O' Hear

6
Scientific Realism

Positivism

At the end of the last chapter, we began to look at the difficult problems which arise when the theories of science postulate the existence of unobservable entities and forces. In this chapter various reactions to the postulation of unobservables in science will be considered, starting with the approach known as positivism. This approach pushes further the scepticism about the unobservable we picked up in Berkeley and Hume, and indeed it may be regarded as the spiritual heir of their empiricism.

Ian Hacking has usefully identified six key ideas characteristically associated with a positivistic approach to natural science.[1] Although not all philosophers who accept one or more of the ideas automatically accept all six, the six taken together do constitute a consistent and coherent position, and one which specifies a particular attitude to science and its problems.

The first idea is an emphasis on verification and/or falsification. The point here is that a significant theory about the physical world should make some difference to experience. It should specify or predict observable states of affairs, and it should be capable of conflicting with observable evidence. This might seem at first sight to be a fairly minimal require-

[1] In his *Representing and Intervening* (Cambridge University Press, 1983), pp. 41–57.

ment, until we reflect on the number and extent of influential ideas which fail to meet it, from religious dogmas through political ideology and ethical beliefs to important and fruitful ideas in science itself. Into this last category will fall not only obviously metaphysical propositions, such as 'Every event has a cause', but also ideas which have suggested something about the basic form of the natural world, without being themselves either verifiable or falsifiable. One may think in this context of atomism (the idea that at bottom the world consists of tiny indivisible particles and their billiard-ball-like movements and configurations) or of mechanism (the idea that there is ultimately no action at a distance, and that all physical processes resemble the workings of clocks). Atomism and mechanism share with determinism the properties of unverifiability and unfalsifiability. That is to say, we can never prove that they are true, for fairly obvious reasons, but neither can we falsify them. Even if a leading current theory is indeterministic or anti-corpuscular or unmechanistic, one may dream that, in the end, causes, atoms, and mechanisms will be found. And such dreams have inspired much positive research, and have led to theories which have been empirically verifiable and falsifiable. So the positivist cannot simply say that all non-verifiable, non-falsifiable theories are without significance, even in the context of natural science. But the account the positivist is inclined to give of such theories will tend to stress their heuristic and hortatory significance, and to play down their claims literally to represent the world.

The second positivist idea noted by Hacking is the thesis that sensory observation founds all genuine knowledge. Traditionally, positivism has often been associated with attacks on religion and metaphysics, and attempts to show that, in contrast to natural science, such enquiries cannot issue in real knowledge. So it would be something of a disaster for the old-fashioned anti-religious positivism if it turned out that natural science made significant use of what

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appears to be non-observational knowledge and claimed such knowledge. We saw in the last chapter that science does appear to give us knowledge of things and processes which cannot be observed, to say nothing of the influential metaphysical theses we find in the history of natural science. So, whatever broader view may be adopted on religion and metaphysics, the positivist will have to deal to his own satisfaction with the claims to knowledge of the unobservable made in modern science.

The third positivistic idea can be traced straight back to Hume, for it is the claim that talk of causation amounts to no more than talk of constant conjunctions between types of event. As Hacking points out, Newton himself, with his rather ambiguous attitude to the force of gravity, helped to smooth the way for a more philosophically inspired scepticism regarding physical necessity; certainly some of Newton's contemporaries, including Leibniz, saw gravity as an illicitly unexplanatory and inexplicable occult power and rejected it, except as an indirect way of referring to observed regularities in the world.

The fourth idea connects with the positivist hostility to causes. It is a suspicion of the role, and even of the possibility, of deep explanations in science. If there is no physical necessity forcing events to happen, and all we have in the world are mere regularities between types of event, then the most we can do by way of explanation is postulate wider ranging regularities. We can see a single event as part of a pattern of regularity, and our first pattern of regularity as part of a wider pattern. But in this wider pattern, all we will have are brute regularities; there will be no sense that the phenomena in the wider pattern have to be connected, or be, as they are. And if there is no necessity at the deepest level, there will be no necessity at any other level. If people hope that a true or deep explanation in some sense explains why things have to happen, then, says the positivist, that hope will not be satisfied in science, at least, when science is

properly understood. And—some positivists will add—if all we are ever really going to be offered in science are mere regularities between types of event, what is so good about regularities between unobservable events? Why do we not rest content with observable regularities?

The fifth positivist thesis is perhaps the most characteristic. It certainly is the most controversial and the one most likely to put most people on their guard. It is a thoroughgoing hostility to unobservable or theoretical entities. The hostility to causes and physical necessity may have softened some people up for this, but it does seem obvious that the history of science has continually progressed from the postulation of entities to their observation and manipulation. Hacking mentions molecules, genes, and viruses. The lesson would appear to be that if science has, precisely through the postulation of theoretical entities, brought us to be able to observe what we could not previously observe, we should not start with scepticism (or worse) about such things. Is the history of science not very much a history of the unobservable becoming observable, and a consequent blurring of the observation–theory distinction?

We will not be able to evaluate the positivist position on theoretical or unobservable entities fully until we have considered the anti-positivist or realist arguments in favour of accepting their existence. For the positivist would surely be right to insist that some argument is needed before we accept that there are unobservable things, as human experience, broadly conceived, is our only touchstone of reality. Raising doubts about unobservable existences is not prima facie unreasonable. It cannot be compared to global or Cartesian scepticism. And the positivist need not be dogmatic or inflexible about observation. He could say that science has extended our powers of observation in many ways, and accept much of what he sees through telescopes and microscopes, say, as observed. What he will probably do at this point is to remain somewhat open-minded about where to

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draw the line between theory and observation, whilst insisting with van Fraassen that such things as space-time, fields, elementary particles, and alternative possible states of affairs are definitely not observable,[2] and should not be regarded as on a par with observable entities. In so far as accepting the real existence of such things depends on our accepting certain controversial and highly theoretical theories, our reasons for accepting the entities can be no stronger than our reasons for accepting the theories. In fact, as we shall see in the next section, when we have theories which greatly transcend any possible observational basis, the reasons for accepting either theory or postulated entities may not be very strong.

Hacking's final mark of the positivist is that he is opposed to metaphysics. We have touched on this aspect of positivism several times already, and no more needs to be said about it now, beyond making the obvious point that where the anti-positivist will point to the cognitively significant input metaphysical ideas make to the construction of empirical theories, the positivist will interpret the significance of metaphysics in heuristic terms, as forms of explanation simply useful in guiding empirical research. But he still has a problem in explaining why some forms of explanation, such as atomism and mechanism, have proved highly successful in our investigations of the real world. In fact, as we are now beginning to see, much of the dispute between positivists and their realist opponents hinges on the different conclusions they would be inclined to draw from the fact that a certain theory is, at a given time, a good explanation of the observational data.

[2] Cf. *The Scientific Image* (Clarendon Press, Oxford, 1980), p. 202. Many of the subtler points I make in this chapter in favour of positivism are derived from van Fraassen.

The Inference to the Best Explanation

The scientific realist believes that the theories of science give us knowledge about the unobservable. If his realism is to have any bite, he will not simply believe that the theories of science make statements about unobservable things. He will also believe that we sometimes have good reasons for believing that those statements are true.

What reason could we have for believing that statements about unobservable entities and powers are true? The evidence here will obviously be indirect as we cannot directly observe these things. But, says the realist, we may on occasion have good indirect evidence for believing in them, for believing that some of the things we can observe are manifestations of, or effects of, unobservable entities or forces. The majority of arguments of this sort either are versions of what is known as the 'inference to the best explanation', or in some way rest on this.

The thought which underlies the inference to the best explanation is that if a theory explains some data better than any other theory explains them, we have thereby a good reason to think it true. In the inference, explanatory power is taken to be a reason for belief. In Conan Doyle's stories, the explanations of the evidence given by Sherlock Holmes were always more complete and all-encompassing than those initially given by Watson or the police, who usually failed to account for some awkward fact or other. The very completeness of Holmes's explanations *qua* explanations gave them a probative force lacking in the other accounts. And this probative force was characteristically corroborated by a confession from the guilty party or something equally incriminating. The inference to the best explanation was both deployed and corroborated in the stories.

Is the situation in science like that in a fictional crime story? In two highly significant ways, the cases are quite different. In the first place, Conan Doyle is writing fiction.

He is able, by artifice, by hint and emphasis, and, indeed, by the nature of the genre itself, to suggest to the reader that Holmes and only Holmes has taken full cognizance of all the salient evidence. The slower witted doctor and policeman, the reader realizes, always overlook some vital clue, or fail to account for it. We have, in other words, a pretty good sense that Holmes does always provide a better explanation of the mystery, even before his evidence is triumphantly endorsed in the denouement. And, of course, it is endorsed. We get in the end a direct proof or admission of what Holmes had indirectly argued for.

In the scientific case, however, the best explanation is not always so clearly signposted. An explanation which does well in one respect may not do so well in another. An explanation which is simple in respect of the formulae used or the number of basic types of entity postulated may not be very accurate. On the other hand, an accurate explanation may be very complicated and difficult to apply and understand. Also, various explanations may make different estimates of just which salient bits of evidence need explaining.

But even if we could unambiguously and uncontroversially establish that one explanation of a given phenomenon was the best of all available competitors, we would still, in respect of the unobservable entities postulated, lack the direct Holmesian admission or confession. The unobservable entities would only be inferred in virtue of the explanation they are a part of. And how strong an argument is that in their favour? We are left with the feeling we noted in connection with scientifically influential metaphysics, that it cannot be merely coincidental that a particular metaphysical or theoretical account actually fits the facts so well. There *must* be some truth in it, over and above its just fitting the observable facts. Popper writes in this vein of 'realists' who

> not only assume that there is a real world but also that this world is by and large more similar to the way modern theories describe it than to the way superseded theories describe it. On this basis, we can argue that it would be a highly improbable coincidence if a theory like Einstein's could correctly predict very precise measurements not predicted by its predecessors unless there is 'some truth' in it.[3]

What those who, like Popper, are inclined to argue in this way need to spell out is just what this 'some truth' in Einstein amounts to. Obviously, what is intended is that there must be some truth at the deep theoretical level: that its ability to make correct predictions of very precise measurements underwrites its picture of the inner and unobservable workings of the world. The positivist, on the other hand, will argue that even very good results at the level of empirical predictability in themselves give us little reason for belief that the world is just as the predicting theory says it is. We cannot, in other words, argue from explanatory power to truth.

The positivist position can seem impregnable once it is realized that conflicting explanations at the deep theoretical level can be equivalent at the level of empirical observability, that they can be empirically equivalent, in other words. And for this to apply, we do not need actual examples of conflicting explanations. Their mere possibility is enough to show that one cannot go directly from explanatory power to truth, because we could get another and inconsistent theory to explain the same data on a completely different theoretical basis. And this will be true whatever we take to be our empirical basis, so the point is not affected by problems involved in the precise demarcation between the theoretical and the observational.

While, from an abstract point of view, the positivist position can seem unassailable, a natural reaction here is that in science we are not involved in mere abstractions. We are concerned with specific highly complex and concrete theories, of a type which have had and continue to have great

[3] K. R. Popper, 'Replies to Critics', in P. A. Schilpp (ed.), *The Philosophy of Karl Popper* (Open Court, La Salle, Ill., 1974), ii. 1192–3.

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success in dealing with the empirical world, and in leading to all sorts of discoveries in that world. These theories are in many ways continuous with our considerable empirical knowledge of the world and seem to flow seamlessly from it. The very problem just alluded to, of demarcating the observational from the theoretical, suggests that there is something highly artificial in, for example, regarding what we see in a powerful light microscope as observed, while disallowing evidence from electron microscopes as 'theoretical'. And what are we to say of evidence gleaned from polarizing microscopes, in which certain coincidences between light rays and particular cell fibres are exploited to allow us to 'see' certain normally transparent fibres of living organisms?[4] The point here would be that it is legitimate to use any relevant property of any kind of wave, including even acoustic waves (as we see in the case of ultrasound), in order to create images of structural features of organisms and other entities. If real properties are imaged in this sort of way, who is to say that observers are not *seeing* what is presented?

The fluidity of the distinction between what is really observed and what is merely inferred is, and is likely to remain, the Achilles' heel of the positivist. In his defence, two points can be made. He is not committed to maintaining an absolute and timeless distinction here, nor to saying that the unaided evidence of our senses is our only way of observing the world. He can certainly allow extensions and corrections of our unaided senses by means of instruments, nor are such extensions to be confined to cases like those of Jupiter's moons, in which Galileo's telescope might be thought of as simply anticipating things which were in principle humanly observable without artificial observational aids. Molecules and cells are never going to fall into that category, and even less are chromosomes or electrons; so, if

[4] On this point, cf. Hacking's *Representing and Intervening*, at p. 197. On microscopes generally, cf. the whole of the relevant chapter in this work.

we can observe them at all, it is only going to be with instruments.

Does this mean that, according to the positivist, such things are forever unobservable, and hence, forever only inferred or theoretical? The positivist, surely, does not need to say anything so extreme. He can say, as Hacking does in his chapter on microscopes, that the observable realm can grow through instrumentation, when certain conditions are satisfied. Basically, what we want when observing through instruments, as with any other form of observation, is an assurance that what we observe is caused by reality, and not due to aberrations of our instruments. And we do get some assurance of this sort when we find that physically completely different instrumental techniques all come up with much the same picture of the reality observed. As Hacking says, 'light microscopes, trivially, all use light, but interference, polarizing, phase contrast, direct transmission, fluorescence and so forth exploit essentially unrelated phenomenological aspects of light'.[5] The conclusion is that it would be a 'preposterous coincidence' if two (or more) completely different kinds of physical systems were to produce the same arrangements of phenomena on their screens or micrographs or whatever.

Now this argument from cross-checking is not the inference to the best explanation. As yet no explanation or account of the microscopic phenomenon which causes the variously observed effects is at issue. What we have here is simply an extension of the way we appeal to cross-checking of sensory sources in the macroscopic world. Our sense of sight is corroborated, and at times corrected, by our sense of touch, and the argument gets stronger when we are able to interfere in predictable ways with the things observed. Evidence from different senses and different observers is mutually reinforcing and correcting, in such a way as to lead

[5] Ibid., pp. 203–4.

to a good sense of those occasions on which what we observe is due to aberration on the part of our sensory organs, rather than produced by direct interaction between an object and, as Hacking puts it, 'a series of physical events that end up in an image of an object'.[6]

Given relevant cross-checking, then, there seems little reason in principle to refuse to say that one might on occasion even see by means of an acoustic microscope, in which bursts of sound are projected on to an object, and the responses transformed into an image on a screen. The seeing here may, in a sense, be indirect, but no more indirect than that involved in watching television, or in a mother seeing her foetus on a screen in front of her, by means of ultrasound. In all these cases, the image tracks the object. The image is caused by and varies according to the behaviour of the object. If the object were different or behaved differently or if we interfered with it in some way, the image would vary in relevant ways. If we are prepared to allow ultrasound observation in the case of a foetus, there seems little reason in principle to disallow the possibility of acoustic microscopes, providing their deliverances can be cross-checked by different types of light microscopes.

It is true that one is able to 'see' better when one has some idea of what it is one is seeing, and some idea of how what it is that one is seeing functions. Hacking points out that the early electron micrographs of genes could not properly be recognized as such before there was some conception of what functions were played by the various bands and interbands on the chromosome.[7] And, as he adds, the same would be the case for a Laplander stranded in a Congolese jungle, who would make very little sense of what he saw around him. If we say that our assurance that we are really observing something increases when we have an explanation for what it is we are observing, we are not thereby committed to the

[6] Hacking, *Representing and Intervening*, p. 207. [7] Ibid., p. 205.

inference to the best explanation, nor to thinking that our explanation receives any great support from what it is we now believe we are observing. For, as we noted earlier in connection with Kuhn, phenomena, once established, can survive changes of explanation, even changes of those very explanations which were instrumental in originally picking out and identifying the phenomena. Explanations and theories may well be helpful in leading us to organize our data, and observe our world. But explanations and theories which are not true can play this role, and once identified via some theoretical perspective, a given phenomenon can become well enough established through cross-checking and manipulation to survive abandonment of the original explanatory framework.

What I am trying to suggest here is that the positivist—or, at least, someone who finds much of the positivism outlined in the previous section persuasive—can go quite a long way in allowing that we can observe many things which we cannot perceive with our unaided senses without giving up his suspicion of full-blooded scientific realism. In particular, he does not have to accept the inference to best explanation, because we do not necessarily need this inference to establish the existence of sensorily unobservable phenomena. What the inference to best explanation attempts, controversially, to do is to establish the truth of explanatory theories just where they go beyond the evidence, however the evidence is conceived. And this leads us to our second consideration in favour of the positivist. The fact that the boundary between the observational and the theoretical is not clear cut, and that it may shift with improved instrumentation, does not show that the positivist is wrong to think that there are some cases which definitely fall on the theoretical side of the line. What we want to know is what we are to say about those entities and forces mentioned in scientific theory which cannot be observed at all, but are at best inferred from their effects (according to our theories). And this question remains

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pressing now, even if at some later stage we might get more direct observational knowledge of them.

Van Fraassen, in his attack on it, takes the inference to best explanation to be of the form: where we have evidence E, and hypotheses H and H′, we should infer H rather than H′ if and only if H is a better explanation of E than H′ is.[8] Van Fraassen mounts a powerful case against any such inference, and hence against belief in any entities established *only* by the inference. But his case, which we will now examine, would not count against (initially) unobservable entities if (as I have just suggested) there might be other ways of establishing their existence. Van Fraassen himself is undoubtedly far less tolerant than we have been of any of the type of extension of observation we have been suggesting, but in a way this is beside the point. His attack on the inference to best explanation still raises considerable problems for anyone tempted to full-blooded scientific realism.

What van Fraassen argues is that all we are entitled to assert by any evidence which supports a hypothesis about the unobservable is that things are *as if* there were forces or photons or electrons, or whatever the unobservable entities in question are. The mere fact that postulating unobservable entities provides some explanatory account of some observed regularity is in itself no reason for belief in the entities. Even if our explanatory hypothesis is the best available, the evidence actually entitles us to say no more than things behave as if they were brought about by whatever our explanatory model postulates.

At this point the realist will probably introduce two further considerations. He will bring in the improbable coincidence argument and also assert that any natural regularity needs explanation, if necessary at the unobservable level. Van Fraassen is able to make rather short work of the second point, showing that such a demand for explanation leads to absurdity. If we 'explain' our observed regularities

[8] Van Fraassen's positivistic attack on the inference to best explanation is in *The Scientific Image*, pp. 19–40.

by postulating unobserved forces and entities, these too will act according to the regularities mentioned in our deep theories, and so themselves need some further explanation. The feeling van Fraassen evinces is that if one is going to be stuck with brute regularities, we might as well stick at the level of observed regularities, rather than pushing our demand for explanation unnecessarily down into the unobserved level.

Van Fraassen is also able to show that an unrestricted demand for explanation of regularity, when it takes the form of the search for a common cause of a particular regularity, falls foul of orthodox quantum physics. The types of instantaneous and correlated action at a distance discussed in the Einstein–Podolski–Rosen paper of 1935 do in fact exist, and are not, in terms of orthodox quantum physics at least, further explicable. That is to say, in quantum physics we get cases where two particles acting randomly *and* independently of each other actually mirror each other's behaviour. One such case has been demonstrated by A. Aspect in a series of experiments reported in 1982.[9] Pairs of photons are emitted from opposite sides of calcium atoms. Each travels via a rapidly changing switch to one of a pair of polarization filters beyond. The switches are not correlated, so only random coincidences in the ultimate behaviour of pairs of particles would be expected. But if one particle reaches a given filter on its side, the chances that the other reaches the corresponding filter on the other side are far greater than they should be on the assumption that the behaviour of one particle is independent of that of the other. While we would naturally want to say that there must be a common cause to explain such a regularity, orthodox quantum theory does not permit the postulation of some hidden variable to explain this puzzling phenomenon nor can we think of the particles communicating with each other faster than the speed of light.

[9] A. Aspect, J. Dalibard, G. Roger, 'Experimental Test of Bell's Inequalities Using Time-varying Analyzers', *Physical Review Letters*, 49 (25), 1982, pp. 1804–7.

In van Fraassen's view, there may be a point in working with explanations of observed regularities, interpreting the regularities as if they were brought about by some unobservable cause. Such explanations can lead us to discoveries of further regularities and entities at the observable level—and, we could add, to pushing back the acceptable limits of observation. But if an explanatory theory does this, would it not then be a highly improbable coincidence if it were actually not true? In other words, does not a theory which, through its explanatory account of reality, actually leads us to more empirical knowledge, gain some degree of confirmation or truth-likeness, by virtue of that very fact? The realist is arguing here that the success of the theory is to be explained in terms of its truth or probability, meaning that what it says about the unobservable world has a good chance of being true.

The positivist will, of course, reply that the most that can be derived from knowledge that a theory is empirically adequate and knowledge-increasing is that it has these properties, and that the world is such as to be as if it were as the theory states. Without assuming some deep isomorphism between the mind and the world, we are entitled to conclude no more. And we can explain the success of theories in science without supposing any such thing. Van Fraassen writes about this in Darwinian vein: 'the success of current scientific theories is no miracle. It is not even surprising to the scientific [Darwinist] mind. For any scientific theory is born into a life of fiercest competition, a jungle red in tooth and claw. Only the successful theories survive—the ones which *in fact* latched on to actual regularities in nature.'[10] This Darwinian metaphor is, in fact, worth taking rather further than van Fraassen does.

A scientific theory can be compared to a species produced by biological evolution in that both can be regarded as

[10] *The Scientific Image*, p. 40.

embodying attempts to cope with and anticipate the environment, to fit with it at the empirical level, so to speak. Successful theories, like members of successful species, do this of course, or at least they both do it enough to survive. But they both do it without any direct instruction from the environment. That is to say, neither the genetic structure nor the inner core of a theory are directly moulded by the environment, and this is true even though scientific theories, unlike biological mutations, are guided by rational considerations. Even though the rational and problem-solving aspect of scientific theories tends to be overlooked by those pressing analogies between science and biological evolution, scientific theories do have a strong element of guesswork about them. They may not be completely blind and undirected, like biological mutation, but both can be regarded at least to some extent as leaps in the dark. Both are guesses, which will be weeded out if they are too wide of the mark empirically. But, precisely because they are leaps into unknown—random genetic sports, initially, or imaginative leaps, in the case of theories—their fitting the environment reasonably well at the points of empirical control presupposes no mirroring of the environment at the level of the genetic code or at that of the theoretical model of the environment. Equally, lack of isomorphism between the theoretical core of a theory and the underlying structure of the world will not preclude accurate anticipations, in either species or theory, of initially unforeseen regularities. In fact, as van Fraassen hints, competition between theories or species may help fairly quickly to pinpoint those theories or species which are able to anticipate new actual regularities. The 'some truth' Popper and other realists are looking for in their successful theories can, not implausibly, be seen as this ability to anticipate as yet unknown facts and regularities. But, once again, this ability can be detached from the truth of the theory at the theoretical level, or precise mirroring of the world by the theory at that level.

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This conclusion could reasonably be resisted by the realist if there were greater theoretical convergence in science than in fact there is. Thinking of scientific theories as if they were biological species might seem to some to be misleading in that we think of science as progressing, while this is not the case in biological evolution. Moreover, biological species often occupy different ecological niches and so are not in direct competition, whereas scientific theories about a given area will be in direct competition. But if we confine our comparisons to scientific theories and biological species in direct lines of competition, there are close parallels. Assuming (which perhaps we shouldn't in the biological case, except for the point of our example) that an ecological niche has not altered too much over time, we might want to envisage later occupants of the niche as improving on the fit of their predecessors, as succeeding where their predecessors have succeeded in fitting the environment and in some other places as well. But to envisage this type of better fit in later occupants of a given niche, we do not have to envisage any very close resemblance between them and the earlier occupants they may have displaced. Indeed, they do not even have to be of the same line of descent.

Something like this piece of armchair biology actually represents the situation in science, and provides the biggest stumbling-block for those who would espouse full-blooded scientific realism. Explanations which were once successful have come and gone, but in their going, their explanatory core—their pictures of what the world is fundamentally like—have often gone too. The empirical knowledge they produced has remained, but without the explanatory accounts. No doubt the inference to the best explanation could have been applied in the past, with great rhetorical effect, to Newton's theory, say, or to atomism. But without the perspective provided by the idea of convergence of knowledge through scientific revolutions, its effect is rhetorical, rather than genuinely persuasive. What price inference to

the best explanation in our case, when, if the history of science teaches us anything, it is that even the best explanations of their time have been eventually overthrown, and many of their theoretical assumptions abandoned with them?

Scientific Laws and the Representation of Reality

In the previous sections we have seen that there are grounds for distinguishing between the empirical predictions and discoveries made by scientific theories and their theoretical core—the model of the universe the theories encapsulate. Empirical adequacy is no reason for thinking that the model advanced by a theory is true. We have, though, so far assumed that we are entitled to think of theories as empirically adequate, as giving fairly precise knowledge at the level of empirical measurement. In this section, we shall see that there are reasons for taking even this to be a somewhat idealized view of scientific theories, which, it turns out, even at the empirical level are often idealized abstractions.

In one very obvious sense of the term, it is clear that many scientific theories are idealizations. Take, for example, Newton's laws:

(1) every body continues in its state of rest or uniform motion in a right (i.e. straight) line unless it is compelled to change that state by forces impressed upon it;
(2) the change of motion of a body is proportional to the motive force impressed, and is made in the direction of the right line in which that force is impressed;
(3) to every action there is always opposed an equal reaction, or, the mutual actions of two bodies upon each other are always equal, and directed to contrary parts.

But Newton's law of gravity states that the attractive force between two bodies is proportional to the product of their masses divided by the square of the distance between them.

From this it follows that no object anywhere in the universe can be free of gravitational forces impressed upon it by other bodies. So no object anywhere in the universe can ever be in the state specified by the First Law.

Is the First Law, then, literally true? If it is, it is certainly a truth we can never check directly by observations. Its literal truth can never be confirmed by observation of even a single case, for no case actually falls under the condition specified, of being under the influence of *no* impressed force. And so we cannot check whether such a body actually does continue in its state of rest or of uniform motion, any more than, as we have already seen, we can check whether unaccelerated bodies have any mass or not. Thus neither the literal truth of the First Law nor the precise interpretation of Newtonian mass can be established by comparing the law or the concept with reality. But, if reality does not determine such things, what are we to say of such laws and concepts themselves? Should we regard them as in the business of giving literally adequate (or inadequate) descriptions of reality? Or should we look at them rather as tools or idealizations or abstractions, more or less useful, within certain limits, for calculating and predicting natural phenomena?

As we noted in Chapter 2, in discussing the achievement of Kepler, modern science has been much concerned with giving mathematical accounts of natural phenomena. Much of its strength, of course, derives from this drive to quantify, and from the successes achieved in the quantification of empirical phenomena. But, how far can we regard the workings of natural phenomena as being really capturable by mathematical formulae? Kepler's own work here was underpinned by his Pythagorean faith in number being the essence of the world. But does experimental work really bear out this faith, or do we rather find that things in reality only approximate to mathematical models, and do not follow them absolutely?

In abstract philosophical discussions of science, the extent

to which the laws and theories of physics are frequently idealizations, approximations, and simplifications is often overlooked. What is actually found in nature is far richer and more untidy than our theories assume, but we often ignore or regard as irrelevant those aspects of actual states of affairs which do not match our theories. Mismatches of detail are characteristically attributed to factors extraneous to what we are attempting to cover with precise theories, and which we have been unable to control. We have just seen that there are respects in which Newton's theories do not precisely map the observable world, but it is interesting to note, as Popper points out, that Newton regarded the solar system itself as imperfect when compared with what pure mathematics might have required.[11]

Popper goes on to quote C. S. Peirce, an experimentalist as well as a philosopher, to the effect that even the 'most refined comparisons' of masses and lengths, which far surpass in accuracy the precision of other physical measurements, 'fall behind the accuracy of bank accounts'. The determination of physical constants generally are, in Peirce's view 'about on a par with an upholsterer's measurements of carpets and curtains'.[12] There is, in other words, a tension between the untidiness of reality, and the simplicity and generality we look for in our theoretical accounts of that reality. Accounts which were not reasonably simple and general would not be widely applicable, but the price of simplicity and generality is idealization and approximation even at the observable level. Our theoretical accounts of nature often apply perfectly only in ideal and controlled situations. To apply them to real environments, we modify them in various ways, through so-called bridge principles, which tell us how to apply a theory in specific circumstances, through *ad hoc* corrections and the like. In order to manipulate nature and to

[11] Popper in *Objective Knowledge* (Oxford University Press, 1972), p. 212.

[12] Cf. Peirce, 'The Doctrine of Necessity', in his *Collected Papers*, Vol. 6 (Belnap Press, Cambridge, Mass., 1935), pp. 35–65, at p. 35.

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get some unified view of how things work in a particular natural domain, we envisage a nature consisting of clearly demarcated natural kinds, all the members of which obey the relevant laws by virtue of their common essence. The virtue of having such an account is obvious, as we suggested at the end of Chapter 3, particularly if our account more or less approximates to how things are. But we can have no a priori assurance that nature is really like that, and that it is not altogether more fuzzy and irregular.

The claim that nature is to a degree fuzzy and irregular has recently been defended by Nancy Cartwright in a collection of essays appropriately entitled *How the Laws of Physics Lie*.[13] She gives examples of how, as she puts it, facts are fitted to equations. In quantum mechanics, for example, free particles are represented as plane waves. Such a wave would naturally go all the way to infinity. But the square of the wave at a given point is what represents the probability of the particle being at a given point, so the integral of the square over all space must equal one. This, though, is impossible if the wave goes to infinity. One widely accepted way round this difficulty is to assume—contrary to fact—that the walls of the tube in which the particle is enclosed produce an infinite potential, from which it will follow that the particle is no longer to be seen in terms of an infinite plane wave.

Cartwright comments that we know that it is the case that there is a probability of one of the particles being in some finite region of space, while it is not literally true that the walls interacting with a particle produce an infinite potential. But 'it is not exactly false either. It is just the way to achieve the results in the model that the walls and environment are supposed to achieve in reality. The infinite potential is a good piece of staging.'[14] Earlier she had spoken of the way Thucidydes conceived his task in writing history, as not being to perform the impossible feat of reproducing exactly what was

[13] Clarendon Press, Oxford, 1983.

[14] Ibid., p. 142.

said on a given occasion, but rather to represent historical agents as saying things which would convey the spirit of their sentiments. She likens this to a dramatic representation of a historical event, in which the conventions of the stage impose limits on the literalness of one's depiction of character and action. Characters whispering to each other in reality have on the stage to speak up, so that the audience might hear what they are saying. This sort of thing is not exactly true to life, but it is not exactly false either, being the natural way to represent an event in a given medium. And it is for a similar sort of reason that, in scientific representation, we may fit facts to our equations or models.

More generally than this conscious fitting of facts to equations, there are also, according to Cartwright, many occasions in practice where actual objects or situations do not obey the laws they are supposed to obey. She takes the example of Maxwell's radiometer, a little windmill, whose vanes are black on one side and white on the other, which is enclosed in an evacuated glass bowl. The vanes rotate when light falls on the radiometer, and the rotation is ascribed to the action of the gas molecules left inside the bowl. Rather in the spirit of Peirce, Cartwright looks at Maxwell's mathematical treatment of the distribution of the gas molecules which is supposed to apply where in the radiometer there are inequalities of temperature and viscosity, and where the viscosity varies 'as the first power of the absolute temperature', as Maxwell puts it. But, says Cartwright, these conditions are not met 'in any of the radiometers we find in the toy department of Woolworth's. The radiometers on the shelves of Woolworth's do not have delicate well-tuned features. They cost $2.29. They have a host of causally relevant characteristics besides the two critical ones Maxwell mentions, and they differ in these characteristics from one to another.'[15] In contrast to the smooth picture of science we are

[15] Ibid., p. 154.

often presented with, in which there are subsidiary laws or bridge principles taking us down in a law-like way from idealized general theories to specific cases, Cartwright says that we know of no principles which will determine how or why Woolworth's radiometers deviate from Maxwell's function for the distribution of their gas molecules. Nor is it the case that even those radiometers which do meet Maxwell's conditions obey his function. Most of them have many other relevant features affecting their behaviour. Cartwright's conclusion is that, in the absence of any laws linking the ideal case to real cases, we have to regard Maxwell's distribution function as a pure fiction, with no explanatory force. It is a mere property of convenience, which we have no idea how to apply outside 'the controlled conditions of the laboratory, where real life mimics explanatory models'.

Cartwright's own view is that when in science we want to apply mathematical theories to reality, we have to use fictional theoretical descriptions (or 'models') of phenomena. These models never exactly fit reality. Their equations do fit the objects postulated in the model, but this is because a model is made to conform to the equations. Different and incompatible models may be used for different purposes. In this context, Cartwright cites a text on quantum optics which mentions various different and mutually incompatible models available for lasers: idealized interacting models, soluble models, simplified dynamical models. In each model, the same phenomenon is explained in a different way, and we select our model according to the properties of the laser we are interested in.

Cartwright draws the conclusion that

> in general, nature does not prepare situations to fit the kinds of mathematical theories we hanker for. We construct both the theories and the objects to which they apply, then match them piecemeal onto real situations, deriving—sometimes with great precision—a bit of what happens, but generally not getting all the facts straight at once. The fundamental laws do not govern reality. What they govern has only the appearance of reality and the appearance is far tidier and more readily regimented than reality itself.[16]

If there is any truth in what Cartwright says—and she certainly provides a good deal of supporting evidence—it seems that we should regard the theories science actually provides us with as far from complete and precisely accurate representations of reality. They are idealizations and abstractions which focus on particular properties of natural phenomena and cases of partial regularity, corresponding no doubt to specific interests and concerns we might have. But in applying our models, we overlook both their incompleteness and their inaccuracy. They do well enough for what we want in predicting and controlling effects, but this 'enough' could be quite consistent with a good deal of inaccuracy and a good deal of overlooking of the full detail of any actual situation. Moreover, we choose our models according to the specific features of the situation we may be interested in, without worrying too much about whether one model can easily be combined with another model we might use for other purposes. None of this militates against the idea that science can discover genuine regularities or new phenomena or new entities. But it does militate against the thought that in science our aim is always the production of ever more general and comprehensive accounts of the whole of a given level of existence, which at the same time are ever more accurate. This ideal may be unattainable. It certainly will be if nature is basically untidy and cannot be divided into clearly demarcated natural kinds. And it may be that most of what we want from science, in the way of the explanation and of the control of nature, can be achieved without assuming the validity of the ideal.

[16] Cartwright, *How the Laws of Physics Lie*, p. 162.

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The Absolute View of the World

Underlying the various disputes we have been considering between positivists and their realist opponents is a fundamental divergence of opinion about what science can do. The dispute is not about the existence of a real world apart from our knowledge of it. Whatever might have been the position of Berkeley or Hume, this is not an issue as between van Fraassen and Hacking or between Cartwright and Popper, or even, I believe, between Kuhn or Feyerabend and Popper. All these philosophers of science accept as a premiss of their enquiries—as indeed I did in the opening passages of this book—that there is a world independent of we who observe it and live in it, a world which impinges on us in various ways, and a world which scientific enquiry can teach us more about than we could discover by random methods or by sticking to sensory observation unaided by scientific theories or instruments. What I have been calling the positivist or, perhaps better, empiricist tendency in the philosophy of science is not, to repeat what I said in considering unobservable entities, a form of Cartesian scepticism. Cartesian scepticism exploits hyperbolic doubt; it doubts wherever there is not a conclusive proof to guard against all and any possible scepticism. In Descartes's *Meditations*, an evil genius could be confusing me about 2 + 2 = 4, or I could be dreaming that I am writing this. The contemporary philosopher of science, by contrast, is likely to accept our sensorily and culturally given account of the world and its inhabitants as a starting-point. His questioning is about the extent to which we can go beyond this starting-point in order to gain knowledge of the world as it is apart from our particular human perspectives on it.

The hope that we can transcend our human perspectives on the world is, of course, entertained in modern science right from its beginning. The very idea of the Copernican revolution is to displace what must have see[illegible]nreflect-

ively obvious to all men. Despite Aristarchus of Samos, who did postulate a heliocentric universe in Hellenistic times, it must have been very hard for Copernicus's contemporaries to accept that the earth was not the centre of the universe. Copernicus asserted that our certainty was derived only from our particular viewpoint on the earth, and that a wider view of things would show this. Observers from other vantage points might locate the centre of the universe elsewhere, but they certainly would not locate it in the earth.

The Copernican example indicates one type of correction of human perspective made by science. It shows us that something we very naturally perceive as being the case is not the case at all, but it serves to cast no doubt on our perceptual faculties or their deliverances as such. Presumably from very many points in space we would be able to see that it was far more plausible to regard the earth as going round the sun, rather than vice versa. As with our seeing Jupiter's moons through a telescope, we can easily entertain the thought that, from a suitable vantage point, we could observe things as they really are with our normal sensory equipment, and our not being able to observe them as they really are is due merely to accidental facts about our current position in space. There can, then, be corrections of human perspective in science which involve no very radical claims about limitations or deficiencies inherent in our sensory apparatus.

On the other hand, as we have already seen, there can be cases where science takes us far beyond our natural sensory apparatus. Much of modern science, of course, does this; and much of our discussion in the last two chapters has centred around the problems involved with this. We have seen that there can be good reasons for thinking that we can extend our powers of observation by means of instrumentation. When this occurs, it would be perfectly in order to think of science as providing us with a wider, more inclusive view of the world than that we are born and brought up with. In so far as we can transcend our biology and natural language in this sort of way, we can regard science as providing us with a less relative, more absolute account of the world. It is an account less relative to our own perspectives and viewpoints. It is more absolute in the sense that in science we are able to arrive at entities and properties of entities which are causally more fundamental than the properties manifested by the world as it appears to us.

In fact, in the theories of physiology and perception which have been dominant in natural science since the seventeenth century, it is claimed that the world does not appear to us as it really is. A distinction is drawn between the manifest image (the world as it appears to us) and the scientific image (the world as it is in itself). According to the former, the world consists of objects which are coloured, noisy, hard, or soft to the touch and having a given smell and taste; in science, though, we learn that all these sensory properties of things are not in the things themselves, but are due to the interaction of particles, which lack these properties, with our sense-organs. Thus, colour vision is due to the interaction of colourless photons with our visual cones. The world as it really is does not contain colour as a fundamental property. Colour and the perception of colour are due to the interaction of causally more basic properties of the world, and the same goes for other sensory qualities we perceive.

Looked at from the point of view of the scientific image we can see ourselves as part of a more inclusive world and our perspective on it as just that. Creatures with other sensory faculties and powers might well have a different type of manifest image. In a sense we can easily accept that this is so: it is common knowledge that dogs and bats, for example, can perceive sounds and vibrations we are insensitive to, while being insensitive to much of what stimulates us. Their picture of the world would presumably be quite different from ours. Nevertheless, if we could imagine dogs and bats and other types of creature doing science, we might think of them pushing beyond their manifest image of the world to a

scientific image not unlike the sensorily purified scientific image we attain in thinking of colours and the rest as due as much to our make-up as to the way the world is in itself. We thus arrive at the idea of an absolute conception of the world, a conception, that is, of the world as it is in itself, independent of any particular mode of perceiving it, and to which all specific modes of perceptions could be related.[17] Such a conception would in principle be available to perceivers of whatever physical and sensory constitution; it would lay out the causally fundamental properties of the world, and in so doing show the types of perception of specific classes of perceiver as due to those more fundamental processes. We might, too, think of the absolute conception of the world as showing how all the different types and levels of property we observe in the world are all reducible to one basic level of property, to one basic essence of matter and of the natural world. In this way, the absolute conception of the world, though not strictly implying either, would connect fairly naturally with anti-Humean essentialism, and with reductionism in the sciences. The biological may thus be reduced to the chemical, and the chemical in turn to the physical, with the natural kinds in each higher level being analysable in terms of lower level natural kinds, whose nature at the physical level necessitates all else. And so the old philosophical dream, that everything in the world, and all its myriad properties and appearances, are produced simply by the regular rule-governed movements of atoms in the void, would be fulfilled.

No doubt this absolute conception of the world is inspired in part by the account of colour and other sensory properties given by Newtonian and post-Newtonian science, as well as by the Newtonian conceptions of absolute space and time. But the idea of the world, as both in principle open to perceivers of whatever constitution and also prescinding

[17] Cf. Bernard Williams, *Descartes: The Project of Pure Enquiry* (Penguin Books, Harmondsworth, 1978), p. 211.

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from all conditions and perspectives of perception in order to give us the world as it is, independent of all thought, is a *philosophical* ideal rather than an actual scientific achievement. In certain respects the image of the world presented by modern science prescinds from our particular modalities of perception, as we have seen, explaining their nature and genesis. But the actual scientific image we get at any time is not uncontaminated by all human interests and perspectives. Nor is it clear that there could ever be one unified account of the world which reduces everything to one fundamental level of explanation.

We shall see, later on, the issues involved in thinking that scientific explanation can be reduced to one fundamental level. What has already become clear is that it is hard to see our science prescinding altogether from our human point of view. Even though through instrumentation we can significantly extend our knowledge of the natural world, our theories and postulations must in the end have some bearing on things we can observe, either through instruments or by our unaided senses. And our observation of instruments itself will normally be through our unaided senses. Of course, this does not mean that we are just left with our unaided senses, or can learn only through them. As we have seen, we can go a long way beyond that in the direction of a less humanly relative picture of the world.

The problem arises not so much in formulating less relative accounts of the world, as in checking them when they go beyond what we can observe. It becomes particularly acute when we are dealing with what might be called metaphysical theories as to the basic essence of the world, for, as we have already seen, these theories are not testable, nor do we find convergence at this level of speculation in the history of science. A positivistic, anti-realist attitude to such theories, suspending judgement on their truth, may well be appropriate. If it is, then this is an implicit admission that there are limits to what we can know about the world, given

initially by our human constitution and physical location, and, to that extent, it will raise a doubt as to the accessibility of an absolute conception of the world.

Whatever might be said about our ability to grasp an absolute conception of the world, or to aim at such a thing, it is important to emphasize that the practice of science does not require us to think that we are aiming at or attaining this. It may be that the regularities uncovered by our scientific theories are not all-pervasive, and apply only in our region of space and time. It may be, too, that our theories do not disclose genuine natural kinds, with essences necessitating their behaviour. There may, indeed, be no natural necessity in the sense denied by Hume. It may also be that our empirical theories are—as Cartwright suggests—models and approximations conditioned by our interests and specific needs. It may further be the case that there are no ultimate reductions of one level of science to another. But none of this would invalidate the results we have gained or the quest for further theories and regularities. The discoveries we have made and will make could still be seen as subserving the Baconian ideal of relieving man's estate. As for the scientific aim of gaining theoretical knowledge of the world, this will to some extent be achieved through our increasing knowledge of regularities at whatever level we can observe the world, by our discoveries of new entities and phenomena, and so on. Someone sceptical of realism at the metaphysical level of science, of the doctrines of essence and of natural necessity, as well as of our ability to achieve a truly absolute conception of the world, could still be seen as following the Baconian edict that nature must be obeyed before being commanded. He will, though, be open to the possibility that our theories and knowledge of the world may have limits given by our position and perceptual status in the world, that they may not take us to the deep essence of the material world, if indeed there is such a thing, and also that such knowledge as we have may be disjointed and not readily combinable into a single picture. Things we know at one level or in one area cannot directly contradict each other, but that is not to say that their connections and relationships are necessarily unproblematic.

Partial Pictures: Schrödinger's Cat

Partly as an illustration of the difficulty of combining our various theories into a single picture, and partly in order to pave the way for our consideration of probability statements in science, we can consider briefly one notorious difficulty in quantum mechanics. Indeed, in some ways, it is *the* difficulty quantum mechanics poses for us, the problem of combining the quantum-mechanical picture of reality, which speaks of collections of micro-states coexisting at any one time and place, with ordinary or classical descriptions of the physical world, in which we think of things and states of affairs as being at any one time in some definite state, rather than in collections or superpositions of states. Apart from the problem of understanding how the spin of an electron can at some time be both 'up' and 'down', without being definitely either, we have to explain how it is that some macroscopic entity connected in some way to the micro-particle or system is enabled to behave as if the particle is definitely in one or other of the states. This is the predicament posed by Schrödinger's famous example of the cat.

We are asked to envisage this unhappy creature trapped in a closed room which contains a Geiger counter and a hammer raised above a flask of prussic acid. The counter contains a trace of radioactive material which, after one hour, has a 50 per cent chance that one of its nuclei will decay. If this happens, the counter will click, the hammer will fall, the flask will be broken, and the cat will die instantaneously. Unless we look into the room, of course, we will not know at the end of the hour whether the cat is alive or dead. Now, the

quantum-mechanical description of the radioactive material does not permit us to speak definitely of the decay or non-decay of the nucleus after one hour. The system is not represented as having a unique value in this respect, but as consisting rather of a superposition of possible values, here, presumably, decay and non-decay.

On the other hand, and perfectly obviously, for an observer who looks into the room after one hour there will not be a superposition of a dead cat and a living cat, and the same will be true for the cat itself, which will be either alive or dead, and not in some half-way state combining both life and death. The difficulty is to see how some essentially fuzzy quantum-mechanical system (a nucleus in a superposition of decayed and non-decayed states) relates to the definite macroscopic state of affairs (cat definitely alive or definitely dead). The point of Schrödinger's example is that in it the definite state of the cat is seen as causally dependent on the indefinite state of a particle, which is apparently consistent with the cat being in either state. We can look at the problem in terms of the chain of events leading from the indeterminate quantum-mechanical state to the definite macroscopic event. How and at what point does the chain firm itself up into something definite? And this is not just a problem of temporal chains, whereby something that was fuzzy eventually becomes definite. Present definite macroscopic states of affairs are now sustained by fuzzy, indefinite quantum states.

As would no doubt be anticipated, there have been a number of attempts to solve the problem of the so-called 'collapse of the wave-packet', whereby a superposition of quantum states, such as conflicting electron spins or nuclei decaying and not-decaying, reduces to one definite state. We can mention first that associated with Eugene Wigner, because it is perhaps the easiest to dismiss. According to Wigner, the intervention of the consciousness of the observer triggers a superposition of states of a system into some

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