Lecture 2. Testing of theories

Essence of scientific method

One puts up a hypothesis, a guess, a leap into the unknown, and from this one deduces consequences and then tests these.

Mill thought that if these tests are to mean anything, they have to establish the hypothesis. But the fundamental procedure is the reverse – the test has to be an attempt to refute the hypothesis. One is, of course, happy if refutation is not done. We can call this view 'falsificationism', i.e. one adopts a hostile attitude to the hypothesis.

What is the procedure of deduction and of test? It is of the form: if A, then B follows.

As an example, take the law of gravity – if you have two bodies in space they give to each other forces which decrease as the square of their distances and increase as their masses. The temporal and spatial universality is the distinguishing character of such a theory, i.e. of A. How can we deduce consequences from this, that is, derive B from A? We must deduce an individual consequence, for we can only observe an individual thing, never anything universal. From a universal law alone, we can deduce nothing positively about an individual case. We must have also initial conditions, e.g. the position of planet and sun, etc., and then deduce movement. This deduction is called prognosis.

For example, the famous Aristotelian deduction – 'All men are mortal, Socrates is a man, therefore Socrates is mortal'.

In science this test by using the prognosis is only of value if we attempt in a most rigid way to falsify the prognosis. But we can never in this way verify a theory. For example, all men may not be mortal even though Socrates is mortal, and is a man. But if the theory is that all men are immortal, then the first dead man refutes the theory. This is, we can falsify a theory, but never prove it.

There is an analogy with a man finding his way through a wood in the dark. He must venture – do something. His method is trial and error, with emphasis on error. If you don't make mistakes, then you learn nothing.

The method of science is like a Darwinian method of selection. We produce theories and then eliminate them. The remainder are those not yet refuted – but not therefore true. We have many competing theories. We must dare to produce many – not just successful theories – and ruthlessly weed them out.

Two things are necessary.

1. Ideas – imagination in producing theories – bold speculation. In itself this has little to do with scientific method.
2. The real essence of science is the ruthless persecution of scientific theories, hounding them till we kill them. But, of course, it is a greater success if we have a theory that has stood up to a rigorous series of tests. The first scientific effort was the killing of a theory or a superstition.

Why is this hostile attitude a necessary complement to these bold leaps of the imagination in producing theories? It is practically always possible to save a theory from the fate of falsification if we want to, i.e. if it is a pet theory. We can, for example, make an *ad hoc* hypothesis which explains away the observation, or can say that we have made a false experiment – apparatus leaked, etc. If we adopt this attitude, then all testing becomes useless, i.e. if we do not accept falsification, then all testing is a farce; hence arises the necessity of attempting to force the falsification.

However, there is a partial withdrawal from this rigorous attitude. You don't need to throw the theory away. There may be something in it, some element perhaps of value. Theories usually are complex, and even though falsified, they may have some component of value. For example, the experiment of diffraction of light led to the refutation of the theory that light was just a stream of corpuscles, but again this theory reappears to some extent as the photon theory. This is an indication that the prejudice which the father of a theory has for the theory has a function – a theory is rarely so simple that it can be rejected wholly in one piece.

However, the people who produce a theory generally take the attitude that they wish to verify it. That is an easy attitude. The testing is left to others. Hence, the development of science is a social affair; as at least two people are necessary – one making theories, the other falsifying them.

A certain school has been questioning the objectivity of sciences. They say that, wherever the interests of the scientist are involved, they won't be objective. Social sciences, where class interests are involved, will not be objective. This is not so, they say, with physics. But this may be criticised, as no tie could be as strong as that which the father of a theory has for his offspring.

Scientific objectivity is fortunately something that does not depend on the objectivity of a scientist, but rather on the character of scientific method, that is, on the public nature both of the publication of a theory and of its attempted falsification.

An important point is the question of the *ad hoc* hypothesis which is introduced to avoid falsification of a theory. For example, accordingly to Newtonian theory, Mercury should behave differently in its orbit. But one could produce an *ad hoc* theory that the sun emits some resisting matter that produces the discrepancy, i.e. just make a special assumption. However, if one makes an addition to the *ad hoc* hypothesis that can be tested by other means, it becomes a hypothesis: for example, that light transmitted through from stars would be dimmed. If this is not found, one could produce a further *ad hoc* hypothesis saying why light is not dimmed, but this is very bad indeed. The better conclusion is that the first *ad hoc* hypothesis is falsified.

An *ad hoc* hypothesis has no other action but to explain the series of facts it was invented for. A proper hypothesis has other consequences which can be tested in order to attempt to falsify it. This is a new and different attack on the method of induction, for induction would only lead to *ad hoc* hypotheses, and these lead nowhere, i.e. process of induction is of no interest scientifically.

If we want to get anything to test we have to have a hypothesis that has a wider field than would be the case with an *ad hoc* hypothesis; and to be able to test is essential, or else we never move beyond the *ad hoc* hypothesis. The *ad hoc* hypothesis is always individual.

In order then that the testing of theories can occur, we have to have a theory that has some new implications, i.e. the scientist has to take risks, to say more than he knows, not as an assertion, but as a hypothesis. This is a direct contradiction of the inductive, rationalistic attitude, which only believes the evidence of the senses and what can be proved from this. This is contrary to the adventurous spirit of true science. It leads to the saying of nothing because it won't take any risks.

The true outlook of the scientist is to take risks and make the widest possible theory, then test it where there is believed to be its weakest point, where there is the greatest likelihood that it will break down.

The inductivist theory

The inductivist theory as described by Bacon (who is a bit overestimated as a thinker) is a kind of commonsense view of science. It went so far as to say that one should never make any unwarranted statements. But the true attitude is to make unwarranted statements. If no risks are taken, one remains silent, for that gives the only chance of making no mistakes.

Bacon described the scientific method as resembling the collection of grapes, then treading on them and squeezing out the juice, which is the essence, or what he called a scientific generalisation.

The whole attitude of inductivism is that nature does not lie, only you lie, hence you do nothing – just observe in your bucket-like mind and be careful you add nothing, for all our mistakes come from our misinterpretations.

The real method of science is the reverse – it is to risk hypotheses, which are not lies, as their hypothetical nature is recognised. The real hypothesis covers a wider field than it was originally invented for, e.g. Newton's laws not only explained Kepler's laws of planetary motion, but also covered falling apples. Further, Einstein's theory covered all this and unified the theory of gravity and inertia, but also covered further fields:

- deviation of light rays in gravitational field,
- spectral analysis of elements in the very strong gravitational field of a heavy star – a red shift,
- other spectral effects – Doppler effects,
- also certain deviations from other theories, e.g. Bohr's theory of the hydrogen atom by accounting for high velocity effects,
- the high velocity effects on the electron's properties,
- Mercury orbital anomaly also.

So, Einstein's theory covered a much wider field, hence there was much more opportunity for it to be falsified. The success of the theory in these very divergent fields is very impressive. The less a theory has the character of an *ad hoc* hypothesis, the better it is, for one can make more attempts to falsify it.

How can this be applied to solve the controversy [over cosmological methodology] in *Nature*,[1] between Milne & Eddington and Dingle? It began with an attack by Dingle in the name of science (empirical science) – Dingle is an empiricist. He says that they are introducing speculative and philosophical methods into science. Eddington says, in fact, that he will squeeze nature into a system of pigeon-holes, i.e. because science is deductive, it cannot be falsified.

This view is not new. It was invented by Poincaré, who says in application to Euclidean geometry that we use it because it is simplest. No tests can ever refute it, as it always can interpret

[1]Editor's note: The debate on cosmological methodology was still topical at the time of Popper's lecture, after a special issue of Nature on the subject had been published in 1937. The Stanford Encyclopedia of Philosophy on 'Cosmology: methodological debates in the 1930s and 1940s' (http://plato.stanford.edu/entries/cosmology-30s/) suggests that Milne was the successful protagonist and his hypothetico-deductive methology was subsequently developed by Bondi, with inspiration from Popper, into the 'perfect cosmological principle' – a steady-state universe – which was falsifiable and 20 years later abandoned by Bondi when evidence of an expanding universe turned up.

facts in terms of the system and, of course, the system is always true. Poincaré extended this to physics, e.g. to the principle of the conservation of energy.

Deductivists are right in saying that science is our making, but wrong in saying that we can't throw it away if we find it contrary to nature. The difference between a conventionalist like Eddington and an empiricist like Dingle is that the conventionalist is not ready to be falsified and the empiricist is ready to be falsified. The attitude is the difference between one who looks to science as having the last word, and the scientist who is ready to be falsified.