

Science and Truth*

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Abstract

Philosophy of science and mainstream epistemology have much to learn from each other. Most twentieth-century philosophers of science set absurdly high standards for knowledge, and so succumb to naive sceptical arguments. They would do well to learn from mainstream epistemology that reliability is a more sensible standard for knowledge than certainty. At the same time, mainstream epistemologists would do well to learn from philosophers of science that intuitions about the everyday concept of knowledge are unimportant, by comparison with the serious issue of how to get at the truth. My own view on this latter issue is that we should look to science itself for the answers, since science itself tells us about different techniques for uncovering the truth in different subject areas. There is nothing viciously circular in this position, though it does imply that there is no external perspective from which science as a whole can be vindicated.

The philosophy of science is a subject with two faces. On the one hand it investigates philosophical problems that arise within science, problems thrown up by scientific theorising itself, but which cannot be solved by empirical data alone. Examples of this kind of philosophy of science include recent work in the philosophy of physics, on the interpretation of quantum mechanics and the structure of space-time theories; or, again, recent work in the philosophy of biology, on the logic of natural selection and the analysis of functional explanation.

Other examples of this kind of philosophy of science are not so closely tied to particular scientific specialities, but grapple with notions that play a role within a number of different areas of scientific theory. Under this heading comes the development of different interpretations of probability, say, or the study of processes which are asymmetrical in time.

This face of philosophy of science, philosophy within science, we might call it, has been well represented in the Department of History of Philosophy of Science in this College, the Department which was originally part of Chelsea College and joined King's College when the colleges merged in 1985. Both my predecessors as Professors and Heads of this Department, Michael Redhead and Heinz Post, have made important contributions in this area of philosophy of science, as have other past and present members of the Department.

These contributions, and others like them, can be regarded as complementing the work done in the rest of philosophy. Just as with other philosophical specialities, like the philosophy of art, say, or the philosophy of history, philosophy within science takes up problems that are peculiar to its own subject matter,

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and interacts with the rest of philosophy only to the extent that it shares the techniques — which in the end are nothing more than argumentative care and patience — that are common to all philosophy.

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There is, however, another face to the philosophy of science, a face whose relation to the mainstream of philosophical theorising is less straightforward. I am thinking here of the subject called scientific method, or methodology, in which philosophers of science study the procedures by which scientists construct and evaluate their theories, the procedures on the basis of which scientists decide which theories should be admitted into the scientific canon in the first place. And the reason the relation of this subject to mainstream philosophy is not straightforward is that there is little difference between the questions, if not the answers, considered in this area, and those considered by non-specialist philosophers under the heading of epistemology, or the theory of knowledge. In epistemology, mainstream philosophers ask how we know about the external world, and how our beliefs about the external world are justified. By asking these questions, they are staking out the same ground as philosophers of science who ask about the authority of our scientific theories, and about how far scientific beliefs are justified.

It is true that to some extent scientific theories have a characteristic content which might be thought to raise special problems of justification. Scientific theories characteristically extrapolate beyond the observational data, to make claims which are general, precise, and often about objects not available to the unaided senses. Still, while science might perhaps be distinguished from other areas of discourse by the salience of such attributes as generality, precision and unobservability, these are also attributes which are present in a large number of non-scientific beliefs, and as such are attributes which no mainstream epistemology worth the name can afford to ignore.

So scientific method and mainstream epistemology address very much the same questions. However, they tend not to coordinate their answers. It is a striking fact, especially in this country, that practitioners of these two disciplines tend to proceed quite independently of each other, each developing views which not only take no notice of those developed by the others, but are often quite inconsistent with them.

Part of what I want to do this evening is show how this state of affairs acts to the detriment of both sides. The philosophers of science would do well to listen more to the mainstream philosophers; and at the same time there are things the mainstream philosophers can learn from the philosophy of science. Perhaps King's College can soon play some small part in bridging this gap. Our Department of History and Philosophy of Science will shortly be merging, in August,

with the Philosophy Department. Among the other benefits of this merger, perhaps the combined Department will go some small way — or perhaps not such a small way, since the new combined Department will, I think, be the largest in this country, after Oxford — to persuading philosophers of science and mainstream philosophers to make the effort to learn from, and help, each other.

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One does not have to look far to notice that the philosophers of science are in need of some help. One striking feature at work in the epistemological side of the philosophy of science — I'll drop this qualification "epistemological" from now on, since I'll be talking exclusively about this second face of philosophy of science in what follows — one striking feature of philosophy of science over the last three decades has been the currency given to extreme relativist views about theory-choice: views which hold that, since choices between alternative scientific theories (between the particle and wave theories of light, say, or between classical and relativistic mechanics) are never conclusively dictated by any finite body of experimental evidence, they must instead rest on non-rational grounds, on some hunch or arbitrary decision. Perhaps the most adamant proponent of this surprising view has been Paul Feyerabend; but similar if less forthright relativistic sentiments have been expressed by a significant number of other philosophers of science, most prominent among them, of course, being the T. S. Kuhn of *The Structure of Scientific Revolutions*.

It would not of course be true to say that this kind of relativism about theory choice is orthodoxy among contemporary philosophers of science. Only a minority would explicitly endorse such views. But relativism has nevertheless been influential beyond its numerical representation, both inside and outside philosophy of science. In particular, I don't think it would be inaccurate to say that it is currently the dominant view among historians and sociologists of science. The historians and sociologists add an extra twist, however. Philosophers of science like Kuhn and Feyerabend have generally had relatively little to say about what does determine theory-choice, if the dictates of rationality do not; being content to attribute such decisions more or less to fashion, or perhaps to the exercise of authentically free choice (this tends to be Feyerabend's line), or the need to find new problems to work on (as Kuhn used sometimes to suggest). But the historians and sociologists have quite reasonably sought better ways of filling the explanatory gap, arguing that these suggestions are scarcely sufficient to explain the widespread agreement of large groups of scientists on theoretical issues, and that the real explanation must therefore lie in political factors, either in the form of affinities of content between certain theories and the interests of certain sections of society, or in the form of the outcome of struggles for power between competing groups of scientists.

I should immediately say that within the philosophy of science itself this kind of political reductionism is widely viewed with suspicion. The consensus among philosophers of science, I would say, is that the relativist arguments of Kuhn and Feyerabend are not only provocative but definitely interesting; implausible perhaps, but certainly worth discussing; the historians and sociologists, on the other hand, have clearly gone too far; and when they start suggesting that scientific theorising is no more impartial an activity than political lobbying for economic advantage, then surely it is time to draw the line.

However, this line turns out not to be an easy one for the philosophers of science to draw. For there are other strands within the philosophy of science, far more orthodox and widely supported than the extreme relativism of Kuhn and Feyerabend, which provide just as fertile a soil for political reductionism. I am thinking here of the falsificationism of Sir Karl Popper and his followers, and of the instrumentalism which has always been popular in the philosophy of science, and which has recently undergone a revival in the work of Bas van Fraassen and others.

It may seem surprising that I should want to associate Popperian falsificationism with relativism. Surely, some of you will be thinking, Popper is the great defender of scientific rationality, and so diametrically opposed to the relativists. Let me explain. Popper of course thinks that the rejection of scientific theories, their falsification, is rationally driven by evidence, as when some prediction issuing from a theory is experimentally disproved. But rejection is the easy part of scientific theorising (though not, perhaps, always as easy as Popper thinks). Still, rejection is at least relatively easy, by comparison with the acceptance of scientific theories, in the sense of believing them to be true. Yet on this question, of when we should believe theories as true, Popper has no advice to offer. Indeed he goes so far as to say it is always irrational to believe any scientific theory to be true, since there is no effective logic that can take you from the experimental evidence to any such positive scientific belief.

This is why I said that Popper provides as fertile a ground for political reductionism as the explicit relativists. For after all, despite Popper condemning it as irrational, scientists do normally believe as true a wide range of general theories about the empirical world. So if Popper is right to hold that rational considerations do not suffice to explain such beliefs, then something else must, such as, for instance, the political connotations of these beliefs, or the power struggles among scientific groupings.

The same point applies to the kind of instrumentalism defended by, for instance, van Fraassen. Instrumentalists don't go as far as the falsificationists, in that they don't think that all general beliefs are irrational; just beliefs about the unobservable world. But even this more limited recommendation for the suspension of belief creates space for the sociologists. For scientists do normally

agree in all kinds of definite positive beliefs about the structure of the unobservable world. Yet, if such beliefs cannot be rationally derived from evidence, then they must derive from something else, like politics.

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There is more to say about the inter-relations between these different strands in contemporary thinking about science — the explicit relativism of Kuhn and Feyerabend; the political reductionism of the historians and sociologists; the falsificationism of Popper and his followers; the instrumentalism of van Fraassen and others. But let me at this point simplify the issues by noting that all these groups are in surprising agreement on one striking claim. Namely, they all accept, either explicitly, or as something that follows very quickly from their assumptions, that all scientific theories are very likely false. Groups of scientists take certain theories to be accurate reflections of the working of the world. But according to the philosophers of science I have mentioned, they have no rational basis for supposing their theories are true, and the chances are they aren't.

It seems to me that this attitude to science is manifestly absurd. It is worth being clear what is at issue here. The philosophers of science I have discussed aren't just claiming that some scientific claims are false; that the latest cosmological speculations about dark matter, say, or the claims of some cancer researcher out to catch a headline, are false. They are saying that all scientific theories are likely to be false, including such theories as that water is made of hydrogen and oxygen, or that chickenpox is caused by a virus, or that the sun is powered by nuclear fusion. And this, I think, amounts to a *reductio ad absurdum*. For nobody who is sufficiently informed about these matters can seriously doubt the truth of these claims.

The absurdity of most contemporary philosophy of science is not always immediately obvious, since most of those who hold these absurd views speak with common sense, using terms such as scientific "knowledge" and "discovery" and "acceptance" to which they are not, given the normal meanings of these words, strictly entitled. I shall not dwell on this aspect of contemporary philosophy of science, however. For those who are interested, I recommend David Stove's little-known but extremely salutary polemic, *Popper and After*.

I say that it is absurd to deny that much scientific theorising is straightforwardly true. But let me immediately add that I am not saying that we should always take the deliverances of scientists at face value. Scientists can certainly sometimes go wrong, and indeed one of the reasons they sometimes go wrong is because of the political implications of their views. And let me also hasten to add that, even in cases where scientists do not go wrong, sociological and historical studies of science can do much to illuminate the internal workings of scientific communities, which turn out to be made up of people whose many

human failings often belie the conventional image of the pure and idealistic scientist. Still, we can accept all this, yet refuse to adopt a general disbelief in all scientific findings, including such overwhelmingly well-attested findings as that water is made of hydrogen and oxygen.

Why exactly have so many contemporary philosophers of science been prepared to embrace such an unacceptable position? My diagnosis is that they oscillate between a good argument of limited scope, and a bad argument that applies quite generally.

The good argument, but with limited scope, is that there are plenty of examples of scientific theories which have turned out to be false, and that we should therefore expect further scientific theories to be false too. The bad argument is that, even apart from past evidence of scientific failings, we should never believe any scientific theory because we can never be certain that it is true.

The debate typically goes as follows. Those doubtful of the claims of science normally start with the good argument of limited scope; they point out that there are plenty of examples, from Ptolemaic heliocentrism to Newtonian assumptions about space and time, of theories that have turned out false. The friends of science then respond that this line of argument only stretches so far — true, it shows that some generally accepted science is sometimes false; but it doesn't show that all is. After all, the theories just mentioned also contained a great deal of true information, alongside their false content. And, what is more, such examples of false theories tend to come from certain specific areas of science, such as general cosmology, or theories of space-time structure, and so cast little doubt on conclusions reached in other areas, such as atomic chemistry or molecular biology.

At this point the doubters then switch tack. They admit that there is no direct reason to doubt the assumptions of, say, atomic chemistry. But still, they say, we can't be sure, we can't be certain, of those assumptions. Maybe the evidence — which originally became available at the end of the 18th century, in the form of the laws of combination in constant proportion by mass and volume — maybe that evidence made it very attractive to conclude that matter is made of atoms, one kind for each element, which are disposed to combine with each other in certain simple fixed whole number ratios. But can we be certain that this is the right explanation for the evidence? Maybe there is some more complicated Heath-Robinsonish mechanism that just happens to generate the same phenomena as the atomic hypothesis would. Or maybe there is no mechanism at all, but just some deity, or some demon, or some population of little green homunculi, who are determined, for their own reasons, to arrange the evidence so as to fit the laws of constant proportion. And so, since we can't be sure that these alternatives are wrong, we aren't justified in holding that the atomic hypothesis is right. Or so the doubters argue.

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This is the point at which I think the mainstream epistemologists can help the philosophers of science. For of course mainstream epistemologists are extremely familiar with this latter form of argument. It would not be too much to say they are brought up on it, from the time they are introduced to Descartes' Meditations in their first philosophy lecture, and are invited to grapple with the argument that since we aren't certain, that since there is room for doubt about our beliefs about the external world, we aren't justified in holding them. However, while the mainstream epistemologists grapple with this argument, they tend not to succumb to it, as in effect do the philosophers of science. For, in mainstream philosophy, the conclusion, say, that we don't know there's a table in front of us, is not regarded as an acceptable resting place, but rather as a paradox, a conclusion that we must find some way of avoiding.

Orthodox analytic philosophers are often made fun of for spending so much time on such questions as how do we know there is a table in front of us. And perhaps this is right — perhaps there are more pressing questions, even within philosophy, than this. But in defence of the mainstream philosophers it can at least be said that they try to keep wrestling with this question until they find a solution. The philosophers of science, by contrast, simply give up. At bottom the philosophers of science I have mentioned are simply sceptics — they are simply succumbing, thought in a different area, to the line of argument which says that since we can't be certain, we shouldn't believe there is a table in front of us.

One strategy by which mainstream philosophy has sought to block scepticism is by questioning whether certainty is not too strong a requirement for knowledge. Certainty, in the sense I have been using it, is the requirement that our beliefs should issue from processes of thought that cannot possibly go astray, that is, from processes of thought that would still lead us to the truth even under such extreme possibilities as that all the evidence has cunningly and precisely been arranged by an evil demon to make things seem other than they are. But this is an extremely strong demand on legitimate belief, and a significant number of mainstream philosophers think that the appropriate requirement for legitimate belief is rather weaker, namely that our beliefs should issue from reliable processes, processes which in general do succeed in giving us true beliefs, given the way the actual world works, even if they would lead us astray in a world which was manipulated by an evil demon.

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My suggestion, then, is going to be that the philosophers of science would do well to follow those mainstream philosophers who seek to replace certainty by reliability as the appropriate requirement for knowledge. But I think there is

also room for some two-way traffic here: that is, I think that, on the question of this choice between certainty and reliability, the philosophers of science can also help the mainstream epistemologists.

The preference for reliability over certainty is by no means unanimous among mainstream epistemologists. One reason for this is that within mainstream epistemology this issue is normally regarded as a question about the concept of knowledge: does “knowledge”, as most people think of it, involve the stronger requirement of certainty, or only the lesser requirement of reliability? And when we view the issue in this way there is undoubtedly some evidence for the requirement of certainty — for it is not difficult to elicit, especially from those with some philosophical training, intuitions in support of the view that any element of uncertainty undermines knowledge.

Now, there is nothing wrong with the enterprise of analysing the everyday concept of knowledge. But it seems to me a mistake to place this enterprise of conceptual analysis at the centre of epistemology. This is not how Descartes or Hume or Kant raised the fundamental questions of epistemology. And neither, for that matter, is it how contemporary philosophers of science think about methodology. Rather than analysing the concept of knowledge, they by-pass issues of conceptual analysis and ask directly, like most of the great philosophers of the past, what, if anything, we can do to ensure that our beliefs are true, that is, what procedures, if any, will be effective in giving us true beliefs.

If the mainstream epistemologists were to follow the philosophers of science in taking this to be the central question of epistemology, rather than the analysis of the concept of knowledge, then it would become much easier for them to see that reliability is a more appropriate requirement for legitimate belief than certainty. Certainty would perhaps be nice, if we could get it — if only because it would enable us to stop worrying about manipulative demons. But, be that as it may, reliable processes are uncontentiously fully adequate if your primary aim is acquiring true beliefs — for, by definition, a reliable process is one which generally delivers true beliefs in this world, even if not in a demon-infested one.

(Let me add a parenthetical point here. I think that these last considerations make a concept of knowledge that requires reliability rather than certainty a good concept of knowledge, even if it is different from the everyday one. That is, I think we have a reason here for replacing the everyday concept of knowledge, if it does indeed call for certainty, with a concept of knowledge which does not. This anyway is how I shall use the word knowledge in the rest of this lecture.)

There is a further respect in which the mainstream epistemologists would do well to learn from philosophy of science. A second reason why certainty, rather than reliability, continues to be upheld as a requirement for knowledge by many mainstream epistemologists, is that they have an alternative programme for

explaining how this strong demand can be met. I am thinking here of idealist, or verificationist, or, in Michael Dummett's terminology, anti-realist theories of judgement, which seek to argue that our claims about the world do not really answer to features of an "external reality" beyond our immediate apprehension, but simply to the detectable symptoms on the basis of which we in fact assert such claims — detectable symptoms the presence of which we can arguably identify with certainty.

This is not the occasion to debate the merits of this programme in any detail. But I would like briefly to observe that it quickly loses plausibility once we turn from the kinds of judgements that first-year philosophers cut their teeth on, to those that are the common currency of philosophy of science. That is, it is not entirely implausible, *pace* Dr Johnson, to hold that judgements about stones, or tables, or hands, are equivalent to judgements about the availability of certain perceptual evidence about which we can be certain. But when we turn to universal laws of nature, or theories about unobservable mechanisms, then it seems quite inescapable that the content of these claims outstrips any evidence of which we can be certain, and that if we are to avoid scepticism about such claims it must be by weakening the demand for certainty, rather than by pretending they are simply claims about perceptual evidence.

There was a time, earlier in this century, when anti-realist accounts of the content of unobservable claims did have some currency among philosophers of science. But the implausibility of this position is now generally recognized, and no contemporary philosophers of science still think they can block scepticism by anti-realist arguments.

True, as I have said, most philosophers of science conclude that they can't block scepticism at all, and simply give in to it. But at least they recognize that the anti-realist option is not open. And on this specific point the mainstream philosophers would do well to follow suit. For, if they recognized that anti-realism is no answer to scepticism about universal generalizations and unobservable claims, they would see more clearly than they do that the only viable anti-sceptical strategy is to weaken the requirements for knowledge.

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Once we do accept reliability rather than certainty as the appropriate requirement for knowledge, the effect on the philosophy of science is liberating. As we have seen, the general argument which has persuaded contemporary philosophers of science that we are not entitled to believe any scientific theories is that there will always be some logically possible alternative consistent with the evidence for theories like the atomic theory of matter. Now, this observation would be damning if we needed certainty, because the existence of such alternatives means we can't be certain the atomic theory is true. But if all we want

is reliability, the existence of these alternatives is in itself no argument against belief at all. For, provided the standard scientific practice of ignoring outlandishly Heath-Robinson theories, or conspiracies by evil demons, is in fact a reliable route to the truth, then scientists will succeed in arriving at true theories. That scientists would be led astray, in a world inhabited by evil demons, or by complex contrivances which produced conspiratorial evidence, is no reason whatsoever to conclude that they are led astray, in the actual world.

At this point a number of you will no doubt be wanting to ask how I know that established methods of theory choice, methods that discount outlandish alternative explanations, are in fact reliable routes to the truth. Now, some philosophers of reliabilist inclinations are inclined to respond to this challenge by observing that we do not need to know that we know, in order to know. But I don't think this will do. It is of course true that not all knowers need to know that they know, in order to know: as a general demand, this is clearly viciously regressive, and therefore self-defeating. But in the specific context at hand the request for some assurance that scientists do know is quite legitimate. For in the present context I am not just a first-order knower with views about tables or atoms or whatever. Rather I am a philosopher, aspiring to an answer to a specific second-order question about knowledge — I am aspiring to establish that scientific knowledge is possible, and indeed actual, on the grounds that the methods that scientists use are reliable for truth in this world. And given this aim, it is surely perfectly legitimate for you to challenge me, and ask what basis I have for my crucial premise that scientific methods are reliable for truth.

Still, while I accept this challenge, I think it is easily enough answered. The way we can find out that the methods used by scientists are reliable, is that science itself tells us so. Part of what you learn, when you become expert in any field of science, is which methods will be effective at answering that science's theoretical questions. In effect, you learn which kinds of possible answers need to be taken seriously as candidates to questions in your fields, and which kinds of answers can be discounted; and consequently you learn what kind of experimental investigation is needed to decide between the serious possible answers to your questions. For example, in medical science, you learn which kinds of agents — viruses, bacteria, parasites, nutritional deficiencies, metabolic malfunction, environmental causes, and so on — could possibly be responsible for which patterns of spread of which kinds of symptoms. Because you know this, you know what kinds of empirical data will suffice to identify the actual causes of given sets of symptoms. That is, your theoretical knowledge of possible causes for ailments implies that certain strategies of investigation will be reliable routes to true conclusions about the causes of diseases. And, of course, medical scientists have used just such strategies to establish the causes, and in some cases the preventions and cures, for a wide range of ailments, from tuberculosis, to smallpox, to gout.

This kind of story will, to many philosophically trained ears, make scientific discovery sound far too easy. What has happened to the well-known difficulties of inductive interpolation, to the impossibility of drawing any unique line through any finite set of data? However, I do not think it a demerit in the position I am defending that it pays little regard to these traditional difficulties. For, while science is often difficult, it is not that difficult. After all, scientists have established that gout, for instance, is caused by an excess of uric acid, and that it can be prevented by a drug, allopurinol, developed by the Wellcome Foundation, which blocks the action of the enzyme xanthine oxidase — a fact for which, as a member of a family highly prone to gout, I am extremely grateful. And this is a fact, a well-established fact, and not just a bold conjecture which is no doubt false. If any philosophical position is to be dismissed out of hand here, it should not be an account, like mine, which implies that scientific knowledge is possible, but those positions which urge, absurdly, that such knowledge can never be achieved.

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At this point I would like to digress briefly and say something about the relation between the approach I am defending and the recent revival of Bayesian ideas about scientific methodology. I have had in mind so far, though I haven't made it explicit, that reliable methods are sure-fire guides to the truth, that reliability is a matter of always delivering true beliefs. The contrast between reliability and certainty is not that reliability sometimes led us astray, whereas certainty doesn't, but rather that while reliable methods in fact always deliver truths, given the way the actual world works, certainty demands methods couldn't but deliver truths, in any crazy world whatsoever. Because of this, I think that if you do have reliable methods, you can still be fully confident in your beliefs, you can bet your life on them, even if your beliefs aren't certain in the somewhat technical sense I have been using. So reliability, as I have been using it, is still a pretty strong requirement. Indeed it is too strong to be a characteristic of all worthwhile scientific methods. What you will in fact discover, when you become expert in any scientific field, is that, while some scientific questions can be answered by sure-fire principles, others will only be addressable by procedures that usually give the right answers, or do so more often than not. And when we need to resort to such methods of of less than perfect reliability, the obvious corollary is that we shouldn't be fully confident of our answers, but should only attach a limited degree of belief to them, a degree of belief commensurate with the degree of reliability of the method.

In this respect I take the kind of reliabilist methodology I have been advocating to incorporate the Bayesian doctrine that beliefs come in degrees with the structure of probabilities and should be treated accordingly. At the same time, I

take reliabilism to provide an objective framework for Bayesianism — which is something Bayesianism sorely needs, since without any such objective basis Bayesianism faces the complaint that the degrees of beliefs it deals with, and its procedures for revising them, are in essence arbitrary, and that therefore it provides no real alternative to scepticism.

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Let me return to my main line of argument, and to some final remarks. I have argued that science itself can tell us that scientific methods are reliable. Some of you may, not without reason, detect a whiff of circularity here. I have argued that the practitioners of any given discipline will know that their characteristic methods of investigation are reliable, because their general assumptions about the working of the world imply this. But don't they need to use those methods of investigation in the first place, in order to acquire their general assumptions about the working of the world?

I agree. And of course this involves a kind of circularity. But I don't think this kind of circularity is at all damning. The challenge raised some while ago was to provide grounds for the claim that scientific procedures of theory choice are reliable. This question, of the reliability of procedures of theory choice, can be viewed as a straightforward empirical question: is it in fact the case that people who form beliefs in such-and-such a way will in general arrive at true beliefs? And, if we do view it in this way, then what is more natural than to answer it by using those methods that we use to answer empirical questions in general, namely, observation, experiment, and existing strategies of theory choice? It is not as if the challenge I am facing involves any argument against these methods of investigation, any rationale for discarding them as a way of answering questions. So why not use them to answer this question, the question of the reliability of methods of scientific investigation? After all, how else should we answer it? We must be allowed to employ some means of thought if we are to have any chance of answering it all. And so why not use those means that are normally used to answer such questions, given, once more, that there are no arguments on the table against those means of thought?

This use of scientific strategies of investigation to answer the question of the reliability of scientific methods obviously has its limitations. It will not do much to persuade thinkers who do not yet employ such methods of thought, or who are attached to alternative methods of thought, of the attractions of the scientific approach. For, while such alternative thinkers would have a good reason for switching to scientific methods, if they concluded that those methods were a reliable source of truth, they will not be persuaded of this conclusion, if the route to it depends on the prior use of scientific methods.

Still, why should we regard it as a condition of satisfactorily demonstrating that scientific methods are reliable, that this demonstration be capable of persuading any conscious intelligence whatsoever, however limited or benighted its initial habits of thought? This is, if you think about it, a great deal to ask of such a demonstration, and by no means an obvious consequence of the challenge raised earlier, which was simply to show, by normal standards, for all that was then said, that there is reason to suppose scientific methods reliable.

I realise that our philosophical tradition normally assumes that any satisfactory vindication of a claim to knowledge ought to be able to convert any consciousness whatever, however limited its initial modes of thought. But I think we ought to stop and ask why we should hanker for such a vindication, for some external pivot by which we can lever ourselves and anybody else into an endorsement of normal strategies of investigation. It is, I have agreed, perfectly sensible to ask whether such strategies of investigation are reliable sources of truth, and to seek an answer to this question. But it is not obviously sensible, and indeed threatens to be intellectually paralysing, to add that this answer must be produced in some way which does not itself utilize normal methods of thought. If we aren't allowed to use such normal methods, how are we to proceed?

It is an interesting question exactly why our intellectual tradition hankers for some external standpoint from which to force universal assent. But this is not the time to pursue this question. Let me simply state my own view on this matter. I think that the expectation that we can find some such external standpoint is a by-product of the assumption that knowledge requires certainty. For if we do assume that knowledge requires certainty, then it quickly follows that the legitimate sources of knowledge are highly limited, to, at best, introspection of our own minds and intuitively compelling steps of deductive logic. Any more complex route to knowledge will be legitimate only insofar as it can be built up from such introspections and deductions. And so, any demonstration of the legitimacy of a route to knowledge will arguably be compelling to any consciousness, however benighted, for it will show how this route reduces to introspective and deductive steps, steps that are arguably inescapable for any conscious mind. But, as I said, all this follows only if you accept that certainty, in the strong technical sense, is a requirement for knowledge. This whole rationale for an external standpoint which will force universal assent falls away once we switch from certainty to reliability.

The lack of an external standpoint that will persuade anybody to think scientifically has an interesting historical corollary. A commitment to scientific methods of investigation is by no means an automatic or inevitable part of human nature. After all, it is only for a fairly short period of human history, the last four hundred years or so, and only within a fairly narrow band of societies, that such methods of investigation have come to the fore. Suppose we ask, in the light of

this, "Why have these methods of investigation become accepted in this narrow portion of human history?" The natural answer, the answer many of us would unthinkingly give, is that in the scientific revolution the mists of superstition and prejudice were somehow finally swept away, and this allowed reason finally to discern the appropriate route to scientific truth. But the position I have been defending gives the lie to this simply story. If there is no external standpoint from which the worth of scientific methods can be rationally demonstrated, then our intellectual ancestors could not have levered themselves rationally into scientific ways of thought. Rather, and this is of course a rather more plausible story anyway, given the length of time the human race had to wait, the emergence of the scientific attitude must have been merely a matter of historical happenstance, due perhaps to the revival of Platonism and its search for simple underlying forms, together with the Reformation's readiness to question authority and test established views against the facts.

This happenstantial explanation implies that our intellectual forefathers in the scientific revolution didn't figure out how to do science by being cleverer than their predecessors. Rather they were just lucky that the right influences were available to set them on the scientific track. To some people, and in particular to those philosophers who place overly strong demands on knowledge, this happenstantial origin for science may seem to belittle its worth, to make it seem just one possible way of approaching the world among others. But I think this is the wrong response. Science may in this sense be just a matter of luck. But it is a lucky break for which we should be thankful, not ashamed. For it is a piece of luck that has enabled us to discover a great wealth of truths we would otherwise be ignorant of.