

CONCLUSION.

Conclusion.On Turning Philosophy Into Science.

Most of the major philosophical problems have been with us for a very long time, and because of this it is quite widely believed that they may be inherently insoluble - and perhaps even that it is the mark of a truly philosophical problem that it should be so. This would seem to lead fairly directly to another widespread belief: that philosophy is a waste of time. It is thus rather odd that the first belief, at least, seems to be held by a number of professional philosophers. I do not merely mean by this the view that philosophical problems need to be **dissolved** rather than solved. I mean the view that we can never hope to be done with them. Despite apparently holding this view, Thomas Nagel insists that it is important to continue wrestling with such problems {1}. But if we seriously believe that problems cannot be solved, it surely seems more sensible to take Richard Rorty's advice, and stop worrying about them {2}. A similar view to Rorty's, at least over the issues of the status of scientific knowledge and the real existence of the theoretical entities of science, seems to be taken by Arthur Fine {3}. He argues that it is fruitless to worry over whether science should aim at truth (the realist view) or merely at empirical adequacy (the instrumentalist view). "Is the threat of scepticism or relativism", he asks,

so enormous that we have to invent over-

arching goals in order to demonstrate the rationality of our scientific practice?

{4}.

As should have been apparent from my introduction, my answer to this is: yes, the threat (and the promise) of relativism is very considerable. To give up the attempt to deal with such a problem on the basis, seemingly, of the frustrations of a few decades seems to me to be shallow in the extreme, especially within a discipline with a history of more than two and a half millennia.

Rorty, however, has a rather better case, for the problems which he wants to forget about, essentially the basic questions of epistemology, are pretty nearly as old as philosophy itself. Furthermore, he suggests that other 'insoluble' problems which once seemed pressing - the question of the nature and the will of God, for example - have in fact only been removed from the centre of the intellectual stage by being forgotten about rather than by being solved {5}. Rorty (who prefers to call himself a "pragmatist" rather than a relativist, but the difference is negligible for our purposes) hopes and expects that something similar is about to happen to epistemology:

Pragmatists think that the history of attempts to isolate the True or the Good, or to define the word "true" or "good", supports the suspicion that there is no interesting work to be done in this area. It might, of course, have turned out otherwise. People have, oddly enough, found something interesting to say about the essence of Force and the definition of "number." They might have found something interesting to say about the essence of Truth. But in fact they haven't. {6}.

"Interesting" here, I take it, means something like

"useful, and on which we can all agree" (surely many things have been said about truth and goodness which might intrigue us - it's just that there is always someone around to argue to the contrary). If that is so then the 'interesting' things about force did not really begin to be said until the scientific revolution, about 400 years ago, and the 'interesting' things about "number" were said even more recently. The two millennia between Aristotle {7*} and Galileo, during which little or nothing 'interesting' was said about force, did not show that there was nothing worth saying. Why should the two and a half millennia between Plato and Rorty show that nothing useful will ever be said about truth {8*}?

The fact is, of course, that it is still possible to argue about the nature of force, and a few philosophers still do so. We do not believe that 'interesting' and valid things have been said about force because someone (Galileo? Newton?) produced a knockdown argument in favour of their particular conception of it. The things which Galileo and Newton (and Einstein) said about force seem 'interesting' because it proved possible to found a scientific research tradition, and a technology, on their basis. Accepting Newton's conception of force (and a few other things) meant that one could go on to ask many more detailed questions about mechanics and astronomy which could not otherwise even have been raised. But Newton and Galileo, unlike Aristotle, are usually claimed as scientists rather than as philosophers. What I am trying to suggest is that

philosophical problems, even if they can never be demonstrably **solved**, do not disappear only through being forgotten about. They can also, and more fruitfully, disappear through some particular proposed solution becoming incorporated into the foundations of a scientific practice. The main purpose of this thesis has been to try and show how a particular sort of position in epistemology and philosophy of science can be incorporated into the science of psychology, and to argue that what we thus get makes better sense **as psychology** than what we get by incorporating rival philosophical assumptions. The principal rivals are: positivism, which gives us a Behaviorist psychology; and what Hacking (9) calls "lingualism", the view that knowledge can only be embodied in language or in something very like language. 'Lingualism' leads on the one hand to relativism (10*), and on the other hand to what Haugeland calls "cognitivism", the sort of psychology built around the notion of 'propositional' or 'sentential' mental representation, and of which we have seen representative samples in the work of people like Pylyshyn, Anderson, Palmer, Baylor, and even Kosslyn. For a number of reasons, some of them 'external' to the scientific practice itself (11*), this seems to be the dominant theoretical approach in cognitive psychology at the present, but many psychologists (not to mention philosophers) remain sceptical, so the situation may not continue like this for ever.

The sort of epistemology towards which I am trying

to grope my way, and which I think goes with the psychology of 'perceptual activity' and 'interactive representation' which I have tried to sketch, has been most nearly expressed in the recent work of Cartwright {12} and Hacking {13}. Their view is that scientific **theories** (i.e. what one is inclined to **say** about reality) should not be treated as true (or, indeed, false). That is to say, we can and should treat **theories** entirely relativistically or pragmatically. On the other hand, Cartwright and Hacking argue, we should consider well established theoretical entities and mechanisms as **real**. Electrons really do exist; salts really do dissociate into charged ions in aqueous solution. We might well express the point by saying that, however they may be described, these things really are pretty much how the scientist who is skilled in handling them imagines them to be (as we saw in our introduction, there is reason to think that scientists generally **do** have imaginative 'pictures' of the behaviour of their 'unobservable' theoretical entities). It thus seems to me that this position of Cartwright and Hacking requires something like the account of non-linguistic representation which I have tried to develop here. Philosophies and psychologies which regard knowledge as being entirely linguistic cannot capture this distinction between adherence to theories and knowledge of things. They must assimilate all to theory. But, I suggest, it is the direct, interactive knowledge of things which is the real, intuitive, scientific understanding. The rôle of theory, of verbal communications and diagrams, is merely to help one to get an appropriate

image into one's head, to form an appropriate schema which will efficiently guide one's interaction with the object. This interaction may, of course, involve the use of scientific instruments (combined, often, with the mathematical calculations required to interpret their readings) as 'prostheses' for one's innate perceptual systems. But this does not make any important epistemological difference. It does not really matter whether the tests being made are carried out using instruments or by perceptual systems which are 'built in' to us. The important thing is that actual perceptual contact is not in any relevant (i.e. distorting) way linguistically or culturally mediated, and it will always play the dominant part in moulding the schema and thus determining the form of the image. What we are told, or what we are able to deduce from a theory, may lead us to form new images, and thus modify our schemata so as to enable us to notice things which we would not otherwise have noticed. But this does not mean that we will see things that are not there. With observational diligence and luck, however, we may notice things which ~~are~~ are there, but for which our present theory has not prepared us. If we can find the words to convey to others how to look for these things then science will have progressed.

Cartwright and Hacking apply their 'entity realism' only to the philosophy of science (in fact Cartwright concerns herself exclusively with physics), but it seems to me that it can usefully be extended to the more mundane

areas of epistemology as well. It is quite widely held these days that ordinary 'folk' belief complexes are formally similar to scientific theories. Churchland (14) has illustrated this point with an account of what he takes to be the 'folk' theory of heat, which he shows to fail in certain circumstances because it lacks conceptions such as specific heats and thermal conductivity. Churchland's view is that these failures show that the theory is simply false, and needs to be replaced by a scientific theory. He wishes to draw a similar conclusion about 'folk' psychology. However, these 'folk' theories are clearly very useful in certain circumstances. We would not want to approach something like everyday cooking, for example, with a battery of thermometers, pH meters, tables of latent and specific heats and of temperatures of protein denaturation, and who knows what else. The 'folk' beliefs about heat are quite sufficient and far less cumbersome. Arguably, 'folk' psychology, which can perhaps be regarded as conveying its images, its theoretical models, through novels, plays and the like, remains better able to predict and explain human behaviour than does its scientific counterpart. History certainly teaches us that mankind's current set of beliefs is likely to be much modified in the future. However, we do not (and surely we **should** not) throw it all out and pin our hopes on the unlikely development of some finalized, 'utopian' science. If we are wise we will allow ourselves to treat both 'folk' and scientific theories pragmatically, relativistically. However, there is no call to be tempted, thereby, to relinquish our belief in hot things or thinking

people, vibrating atoms or unconscious wishes. Perhaps, although it is too soon to say for certain, perceptual-imaginal schemata also really exist.

I am arguing that agreement about basic issues in the philosophy of science will only be reached when some attitude to them has been incorporated into the foundations of a successful psychology. It is my impression, however, that psychologists (with occasional exceptions, like Gibson and Gregory) are like most scientists in that they do not usually pay much attention to their work's epistemological implications. Also, there is not a very great amount of interest specifically in the psychology of science itself {15*}. However, there is a group of people who claim to be involved in doing the 'science of science' and who are much given to drawing epistemological conclusions, usually strongly relativistic ones. The people I have in mind are the contemporary sociologists of science, and their philosophical supporters {16*}. Many sociological studies of science are interesting and useful. However, it is of the nature of sociology that it studies the relationships and communication between people. As far as science goes, the field of the sociologist is to study the communication, formal and informal, between scientists, which is, broadly, the theoretical, linguistic aspect of science. We have argued above that it is indeed sensible to treat such 'theory' relativistically, and it is, therefore, quite understandable that sociologists come to, or come away from their study of scientific practice convinced of ^{the} relativity

of scientific knowledge. We have also argued, however, that the scientist (or, indeed, anyone) can have a direct perceptual contact with the natural world. If this is right, then it supports the 'entity realism' of Cartwright and Hacking. But whether or not it is right is a matter for psychology; it is outside the ambit of sociology. It is thus quite illegitimate to conclude that sociological study can imply relativism about entities {17*}, or even that it incorporates, and therefore effectively legitimates, it. My view is that a well established sociology of science is very likely to legitimate theory relativism, whereas a workable psychology of perception is likely to legitimate entity realism, and that there is no inconsistency, but rather a complementarity, between these two positions.

The combination of theory relativism and entity realism endorses our freedom to experiment, earnestly or playfully, with theories and ideas, whilst allowing us to keep our feet firmly planted on the ground. It enables us to say that one theory may be better or worse than another, may throw more light on the relevant matters, without our having to say that everything but the very best possible theory is a pack of lies or nonsense. This is what I mean by getting as much as we can of the best consequences of relativism without getting too much of the worst. What it requires, however, is that we are able to see how it is possible for us to have two qualitatively different forms of knowledge: the linguistic, and the tacit or imaginative. Any illumination of this mental realm, if the light is to

last, must come from, or at least be incorporated into, the science of psychology.