A Defence of Empiricism

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I am very much honoured to have been asked to make the closing speech at this Conference.[1] Since this is the first time for over fifty years that a philosophical congress of this scope has been held in England, I hope that you will think it suitable for me to devote my lecture to the revival of the empiricist tradition in British philosophy during this century. I shall begin by examining the contribution of the Cambridge philosopher G. E. Moore. Though he first owed his fame to his book *Principia Ethica* (Moore, 1903) regarded as a work of genius by the Cambridge Apostles and their associates in Bloomsbury, who did not venture to question Moore's mistaken view of 'good' as an unanalysable non-natural quality, his reputation now chiefly rests on his subsequent defence of common sense.

The core of Moore's defence of common sense was that he knew the truth of a huge number of propositions of kinds that we all accept without question in the course of our everyday concerns, such as that I am standing in a room with walls and a ceiling and a floor, that I have two arms and two legs, that I am perceiving the furniture and the other people in the room, that I have a variety of memories and beliefs, and that the other people in the room are having or have had experiences which are counterparts of my own. It is true that Moore diminishes the force of his position by adding that no one knows the correct analysis of these propositions of which we all know the truth, and still more by admitting as possible analyses interpretations of the propositions in question that one would suppose him to have ruled out of court, but I shall ignore this complication in the present context.

As he himself made clear, the point of Moore's proceeding was not so much to come to the rescue of common sense, which he cannot suppose to have been widely endangered, as to demolish a certain type of metaphysics, namely the neo-Hegelianism which had come to the fore in Britain in the latter half of the nineteenth century, its foremost representatives being F. H. Bradley in Oxford and J. Ellis McTaggart at Cambridge. Moore was the more anxious to put paid to this doctrine as both he and Russell had temporarily succumbed to it as philosophical apprentices at Cambridge under McTaggart's influence. The neo-

[1] This lecture was written in April 1988 for the closing plenary session of the World Congress of Philosophy held in Brighton in August of 1988.

Hegelians did not take the same view of Reality. For example, Bradley believed that it consisted in what he called an Absolute, a whole of Experiences, somehow embracing in a coherent fashion all the appearances which he had previously stigmatized as self-contradictory. McTaggart believed that it was a community of immortal souls. Where they agreed was in denying the reality of matter, space and time. Bradley tried to hedge by saying that they were real as appearances, but since his charge against them, as I have just remarked, was that the very conceptions of them were self-contradictory, he must be held to have maintained that they were not real.

Now obviously if space and time and matter were unreal, Moore's common-sense propositions could not be true. If space is unreal, I cannot be standing in this room.[2] If time is unreal, I could not have arrived here yesterday. If matter is unreal, since we are all embodied persons, none of us exists.

There Moore was content to leave it. He thought it strange that philosophers should assert propositions which were straightforwardly inconsistent with what they knew to be true, but he did not speculate as to how this came about. He dismissed the metaphysical pronouncements which he had refuted by drawing out their practical implications as being simply false as indicative statements of empirical fact.

His disciples reacted in various ways. Those of us who had been influenced by Wittgenstein's *Tractatus* (Wittgenstein, 1922) and, in my own case, by the productions of the Vienna Circle, rejected metaphysical utterances as being not even false but nonsensical. Returning in fact to Hume, we divided sentences, said to be literally meaningful, into two classes: those that expressed *a priori* propositions—what Hume had called relations of ideas—the validity of which was purely formal, depending wholly on the meaning of the signs which the sentences contained, and those that expressed propositions which were empirically verifiable, that is to say, confirmable or disconfirmable through sense experience, dealing with what Hume had called matters of fact. Utterances which did not fall into either of these classes might have some poetic appeal but they were literally nonsensical. This was taken to apply not only to the work of the neo-Hegelians but to that of Hegel himself and indeed to a very large part of what had traditionally passed for philosophy.

This limitation of literally significant sentences into two classes was deduced from an axiom which became famous as the Principle of

[2] Ayer was at this time not well enough to attend the World Congress, and this lecture was read for him by another. This enabled one of the less incompetent journalists reporting the Congress to make a joke. Freddie was good-naturedly, if not vastly, amused.

Verification: a principle somewhat cryptically couched by Moritz Schlick, the leader of the Vienna Circle, as 'The meaning of a proposition consists in its method of verification'. As the American philosopher Morris Lazerowitz was quick to point out, the principle was a *petitio principii* in the sense that it did no more than summarize the criteria of significance that it was designed to underpin. No attempt was made to prove it independently and indeed it is not easy to see how it could be proved.

Lazerowitz also complained that our use of the word nonsensical to characterize metaphysical utterances was itself a misuse of language, but here I am not in agreement or at least not in full agreement with him. It is true, as we shall see in a moment, that his criticism may apply to his own re-interpretation of the utterances of Moore's metaphysicians, but Moore obtained his results by treating these utterances as empirical and, though I agree with Lazerowitz that this was an error, it is not clear to me what sense they retain as he reconstructs them. What I want to say now is that the main contentions of such a work as Bradley's *Appearance and Reality* (Bradley, 1897) are literally nonsensical and that the same is true of much of Hegel's own work, not to speak of the outpourings of such modern charlatans as Heidegger and Derrida. It makes me very sad to learn that their rubbish is acquiring popularity in this country, appealing to those who mistake obscurity for profundity, and find the serious work of such first-rate American philosophers as Quine, Goodman, Putnam and Davidson too difficult.

Before embarking on my main theme, I should like to say a final word about Morris Lazerowitz who took a line of his own. In his view, the metaphysicians whom Moore attacked and indeed metaphysicians of every type, such as Plato, when he maintained that what he called Forms and we now call Universals alone were real, or Spinoza who claimed to reduce the nature of reality in geometrical fashion from the definition of a few key terms like Substance, Cause and Attribute, did not really mean what they seemed to be saying. They were neither making empirical assertions, in the literal way that Moore had foisted on them, nor were they making *a priori* claims, which would need to be vindicated by an accurate account of our actual use of language. If their assertions were interpreted in either of these ways, the most simple investigation would show them to be false. With a few exceptions, these metaphysicians are not charlatans. In most cases they have been highly intelligent men. Why then should they advance theories which are so easily disproved? Lazerowitz's answer was that they were not advancing any such theories. What they were doing, whether they were aware of it or not, was making linguistic recommendations, and their motive for making these recommendations lay in their unconscious desires.

I admire the boldness of this theory and I wish that I could accept it, if only because it would supply a solution to what still remains very largely an open question. Nevertheless I do not accept it. I do not mind admitting that not only metaphysicians but philosophers of all sorts have unconscious motives, which may intrude into their work, though I should need to be convinced, by much stronger evidence than I have yet seen produced, that they wholly accounted for it. What I find very implausible is the view that the metaphysicians's statements, or what masquerade as such, are linguistic recommendations in disguise. Indeed, I am not sure that it is even coherent.

Let me support this charge by examining one of Lazerowitz's own examples, the contention of Parmenides and his school, including the celebrated Zeno, that nothing changes. Lazerowitz interprets this as a proposal to denude the language of any expression which implies that things suffer any alteration whether in quality or in position; hence Zeno's denial of the reality of motion. Now I can see that one might have an unconscious resistance to any form of change, just as one may have an unconscious desire for perpetual novelty, as expressed in the Heraclitan conception of everything as being in a constant state of flux. But if you fulfil the first of these desires by removing all words implying any process of change from the language, then so far from ensuring the triumph of stability, you abolish the contest. If there is no longer even the possibility of saying that something changes, the assertion that nothing changes loses its meaning.

Lazerowitz was disposed to extend his account of the practice of metaphysics to that of philosophy in general, accusing philosophical analysts, especially myself, of obvious misuses of languages if their words were taken literally. Though I shall be defending myself against these accusations, I admit without further ado that Moore did pose a serious problem for his followers concerning the function of philosophy. The problem arose initially because it was obvious that Moore's claim to know the truth of the propositions which he assigned to common sense was not made in the void. He did not say how he knew that they were true, but if the question had been put to him he could have answered it. In the case of the propositions relating to his current situation, such propositions as that he was seated in a room containing such and such items of furniture and a number of other human beings, his answer would have been that he was relying on the present evidence of his senses, reinforced no doubt by his memory of having often had similar experiences. What is more, his assumption that this evidence gave him the right to claim knowledge of the truth of the propositions in question implied that he took it to be sufficient evidence. There might be some queries about the interpretation of the propositions which it established, upon which enlightenment could be sought, but none

about their truth. In particular, it was not to be impugned by any philosophical argument.

What Moore did not point out, though it should have occurred to him, was that this conclusion could be generalized. There is no reason why the truths of common sense should be the only ones to be immune from philosophical attack. Every branch of knowledge employs its own criteria of evidence. They may indeed differ in the degree of credibility which they bestow upon the propositions which satisfy them. The researches which meet the standards which are required of a modern historian get more secure results than the paleontologist is commonly in a position to claim. Even so, each discipline is master of its own territory. If mistakes are made, they are corrected internally. There is no special source of information, available only to philosophers, which would enable them either on the one hand to over-ride pure mathematics or, on the other, to say that the conclusions reached, each in their own way, by classical scholars, modern linguists, lawyers, historians, physicists, chemists, botanists, and biologists are empirically false.

We shall see later on that the position may not be quite so simple. Doubts about analysis, which Moore allowed to be legitimate, may foster doubts about truths. Nevertheless, the position which it seemed to us that we were facing in the nineteen thirties was what I have just outlined. If the entire domain of knowledge was self-sufficiently occupied by the formal, natural and social sciences, what role was there for philosophy? There were those who took the same road as Lazerowitz, or perhaps I should say a similar road, to the point of saying that philosophy was an activity, not a doctrine, but still they regarded it as a cognitive activity. What kind of cognition could it possibly yield?

The authoritative answer had been given by F. P. Ramsey in one of the 'Last Papers' reprinted in his posthumous *Foundations of Mathematics*:

> Philosophy must be of some use and we must take it seriously; it must clear our thoughts and so our actions. Or else it is a disposition we have to check, and an inquiry to see that this is so; i.e. the chief proposition of philosophy is that philosophy is nonsense. And again, we must then take seriously that it is nonsense and not pretend, as Wittgenstein does, that it is important nonsense!
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> In philosophy we take the propositions we make in science and everyday life, and try to exhibit them in a logical system with primitive terms and definitions, etc. Essentially, philosophy is a system of definitions or, only too often, a system of descriptions of how definitions might be given. (Ramsey, 1931, p. 263)

Ramsey goes on to raise the question what definitions we feel it up to philosophy to provide, and answers that philosophy 'does not propose to define particular terms of art or science, but to settle e.g. problems which arise in the definition of any such terms or in the relation of any terms in the physical world to the terms of experience' (ibid. 264). He admits that we seldom get actual definitions but have to be content with explanations of the use of the symbols and sees a difficulty in the fact that the explanation may refer to entities for which we have no names. This applies particularly to descriptions of definitions of sensory characteristics, in reference to which our language is very fragmentary. 'For instance, "Jane's voice" is a description of a characteristic of sensations for which we have no name. We could perhaps name it, but can we identify and name the different inflections of which it consists?' (ibid. 265).

Finally, Ramsey considers the question whether we can avoid committing a *petitio principii*. The difficulty is that there are terms and sentences about which we cannot get clear without getting clear about meaning and that we cannot understand meaning without understanding, for example, 'what we say about time and the external world' (ibid. 268). He finds this circularity unavoidable and so do I. For example, the Principle of Verification commits one to a certain view of the world, and conversely a certain view of the world is secured by the Principle of Verification.

Ramsey's reference to Wittgenstein is of course to the well-known passage at the end of the *Tractatus* in which Wittgenstein states of his own characterizations of the relation between language and reality, which is the main theme of the book, that they are nonsensical, but goes on to remark that the reader will recognize them as nonsensical only when he has used them as steps to climb beyond them. He then throws away the ladder and sees the world rightly. This is patently disingenuous and it is interesting that Ramsey in his critical notice of the *Tractatus*, which was also reprinted in *The Foundations of Mathematics*, made a valiant attempt to reinterpret many of Wittgenstein's sayings in such a way that they did not violate his conditions of significance. Russell did the same in his Introduction, although in an altogether different fashion. Wittgenstein insisted that both Russell and Ramsey had misunderstood him, but that was common form.

Ramsey died in 1930, an irreparable loss to British philosophy, and I do not know how he would have reacted to the steps by which Wittgenstein gradually moved from the position of the *Tractatus* to that of *The Philosophical Investigations* (Wittgenstein, 1953) and its sequels. So far as I know, Wittgenstein did not say of the vast quantity of material which constitutes this development that it was nonsensical and I think

that he would have rejected any general characterization of it. I shall be dealing with one important feature of it later on.

Before I abandon Ramsey I have to remark that his programme was never carried through. The most successful contribution to it had already been made by Bertrand Russell, with his Theory of Descriptions, which Ramsey himself hailed as a paradigm of philosophy. Russell's achievement was to show how nominative expressions could be systematically transformed into predicates, thereby disposing of the problem how the sentences which contained them could be meaningful even when there was nothing that the expressions denoted. The theory has been criticized on the ground that it obliges us to count propositions as false which we should be more naturally inclined to view as lacking in truth-values, the point turning on the question whether in the case of a sentence like 'The present King of France is bald' the existence of one and only one present King of France is covertly asserted, as Russell would have it; or merely presupposed; and it is open to the objection that more often than not, when we make use of definitive descriptions, the target of the description is not wholly identified by our utterance, but is picked out by the context in which the utterance is made, with the result that Russell's reformulations need to be amplified if they are to pass as translations of the sentences which they replace. Nevertheless, I believe that Russell's theory continues to be important, not only as eliminating a problem which perhaps should not have been taken very seriously in the first place, since it depended on a rather crude identification of meaning with denotation, but as supplying us with a method of getting rid of singular terms.

Perhaps Carnap's *Logische Aufbau der Welt* (The Logical Structure of the World) which had been published in 1928, an extraordinarily valiant attempt to show how the whole body of factual concepts could be created on the basis of the single relation of remembered similarity between the total momentary experience of the author, with no other assistance than the apparatus of Russellian logic, would have met Ramsey's requirements if it had been successful. Unfortunately, it was not successful. As Nelson Goodman showed in his book *The Structures of Appearance*, first published in 1951, Carnap did not even bring about the adequate definition of sensory kinds, and the really difficult task of passing from the experiential to the physical domain was not seriously attempted by him.

A point which Goodman brings out is that the characterization of our sense-impressions is not so straightforward a matter as it had been taken to be, at least by a great many philosophers who have taken a professional interest in the nature of perception. The same point has been made by Michael Dummett (1979) in his contribution to *Perception and Identity*, a volume containing a set of dozen essays, commenting on

various features of my philosophical work, and my replies to them (Macdonald, 1979). Dummett went further than Goodman, in that having defined a simple observational quality as one the presence of which could be described simply by observing it, in the sense that a judgment affirming its presence could be over-ridden only by a judgment made on a similar basis but possibly under different circumstances, he denied that there could be such qualities. His argument was that his definition implied that the qualities needed to satisfy what he called *the indiscernibility condition* 'namely that, if one object possesses the quality, and no relevant difference between it and another object can be perceived, then the second object possesses it'. At first sight we may be inclined to take it for granted that this condition is satisfied by a great many predicates like 'red' and 'sweet' but it turns out that the supposition 'that any quality always satisfies it is incoherent' (op. cit. 9).

The reason why it is incoherent depends on the empirical fact that sensory qualities constitute a continuum, with the result that the relation of indiscernibility between qualities of the same kind is not transitive. Thus, for example, in the case of colour, one discovers a series of patches A, B and C such that A is indiscernible in colour from B and B from C but A is discernible from C. Consequently if we assume that things are of the same colour if they are indiscernible in colour from one another, and also that colours are continuous throughout the spectrum, we reach the conclusion that there is only one colour, since the colour of B being indiscernible from that of A will be of the same colour as that of A, and so will the colour of C since it is indistinguishable from that of B, and so *seriatim*. This conclusion is no worse than empirically false, but since we can also start at the other end of the spectrum or indeed at any point in it with a patch of a different colour from that of A, and go backwards to A, we do get a contradiction.

Goodman has shown that the contradiction can be avoided if we require for X and Y to be of the same colour not only that they be indiscernible in colour but that there be no other patch Z from which either X or Y is discernible in colour and the other not. This proposal meets the difficulty of the continuum, but it does have the disadvantage that we are debarred from asserting with complete confidence that any two patches are of the same colour until we have examined every patch of colour that there is.

Dummett himself sees no need to follow Goodman, since he sees no point in developing what Goodman calls a language of appearance, and makes our ascriptions of colour and other observational qualities depend not only on the way things look or feel or sound or smell or taste to different observers in different circumstances but also on the applic-

ations of scientific theories. This is another point to which I shall return later on.

The view that there are no simple observable qualities runs counter to a long tradition in philosophy, going back at least to Descartes. The theory of sense-data has gone out of fashion increasingly since the war, but in one form or another it was taken for granted by Descartes, Locke, Berkeley, Hume, Kant, John Stuart Mill, Husserl, G. E. Moore, Bertrand Russell, Moritz Schlick, C. D. Broad and Henry Price, and considered seriously by Rudolf Carnap and Ludwig Wittgenstein though they both finally turned against it. Its cardinal feature is the assumption that our everyday judgments of perception, such perceptual judgments as Moore assigned to common sense, are founded on the reception of sensory items which are luminous either in the strong sense that one's assessment of their character allows no possibility of error or in the slightly weaker sense that one's assessment is authoritative: one may be uncertain about its character, while it is still present, or even revise one's original judgment, but one's decision cannot be over-ridden by anybody else. This has been taken to be true of the entities, variously denominated as ideas of sense, sensible qualities, impressions, representations, sense-data, sensa, sense-contents and sense-qualia, and what is common to them all is that they or their instances in the cases where sensible qualities, or qualia, are taken as primitive, is that in the process of perception they are objects of what Russell called direct acquaintance.

Is every theory of this sort demolished by Dummett's argument against the possibility of there being simple observational qualities? I do not think so, because I do not agree that the theory commits one to the indiscernibility condition. I admit that if two mutually indiscernible patches of colour are presented in the same sense-field, with no third instance in relation to which they could be differentiated, it would be inconsistent to describe them differently. I do not, however, believe that the inconsistency extends to the case where the colours are instantiated in different sense-fields and one is merely going on the supposition that if they were brought together one would not be able to discriminate between them.

But does not Dummett's argument prove that any characterization of sensory qualities, purely on the strength of their appearance, is inconsistent? Not unless one accepts his condition of indiscernibility or rather his interpretation of it. My view is that it forces us to conclude not that the use of simple observational predicates leads to contradiction but that at numerous points it is bound to be arbitrary. Consider, for example, a colour atlas which runs through the colours in a continuous fashion, so that between any two specimens which are distinguishable in colour there is one that is indistinguishable from each of

its neighbours. Let us suppose that blue runs into green in this fashion. At a certain point we are going to encounter an item which on the basis of its similarity to its neighbour we shall be equally justified in calling blue or green. We then take an arbitrary decision. In Quine's terminology there is no fact of the matter that we shall be contravening.

There is a similarity between this case and the sorites paradox. It seems obvious that if n is some large number, and a crowd of n persons is present on some occasions, the number of those present does not cease to be a crowd if one is added or one removed. Yet if one adopts the general principle that if n persons form a crowd so do $n-1$, we shall gradually reduce the crowd to nobody at all. Our only resort is to fix arbitrarily on some number m, and say that anything less than that number of persons does not constitute a crowd, though the choice of $m+1$ or $m-1$ would have been equally acceptable. The same applies to other vague terms like 'warm' or 'bald'. It does not follow that we have no use for them.

If reference to colours and other sensory items can be made in a purely observational way, without the assistance of terms which imply the existence of physical objects, then I think that a very simple argument justifies their choice as a basis for a theory of perception. We need only consider the range of assumptions which our ordinary judgments of perception carry. I am not now embarking on an excursion into scepticism. I have no doubt that I can see a door at the further end of the room. I am merely remarking that for there really to be a door there, or indeed any other physical object in the room that I could mention, it is not enough that it be visible to me. It has to be accessible to my sense of touch and it has to be accessible to other observers. It has to occupy a position in three-dimensional space and to endure throughout a period of time. Moreover, if it is correctly identified as a door it has, at least potentialy, to fulfil a certain function: it needs to be solid; there is a limit to the sort of material of which it can be made. And similar considerations would apply to any other physical object that I had chosen for an example.

But now can it possibly be the case that all this can fall within the content of a single act of perception? Can it follow from the appearance of a door in my present visual field that I am seeing something that is also tangible or perceptible by other observers? Can it follow even that what I am now seeing persists beyond the brief duration of my present visual experience, let alone that it possesses all the other properties with which my perceptual judgment endows it? Surely not. But then it follows that my judgment of perception embodies a set of inferences, which do not cease to be inferences because I am not conscious of making them. Their existence is established by the fact that I reach a