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Introduction

I am pleased that this selection of my essays is coming out in Spanish. Much of my work has arisen out of dialogue with other philosophers. This translation affords the opportunity to engage in conversation in a language that I do not speak.

This is connected with a theme of my early work on incommensurability. Some philosophers argued that untranslatability is impossible on conceptual grounds. But I thought that translation might sometimes fail to be possible. Thus, I found myself prepared to defend an aspect of the incommensurability thesis, namely, the idea that it might not be possible to translate between the vocabulary of semantically variant theories. But, while this might be so, I did not think this meant that the theories are unable to be compared or that the advocates of such theories should be unable to understand each other.

Thus, I sought to show in my early work that the claim of incommensurability did not lead to the radically irrationalist or anti-realist consequences so often associated with the claim. Arguing in this way for a more moderate reading of the claim of incommensurability does require one to pay detailed attention to the arguments that were given on its behalf. Thus, in one of the articles included here, 'Kuhn's Changing Concept of Incommensurability', I argue that Kuhn's own thinking about incommensurability had undergone a process of modification. This process continued even after the works by Kuhn discussed in the article. In my view, the version of incommensurability that Kuhn

endorsed in his later work has few, if any, of the radical implications usually associated with the doctrine of incommensurability.

In my early work, I concentrated on semantic aspects of the problem of incommensurability. But I also thought that methodological aspects of the idea had often been exaggerated. Even if it is the case that the rules or standards of scientific method undergo change, that does not entail that the choice between theories is irrational. Nor does it lead to a relativist view that rationality varies from paradigm to paradigm with shifting methodological standards. My paper, 'The Problem of Rational Theory-Choice', reprinted here, attempts to show that ideas of both Kuhn and Feyerabend might be embraced within a rationalist outlook. The key is to recognize that the choice between theories is not algorithmic, based on a fixed set of mechanical procedures which determine an outcome. The choice may be based on methodological standards which provide the rationale for the choice even if the standards are subject to variation.

As a philosopher trained in New Zealand and Australia, I adopt a thoroughly realistic conception of the world, which underlies my thinking about language, knowledge and scientific inquiry. However, as I began to interact with philosophers from continental Europe, I increasingly realized that the realism widely found throughout Australasian philosophical circles was considered to be controversial in at least some other philosophical environments. As a result of my conversations with philosophers working in Europe, I realized that the position of scientific realism requires careful articulation if it is to be made palatable to such philosophers. The articles that make up the main body of this book date

from work that was prompted by my engagement with European philosophers of science, for whom scientific realism was not just controversial, but often philosophical anathema.

The paper, ‘Scientific Realism: An Elaboration and a Defence’, is an example of such an attempt to develop a clear articulation of the position. In it, I seek to characterize scientific realism in a way that allows for variation between scientific realists, who may disagree amongst themselves on points of detail. The key point is that scientific realists hold that theories may yield true descriptions of unobservable theoretical entities. To be a realist in that sense does not entail commitment on further metaphysical question, such as, whether theoretical entities belong to natural kinds or whether laws of nature are grounded in natural kinds. Thus, I distinguished between the basic principles of scientific realism and optional questions which the realist may address. The basic principles are doctrines that any scientific realist would endorse. But realists may answer the optional questions in different ways.

The main argument for scientific realism has always been the so-called success or no miracles argument made famous by Hilary Putnam. But, while this has certainly been the central argument discussed in the literature, I have never felt that it should play so central a role. I think there is a realistic perspective that underlies scientific realism and which operates at a deeper level than the success argument. So in this paper, I distinguish a number of different considerations which unite to constitute the case for scientific realism, only one of which is the success argument as it is customarily understood. Apart

from the success argument, I think that considerations of common sense as well as rejection of anthropocentrism should predispose one toward realism.

In fact, I distinguish between two applications of the success argument. The success argument may be applied to theories, and lead to a conclusion about the truth or approximate truth of theories. But it may also be applied to the scientific method, leading to a conclusion about the reliability or truth-conduciveness of the rules of method. In ‘Realism, Method and Truth’, I employ this second application of the success argument as part of my attempt to develop a realistic theory of method. For the realist who holds a non-epistemic theory of truth, it is a genuine question why we should expect that use of the methods of science leads to the truth. I argue that the best explanation of the success of theories based on these methods is that the methods are themselves truth-conducive instruments of inquiry, which reliably lead to truth about the world.

Once, when I presented a lecture on scientific realism in Italy, I was asked about the problem of the God’s eye point of view. Doesn’t scientific realism require a God’s eye point of view? Since it is impossible for us to occupy a God’s eye point of view, isn’t the realist position incoherent?

My paper, ‘Scientific Realism and the God’s eye point of view’, is my response to this question. I do not think that we must occupy a God’s eye point of view in order to articulate the position of realism. Rather, we may propose realism as a theory about our human situation, including our relationship to the world around us. To propose realism as a theory about our relationship to the world does not require us to adopt a God’s eye point

of view. For we may propose this theory about our relationship to the world from within our human perspective.

This may not persuade everyone. One might still think that realism requires us to adopt some sort of position outside of the human viewpoint. But, even if this is so, it is not clear just what the problem is. We may not be able to adopt a God's eye point of view as such. But we are able to adopt an external perspective for certain epistemological purposes. For example, we may conduct an epistemological analysis of the relationship between the cognitive states of an animal and the environment in which the animal operates. In a similar manner, we may conduct an epistemological investigation of the epistemic relationships between humans and the world around us. To put the point in naturalistic terms, it is possible to empirically investigate the epistemic relationships between the human mind and the mind-independent world which we inhabit. There is no paradox in that. It is a simple matter of empirical inquiry into our perceptual and cognitive interaction with the world.

Some years ago, I participated in a workshop in France about the question of the contingency or inevitability of the results of science. This was not a problematic with which I was familiar. To my surprise, there appeared to be a sense that scientific realism is committed to the inevitability of science. It was as if the realist must expect that science will inevitably converge on a single uniform picture of the world.

But it was not clear to me that scientific realism is committed to such an inevitabilist conception of science. Thus, in 'Scientific Realism and the Inevitability of Science', I

attempt to show how scientific realism, in the generic form in which I construe the doctrine, does not entail that the advance of science must inevitably lead to the one true picture of the world. However, if one combines scientific realism with certain other assumptions about the reliability of the methods of science, then something like an inevitabilist view of science may be the result.

I mentioned before that common sense should predispose us toward a scientific realist outlook. I certainly think that this is the case. For me, common sense is realist through and through. We are surrounded by objects with which we interact in our everyday lives. These objects exist independently of us. We can affect these objects by way of bodily action. Mere thought alone has no effect on such independent objects, even objects which were originally made by us, such as tables and chairs. For they are things which exist independently of thought. Thus, I hold that commonsense realism is a kind of metaphysical realism.

But there is a problem. There is a strong realist tradition which favours science over common sense. As science advances, we find more and more out about the world, including the world immediately around us. We discover that the tables and chairs around us are not solid objects, but are instead made up of myriads of minuscule particles. We discover that the sun does not rise and set everyday, as it passes overhead in the sky. Rather, it comes into view each morning as the earth rotates, and disappears from view in the evening as the earth continues its rotation.

In these ways, we continuously discover that common sense is mistaken. And so science leads to the elimination of common sense. How can the scientific realist hold that the case for scientific realism rests on common sense? If science undermines common sense, then science undermines the case for scientific realism. Common sense is unable to support realism if it is to be rejected on the basis of science.

I turn to these matters in the final paper, 'Science, Common Sense and Reality'. I suggest that a distinction is needed between common sense and deeply held belief. It is true that we have many deeply held beliefs which are rejected as a result of the advance of science. But common sense lies at a deeper level than this. Common sense is something that operates at a basic human level, which may well be shared with non-human animals. We possess a variety of commonsense beliefs about the objects in our immediate environment, as well as about our relationship to these objects, on the basis of which we interact with this environment. These are quite basic beliefs, which, it seems to me, are not subject to variation with change of scientific theory. They are, instead, our natural human endowment, without which we would hardly be able to survive.

It has been suggested to me that I should not call this basic level of human interaction with the world "common sense". The expression 'common sense' carries excess baggage. It is usually used to refer to what I describe as deeply held belief. So I must seek some other word to describe the basic beliefs about the world and our interactions with the world. Perhaps I should call it instead basic common sense. Or perhaps, to use the words of David Armstrong, I might call it "bedrock common sense".

One way or the other, I don't think that beliefs at this level really are subject to elimination with the advance of science.

This brings me to some of my current research, not represented in this volume. I hope to show not only that common sense provides a starting-point for scientific realism, but that it may serve as foil against relativism. I regard contemporary epistemic relativism as resting tacitly on the ancient sceptical problem of the criterion, according to which no epistemic standard may be justified without circularity or regress. This problem, I believe, may be met in a particularist manner, by appeal to concrete instances of knowledge, such as items of commonsense knowledge. On the basis of concrete instances of knowledge, it is possible to empirically investigate the comparative reliability of epistemic standards. There is no need to say that one epistemic standard is just as good as any other.

But that is a conversation for another occasion. Like all philosophical conversations, that conversation will develop out of interaction with other thinkers. The work of philosophy is not just a conversation. But without conversation, I do not think it could exist at all.