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Popper's Critique of Inductivism.
His Theory of Conjectures and Refutations (or Falsificationism)

2.1 Popper's Critique of Inductivism

The inductivist thinks that science proceeds by first collecting observations or data (Bacon's 'countless grapes... ripe and fully seasoned') and then inferring laws and predictions from this data by induction. Popper argues against this that one cannot simply observe without a theoretical background. Here is how he puts the argument:

The belief that science proceeds from observation to theory is still so widely and so firmly held that my denial of it is often met with incredulity. I have even been suspected of being insincere – of denying what nobody in his senses can doubt.

But in fact the belief that we can start with pure observations alone, without anything in the nature of a theory, is absurd; as may be illustrated by the story of the man who dedicated his life to natural science, wrote down everything he could observe, and bequeathed his priceless collection of observations to the Royal Society to be used as inductive evidence. This story should show us that though beetles may profitably be collected, observations may not.

Twenty-five years ago I tried to bring home the same point to a group of physics students in Vienna by beginning a lecture with the following instructions: 'Take pencil and paper; carefully observe, and write down what you have observed!' They asked, of course, what I wanted them to observe. Clearly the instruction, 'Observe!' is absurd. . . . Observation is always selective. It needs a chosen object, a definite task, an interest, a point of view, a problem. And its description presupposes a descriptive language, with property words; it presupposes similarity and classification, which in its turn presupposes interests, points of view, and problems. (Popper, 1963, p. 46)

The same thing applies even if we go right back to the beginnings of science or of an individual human life. Popper argues that something like modern science developed in ancient Greece through criticism and modification of an older mythological picture of the world. New-born babies do not have blank minds, but inborn expectations as the result of genetic inheritance. However, as Popper points out, these expectations may be disappointed. The new-born child expects to be fed, but may be abandoned and starve.

Let us see how this argument of Popper's applies to the Kepler example. It will be remembered that Tycho Brahe made his observations of the heavens, particularly of the planets, between 1576 and 1597. Now Copernicus' new theory of the universe had been published in 1543. By the 1570s there was a major theoretical dispute in astronomy between the upholders of the older Aristotelian-Ptolemaic view that the Earth was stationary at the centre of the universe and the Sun went round the Earth and the Copernicans, who thought that the Sun was stationary at the centre and the Earth went round the Sun. Tycho Brahe's observations were relevant to this theoretical controversy. At a more basic level, even his division of heavenly bodies into stars and planets involved a theoretical classification. Indeed, this classification was different in the two contending theories. In the Ptolemaic theory, a planet was a heavenly body which was not a fixed star and which moved round the Earth; so the Sun was a planet, but the Earth was not a planet. In the Copernican theory, a planet was a body which was not a fixed star and which moved round the Sun; so, on this account, the Sun was not a planet, but the Earth was.

Let us turn to a second argument of Popper's. This concerns the principle of induction. Some inductivists think that their inductive inferences are justified by the principle of induction. But how is the principle of induction itself justified? If we try to justify it inductively by experience, we get into a circle, since any induction from experience depends on the principle of induction. We might try to avoid making this circle vicious by justifying the basic principle of induction by a higher-order principle of induction. But then this would, in turn, have to be justified by a still higher-order principle of induction, and so on. Thus, if we try to justify the principle of induction inductively by experience, we get either a vicious circle or an infinite regress. The other possibility is to try to justify it independently of experience, or a priori. However, this looks like a blind act of faith. This is how Popper puts the argument:
Inconsistencies may easily arise in connection with the principle of induction. . . . For the principle of induction must be a universal statement in its turn. Thus if we try to regard its truth as known from experience, then the very same problems which occasioned its introduction will arise all over again. To justify it, we should have to employ inductive inferences; and to justify these we should have to assume an inductive principle of a higher order; and so on. Thus the attempt to base the principle of induction on experience breaks down, since it must lead to an infinite regress.

Kant tried to force his way out of this difficulty by taking the principle of induction (which he formulated as the 'principle of universal causation') to be 'a priori valid'. But I do not think that his ingenious attempt to provide an a priori justification for synthetic statements was successful.

My own view is that the various difficulties of inductive logic here sketched are insurmountable. (1934, p. 29)

Popper (1963, p. 289) argues that the same objection applies to the principle of uniformity of nature, which he regards as a kind of principle of induction.

It is interesting to see how Russell responded to objections of this sort. Russell agreed that we cannot justify the principle of induction by experience, and concluded that we must accept the principle a priori, or, as he put it, 'on the ground of its intrinsic evidence'. This is what Russell said: 'We can never use experience to prove the inductive principle without begging the question. Thus we must either accept the inductive principle on the ground of its intrinsic evidence, or forgo all justification of our expectations about the future' (Russell, 1912, p. 106). He regards forgoing all justification of our expectations about the future — that is, complete scepticism — as nothing less than intellectually frivolous. Thus he advocates an a priori acceptance of the principle of induction 'on the ground of its intrinsic evidence'. He thinks that the acceptance of the principle of induction is necessary in order to be a scientist: 'The general principles of science, such as the belief in the reign of law, and the belief that every event must have a cause, are as completely dependent upon the inductive principle as are the beliefs of daily life' (p. 107).

Russell thus holds that we must, however reluctantly, believe in the principle of induction as a kind of blind act of faith in order to do science. And here we come to Popper's most fundamental criticism; for Popper thinks that we can be scientists and do science without making any inductive inferences. Hence we do not need a principle of induction to justify inductive inferences; consequently, there is no need to have blind faith in such a principle. Popper thus gets round the problem by suggesting a non-inductive theory of scientific method. This is his method of conjectures and refutations, which I will now expound.

2.2 Popper's Theory of Conjectures and Refutations
(or Falsificationism)

Hume pointed out that, from observations and deductive logic, we can never infer the truth of a generalization. Thus however many white swans we see, we can never infer the truth that all swans are white.

Popper, however, observed that, although observations and deductive logic cannot establish the truth of a scientific generalization (or verify it), they can establish its falsity (or refute or falsify it). Thus, from the observation 'This is a black swan', we can infer by deductive logic that the generalization 'All swans are white' is false. In other words, we can refute or falsify a scientific generalization. Popper refers to this as the asymmetry between falsification and verification.

This leads Popper to his conjectures and refutations, or falsificationist, account of scientific method. Science does not start with observations, as the inductivist claims, but with conjectures. The scientist then tries to refute these conjectures by criticism and testing (experiments and observations). A conjecture which has withstood a number of severe tests may be tentatively accepted, but only tentatively. We can never know a scientific theory, law, or generalization with certainty. It may break down on the very next test or observation (as in the case of the discovery of black swans in Australia).

To illustrate the tentative, conjectural nature of scientific knowledge, Popper is fond of citing the example of Newtonian mechanics. Newton's theory produced an extraordinarily good fit with observation and experiment from the time it was published (1687) until 1900. Nevertheless, between 1900 and 1920 it was found to be inaccurate in some respects, and corrections were introduced using relativistic mechanics.
2.3 The Distinction between Discovery and Justification

Popper's theory of conjectures and refutations leads naturally to a distinction between the discovery of scientific hypotheses and their justification or validation. This is how Popper himself puts the matter:

The work of the scientist consists in putting forward and testing theories.

The initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it. The question how it happens that a new idea occurs to a man — whether it is a musical theme, a dramatic conflict, or a scientific theory — may be of great interest to empirical psychology; but it is irrelevant to the logical analysis of scientific knowledge. This latter is concerned not with questions of fact (Kant's quid facti?), but only with questions of justification or validity (Kant's quid juris?). Its questions are of the following kind. Can a statement be justified? And if so, how? Is it testable? Is it logically dependent on certain other statements? Or does it perhaps contradict them? (1934, p. 31)

Note that this theory of Popper's brings about a certain rapprochement between science and the arts. Great scientists can, according to Popper, possess the kind of creativity which is recognized as a possession of great artists. A classic example of creative intuition in science is provided by Kekulé's discovery in 1865 that the six carbon atoms of the benzene molecule are arranged in a ring. Kekulé, while meditating on the structure of benzene, fell asleep and dreamt of a snake biting its tail. On awakening, he hit on the solution of his problem.

Popper goes on to consider the view that philosophers of science should attempt to give a rational reconstruction of the process of discovery. As he puts it: 'Some might object that it would be more to the purpose to regard it as the business of epistemology to produce what has been called a “rational reconstruction” of the steps that have led the scientist to a discovery — to the finding of some new truth' (p. 31). Popper, however, rejects such a view, because he thinks that discovery always contains an irrational, creative element. As he puts it:

My view of the matter, for what it is worth, is that there is no such thing as a logical method of having new ideas, or a logical reconstruction of this process. My view may be expressed by saying that every discovery contains 'an irrational element', or 'a creative

intuition', in Bergson's sense. In a similar way, Einstein speaks of the 'search for those highly universal laws . . . from which a picture of the world can be obtained by pure deduction. There is no logical path, he says, 'leading to these . . . laws. They can only be reached by intuition, based upon something like an intellectual love (Einfühlung) of the objects of experience. (p. 32)

So Popper's view is that there is no such thing as a logic of scientific discovery — only a logic of scientific testing.

The distinction between discovery and justification is related to the distinction between inductivism and Bayesianism which we made in the last chapter. Bayesianism is indeed a theory of justification, not of discovery. Bayesians seek to justify scientific generalizations or predictions by showing that, although they are not certain, they can none the less be shown to be probable, given the evidence used to support them. It is thus possible to be a Bayesian regarding the justification of scientific generalizations and predictions, while denying the inductivist theory of how they are discovered. Indeed, as we remarked in the last chapter, Carnap held a position of this sort in his later period. Thus, in his 1950 book, he accepts, with acknowledgement, the Einstein—Popper position on discovery. As he puts it:

But in one point the present opinions of most philosophers and scientists seem to agree, namely, that the inductive procedure is not, so to speak, a mechanical procedure prescribed by fixed rules. If, for instance, a report of observational results is given, and we want to find a hypothesis which is well confirmed and furnishes a good explanation for the events observed, then there is no set of fixed rules which would lead us automatically to the best hypothesis or even a good one. It is a matter of ingenuity and luck for the scientist to hit upon a suitable hypothesis; . . . This point, the impossibility of an automatic inductive procedure, has been especially emphasized, among others by Karl Popper . . ., who also quotes a statement by Einstein . . . The same point has sometimes been formulated by saying that it is not possible to construct an inductive machine. The latter is presumably meant as a mechanical contrivance which, when fed an observational report, would furnish a suitable hypothesis, just as a computing machine when supplied with two factors furnishes their product. I am completely in agreement that an inductive machine of this kind is not possible. (Carnap, 1950, p. 192)

The development since 1950 of ever more powerful computers has reawakened hopes that it might after all be possible to construct an
automatic inductive machine. Indeed, a branch of the subject of artificial intelligence, known as machine learning, has as its aim the programming of computers to produce generalizations when fed with data. In the next chapter we shall examine one approach to machine learning which has been developed by Herbert Simon and his team at Carnegie-Mellon University.

Returning to Carnap in the 1950s, we must next emphasize that, although he rejected an inductivist, or Baconian, account of scientific discovery, he accepted the Bayesian theory of scientific justification. He believed that scientific predictions could be justified inductively by showing that they have a high probability on the known evidence. Popper, on the other hand, has always rejected both inductivism as a theory of discovery and Bayesianism as a theory of justification.

2.4 Some General Observations on Popper's Theory of Scientific Method

Popper gives the following summary of his theory of scientific method:

Knowledge can grow, and... science can progress—just because we can learn from our mistakes.

The way in which knowledge progresses, and especially our scientific knowledge, is by unjustified (and unjustifiable) anticipations, by guesses, by tentative solutions to our problems, by conjectures. These conjectures are controlled by criticism; that is, by attempted refutations, which include severely critical tests. They may survive these tests; but they can never be positively justified: they can neither be established as certainly true nor even as 'probable' (in the sense of the probability calculus). (1963, Preface, p. vii)

This is a very interesting passage, and I will make a number of comments on it. To begin with, Popper speaks of our knowledge progressing ‘by unjustified... anticipations’. A learned reader might suspect that there is here a hidden reference to Bacon, and a desire to make what Bacon regarded as undesirable into an integral part of scientific procedure. These suspicions would be quite correct, for, to a passage in his earlier Logic of Scientific Discovery (1934), Popper adds a footnote referring to a section of the Novum Organum (First Book, XXVI), which was quoted in 1.1. The passage in question runs as follows:

Like Bacon, we might describe our own contemporary science—‘the method of reasoning which men now ordinarily apply to nature’—as consisting of ‘anticipations, rash and premature’ and of ‘prejudices’.

But these marvellously imaginative and bold conjectures or ‘anticipations’ of ours are carefully and soberly controlled by systematic tests. Once put forward, none of our ‘anticipations’ are dogmatically upheld. Our method of research is not to defend them, in order to prove how right we were. On the contrary, we try to overthrow them. Using all the weapons of our logical, mathematical, and technical armory, we try to prove that our anticipations were false—in order to put forward, in their stead, new unjustified, and unjustifiable anticipations... (Popper, 1934, 278–9)

It should next be observed that at the end of the passage quoted from the preface to his 1963 book, Popper explicitly rejects Bayesianism. He argues that ‘these conjectures... can neither be established as certainly true nor even as “probable” (in the sense of the probability calculus).’ Of course, the Bayesian thesis is precisely that scientific conjectures can be made probable in the sense of the mathematical calculus of probability. However, Popper is not content just to criticize the Bayesian attempt to justify scientific conjectures; he states the much stronger thesis that such conjectures cannot be justified at all, or, as he puts: ‘the conjectures... can never be positively justified’. He also speaks in both the 1934 and the 1963 passages of ‘unjustified (and unjustifiable) anticipations’ (my emphasis). It should be clear from all this that Popper’s critique of inductivism contains quite a number of different theses, of different kinds and of different strengths. I will next try to disentangle a few of them, and to comment on their plausibility.

Let us take first Popper’s thesis that there are no such things as inductive inferences, analogous to deductive inferences, by which scientific generalizations and predictions can be obtained from observational data. Popper suggests instead that all such generalizations and predictions are conjectures, and that the important point is not how such conjectures are obtained—any method will do—but that they should be tested severely once they have been proposed.

This thesis of Popper’s seems to me plausible, and it also brings about a great simplification in the theory of scientific method. We do not have to postulate any curious process of inductive inference
and investigate its character. Instead, the simple procedure of conjecturing, followed by deductive inferences, suffices. Moreover, the simplification of scientific method does not end here.

As we have seen, those who, like Russell, postulate inductive inferences naturally raise the question of how such inferences can be justified. This leads them to claim that inductive inferences need to be justified by some principle of induction, or principle of the uniformity of nature. It has to be said, however, that this whole approach is most unsatisfactory. As Popper points out, the justification of these principles in turn is liable to lead to a vicious circle or an infinite regress. Russell's attempt to avoid this by saying that we must accept these principles a priori on the ground of their intrinsic evidence is hardly very plausible. As a matter of fact, Russell's own formulation of the principle of induction contains an error, as we saw, and even when this error is corrected, it is by no means clear that the resulting principle is correct. Matters are no better with the principle of the uniformity of nature, which Russell formulates as follows: 'The belief in the uniformity of nature is the belief that everything that has happened or will happen is an instance of some general law to which there are no exceptions' (1912, p. 99).

There seems more reason for believing this to be false than true, however. Is it not more likely that some things happen by chance and are not instances of general laws? The principle does not appear to be necessary for science either. Surely, science would still be possible, even if the universe contained a certain amount of intrinsic randomness. To sum up then: it seems to be difficult, if not impossible, to formulate the alleged principles of induction and uniformity of nature in such a way that they are even plausible, let alone obviously true a priori. Surely, then, it is better to get rid of these obscure and unsatisfactory principles if we can. Popper's first thesis shows a way in which this can indeed be done. It is thus marks, in my view, a definite advance over the Cambridge school.

Popper's second thesis is that Bayesianism should be rejected. Unfortunately, we cannot discuss the arguments for and against this view in the present, non-technical book. It can be observed in general terms, however, that this thesis, like the first, is by no means implausible. The Bayesian claims to be able to calculate the probability of some scientific prediction, given the evidence in its favour. Can such computations really be performed? Or do we have here a misuse of the mathematical theory of probability? Popper's scepticism about such calculations seems prima facie quite reasonable.

But, while Popper's first two theses are quite plausible, the same cannot be said of his third thesis, the thesis that scientific conjectures can never be positively justified. Consider a theory (T, say) put forward at time t₁ by a scientist (Dr E, say). Let us suppose that at t₁ there is really no evidence in favour of T, so that it can be regarded as purely conjectural. Between t₁ and t₂, however, Dr E and others show that T can explain a whole mass of observational data. T is, moreover, subjected to a whole series of experimental tests, and by t₂ has passed every single one of them. Now most people would surely say that, while there was no evidential justification for T at time t₁, the evidence which has accumulated by time t₂ has strongly vindicated T, and that any technologist would by then be justified in using T as the basis for some practical application. But Popper in his third thesis seems committed to the view that T is just as unjustified at t₂ as it was at t₁. Indeed, a theory like T cannot be justified, because such theories ('anticipations') are intrinsically unjustifiable.

In short, Popper's third thesis flies in the face of common sense, and does not seem to me to be acceptable. Admittedly, those who deny Popper's third thesis have to explain how exactly scientific conjectures can come to be justified by the evidence used to support them, and this is certainly no easy matter. An investigation of the matter would once again involve us in mathematical questions about probability, so cannot be attempted here. But the point I would like to stress by way of conclusion is that it is perfectly possible to accept Popper's first two quite plausible theses without accepting his rather implausible third thesis.

Let us now turn from Popper's critique of inductivism to his own positive view that scientific knowledge grows through conjectures and refutations. Now it would seem to be a good plan to apply Popper's own methodological principles to his theory of scientific method and to subject that theory to the most severe tests we can devise. Any theory of scientific method can be tested against episodes from the history of science. Let us therefore consider some famous advances in science which appear at first sight to fit the inductive model much better than Popper's falsificationist model. We can then see whether it might be possible to account for these scientific developments in terms of conjectures and refutations. I have already introduced one such example: namely, Kepler's discovery that planets move in ellipses, and we shall begin a more detailed consideration of this example in the next section.

Kepler's achievement belongs to astronomy and physics, and was carried out in the seventeenth century; but we must beware of basing any account of scientific method too much on examples
Drawn from a single branch of natural science or from a single historical period of scientific development. I will therefore supplement the Kepler example with two further examples drawn from twentieth-century science, and from the biological and medical field. The first of these is Alexander Fleming's discovery of penicillin. I will recount the story in more detail later, but its outlines are well known. Fleming observed a culture-plate which had become accidentally contaminated by a mould. His own photograph of the culture-plate is reproduced as Plate 1. Fleming concluded that the mould was producing a substance which had destroyed the bacteria growing on the culture-plate and which might, therefore, be a suitable antibiotic for the treatment of bacterial infections. Surely, here, observation preceded theory, and Fleming made an inductive inference from his observation. My final example is closely related to Fleming's discovery, but has some rather different features. It is the discovery of the sulphonamide drugs by the German chemical company I. G. Farben Industrie. The chemists working for the company examined literally hundreds of chemical compounds, until they hit, to some extent by accident, on one which cured mice infected by haemolytic streptococci. This really seems to fit the Baconian model of a mass of careful observations which eventually reveal a 'secret of excellent use'. These three examples, therefore, appear at first sight to fit the inductiveist model much better than the falsificationist model. But our further examination of them will show that this initial impression is to some extent misleading, and that an analysis in terms of conjectures and refutations is, in each case, more plausible than might at first be thought. Yet the apparently inductiveist features of the three examples are not illusory, and our analysis will lead us eventually to a kind of synthesis between inductivism and falsificationism.

2.5 Kepler's Discovery of the Elliptic Orbits of the Planets

Let us consider, then, Kepler's discovery that the planet Mars moves in an ellipse with the Sun at one focus. At first sight it seems as if Kepler inferred his law inductively from Tycho Brahe's observations. But let us look a little more closely at what happened.

To begin with, Kepler started his investigation with a number of theoretical assumptions. He was already a convinced Copernican, and so related Mars's orbit to the Sun rather than the Earth. Indeed, he assumed that the Sun was a centre of force which governed the motions of the planets. If Kepler had tried to relate Mars to the Earth rather than to the Sun, he would never have found the elliptic orbit. He also began with the theoretical assumption that the motion of heavenly bodies was either circular or composed of a small number of circular motions. This assumption (which Kepler later rejected) had been made by astronomers since the time of Plato and Aristotle.

With these background assumptions, Kepler formulated his first hypothesis:

The orbit of Mars is a circle round a centre C somewhat displaced from the sun S, and its motion is uniform with respect to a point U.

On this hypothesis (illustrated in figure 2.1) the planet moves faster when nearer the Sun, in conformity with the idea that the Sun is a centre of force influencing its motion.

Kepler made 900 folio pages in small handwriting of draft calculations relating to this hypothesis. He based it on four observed positions of Mars in opposition, and it agreed within two minutes of arc with another ten oppositions. But he then went on to test the hypothesis against some further observations of Tycho's, and this produced a deviation of 8°. This led Kepler to reject his first hypothesis.

\begin{figure}[h]
\centering
\includegraphics[width=0.5\textwidth]{keplers-hypothesis.png}
\caption{Kepler's first hypothesis}
\end{figure}
Fleming's Discovery of Penicillin: Creative Induction

The research work which led to the discovery of penicillin began when Fleming had been invited to contribute a section on the staphylococcus group of bacteria for the Encyclopaedia Britannica. While examining cultures of this organism, he noticed that a zone of inhibition was present around a particular strain. Further investigation revealed that this zone was produced by a substance that was later identified as penicillin. Fleming's discovery was not the result of a systematic search but rather an observation made while observing the behavior of bacteria in culture dishes.

2.6 Fleming's Discovery of Penicillin: Creative Induction

1. The assumption about the circular motion of heavenly bodies could not be derived from the observations made by Tycho Brahe. Kepler was able to use only the observations made by Tycho Brahe, and he did not use any observations made by Copernicus. Instead, he used his own observations to derive new laws of planetary motion.

2. The data which Kepler used to test his hypotheses were collected by Tycho Brahe before the hypotheses had been formulated in this sense, observation did precede theory.

Let us say more about these two points in turn. In connection with the first, we should make some mention of the views of Sir Karl Popper, the leading exponent of the philosophy of science. As a general rule, Popper's theory applies to revolutionary periods, calling into question the overthrow of some high-level scientific assumptions. This, in turn, corrects the overthrow of some high-level scientific assumptions. It is necessary to recognize that high-level assumptions cannot be directly refuted by observations, and that observations can only directly refute specific hypotheses based on those assumptions. Thus, a creative scientist is never forced by Observations to give up a high-level theoretical assumption. Kepler never had to give up Descartes' theoretical assumption of a circular motion of the heavenly bodies. The nature of the overthrow of high-level scientific assumptions is a matter of detailed discussion, and it need not concern us at the moment.

The data which Kepler used to test his hypotheses were collected by Tycho Brahe before the hypotheses had been formulated in this sense, observation did precede theory.
Bacteriology which was being produced by the Medical Research Council. Fleming's contribution did indeed appear in the second volume in 1929. Staphylococci are spherical bacteria which are responsible for a variety of infections. In particular, the golden-coloured Staphylococcus aureus is the common cause of boils, carbuncles, and other skin infections. While reading the literature on staphylococci, Fleming came across an article by Bigger, Boland, and O'Meara of Trinity College, Dublin, in which it was suggested that colour changes took place if cultures of staphylococci were kept at room temperature for several days. This interested Fleming, because the colour of a staphylococcus can be an indicator of its virulence in causing disease. He therefore decided to carry out an experimental investigation of the matter with the help of D. M. Pryce, a research scholar.

The staphylococci were cultured in glass dishes, usually 85 mm in diameter, known as Petri dishes. These dishes were filled with a thin layer of a gelatinous substance called agar to which enough nutrients could be added to allow the microbes to multiply. Using a platinum wire, some staphylococci were spread across the surface of the agar, and the plate was then incubated at a suitable temperature (usually 37°C), to allow the microbes to multiply. After this period of incubation, the dish was set aside on the bench, and was examined every few days to see if changes in the colour of some of the staphylococci could be observed.

While this fairly routine investigation was continuing, Pryce left the laboratory in February 1928 to start another job, but Fleming continued the work on his own throughout the summer. At the end of July Fleming went off for his usual summer holiday, leaving a number of culture-plates piled at the end of the bench where they would be out of the sunlight. Early in September (probably on 3 September) when Fleming had returned from his holiday, Pryce dropped in to see him. Pryce found Fleming sorting out the pile of plates on his bench. Discarded plates were put in a shallow tray containing the antiseptic lysiol. This would kill the bacteria, and make the Petri dishes safe for the technicians to wash and prepare for use again. Fleming's tray was piled so high with dishes that some of them were protruding above the level of the lysiol. Fleming started complaining about the amount of work he had had to do since Pryce had left him. He then selected a few of the dishes to show to Pryce. More or less by chance he picked up one in the tray of discard but above the level of the lysiol. According to Pryce's later recollection, Fleming looked at the plate for a while, and then said: 'That's funny.' The plate was in fact the famous penicillin plate.

This is how Fleming himself described what happened in the paper he published in June 1929:

While working with staphylococcus variants a number of culture-plates were set aside on the laboratory bench and examined from time to time. In the examinations these plates were necessarily exposed to the air and they became contaminated with various micro-organisms. It was noticed that around a large colony of a contaminating mould the staphylococcus colonies became transparent and were obviously undergoing lysis. (p. 226)

Fleming's photograph of the original penicillin plate is reproduced as Plate 1, and it is easy to follow his description when examining the photograph. The colonies of staphylococci are the small circular blobs, and the contaminating mould is very obvious. Near the mould the staphylococci become transparent or disappear altogether. They are obviously, as Fleming says, undergoing lysis, which means the dissolution of cells or bacteria. From his observation of the plate, Fleming inferred that the mould was producing a substance capable of dissolving bacteria. The mould was identified as being a Penicillium. At first it was incorrectly classified as Penicillium rubrum, but later it was found to be the much rarer species Penicillium notatum. Fleming accordingly gave the name penicillin to the bacteriolytic substance which he thought was being produced by the mould.

The events described so far may make it look as if Fleming's discovery was simply a matter of luck. Indeed, there is no doubt that a lot of luck was involved. Hare subsequently tried to reproduce a plate similar to Fleming's original one, and found to his surprise that it was quite difficult (see Hare, 1970, pp. 54–87). The general effect of Fleming's plate could be produced only if the mould and the staphylococci were allowed to develop at rather a low temperature. Even room temperature in the summer would usually be too high, but here the vagaries of the English weather played their part. By examining the weather records at Kew, Hare discovered that for nine days after 28 July 1928 (just when Fleming had gone on holiday!), there was a spell of exceptionally cold weather. A final point is that the strain of penicillium which contaminated Fleming's plate is a very rare variety, and most penicillia do not produce penicillin in sufficient quantity to give rise to the
effect which Fleming observed. How did such a rare mould find its way into Fleming’s laboratory? The most likely explanation is a curious one. There was at that time a theory that asthma was caused by moulds growing in the basements of the houses in which the asthmatics lived. This theory was being investigated by the scientist (C. J. La Touche) in the laboratory immediately below Fleming’s, and La Touche had as a result a large collection of moulds taken from the houses of asthma sufferers. It seems probable that *penicillium notatum* was one of these moulds.

There is no doubt then that a great deal of luck was involved in the discovery of penicillin. Yet it still needed creativity and insight on Fleming’s part to seize the opportunity which chance had presented to him. Nothing shows this more clearly than a comparison of Fleming’s reaction to the contaminated plate with that of his colleagues in the laboratory (including the head of the laboratory, Sir Almroth Wright) when he showed it to them. With characteristic candour, Hare describes the complete lack of interest shown by himself and the others:

> The rest of us, being engaged in researches that seemed far more important than a contaminated culture plate, merely glanced at it, thought that it was no more than another wonder of nature that Fleming seemed to be forever unearthing, and promptly forgot all about it.

The plate was also shown to Wright when he arrived in the afternoon. What he said, I do not recollect, but... one can assume that he was no more enthusiastic – he could not have been less – than the rest of us had been that morning. (1970, p. 55)

Fleming was by no means discouraged by his colleagues’ cool reaction. He took a minute sample of the contaminating mould, and started cultivating it in a tube of liquid medium. At some later stage he photographed the plate, and made it permanent by exposing it to formalin vapour, which killed and fixed both the bacteria and the mould. Fleming kept the plate carefully, and it is now preserved in the British Museum. So we have here a case of fortune favouring the prepared mind. But what exactly had prepared Fleming’s mind to realize something which his colleagues missed? We can answer this question by giving a brief account of the researches which had occupied Fleming in the fourteen years preceding his discovery of penicillin.

When the First World War broke out, Fleming was already working in the inoculation department headed by Sir Almroth Wright. Wright, Fleming, and the others were sent to Boulogne to deal with the war wounded, and, in particular, to try to discover the best way of treating infected wounds. At that time wounds were routinely filled with powerful antiseptics which were known to kill bacteria outside the body. Fleming, however, soon made the remarkable discovery that bacteria seemed to flourish in wounds treated with antiseptics even more than they did in untreated wounds. The explanation of this apparent paradox was quite simple. In an untreated wound the bacteria causing the infection were attacked by the body’s natural defences, the white cells, or *phagocytes,*
which ingested the invading bacteria. If the wound was treated with an antiseptic, some bacteria were indeed killed, but the protective phagocytes were also killed, so that the net effect was to make the situation worse than before. Wright and his group therefore maintained (quite correctly) that wounds should not be treated with antiseptics. They advocated the earliest possible surgical removal of all dead tissue, dirt, foreign bodies, and so forth, and then irrigating the wound with strong, sterile salt solution. The medical establishment of the day rejected this recommendation, and so the superior treatment was accorded only to those directly in the care of Wright and his team.

After the war, Fleming returned to the inoculation department in London, and here in 1921 he discovered an interesting substance which was given the name lysozyme. Lysozyme was capable of destroying a considerable range of bacteria, and was found to occur in a variety of tissues and natural secretions. Fleming first came across lysozyme while studying a plate-culture of some mucus which he took from his nose when he had a cold. He later discovered that lysozyme is to be found in tears, saliva, and sputum, as well as in mucus secretions. He extended his search quite widely in the animal and vegetable kingdoms, and found lysozyme in fish eggs, birds' eggs, flowers, plants, vegetables, and the tears of more than fifty species of animals. Lysozyme destroyed about 75 per cent of the 104 strains of airborne bacteria and some other bacteria as well. Moreover, Fleming was able to show that, unlike chemical antiseptics, even the strongest preparations of lysozyme had no adverse effects on living phagocytes, which continued their work of ingesting bacteria just as before. From all this, it seemed that lysozyme was part of many organisms' natural defence mechanisms against bacterial infection. Lysozyme had only one drawback. It did not destroy any of the bacteria responsible for the most serious infections and diseases. The hypothesis naturally suggested itself that the pathogenic bacteria were pathogenic partly because of their resistance to lysozyme.

If we put together Fleming's research on war wounds and his research on lysozyme, a problem situation emerges which I will call the 'antiseptic problem situation'. On the one hand, the chemical antiseptics killed pathogenic bacteria outside the body, but were less effective for infected wounds, partly because they destroyed the phagocytes as well. On the other hand, the naturally occurring antiseptic lysozyme did not kill the phagocytes, but also failed to destroy the most important pathogenic bacteria. The problem, then, was to discover the 'perfect antiseptic' which would kill the pathogenic bacteria without affecting the phagocytes. The work on lysozyme suggested that such an antiseptic might be produced by some naturally occurring organisms.

It is commonly remarked that creativity consists in establishing a hitherto unsuspected connection between two apparently different areas, or problem situations. This was precisely what Fleming did when he realized the significance of the penicillin plate. Instead of dismissing the contaminated plate as a failure in his current investigation of the colonies of staphylococci, he saw it as perhaps providing the solution to the antiseptic problem situation which had arisen from his earlier researches. In effect, he must have conjectured that the mould might be producing the 'perfect antiseptic' capable of destroying pathogenic bacteria without disturbing the phagocytes.

The assumption that Fleming made such a conjecture is borne out by his subsequent actions. Fleming grew the mould on the surface of a meat broth, and then filtered off the mould to produce what he called 'mould juice'. He then tested the effect of this mould juice on a number of pathogenic bacteria. The results were encouraging. The virulent streptococcus, staphylococcus, pneumococcus, gonococcus, meningococcus, and diphtheria bacillus were all powerfully inhibited. In fact, mould juice was a more powerful germicide than carbolic acid. At the same time, mould juice had no ill effects on phagocytes. Here at last seemed to be the 'perfect antiseptic'.

At this point in the story, however, a series of difficulties began to emerge. Some further experimental work suggested, misleadingly, that penicillin might not be effective in the body. At the same time, it proved difficult to isolate and purify the compound, and there were difficulties about storing penicillin in such a way that it would not rapidly lose its power to destroy bacteria. In his 1929 paper, Fleming wrote: 'It is suggested that it [penicillin] may be an efficient antiseptic for application to, or injection into, areas infected with penicillin-sensitive microbes' (p. 236). Yet the problems just mentioned caused him to become despondent about the possible uses of penicillin as an antiseptic; and not long after his paper was completed, he abandoned research in that direction.

At this juncture, fortune once again favoured penicillin. Although Fleming abandoned his earlier hopes that penicillin might be the 'perfect antiseptic', he found another practical use for it. This resulted in a continued cultivation of the penicillin mould and production of
mould juice. Thus both mould and mould juice were readily available when Florey and his team at Oxford decided, a decade later, to make another attempt to develop penicillin as an antiseptic.

The main source of income of the inoculation department where Fleming worked was the production and sale of vaccines. There was indeed an efficient unit for producing vaccine (a vaccine laboratory, as it was then called) within the walls of the department, and Fleming had been in charge of the production of vaccines since 1920. In particular, a vaccine was made against Pfeiffer’s bacillus (Bacillus influenzae) which was believed to cause influenza and other respiratory infections. It was difficult to isolate this bacillus because cultures were apt to be swamped by other micro-organisms. Fleming, however, had discovered that penicillin, despite its effect on so many virulent bacteria, left Pfeiffer’s bacillus unaffected. By incorporating penicillin into the medium on which he was growing Pfeiffer’s bacillus, he could eliminate the other germs, and produce good samples of the bacillus itself. Fleming in fact used this method for preparing the influenza vaccine in his vaccine laboratory, and penicillin was made in the vaccine laboratory for this purpose every week after its discovery. Significantly, the title of Fleming’s 1929 paper on penicillin was: ‘On the antibacterial action of cultures of a penicillium with special reference to their use in the isolation of B. influenzae’. Fleming also sent samples of the mould to other centres concerned with the isolation of B. influenzae, and in this way cultures of the mould were established at the Lister Institute, Sheffield University Medical School, and at George Dreyer’s School of Pathology in Oxford. Thus, when Florey and his team decided to take up again the question of whether penicillin might be an efficient antiseptic, they were able to find samples of Fleming’s strain of Penicillium notatum just down the corridor in the Dreyer School of Pathology where they were working. Fleming’s work, like Kepler’s, illustrates the close connection which so often exists between scientific discovery and practical applications.

Let us next try to examine the relevance of this important scientific episode to the question of inductivism versus falsificationism. As a matter of fact, Fleming’s researches appear to involve both induction and conjectures and refutations. On examining the contaminated plate, Fleming arrived at two hypotheses. The weaker was that the mould produced a bacteriolytic substance, while the stronger was that this substance might be the ‘perfect antiseptic’ which his earlier researches had led him to desire. Now here, observation definitely preceded the formation of the hypotheses, and the hypotheses could certainly not have been formulated without the observation. It thus seems correct to say that these hypotheses were obtained by induction from a chance observation. The hypotheses were, however, conjectures, and Fleming proceeded at once to test them by preparing his mould juice and investigating its effect on various kinds of pathogenic bacteria and on phagocytes. As we have seen, the early tests corroborated the ‘perfect antiseptic’ hypothesis, though some later results gave rather misleading counter-indications. In short, we seem to have a case of hypotheses generated by induction from observations and then developed by the method of conjectures and refutations. Does this mean that some kind of synthesis of inductivism and falsificationism is possible?

Such a synthesis has been suggested by Mitchell (1989). Mitchell emphasizes what could be called conjectural induction, and describes some features of the process in the following two passages:

It is a surprising characteristic of the conjectural process of induction that a useful theory appears to contain more information than the limited set of singular data from which it was originally induced. The useful theory must contain, not only the pattern of the singular data as presented, but also the pattern of some natural general principles of which the singular data were symptomatic. The task of the imaginative research worker is to guess what the general principles might be. (1989, p. 11)

And again:

Although . . . the inductive process may proceed by small and apparently undramatic steps, these steps take the form of guesses, and the process of theory generation is non-deductive and basically conjectural in the sense described by Popper. (p. 12)

In fact, reflection shows that reconciling Popper’s theory with some form of induction is not difficult. Popper gives no account of how scientific conjectures originate, and he considers this to be a matter for empirical psychology rather than philosophy of science. It is therefore possible to supplement his account by claiming that scientific conjectures sometimes originate by induction from observation. This would be Mitchell’s conjectural induction. As we have seen, this view fits very well with the details of Fleming’s discovery of penicillin. It is interesting that Mitchell speaks of ‘the task of the imaginative research worker’ (my emphasis); and indeed, I argued earlier that Fleming’s conjectural induction involved considerable
creativity. In cases like this, we could also use the term *creative induction*. We can, moreover, contrast it with another way of generating conjectures which could be called *creative theorizing*. In the latter case the conjecture is formed by meditating on earlier theoretical developments rather than by considering observations. A nice example of creative theorizing is provided by Copernicus. Copernicus was not much of an observational astronomer. His *De Revolutionibus Orbium Caelestium* of 1543 contains only twenty-seven observations made by himself, and neither these nor any other new observations seem to have had any influence in the genesis of his new hypothesis. Copernicus arrived at his heliocentric theory in something like the following way. He became dissatisfied with the Ptolemaic models used in the astronomy of his time. He therefore read many ancient Greek texts to see if an alternative approach could be found. In this way he lighted on the Pythagorean view that the Earth moved, and, starting with this hint, developed his own hypothesis. Here, then, we certainly have creative theorizing rather than any form of induction. Perhaps, however, Copernicus and Fleming occupy the two extreme points of a scale, and most hypothesis formation in science falls somewhere in between, being generated by reflection both on earlier theory and on new observations. Kepler in fact affords an example of such a mixture of creative theorizing and creative induction.

So far, then, all our examples have involved some human creativity; but the question naturally arises as to whether some form of *mechanical* or *Baconian induction* might not be possible as well. To pursue this question, we will examine in the next section another example — that of the discovery of the sulphonamide drugs.

### 2.7 The Discovery of the Sulphonamide Drugs: Mechanical or Baconian Induction

The sulphonamide drugs were discovered in Germany as a byproduct of the activities of the giant chemical company I. G. Farben. The discovery was made by a team headed by Gerhard Domagk, who was born in 1895 and appointed at the early age of thirty-two as director of research in experimental pathology and bacteriology in the institute attached to the I. G. Farben works at Elberfeld. Domagk and his team had huge laboratories in which they routinely tested compounds produced by the firm’s industrial chemists on thousands of infected animals to see if the compounds had any therapeutic value.

The I. G. Farben chemists Hoerlien, Dressel, and Koth produced a rich red dye which was very effective with protein materials such as wool and silk. This was known as *Prontosil rubrum*. Domagk and his team then discovered that this same compound possessed the definite ability to cure mice infected with haemolytic streptococci. Domagk published this finding in 1935, but referred back to experiments carried out in 1932.

This sequence of events can be described using the schema of conjectures and refutations. As each new compound was produced by I. G. Farben’s industrial chemists, it was conjectured that it might have the ability to cure one or more bacterial infections. This conjecture was then tested by administering the compound to infected animals and seeing whether any improvement resulted. In the case of nearly all the compounds produced, the conjecture was refuted, but at last a compound appeared for which the corresponding conjecture was confirmed. In this example, then, the conjectures were not produced using any scientific imagination or creativity, but in a routine fashion. The process can therefore fairly be described as *mechanical*. But was it mechanical *induction*? The answer seems to be ‘No’. At no point was there any inference of a conjecture from observations. The conjectures were generated in a routine fashion, and then tested experimentally. The observations came after the conjecture, as in Popper’s model. I will, therefore, call this procedure *mechanical falsificationism* rather than *mechanical induction*. It should, however, be added that mechanical falsificationism bears some resemblance to the type of induction advocated by Bacon.

The discovery of the sulphonamide drugs does indeed have many Baconian features. To begin with, Bacon stresses the desirability of team work in the sciences. As he says:

> It is not a way over which only one man can pass at a time (as is the case with that of reasoning), but one in which the labours and industries of men (especially as regards the collecting of experience), may with the best effect be first distributed and then combined. For then only will men begin to know their strength when instead of great numbers doing all the same things, one shall take charge of one thing and another of another. (1620, p. 293)

Far from emphasizing imagination, creativity, or genius, Bacon seems to want to make science into a routine activity which could
be carried out by anyone of average intelligence. As he says: 'But the course I propose for the discovery of sciences is such as leaves but little to the acuteness and strength of wits, but places all wits and understandings nearly on a level' (p. 270). Thus the army of scientists in Domagk's laboratories performing routine tests on routinely generated hypotheses seems to conform well to Bacon's ideas. Moreover, all this activity did indeed result in what Bacon called a 'secret of excellent use' (p. 292).

Bacon stressed that he wanted to introduce a new form of induction which was not induction by simple enumeration, but involved exclusion and rejection. This is how he puts it:

'But the greatest change I introduce is in the form itself of induction and the judgment made thereby. For the induction of which the logicians speak, which proceeds by simple enumeration, is a puerile thing; concludes at hazard; is always liable to be upset by a contradictory instance; takes into account only what is known and ordinary; and leads to no result.

Now what the sciences stand in need of is a form of induction which shall analyse experience and take it to pieces, and by a due process of exclusion and rejection lead to an inevitable conclusion. (p. 249)

By 'induction by simple enumeration' Bacon means something like the induction from the observation of several thousand white swans to the conclusion that the next observed swan will be white. Bacon regards this as 'a puerile thing'. Now what happened in the case of Prontosil rubrum was the 'exclusion and rejection' of a very large number of compounds until eventually one of therapeutic value was discovered. Because of all these parallels, I will use the term Baconian induction as a synonym for mechanical falsificationism. Admittedly this terminology could be a little misleading, since, as already pointed out, Baconian induction is not really induction at all. Bacon, moreover, fails to take account of a rather crucial difficulty which we must next consider.

Bacon seems to have thought that in any particular instance there will be only a few possible hypotheses available. Thus, by quite a short process of 'exclusion and rejection', we will be led to the truth as 'an inevitable conclusion'. In fact, however, it is often possible to generate with ease an enormous number of hypotheses, and there may not be sufficient time or resources to test all of them in the hope of hitting on one which works. Thus it would scarcely be possible to test for therapeutic properties every single compound which the chemists of today are capable of synthesizing. In this situation there has to be recourse to what are called heuristics.

A heuristic (from the Greek heuriskin, 'to discover') is a guide to discovery. In the context of mechanical falsificationism, hypotheses are generated by some routine or mechanical procedure; but in practice this procedure is unlikely to be totally random. It will usually be devised in accordance with some heuristic. Even the search procedure which led to the discovery of Prontosil rubrum, the first of the sulphonamide drugs, was guided by various heuristics. One of these was the idea that dyes capable of staining textiles might also have useful therapeutic properties. The 'dye heuristic', as it might be called, had been introduced before Domagk by Paul Ehrlich. Ehrlich discovered that if certain dyes are injected into living organisms, they are taken up and stain only some particular tissues and not others. Ehrlich gives the following example, which played an important role in his discovery of the method of 'vital staining':

'... Thus, for example, methylene blue causes a really wonderful staining of the peripheral nervous system.

If a small quantity of methylene blue is injected into a frog, and a small piece of the tongue is excised and examined, one sees the finest twigs of the nerves beautifully stained, a magnificent dark blue, against a colourless background. (1906, p. 235)

Ehrlich goes on to observe that this specific staining property is lost if the chemical composition of the dye is changed even to a small extent. Thus he says:

I was able to prove that the nerve-staining property of methylene blue is conditioned by the presence of sulphur in the methylene-blue molecule. Synthetic chemistry has, in fact, given us a dye which, apart from the absence of sulphur, corresponds exactly in its chemical constitution to methylene blue. This is BINDSCHDLEIER'S green. With the absence of the sulphur, there is associated the inability to stain living nerves. (Ibid.)

In the light of these interesting discoveries, Ehrlich reasoned somewhat as follows. Suppose we know that a particular disease is caused by the invasion of some micro-organisms. To cure the disease, we need to find a chemical which is highly toxic to these micro-organisms, but which does not harm the patient. This can be achieved if we can find a chemical which kills the micro-organisms and which is taken up only by the micro-organisms and not by the
other tissues. Now dyes like methylene blue are highly specific, in that they are taken up by some tissues and not by others. Many dyes are also toxic. So it is not unreasonable to think that some dyes might have good therapeutic properties. Indeed, Ehrlich was able to show that his favourite, methylene blue, was helpful in curing malaria. As he says: 'In my further experiments . . . I started from the supposition that dyes with a maximal tintorial activity might also have a special affinity for parasites within the host-organism . . . . I chose the malaria parasites and was able, in association with Professor GUTTMANN, to show that methylene blue can cure malaria' (p. 241).

So the 'dye heuristic' proved successful first for Ehrlich, then for Domagk. Ironically, however, it turned out that the therapeutic properties of Prontosil rubrum have nothing to do with its ability to dye fabrics.

The molecular structure of Prontosil rubrum is shown in figure 2.2, where the hexagons are the benzene rings whose discovery by Kekulé we described earlier, and, as usual, N denotes one atom of nitrogen, S of sulphur, O of oxygen, and H of hydrogen. It is clear that the molecule consists of two halves joined by the double bond denoted by =. In the body, four hydrogen atoms are added to the molecule through the action of enzymes, and the molecule splits into two different molecules, sulphanilamide and tri-amino-benzene (see figure 2.3).

![Figure 2.2 The molecular structure of Prontosil rubrum](image1)

![Sulphanilamide and Tri-amino-benzene](image2)

Figure 2.2 The molecular structure of Prontosil rubrum

Figure 2.3 Reduction of Prontosil rubrum in the body