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# The Limitations of Falsificationism

## 1. Theory-dependence of observation and the fallibility of falsifications

The naive falsificationist insists that scientific activity should be concerned with attempts to falsify theories by establishing the truth of observation statements that are inconsistent with them. The more sophisticated falsificationist realizes the inadequacy of this and recognizes the importance of the role played by confirmation of speculative theories as well as the falsification of well-established ones. One thing that both types of falsificationist hold in common, however, is that there is an important qualitative difference in the status of confirmations and falsifications. Theories can be conclusively falsified in the light of suitable evidence, whereas they can never be established as true or even probably true whatever the evidence. Theory acceptance is always tentative. Theory rejection can be decisive. This is the factor that earns falsificationists their title.

The claims of the falsificationist are seriously undermined by the fact that observation statements are theory-dependent and fallible. This can be seen immediately when one recalls the logical point invoked by the falsificationists in support of their case. If true observation statements are given, *then* it is possible to logically deduce from them the falsity of some universal statements, whereas it is not possible to deduce from them the truth of any universal statements. This is an unexceptional point, but it is a conditional one based on the assumption that perfectly secure observation statements are available. But they are not, as was argued at length in Chapter 3. All observation statements are fallible. Consequently, if a universal statement or complex of universal statements constituting a theory or part of a theory clashes with some observation statement, it may be the observation statement that is at fault. Nothing in the logic of the situation requires that it should always be the theory that is rejected on the occasion of a clash with observation. A fallible observation statement might be rejected and the fallible theory with which it clashes retained. This is precisely what was involved when Copernicus's theory was retained and the naked-eye observation that Venus does not change size appreciably during the course of the year, which is inconsistent with the Copernican theory, was rejected. It is also what is involved when modern descriptions of the moon's trajectory are retained and observation statements referring to the fact that the moon is much larger when it is near the horizon than when it is high in the sky are regarded as resulting from an illusion, even though the cause of the illusion is not well understood. Science abounds with examples of the rejection of observation statements and the retention of the theories with which they clash. However securely based on observation a statement may seem to be, the possibility that new theoretical advances will reveal inadequacies in that statement cannot be ruled out. Consequently, straightforward, conclusive falsifications of theories are not achievable.

## 2. Popper's inadequate defence

Popper was aware of the problem discussed in section 1 right from the time he first published the German version of his book *The Logic of Scientific Discovery* in 1934. In Chapter 5 of that book, entitled "The Problem of the Empirical Base", he set out an account of observation and observation statements that took account of the fact that infallible observation statements are not given directly through sensory preceptions. In this section, I will first summarize his account, and then argue that it does not save the falsificationist from the objections of section 1.

Popper's position highlights the important distinction between public observation statements on the one hand and the private perceptual experiences of individual observers on the other. The latter are in some sense "given" to individuals in the act of observing, but there is no straightforward step from those private experiences (which will depend on factors peculiar to each individual observer such as his expectations, prior knowledge, etc.) to an observation statement that is meant to describe the observed situation. An observation statement, formulated in a public language, will be testable and open to modification or rejection. Individual observers may or may not accept a particular observation statement. Their *decision* on the matter will be *motivated* in part by the relevant perceptual experiences, but no perceptual experience on the part of an individual will be sufficient to establish the validity of an observation statement. An observer may be led to accept some observation statement on the basis of a perception and yet that observation statement may be false.

These points can be illustrated by the following examples. "Moons of Jupiter are visible through a telescope" and "Mars is square and intensely coloured" are public observation statements. The first might well have been uttered by Galileo or a supporter and the second was recorded in Kepler's notebook. Both are public, in the sense that they can be entertained and criticized by anyone who has the opportunity to do so. The Galileans' decision to defend the first was motivated by the perceptual experiences that accompanied their telescopic sightings of Jupiter, and Kepler's decision to record the second was likewise based on his perceptual experiences when directing a telescope towards Mars. Both observation statements are testable. Galileo's adversaries insisted that the patches that Galileo had interpreted as moons of Jupiter were aberrations attributable to the functioning of the telescope. Galileo defended his claim about the visibility of Jupiter's moons by arguing that if the moons were aberrations, then moons should appear near the other planets also. The public debate continued, and in this particular case, as telescopes were improved and optical theory developed, the observation statement referring to the moons of Jupiter survived the criticism levelled at it. Most scientists eventually decided to accept the statement. By contrast, Kepler's statement concerning the shape and colour of Mars did not survive criticism and tests. It was soon decided to reject the statement.

The essence of Popper's position on observation statements is that their acceptability is gauged by their ability to survive tests. Those that fail subsequent tests are rejected, while those that survive all the tests to which they are subjected are tentatively retained. In his early work at least, Popper emphasizes the role of decisions made on the part of individuals and groups of individuals to accept or reject what I have called observation statements, and what Popper refers to as "basic statements". Thus he writes, "Basic statements are accepted as the result of a decision or agreement, and to that extent they are conventions",[1] and again,

> Any empirical scientific statement can be presented (by describing experimental arrangements etc.) in such a way that anyone who has learned the relevant technique can test it. If, as a result, he rejects the statement, then it will not satisfy us if he tells us all about his feelings of doubt or about his feelings of conviction as to his perceptions. What he must do is to formulate an assertion which contradicts our own, and give us his instruction for testing it. If he fails to do this, we can only ask him to take another and perhaps a more careful look at our experiment, and think again.[2]

Popper's emphasis on the conscious decisions of individuals introduces a subjective element that clashes somewhat with Popper's later insistence on science as "a process without a subject". This matter will be discussed more fully in later chapters. For the present, I would prefer to reformulate Popper's position on observation statements in a less subjective way, thus: An observation statement is acceptable, tentatively, at a particular stage in the development of a science, if it is able to withstand all the tests made possible by the state of development of the science in question at that stage.

According to the Popperian position, the observation statements that form the basis with respect to which the merit of a scientific theory is to be assessed are themselves fallible. Popper emphasizes the point with a striking metaphor.

> The empirical basis of objective science has thus nothing "absolute" about it. Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were above a swamp. It is like a building erected on piles. The piles are driven down from above into the swamp, but not down to any natural or "given" base; and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, as least for the time being.[3]

But it is precisely the fact that observation statements are fallible, and their acceptance only tentative and open to revision, that undermines the falsificationist position. Theories cannot be conclusively falsified because the observation statements that form the basis for the falsification may themselves prove to be false in the light of later developments. Knowledge available at the time of Copernicus did not permit a legitimate criticism of the observation that the apparent sizes of Mars and Venus remain roughly constant, so that Copernicus's theory, taken literally, could be deemed falsified by that observation. One hundred years later, the falsification could be revoked because of new developments in optics.

Conclusive falsifications are ruled out by the lack of a perfectly secure observational base on which they depend.

## 3. The complexity of realistic test situations

"All swans are white" is certainly falsified if an instance of a non-white swan can be established. But simplified illustrations of the logic of a falsification such as this disguise a serious difficulty for falsificationism that arises from the complexity of any realistic test situation. A realistic scientific theory will consist of a complex of universal statements rather than a single statement like "All swans are white". Further, if a theory is to be experimentally tested, then more will be involved than those statements that constitute the theory under test. The theory will need to be augmented by auxiliary assumptions, such as laws and theories governing the use of any instruments used, for instance. In addition, in order to deduce some prediction the validity of which is to be experimentally tested it will be necessary to add initial conditions such as a decription of the experimental set-up. For instance, suppose an astronomical theory is to be tested by observing the position of some planet through a telescope. The theory must predict the orientation of the telescope necessary for a sighting of the planet at some specified time. The premises from which the prediction is derived will include the interconnected statements that constitute the theory under test, initial conditions such as previous positions of the planet and sun, auxiliary assumptions such as those enabling corrections to be made for refraction of light from the planet in the earth's atmosphere, and so on. Now if the prediction that follows from this maze of premises turns out to be false (in our example, if the planet does not appear at the predicted location), then all that the logic of the situation permits us to conclude is that at least one of the premises must be false. It does not enable us to identify the faulty premise. It may be the theory under test that is at fault, but alternatively it may be an auxiliary assumption or some part of the description of the initial conditions that is responsible for the incorrect prediction. A theory cannot be conclusively falsified, because the possibility that some part of the complex test situation, other than the theory under test, is responsible for an erroneous prediction cannot be ruled out.

Here are some examples from the history of astronomy that illustrate the point.

In an example utilized previously, we discussed how Newton's theory was apparently refuted by the orbit of the planet Uranus. In this case, it turned out not to be the theory at fault but the description of the initial conditions, which did not include a consideration of the yet-to-be-discovered planet Neptune. A second example involves an argument by means of which the Danish astronomer Tycho Brahé claimed to have refuted the Copernician theory a few decades after the first publication of that theory. If the earth orbits the sun, Brahé argued, then the direction in which a fixed star is observed from earth should vary during the course of the year as the earth moves from one side of the sun to the other. But when Brahé tried to detect this predicted parallax with his instruments, which were the most accurate and sensitive ones in existence at the time, he failed. This led Brahé to conclude that the Copernican theory was false. With hindsight, it can be appreciated that it was not the Copernican theory that was responsible for the faulty prediction, but one of Brahé's auxiliary assumptions. Brahé's estimate of the distance of the fixed stars was many times too small. When his estimate is replaced by a more realistic one, the predicted parallax turns out to be too small to be detectable by Brahé's instruments.

A third example is a hypothetical one devised by Imre Lakatos. It reads as follows:

> The story is about an imaginary case of planetary misbehaviour. A physicist of the pre Einsteinian era takes Newton's mechanics and his law of gravitation, N, the accepted initial conditions, I, and calculates, with their help, the path of a newly discovered small planet, *p*. But the planet deviates from the calculated path. Does our Newtonian physicist consider that the deviation was forbidden by Newton's theory and therefore that, once established, it refutes the theory N? No. He suggests that there must be a hitherto unknown planet *p′*, which perturbs the path of *p*. He calculates the mass, orbit, etc. of this hypothetical planet and then asks an experimental astronomer to test his hypothesis. The planet *p′* is so small that even the biggest available telescopes cannot possibly observe it; the experimental astronomer applies for a research grant to build yet a bigger one. In three years time, the new telescope is ready. Were the unknown planet *p′* to be discovered, it would be hailed as a new victory of Newtonian science. But it is not. Does our scientist abandon Newton's theory and his idea of the perturbing planet? No. He suggests that a cloud of cosmic dust hides the planet from us. He calculates the location and properties of this cloud and asks for a research grant to send up a satellite to test his calculations. Were the satellite's instruments (possibly new ones, based on a little-tested theory)

> to record the existence of the conjectural cloud, the result would be hailed as an outstanding victory for Newtonian science. But the cloud is not found. Does our scientist abandon Newton's theory, together with the idea of the perturbing planet and the idea of the cloud which hides it? No. He suggests that there is some magnetic field in that region of the universe which disturbed the instruments of the satellite. A new satellite is sent up. Were the magnetic field to be found, Newtonians would celebrate a sensational victory. But it is not. Is this regarded as a refutation of Newtonian science? No. Either yet another ingenious auxiliary hypothesis is proposed or . . . the whole story is buried in the dusty volumes of periodicals and the story never mentioned again.[4]

If this story is regarded as a plausible one, it illustrates how a theory can always be protected from falsification by deflecting the falsification to some other part of the complex web of assumptions.

## 4. Falsificationism inadequate on historical grounds

An embarrassing historical fact for falsificationists is that if their methodology had been strictly adhered to by scientists then those theories generally regarded as being among the best examples of scientific theories would never have been developed because they would have been rejected in their infancy. Given any example of a classic scientific theory, whether at the time of its first proposal or at a later date, it is possible to find observational claims that were generally accepted at the time and were considered to be inconsistent with the theory. Nevertheless, those theories were not rejected, and it is fortunate for science that they were not. Some historical examples to support my claim follow.

In the early years of its life, Newton's gravitional theory was falsified by observations of the moon's orbit. It took almost fifty years to deflect this falsification on to causes other than Newton's theory. Later in its life, the same theory was known to be inconsistent with the details of the orbit of the planet Mercury, although scientists did not abandon the theory for that reason. It turned out that it was never possible to explain away this falsification in a way that protected Newton's theory.

A second example concerns Bohr's theory of the atom, and is due to Lakatos.[5] Early versions of the theory were inconsistent with the observation that some matter is stable for a time that exceeds about $10^{-8}$ seconds. According to the theory, negatively charged electrons within atoms orbit around positively charged nuclei. But according to the classical electromagnetic theory presupposed by Bohr's theory, orbiting electrons should radiate. The radiation would result in an orbiting electron losing energy and collapsing into the nucleus. The quantitative details of classical electromagnetism yield an estimated time of about $10^{-8}$ seconds for this collapse to occur. Fortunately, Bohr presevered with his theory, in spite of this falsification.

A third example concerns the kinetic theory and has the advantage that the falsification of that theory at birth was explicitly acknowledged by its originator. When Maxwell published the first details of the kinetic theory of gases in 1859, in that very same paper he acknowledged the fact that the theory was falsified by measurements on the specific heats of gases.[6] Eighteen years later, commenting on the consequences of the kinetic theory, he wrote.

> Some of these, no doubt, are very satisfactory to us in our present state of opinion about the constitution of bodies, but there are others which are likely to startle us out of our complacency and perhaps ultimately to drive us out of all the hypotheses in which we have hitherto found refuge into that thoroughly conscious ignorance which is a prelude to every real advance in knowledge.[7]

All the important developments within the kinetic theory took place after this falsification. Once again, it is fortunate that the theory was not abandoned in the face of falsifications by measurements of the specific heats of gases, as the naive falsificationist at least would be forced to insist.

A fourth example, the Copernican Revolution, will be outlined in more detail in the following section. This example will emphasize the difficulties that arise for the falsificationist when the complexities of major theory changes are taken into account. The example will also set the scene for a discussion of some more recent and more adequate attempts to characterize the essence of science and its methods.

## 5. The Copernican Revolution

It was generally accepted in mediaeval Europe that the earth lies at the centre of a finite universe and that the sun, planets and stars orbit around it. The physics and cosmology that provided the framework in which this astronomy was set was basically that developed by Aristotle in the fourth century B.C. In the second century A.D., Ptolemy devised a detailed astronomical system that specified the orbits of the moon, the sun and all the planets.

In the early decades of the sixteenth century, Copernicus devised a new astronomy, an astronomy involving a moving earth, which challenged the Aristotelian and Ptolemaic system. According to the Copernican view, the earth is not stationary at the centre of the universe but orbits the sun along with the planets. By the time Copernicus's idea had been substantiated, the Aristotelian world view had been replaced by the Newtonian one. The details of the story of this major theory change, a change that took place over one and a half centuries, do not lend support to the methodologies advocated by the inductivists and falsificationists, and indicate a need for a different, more complexly structured account of science and its growth.

When Copernicus first published the details of his new astronomy, in 1543, there were many arguments that could be, and were, levelled against it. Relative to the scientific knowledge of the time, these arguments were sound ones and Copernicus could not satisfactorily defend his theory against them. In order to appreciate this situation, it is necessary to be familiar with some aspects of the Aristotelian world view on which the arguments against Copernicus were based. A very brief sketch of some of the relevant points follows.

The Aristotelian universe was divided into two distinct regions. The sub-lunar region was the inner region, extending from the central earth to just inside the moon's orbit. The super-lunar region was the remainder of the finite universe, extending from the moon's orbit to the sphere of the stars, which marked the outer boundary of the universe. Nothing existed beyond the outer sphere, not even space. Unfilled space is an impossibility in the Aristotelian system. All celestial objects in the super-lunar region were made of an incorruptible element called aether. Aether possessed a natural propensity to move around the centre of the universe in perfect circles. This basic idea became modified and extended in Ptolemy's astronomy. Since observations of planetary positions at various times could not be reconciled with circular, earth-centred orbits, Ptolemy introduced further circles, called epicycles, into the system. Planets moved in circles, or epicycles, the centres of which moved in circles around the earth. The orbits could be further refined by adding epicycles to epicycles etc. in such a way that the resulting system was compatible with observations of planetary positions and capable of predicting future planetary positions.

In contrast to the orderly, regular, incorruptible character of the super-lunar region, the sub-lunar region was marked by change, growth and decay, generation and corruption. All substances in the sub-lunar region were mixtures of four elements, air, earth, fire and water, and the relative proportions of elements in a mixture determined the properties of the substance so constituted. Each element had a natural place in the universe. The natural place for earth was at the centre of the universe; for water, on the surface of the earth; for air, in the region immediately above the surface of the earth; and for fire, at the top of the atmosphere, close to the moon's orbit. Consequently, each earthly object would have a natural place in the sub-lunar region depending on the relative proportion of the four elements that it contained. Stones, being mostly earth, have a natural place near the centre of the earth, while flames, being mostly fire, have a natural place near to the moon's orbit, and so on. All objects have a propensity to move in straight lines, upwards or downwards, towards their natural place. Thus stones have a natural motion straight downwards, towards the centre of the earth, and flames have a natural motion straight upwards, away from the centre of the earth. All motions other than natural motions require a cause. For instance, arrows need to be propelled by a bow and chariots need to be drawn by horses.

These, then, are the bare bones of the Aristotelian mechanics and cosmology that were presupposed by contemporaries of Copernicus, and which were utilized in arguments against a moving earth. Let us look at some of the forceful arguments against the Copernican system.

Perhaps the argument that constituted the most serious threat to Copernicus was the so-called tower argument. It runs as follows. If the earth spins on its axis, as Copernicus had it, then any point on the earth's surface will move a considerable distance in a second. If a stone is dropped from the top of a tower erected on the moving earth, it will execute its natural motion and fall towards the centre of the earth. While it is doing so the tower will be sharing the motion of the earth, due to its spinning. Consequently, by the time the stone reaches the surface of the earth the tower will have moved around from the position it occupied at the beginning of the stone's downward journey. The stone should therefore strike the ground some distance from the foot of the tower. But this does not happen in practice. The stones strikes the ground at the base of the tower. It follows that the earth cannot be spinning and that Copernicus's theory is false.

Another mechanical argument against Copernicus concerns loose objects such as stones, philosophers, etc. resting on the surface of the earth. If the earth spins, why are such objects not flung from the earth's surface, as stones would be flung from the rim of a rotating wheel? And if the earth, as well as spinning, moves bodily around the sun, why doesn't it leave the moon behind?

Some arguments against Copernicus based on astronomical considerations have been mentioned earlier in this book. They involved the absence of parallax in the observed positions of the stars and the fact that Mars and Venus, as viewed by the naked eye, do not change size appreciably during the course of the year.

Because of the arguments I have mentioned, and others like them, the supporters of the Copernican theory were faced with serious difficulties. Copernicus himself was very much immersed in Aristotelian metaphysics and had no adequate response to them.

In view of the strength of the case against Copernicus, it might well be asked just what there was to be said in favour of the Copernican theory in 1543. The answer is, "not very much". The main attraction of the Copernican theory lay in the neat way it explained a number of features of planetary motion, which could be explained in the rival Ptolemaic theory only in an unattractive, artificial way. The features are the retrograde motion of the planets and the fact that, unlike the other planets, Mercury and Venus always remain in the proximity of the sun. A planet at regular intervals regresses, that is, stops its westward motion among the stars (as viewed from earth) and for a short time retraces its path eastward before continuing its journey westward once again. In the Ptolemaic system, retrograde motion was explained by the somewhat *ad hoc* manoeuvre of adding epicycles especially designed for the purpose. In the Copernican system, no such artificial move is necessary. Retrograde motion is a natural consequence of the fact that the earth and the planets together orbit the sun against the background of the fixed stars. Similar remarks apply to the problem of the constant proximity of the sun, Mercury and Venus. This is a natural consequence of the Copernican system once it is established that the orbits of Mercury and Venus are inside that of the earth. In the Ptolemaic system, the orbits of the sun, Mercury and Venus have to be artificially linked together to achieve the required result.

There were some mathematical features of the Copernican theory that were in its favour, then. Apart from these, the two rival systems were more or less on a par as far as simplicity and accord with observations of planetary positions are concerned. Circular sun-centred orbits cannot be reconciled with observation, so that Copernicus, like Ptolemy, needed to add epicycles, and the total number of epicycles needed to produce orbits in accord with known observations was about the same for the two systems. In 1543, the arguments from mathematical simplicity that worked in favour of Copernicus could not be regarded as an adequate counter to the mechanical and astronomical arguments that worked against him. Nevertheless, a number of mathematically capable natural philosophers were to be attracted to the Copernican system, and their efforts to defend it became increasingly successful over the next hundred years or so.

The person who contributed most significantly to the defence of the Copernican system was Galileo. He did so in two ways. Firstly, he used a telescope to observe the heavens, and in so doing he transformed the observational data that the Copernican theory was required to explain.[8] Secondly, he devised the beginnings of a new mechanics that was to replace Aristotelian mechanics and with reference to which the mechanical arguments against Copernicus were defused.

When, in 1609, Galileo constructed his first telescopes and trained them on the heavens, he made dramatic discoveries. He saw that there were many stars invisible to the naked eye. He saw that Jupiter had moons and he saw that the surface of the earth's moon was covered with mountains and craters. He also observed that the apparent size of Mars and Venus, as viewed through the telescope, changed in the way predicted by the Copernican system. Later, Galileo was to confirm that Venus had phases like the moon, as Copernicus had predicted but which clashed with Ptolemy's system. The moons of Jupiter defused the Aristotelian argument against Copernicus based on the fact that the moon stays with an allegedly moving earth. For now Aristotelians were faced with the same problem with respect to Jupiter and its moons. The earthlike surface of the moon undermined the Aristotelian distinction between the perfect, incorruptible heavens and the changing, corruptible earth. The discovery of the phases of Venus marked a success for the Copernicans and a new problem for the Ptolemaics. It is undeniable that once the observations made by Galileo through his telescope are accepted, the difficulties facing the Copernican theory are diminished.

The foregoing remarks on Galileo and the telescope raise a serious epistemological problem. Why should observations through a telescope be preferred to naked-eye observations? One answer to this question might utilize an optical theory of the telescope that explains its magnifying properties and that also gives an account of the various aberrations to which we can expect telescopic images to be subject. But Galileo himself did not utilize an optical theory for that purpose. The first optical theory capable of giving support in this direction was devised by Galileo's contemporary, Kepler, early in the sixteenth century, and this theory was improved and augmented in later decades. A second way of facing our question concerning the superiority of telescopic to naked-eye observations is to demonstrate the effectiveness of the telescope in a practical way, by focusing it on distant towers, ships, etc. and demonstrating how the instrument magnifies and renders objects more distinctly visible. However, there is a difficulty with this kind of justification of the use of the telescope in astronomy. When terrestrial objects are viewed through a telescope, it is possible to separate the viewed object from aberrations contributed by the telescope because of the observer's familiarity with what a tower, a ship, etc. looks like. This does not apply when an observer searches the heavens for he knows not what. It is significant in this respect that Galileo's drawing of the moon's surface as he saw it through a telescope contains some craters that do not in fact exist there. Presumably those "craters" were aberrations arising from the functioning of Galileo's far-from-perfect telescopes. Enough has been said in this paragraph to indicate that the justification of telescopic observations was no simple, straightfoward matter. Those adversaries of Galileo who queried his findings were not all stupid, stubborn reactionaries. Justifications were forthcoming, and became more and more adequate as better and better telescopes were constructed and as optical theories of their functioning were developed. But all this took time.

Galileo's greatest contribution to science was his work in mechanics. He laid some of the foundations of the Newtonian mechanics that was to replace Aristotle's. He distinguished clearly between velocity and acceleration and asserted that freely falling objects move with a constant acceleration that is independent of their weight, dropping a distance proportional to the square of the time of fall. He denied the Aristotelian claim that all motion requires a cause and in its place proposed a circular law of inertia, according to which a moving object subject to no forces will move indefinitely in a circle around the earth at uniform speed. He analyzed projectile motion by resolving the motion of a projectile into a horizontal component moving with a constant velocity obeying his law of inertia, and a vertical component subject to a constant acceleration downwards. He showed that the resulting path of a projectile was a parabola. He developed the concept of relative motion and argued that the uniform motion of a system could not be detected by mechanical means without access to some reference point outside of the system.

These major developments were not achieved instantaneously by Galileo. They emerged gradually over a period of half a century, culminating in his book *Two New Sciences,*[9] which was first published in 1638, almost a century after the publication of Copernicus's major work. Galileo rendered his new conceptions meaningful and increasingly more precise by means of illustrations and thought experiments. Occasionally, Galileo described actual experiments, for instance, experiments involving the rolling of spheres down inclined planes, although just how many of these Galileo actually performed is a matter of some dispute.

Galileo's new mechanics enabled the Copernican system to be defended against some of the objections to it mentioned above. An object held at the top of a tower and sharing with the tower a circular motion around the earth's centre will continue in that motion, along with the tower, after it is dropped and will consequently strike the ground at the foot of the tower, consistent with experience. Galileo took the argument further and claimed that the correctness of his law of inertia could be demonstrated by dropping a stone from the top of the mast of a uniformly moving ship and noting that it strikes the deck at the foot of the mast, although Galileo did not claim to have performed the experiment. Galileo was less successful in explaining why loose objects are not flung from the surface of a spinning earth. With hindsight, this can be attributed to the inadequacies of his principle of inertia and of his lack of a clear conception of gravity as a force.

Although the bulk of Galileo's scientific work was designed to strengthen the Copernican theory, Galileo did not himself devise a detailed astronomy, and seemed to follow the Aristotelians in their preference for circular orbits. It was Galileo's contemporary, Kepler, who contributed a major breakthrough in that direction when he discovered that each planetary orbit could be represented by a single ellipse, with the sun at one focus. This eliminated the complex system of epicycles that both Copernicus and Ptolemy had found necessary. No similar simplification is possible in the Ptolemaic, earth-centre system. Kepler had at his disposal Tycho Brahe's recordings of planetary positions, which were more accurate than those available to Copernicus. After a painstaking analysis of the data, Kepler arrived at his three laws of planetary motion, that planets move in elliptical orbits around the sun, that a line joining a planet to the sun sweeps out equal areas in equal times, and that the square of the period of a planet is proportional to the cube of its mean distance from the sun.

Galileo and Kepler certainly strengthened the case in favour of the Copernican theory. However, more developments were necessary before that theory was securely based on a comprehensive physics. Newton was able to take advantage of the work of Galileo, Kepler and others to construct that comprehensive physics that he published in his *Principia* in 1687. He spelt out a clear conception of force as the cause of acceleration rather than motion, a conception that had been present in a somewhat confused way in the writings of Galileo and Kepler. Newton replaced Galileo's law of circular inertia with his own law of linear inertia, according to which bodies continue to move in straight lines at uniform speed unless acted on by a force. Another major contribution by Newton was of course his law of gravitation. This enabled Newton to explain the approximate correctness of Kepler's laws of planetary motion and Galileo's law of free fall. In the Newtonian system, the realms of the celestial bodies and of earthly bodies were unified, each set of bodies moving under the influence of forces according to Newton's laws of motion. Once Newton's physics had been constituted, it was possible to apply it in detail to astronomy. It was possible, for instance, to investigate the details of the moon's orbit, taking into account its finite size, the spin of the earth, the wobble of the earth upon its axis, and so on. It was also possible to investigate the departure of the planets from Kepler's laws due to the finite mass of the sun, interplanetary forces, etc. Developments such as these were to occupy some of Newton's successors for the next couple of centuries.

The story I have sketched here should be sufficient to indicate that the Copernican Revolution did not take place at the drop of a hat or two from the Leaning Tower of Pisa. It is also clear that neither the inductivists nor the falsificationists give an account of science that is compatible with it. New concepts of force and inertia did not come about as a result of careful observation and experiment. Nor did they come about through the falsification of bold conjectures and the continual replacement of one bold conjecture by another. Early formulations of the new theory, involving imperfectly formulated novel conceptions, were persevered with and developed in spite of apparent falsifications. It was only after a new system of physics had been devised, a process that involved the intellectual labour of many scientists over several centuries, that the new theory could be successfully matched with the results of observation and experiment in a detailed way. No account of science can be regarded as anywhere near adequate unless it can accommodate such factors.

## FURTHER READING

Lakatos's criticism of anything but the most sophisticated brands of falsificationism is in his article "Falsification and the Methodology of Scientific Research Programmes", in *Criticism and the Growth of Knowledge,* ed. I. Lakatos and A. Musgrave (Cambridge: Cambridge University Press, 1974), pp.91-196. Other classic criticisms are in P. Duhem, *The Aim and Structure of Physical Theory* (New York: Athenum, 1962) and W.V.O. Quine's article "Two Dogmas of Empiricism", in his *From a Logical Point of View* (New York: Harper and Row, 1961), pp.20-46. Historical accounts of the Copernican Revolution that poses difficulties for falsificationists are in T. Kuhn, *The Copernican Revolution* (New York: Random House, 1959); A. Koyré, *Metaphysics and Measurement* (London: Chapman and Hall, 1968); and P.K. Feyerabend, *Against Method: Outline of an Anarchistic Theory of Knowledge* (London: New Left Books, 1975). Lakatos's article, "Popper on Demarcation and Induction", in *The Philosophy of Karl R. Popper,* ed. P.A. Schilpp (La Salle, Illinois: Open Court, 1974), is critical of the falsificationist claim to have solved the problem of induction. Kuhn criticizes falsificationism in *The Structure of Scientific Revolutions* (Chicago: Chicago University Press, 1970) and in "Logic of Discovery of Psychology of Research?", in *Criticism and the Growth of Knowledge,* ed. Lakatos and Musgrave, pp.1-23.

1. K.R. Popper, *The Logic of Scientific Discovery* (London: Hutchinson, 1968), p.106.
2. Ibid., p.99.
3. Ibid., p.111.
4. I. Lakatos, "Falsification and the Methodology of Scientific Research Programmes", in *Criticism and the Growth of Knowledge,* ed. I. Lakatos and A. Musgrave (Cambridge: Cambridge University Press, 1974), p.100-101.
5. Ibid., p.140-54.
6. J.C. Maxwell, "Illustrations of the Dynamical Theory of Gases", read before the British Association in 1859 and reprinted in *The Scientific Papers of James Clerk Maxwell,* 2 vols., ed. W.D. Niven (New York: Dover, 1965), vol.1, pp.377-409. See especially the final paragraph of the paper.

7. J.C. Maxwell, "The Kinetic Theory of Gases", *Nature* 16 (1877): 245-46.
8. My remarks on Galileo and the telescope, and several other aspects of my estimate of Galileo's physics stem from Feyerabend's provocative account in *Against Method: Outline of an Anarchistic Theory of Knowledge,* (London: New Left Books, 1975), pp.69-164.
9. Galileo Galilei, *Two New Sciences,* trans. Stillman Drake (Madison: University of Wisconsin Press, 1974).

# 7

# Theories as Structures: 1. Research Programmes

## 1. Theories should be considered as structural wholes

The sketch of the Copernican Revolution presented in the previous chapter strongly suggests that the inductivist and falsificationist accounts of science are too piecemeal. Concentrating on the relationships between theories and individual observation statements or sets of them, they fail to take account of the complexity of major scientific theories. Neither the naive inductivist emphasis on the inductive derivation of theories from observation, nor the falsificationist scheme of conjectures and falsifications, is capable of yielding an adequate characterization of the genesis and growth of realistically complex theories. More adequate pictures involve the depiction of theories as structured wholes of some kind.

One reason why it is necessary to regard theories as structures stems from a study of the history of science. Historical study reveals that the evolution and progress of major sciences exhibit a structure that is not captured by the inductivist or falsificationist accounts. The programmatic development of the Copernican theory over more than a century has already provided us with one example. Later in this chapter we will meet others. However, the historical argument is not the only ground for the claim that theories are structural wholes of some kind. Another more general philosophical argument is closely linked with the theory-dependence of observation. It was stressed in Chapter 3 that observation statements must be formulated in the language of some theory. Consequently, the statements, and the concepts figuring in them, will be as precise and informative as the theory in whose language they are framed is precise and informative. For example, I think it will be agreed that the Newtonian concept of mass has a