about the truth of observation statements depend on what is already known or assumed, thus rendering the observable facts as fallible as the presuppositions underlying them. Both kinds of difficulty suggest that maybe the observable basis for science is not as straightforward and secure as is widely and traditionally supposed. In the next chapter I try to mitigate these fears to some extent by considering the nature of observation, especially as it is employed in science, in a more discerning way than has been involved in our discussion up until now.

Further reading

For a classic discussion of how knowledge is seen by an empiricist as derived from what is delivered to the mind via the senses, see Locke (1967), and by a logical positivist, see Ayer (1940). Hanfling (1981) is an introduction to logical positivism generally, including its account of the observational basis of science. A challenge to these views at the level of perception is Hanson (1958, chapter 1). Useful discussions of the whole issue are to be found in Brown (1977) and Barnes, Bloor and Henry (1996, chapters 1–3).

CHAPTER 2

Observation as practical intervention

Observation: passive and private or public and active?

A common way in which observation is understood by a range of philosophers is to see it as a passive, private affair. It is passive insofar as it is presumed that when seeing, for example, we simply open and direct our eyes, let the information flow in, and record what is there to be seen. It is the perception itself in the mind or brain of the observer that is taken to directly validate the fact, which may be "there is a red tomato in front of me" for example. If it is understood in this way, then the establishment of observable facts is a very private affair. It is accomplished by the individual closely attending to what is presented to him or her in the act of perception. Since two observers do not have access to each other's perceptions, there is no way they can enter into a dialogue about the validity of the facts they are presumed to establish.

This view of perception or observation, as passive and private, is totally inadequate, and does not give an accurate account of perception in everyday life, let alone science. Everyday observation is far from passive. There are a range of things that are *done*, many of them automatically and perhaps unconsciously, to establish the validity of a perception. In the act of seeing we scan objects, move our heads to test for expected changes in the observed scene and so on. If we are not sure whether a scene viewed through a window is something out of the window or a reflection in the window, we can move our heads to check for the effect this has on the direction in which the scene is visible. It is a general point that, if for any reason we doubt the validity of what seems to be the case on the basis of our perceptions, there are various actions we can take to remove the problem. If, in the example about the truth of observation statements depend on what is already known or assumed, thus rendering the observable facts as fallible as the presuppositions underlying them. Both kinds of difficulty suggest that maybe the observable basis for science is not as straightforward and secure as is widely and traditionally supposed. In the next chapter I try to mitigate these fears to some extent by considering the nature of observation, especially as it is employed in science, in a more discerning way than has been involved in our discussion up until now.

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With these few, somewhat elementary, observations I have only touched the surface of the detailed story psychologists can tell about the range of things that are done by individuals in the act of perception. More important for our task is to consider the significance of the point for the role of observation in science. An example that illustrates my point well is drawn from early uses of the microscope in science. When scientists such as Robert Hooke and Henry Powers used the microscope to look at small insects such as flies and ants, they often disagreed about the observable facts, at least initially. Hooke traced the cause of some of the disagreements to different kinds of illumination. He pointed out that the eye of a fly appears like a lattice covered with holes in one kind of light (which, incidentally, seems to have led Powers to believe that this was indeed the case), like a surface covered with cones in another and in yet another light like a surface covered with pyramids. Hooke proceeded to make practical interventions designed to clear up the problem. He endeavoured to eliminate spurious information arising from dazzle and complicated reflections by illuminating specimens uniformly. He did this by using for illumination the light of a candle diffused through a solution of brine. He also illuminated his specimens from various directions to determine which features remained invariant under such changes. Some of the insects needed to be thoroughly intoxicated with brandy to render them both motionless and undamaged.

Hooke's book, *Micrographia* (1665), contains many detailed descriptions and drawings that resulted from Hooke's actions and observations. These productions were and are public, not private. They can be checked, criticised and added to by others. If a fly's eye, in some kinds of illumination, appears to be covered with holes, then that state of affairs cannot be usefully evaluated by the observer closely attending to his or her perceptions. Hooke showed what could be *done* to check the authenticity of the appearances in such cases, and the procedures he recommended could be carried out by anyone suitably inclined and skilled. The observable facts about the structure of a fly's eye that eventuate result from a process that is both active and public.

The point that action can be taken to explore the adequacy of claims put forward as observable facts has the consequence that subjective aspects of perception need not be an intractable problem for science. Ways in which perceptions of the same scene can vary from observer to observer depending on their background, culture and expectations were discussed in the previous chapter. Problems that eventuate from this undoubted fact can be countered to a large extent by taking appropriate action. It should be no news to anyone that the perceptual judgments of individuals can be unreliable for a range of reasons. The challenge, in science, is to arrange the observable situation in such a way that the reliance on such judgments is minimised if not eliminated. An example or two will illustrate the point.

The moon illusion is a common phenomenon. When it is high in the sky, the moon appears much smaller than when it is low on the horizon. This is an illusion. The moon does not change size nor does its distance from earth alter during the few hours that it takes for its relative position to undergo the required change. However, we do not have to put our trust in subjective judgments about the moon's size. We can, for example, mount a sighting tube fitted with cross-wires in such a way that its orientation can be read on a scale. The angle subtended by the moon at the place of sighting can be determined by aligning the cross-wires with each side of the moon in turn and noting the difference in the corresponding scale readings. This can be done when the moon is high in the sky and repeated when it is near the horizon. The fact that the apparent size of the moon has remained unchanged is reflected in the fact that there is no significant variation in the differences between the scale readings in the two cases.

Galileo and the moons of Jupiter

In this section the relevance of the discussion in the previous chapter is illustrated with an historical example. Late in 1609 Galileo constructed a powerful telescope and used it to look at the heavens. Many of the novel observations he made in the ensuing three months were controversial, and very relevant to the astronomical debate concerning the validity of the Copernican theory, of which Galileo became an avid champion. Galileo claimed, for instance, to have sighted four moons orbiting the planet Jupiter, but he had trouble convincing others of the validity of his observations. The matter was of some moment. The Copernican theory involved the controversial claim that the earth moves, spinning on its axis once a day and orbiting the sun once a year. The received view that Copernicus had challenged in the first half of the previous century was that the earth is stationary, with the sun and planets orbiting it. One of the many, far from trivial, arguments against the motion of the earth was that, if it orbited the sun as Copernicus claimed, the moon would be left behind. This argument is undermined once it is acknowledged that Jupiter has moons. For even the opponents of Copernicus agreed that Jupiter moves. Consequently, any moons it has are carried with it, exhibiting the very phenomenon that the opponents of Copernicus claimed to be impossible in the case of the earth.

Whether Galileo's telescopic observations of moons around Jupiter were valid was a question of some moment then. In spite of the initial skepticism, and the apparent inability of a range of his contemporaries to discern the moons through the telescope, Galileo had convinced his rivals within a period of two years. Let us see how he was able to achieve that — how he was able to "objectify" his observations of Jupiter's moons.

Galileo attached a scale, marked with equally spaced horizontal and vertical lines, to his telescope by a ring in such a way that the scale was face-on to the observer and could be slid up and down the length of the telescope. A viewer looking through the telescope with one eye could view the scale with the other. Sighting of the scale was facilitated by illuminating it with a small lamp. With the telescope trained on Jupiter, the scale was slid along the telescope until the image of Jupiter viewed through the telescope with one eye lay in the central square of the scale viewed with the other eye. With this accomplished, the position of a moon viewed through the telescope could be read on the scale, the reading corresponding to its distance from Jupiter in multiples of the diameter of Jupiter. The diameter of Jupiter was a convenient unit, since employing it as a standard automatically allowed for the fact that its apparent diameter as viewed from earth varies as that planet approaches and recedes from the Earth.

Using these, Galileo was able to record the daily histories of the four "starlets" accompanying Jupiter. He was able to show that the data were consistent with the assumption that the starlets were indeed moons orbiting Jupiter with a constant period. The assumption was borne out, not only by the quantitative measurements but also by the more qualitative observation that the satellites occasionally disappeared from view as they passed behind or in front of the parent planet or moved into its shadow.

Galileo was in a strong position to argue for the veracity of his observations of Jupiter's moons, in spite of the fact that they were invisible to the naked eye. He could, and did, argue against the suggestion that they were an illusion produced by the telescope by pointing out that that suggestion made it difficult to explain why the moons appeared near Jupiter and nowhere else. Galileo could also appeal to the consistency and repeatability of his measurements and their compatibility with the assumption that the moons orbit Jupiter with a constant period. Galileo's quantitative data were verified by independent observers, including observers at the Collegio Romano and the Court of the Pope in Rome who were opponents of the Copernican theory. What is more, Galileo was able to predict further positions of the moons and the occurrence of transits and eclipses, and these too were confirmed by himself and independent observers, as documented by Stillman Drake, (1978, pp. 175–6, 236–7).

The veracity of the telescopic sightings was soon accepted by those of Galileo's contemporaries who were competent observers, even by those who had initially opposed him. It is true that some observers could never manage to discern the moons, but I suggest that this is of no more significance than the inability of James Thurber (1933, pp. 101–103) to discern the structure of plant cells through a microscope. The strength of Galileo's case for the veracity of his telescopic observations of the moons of Jupiter derives from the range of practical, objective tests that his claims could survive. Although his case might have stopped short of being absolutely conclusive, it was incomparably stronger than any that could be made for the alternative, namely, that his sightings were illusions or artifacts brought about by the telescope.

Observable facts objective but fallible

An attempt to rescue a reasonably strong version of what constitutes an observable fact from the criticisms that we have levelled at that notion might go along the following lines. An observation statement constitutes a fact worthy of forming part of the basis for science if it is such that it can be straightforwardly tested by the senses and withstands those tests. Here the "straightforward" is intended to capture the idea that candidate observation statements should be such that their validity can be tested in ways that involve routine, objective procedures that do not necessitate fine, subjective judgments on the part of the observer. The emphasis on tests brings out the active, public character of the vindication of observation statements. In this way, perhaps we can capture a notion of fact unproblematically established by observation. After all, only a suitably addicted philosopher will wish to spend time doubting that such things as meter readings can be securely established, within some small margin of error, by careful use of the sense of sight.

A small price has to be paid for the notion of an observable fact put forward in the previous paragraph. That price is that observable facts are to some degree fallible and subject to revision. If a statement qualifies as an observable fact because it has passed all the tests that can be levelled at it hitherto, this does not mean that it will necessarily survive new kinds of tests that become possible in the light of advances in knowledge and technology. We have already met two significant examples of observation statements that were accepted as facts on good grounds but were eventually rejected in the light of such advances, namely, "the earth is stationary" and "the apparent size of Mars and Venus do not change appreciably during the course of the year".

According to the view put forward here, observations suitable for constituting a basis for scientific knowledge are both objective and fallible. They are objective insofar as they can be publicly tested by straightforward procedures, and they are fallible insofar as they may be undermined by new kinds of tests made possible by advances in science and technology. This point can be illustrated by another example from the work of Galileo. In his *Dialogue Concerning the Two Chief World Systems* (1967, pp. 361–3) Galileo described an objective method for measuring the diameter of a star. He hung a cord between himself and the star at a distance such that the cord just blocked out the star. Galileo argued that the angle subtended at the eye by the cord was then equal to the angle subtended at the eye by the star. We now know that Galileo's results were spurious. The apparent size of a star as perceived by us is due entirely to atmospheric and other noise effects and has no determinate relation to the star's physical size. Galileo's measurements of star-size rested on implicit assumptions that are now rejected. But this rejection has nothing to do with subjective aspects of perception. Galileo's observations were objective in the sense that they involved routine procedures which, if repeated today, would give much the same results as obtained by Galileo. In the next chapter we will have cause to develop further the point that the lack of an infallible observational base for science does not derive solely from subjective aspects of perception.

Further reading

For a classic discussion of the empirical basis of science as those statements that withstand tests, see Popper (1972, chapter 5). The active aspects of observation are stressed in the second half of Hacking (1983), in Popper (1979, pp. 341–61) and in Chalmers (1990, chapter 4). Also of relevance is Shapere (1982).

CHAPTER 3

Experiment

Not just facts but relevant facts

In this chapter I assume for the sake of argument that secure facts can be established by careful use of the senses. After all, as I have already suggested, there are a range of situations relevant to science where this assumption is surely justified. Counting clicks on a Geiger counter and noting the position of a needle on a scale are unproblematic examples. Does the availability of such facts solve our problem about the factual basis for science? Do the statements that we assume can be established by observation constitute the facts from which scientific knowledge can be derived? In this chapter we will see that the answer to these questions is a decisive “no”.

One point that should be noted is that what is needed in science is not just facts but relevant facts. The vast majority of facts that can be established by observation, such as the number of books in my office or the colour of my neighbour’s car, are totally irrelevant for science, and scientists would be wasting their time collecting them. Which facts are relevant and which are not relevant to a science will be relative to the current state of development of that science. Science poses the questions, and ideally observation can provide an answer. This is part of the answer to the question of what constitutes a relevant fact for science.

However, there is a more substantial point to be made, which I will introduce with a story. When I was young, my brother and I disagreed about how to explain the fact that the grass grows longer among the cow pats in a field than elsewhere in the same field, a fact that I am sure we were not the first to notice. My brother was of the opinion that it was the fertilising effect of the dung that was responsible, whereas I suspected that it was a mulching effect, the dung trapping