PHILOSOPHICAL PROBLEMS OF *SCIENCE & TECHNOLOGY*

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ZIMAN, "WHAT IS SCIENCE?"

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READING 1

What Is Science?

John Ziman

To answer the question 'What is Science?' is almost as presumptuous as to try to state the meaning of Life itself. Science has become a major part of the stock of our minds; its products are the furniture of our surroundings. We must accept it, as the god lady of the fable is said to have agreed to accept the Universe.

Yet the question is puzzling rather than mysterious. Science is very clearly a conscious artifact of mankind, with well-documented historical origins, with a definable scope and content, and with recognizable professional practitioners and exponents. The task of defining Poetry, say, whose subject matter is by common consent ineffable, must be self-defeating. Poetry has no rules, no method, no graduate schools, no logic: the bards are self-anointed and their spirit bloweth where it listeth. Science, by contrast, is rigorous, methodical, academic, logical and practical. The very facility that it gives us, of clear understanding, of seeing things sharply in focus, makes us feel that the instrument itself is very real and hard and definite. Surely we can state, in a few words, its essential nature.

It is not difficult to state the order of being to which Science belongs. It is one of the categories of the intellectual commentary that Man makes on his World. Amongst its kith and kin we would put Religion, Art, Poetry, Law, Philosophy, Technology, etc.—the familiar divisions or 'Faculties' of the Academy or the Multiversity.

At this stage I do not mean to analyse the precise relationship that exists between Science and each of these cognate modes of thought; I am merely asserting that they are on all fours with one another. It makes some sort of sense (though it may not always be stating a truth) to substitute these words for one another, in phrases like 'Science teaches us . . .' or 'The Spirit of *Law* is . . .' or '*Technology* benefits mankind by . . .' or 'He is a student of

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Philosophy'. The famous 'conflict between Science and Religion' was truly a battle between combatants of the same species—between David and Goliath if you will—and not, say, between the Philistine army and a Dryad, or between a point of order and a postage stamp.

Science is obviously like Religion, Law, Philosophy, etc. in being a more or less coherent set of ideas. In its own technical language, Science is information; it does not act directly on the body; it speaks to the mind. Religion and Poetry, we may concede, speak also to the emotions, and the statements of Art can seldom be written or expressed verbally—but they all belong in the non-material realm.

But in what ways are these forms of knowledge *unlike* one another? What are the special attributes of Science? What is the criterion for drawing lines of demarcation about it, to distinguish it from Philosophy, or from Technology, or from Poetry?

This question has long been debated. Famous books have been devoted to it. It has been the theme of whole schools of philosophy. To give an account of all the answers, with all their variations, would require a history of Western thought. It is a daunting subject. Nevertheless, the types of definition with which we are familiar can be stated crudely.

Science Is the Mastery of Man's Environment. This is, I think, the vulgar conception. It identifies Science with its products. It points to penicillin or to an artificial satellite and tells us of all the wonderful further powers that man will soon acquire by the same agency.

This definition enshrines two separate errors. In the first place it confounds Science with Technology. It puts all its emphasis on the applications of scientific knowledge and gives no hint as to the intellectual procedures by which that knowledge may be successfully obtained. It does not really discriminate between Science and Magic, and gives us no reason for studies such as Cosmology and Pure Mathematics, which seem entirely remote from practical use.

It also confuses ideas with things. Penicillin is not Science, any more than a cathedral is Religion or a witness box is Law. The material manifestations and powers of Science, however beneficial, awe-inspiring, monstrous, or beautiful, are not even symbolic; they belong in a different logical realm, just as a building is not equivalent to or symbolic of the architect's blueprints. A meal is not the same as a recipe.

Science Is the Study of the Material World. This sort of definition is also very familiar in popular thought. It derives, I guess, from

the great debate between Science and Religion, whose outcome was a treaty of partition in which Religion was left with the realm of the Spirit whilst Science was allowed full sway in the territory of Matter.

Now it is true that one of the aims of Science is to provide us with a Philosophy of Nature, and it is also true that many questions of a moral or spiritual kind cannot be answered at all within a scientific framework. But the dichotomy between Matter and Spirit is an obsolete philosophical notion which does not stand up very well to careful critical analysis. If we stick to this definition we may end up in a circular argument in which Matter is only recognizable as the subject matter of Science. Even then, we shall have stretched the meaning of words a long way in order to accommodate Psychology, or Sociology, within the Scientific stable.

This definition would also exclude Pure Mathematics. Surely this is wrong. Mathematical thinking is so deeply entangled with the physical sciences that one cannot draw a line between them. Modern mathematicians think of themselves as exploring the logical consequences (the 'theorems') of different sets of hypotheses or 'axioms', and do not claim absolute truth, in a material sense, for their results. Theoretical physicists and applied mathematicians try to confine their explorations to systems of hypotheses that they believe to reflect properties of the 'real' world, but they often have no license for this belief. It would be absurd to have to say that Newton's *Principia,* and all the work that was built upon it, was not now Science, just because we now suppose that the inverse square law of gravitation is not perfectly true in an Einsteinian universe. I suspect that the exclusion of the 'Queen of the Sciences' from her throne is a relic of some ancient academic arrangement, such as the combination of classical literary studies with mathematics in the Cambridge Tripos, and has no better justification than that Euclid and Archimedes wrote in Greek.

Science Is the Experimental Method. The recognition of the importance of experiment was the key event in the history of Science. The Baconian thesis was sound; we can often do no better today than to follow it.

Yet this definition is incomplete in several respects. It arbitrarily excludes Pure Mathematics, and needs to be supplemented to take cognisance of those perfectly respectable sciences such as Astronomy or Geology where we can only observe the consequences of events and circumstances over which we have no control. It also fails to give due credit to the strong theoretical and logical sinews that are needed to hold the results of experiments and observations together and give them force. Scientists do not in fact work in the way that operationalists suggest; they tend to look for, and find, in Nature little more than they believe to be there, and yet they construct airier theoretical systems than their actual observations warrant. Experiment distinguishes Science from the older, more speculative ways to knowledge but it does not fully characterize the scientific method.

Science Arrives at Truth by Logical Inferences from Empirical Observations. This is the standard type of definition favoured by most serious philosophers. It is usually based upon the principle of induction—that what has been seen to happen a great many times is almost sure to happen invariably and may be treated as a basic fact or Law upon which a firm structure of theory can be erected.

There is no doubt that this is the official philosophy by which most practical scientists work. From it one can deduce a number of practical procedures, such as the testing of theory by 'predictions' of the results of future observations, and their subsequent confirmation. The importance of speculative thinking is recognized, provided that it is curbed by conformity to facts. There is no restriction of a metaphysical kind upon the subject matter of Science, except that it must be amenable to observations and inference.

But the attempt to make these principles logically watertight does not seem to have succeeded. What may be called the positivist programme, which would assign the label 'True' to statements that satisfy these criteria, is plausible but not finally compelling. Many philosophers have now sadly come to the conclusion that there is no ultimate procedure which will wring the last drops of uncertainty from what scientists call their knowledge.

And although working scientists would probably state that this is the Rule of their Order, and the only safe principle upon which their discoveries may be based, they do not always obey it in practice. We often find complex theories—quite good theories—that really depend on very few observations. It is extraordinary, for example, how long and complicated the chains of inference are in the physics of elementary particles; a few clicks per month in an enormous assembly of glass tubes, magnet fields, scintillator fluids and electronic circuits becomes a new 'particle', which in its turn provokes a flurry of theoretical papers and ingenious interpretations. I do not mean to say that the physicists are not correct; but no one can say that all the possible alternative schemes of explanation are carefully checked by innumerable experiments before the discovery is acclaimed and becomes part of the scientific canon. There is far more faith, and reliance upon personal experience and intellectual authority, than the official doctrine will allow.

A simple way of putting it is that the logico-inductive scheme does not leave enough room for genuine scientific error. It is too black and white. Our experience, both as individual scientists and historically, is that we only arrive at partial and incomplete truths; we never achieve the precision and finality that seem required by the definition. Thus, nothing we do in the laboratory or study is 'really' scientific, however honestly we may aspire to the ideal. Surely, it is going too far to have to say, for example, that it was 'unscientific' to continue to believe in Newtonian dynamics as soon as it had been observed and calculated that the rotation of the perihelion of Mercury did not conform to its predictions.

This summary of the various conceptions of science obviously fails to do justice to the vast and subtle literature on the subject. If I have emphasized the objections to each point of view, this is merely to indicate that none of the definitions is entirely satisfactory. Most practicing scientists, and most people generally, take up one or other of the attitudes that I have sketched, according to the degree of their intellectual sophistication—but without fervour. One can be zealous for Science, and a splendidly successful research worker, without pretending to a clear and certain notion of what Science really is. In practice it does not seem to matter.

Perhaps this is healthy. A deep interest in theology is not welcome in the average churchgoer, and the ordinary taxpayer should not really concern himself about the nature of sovereignty or the merits of bicameral legislatures. Even though Church and State depend, in the end, upon such abstract matters, we may reasonably leave them to the experts if all goes smoothly. The average scientist will say that he knows from experience and common sense what he is doing, and so long as he is not striking too deeply into the foundations of knowledge he is content to leave the highly technical discussion of the nature of Science to those self-appointed authorities the Philosophers of Science. A rough and ready conventional wisdom will see him through.

Yet in a way this neglect of—even scorn for—the Philosophy of Science by professional scientists is strange. They are, after all, engaged in a very difficult, rather abstract, highly intellectual activity and need all the guidance they can from general theory. We may agree that the general principles may not in practice be very helpful, but we might have thought that at least they would be taught to young scientists in training, just as medical students are taught Physiology and budding administrators were once encouraged to acquaint themselves with Plato's *Republic*. When the student graduates and goes into a laboratory, how will he know what to do to make scientific discoveries if he has not been taught the distinction between a scientific theory and a non-scientific one? Making all allowances for the initial prejudice of scientists against speculative philosophy, and for the outmoded assumption that certain general ideas would communicate themselves to the educated and cultured man without specific instruction, I find this an odd and significant phenomenon.

The fact is that scientific investigation, as distinct from the theoretical *content* of any given branch of science, is a practical art. It is not learnt out of books, but by imitation and experience. Research workers are trained by apprenticeship, by working for their Ph.D.'s under the supervision of more experienced scholars, not by attending courses in the metaphysics of physics. The graduate student is given his 'problem': 'You might have a look at the effect of pressure on the band structure of the III–V compounds; I don't think it has been done yet, and it would be interesting to see whether it fits into the pseudopotential theory'. Then, with considerable help, encouragement and criticism, he sets up his apparatus, makes his measurements, performs his calculations, etc. and in due course writes a thesis and is accounted a qualified professional. But notice that he will not at any time have been made to study formal logic, nor will he be expected to defend his thesis in a step by step deductive procedure. His examiners may ask him why he had made some particular assertion in the course of his argument, or they may enquire as to the reliability of some particular measurement. They may even ask him to assess the value of the 'contribution' he has made to the subject as a whole. But they will not ask him to give any opinion as to whether Physics is ultimately *true*, or whether he is justified now in believing in an external world, or in what sense a theory is verified by the observation of favourable instances. The examiners will assume that the candidate shares with them the common language and principles of their discipline. No scientist really doubts that theories are verified by observation, any more than a Common Law judge hesitates to rule that hearsay evidence is inadmissible.

What one finds in practice is that scientific argument, written or spoken, is not very complex or logically precise. The terms and concepts that are used may be extremely subtle and technical, but they are put together in quite simple logical forms, with expressed or implied relations as the machinery of deduction. It is very seldom that one uses the more sophisticated types of proof used in Mathematics, such as asserting a proposition by proving that its negation implies a contradiction. Of course actual mathematical or numerical analysis of data may carry the deduction through many steps, but

the symbolic machinery of algebra and the electronic circuits of the computer are then relied on to keep the argument straight.* In my own experience, one more often detects elementary *non sequiturs* in the verbal reasoning than actual mathematical mistakes in the calculations that accompany them. This is not said to disparage the intellectual powers of scientists; I mean simply that the reasoning used in scientific papers is not very different from what we should use in an everyday careful discussion of an everyday problem.

This is a point to which we shall return in a later chapter. It is made here to emphasize the inadequacy of the 'logico-inductive' metaphysic of Science. How can this be correct, when few scientists are interested in or understand it, and none ever uses it explicitly in his work? But then if Science is distinguished from other intellectual disciplines neither by a particular style or argument nor by a definable subject matter, what is it?

The answer proposed in this essay is suggested by its title: *Science Is Public Knowledge*. This is, of course, a very cryptic definition, with almost the suggestion of a play upon words.† What I mean is something along the following lines. Science is not merely *published* knowledge or information. Anyone may make an observation, or conceive a hypothesis, and, if he has the financial means, get it printed and distributed for other persons to read. Scientific knowledge is more than this. Its facts and theories must survive a period of critical study and testing by other competent and disinterested individuals, and must have been found so persuasive that they are almost universally accepted. The objective of Science is not just to acquire information nor to utter all noncontradictory notions; its goal is a *consensus* of rational opinion over the widest possible field.

In a sense, this is so obvious and well-known that it scarcely needs saying. Most educated and informed people agree that Science is true, and therefore impossible to gainsay. But I assert my definition much more positively; this is the basic principle upon which Science is founded. It is not a subsidiary consequence of the 'Scientific Method'; it *is* the scientific method itself.

The defect of the conventional philosophical approach to Science is that it considers only two terms in the equation. The scientist is seen as an individual, pursuing a somewhat one-sided dialogue

* This point I owe to Professor Körner.

† There is also, unfortunately, the hint of an antithesis to *Personal Knowledge*, the title of Polanyi's book to which I have already referred. No antagonism is meant. Polanyi goes a long way along the path I follow, and is one of the few writers on Science who have seen the social relations between scientists as a key factor in its nature.

with taciturn Nature. *He* observes phenomena, notices regularities, arrives at generalizations, deduces consequences, etc. and eventually, Hey Presto! a Law of Nature springs into being. But it is not like that at all. The scientific enterprise is corporate. It is not merely, in Newton's incomparable phrase, that one stands on the shoulders of giants, and hence can see a little farther. Every scientist sees through his own eyes—and also through the eyes of his predecessors and colleagues. It is never one individual that goes through all the steps in the logico-inductive chain; it is a group of individuals, dividing their labour but continuously and jealously checking each other's contributions. The cliché of scientific prose betrays itself 'Hence we arrive at the conclusion that . . .' The audience to which scientific publications are addressed is not passive; by its cheering or booing, its bouquets or brickbats, it actively controls the substance of the communications that it receives.

In other words, scientific research is a social activity. Technology, Art and Religion are perhaps possible for Robinson Crusoe, but Law and Science are not. To understand the nature of Science, we must look at the way in which scientists behave towards one another, how they are organized and how information passes between them. The young scientist does not study formal logic, but he learns by imitation and experience a number of conventions that embody strong social relationships. In the language of Sociology, he learns to play his *role* in a system by which knowledge is acquired, sifted and eventually made public property.

It has, of course, long been recognized that Science is peculiar in its origins to the civilization of Western Europe. The question of the social basis of Science, and its relations to other organizations and institutions of our way of life, is much debated. Is it a consequence of the 'Bourgeois Revolution', or of Protestantism—or what? Does it exist despite the Church and the Universities, or because of them? Why did China, with its immense technological and intellectual resources, not develop the same system? What should be the status of the scientific worker in an advanced society; should he be a paid employee, with a prescribed field of study, or an aristocratic dilettante? How should decisions be taken about expenditure on research? And so on.

These problems, profoundly sociological, historical and political though they may be, are not quite what I have in mind. Only too often the element in the argument that gets the least analysis is the actual institution about which the whole discussion hinges—scientific activity itself. To give a contemporary example, there is much talk nowadays about the importance of creating more effective systems for storing and indexing scientific literature, so that every scientist can very quickly become aware of the relevant work of every other

scientist in his field. This recognizes that publication is important, but the discussion usually betrays an absence of careful thought about the part that conventional systems of scientific communication play in sifting and sorting the material that they handle. Or again, the problem of why Greek Science never finally took off from its brilliant taxying runs is discussed in terms of, say, the aristocratic citizen despising the servile labour of practical experiment, when it might have been due to the absence of just such a communications system between scholars as was provided in the Renaissance by alphabetic printing. The internal sociological analysis of Science itself is a necessary preliminary to the study of the Sociology of Knowledge in the secular world.

The present essay cannot pretend to deal with all such questions. The 'Science of Science' is a vast topic, with many aspects. The very core of so many difficulties is suggested by my present argument—that Science stands in the region where the intellectual, the psychological and the sociological coordinate axes intersect. It is knowledge, therefore intellectual, conceptual and abstract. It is inevitably created by individual men and women, and therefore has a strong psychological aspect. It is public, and therefore moulded and determined by the social relations between individuals. To keep all these aspects in view simultaneously, and to appreciate their hidden connections, is not at all easy.

It has been put to me that one should in fact distinguish carefully between Science as a body of knowledge, Science as what scientists do, and Science as a social institution. This is precisely the sort of distinction that one must *not* make; in the language of geometry, a solid object cannot be reconstructed from its projections upon the separate cartesian planes. By assigning the intellectual aspects of Science to the professional philosophers we make of it an arid exercise in logic; by allowing the psychologists to take possession of the personal dimension we overemphasize the mysteries of 'creativity' at the expense of rationality and the critical power of well-ordered argument; if the social aspects are handed over to the sociologists, we get a description of research as an N-person game, with prestige points for stakes and priority claims as trumps. The problem has been to discover a unifying principle for Science in all its aspects. The recognition that scientific knowledge must be public and *consensible* (to coin a necessary word) allows one to trace out the complex inner relationships between its various facets. Before one can distinguish and discuss separately the philosophical, psychological or sociological dimension of Science, one must somehow have succeeded in characterizing it as a whole.*

* 'Hence a true philosophy of science must be a philosophy of scientists and laboratories as well as one of waves, particles and symbols.' Patrick Meredith in *Instruments of Communication*, p. 40.

In an ordinary work of Science one does well not to dwell too long on the hypothesis that is being tested, trying to define and describe it in advance of reporting the results of the experiments or calculations that are supposed to verify or negate it. The results themselves indicate the nature of the hypothesis, its scope and limitations. The present essay is organized in the same manner. Having sketched a point of view in this chapter, I propose to turn the discussion to a number of particular topics that I think can be better understood when seen from this new angle. To give a semblance of order to the argument, the various subjects have been arranged according to whether they are primarily *intellectual*—as, for example, some attempt to discriminate between scientific and non-scientific disciplines; *psychological*—e.g. the role of education, the significance of scientific creativity; *sociological*—the structure of the scientific community and the institutions by which it maintains scientific standards and procedures. Beyond this classification, the succession of topics is likely to be pretty haphazard; or, as the good lady said, 'How do I know what I think until I have heard what I have to say?'

The subject is indeed endless. As pointed out in the Preface, the present brief essay is meant only as an exposition of a general theory, which will be applied to a variety of more specific instances in a larger work. The topics discussed here are chosen, therefore, solely to exemplify the main argument, and are not meant to comprehend the whole field. In many cases, also, the discussion has been kept abstract and schematic, to avoid great marshlands of detail. The reader is begged, once more, to forgive the inaccuracies and imprecisions inevitable in such an account, and to concentrate his critical attention upon the validity of the general principle and its power of explaining how things really are.

SCIENCE AND NON-SCIENCE

In this chapter Science will be considered mainly in its intellectual aspects, as a system of ideas, as a compilation of abstract knowledge. The first question to be answered has already been posed in the introductory chapter: what distinguishes Science from its sister 'Faculties'—Law, Philosophy, Technology, etc.? The argument is that Science is unique in striving for, and insisting on, a consensus.

Take Law, for example. We all feel that legal thought is quite different from scientific thought—but what is the basis of this intuition? There are many ways in which legal argument is very close to Science. There is undoubtedly an attempt to make every judgement follow logically on statutes and precedents. Every lawyer

seeks to clarify a path of implications through successive stages to validate his case. The judge reasons it out, on the basis of universal principles of equity, in the effort to arrive at a decision that will command the assent of all just and learned men.

The kinship of Law with the mathematical sciences is emphasized by the interesting suggestion that legal decisions might be arrived at automatically by a computer, into which all the conditions and precedents of the case would be fed and a purely mechanical process of logical reduction would produce exactly the correct judgement.* Although perhaps the idea is somewhat fanciful, if this procedure were technically feasible it would provide decisions that could not but command the assent of all lawyers—just as a table of values of a mathematical function printed out by a computer commands the assent of all mathematicians. To the extent, therefore, that the Law is strictly logical, it can be made 'scientific'.

Again, in the concept of 'evidence' there is close similarity. This is too primary and basic an idea to be defined readily, but, roughly speaking, it means 'any information that is relevant to a disputed hypothesis'. In Science, as in Law, we are almost always dealing with theories that are disputable, and that can only be challenged by an appeal to evidence for and against them. It is the duty of scientists, as of lawyers, to bring out this evidence, on both sides, to the full.

In the end, the case may hang upon some very minor item of information—was the man who got off the 3.57 at Little Puddlecome on Monday, 27 May, wearing a black hat? A scientific theory also may be validated by some tiny fact—for example, the almost imperceptible changes in the orbit of the planet Mercury. The question of the *credibilty* of evidence can become very important. We may find everyone in full agreement that, if a fact is as stated by a witness, it has vital logical implications for the hypothesis under consideration; yet the court may be completely undecided as to whether this evidence is true or not. The existence of honest error has to be allowed for. This sort of thing happens in Science too, though it does not usually get remembered in the conventional histories. For example, many scientists will recall the interest that was aroused by the publication of evidence for organic compounds in meteorites—probably an erroneous interpretation of a complex observation, but of the most profound significance if it proved to be true. In such cases there may even be questions about the relative reliability, in general, of two different observers—an assessment, perhaps, based upon their scientific standing and expert authority—just as the rela-

* I am indebted to Professor Julius Stone for sending me his fascinating critical essay on this subject.

tive veracity of conflicting witnesses may become the key issue in a legal case.

But, of course, in Science, when the evidence is conflicting, we withhold our assent or dissent, and do the experiment again. This cannot be done in legal disputes, which must be terminated yea or nay. If we are forced to a premature opinion on a scientific question, we are bound to give the Scottish verdict *Not Proven*, or say that the jury have disagreed, and a new trial is needed. In Criminal Law, where the case for the prosecution must be proved up to the hilt, or the accused acquitted, this is well enough; but Civil Law demands a decision, however difficult the case.

The Law is thus unscientific because it *must* decide upon matters which are not at all amenable to a consensus of opinion. Indeed, legal argument is concerned with the conflict between various principles, statutes, precedents, etc.; if there were not an area of uncertainty and contradiction, then there would be no need to go to law about it and get the verdict of the learned judge. In Science, too, we are necessarily interested in those questions that are not automatically resolved by the known 'Laws of Nature' (the analogy here with man-made Laws is only of historical interest) but we agree to work and wait until we can arrive at an interpretation or explanation that is satisfactory to all parties.

There are other elements in the Law that are quite outside science—normative principles and moral issues that underlie any notion of justice. As is so often said, Science cannot tell us what *ought* to be done; it can only chart the consequences of what *might* be done.

Normative and moral principles cannot, by definition, be embraced in a consensus; to assert that one *ought* to do so and so is to admit that some people, at least, will not freely recognize the absolute necessity of not doing otherwise. Legal principles and norms are neither eternal nor universal; they are attached to the local, ephemeral situation of this country here and now; their arbitrariness can never be mended by any amount of further logical manipulation. Thus, there are components of legal argument that are necessarily refractory to the achievement of free and general agreement and these quite clearly discriminate between Law and Science as academic Faculties.

To the ordinary Natural Scientist this discussion may perhaps have seemed quite unnecessary—Law, he would say, is a man-made set of social conventions, whilst Science deals with material, objective, eternal verities. But to the Social Scientist this distinction is by no means so clear. He may, for example, find it impossible to disentangle such legal concepts as personal responsibility from his

scientific understanding of the power of social determination in a pattern of delinquency. The criterion of consensibility might temper some of the scientific arrogance of the expert witness—'Would *every* criminologist agree with you on this point, Dr. X?'—whilst at the same time throwing the full weight of personal decision and responsibility upon the judge, who should never be allowed to shelter behind the cruel and mechanical absolutes of 'Legal Science'. The intellectual authority of Science is such that it must not be wielded incautiously or irresponsibly.

At first sight, one would not suppose that much need be said about what distinguishes Science from those disciplines and activities that belong to the Arts and Humanities—Literature, Music, Fine Arts, etc. Our modern view of Poetry, say, is that it is an expression of a private personal opinion. By his skill the poet may strike unsuspected chords of emotion in a vast number of other men, but this is not necessarily his major intention. A poem that is immediately acceptable and agreeable to everyone must be banal in the extreme.

But, of course Arts dons do not write Poetry: they write about it. Literary and artistic critics do sometimes pretend that their judgements are so convincing that it is wilful to oppose them. An imperious temper demands that we accept their every utterance of interpretation and valuation. Fortunately, we have the right of dissent and if our heart and mind carry us along a different path we have no need to be frightened by their shrill cries of contempt.

The point here is that there are genuine differences of taste and feeling, just as there are genuine differences of moral principle. At the back of our definition of Science itself is the assumption that men are free to express their true feelings; without this condition, the notion of a consensus loses meaning. Under a dictatorship we might be constrained to pay lip service to a uniform standard of style or taste, but this is the death of criticism.

There are, of course, periods of 'classicism' and 'academicism' when some style or techique is overwhelmingly praised and practised, but no one supposes that this is in obedience to the commands of absolute necessity. The attempts of the stupider sort of academic critic to rationalize the taste of his age by rules of 'harmony' or 'dramatic unity' are invariably by-products of the fashion whose dominance he is seeking to justify, not its determining factors. No sooner are such rules formulated than a great artist cannot resist the temptation to break them, and a new fashion sweeps the land. By their very nature, the Arts are not consensible, and hence are quite distinct from Science as I conceive it.

Science is not immune from fashion—a sure sign of its socio-psychological nature. We shall return to some of the symptoms of this disease later. But what, abstractly, *is* fashion? It means doing what other people do for no better reason than that that is what is done. If everyone were to follow only fashionable lines of thought, there would be a false impression of a consensus; the inhibition of the critical imagination by such a conformist sentiment is the antithesis of the scientific attitude. It is also, of course, another way of death for true Poetry and Art.

But the products and producers of Literature, Art and Music may be studied in more factual aspects than for their emotional or spiritual message. For example, they are the outcome of, or participants in, historical events.

The place of *History* in this analysis is very significant; it seems to be truly one of the borderlands marching between scientific and non-scientific pursuits. Suppose that we are investigating such a problem as the date and place of birth of a writer or statesman. We search in libraries and other collections of material documents for written evidence. From various oblique references we might build up an argument in favour of some particular hypothesis—an argument to persuade our colleagues by its invincible logic that no other interpretation is tenable. This procedure seems quite as scientific as the research of a palaeontologist, who might reconstruct the anatomy of an extinct animal by piecing together fragments of fossil bone. Our aim is the same—to make a thoroughly convincing case which no reasonable person can refute. If, unfortunately, we cannot find sufficient evidence to clinch the case, we do not cling to our hypothesis and abuse our opponents for not accepting it; we quietly concede that the matter is uncertain, and return once more to the search. On such material points, the mood of historical scholarship is perfectly scientific.

The other mood in History is much more akin to Literature or Theology; it is the attempt to understand human history imaginatively and to 'explain' it. Having ascertained the 'facts', the historian tries to uncover the hidden motives and forces at work, just as the scientist goes behind the phenomena to the laws of their being.

The trouble is that the complex events of history can seldom be explained convincingly in the language of elementary cause and effect. To ascribe the English Civil War, for example, to the 'Rise of the Gentry' may be a brilliant and fruitful hypothesis, but it is almost impossible to prove. Even though one may feel that this is the essence of the matter, and though one may marshal factual evidence forcefully in its favour, the case can be no more than circumstantial and hedged with vagueness and provisos. It will go into the canon of interesting historical theories, but experience tells us that it will not, as would a valid scientific theory, be so generally acceptable as to eliminate all competitors.

The rule in Science is not to attempt explanations of such complex phenomena at all, or at least to postpone this enterprise until answers are capable of being agreed upon. Imagination in the search for such problems is essential, but speculation is always kept rigidly under control. Even in such disciplines as Cosmology, where it sometimes seems as if a new theory of the Universe is promulgated each week, the range of discussion is limited quite narrowly to model systems whose mathematical properties are calculable and can be critically assessed by other scholars.

History does not impose such restrictions upon its pronouncements. It is felt, quite naturally, that the larger questions, although more difficult, are very important and must be discussed, even if they cannot be answered with precision. To restrict oneself to decidable propositions would be to miss the lessons that the strange sad story has for mankind. A history of 'facts', of dates and kings and queens, although acceptable to the consensus, would be banal and trivial. In other words, History also has to provide other spiritual values, and to satisfy other normative principles, than scientific accuracy.

There are, of course, historians who have claimed universal 'scientific' validity for their larger schemes and 'Laws'. It is not inconceivable that historical events do follow discernible patterns, and that there are, indeed, hidden forces—the class struggle, say, or the Protestant ethic—which largely determine the outcome of human affairs. It would not be necessary for such a theory to be absolute and mathematically rigorous for it to acquire scientific validity, any more than the proof that smoking causes lung cancer requires every smoker to die at the age of 50. It is not inherently absurd to search for historical laws, any more than it was absurd, 200 years ago, to search for the laws governing smallpox. Seemingly haphazard events often turn out to have their pattern, and to be capable of rational explanation.

All I am saying is that no substantial general principles of historical explanation have yet won universal acceptance. There have been fashionable doctrines, and dogmas backed by naked force, but never the sort of consensus of free and well-informed scholars that we ordinarily find in the Natural Sciences. Many historians assert that historical events are the outcome of such a variety of chance causes that they could never be subsumed to simpler, more general laws. Others say that the number of instances of exactly similar situations is always too small to provide sufficient statistical evidence to support an abstract theoretical analysis.

Whatever their reasons, historians do not agree on the general theoretical foundations or methodology of their studies. Instead of establishing, by mutual criticism and tacit cooperation, a limited common basis of acceptable theory, from which to build upwards and outwards, they often feel bound to set up antagonistic 'schools' of interpretation, like so many independent walled cities.

They are not to be blamed for such behaviour; it only shows that this is a field where a scientific consensus is not the main objective. If you insisted that historians should work more closely together, they would object that the knowledge that they have in common is too dull, too trivial, too distant from the interesting problems of History, to circumscribe the thought of a serious scholar. To write about the Civil War without asking why the whole extraordinary thing happened is to compose a mere chronicle. For that reason, much of historical scholarship is not essentially scientific.

It would be wrong, on the other hand, to give the approving label 'Science' only to the new techniques of historical research derived from the physical and biological sciences and technologies—carbon dating, aerial photography, demographic statistics, chemical analysis of ink and parchment. Such techniques are often powerful, but they are not more 'scientific' than the traditional scholarly exercises of editing texts, verifying references and making rational deductions from the written words of documents. There is no reason at all why marks on paper in comprehensible language should be treated as inherently less evidential than the pointer readings of instruments or the print-out from a computer. In German the word *Wissenschaft*, which we translate as *Science*, includes quite generally all the branches of scholarship, including literary and historical studies.

To maintain, therefore, an impassable divide between Science and the Humanities is to perpetrate a gross misunderstanding that springs in the British case solely from a peculiarity of educational curricula. The Story, the Arts, the Poetry of Mankind are worthy both of spiritual contemplation and scholarly study, whether by laymen in general education or by experts as their life career. In many aspects this study is perfectly akin to the scientific study of electrons, molecules, cells, organisms or social systems: consensible knowledge may be acquired whether as isolated facts or as generally valid explanations. But to confine oneself, in education as in scholarship, to such aspects would impoverish the imagination, and even restrict the scope of possible further advance. Without general concepts as a guide—however uncertain, personal and provisional—we simply could not see any larger patterns in the picture. Historical and literary scholarship cannot therefore pretend to be scientific through and through, but that does not prevent their making progress towards a closer definition of the truth. In the end, bold speculative generalization and unverifiable psychological insight may go further in establishing a convincing narrative than a rigid insistence on precise minutiae.

It scarcely needs to be said that *Religion*, as we nowadays study and practise it, is also quite distinct from Science. This seems so obvious in our enlightened age that one wonders how there could have been any conflict and confusion between them. But was not Religion primitive Science—the corpus of generally accepted public knowledge? Should we not see Science as growing out of, and eventually severing itself from, this parent body—or perhaps as a process of differentiation and specialization within the unity of the medieval *Summum*? Just because many religious beliefs are now seen to be wrong, it does not follow that they were not seriously, freely and rationally accepted in their time. Conventional science too can be wrong at times.

Let me give an example. In the late eighteenth and early nineteenth centuries, prehistoric remains were found that we now see as pointing to the great antiquity of Man. But many scholars stood out against this interpretation because it did not square with the Biblical chronology of the past. Is it fair to treat this as a conflict between scientific rationality and religious prejudice? Would it not be more just to say that a widely accepted theory was being ousted by a better one as new evidence came to light?

The point is that this debate was open and free. The participants on one side may have been blinkered by their upbringing, but their beliefs were honestly held and rationally maintained. They may often have used poor arguments to defend their case—but they did not call in the secular arm or the secret police. In the end, they lost; and since then the appeal to Divine Scripture has ceased to be an acceptable element in scientific discussion.

What I am arguing is that there is a progressive improvement in the techniques and criteria of such discussion, and that the use of abstract theological principles was once respectable but is now discredited, just as the absolute justification of Euclidean geometry from the Parallelism Axiom is now discredited. The 'Scientific Revolution' of the seventeenth century is not a complete break with the past. The idea of presenting a rational non-contradictory account of the universe is perhaps a legacy of Greece, but it is very strong in medieval Philosophy and Theology. It may be that the very existence of a dogmatic system of metaphysics, implying a rational order of things and fiercely debated in detail, was the prerequisite for the development of an alternative system, using some of the same logical techniques but based upon different principles and more extensive evidence.* The doctrinaire consensus of the Church may have been prolonged beyond its acceptability to free men by the power of the Holy Office, but it had originally provided an example of a generally agreed picture of the world. These are subtle and deep questions which I am not competent to discuss, but I wonder whether the failure of Science to grow in China and India was due as much to the general doctrinal permissiveness of their religious systems as to any other cause. Toleration of deviation, and the lack of a very sharp tradition of logical debate may have made the very idea of a consensus of opinion on the Philosophy of Nature as absurd to them as the idea of absolute agreement on ethical principles would be to us.

The relationship between Science and *Philosophy* is altogether more complex and confused. In a sense, all of modern science is the Philosophy of Nature, as distinct from, say, Moral or Political Philosophy. But this terminology is somewhat old-fashioned, and we try to make a distinction between Physics and Metaphysics, between the Philosophy *of* Science and Philosophy *as* Science. Some philosophers attempt to limit themselves to statements as precise and verifiable as those of scientists, and confine their arguments to the rigid categories of symbolic logic. The consensus criterion would be acceptable to them, for they would hold that by a continuous process of analysis and criticism they would make progress towards creating a generally agreed set of principles governing the use of words and the establishment of valid truths. Others hold that such a hope can never be realized and that by limiting philosophical discourse in this way they would only allow themselves to make trivial statements, however unexceptionable. For this school of Philosophy it is important to be free to comment on grander topics, even though such comments will only reveal the variety and contradictory character of the views of different philosophers.

As with History we can only say that if Philosophy is what academic philosophers write in their books, then some of it is not very different from Science. But generally the motivation is nonscientific, by our definition, and the multiplicity of viewpoints indicates that there is no dominant urge to find maximum regions of agreement. Whatever their claims, the proponents of 'scientific' philosophical systems do not convince the majority of their colleagues that theirs is the only way to truth.

* This point is made in *Science in the Modern World* by A. N. Whitehead (New York: Macmillan, 1931).

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Let us now consider *Technology*—Engineering, Medicine, etc. For the multitude, Science is almost synonymous with its applications, whereas scientists themselves are very careful to stress the distinction between 'pure' knowledge, studied 'for its own sake', and technological knowledge applied to human ends.

The trouble is that this distinction is very difficult to make in practice. Suppose, for example, that we are researching on the phenomenon of 'fatigue' in metals. We are almost forced into the position of saying that on Monday, Wednesday and Friday we are just honest seekers after truth, adding to our understanding of the natural world, etc., whilst on Tuesday, Thursday and Saturday we are practical chaps trying to stop aeroplanes from falling to pieces, advancing the material welfare of mankind and so on. Or we may have to make snobbish distinctions between Box, a pure scientist working in a University, and Cox, a technologist, doing the same research but employed by an aircraft manufacturer. There was once a time when Science was academic and useless and Technology was a practical art, but now they are so interfused that one is not surprised that the multitude cannot tell them apart.

Here again, a definition in terms of the scientific consensus can be really effective. The technologist has to fulfil a need; he must provide the means to do a definite job—bridge this river, cure this disease, make better beer. He must do the best he can with the knowledge available. That knowledge is almost always inadequate for him to calculate the ideal solution to his problem—and he cannot wait while all the research is done to obtain it. The bridge must be built this year; the patient must be saved today; the brewery will go bankrupt if its product is not improved.

So there will be a large element of the incalculable, of sheer art, in what he does. A different engineer would come out with quite a different design; a different doctor would prescribe quite different treatment. These might be better or worse, in their results—but nobody quite knows. Each situation is so complex, and has so many unassessable factors, that the only sensible policy for the client is to choose his engineer or doctor carefully and then rely upon his skill and experience. To look for a solution acceptable to all the professional experts is a familiar recipe for disaster—'Design by Committee.'

The technologist's prime responsibility is towards his employer, his customer or his patient, not to his professional peers. His task is to solve the problem in hand, not to address himself to the opinions of the other experts. If his proposed solution is successful, then it may well establish a lead, and eventually add to the 'Science' of his Technology; but that should not be in his mind at the outset.

What we find, of course, is that a corpus of generally accepted principles develops in every technical field. Modern Technology is deliberately scientific, in that there is continuous formal study and empirical investigation of aspects of technique, in addition to the mere accumulation of experience from successfully accomplished tasks. The aim of such research is not to solve immediate specific problems, but to acquire knowledge for the use of the experts in their professional work. It is directed, therefore, at the mind of the profession, as a potential contribution to the consensus opinion. This sort of work is thus genuinely scientific, however trivial and limited its scope may be.

The abstract distinction here being made between a 'scientific Technology' and 'technological Science' has its psychological counterpart. It is a commonplace in the literature on the Management of Industrial Research that applied scientists often suffer from divided loyalties. On the one hand, they owe their living to the company that employs them, and that expects its return in the profitable solution of immediate problems. On the other hand, they give their intellectual allegiance to their scientific profession—to Colloid Chemistry, or Applied Mathematics, or whatever it is—where they look for scholarly recognition. Although the rewards for 'technological' work are greater and more direct, they very often prefer to stick to their 'scientific' research.

This preference seems almost incomprehensible to management experts, because they fail to see that the scientific loyalty is not just towards a prestigious professional group but to an ideology. The young scientist is trained to make contributions to public knowledge. All the habits and practices of his years of apprenticeship emphasize the importance of making them convincing, and thus making them part of the common pool. Being a successful scientist is not just winning prizes; it is having other scientists cite your work. To give this up is worse than losing caste; it is to give up one's faith and be made to worship foreign idols.

Nevertheless, one must agree that Science and Technology are now so intimately mingled that the distinction can become rather pedantic. Take, for example, a typical Consumers Association report on a motor car. Some of the tests, such as the measure of petrol consumption, may be perfectly scientific in that their validity would be universally acceptable. Other tests, such as whether the springing was comfortable, would not satisfy this criterion, although it would be one of the important skills of the designer to attend to just such 'subjective' and 'qualitative' features. For this reason, to say

that a car has been 'scientifically designed' is merely to assert that it has been well designed by competent engineers. Yet an account, by the designer, of the rationale behind various technical features of the model could rank as a serious contribution to the Science of Automobile Engineering by adding to the body of agreed principles at the basis of that mysterious art.

All that I can claim is that these distinctions, although subtle and perhaps pedantic, are not entirely arbitrary or unreal. We do not need to look far ahead to some conceivable remote application of the knowledge in question, nor do we need to examine the hidden, perhaps unconscious, motives of those who produce it. We do not need to decide whether some particular laboratory is 'technological' or 'scientific', and then attach the appropriate label to its products. The criterion is in the work itself, in the form in which it is presented, and in the audience to which it is addressed.

What are we to make of the so-called 'Social Sciences' in the light of this discussion? It is obvious that such a subject as Politics is very close to History and to Philosophy in its goals and achievements; to stick to ascertainable public 'facts' is to limit the discourse to the banal. To give this discipline the name of Political Science is unfortunate; it offers more than it can deliver and debases its ethical message.

On the other hand, *Economics* is a very technological subject; the experts are always being asked to diagnose the ills of the nation and to propose specific cures, long before they have sufficient scientific understanding to make a valid analysis. Yet the totally quantitative material medium—money—allows of convincing proofs, statistical or algebraic, of precise hypotheses, so that a body of agreed principles is gradually emerging. Leading economists may debate in public, and seem to be at loggerheads, but behind the scenes they teach much the same things to their students. It is typical of the tacit cooperation between scholars in a scientific subject that American and Soviet economists respect and learn from each other's work on Input-Output Analysis, however much they may disagree on more speculative issues of general social policy.

These are the neighbours of the new discipline of *Sociology* which is an attempt to escape from the 'unscientific' traditions of History and Politics, and to make the study of social systems, and of man in society, at least as scientific as Economics.

That is the reason why so much sociological research is by questionnaire and statistical analysis; the aim is to provide the necessary factual basis for firmly scientific theories. To the extent that observations of this sort are verifiable by repetition, and capable of being made quite convincing to a critical public, this attitude is sound. But the intractability of the subject must be reckoned with. Vast quantities of information do not add up to much serious knowledge without theories to give it meaning. Moreover, even to accept 'facts' of this sort may imply the acceptance of dubious hidden theories. Suppose, for example, that our car-testing organization decided to assess the comfort of various models by asking a hundred people to give their views, and reported that car A 'rated' 87 per cent and car B only 63 per cent. This is objective information, which might well be 'verifiable'. But there is the implication that it 'measures' something—'comfort'—which may not exist at all. This is a crude case, but sociological research is full of more subtle examples of the same difficulty.

Some sociologists have taken quite a different line. They deal in abstract categories, which they manipulate logically into various hypothetical relations in a sort of formal calculus. This approach also strives towards the creation of a consensus, in that the structure of the argument can be purged of contradictions and hence made unexceptionable and theoretically acceptable. But without much more rigorous connection of these abstractions with real systems and actual phenomena it is vacuous knowledge, without the power to persuade us that thus and thus is the world of men.

Nevertheless, Sociology is often genuinely scientific in spirit, although it has turned out to be an exceedingly difficult science whose positive achievements do not always match the effort expended on it. The 'methodological problem' has not been surmounted; there is not yet a reliable procedure for building up interesting hypotheses that can be made sufficiently plausible to a sufficient number of other scholars by well-devised observations, experiments or rational deductions. It was the sort of problem facing Physics before Galileo began seriously to apply mathematical reasoning and numerical measurement to the subject. The ideal of a consensus is there, but the intellectual techniques by which it might be created and enlarged seem elusive.

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This survey of the Faculties has necessarily been brief and schematic. Why should we even want to decide whether a particular discipline is scientific or not? The answer is, simply, that, *when it is available,* scientific knowledge is more reliable, on the whole, than non-scientific. When there are conflicts of authority, when Sociolgy tells us to go one way and History another, we need to weigh their respective claims to validity. Our general argument here is that

in a discipline where there is a scientific consensus the amount of *certain* knowledge may be limited, but it will be honestly labelled: 'Trust your neck to this', or 'This ladder was built by a famous scholar, but no one else has been able to climb it'.

In the end, the best way to decide whether a particular body of knowledge is scientific or not is often to study the attitudes of its professional practitioners to one another's work. A sure symptom of non-science is personal abuse and intolerance of the views of one scholar by another. The existence of irreconcilable 'schools' of thought is familiar in such academic realms as Theology, Philosophy, Literature and History. When we find them in a 'scientific' discipline, we should be on our guard.

This is the reason why for example we should be very suspicious of the claims of Psychoanalysis. The history of this subject is a continuous series of bitter conflicts between persons, schools and theories. Freud himself had the most honest and sincere desire to create a thoroughly respectable scientific discipline, but for some reason he failed to understand this key point—the need to move slowly forward, step by step, from a basis of generally accepted ideas. Perhaps the struggle to get anyone to listen at all was too bitter, or perhaps his mind was too active and impatient to endure continuous critical assessment of each new theory or interpretation. Whatever the reason, the mood of Psychoanalysis in its formative period was antagonistic to the covert cooperative spirit of true Science. Its clinical successes were only of technological significance, and did not scientifically validate the theories on which they were said to be based.

I have given this example, not out of prejudice against psychoanalytic ideas (one or other of the contending schools may well be right: we shall see) but to show that the principle of the consensus is a powerful criterion, with something definite to say on this vexed topic. To some people the words 'scientific' and 'unscientific' have come to mean no more than 'true' and 'false', or 'rational' and 'irrational'. In this chapter I have tried to show, by reference to other organized bodies of knowledge, that this usage is quite improper, and grossly unfair to those scholars who seek rationality and truth in bolder ways than by microscopic dissection of minutiae.