3 The Theory-dependence of Observation

We have seen that, according to our naive inductivist, careful and unprejudiced observation yields a secure basis from which probably true, if not true, scientific knowledge can be derived. In the last chapter, this position was criticized by pointing to difficulties involved in any attempt to justify the inductive reasoning involved in the derivation of scientific laws and theories from observation. Some examples suggested that there was positive grounds for suspecting the alleged reliability of inductive reasoning. Nevertheless, these arguments do not constitute a definitive refutation of inductivism, especially as it turns out that many rival theories of science face a similar, related difficulty.[1] In this chapter, a more serious objection to the inductivist's position is developed that involves a criticism, not of the inductions by which scientific knowledge is supposed to be derived from observation, but of the inductivist's assumptions concerning the status and role of observation itself.

There are two important assumptions involved in the naive inductivist position with respect to observation. One is that *science starts with observation*. The other is that *observation yields a secure basis from which knowledge can be derived*. In the present chapter, both of these assumptions will be criticized in a variety of ways and rejected for a variety of reasons. But first of all, I will sketch an account of observation that I think it is fair to say is a commonly held one in modern times, and which lends plausibility to the naive inductivist position.

1. A popular account of observation

Partly because the sense of sight is the sense most extensively used in the practice of science, and partly for convenience, I will restrict my

discussion of observation to the realm of seeing. In most cases, it will not be difficult to see how the argument presented could be re-cast so as to be applicable to observation via the other senses. A simple, popular account of seeing might run as follows. Humans see by using their eyes. The most important components of the human eye are a lens and the retina, the latter acting like a screen on which images of objects external to the eye are formed. Rays of light from a viewed object pass from the object to the lens via the intervening medium. These rays are refracted by the material of the lens in such a way that they are brought to a focus on the retina, so forming an image of the viewed object. Thus far, the functioning of the eye is very much like that of a camera. A big difference lies in the way the final image is recorded. Optic nerves pass from the retina to the central cortex of the brain. These carry information concerning the light falling on the various regions of the retina. It is the recording of this information by the human brain that corresponds to the seeing of the object by the human observer. Of course, many details could be added to this simple description, but the account offered does capture the general idea.

Two points are strongly suggested by the foregoing sketch of observation via the sense of sight, points that are key ones for the inductivist. The first is that a human observer has more or less direct access to some properties of the external world insofar as those properties are recorded by the brain in the act of seeing. The second is that two normal observers viewing the same object or scene from the same place will "see" the same thing. An identical combination of light rays will strike the eye of each observer, will be focussed on their normal retinas by their normal eye lenses and give rise to similar images. Similar information will then travel to the brain of each observer via their normal optic nerves, resulting in the two observers "seeing" the same thing. These two points will be attacked fairly directly in the next section. Later sections will cast further and more consequential doubt on the adequacy of the inductivist stance on observation.

2. Visual experiences not determined by the images on the retina

There is a vast fund of evidence to indicate that it is just not the case that the experience that observers undergo when viewing an object is determined solely by the information, in the form of light rays, entering the observer's eyes, nor is it determined solely by the images on the retinas of an observer. Two normal observers viewing the

same object from the same place under the same physical circumstances do not necessarily have identical visual experiences, even though the images on their respective retinas may be virtually identical. There is an important sense in which the two observers need not "see" the same thing. As N.R. Hanson has put it, "There is more to seeing than meets the eyeball." Some simple examples will illustrate the point.

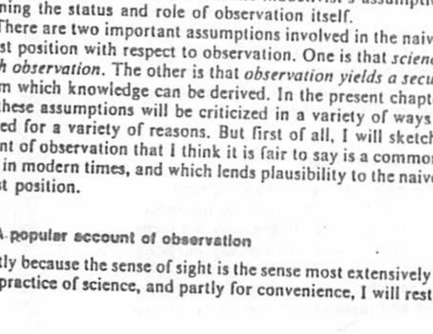

Figure 3

Most of us, when first looking at Figure 3, see the drawing of a staircase with the upper surface of the stairs visible. But this is not the only way it can be seen. It can without difficulty also be seen as a staircase with the under surface of the stairs visible. Further, if one looks at the picture for some time, one generally finds, involuntarily, that what one sees changes frequently from a staircase viewed from above to a staircase viewed from below and back again. And yet it seems reasonable to suppose that, since it remains the same object viewed by the observer, the retinal images do not change. Whether the picture is seen as a staircase viewed from above or a staircase viewed from below seems to depend on something other than the image on the retina of the viewer. I suspect that no reader of this book has questioned my claim that Figure 3 looks like a staircase of some kind. However, the results of experiments on members of a number of African tribes whose culture does not include the custom of depicting three-dimensional objects by two-dimensional perspective drawings indicate that the members of those tribes would not have seen Figure 3 as a staircase but as a two-dimensional array of lines. I presume that the nature of the images formed on the retinas

of observers is relatively independent of their culture. Again, it seems to follow that the perceptual experiences that observers have in the act of seeing is not uniquely determined by the images on their retinas. This point has been made and illustrated with a number of examples by Hanson.[2]

What an observer sees, that is, the visual experience that an observer has when viewing an object, depends in part on his past experience, his knowledge and his expectations. Here are two simple examples to illustrate this particular point.

In a well-known experiment, subjects were shown playing-cards for a small duration of time and asked to identify them. When a normal pack of cards was employed, subjects were able to accomplish this task very successfully. But when anomalous cards were introduced, such as a red Ace of Spades, then, at first, nearly all subjects initially identified such cards incorrectly as some normal card. They saw a red Ace of Spades as a normal Ace of Diamonds or a normal Ace of Spades. The subjective impressions experienced by the observers were influenced by their expectations. When, after a period of confusion, subjects began to realize, or were told, that there were anomalous cards among the pack; they then had no trouble correctly identifying all the cards shown to them, anomalous or otherwise. The change in their knowledge and expectations was accompanied by a change in what they saw, although they were still viewing the same physical objects.

Another example is provided by a children's picture puzzle that involves finding the drawing of a human face amongst the foliage in the drawing of a tree. Here, what is seen, that is, the subjective impression experienced by a person viewing the drawing, at first corresponds to a tree, with trunk, leaves, branches. But this changes once the human face has been detected. What was once seen as foliage and parts of branches is now seen as a human face. Again, the same physical object has been viewed before and after the solution of the puzzle, and presumably the image on the observer's retina does not change at the moment the solution is found and the face discovered. And if the picture is viewed at some later time, the face can be easily seen again by an observer who has already solved the puzzle once. In this example, what an observer sees is affected by his knowledge and experience.

"What", it might be suggested, "have these contrived examples got to do with science?" In response, it is not difficult to produce examples from the practice of science that illustrate the same point, namely, that what observers see, the subjective experiences that they undergo, when viewing an object or scene is not determined solely by the images on their retinas but depends also on the experience, knowledge, expectations and general inner state of the observer. It is necessary to learn how to see expertly through a telescope or microscope, and the unstructured array of bright and dark patches that the beginner observes is different from the detailed specimen or scene that the skilled viewer can discern. Something of the kind must have been operative when Galileo first introduced the telescope as an instrument for exploring the heavens. The reservations that Galileo's rivals held about accepting phenomena such as the moons of Jupiter that Galileo had learnt to see must have been in part due, not to prejudice, but to genuine difficulties encountered when learning to 'see' through what were, after all, very crude telescopes. In the following passage, Michael Polanyi describes the changes in a medical student's perceptual experience when he is taught to make a diagnosis by inspecting an X-ray picture.

> Think of a medical student attending a course in the X-ray diagnosis of pulmonary diseases. He watches, in a darkened room, shadowy traces on a fluorescent screen placed against a patient's chest, and hears the radiologist commenting to his assistants, in technical language, on the significant features of these shadows. At first, the student is completely puzzled. For he can see in the X-ray picture of a chest only the shadows of the heart and ribs, with a few spidery blotches between them. The experts seem to be romancing about figments of their imagination; he can see nothing that they are talking about. Then, as he goes on listening for a few weeks, looking carefully at ever-new pictures of different cases, a tentative understanding will dawn on him; he will gradually forget about the ribs and begin to see the lungs. And eventually, if he perseveres intelligently, a rich panorama of significant details will be revealed to him: of physiological variations and pathological changes, of scars, of chronic infections and signs of acute disease. He has entered a new world. He still sees only a fraction of what the experts can see, but the pictures are definitely making sense now and so do most of the comments made on them.[3]

A common response to the claim that I am making about observation, supported by the kinds of examples I have utilized, is that observers viewing the same scene from the same place see the same thing but interpret what they see differently. I wish to dispute this. As far as perception is concerned, the only things with which an observer has direct and immediate contact are his or her experiences. These experiences are not uniquely given and unchanging but vary with the expectations and knowledge of the observer. What is uniquely given by the physical situation is the image on the retina of an

observer, but an observer does not have direct perceptual contact with that image. When the naive inductivist, and many other empiricists, assume that there is something unique given to us in experience that can be interpreted in various ways, they are assuming, without argument and in spite of much evidence to the contrary, some one-to-one correspondence between the images on our retinas and the subjective experiences that we have when seeing. They are taking the camera analogy too far.

Having said this, let me try to make clear what I do *not* mean to be claiming in this section, lest I be taken to be arguing for more than I intend to be. Firstly, I am certainly not claiming that the physical causes of the images on our retinas have nothing to do with what we see. We cannot see just what we like. However, while the images on our retinas form part of the cause of what we see, another very important part of the cause is constituted by the inner state of our minds or brains, which will clearly depend on our cultural upbringing ,our knowledge, our expectations, etc. and will not be determined solely by the physical properties of our eyes and the scene observed. Secondly, under a wide variety of circumstances, what we see in various situations remains fairly stable. The dependence of what we see on the state of our minds or brains is not so sensitive as to make communication, and science, impossible. Thirdly, in all the examples quoted here, there is a sense in which all observers see the same thing. I accept, and presuppose throughout this book, that a single, unique, physical world exists independently of observers. Hence, when a number of observers look at a picture, a piece of apparatus, a microscope slide, or whatever, there is a sense in which they are all confronted by, look at, and so, in a sense, "see" the same thing. But it does not follow from this that they have identical perceptual experiences. There is a very important sense in which they do not see the same thing, and it is this latter sense upon which my criticism of the inductivist position has been based.

3. Observation statements presuppose theory

Even if there were some unique experience given to all observers in perception, there still remain some major objections to the inductivist assumption concerning observations. In this section, we focus attention on the observation *statements* based on and allegedly justified by the perceptual experiences of the observers who assert the statements. According to the inductivist account of science, the secure basis on which the laws and theories that constitute science

are built is made up of public observation statements rather than the private, subjective experiences of individual observers. Clearly, the observations made by Darwin during his voyage on the *Beagle*, for example, would have been inconsequential for science had they remained Darwin's private experiences. They became relevant for science only when they were formulated and communicated as observation statements capable of being utilized and criticized by other scientists. The inductivist account requires the derivation of universal *statements* from singular *statements* by induction. Inductive as well as deductive reasoning involves the relationships between various sets of statements and not relationships between statements on the one hand and perceptual experiences on the other.

We might assume that perceptual experiences of some kind are directly accessible to an observer, but observation statements certainly are not. The latter are public entities, formulated in a public language, involving theories of various degrees of generality and sophistication. Once attention is focussed on observation statements as forming the alleged secure basis for science, it can be seen that, contrary to the inductivists' claim, theory of some kind must preceed all observation statements and observation statements are as fallible as the theories they presuppose.

Observation statements must be made in the language of some theory, however vague. Consider the simple sentence in commonsense language, "Look out, the wind is blowing the baby's pram over the cliff edge!" Much low-level theory is presupposed here. It is implied that there is such a thing as wind, which has the property of being able to cause the motion of objects such as prams, which stand in its path. The sense of urgency conveyed by the "Look out," indicates the expectation that the pram, complete with baby, will fall over the cliff and perhaps be dashed on the rocks beneath and it is further assumed that this will be deleterious for the baby. Again, when an early riser in urgent need of coffee complains, "The gas won't light," it is assumed that there are substances in the world that can be grouped under the concept "gas", and that some of them, at least, ignite. It is also to the point to note that the concept "gas" has not always been available. It did not exist until the mid-eighteenth century, when Joseph Black first prepared carbon dioxide. Before that, all "gases" were considered to be more or less pure samples of air.[4] When we move towards statements of the kind occurring in science, the theoretical presuppositions become less commonplace and more obvious. That there is considerable theory presupposed by the assertion, "The electron beam was repelled by the North Pole of

the magnet," or by a psychiatrist's talk of the withdrawal symptoms of a patient, should not need much arguing.

Observation statements, then, are always made in the language of some theory and will be as precise as the theoretical or conceptual framework that they utilize is precise. The concept "force" as used in physics is precise because it acquires its meaning from the role it plays in a precise, relatively autonomous theory, Newtonian mechanics. The use of the same word in everyday language (the force of circumstance, gale-force-winds, the force of an argument, etc.) is imprecise just because the corresponding theories are multifarious and imprecise. Precise, clearly formulated theories are a prerequisite for precise observation statements. In this sense theories precede observation.

The foregoing claims about the priority of theory over observation run counter to an inductivist thesis that the meanings of many basic concepts are acquired through observation. Let us consider the simple concept "red" as an example. An inductivist account might run roughly as follows. From all the perceptual experiences of an observer arising from the sense of sight, a certain set of them (those corresponding to the perceptual experiences arising from sightings of red objects) will have something in common. The observer, by inspection of the set, is somehow able to discern the common element in these perceptions, and come to understand this common element as redness. In this way, the concept "red" is arrived at through observation. This account contains a serious flaw. It assumes that from all the infinity of perceptual experiences undergone by an observer, the set of perceptual experiences arising from the viewing of red things is somehow available for inspection. But that set does not select itself. What is the criterion according to which some perceptual experiences are included in the set and others are excluded? The criterion, of course, is that only perceptions of *red* objects are included in the set. The account presupposes the very concept, redness, the acquisition of which it is meant to explain. It is not an adequate defence of the inductivist position to point out that parents and teachers select a set of red objects when teaching children to understand the concept "red", for we are interested in how the concept first acquired its meaning. The claim that the concept "red" or any other concept is derived from experience and from nothing else is false.

So far in this section the naive inductivist account of science has been undermined largely by arguing that theories must precede observation statements, so that it is false to claim that science starts with observation. We now come to a second way in which inductivism is undermined. Observation statements are as fallible as the theories they presuppose and therefore do not constitute a completely secure basis on which to build scientific laws and theories.

I will first illustrate the point with some simple, somewhat contrived examples, and then proceed to indicate the relevance of the point for science by citing some examples from science and its history.

Consider the statement, "Here is a piece of chalk," uttered by a teacher as he indicates a cylindrical white stick held in front of the blackboard. Even this most basic of observation statements involves theory, and is fallible. Some very low-level generalization, such as "White sticks found in classrooms near blackboards are pieces of chalk," is assumed. And, of course, this generalization need not be true. The teacher in our example may be wrong. The white cylinder in question may not be a piece of chalk but a carefully contrived fake placed there by a scheming pupil in search of amusement. The teacher, or anyone else present, could take steps to test the truth of the statement, "Here is a piece of chalk," but it is significant that the more stringent the test the more theory is called upon, and further, absolute certainty is never attained. For instance, on being challenged, the teacher might draw the white cylinder across the board, point to the resulting white trace and declare, "There you are, it *is* a piece of chalk." This involves the assumption, "Chalk leaves white traces when drawn across a blackboard."The teacher's demonstration might be countered by the retort that other things besides chalk leave white traces on a blackboard. Perhaps, after other moves by the teacher, such as crumbling the chalk, being countered in a similar way, the determined teacher might resort to chemical analysis. Chemically, chalk is largely calcium carbonate, he argues, and so should yield carbon dioxide if immersed in an acid. He performs the test and demonstrates that the evolving gas is carbon dioxide by showing that it turns lime water milky. Each stage in this series of attempts to consolidate the validity of the observation statement, "Here is a piece of chalk" involves an appeal not only to further observation statements but also to more theoretical generalizations. The test that formed the stopping-point in our series involved a certain amount of chemical theory (the effect of acids on carbonates, the peculiar effect of carbon dioxide on lime water). In order to establish the validity of an observation statement, then, it is necessary to appeal to theory, and the more firmly the validity is to be established, the more extensive will be the theoretical knowledge

employed. This is in direct contrast to what we might expect to follow according to the inductivist view, namely, that in order to establish the truth of some problematic observation statement we appeal to more secure observation statements, and perhaps laws derived inductively from them, but not to theory.

In everyday language, it is often the case that an apparently unproblematic "observation statement" is found to be false when an expectation is disappointed, due to the falsity of some theory presupposed in the assertion of the observation statement. For instance, some picnickers at the top of a high mountain, directing their glance towards the camp-fire, may observe, "The water is hot enough to make the tea," and then find they were sadly wrong when tasting the resulting brew. The theory that had wrongly been supposed is that boiling water is hot enough to make tea. This need not be the case for water boiling under the low pressures experienced at high altitudes.

Here are some less-contrived examples more helpful for our attempt to understand the nature of science.

At the time of Copernicus (before the invention of the telescope), careful observations were made of the size of Venus. The statement, "Venus, as viewed from earth, does not change size appreciably during the course of the year" was generally accepted by all astronomers, both Copernicans and non-Copernicans, on the basis of those observations. Andreas Osiander, a contemporary of Copernicus, referred to the prediction that Venus should appear to change size during the year as "a result contradicted by the experience of every age".[3] The observation was accepted in spite of its inconvenience, since the Copernican theory as well as some of its rivals predicted that Venus should appear to change size appreciably during the course of the year. Yet the statement is now considered to be false. It presupposes the false theory that the size of small light sources is accurately gauged by the naked eye. Modern theory can offer some explanation of why naked-eye estimates of the size of small light sources will be misleading and why telescopic observations, which show the apparent size of Venus to vary considerably during the course of the year, are to be preferred. This example clearly illustrates the theory dependence and hence fallibility of observation statements.

A second example concerns electrostatics. Early experimenters in that field reported observations of electrified rods becoming sticky, as evidenced by small pieces of paper sticking to them, and of the rebounding of one electrified body from another. From a modern

point of view, those observation reports were mistaken. The false conceptions that facilitated those observations would now be replaced by the notions of attractive and repulsive forces acting at a distance, leading to quite different observation reports.

Finally, in lighter vein, modern scientists would have no difficulty in exposing the falsity of an entry in honest Kepler's notebook, following observations through a Galilean telescope, which reads, "Mars is square and intensely coloured."[4]

In this section, I have argued that the inductivist is wrong on two counts. Science does not start with observation statements because theory of some kind precedes all observation statements, and observation statements do not constitute a firm basis on which scientific knowledge can be founded because they are fallible. However, I do not wish to claim that it follows from this that observation statements should play no role in science. I am not urging that all observation statements should be discarded because they are fallible. I am merely arguing that the role in science attributed to observation statements by the inductivist is incorrect.

4. Observation and experiment are guided by theory

According to the most naive of inductivists, the basis of scientific knowledge is provided by observations made by an unprejudiced and unbiased observer.[5] If interpreted anything like literally, this position is absurd and untenable. To illustrate this, let us imagine Heinrich Hertz, in 1888, performing the electrical experiment that enabled him to produce and detect radio waves for the first time. If he is to be totally unbiased when making his observations, then he will be obliged to record not only the readings on various meters, the presence or absence of sparks at various critical locations in the electrical circuits, the dimensions of the circuit, etc. but also the colour of the meters, the dimensions of the laboratory, the state of the weather, the size of his shoes and a whole host of "clearly irrelevant" details, irrelevant, that is, to the kind of theory in which Hertz was interested and which he was testing. (In this particular case, Hertz was testing Maxwell's electromagnetic theory to see if he could produce the radio waves predicted by that theory.) As a second, hypothetical example, suppose that I was keen to make some contribution to human physiology or anatomy, and suppose I noted that very little work has been done on the weight of human earlobes. If, on the basis of this, I were to proceed to make very careful observations of the weights of a wide variety of human earlobes, recording

and categorizing the many observations, I think it is clear that I would not be making any significant contribution to science. I would be wasting my time, unless some theory had been proposed rendering the weight of earlobes important, such as a theory connecting the size of earlobes with the incidence of cancer in some way.

The foregoing examples illustrate an important sense in which theory precedes observation in science. Observations and experiments are carried out in order to test or shed light on some theory, and only those observations considered relevant to that task should be recorded. However, insofar as the theories that make up our scientific knowledge are fallible and incomplete, the guidance that theories offer as to what observations are relevant to some phenomenon under investigation may be misleading, and may result in some important factors being overlooked. Hertz's experiment referred to above provides a nice example.One of the factors I referred to as "clearly irrelevant" was in fact very relevant. It was a consequence of the theory under test that radio waves should have a velocity equal to the velocity of light. When Hertz measured the velocity of his radio waves, he found repeatedly that their velocity was significantly different from that of light. He was never able to solve the problem. It was not until after his death that the source of the problem was really understood. Radio waves emitted from his apparatus were being reflected from the walls of his laboratory back on to the apparatus and were interfering with his measurements. It turned out that the dimensions of the laboratory were very relevant. The fallible and incomplete theories that make up scientific knowledge may give false guidance to an observer, then. But this problem is to be tackled by improving and extending our theories and not by recording an endless list of aimless observations.

5. Inductivism not conclusively refuted

The theory-dependence of observation discussed in this chapter certainly undermines the inductivist claim that science starts with observation. However, only the most naive of inductivists would wish to adhere to that position. None of the modern, more sophisticated inductivists would wish to uphold the literal version of it. They can dispense with the claim that science must start with unbiased and unprejudiced observation by making a distinction between the way a theory is first thought of or discovered on the one hand, and the way in which it is justified or its merits assessed on the other. According to this modified position, it is freely admitted that new theories are conceived of in a variety of ways and often by a number of routes. They may occur to the discoverer in a flash of inspiration, as in the mythical story of Newton's discovery of the law of gravitation being triggered by his seeing an apple fall from a tree. Alternatively, a new discovery might occur as the result of an accident, as Roentgen was led to the discovery of X-rays by the constant blackening of photographic plates stored in the vicinity of his discharge tube. Or, again, a new discovery might be arrived at after a long series of observations and calculations, as exemplified by Kepler's discoveries of his laws of planetary motion. Theories may be, and usually are, conceived of prior to the making of those observations necessary to test them. Further, according to this more sophisticated inductivism, creative acts, the most novel and significant of which require genius and involving as they do the psychology of individual scientists, defy logical analysis. Discovery and the question of the origin of new theories is excluded from the philosophy of science.

However, once new laws and theories have been arrived at, no matter by what route, there remains the question of the adequacy of those laws and theories. Do they correspond to legitimate scientific knowledge or don't they? This question is the concern of the sophisticated inductivists. Their answer is roughly as I have outlined in Chapter 1. A large number of facts relevant to a theory must be ascertained by observation under a wide variety of circumstances, and the extent to which the theory can be shown to be true or probably true in the light of those facts by some kind of inductive inference must be established.

The separation of the mode of discovery and the mode of justification does enable the inductivists to evade that part of the criticism levelled at them in this chapter which was directed at the claim that science starts with observation. However, the legitimacy of the separation of the two modes can be questioned. For instance, it would surely seem reasonable to suggest that a theory that anticipates and leads to the discovery of new phenomena, in the way Clerk Maxwell's theory led to the discovery of radio waves, is more worthy of merit and more justifiable than a law or theory devised to account for phenomena already known and not leading to the discovery of new ones. It will, I hope, become increasingly clear as this book progresses that it is essential to understand science as an historically evolving body of knowledge and that a theory can only be adequately appraised if due attention is paid to its historical context. Theory appraisal is intimately linked with the circumstances under which a theory first makes its appearance.

Even if we allow the inductivists to separate the mode of discovery and the mode of justification, their position is still threatened by the fact that observation statements are theory-laden and hence fallible. The inductivist wishes to make a fairly sharp distinction between direct observation, which he hopes will form a secure foundation for scientific knowledge, and theories, which are to be justified by the extent to which they receive inductive support from the secure observational foundation. Those extreme inductivists, the logical positivists, went so far as to say that theories only have meaning insofar as they can be verified by direct observation. This position is undermined by the fact that the sharp distinction between observation and theory cannot be maintained because observation, or rather the statements resulting from observation, are permeated by theory.

Although I have severely criticized inductivist philosophies of science in this and the previous chapter, the arguments I have presented do not constitute an absolutely decisive refutation of that programme. The problem of induction cannot be regarded as a decisive refutation because, as I have previously mentioned, most other philosophies of science suffer from a similar difficulty. I have just indicated one way in which criticism centred on the theory dependence of observation can be to some extent evaded by the inductivists, and I am convinced that they will be able to think of further ingenious defences. The main reason why I think inductivism should be abandoned is that, compared with rival and more modern approaches, it has increasingly failed to throw new and interesting light on the nature of science, a fact that led Imre Lakatos to describe the programme as a degenerating one. The increasingly more adequate, more interesting and more fruitful accounts of science developed in later chapters will constitute the strongest case against inductivism.

FURTHER READING

The dependence of perceptual experiences on theory is discussed and illustrated with examples in N.R. Hanson, *Patterns of Discovery* (Cambridge: Cambridge University Press, 1958). The writings of Popper, Feyerabend and Kuhn abound with arguments and examples supporting the thesis that observations and observation statements are theory-dependent. Some passages dealing fairly specifically with the topic are K.R. Popper, *The Logic of Scientific Discovery* (London: Hutchinson, 1968), ch. 5 and Appendix * 10; Popper, *Objective Knowledge* (Oxford: Oxford University Press, 1972)

pp.341-61, P.K. Feyerabend, *Against Method: Outline of an Anarchistic Theory of Knowledge* (London: New Left Books, 1975), ch. 6 and 7; and T.S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: Chicago University Press, 1970), ch. 10. Chapter 1 of Carl R. Kordig, *The Justification of Scientific Change* (Dordrecht: Reidel Publ. Co., 1971) contains a discussion of the topic, which is critical of Hanson and Feyerabend. A circumspect but somewhat dry account is Israel Scheffler, *Science and Subjectivity* (New York: Bobbs-Merrill, 1967). Entertaining discussions of perception that are relevant to the philosophical issue are R.L. Gregory, *Eye and Brain* (London: Weidenfeld and Nicolson, 1972) and Ernst Gombrich, *Art and Illusion* (New York: Pantheon, 1960). I would also like to enthusiastically recommend a very exciting book on animal perception, Vitus B. Droscher, *The Magic of the Senses* (New York: Harper and Row, 1971). This book strongly conveys a sense of the limitations and restrictedness of human perception and the arbitrariness of attempts to attach fundamental significance to the information humans happen to receive through their senses.

1. See Chapter 12, Section 4.
2. N.R. Hanson, *Patterns of Discovery* (Cambridge: Cambridge University Press, 1958), ch. 1.
3. M. Polanyi, *Personal Knowledge* (London: Routledge and Kegan Paul, 1973), p. 101.
4. See T.S. Kuhn, *The Structure of Scientific Revolutions* (Chicago: University of Chicago Press, 1970), p.70.
5. E. Rosen, *Three Copernican Treatises* (New York: Dover, 1959), p.25.
6. P.K. Feyerabend, *Against Method: Outline of an Anarchistic Theory of Knowledge* (London: New Left Books, 1975), p.126.
7. See, for example, the quotation on p.9.

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