1/100 th of its length under otherwise normal conditions. Now, we may wonder whether this is true and might even undertake experiments to see whether it is in fact true. Suppose, all the experiments we have carried out to verify the theory gave a negative result. Then we would conclude that it was false or at least disconfirmed.

The question now is: Would we also be ready to react in the same way to Hume's theory concerning meaningfulness? It is obvious that some of us would want to reject it out of hand, refusing to apply his introspective method on the ground that no sane person would be using meaningless expressions or that in the most usual sense of 'meaningful' any term used in language must thereby be meaningful. As a matter of fact, we might even suspect that Hume's statement, in spite of its grammatical form, does not really express a *proposition*, but a *proposal* as to how we *should* construe the term 'meaningful', or, to put it in a different mode of speech, what expressions we should consider as meaningful. So they would reject Hume's assertion by saying that it was really a new *criterion* of meaning disguised as a scientific *theory*.

But suppose, for the sake of argument, that there are other people who even if few in number, are willing to apply Hume's method and that, after very many introspective observations to see whether, as Hume held, every meaningful term they were using had an idea annexed to it or not, find out that this is not always the case. What would they then conclude from this? Even supposing that some among these experimentally-minded fellows were candid enough to conclude that they were using some meaningless expressions

all the time without being aware of it, thus admitting Hume's theory, some others most likely would say that Hume's theory had been *falsified*. Now, one might think that this is usually what happens to any *scientific* theory and that, consequently, both conclusions would prove that Hume's theory of meaningfulness was scientific. But can we say at this stage that Hume's theory was *really* scientific? I don't think we can. To decide the issue we must also ask ourselves what would Hume's own reaction to the falsification thesis be? In other words, would he also admit that his theory had been falsified? I don't think he would, since he knew quite well that many terms we use in ordinary discourse, terms such as 'God' and 'immortality', for example, are meaningful by *common standards*, since they are being used by everyone and nobody would use an expression which he knew to be meaningless.* But Hume believed that we may be using meaningless expressions without knowing it, that is, *mistakenly* and the point of his theory of meaning was to make people aware of such mistaken uses. In other words, he hoped that his criterion of meaningfulness would help them to get rid of the habit of using meaningless expressions unawares.

Now, we have seen that it is possible to *falsify* (read: *disconfirm*) an empirical theory by finding counter-instances; could Hume also show his recalcitrant interlocutors that they were *mistaken* in using some expressions by producing *empirical* evidence? What kind of an empirical evidence could he possibly produce? It seems to me that he would be obliged to look for another way to persuade them of the meaninglessness of some of their expressions. He might, for instance, argue in some such way as this: "Look, terms such as 'house', 'father', 'white' or even 'three' are meaningful in that they refer to things or qualities we can point at, or else to operations we can execute, and all these terms are found to have an idea annexed to them in our mind. So *all* meaningful expressions must be of this kind." To this his opponents would answer by saying that such a generalization is illegitimate, since even if it be

* That Hume would actually refuse to accept that his theory was falsified by a counter-example can be substantiated by what Hume himself says concerning another methodological maxim of his, namely that we cannot have any idea in our minds that was not derived from a previous impression.

Both in *Treatise* and *Enquiry* Hume asks whether we could have an idea of a particular shade if we had had impressions only of the shades bracketing it on a colour chart. He says that we could, admits this as an authentic exception to his rule, but interestingly enough, does not take it seriously: "This instance is so singular that it is scarcely worth our observing and *does not merit that for it alone we should alter our general maxim.*" (cf. *Enquiry*, II, p. 30; italics mine.)

granted that *most* of our words are indeed meaningful in the way described by Hume, it does not logically follow that *all* of them must be meaningful in the same way. It may well be that there are different ways of being meaningful, they would argue, and would even want to confirm this by saying that, although meaningful, terms such as 'and', 'or' and 'some' do not seem to have any idea annexed to them. Why should it, then, be required, they would ask, that words such as 'God' and 'immortality' be meaningful in the same way as 'house', 'white', etc. are meaningful?

Hume might try to counter such objections by another method and say something like this: "Well, suppose you are right. But what reasons do *you* have to insist that the term 'God', for instance, *is* meaningful? Isn't it because you hold some statements in which it occurs to be true? But surely we believe very many statements to be true, even though they are false. How do you know that your statements about God *are* true? Can you show me any *reliable* procedure by means of which we may come to know that they are true? You can neither use formal nor empirical methods to substantiate them, so your statements about God cannot be known to be true or false, hence they are meaningless".

To this the other side can reply in this way: "One sees that you are an incorrigible atheist. This is quite alright as long as you keep it to yourself and do not try to prove that statements about God can never be *known* to be true. If we admit two methods only as *reliable* methods of coming to know any statement, then you are right. But who says and how can you prove that these are the only reliable methods of coming to know the truth-value of a statement? Why shouldn't there be other such methods? What about *intuition*, for example? Or *religious experience*?"

I do not think that to this Hume would be able to give a scientific answer, an answer that is, the truth or falsity of which could be ascertained by what he himself considers as *legitimate* (read: *scientific*) methods of decision. In other words, he could neither prove deductively nor confirm inductively that there can be no other reliable (or legitimate) ways of knowing than the formal and empirical ones. But, of course, this does not mean that Hume could not produce a different kind of argument to support his thesis; he might, for instance, say that only these two methods have thus far been *successful* in providing us with reliable pieces of knowledge. But again, such an argument would not be a scientific argument, but merely a *pragmatical* one, and some of his opponents might even want to dispute its pragmatical superiority, by

giving a quite different interpretation of the term 'successful', by saying, for example, as some Pragmatists have actually said, that the acceptance of some statements about God as true may have a very useful psychological function, etc. But in any case it is clear that the dispute would now turn around the *practical* question as to which *criteria* or *standards of excellence* one ought to adopt.

Perhaps I am already anticipating myself. So let me take another example in order to see whether Hume is actually abiding by his own methodological principles. For this purpose I propose to consider what Hume has to say about knowledge. I think this example will show Hume's inconsistency with his own principles more convincingly than any other that I might have chosen instead.

As I already intimated above, Hume seems at first to assume that there must at least be two kinds of knowledge, those concerning "relations of ideas" on the one hand, and those relating to "matters of fact" on the other. And since his aim is to revolutionize "moral philosophy" itself by introducing into it the well-tried "experimental" procedures of "natural philosophy", establishing thereby "a complete system of the *sciences*", one might naturally suppose that he must be attaching a fundamental importance to "empirical" knowledge. He does so in a way, but after having first distinguished two *kinds* of knowledge, thus confirming the reader's belief that in Hume's view, as in his, both kinds, as being kinds of *knowledge*, are knowledge in some truly fundamental sense and that in this respect he was just following the general climate of opinion of his age, he bewilders us by his later contention to the effect that what people usually take for knowledge concerning any matter of fact is but "mere *belief*" and hence ought to be carefully distinguished from "genuine" knowledge. Our bewilderment is even more increased when we hear him declare roundly that "reason is but a wonderful and unintelligible *instinct* in our souls".*

I will not labor this last point which seems to be diametrically opposed to our Common-Sense conception of reason; I find it remarkable —not to say paradoxical— enough that Hume should consider even the most firmly established pieces of empirical knowledge just as a subset of "belief".

I find Hume's behavior remarkable on two counts: firstly because, by acting as he does, he seems to run counter to the spirit of his allegedly *experi-*

* cf. *Treatise*, I, iii, 16 (s. 179).

mental methods; and secondly because, though a sworn "empiricist", he seems to follow in the footsteps of the most extreme of "rationalists", namely Plato.

Let me explain. To take the second point first: Plato was the first philosopher to make a *radical* distinction between *knowledge* (episteme) and mere *belief* or opinion (doxa). Hume takes this epistemological view over from Locke, another empiricist philosopher, who had taken it over from Plato. According to this rationalist tradition, there are two types or kinds of assurance, the one absolute, the other relative, and it is only the first kind of assurance that entitles us to say that we *know* something. In other words, what we know, we know with absolute certainty, and so long as there is even the bare possibility of error, knowledge is absent. What differentiates knowledge from mere *belief* (doxa) or *opinion* is that its opposite is not even conceivable. If, for instance, we know that $2 \times 2 = 4$, then it can never be false, because the denial of it would involve us in self-contradiction. It would even be meaningless to talk of *believing* such a proposition. Belief, on the other hand, is not only the most fundamental factor in the sphere of opinion, but in this sphere we can never go *beyond* belief, however well supported it may be, and this holds for *any* opinion concerning "matters of fact," because our assurance in such matters can never be *complete*, that is, exclude the possibility of error. And wherever the slightest possibility of error remains, we cannot legitimately claim to know anything. Thus our alleged "knowledge" of matters of fact is *really* opinion; it is only our epistemological naivete that misleads us into believing that we have *knowledge* when what we really have is more or less probable *opinion* only. This is so even in the case of Newtonian physics, that paradigm of empirical knowledge.

I do not intend to quarrel with Hume on the ground that even if he was not willing to commit to the flames scientific works simply because they did not provide us with genuine knowledge, but mere opinion, such a Platonic view might sound rather disparaging for modern science; nor do I wish to take issue with him on the ground that, as some modern philosophers would put it, Hume's view is in blatant contradiction with the most common and hence correct use of the verb 'to know'. For anyone who understands Hume's intentions well it is quite obvious that he was neither contemptuous of modern science (was not his own ambition to become the Newton* of the "moral"

* In fact Hume had at least as great an admiration for Newton as Locke, whom he characterized as "the greatest and rarest genius that ever rose for the ornament and instruction of the species." (cf. *History of England*.)

science?) nor was he unaware of the fact that the word 'knowledge' had a much looser and more generous use in both everyday and scientific language. He was never for restricting the common use of 'know' and its cognates, since he himself made ample use of them even in contexts where, according to his stricter logical criteria, 'opinion' or 'belief ought to be used. He knew quite well that people have their age-old linguistic habits and that it would be of no use to want to change them; so they may go on saying things like 'He *knew* that he would die soon' instead of saying 'He strongly *believed* that he would die soon'. He also knew that a well-supported opinion, that is, one that was "beyond reasonable doubt" could well pass for knowledge for all *practical* purposes, although from a purely *theoretical* (read: *logical*) point of view it cannot constitute a piece of genuine knowledge.

The word 'logical' seems to me to be crucial to explain the paradox in Hume's rationalism. His intention seems to be to draw our attention to the following logical feature of our alleged knowledge concerning "matters of fact": That the evidence in support of such knowledge can never be complete, and that consequently, the possibility of error can never be completely precluded. In view of this situation, Hume felt that we ought not to be too optimistic about our achievements in the field of empirical knowledge, keeping constantly in mind the possibility of a future revision in this field and that, consequently, a rather *sceptical* attitude is to be cultivated and encouraged if we have an interest in avoiding the pitfalls of *dogmatism* in science. He knew that we would go on using the word 'knowledge' for very many of our Common Sense opinions as well as for our well established scientific hypotheses, and that we ought not to be blamed for that, but he also wished us to be modest in our claims by keeping in mind that these opinions and hypotheses are devoid of "absolute certainty" and hence "fallible". Even though he seemed to give at least lip-service to traditional rationalism by restricting the use of the epithet of *knowledge* to "logically indubitable propositions", Hume's rationalism differed from that of his predecessors in that he vigorously denied that our knowledge of "matters of fact" can ever possess the same certainty as logically true propositions. He felt as though he could better bring home this point by saying that we drop the epithet of knowledge altogether from *any* opinion we might hold concerning matters of fact.

I should think that this is a judicious evaluation of Hume's intentions; still I cannot refrain from asking why it is that Hume did not choose another course in this matter, thus protecting himself from any suspicion of rational-

istic allegiance. Indeed, wouldn't it be much more in keeping with his basic empiricism to declare that there are two kinds of *knowledge*, one *a priori*, the other empirical, but that whereas the first (and only the first) is absolutely certain or indubitable, the other can only be probable? This is actually the course taken by his modern successors, the logical empiricists. Why is it that Hume himself was not as generous as his modern followers in characterizing as "knowledge" our well-established empirical opinions?

I think at this juncture I can take up the first controversial point, namely the question whether Hume actually ran counter to the spirit of his allegedly experimental methods or not.

Now, according to Hume's own assumption, what he says about knowledge and belief must express a *factual* claim; in other words, he was putting forward an *empirical* hypothesis. But can we say that it is really so? If it is actually a factual claim, as Hume assumes, then it must be capable of verification by empirical methods, that is, either by observation or experiment. In the present case only observation, more specifically, public-opinion polls could be resorted to. But would Hume himself be ready to accept the results of such a procedure? I don't think he would, since he knew quite well that his own *conception* of knowledge was diametrically opposed to both Common Sense and Scientific views, and the whole point of his *philosophical* conception was to show that these latter views were really mistaken. In other words, his real aim was to cure both the ordinary man and the scientist from their "epistemological naivete", as I have already intimated. To take a concrete example, he found nothing wrong with such expressions as 'He *knew* that he would die soon' or 'We all *know* that the sun will rise to-morrow' as used by people in their non-philosophical moments; and he knew beforehand that a survey about the question whether anybody *can* know that he is going to die soon or whether we *actually* know that the sun will rise to-morrow would give an unmistakably positive result. But the interesting point is that this would in no way shake Hume's confidence in the truth of his own conception of knowledge; quite the contrary, for him this would merely show how widespread *a false* belief can be. So nothing could shake his conviction that, even if universally shared, this belief *was* false and the whole point of his *philosophical* efforts concerning the notion of knowledge was to uproot this firmly established belief among ordinary people as well as among scientists. To cut a long story short, from the very start Hume seems to take it for granted that no empirical investigations could ever falsify his own thesis. But, then, can we still say

that his thesis was *factual*, as Hume assumed it to be? What kind of a factual claim is it that seems to be immune against all attempts at falsification? Thus we are tempted to conclude from this that Hume's own thesis was not *scientific*, but *philosophical* or even *metaphysical*, to use a disparaging term, since it does not seem to be capable of empirical verification. We might even wonder at this point whether we ought to follow Hume's injunction concerning the fate of *metaphysical* views.

I believe, however, that we ought to resist this temptation for a while yet, since the mere fact that Hume himself would in no way desist from his thesis is not sufficient to prove that the thesis was not factual; after all, there are so many *dogmatic* scientists.* Thus the crucial problem still remains: How can we distinguish *philosophical* from *scientific* problems?

It would surely be preposterous to think that Hume was unaware of a *difference* between problems dealt with by scientists and those that usually occupy philosophers. He surely knew that the problem of *causality*, for instance, or of *induction* for that matter, was somehow different from the standard problems dealt with by scientists ("*natural* philosophers", to use the jargon of Hume's times). Indeed he could not ignore the obvious fact that scientists *presupposed* causality as an ontological principle just as they assumed induction as a methodological one, and that *as a philosopher* (as "a *moral*" philosopher), he himself was dealing with the presuppositions or assumptions of our commonsense and scientific thinking. To put it in another way, he certainly knew that *philosophical* problems differed from the scientific ones in that they were not directly related to "matters of fact", but to various assumptions (principles, rules, criteria, etc.) underlying our dealings with matters of fact. Still he believed that *these* problems, (whether you call them meta-problems or *second-order* problems does not matter much) can or even must be dealt with by using the same *empirical* methods used for the solution of problems directly related to matters of fact. He does not indeed seem to have realized that problems of a different nature require different methods and that, conversely, if different methods seem to be required for the solution of certain problems, then we ought to suspect that these must be of a different *nature*. There must have been various reasons for this: one reason was that **Hume** was deeply impressed with the brilliant successes of Newtonian physics; and

* If we have to believe what Thomas Kuhn says in his "*The Structure of Scientific Revolutions*", no revolutionary theory in science will be accepted by the followers of "established" science.

then some problems such as that of *causality* might have easily persuaded him of the possibility of applying observational (introspective) methods to philosophy, though it is rather doubtful whether even in this specific case such methods were of much use; still other explanations can be found. Yet it remains a real puzzle for me how Hume could not perceive that his solution of the problem of *induction*, for instance, though quite plausible or even convincing in itself, had nothing experimental about it? The same can be said of the most important philosophical problems Hume ever cared to inquire into.

III

Controversies in philosophy are often, if not always, due to misunderstandings, and I wonder now whether the accusation of self-inconsistency I have been levelling at Hume all along was not also due to such a misunderstanding, occasioned perhaps by too narrow a construal of his concept of "experimental reasoning". It may in fact well be the case that, as used by Hume, this concept included more than we would be inclined to concede to it after two hundred years of refinement of our scientific and/or philosophical terminology. There is actually some evidence in Hume's philosophical work in favor of this possibility: one may indeed think that his theory of the association of ideas, for instance, is as good a *scientific* (empirical) theory as any other in psychology. Still other such examples could be produced in support of the thesis that Hume's own philosophizing was in fact *experimental*, if the term 'experimental' be understood in a broader sense than it usually is. And it might be that it was for this reason that Hume was inclined to *identify* philosophy with psychology, assuming that any meaningful problem can or even must be dealt with by empirical methods. But the problem is whether Hume was *justified* in this identification. To put it in another way: Was Hume justified in using the term 'experimental' as broadly as he would wish to use it? To avoid any suspicion of Platonistic assumptions in this form of question, I might better reformulate it in the following way: Was there any internal inconsistency in Hume's use of the term 'experimental'?

I think that there was. I quite agree with Flew's remark that "experiments in his book are not what would rate as such with scientists. 'Experimental' can be taken as equivalent to 'experiential.'"* But the important

* A. Flew, "Hume" in *A Critical History of Western Philosophy*, p. 260 (Ed. by O'Conner).

point is that most of Hume's disquisitions are far from "experimental" even in this qualified sense. In fact, Hume might have been right in thinking that, in some sense of the word 'experimental',* he was using the experimental method when dealing with the problem of *causality*, but can we say the same of his way of tackling the problem of *induction*? To what kind of *experiment* is he appealing when he is trying to convince us that no universal law can ever be deduced from any evidence provided by experience? Isn't it rather obvious that his argument here is a *logical* one, since, instead of advising us to undertake certain experiments (observation) he tries to show us that there will always be a *logical gap* between the limited evidential basis for an inductive conclusion and the conclusion itself, that, in other words, we have no logical right to derive a universal conclusion from a limited number of cases? I don't think that Hume's arguments concerning the existence of material objects, of other minds, etc. are very much unlike his argument about induction. As a matter of fact, I am sure that if someone rejected any of these arguments on an allegedly experimental basis, saying for instance (as Moore actually said), that we all *see* physical objects and that for this reason there can be no doubt that they exist, Hume could only reply by repeating his argument until his objector could see the *logical* point he was trying to make, namely, that from the existence of our own perceptions we have no logical right to infer to the existence of objects that are supposed to exist independently from us and also to constitute the very substance from which we get our perceptions. Everybody would agree that both a *physicalist* and a *phenomenalist* have the same sense-experiences or make the same observations, and that, since this is so, there must be other than "experimental" reasons for their "ontological" disagreement.

It is interesting to note that for Hume it cannot "be *proved* that the perceptions of the mind must be caused by external objects"*** so that what are called "ontological" problems are really *factual* problems. This is what he says: "It is a *question of fact* whether the perceptions of the senses be produced by external objects resembling them" though "here *experience is and must be entirely silent*" for "the mind has never anything present to it but the perceptions."*** Isn't it rather puzzling that Hume should admit the existence of

* The term 'introspective' or 'reflective' would better suit to characterize Hume's philosophical method.

** Cf. *Enquiry*, p. 161. My italics.

*** *Enquiry*, p. 169, 170. My italics.

facts that, though *undeniable*, can neither be *proved* logically nor *substantiated* experimentally? And, moreover, isn't it astonishing that, in spite of this paradoxical situation, he should not suspect in such cases the existence of typically philosophical problems?

It is again quite interesting to note that it was Hume's modern followers, the so-called *logical positivists*, who opposed his conception of philosophy in most clear and explicit terms, although they completely agreed with his dichotomous classification of human knowledge and consequently, of "meaningful" statements. Wittgenstein is in fact unequivocal in his contention that "philosophy is *not* one of the natural sciences." Here one immediately thinks of the other alternative: that philosophy is perhaps a branch of logic? This was also denied by Wittgenstein, for this would endow philosophical statements with logical necessity while emptying them of any factual content. The logical consequence of this was quite clear to Wittgenstein: that philosophical statements must be declared to be meaningless. This consequence was actually drawn by Wittgenstein himself, but not quite wholeheartedly, since in spite of his methodological principle he felt that he had to grant to philosophy some legitimate function. Unlike science, philosophy was not a *theory*, but an *activity*; as he put it, "the activity of clarification." The same idea was first taken over enthusiastically by philosophers like Carnap and Ayer, but later experienced a complete transformation in their hands:

"Thus, philosophy is no longer viewed as a domain of knowledge in its own right, on a par with, or superior to, empirical sciences... Logic is no longer one philosophical doctrine among others, but we are able to say outright: *Logic is the method of philosophizing.*"*

This means identification of philosophy with logic.

Ayer is a little bit more explicit in this respect:

"The propositions of philosophy are not factual, but *linguistic* in character. . . . they express definitions, or are formal consequences of definitions. Accordingly, we may say that *philosophy is a department of logic*. For the characteristic mark of a purely logical inquiry is that it is concerned with the formal consequences of our definitions and not with questions of empirical fact."**

* R. Carnap: "The Old and The New Logic" in *Logical Positivism*, ed. A. J. Ayer, 1959, p. 133. My italics.

** A. J. Ayer: *Language, Truth and Logic*, p. 57. My italics.

Isn't it rather surprizing that these modern followers of Hume's should propound a conception of philosophy that is diametrically opposed to that of their positivistic master? And how are we to explain this puzzling situation?

The most plausible explanation I can think of is this: Both Hume and his modern disciples were deeply impressed by the achievements of modern science and they felt that the reason for this must lie in the efficiency of the methods they use. From this they concluded that philosophy can also hope to be successful only if it adopts a *scientific* method. Hume thought that this could only be the *experimental* methods of empirical sciences, not only because he hoped to achieve in "moral" philosophy what Newton had achieved in the "natural", but also because he was well aware that one could not apply to philosophical problems the *a priori methods of mathematics* which, according to him, dealt only with "quantity and number". Moreover, we must not forget that in Hume's time logic as we have it now was inexistent, so that even with the greatest stretching of his imagination he could not think of philosophy as a branch of logic. In any case he felt that philosophy ought to deal with (substantial) "matters of fact" relating to human nature and not merely with (empty) "relations of ideas". In his enthusiasm for a Newtonian reconstruction of philosophy he overlooked the fact that certain philosophical problems he was dealing with were quite unlike standard problems of empirical science, and hence cannot be dealt with by the experimental methods of these sciences. This oversight is somehow understandable and excusable, since in Hume's day scientists themselves did not have a very clear idea of the nature of their methods. Even the great Newton himself is no exception in this respect, for certain of his pronouncements concerning method do not seem to correspond exactly to what he was actually doing. And we must not forget that even today methodological issues are far from having been clarified and agreed upon to the satisfaction of all. It may even be that science continuously refines or even changes its methods as it progresses, so that it would be a mistake to assume that it has some clear-cut methods that are complete and established once for all.

This partly explains, I should think, why Hume believed that "moral matters" must be capable of treatment by empirical methods. As a matter of fact, some of the problems he discussed were more "psychological" than philosophical. His theory of the association of ideas, for instance, can well be considered as the beginning of an empirical psychology. I am even inclined to think that this is perhaps one of the most typical instances of the so-called

"separation of the sciences from philosophy". We may certainly find theories of an empirical character in the work of any philosopher. But I still think that to infer from this that *all* philosophical problems must be capable of an empirical treatment or that philosophy as such can be reconstructed as a science is to commit the fallacy of *non sequitur*. In actual fact, the majority of the problems discussed by Hume in his *Treatise* and *Enquiry* proved themselves recalcitrant until today to treatment by empirical methods. After all, there must be some reason why we still call them "philosophical" problems.

This point was well understood by people like Carnap and Ayer. They agreed that philosophical problems must be distinguished from *empirical* ones since no empirical *methods* can be used for their solution. And if they are not empirical, but still meaningful, they argued, then they can be but "logical". Thus a *radical* shift of perspective in looking at philosophy took place in modern times. One wonders why.

Two considerations might have been responsible for this shift. One was the suspicion that, had they been really empirical, philosophical problems would have been solved long ago, as so many other empirical problems. Of course Hume himself believed that he had solved many of them, but as it turned out later, his proposed solutions could not achieve universal acceptance, as is expected of any empirical solution. Not only did philosophical problems prove "perennial" once again, in spite of Hume's optimistic attempts, but philosophy seemed to fare worse than ever to these positivistically minded philosophers who felt rather depressed by its eternal stagnancy while empirical sciences were progressing continually.

Another consideration was this: Philosophical activity seemed more akin to logical activity in being purely "reflective". In fact, no philosopher as such seemed to have any interest in resorting to observation or experiment as is usual in empirical sciences; just like logicians they conducted their inquiries in the loneliness of their studies by mere thinking. And the interesting point is that not only would they not resort to any observation or experiment, but unlike scientists, they had also the curious habit of clinging obstinately to their own theories even in the face of the most telling counter-instances.

There might have been still other considerations that induced logical positivists into identifying philosophy with logic, but it is again interesting to note that they were ready to identify with logic only one part of philosophy and not the whole of it. They must have felt that, however much one may want to extend the scope of logic, it would really be farfetched to consider all

traditional problems of philosophy as belonging to logic. So they had to resort to the trick first used by Hume, namely, first to distinguish between philosophy *proper* and *metaphysics* and then to declare that those among traditional problems that proved recalcitrant to treatment by purely logical means were really pseudo-problems and hence to be discarded. This was a rather cavalier way of disposing of difficulties, but, as we know, these new attempts at *dissolving* philosophical problems instead of *solving* them failed as disastrously as the attempt Hume's. After a short interval of jubilation in the positivistic camp all the problems of traditional metaphysics declared "meaningless" by them came rushing back, as soon as the positivistic criterion of "meaningfulness" itself turned out to be not less problematical than any problem of metaphysics. This time people like Carnap tried to dispose of them by declaring them "practical"; still others by considering them as "intellectual puzzles" to be cured by "linguistic analysis" and so on. The remarkable thing is that all the traditional problems of philosophy proved themselves immune against all these attempts at dissolution and continue to be as interesting and as challenging as ever. So the problem of philosophical method is still with us, not only what concerns the so-called "metaphysical" problems, but also with respect to those philosophical problems considered worthy of either "logical" or "linguistic" analysis. Is the method of philosophy really "logical" or "linguistic" and, if so, in what sense? This is a rather difficult question which I hope to take up in another paper.