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The search for truth in science and theology

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Abstract

How do we come to believe things? Does it happen quickly or slowly? Does it depend on what we already know? You may have heard of the old farmer who was shown a picture of a giraffe and exclaimed: There is no such animal! He was taken to the zoo and shown one, and made a rapid transition from scepticism to belief. The ways to more subtle beliefs are more complicated. Science textbooks often present them as logical processes proceeding from evident premises to the final conclusion, usually with the help of some observations or the results of experiments. It is much the same in textbooks of theology. This may be justified from a pedagogical point of view, but I want to argue that the reality is usually quite different, and can only put in a logical form at a late stage. As remarked in a well-known textbook: Confusion usually reigns when important physical advances are being made; it is only afterwards that a clear-cut logical path can be laid leading straight to the goal [15]. This can be illustrated by two examples from the history of physics, namely the belief in the reality of atoms and that the earth rotates around the sun.

1. The reality of atoms

The ancient Greeks realised that matter is either discrete or continuous, but had no means of distinguishing between these possibilities. Democritus suggested that everything is made of atoms moving in the void. This hypothesis enabled him to account for change, but he could give no supporting reasons. An argument for the existence of atoms is to be found in the structure of crystals with their characteristic shapes and angles between their faces. This is difficult to explain if matter is continuous like a jelly, but is easily explained if they are formed of atoms in a regular array. Further support was obtained early in the nineteenth century, when several chemists such as Dalton found that substances always combine in definite proportions. The case for atoms was greatly strengthened by the results of many different researches that gave essentially the same value for Avogadros number, the number of atoms in a gram-molecule of a substance. Around the turn of the century the issue was still undecided. The more pragmatic Anglo-Saxons were practically sure that atoms exist, whereas on the continent the school of energetic led by the chemist Ostwald still disputed the evidence. Eventually the work of Perrin on the Brownian motion provided definite evidence for the existence of atoms. The philosopher Ernst Mach resisted to the end, though even he was shaken when he was shown the scintillation flashes made by alpha particles when they hit a zinc sulphide screen. It is not surprising that he ended his life as a Buddhist. The important point to notice is that the acceptance of the reality of atoms was the result of a long process of accumulation of individual observations all pointing in the same direction. None of them were conclusive on their own, but together they were sufficient to give certainty. It is now possible to give a definite proof of the reality of atoms: they can be seen using an atomic force microscope.

2. The heliocentric theory

Today anyone will confidently tell you that the earth rotates around the sun, although almost no one will be able to explain how we know this. A millennium or two millennia ago, we would have been told with equal confidence that the earth is stationary at the centre of the universe, and the sun rotates around it once every day. Why have peoples views shifted so decisively?

Aristotle believed that the earth is stationary and the sun moves around it. This is obviously true: we see it happen every day, until we realise that we would see the same if it was the earth that was rotating. He also believed that the celestial realm is perfect, incorruptible and unchangeable. Since the circle is the most perfect curve, the orbits of the stars and planets must be circular. The motions of the planets posed a problem as they are obviously not circular, but Ptolemy found that they could be quite well represented by a complicated system of cycles and epicycles. Aristarchus of Samos suggested that the sun is in the centre of the solar system with the earth going around it. There were serious objections to this brilliant insight and it was not further considered until it was revived by Copernicus almost two thousand years later.

Copernicus showed that if the sun is in the centre and the planets rotate around it, then the radii of the orbits and the periods of rotation increase in an orderly sequence from the innermost planet Mercury to the furthest planet Saturn. In addition, this provides a natural explanation of the retrograde motions of the planets and why they are brightest at that time. Also explained is the fact that the inner planets Mercury and Venus are never very far from the sun. These somewhat technical advantages, appreciated by astronomers, were offset by two very strong counterarguments, namely that if the earth rotates and moves around the sun it would be subjected to such high winds that everything would be blown off, and by the absence of stellar parallax. On purely scientific grounds the Aristotelian-Ptolomaic theory was still to be preferred.

Soon after, the telescopic observations of Galileo provided serious arguments against the Aristotelian-Ptolomaic theory. He observed the satellites revolving around Jupiter; this is contrary to the Aristotelian belief that all circular motions of the celestial bodies are centred on the earth. He observed the mountains on the moon and the spots on the sun, contrary to the Aristotelian belief that the heavens are incorruptible. He observed the phases of Venus, showing that Venus rotates around the sun.

Galileo also realised that the absence of stellar parallax could be explained if the stars are much further away than was generally believed. He also developed a new theory of dynamics that shows that things on the earth share its motion and so are not affected by its motion, either daily on its axis or yearly around the sun. This answered the objections to the heliocentric theory.

At this point the Aristotelian-Ptolomaic theory was discredited, but the heliocentric theory was not proved. Nevertheless, Galileo realised that with the heliocentric theory everything fitted together in a convincing way. The convergence of many lines of thought, each inconclusive on its own, was sufficient to give him certainty. He thought that he could find an apodictic proof, but in this he was mistaken.

This is an example of how a scientist, immersed in the details and holding all the arguments together in his mind, can become convinced of the truth, without it being possible for him to convince those who lack this knowledge. This throws some light on the difficulties Galileo experienced later.

Almost two hundred years later, the observation of stellar aberration by Bradley in 1728 showed that the earth is rotating, and Bessel in 1738 succeeded in measuring stellar parallax. This provided the definitive proof of the heliocentric theory, but it caused little stir, as it had already been believed for centuries. These two examples of the way scientists reach the truth are quite typical. This was already realised in the thirteenth century by Roger Bacon who, in his Opus Majus wrote:

> *There are two modes in which we acquire knowledge, argument and experiment. Argument gives no proof, nor does it remove doubt and cause the mind to rest in the conscious possession of truth, unless the truth is discovered by the way of experience. Thus, if any man who had never seen fire were to prove, by satisfactory argument, that fire burns and destroys things, the hearers mind would not rest satisfied, nor would he avoid fire; until, by putting his hand or some combustible thing into it, he proved by actual experiment what the argument laid down. But after the experiment has been made, his mind receives certainty and rests in the possession of truth, which could not be given by argument, but only by experience.*

Several additional examples are given by Collins and Pinch in [3], who discuss the way science is really done in contrast to the way it is described in papers and reviews and is generally assumed to be done. They quote Mermin who says that the existence of many strands of evidence can transform a hypothesis into a fact, even in the absence of a single unimpeachable experiment. Thus conclusions are reached through a gradual meeting of minds over a wide field of debate, rather than through brief exhibitions of technical virtuosity. Referring to the contemporary tests of relativity, they conclude that no test on its own was decisive or clear cut, but taken together they acted as an overwhelming movement. Thus scientists come to their conclusions by assembling many strands of evidence, each of which is itself weak and these weak strands are woven into strong ropes.

Sir Cyril Hinshelwood in 1959 has commented on the reasons why we believe in the existence of atomic nuclei: The atomic nucleus is not directly observed. It is inferred by elaborate reasoning from many complex experiments, any one of which could probably be given an alternative explanation. What carries conviction is the fact that a coherent body of doctrine emerges from a large number of varied tests.

Another example of the relationship between these two ways to truth is provided by the continuing controversy about the origin of the sound in singing sand dunes. Several explanations have been proposed, but the problem remains open. Eventually it will be solved and, as one of the participants, Stephane Douady, remarks: In 10 or 15 years from now – when researchers have solved the mystery and textbook chapters have been written – the physics of singing dunes will doubtless be recast as the product of a sequence of logical steps, all other accounts having gradually been buried like skeletons on the sand [1].

In some cases a theory is firmly believed although an apodictic proof is impossible. An example is the theory of evolution. As Stephen Gould has remarked: Truly grand and powerful theories – evolution pre-eminently among them – do not and cannot rest on single observations. Evolution is an inference from thousands of independent sources, the only conceptual structure that can make sense of all this disparate information. In his autobiography Francis Crick remarks that a single isolated bit of evidence, however striking, is always open to doubt. It is the accumulation of several different lines of evidence that is compelling.

It could be objected that it is logically unacceptable to maintain that we can attain certainty by summing a number of inconclusive arguments. However, consider the track of a charged particle in a nuclear emulsion. Such a particle activates the silver bromide grains and on development the track of the particle is shown by a line of developed grains. There are also many other random grains due to a variety of causes. It could therefore be argued that what we though was the track of a particle is in fact due to many random grains that happen to be on a line. We would of course reject this argument as simply preposterous. Thus, Newman remarks, from the accumulation of various probabilities we may construct legitimate proof, sufficient for certitude [9].

A notable feature of scientific truth is that theory and fact are equally strong and utterly interdependent; one has no meaning without the other. We need theory to organise and interpret facts, even to know what we can and might observe. And we need facts to validate theories and give them substance [5]. It is sometimes said that no one believes a theory unless it agrees with the facts, but it is equally true that no one believes the facts unless it agrees with or is explained by a theory. This may seem surprising, but it is borne out by many examples. Thus the experiments of Kaufmann disagreed with Einsteins theory of relativity, and agreed with a rival theory [4]. Einstein was quite unmoved; he knew that his theory was correct. He was eventually vindicated when a flaw was found in Kaufmanns experiment. What is convincing is a symbiotic resonance between theory and experiment each reinforcing and confirming the other.

Many accounts by scientists show that the process of achieving an understanding of some phenomenon passes through several stages. First there is a period of intensive study, when the phenomenon is observed in many different conditions and detailed measurements are made whenever possible. In this way the scientist acquires an instinctive feel of the phenomenon, what Polanyi has called tacit knowledge, so that in his phrase we know more than we can tell. Eventually all the relevant facts are mastered and held in the mind. Then, sometimes when one is thinking of something else, it all falls into place in a convincing way. Thus for example Ramon y Cajal has recalled: The new truth, laboriously sought and so elusive during two years of vain efforts, rose up suddenly in my mind like a revelation [13]. On another occasion, when his daughter was dying and he was trying to drown his sorrows by working through the night, there suddenly blazed forth in my mind the splendour of a new truth. There is no doubt about it, and it is almost impossible for a scientist to recover his previous views, even if he wanted to. Once one has seen the light, it is no longer possible to see in the dark.

Sometimes a new truth is immediately accepted; sometimes it is not. When Frisch and Meitner suddenly realised why barium is found after uranium is irradiated with neutrons their explanation was immediately accepted by other physicists, whose minds were well prepared. What fools we have been, exclaimed Bohr. But when Semmelweis realised why more women were dying after childbirth in hospitals than in clinics, no one believed him, and he was driven insane.

The final stage is the confirmation of the new truth by putting it all together in a logical way, and checking its consequences by further experiments. It is then written up as a neat logical story and published in a journal. Anyone who has never set foot in a messy laboratory, or struggled to find a mathematical description of some obscure phenomenon, and reads these accounts can well come to believe that scientific research is a simple matter of following a series of neat logical steps that he calls the scientific method.

Once this account of the way to truth in science is understood, it throws a flood of light on a whole range of otherwise obscure phenomena. Why, for example, do we find it so difficult to convince anyone of a truth that we see so clearly? Simply because he does not hold in his mind that totality of separate indications that assure us of its truth. If he is disinclined to believe what we are trying to tell him, he can quite reasonably dismiss our arguments one by one. If we look back to the time when we did not see that truth, we realise that we had to work quite hard to master the separate indications one by one until suddenly they coalesced and we saw the truth. Without a strong incentive to learn the truth, the labour would not even have been attempted. Often the truth we profess is built on layers of subordinate truths, each of which must be apprehended by the same laborious process. It is like climbing a mountain and if one who has reached the summit calls out to one at the foot of the mountain and invites him to admire the view, he cannot do so unless he undertakes the long weary climb himself.

The result of this process is that the scientists have learned to see through the results of their experiments and measurements, aided by his theories, to the underlying reality. When they try to describe this they have to use existing words and often in the process give them a new and more precise meaning. Their accounts may be read and analysed by philosophers who focus their attention on the words and do not see the reality that the scientist is trying to describe. They fail to realise that what is important is not the words themselves but the underlying reality that the words imperfectly express. Without a grasp of that reality all their arguments are worthless.

3. Michael Polanyi

Further insight into the process of scientific discovery is provided by the work of Michael Polanyi. His views have the richness and depth that comes from an intimate knowledge of scientific research from the inside. He worked for many years as a physical chemist, attaining great distinction in that field. In addition, he was widely read in the history of science, and developed his views in the light of his first-hand experience of research and his knowledge of the past. This accounts for much of the attraction of Polanyis writings for the scientist. Many of the writings on the philosophy of science appear to the working scientist as desiccated abstractions in which he has difficulty in seeing the living reality he knows so well. In Polanyi, the scientist recognises one who has himself experienced the struggles and disappointments of scientific research, and is correspondingly more ready to accept his account of what he is doing.

Polanyi knew that scientific research is a complex activity not describable by a series of instructions. It is an activity that engages many of our abilities to the full: judgement, integrity and perseverance and all deployed in the day-to-day activity of scientific research. He also knew very well that scientists are members of a worldwide community, and understood the complex interactions between the members of that community that makes science possible. This brings other abilities into play and raises many of the problems of the relation to the community that are familiar in society as a whole. These include the age-old questions of authority and freedom, as well as problems connected with the right to choose the subject of research and the means to carry it out. The extent and depth of Polanyis analysis is the reason for the widespread interest in his work, not only among scientists but also among philosophers, teachers and educationalists.

Polanyis work on the philosophy of science must be seen in the context of the fundamental debate on the objectivity of scientific knowledge. This is very far from an abstract debate of interest only to academic philosophers; it is of the highest possible relevance today. Contemporary debates are being destroyed by social constructivism which maintains that there is no objective truths and all our views are a result of social conditioning. Polanyi was well aware of such attempts to destroy scientific objectivity from his studies of science in societies dominated by Marxism.

Basically the fundamental issue is whether the scientific enterprise is the progressive discovery of the structure of the real objective world, or whether it is the ordering of our sense-impressions into the most convenient pattern. According to the former view, there exists a world independent of ourselves (though of course we are a part of that world) that continues on its way whether we attend to it or not. Some of us are interested n the innermost workings of that world, and we spend our lives tying to understand it. In this endeavour we are partly successful: we do understand its structure and workings in some respects, though other aspects remain unknown. We are convinced that much of what we learn is real enduring objective knowledge.

The contrary view thinks of us as continually receiving a stream of sense impressions, sights, sounds and pointer readings. If we are to render this intelligible we must organise these sense impressions into a pattern, into a system of relationships. Scientific theories are thus no more than convenient maps that enable us to find our way around the world, and to predict its future behaviour from past and present observations. On this view it is quite conceivable that an entirely different map will prove as good or better than the first map. Indeed this process of discarding one map in favour of another frequently happens in the advance of science, as for example when Einsteins relativity replaced Newtons classical mechanics. This theory of science has many variants and a complex history; broadly speaking it stems from the positivism of the Vienna circle and was most influential during the middle decades of the twentieth century.

This difference between two views of science has been described by Polanyi in his Personal Knowledge [11]:

> *The discovery of objective truth in science consists in the apprehension of a reality which commands our respect and arouses our contemplative imagination; such discovery, while using the experience of our senses as clues, transcends this experience by embracing a vision of reality beyond the impressions of our senses, a vision which speaks for itself in guiding us to an ever-deeper understanding of reality.*

The opposing view he describes as the reduction of a scientific theory to the rank of a convenient contrivance, a device for recording events and computing their future course. Polanyi laments that this is the widely accepted view, and that the conception of the objectivity of science is generally shrugged aside as out-dated Platonism, a piece of mystery-mongering unworthy of an enlightened age.

Polanyis response is to examine in detail the history of science, and from this he concludes that

> *twentieth century physics, and Einsteins discovery of relativity in particular, which are usually regarded as the fruits and illustrations of the positivistic conception of science, demonstrate on the contrary the power of science to make contact with reality in nature by recognising what is rational in nature.*

One of the initial difficulties in studying the implications of Einsteins theory of relativity is that the account given in most textbooks of its historical development is completely false. The history of any scientific discovery is usually exceedingly complicated, and most scientists (and in particular the writers of textbooks) have little interest in it. They want to present the essential scientific results as clearly and as simply as possible, and they tend to rearrange and to simplify the historical development to serve this purpose.

According to the textbooks, Einstein developed his theory of relativity in order to explain the unexpected result of the Michelson-Morley experiment, which showed that the velocity of light is the same in all directions. This is not what would be expected from the idea that light consists of oscillations in an all-pervading aether because since the earth is moving around the sun, it must be moving relative to the aether. The experiment was designed to measure its motion, and gave a null result. Einstein, following Mach, then realised that scientific theories must contain no reference to quantities that are not observable: reference must be made only to observables. Now the aether is unobservable, and so therefore is motion relative to it, and so we must exclude them from our theory and concentrate instead on relating the results of measurements of position and time. In this way he obtained his theory of relative motion, of relativity.

This account is misleading in several respects. As he relates in his autobiography, Einstein recalls that he discovered relativity

> *after ten years reflection on a paradox which I had already hit upon at the age of sixteen: if I pursue a beam of light with a velocity c I should observe it as a spatially oscillatory electromagnetic field at rest. However, there seems to be no such thing, whether on the basis of experience or according to Maxwells equations. From the very beginning it appeared to me intuitively clear that, judged from the standpoint of such an observer, everything would have to happen according to the same laws as an observer who, relative to the earth, was at rest.*

He went on to ask himself what transformation must be used to connect measurable quantities in one frame of reference to those of the same quantities in another frame moving with respect to the first in such a way that the equations remain unchanged. The required transformation had indeed already been found by Lorentz. This explained not only the result of the Michelson-Morley experiment, but also led to many other results that have established the theory as an essential part of physics.

The essential point is that Einstein reached his new ideas by thinking about how reality must be. As Polanyi says, he discovered rationality in nature. His theory was a construction of the mind that was found to account for reality. In his account there is no mention of the Michelson-Morley experiment. As Polanyi says, its findings were rationally intuited by Einstein before he had ever heard about it. To make sure of this I addressed an enquiry to the late Professor Einstein, which confirmed that the Michelson-Morley experiment had a negligible effect on the discovery of relativity. Thus

> *the usual textbook account of relativity as a theoretical response to the Michelson-Morley experiment is an invention. It is the product of philosophical prejudices. When Einstein discovered rationality in nature, unaided by any observation that had not been available for at least fifty years before, our positivist textbooks promptly covered up the scandal by an appropriately-embellished account of his discovery.*

One of the earliest and most influential exponents of the idea that scientific theories have no claim to inherent rationality was Ernst Mach who by his book *Die Mechanik* published in 1883 founded the Vienna school of positivism. According to Mach, a scientific theory is not a more or less accurate account of reality, but is no more than a convenient summary of our experiences.

Its purpose is to save time and trouble in recording observations. It is the most accurate adaptation of thought to facts, and just as external to the facts as a map, or a timetable or a telephone directory; indeed, this conception of scientific theory would include a timetable or telephone directory among scientific theories. Accordingly, a scientific theory is denied all persuasive power that is intrinsic to itself, as a theory. It must not go beyond experience by affirming anything that cannot be tested by experience; and above all, scientists must be prepared immediately to drop a theory the moment an observation turns up that conflicts with it. In so far as a theory cannot be tested by experience --or appears not capable of being so tested– it ought to be revised so that its predictions are restricted to observable magnitudes.

Mach criticised Newtons definitions of space and absolute rest because they cannot be tested experimentally: to him they were not only dogmatic, but meaningless. He therefore urged that mechanics be reformulated so as to avoid referring to any movement of bodies except relative motion with respect to each other. This is essentially what Einstein achieved by his theory of relativity and indeed he acknowledged the influence that Mach had on his work, and in particular on his discovery of relativity.

It is worth noting, as Polanyi pointed out, that Mach forgot about the propagation of light and so did not realise that Newtons conception of space is far from being untestable. Einstein showed that it can be tested, and it is false. There is no single point at rest, and it is Machs achievement that this was eliminated from physics.

In spite of his debt to Mach, Einstein in his later years repudiated the positivistic conception of science. In 1909 Einstein praised Machs *Die Mechanik*, and in 1913 he wrote to Mach referring to his inspired investigation of the foundation of mechanics. Mach endorsed the theory of relativity in the second edition of his book on ‘The History and Root of the Principle of the Conservation of Energy‘, adding that the latest advances in physics were turning into reality his often expressed view that the foundations of physics may be thermal and electric. This phrase reveals that Mach had always been, not a physicist or a historian of physics, but a philosopher of sensations. Further evidence is provided by the Preface that Mach wrote on 1913 to his book on physical optics which showed that there are two genuine interpretations of Einsteins relativity, namely by Mach and Planck: Mach by repudiating its absolutist character and Planck by perceiving and enthusiastically endorsing it. Einstein at that time had not fully understood the full depth of his achievement, but the words of Mach and Planck gradually revealed it to him. Einstein realised that although like Mach he had read Kants Critique as a young man, unlike Mach he did not remain its captive. Einsteins cosmic vision was diametrically opposed to that of Mach. For Mach the cosmos was reduced to his own ego and sensations. For Einstein the cosmos loomed large in its own right. As early as 1901 he had written [8]:

> *As regards science I have got a few wonderful ideas in my head which need to be worked out in due course. I am now almost sure that my theory of the power of attraction of atoms can be extended to gases . . . It is a magnificent feeling to recognise the unity of a complex of phenomena which appear to be things quite apart from the direct physical truth.*

Einstein finally understood and repudiated Mach in 1922. He described the potentiality of Machs method as one that would provide a catalogue but not a system. He added that Mach was a good mechanic but a deplorable philosopher. In a letter to his friend Michael Besso, Einstein wrote of an essay on Adler, a protagonist of Mach, that Adler rides poor Machs horse to exhaustion. In his reply, Besso, an admirer of Mach, pictured Einstein as a latter-day Dom Quixote riding Machs horse. Einstein, as if cut to the quick, replied: I do not inveigh against Machs little horse: it cannot give birth to anything living; it can only exterminate harmful vermin.

In later years Einstein continued to repudiate positivism. This was not good news to the Vienna Circle of positivistic philosophers of science. In the late twenties, Franck warned German physicists against metaphysics and urged them to embrace Machs intellectual bequest. He was discomforted to learn from another German physicist that Einstein is entirely in accord with Plancks view that physical laws describe a reality in space and time that is independent of ourselves. In 1930 he told Maurice Schlick that he found the presentation of physical theory too positivistic. Einstein believed that the aim of physical theory is to find out not only how natures transactions are carried out but also why nature was exactly the way it was and not otherwise.

The example of Einsteins theory of relativity shows the primacy of rational thought in scientific discovery. The scientist first of all has an idea, a vision of reality, if you will, and he proceeds to test it by comparing it with experience. If it appears to be refuted by experience, he need not abandon his idea immediately, for the apparent exceptions may later fall into place for some reason not understood at the time. If however it contains a true account of reality, it will succeed not only where it was designed to succeed, but will bear fruit elsewhere. Thus the basis of Einsteins relativity theory is much wider than the Michelson-Morley experiment and has led to new conceptions such as the equivalence of mass and energy.

The alternative view, that science is simply the most economical description of the facts of experience, is refuted as soon as we examine what this implies in more detail. In the first place the scientist does not collect facts at random; he does so in a most careful and systematic manner, and his way of doing this is controlled by more or less well developed theories. When he has his facts they are thus immediately confronted by a theory. If scientific research were simply a matter of correlating facts, this can always be done in an infinite number of ways, and who is to decide between them? As Polanyi remarks [10]:

> *There are an infinite number of mathematical formulae which will cover any series of numerical observations. Any additional future observations can still be accounted for by an infinite number of formulae. Moreover, no mathematical function connecting instrument readings can ever constitute a scientific theory. Future instrument readings cannot ever be predicted. But this is a symptom of a deeper inadequacy, namely that the explicit content of a theory fails to account for the guidance it offers to future discoveries. To hold a natural law to be true is to believe that its presence will manifest itself in an indeterminate range of yet unknown and perhaps unthinkable consequences. It is to regard the law as a real feature of nature which, as such, exists beyond our control.*

It is one of the great merits of Polanyis analysis of scientific research that he brings out the essential contribution of the personal judgement of the scientist. There is no such thing as the scientific method; if there were, it could be carried out by a computer or a robot. It is an intensely personal activity that depends on judgement at every stage: what problem to study, what theory to use, how to carry out the experiment, how to make the measurements, how to interpret the results, what to do if there is a discrepancy between theory and experiment, when to stop the measurements, what to do next and so on.

According to Polanyi, scientific propositions do not refer definitely to any observable facts, but describe something real which may manifest itself in many indefinite ways. Thus

> *[...] there exist no explicit rules by which a scientific proposition can be obtained from observational data, and we must therefore accept also that no explicit rules exist to decide whether to uphold or abandon any scientific proposition in face of any particular new observation. The part of observation is to supply clues for the apprehension of reality: that is the process underlying scientific discovery. The apprehension of reality thus gained forms in its turn a clue to future observations: that is the process underlying verification. In both processes there is involved an intuition of the relation between observation and reality: a faculty which can range over all grades of sagacity, from the highest level present in the inspired guesses of scientific genius down to the minimum required for ordinary perception. Verification, even though usually more subject to rules than discovery, rests ultimately on mental powers which go beyond the application of any definite rules.*

Such a conclusion may appear less strange if we consider the phases through which the propositions of science are usually brought into existence. In the course of any single experimental enquiry the mutual stimulus between intuition and observation goes on all the time and takes on the most varied forms. Most of the time is spent in fruitless efforts, sustained by a fascination which will take beating after beating for months on end, and produce ever new outbursts of hope, each as fresh as the last so bitterly crushed the week or the month before. Vague shapes of the surmised truth suddenly take in the outlines of certainty, only to dissolve again in the light of second thoughts or of further experimental observations. Yet from time to time, certain visions of the truth, having made their appearance, continue to gain strength both by further reflection and additional evidence. These are the claims that may be adopted as final by the investigator and for which he may assume public responsibility by communicating them in print. This is how scientific propositions normally come into existence.

The certainty of such proposition can differ therefore only in degree from the previous preliminary results, many of which had appeared final at first and only later turned out to have been only preliminary. Which is not to say that we must always remain in doubt, but only that our decision what to accept as finally established cannot be wholly derived from any specific rules but must be taken in the light of our own personal judgment of the evidence.

In choosing a problem the investigator takes a decision fraught with risks. The task may be insoluble or just too difficult. In that case his effort will be wasted and with it the effort of his collaborators, as well as the money spent on the whole project. But to play safe may be equally wasteful. Mediocre results are no adequate return for the employment of high gifts, and may not even repay the money spent on achieving them. So the choice of the problem must not only anticipate something that is hidden and yet not inaccessible, but also assess the investigators own ability (and those of his collaborators) against the hardness of the task, and make a reasonable guess as to whether the hoped-for solution will be worth its price in terms of talent, labour and money. To form such estimates of the approximate feasibility of yet unknown prospective procedures, leading to unknown prospective results, is the day-to-day responsibility of anyone undertaking independent scientific or technical research. On such grounds as these he must even compare a number of different possible suggestions and select from them for attack the most promising problem. Yet I believe experience shows such a performance to be possible and that it can be relied upon to function with a considerable degree of reliability [11].

As an illustration of the subtlety of Polanyis analysis of scientific research it is useful to examine his attitude to new discoveries. Scientists are continually checking each others work, and it is this network of validations, in a large number of overlapping areas, that validate the truth of science as a whole. Now what does a scientist do if he finds a result which disagrees with what was previously found experimentally, or with the predictions of an established theory? The usual accounts of scientific research say that such differences are taken very seriously. In case of disagreements between two sets of experiments, new experiments are made to resolve the discrepancy; in the case of disagreement with a theory, then the theory is rejected.

In fact none of this happens. Scientific research is beset with so many pitfalls that it very often happens that discrepant results are obtained. What is the scientist to do? If he were to take each such result seriously, he would not have time to do much else. So he uses his judgement, and in most cases he rejects the discrepant result as due to some external and uninteresting cause. Polanyi gives many examples of this from the history of science. He recalls that Rutherford received a stream of reports of new results from all over the world; most of them he discarded, but a very few he seized upon and saw in them the germs of a new line of enquiry, and developed them with all the resources at his command.

As another example he recalls the work of Miller, a respected American physicist, who spent many years repeating the Michelson-Morley experiment and obtained results indicating a small but definite velocity of the earth relative to the aether. Although this work was done with extreme care, his results were simply ignored. The theory they threatened, Einsteins theory of relativity, seemed to be so securely based that a refutation was just not conceivable.

It may of course happen that the scientist is wrong to reject a discrepant observation. Here again Polanyi quotes a revealing example. The French astronomer Lalande, in the course of his extensive series of measurements of the positions of the stars, noticed that the coordinates he measured for a certain star on 8 and 10 May 1975, were discrepant. So he crossed out one and marked the other as doubtful. After all, if one is measuring the coordinates of thousands of stars, it is expected that one makes the occasional mistake. And yet it was not a star that he observed but the planet Neptune, which was identified as such only in 1846. If Lalande had followed up that discrepant observation he would have discovered the planet himself.

How then does the scientist decide what to take seriously and what to ignore? To answer this question Polanyi developed his theory of tacit knowledge, summed up in the expression We know more than we can tell. To illustrate this he quotes the familiar example of riding a bicycle. We all know how to do this, and yet we would be quite unable to write down a series of instructions so explicitly that someone who had never ridden a bicycle would, after reading them, be able to ride immediately. Most people would be unable to describe how they keep their balance or how they turn a corner. It is quite a subtle and complicated affair that we soon relegate to our subconscious without ever articulating it in detail. Another example given by Polanyi is the recognition of the face of a friend [12]. We are quite unable to describe the face in sufficient detail to enable a third person to perform the act of recognition, and yet we are able to recognise the face ourselves without difficulty. Further examples are the ability to swim, which is similar to the skill of riding a bicycle, and the skills necessary for medical diagnosis [11].

Scientific research is thus a skill, which can be learnt only by attending carefully to the example set by someone who already has that skill. We watch what he does, follow his movements as far as we can, and at a certain point catch on. If we dont succeed in doing this, we never learn the skill, and the teacher can do nothing beyond repeating the lesson in the hope that he will catch on the second time. Polanyi remarks that many of the great scientists were taught in their youth by other great scientists, as they in their turn must have been able to convey to their students some of their more subtle skills. This account of learning stresses that it is not the technical transference of objectively specifiable information, but that there is a gap that must be bridged by the intelligent cooperation of the pupil. The reception of the message in its entirety depends on the receiver discovering for himself the components that cannot be communicated explicitly.

Polanyi draws attention to the role of the subconscious in the learning process. Experiments have been made that show that subconscious learning does indeed take place, and Polanyi suggests that this is a component of all learning (Polanyi 1966, 8). Most of us know what it is like to try to understand something for an extended period without success, and then suddenly finding that it becomes clear. This may happen when we are not actively thinking about the problem at all; the solution just comes to our mind. These experiences do suggest that subconscious processes are going on continually and are an essential part of the learning process.

It is characteristic of tacit knowing that we become aware of the general through the particulars, our attention being concentrated on the general and not on the particulars. Once we look directly at the particulars we lose the whole. As an example of this, we recognise a face through the individual features, but if we were to look at the features individually we would lose the whole face. Similarly, if we concentrate our attention on the individual letters in a paragraph, or on the individual notes of a piece of music, we again lose the whole.

The admission of a tacit component to our knowledge raises a serious problem for the philosophy of science. The declared aim of modern science is to establish a strictly detached objective knowledge. But if tacit knowledge is always a component of scientific knowledge, then how can it be truly objective? Polanyi accepts this limitation to scientific knowledge, but it does seem possible to argue that although part of our knowledge is tacit, it becomes shared knowledge as it is assimilated by the whole body of scientists in a certain field. The knowledge is known to be shared by the unity of discourse among the scientists concerned. This is indeed a quite common experience of a scientist entering a new area, even if it is quite closely related to what he already knows. At first the papers he reads seem to be catalogues of partially related particulars. Then, as he thinks about it from the inside, it begins to cohere as a unified body of knowledge, and he understands why this scientist has studied this aspect and another one a different aspect, each with the aim of adding to our knowledge of the whole. This integration is a tacit process and yet the result can be properly termed the knowledge of an objective reality.

Another aspect of scientific research to which Polanyi draws attention is the changing criteria as to what constitutes acceptable science. As an example of this he quotes the work of Kepler, who found that the orbits of the seven planets corresponded quite well to the radii of the seven regular polyhedra one inside the other and just touching. He considered this to be a major discovery, whereas we would dismiss it as a mere coincidence. Another, more bizarre example, is provided by a letter in Nature recording that the average periods of gestation of a number of different animals from rabbits to cows are integral multiples of pi. The evidence was considerable and the agreement good, but no biologist would accept such a result. Our conception of the nature of things tells us that such a relationship is absurd, and indeed it was only published as a joke.

These considerations draw attention to an important aspect of the scientists work, namely deciding what is important and what is trivial. Polanyi proposes three criteria for the scientific interest of a contribution: its exactitude, its systematic importance and the intrinsic interest of the subject matter. These criteria cannot function apart from the consensus of the scientific community [11].

Polanyi rejected any specific set of rules for empirical induction. He mentions some of these claims to show

> *[...] (a) how to proceed by a prescribed operation from clues to discovery or at least (b) to show how to verify, or at the very least, (c) how to falsify an empirical proposition according to such rules. Claim (a) must be rejected in view of the demonstrable fact that discovery is separated by a logical gap from the grounds on which it is made. It is a travesty of the scientific method to conceive of it as an automatic process depending on the speed of piling up evidence for hypotheses chosen at random. The history of the great scientific controversies teaches us that claims (b) and (c) are equally unfounded.*

All formal rules of scientific procedures must prove ambiguous, for they will be interpreted quite differently, according to the particular conceptions about the nature of things by which the scientist is guided. And his chance of reaching true and important conclusions will depend decisively on the correctness and penetration of these conceptions. There is a type of empirical discovery that is achieved without any process of induction. De Broglies wave theory, the Copernican system and the theory of relativity, were all found by pure speculation guided by criteria of internal rationality. The triumph of the Michelson-Morley experiment, despite it giving the wrong result, the tragic sacrifice of D. D. Millers professional life to the pursuit of purely empirical tests of a great theoretical vision, are sardonic comments on the supposed supremacy of experiment over theory. Admittedly, other controversies, like those of fermentation, hypnotism and extra-sensory perception, seem to centre altogether on questions of factual evidence. By looking at these disputes more closely, it appears that the two sides do not accept the same facts as facts, and still less the same evidence as evidence. These terms are ambiguous precisely to the extent to which the two opposing opinions differ. For within two conceptual frameworks the same range of experience takes the shape of different facts and different evidence.

The common perception of the scientist is of one coldly detached, recording dispassionately the results of his observations. This is far from reality. Polanyi draws attention to what he calls the intellectual passion of science: Passions charge objects with emotions, making them repulsive or attractive; positive passions affirm that something is precious. The excitement of the scientist making a discovery is an intellectual passion, telling him that something is intellectually precious and, more particularly, that it is precious to science.

The function which I attribute here to scientific passion is that of distinguishing between demonstrable facts which are of scientific interest, and those which are not. Only a tiny fraction of knowable facts are of interest to scientists, and scientific passion serves also as a guide in the assessment of what is of higher and what is of lesser interest; of what is great in science and what is relatively slight. I want to show that this appreciation depends on a sort of intellectual beauty; that it is an emotional response which can never be dispassionately defined, any more than we can dispassionately define the beauty of a work of art or the excellence of a noble action.

Scientific discovery reveals new knowledge, but the new vision which accompanies it is not knowledge. It is less than knowledge, for it is a guess; but it is more than knowledge, for it is a foreknowledge of things yet unknown and at present perhaps inconceivable. Our vision of the general nature of things is our guide for the interpretation of all future experience. Such guidance is indispensable. Theories of the scientific method which try to explain the establishment of scientific truth by any purely objective formal procedure are doomed to failure. Any process of enquiry unguarded by intellectual passions would inevitably spread out into a desert of trivialities. Our vision of reality, to which our sense of scientific beauty responds, must suggest to us the kind of questions that it should be reasonable and interesting to explore. It should recommend the kind of conceptions and empirical relations that are intrinsically plausible and which should therefore be upheld, even when some evidence seems to contradict them, and tell us also, on the other hand, what empirical connections to reject as spurious, even thought there is evidence for them evidence that we may as yet be unable to account for on any other assumption. In fact, without a scale of interest and plausibility based on a vision of reality, nothing can be discovered that is of value to science; and only our grasp of scientific beauty, responding to the evidence of our senses, can evoke the vision.

One of the central problems in the philosophy of science is how to reconcile the working scientists sense that in the course of his work he is discovering certain truths about an objectively existing world, and the philosophical arguments that can be urged against that conviction, supported by examples from the history of science.

Thus it can be argued that the ability of the scientist consists essentially in ordering his observations and measurements into the most convenient and coherent pattern. When new results become available he either fits them into the existing pattern or, if this is not possible, he rearranges the pattern. In periods of revolutionary change it may be necessary to change the whole pattern, as for example occurred when Einsteins relativistic dynamics replaced that of Newton. In what sense, therefore, can we say that Newtons laws are true? And if we say that they are not true, then how can we say that anything in science is anything but provisional and subject to replacement by a more comprehensive theory?

In answer to this one can say that it is oversimplified to say that a theory is true or false. There are degrees of truth, and it is quite possible for a theory or concept to be a useful approximation to the truth; it is neither wholly true nor wholly false. Thus we can say that Newtons laws describe very well the behaviour of particles moving with velocities much less than the velocity of light, and this remains true. Einsteins theory of relativity shows us how to treat motions at much higher velocities, and his equations reduce to those of Newton in the limit of low velocities. It could be argued that nevertheless the Newtonian and Einsteinian concepts of space and time are quite different, so that there is a decisive break between the two theories at a very fundamental level. Once again it can be replied that this distinction is too sharp, and that there is an analogical relation between the two sets of relationships.

But how can we be sure that the advances in science will always be of this type? Is it not conceivable that new observations may require a conceptual reformulation so radical that there will be no such links between new and old? This does not seem to be possible, if only because whatever new observations are made on matter under extreme conditions, the old observations still remain. It is always necessary that the new theory accounts for both the old and the new observations, and this necessarily implies an analogical relation between the two theories. The problem can usefully be approached from a quite different direction. Einstein himself expressed his ideas in quasi-theological terms. Thus when discussing Heisenbergs uncertainty principle he remarked that I like to think of the electron as God sees it. God sees the world as it really is, in its innermost essence, and He knows the laws that it obeys. The laws devised by physicists are thus inevitably approximations, both mathematically and conceptually, to the reality as known by God.

It does not follow from this that the development of science must inevitably follow the same road. If one imagines a completely different scientific development on another planet, it will not be the same as the development that has taken place here on earth. In what ways would it be different? Obviously in numerous superficial ways, such as the choice of units and notation. If the physical condition on the other planet are very different from those on earth the phenomena that first attracted systematic investigation might be different from those first studied here. Yet as science develops, as concepts of greater generality are formulated, it is inevitable that the different scientific developments will approach one another as they both approach more closely to the truth as seen by God.

4. The way to truth in theology

There are fundamental differences between science and theology that affect their ways of attaining truth. In science truth comes from studying the natural world, whereas in theology it comes by Revelation and the teaching of the church. A consensus is readily attained in science, whereas in theology important differences persist for centuries. Nevertheless, in spite of these differences, the way to truth in theology still has many similarities to that in science, in particular the recognition of theological truth is also a process involving the cumulative effect of many separate indications.

The difference in attaining the recognition of truth in science and theology is essentially a matter of timescale. It is much easier in science to verify the truth by making observations, experiments and measurements. A scientific speculation can be expressed mathematically and its consequences compared with the results of measurements. The criterion is whether the theoretical and experimental numbers agree within the uncertainties of measurement. Such sharp tests are not available in theology, but that does not mean that tests are impossible. We can test theological belief by seeing if we can live by them. The difference from science is that it takes much longer to test theological beliefs in this way. Throughout history there have been numerous systems of belief that have flourished for a few decades or even for hundreds of years, but have eventually faded away. The ability to accept a theological truth depends on the beliefs we inherit as part of our culture. Christians believe that the natural world is good, orderly, contingent and open to the human mind, and these beliefs form the basis of science. Other cultures, such as those of Asia and Africa, lack one or more of these beliefs. They can be learnt and then scientists in those cultures can accept the scientific consensus. It is however much more difficult for them to attain consensus of theological beliefs. Apart from this, the way to the acceptance of truth may be similar in different cultures.

This can be illustrated by Newmans analysis of the attainment of theological truth. He wanted to understand how uneducated peasants are able to attain certitude about their Christian beliefs, and to answer this question developed his grammar of assent. Newman saw that the peasant reaches faith by the convergence of many separate indications, none individually conclusive but together compelling. He called this the unity of indirect reference, and the way it is attained the illative sense. This is just the same way that the scientists attain truth [14].

He begins his argument by remarking that man is NOT a rational animal; he is a seeing, feeling, contemplating, acting animal [9]. From this he argued that assent or belief is not arrived at primarily by logic but by the whole man. He is sure that logic is inadequate to a complete statement of our mental processes since so much of our reasoning is done subconsciously, mingling memories, emotions, associations with strictly ratiocinate elements, so that the whole man reasons, not just the mind. Thus Newmans entrance to the Roman Catholic Church was motivated not merely by the vast array of arguments that fill his Essay on the Development of Christian Doctrine but by the convergence of innumerable probabilities. He believed that the act of assent to a new conclusion, whether ostensibly from an act of conscious inference or not, is always a definite step in response to many rational and non-rational inferences, unconscious as well as conscious not the mere mechanical passive logical recognition then and there of an inference from premises [6].

It is the penetrating and subtle action of the human mind that it passes from verbal argumentation to conclusions in the concrete. It determines what science cannot determine, the limit of converging probabilities and the reasons sufficient for a proof. It is the ratiocinative mind itself, and no trick of art, however simple in its form and sure in operation, by which we are able to determine, and thereupon to be certain, that a moving body left to itself will never stop, and that no man can live without eating.

He distinguished between notional and real assent. We give notional assent, for example, to the result of a mathematical demonstration, or to the suggested interpretation of a single experiment. We follow the steps of the argument, we see no flaw in the reasoning and the result seems acceptable. And yet it does not engage us personally, and furthermore we know from previous experience that such demonstrations often have hidden flaws. We may accept the result provisionally, but would not be inclined to die for it. As Newman wrote: the heart is commonly reached, not through the reason, but through the imagination, by means of direct impressions, by the testimony of facts and events, by history, by description. Many a man will live and die upon a dogma: no man will be a martyr for a conclusion . . . no one will die for his own calculations: he dies for realities [9]. Furthermore, to most men argument makes the point in hand only more doubtful, and considerably less impressive. Life is for action. If we insist on proofs for everything, we shall never come to action: to act you must assume, and that assumption is faith: A man convinced against his will is of the same opinion still.

Real assent is quite different. It is based on the convergence and coherence of a large number of separate indications. The whole is so compelling that it is impossible to imagine it being refuted, thought it may be refined and perhaps shown to be part of an even more comprehensive system. Its strength is such that if one or other of the contributing indicators is shown to be defective, we are confident that in the fullness of time the apparent discrepancy will be resolved. Real assent engages the whole person. Sometimes a real assent obtained in this way is confirmed by an apodictic proof, but in other cases this is impossible due to the nature of the case. This is usual in theology because God respects our freedom and so does not force us to believe. Thus real assent is unconditional, in contrast to inference, which is always conditional. Notional assent does not affect our actions, but real assent does. Real assent, Newman wrote,

> *is always an unconditional acceptance of a proposition, while notional assent is an acceptance on condition of acceptance of the premises. In its Notional Assents as well as in its inferences, the mind contemplates it own creations instead of things; in Real, it is directed towards things, represented by the impressions which they have left on the imagination. These images, when assented to, have an influence both on the individual and on society, which mere notions cannot exert.*

Thus acts of Notional Assent and of Inference do not affect our conduct, and acts of Belief, that is, of Real Assent, do (not necessarily, but do) affect it.

As an example, Newman contrast the ways Job apprehended God before and after his afflictions. He says he had a true apprehension of the Divine Attributes before as well as after, but with the trial came a great change in the character of that apprehension: With the hearing of the ear, he says, 'I have heard Thee, but now mine eyes seeth Thee; therefore I reprehend myself, and do penance in dust and ashes .

Acts of inference are both the antecedents of assent before assenting, and its usual concomitants after assenting. For instance, I hold absolutely that the country which we call India exists, upon trustworthy testimony; and next, I may continue to believe it on the same testimony. In like manner, I have ever believed that Great Britain is an island, for certain sufficient reasons; and on the same reasons I may persist in the belief. But it may happen that I forget my reasons for what I believe to be absolutely true; or I may never have asked myself about them, or formally marshalled them in order, and have been accustomed to assent without a recognition of my assent or of its grounds, and then perhaps something occurs which leads to my reviewing and completing these grounds, analysing and arranging them, yet without on that account implying of necessity of any suspense, ever so slight, of assent, to the proposition that India is in a certain part of the earth, and that Great Britain is an island.

While a syllogism is readily accepted by a logical mind, the assessment of probabilities may more readily convince one person rather than another:

> *A syllogism is at least a demonstration, when the premises are granted, but a cumulation of probabilities, over and above their implicit character, will vary both in their number and their separate estimated value, according to the particular intellect which is employed upon it. It follows that what to one intellect is a proof it is not so to another, and that the certainty of a proposition, does properly consist in the certitude of the mind which contemplates it.*

Thus, he adds, it is the fact that many of our most obstinate and most reasonable certitudes depend on proofs which are informal and personal, which baffle our powers of analysis, and cannot be brought under logical rule, because they cannot be submitted to logical statistics. As Aristotle remarked, each man judges skilfully in those things about which he is well-instructed; it is of these that he is a good judge; viz. he, in each subject matter, is a judge, who is well-educated in that subject matter, and he is an absolute sense a judge, who is in all of them well-educated.

Locke also believed that there are cases in which evidence, not sufficient for a scientific proof, is nevertheless sufficient for assent and certitude. Thus supralogical judgment, which is the warrant for our certitude about them, is not mere common-sense, but the true healthy action of our ratiocinative powers, an action more subtle and more comprehensive than the mere application of a syllogistic argument.

Newman also applied his analysis to scientific questions, remarking that

> *moral evidence and moral certitude are all that we can attain, not only in the case of ethical and spiritual subjects, such as religion, but of terrestrial and cosmical questions also. So far, physical Astronomy and Revelation stand on the same footing. Vince, in his treatise on Astronomy, does but use the language of philosophical sobriety, when, after speaking of the proofs of the earths rotary motion, he says, When these reasons, all upon different principles, are considered they amount to a proof of the earths rotation about its axis, which is as satisfactory to the mind as a direct demonstration of it(242).*

Newman cites several other examples, such as the ability of a peasant to foretell the weather, of a doctor to diagnose a patient and of a lawyer to perceive guilt.

In science, as well as theology, the recognition that a series of separate indications cohere to demonstrate a truth may come suddenly. Chesterton has described the sensation when he realised that Christianity is the key to life. As he reviewed the argument [2],

> *. . . one after the other, all the parts fitted and fell in with an eerie exactitude. I could hear bolt after bolt all over the machinery falling into its place with a kind of click of relief . . . . The evidence is not really in this or that alleged demonstration; it is in an enormous accumulation of small but unanimous facts.*

He realised that he believed in Christianity not because of a logical argument, but because it makes sense of everything: When one believes in a creed, one is proud of its complexity, as scientists are proud of the complexity of science. It shows how rich it is in discoveries. If it is right at all, it is a compliment to say that its elaborately right. The scientist and the Christian thus react in the same way when challenged to prove their beliefs. The truth of Christianity or of the atomic theory or of heliocentrism does not rest on one or a few logical arguments; it rests in each case on an enormous accumulation of individual experiences that is quite impossible to summarise in a concise way.

Newman also applies his theory of assent to the belief in Christianity. The mind is addressed both through the intellect and through the imagination; creating a certitude of its truth by arguments too various for direct enumeration, too personal and deep for words, too powerful and concurrent for refutation. Nor need reason come first and faith second (though this is the logical order), but one and the same teaching is in different aspects both object and proof, and elicits one complex act both of inference and assent. It speaks to us one by one, and it is received by us one by one, as the counterpart, so to say, of ourselves, and is real as we are real.

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