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SCIENCE: OBSERVATION AND BELIEF

I.

People who believe in science do not usually regard this as a personal act of faith. They consider themselves as submitting to evidence which by its nature compels their assent and which has the power to compel a similar measure of assent from any rational human being. For modern science was founded on a critical struggle against all authority. Critical thought broke the fetters of Aristotelian and of Biblical authority. Descartes led the way by his programme of universal doubt: *de omnibus dubitandum*. The Royal Society was founded with the motto: *nullius in verba*. We accept no authority. Bacon had claimed that science was to be based on purely empirical methods, and *hypotheses non fingo*, No speculations! echoed Newton. Science has been through the centuries the scourge of all creeds which embodied an act of faith and was supposed—and is commonly still supposed—to be built, in contrast to these creeds, on a foundation of hard facts. It is thought that in science facts alone count.

Yet it is quite easy to see that this is not true, as David Hume had found out already some 200 years ago. The argument can be stated without any verbal ambiguities in simple mathematical terms. Suppose the evidence on which a scientific proposition is to be based consists of a number of measurements made at various observed times or in coincidence with some other measurable parameter. Let us in other words have pairs of two measured variables V_1 and V_2. Can we decide from a series of points V_1 plotted against V_2 whether there is a function $V_1 = f(V_2)$ and if so, what it is? Clearly we can do nothing of the kind. Any set of pairs of V_1 and V_2 values is compatible with an infinite number of functional relations between which there is nothing to choose from the point of view of the underlying data. To choose any of the infinite possible functions and give it the distinction of a scientific proposition is so far without any justification. The measured data are insufficient for the construction of a definite function $V_1 = f(V_2)$ in exactly the same sense as two elements of a triangle are insufficient to determine a definite triangle.

This conclusion is not altered but only obscured by introducing the element of scientific prediction. For one thing, prediction is not a regular attribute of scientific propositions. Kepler's laws and the Darwinian theory predicted nothing. At any rate, successful prediction does not fundamentally change the status of a scientific proposition. It only adds a number of observations, the predicted observations, to our series of measurements and cannot change the fact that any series of measurements is incapable of defining a function between the measured variables.

Since some readers may be reluctant to accept this, I shall illustrate it a little further. Suppose a player of roulette observes the numbers or colours that have turned up in a hundred consecutive throws. He may plot them in a graph and derive a function in the light of which he will make a prediction. He may try it out and win. He may try it again and win. And win a third time. Would

that prove this generalisation? No, it would, in our view, only prove that some roulette players are very lucky—i.e., we would consider these predictions to be mere coincidences.

A few years ago I saw in *Nature* a table of figures proving with great accuracy that the time of gestation, measured in days, of a number of different rodents is a multiple of the number π. An exact relationship of this kind makes no impression on the modern scientist and no amount of further confirmatory evidence would convince him that there is any relation between the period of gestation of rodents and multiples of the number π.

Anyone who has friends among astrologers can have from them instances of strikingly fulfilled predictions which would be hard to rival in science. Yet scientists refuse even to consider the merits of astrological predictions.

In science itself I could tell you of the most amazing predictions gone true, like the discovery of heavy hydrogen, which turned out to be based on premisses which later were found to be quite erroneous. There is no definite and rational criterion by which the accidental fulfilment of a prediction can be discriminated from its true confirmation.

Scientists and philosophers who are convinced that science can be based exclusively on data of experience, have tried to avoid the weight of such critical analysis of science by reducing the claims of science to a more moderate level. They point out that scientific propositions do not claim to be true, but only to be likely. That they do not predict anything with certainty, but only with probability. That they are provisional and make no claim to finality.

All this is entirely beside the point. If anyone claims that, given two angles of a triangle, he can construct the triangle, his claim is equally nonsensical, whether he claims to give a true construction or merely a probable construction, or the construction of a merely probable triangle. The selection of one element out of an infinite set of elements all of which satisfy the conditions set by the problems, remains equally unjustifiable whatever positive quality we attach to our selection. Its value is exactly nought. In fact, scientists would object just as much to serial rules in games of chance or astrological predictions, or to relations between time of gestation of rodents and the number π, whether these are claimed with certainty, or only with probability or else merely provisionally. They would be regarded as no less nonsensical for that.

Nor does another attempt to lessen the burden of responsibility on scientists' shoulders prove more successful. Science, it is said, does not claim to discover the truth but only to give a description or summary of observational data. But why then object to astrology or to the description of periods of pregnancy in multiples of the number π? Obviously, because these are not held to be true or rational descriptions; which brings the problem back exactly to where it was before. For it is no easier to find a justification for picking out one description of the observational data as true or as rational than it is to pick out any other relationship whatever its claims may be.

Again, the attempt has been made to lessen the difficulty of justifying the claims of science by suggesting that the statements of science do not claim to be true except in the sense of being simple. But this of course is pure nonsense.

Scientists do not reject astrology, magic or the cosmogony of the Bible because these are not simple enough. That has nothing to do with it; unless indeed the word " simple " is tortured into meaning "rational", and finally made to coincide with " true ".

II.

So whichever way we turn we cannot avoid being faced with the fact that the validity of scientific statements is not compellingly inherent in the evidence to which they refer. Those who believe in science must, therefore, accept that they are placing on the evidence of their senses an interpretation for which they must themselves take a considerable amount of responsibility. In accepting science as a whole and in subscribing to any particular statement of science, they are relying to a certain extent on a personal conviction of their own.

Some of these personal convictions we derive from our own upbringing. The commonly accepted causal interpretation of nature on which all scientific thought is based we absorb automatically when growing up in a modern environment. It is in sharp contrast to the magical outlook of primitive cultures in which all events that have any bearing on the interests of man are exempted from ordinary causation and assumed to be due to magic influence, whether beneficent or malign. This magical outlook is apparently more readily acceptable to man than the causal view of events. It prevails in the child's approach to its surroundings, who is inclined to think that everything that happens to it is the result of a purposive action of some obscure agency. The magic outlook underlies also our works of fiction in which everything that happens to a character in the course of the story must have some justification in the sense of the story; otherwise it is not a work of art but a meaningless chronicle. Not fiction, but simply an untrue and irrelevant statement. People who normally use the magical outlook are of the same average grade of intelligence as their naturalistically instructed fellow men. But to them the scientific way of thinking is inaccessible over a large area, namely wherever human interests are involved.

The rejection *in limine* of the evidence of astrology is a definite consequence of a break with the magical outlook. But there are more specific beliefs than that involved in the acceptance of science. Take for example our rejection of the evidence connecting the period of gestation of rodents with the multiples of the number π. This represents a comparatively recent point of view in science. To a scientist like Kepler there would have been nothing repugnant in the relationship suggested here. He had himself derived the existence of the then known seven planets and the relative size of their orbits from a supposed connection with the existence of seven perfect solids and the relative size of spheres inscribed in them and enveloping them, the edges of the solids being taken to be constant. The science of his generation was still largely pursuing the Phythagorean supposition of the world being governed by the number rules and geometrical relationships. The fundamental discovery of Pythagoras of acoustic harmonies connected with simple ratios of the length of chord emitting the tone, had impressed this supposition for centuries on the speculative mind. The discovery of Copernicus was still largely based on it.

It would take me too long to trace here in detail the successive stages through which the premisses of science have passed from Kepler's day to our own. The

main period from Galileo to Young, Fresnel and Faraday was dominated by the idea of a mechanical universe consisting of matter in motion. This was modified by the field theories of Faraday and Maxwell but not radically changed so long as the postulate of a material ether was upheld. Until the end of the 19th century, scientists believed implicitly in the mechanical explanation of all phenomena. In the last 50 years these premisses of science were abandoned but not without having caused considerable delay in the progress of discoveries which were inaccessible from such premisses. A good deal of evidence for the existence of the electron had been available for a long time before it overcame the resistance offered by the assumptions of Galileo that all properties of matter had to be explained by mass in motion. An entirely new assumption was imported into science from Mach's philosophy by Einstein in his discovery of relativity. Mach had set out to eliminate all tautologies from scientific statements: Einstein assumed that by modifying our conceptions of space and time on the lines of such a programme it should be possible to draw up a system which would eliminate some existing anomalies and possibly lead to new verifiable conclusions. This is the "epistomological" method which is profoundly ingrained to-day in our conception of the universe. We may illustrate this by the following event. In 1928 a reputable American physicist called Milner repeated for the first time after a generation, Michelson's experiment on which the theory of relativity was originally based. Equipped with the most modern instruments he thought he had a good right to check up on these rather hoary observations of a great master. His results controverted those of Michelson and he announced this to a representative gathering of physicists. But not one of them thought for one moment of abandoning relativity. Instead—as Sir Charles Darwin described it—they sent Milner home to get his results right.

Twenty years after the discovery of relativity there came a further fundamental modification of our outlook on nature by the acceptance of a purely statistical interpretation of atomic interactions. Einstein who was then 45 years old rejected this view on the grounds of common sense. He continued to believe, and still believes, alone among physicists, so far as I know, that atomic processes are fundamentally causal. He does not believe that any process can be fundamentally indeterminate. His old friends have reproached him that he was sticking to the same kind of prejudice by which the opponents of relativity had obstructed its path in earlier years. But in spite of that and of much persuasion on the part of Niels Bohr, Einstein holds fast to his dissenting view of nature. Maybe he is right.

III.

We can see even from this brief sketch how the beliefs of scientists regarding the essence of nature are held by them on their own responsibility, underlying their methods of discovery and determining their readiness to accept a certain type of evidence or to reject it as the case may be.

The whole activity of scientists is based on a set of surmises of different grades. Some held as implicit, quite unconscious beliefs, others as more or less definite assumptions, others again entertained as personal hunches. They are embodied in the general aims of all sciences and in their general methods. They are to be found taking on special shapes in each separate branch.

Little or nothing of these beliefs of science is codified. They are not taught as such in textbooks, it is impossible to formulate them in explicit terms. They are impalpable, like the rules of an art. They are, in fact, rules of an art. They are transmitted personally from master to pupil by the imitation of the practice of discovery and of the practice of verification. This is why science has such a strong local tradition in certain countries. It is extremely difficult to transplant science to new countries lacking that tradition.

There is no essential difference in this connection between science as it emerges in the process of discovery and science as established in textbooks. At all stages of consolidation science must ultimately rely on a set of beliefs derived mainly from the scientific tradition. That is why science cannot be taught properly unless its teaching is informed by the personal experience of discovery. Such a spirit alone can restore to science that fundamental uncertainty and plasticity, that sense of inexhaustible new possibilities, which are proper to science at all stages of achievement.

Moreover, we have to realise that apart from beliefs held in common with other people—whether other scientists or members of the general public—there is an element of personal judgment in every scientific affirmation. Personal creative judgment is at the source of all discovery. And again it is indispensable at each stage of research, not the least at the end when a claim is to be made in public. In the case of many great discoveries, such as the Copernican system, or Darwinism, Mendelism, the bacterial theory, relativity, quantummechanics, etc., the evidence at first does not induce general approval among scientists. The discoverer has to put his claim forward supported by his own conviction alone. But in any case, there is always a conceivable doubt of any particular statement in science, and it is the last resort for the scientist's conscience to decide whether the doubt is reasonable or not.

Science, we find, cannot be based on a radical empiricism but is on the contrary invalidated from the empiricist point of view by an analysis of its observational foundations. The validation of science is based, in fact, apart from sense experience, on the holding of certain beliefs which are: (1) Partly instilled in us by general tradition and held implicitly by all modern men; (2) partly accepted by scientists as an element of the scientific tradition; and (3) partly nurtured as individual hunches; or else (4) affirmed as the dictates of our personal consciences. In other words, science is based on experience selected and interpreted in the light of certain traditional, intuitive and conscientious beliefs.

IV.

Science cannot go on relying much longer on an empiricism which does not in fact justify its claim to validity. Powerful forces are already taking advantage of this weakness. It leaves science defenceless against a radical denial of its objectivity. It opens the door to its Marxist interpretation which would reduce science to an ideology. It invites the intervention of the State to direct the pursuit of science in the visible interests of society. (I have discussed this situation at some length on other occasions).

Science, free science, can survive in future only by recognising and consciously affirming its true basis: its groundwork of scientific beliefs. Scientists must

henceforth profess their adherence to these beliefs by an explicit declaration of faith.

But why should we believe in anything? In anything that we could also disbelieve? Why should we follow a tradition that is the example of people who have died long ago and who knew less about our problems than we do? Why should we share the beliefs which scientists hold to-day, which they are quite likely to change again before long? Why should we allow ourselves to be guided by our own hunches in research or in accepting or rejecting some modern theory? Why should we wrestle with our scientific conscience and try to satisfy its demands?

Why indeed? I do not propose to attempt a full answer to this question. But one or two suggestions may be made here. A path to a conscious acceptance of belief may be found in the fact that we must believe in something. There is no way short of death or idiocy to keep all our judgments in suspense. All we can do is to drive out all positive and explicit forms of beliefs until we are left to hold only instinctive and unconscious beliefs. *There is no reason why involuntary beliefs should be better than beliefs which we deliberately profess.* To assume this was the fundamental error of rationalism since Descartes. This critical age of ours has seen outbreaks of fanaticism rarely paralleled in the history of professed creeds. Our most critical and advanced minds have often succumbed to propaganda and shown a measure of gullibility which was not common among intellectual leaders of less hard-boiled centuries. Any thinking person must believe in something uncritically, for he must believe in the premisses of his own thinking. *Cogito ergo credo*—I think, therefore I believe. Let us accept this fact and believe with open eyes. We have then a chance to hold our beliefs in mature consideration of alternative beliefs, and not merely to succumb to some uncontrolled residue of belief.

For that end we may also seek contact with other essentials of our civilisation which require to be upheld by a positive belief. Science has close relations with other realms of truth. The new scientific approach to nature which arose in the 16th century was the fruit—and a late fruit—of the same movement which liberated art, literature, scholarship and religious conscience. The new demands for an open-minded and fair examination of evidence was paralleled in many other fields of life. It soon entered into public life by the new doctrine of tolerance—that great contribution of England, Holland and America to modern civilisation. The scientific mode of thought caused also a revolution in law. Within a few decades of the foundation of the Royal Society the law of evidence was radically reformed and placed on a scientific basis.

All these developments hang together and are commonly rooted in the great traditions of our civilisation: in the tradition of intellectual integrity descended from the Greeks, of legal reason inherited from Rome and of brotherhood inspired by Christianity. These traditions embody transcendent beliefs which for centuries could safely be taken for granted. They cannot be so taken any more to-day. Our whole civilisation, including science, now needs reformation by a positive profession of the beliefs which form its foundation.

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