BULLETIN
OF THE
BRITISH SOCIETY
FOR THE
HISTORY OF SCIENCE

VOL. 2 JULY, 1957 No. 17

Presidential Address

The Dependence of Science on its History

Delivered by PROFESSOR HERBERT DINGLE *on 6th May*, 1957.

(Published September 1958)

A Presidential Address, which the wisdom or otherwise of several generations has imposed on scientific societies as a recurring element in their experience, is, I suppose, an occasion when the holder for the time being of the leading office in the society should give his views on subjects of more than topical interest. Ideally he should direct the attention of his flock to the ultimate purpose of their existence, as he sees it, and recall their thoughts from the details of particular, and often apparently unrelated, problems to the underlying unity which is the basic justification for considering them. But, like all ideals, this must in practice be tempered by other demands. The prospect of an unending succession of Presidents, reciting in turn the platitudes which all statements of principles, however lofty, must in such circumstances become, is one which would effectively ensure the shortest of half-lives for the attendance at Annual Meetings and make more and more difficult the capture of a victim to be burdened with this responsibility for the ensuing period. Some compromise between the most general and the most special must be found, and that must be determined by the peculiar interests of the President concerned. He may indulge those interests to the full, but he must bring them into relation with the general purposes of the Society and so exhibit them as aspects of a larger whole. This may be irksome, but it is necessary, and in the long run will be for his own benefit as well as for that of his audience.

It is easy for me to make these sententious remarks because it happens that my peculiar interests are also the most general ones. The study of the history of science may be justified on various grounds. It has, in the first place, the same title to our respect as the study of any history ; namely, that the knowledge of what has happened in the past, merely because it has happened, is a necessary part of a liberal education. Normal human beings are inevitably curious, as much about what was and what will be as about what is, and whether or not the study of the past is necessary to the understanding of the present and the determination of the future—to that I shall return presently—no one who is entirely without desire to know what happened before he arrived on the scene can be regarded as completely human. He is just as truly defective as though he were colour-blind or tone-deaf or lacking in the normal number of limbs. In studying history, therefore, we are, at the very minimum, exhibiting the natural behaviour of members of our species.

But there is a more particular reason which is frequently advanced for studying the history of science as distinct from general history. The development of our educational system has resulted in a division between what are often called the sciences and the humanities. That this division is artificial is shown clearly enough by the fact that it did not exist in the Middle Ages. The universe was then in fact a *universe*, and philosophy embraced equally what we now call the scientific and the humanistic. But, artificial or not, it is a fact, and the majority of modern scholars belong to one or other of the two classes, rarely if ever to both. If this were a mere practical necessity, resulting from the enormous bulk of modern knowledge and the limitations of the ordinary human intellect, it would simply have to be accepted, and no great harm would be done : each could recognise the other as his complement and the collective knowledge of humanity could be recognised as more complete than the partial knowledge of the individual.

But that, unfortunately, is not the position. For some reason the study of science is regarded as less exalted than the study of the humanities, and the man who, for instance, disentangles the complexities of the overlapping strata in the Earth's crust is supposed to be concerned with a more debasing occupation than is he who devotes his life to the study of the ancient classics. Surprisingly enough, this view is very prevalent among theologians, who would hardly have been expected to rate the study of the works of God lower than that of the works of pagan man ; but so, in fact, it is. The unmistakable trend of modern life has therefore come with something of a shock to those who look upon the sciences and the humanities in this light, for it is evident that the course of social history in the future is going to be more and more determined by scientific research, the preparation for which must therefore claim a larger and larger share of school curricula and lead to a greatly increased proportion of our graduates qualifying on the scientific side.

In these circumstances the history of science has been envisaged as a tempering influence, preventing the worst excesses of a predominantly scientific education. If our young men and women must needs undergo the degradation of enquiring into the laws of nature, let them learn the history of such an enquiry, for history at least is a humanity. If we cannot wholly prevent investigation of the works of God, we might at least partially transform it into an investigation of what man has made of those works. This argument, though it has not always been expressed so simply, has frequently been heard in recent years, and is, in fact, among the most prominent of current reasons for encouraging the study of the history of science.

I am far from denying the cogency of this argument. The division between the sciences and the humanities is, as I have said, a fact, and that being so, it is not out of place to look for a bridge to span the division. The history of science is, I believe, the best bridge that could be found, and if a practicable way of introducing it into our general educational system could be devised, I do not see that anything but good could result. But there is, I believe, a fundamental error in teaching science and the humanities in such a way that an illusory division between them should ever appear. In the general world-picture which, for practical purposes, we all contemplate today, the external world developed from some primordial form of matter to stars with attendant planets, and on one of these planets a crust formed on which grew first simple and then more complicated forms of life until man appeared. After many thousands of years of evolution he at last discovered a means of making permanent records of his thoughts and activities, and from that time—which was

but a moment ago in the whole course of development—we have what we call the historic period. The study of the process up to that moment is *science* ; thereafter it becomes *history*, one of the humanities, which is supposed to be fundamentally different from science. But why ? I do not know ; but lest I be misunderstood let me say that it is not only humanists who create the false division. Unfortunately the majority of scientists, in this country at least, do so, and fail to see that the study of the history of science—the study, that is, of the behaviour of the men who have created science as it is today—is as much a need of science as of history. In a recent discussion on the desirability of Great Britain joining the International Union for the History of Science, a Fellow of the Royal Society said that he saw not the slightest reason for that body associating itself with the project. He himself had attained scientific distinction through the study of the behaviour of animals in the Zoo.

This leads me to what is, if not the most fundamental reason for studying the history of science, at least the most " scientific " reason, namely, that we cannot understand science itself without studying its history. The history and the philosophy of science are one subject. It is possible to divorce the history from the philosophy of science and study it as a mere record of events, but it is not possible to study the philosophy of science in isolation. Without the history of science it withers away, or becomes transmuted into the multifarious pseudo-philosophies which mistake some temporary theory for a fundamental philosophical principle and distort the whole of experience to fit its requirements. For it is perhaps the most distinguishing characteristic of science that it is a progressive, age-long process of acquiring knowledge, in which each successive generation builds on the work of its predecessors and can accomplish nothing lasting if that work is not soundly based. In this it stands in striking contrast to, I think, every other human activity. In every form of art the work of each creator exists in its own right. The value of Wordsworth does not rest on that of Milton, nor Milton's on Shakespeare's. If the works of Bach were non-existent, those of Beethoven would be no less prized, and the reputation of Rembrandt owes nothing to that of Leonardo. In literary criticism there is the same independence. We read what Aristotle, Longinus, Johnson, Arnold, Sainte-Beuve have to say about the matter, and treat each as though the others had not written. Historians are in like case ; Gibbon, Macaulay, Mommsen, Toynbee, we read them all, with not the slightest thought of testing the views of one by their conformity to those of the others. In theology, George Fox does not build on Martin Luther or John Wesley on George Fox. In non-scientific philosophies there is the same independence ; Platonism, Aristotelianism, Scholasticism—each is complete in itself and you take your choice ; the later in no sense includes and transcends the earlier. But in science it is otherwise. Newton saw further than others because he stood on the shoulders of giants. We, though of lesser stature, see further than Newton because we stand on his shoulders. In science, and in science alone, does each successive worker start from the point at which his predecessors left off and augment the basis on which his successors begin to build.

If you take any scientific publication of the present day—say the current number of the *Proceedings of the Royal Society*—you will find a number of papers and, at the end of each, a list of references to earlier work which the author has used and which he accepts as accurate. You turn up one of the papers so indicated, and you will find that it too has a list of references to still earlier work whose validity is necessary to that of its own conclusions. You

continue the process back until you come to the seventeenth century. Except in pure mathematics you will in general not need to go further, for the science of the present day effectively started at that time ; there was nothing that was known earlier* that cannot be more firmly established by later observations. But from the seventeenth century onwards, science is a gradually ascending structure, everything that is done at any time resting on what has been laid down before and helping to support what has come after. That is the nature of science, and to understand it you must understand the whole. A weakness at any point imperils all that is built on it, and the longer the catastrophe is delayed the greater the fall when it comes. The exposure of the inconsistency of the phlogiston theory caused a moderate upheaval in chemistry : the detection of the misunderstanding of Newtonian mechanics shook physics to its foundations.

The history of science, then, is the history of a more and more rapidly mounting pile of knowledge, in which the strain on the supporting layers grows greater and greater as the pile rises. In the early stages it was possible for the builders themselves to examine the stability of the foundations and assure themselves that their work was well sustained : today that is out of the question. The conditions of modern scientific work are such that anyone who is to stand any chance at all of adding to the building can devote no attention to what has already been done. He must accept it on trust. The result is that, to change the metaphor, we have wandered out into the unknown and lost touch with our base. We push onwards faster and faster, but we have no contact with our source of supplies. And the longer the process goes on the more perilous is our position.

In these circumstances it is clear that some special, *ad hoc* provision for the study of the history of science is necessary, not merely for curiosity or for the completion of the education of the individual, but for the successful prosecution of science itself. It is no longer a frill, an ornament, giving to science the appearance of a kind of culture which it does not itself possess, but a vital necessity, a complement to the forward movement without which that movement can scarcely fail to come to grief. It has become an indispensable condition of the success of all scientific activity ; and not merely of its success, but, so dependent is our present civilisation on the work of scientists, of the avoidance of disasters which it were pleasanter not to try to contemplate. Let our interests be as narrowly scientific as you like ; we cannot avoid the need of a continuous and systematic examination of the work of the past which we necessarily use and which, without the understanding which comes from such an examination, becomes an arbitrary and all too probably a mistaken presupposition. It is the duty of scientists, for the sake of their own calling, to see that the work which it is now humanly impossible for the seekers after new knowledge to undertake, is provided for by the activities of others with whom they can work as co-equals, as labourers towards the same end of both understanding and utilising the laws of the natural world.

It is not hard to find examples. I choose one—with some hesitation since it is still the subject of controversy, but with no doubt that it is the most suitable because of its immense potential importance. A discussion has recently arisen concerning the requirements of the theory of relativity in the matter of a return journey into outer space at high velocity. It has been said by some, and contradicted by others, that the theory of relativity requires that a traveller who makes such a journey will have " aged " less on his return than

* Apart, of course, from unique occurrences, such as outbreaks of new stars.

https://doi.org/10.1017/S0950563600001391 Published online by Cambridge University Press

those remaining at home. In a rather extreme case, if the traveller were one member of a newly-born pair of twins, he might return still a baby and find his brother an old man. The question, be it noted, is not whether this would actually happen : that is a matter which could be finally settled only by making the actual experiment. The question is whether Einstein's theory of relativity requires this result or not, and that is clearly a question in the history and philosophy of science : we must know what Einstein's theory of relativity is, and what its logical requirements are in a situation transcending the observational and experimental facts on which it is based.

The theory in question dates from 1905—just over 50 years ago. That is not a long time. To the older among us it scarcely counts as history ; it is rather current science. Nevertheless, the course of the controversy has shown that the majority of the participants are quite ignorant of the circumstances in which the theory arose, of its essential nature, and of what its simplest requirements are. They consequently draw conclusions, which they advance as necessary demands of the theory, which are in fact plain violations of its basic principles. We have therefore a situation exactly equivalent to that of a " proof " from the second law of thermodynamics that in certain specified circumstances heat will necessarily of itself flow against a temperature gradient.

The matter is really very simple. A reference to Einstein's papers—both the 1905 paper in which he put forward the restricted theory and the 1916 paper in which he generalised the postulate of relativity—will show quite clearly that what we now call the theory of relativity comprises two distinct postulates. The first is the " postulate of relativity ", which is simply, to use Einstein's own words, the postulate that the phenomena of nature " possess no properties corresponding to the idea of absolute rest ". This was extended in the second paper to the postulate that those phenomena also possess no properties corresponding to the idea of rest or uniform motion. In this form it is called the general postulate of relativity. This is the basis of the whole theory, and it is entirely non-mathematical. It is a generalisation from universal experience : we have never found any phenomenon that will enable us to say, in a case of relative motion of two bodies, that it is this body rather than the other that is moving, or this rather than the other that is at rest.

But now, according to ideas current at the time, we would have expected such phenomena to exist. In the case of uniform motion we would have expected experiments with light rays to have given different results according to which of the two bodies was the moving one. In the case of accelerated motion we would have expected the phenomena of gravitation to tell us which was the moving body. Einstein therefore provided a *second* postulate in each case which, if it were true, would prevent the results of the proposed experiments from giving the expected criterion. For uniform motion this second postulate was called the " postulate of the constancy of the velocity of light "—not the most fortunate name perhaps, but that does not matter for our present purpose. For uniformly accelerated motion the second postulate was called the " principle of equivalence ". I do not need to state what those postulates were : the essential points are, first, that they were additional and secondary to the postulate of relativity, and second, that they were such as to prevent our expecting any violation of that postulate. Unlike the postulate of relativity, they were mathematical, because the phenomena which appeared to violate that postulate were metrical phenomena. It was therefore necessary to provide a method of calculation which would necessarily result in the measure of the supposed phenomena becoming zero.

We have, then, first a general postulate, based on unvarying experience, that no observation is possible that will distinguish which of two relatively moving bodies is actually the moving one ; and secondly, subsidiary mathematical postulates designed for the purpose of annulling all expectation of such an observation. Now what conclusion should we draw if a calculated deduction from one of the subsidiary postulates *did* give an expectation of such an observation ? Clearly we would have to say that either the deduction was not mathematically correct, or else the subsidiary postulate had failed to conform to the primary postulate of relativity and must therefore be abandoned. It would be quite illogical to accept the supposed implication of the subsidiary postulate and abandon the postulate of relativity, because the subsidiary postulate would never have been thought of except as a means of maintaining the postulate of relativity.

Now that is just the position we have reached in this space-travel controversy. A deduction from the postulate of the constancy of the velocity of light appears to show that the returning twin would be younger than his brother because motion slows down our vital processes. I believe the calculation leading to this conclusion to be erroneous, but that is still a matter of controversy, and I do not wish to intrude it here. What is inescapable is that, if it is not erroneous, the " younger " twin is the moving one, because it is his vital processes, and not his brother's, that have been slowed down. Hence we have found a phenomenon that distinguishes which of the two bodies is the moving one, so the postulate of relativity is violated. The only possible logical conclusion, therefore, is that, if the calculation is correct, the postulate of the constancy of the velocity of light must be abandoned.

The astonishing fact emerges, however, that the majority of mathematicians and mathematical physicists who have discussed the question do not draw this conclusion : they believe that the returning twin would indeed be younger. They do not realise that they are thereby abandoning the postulate of relativity, because they do not know what the postulate of relativity is. They talk merely of " Einstein's theory of relativity ", by which they mean only the postulate of the constancy of the velocity of light and its mathematical consequences. They say that " Einstein's theory of relativity " requires this asymmetrical ageing, and are quite unaware that the fundamental postulate of that theory is simply a denial that such a phenomenon can occur. Thus within fifty years one of the greatest advances in the whole of scientific history is so distorted as to be believed capable of proving its own contrary.

Let me repeat that I am not saying what the result of the experiment, could we make it, would be. I think that in fact the chance that the returning twin would appear younger than his brother is rather less than that he would appear with wings (evolution may take strange courses in other environments), but that is quite beside the point. Neither am I saying what the mathematical theory requires. I am myself perfectly convinced that it does not require the asymmetrical ageing, but again that is quite beside the point. The point is that, *if* the mathematical theory requires such a phenomenon, it is inconsistent with the postulate of relativity which it was designed to preserve, and must therefore be given up as ineffective for its purpose. That is not a matter of experiment or of mathematics ; it is simply elementary logic. All that we need in order to be convinced of it is a knowledge of recent history and the rudiments of reasoning, and that, alas, is what we lack.

This is a calamity for its own sake : it shows what happens to science when it neglects its history and philosophy. But in this instance it may have other

consequences also which serve to show the responsibility that rests on a science that has acquired so commanding a position in social life as physical science has today. High speed space-travel may or may not be imminent, but serious talk of it is actually here. We know from bitter experience that there are fools as well as wise men in the world, and that they are not without influence. One need not enter into details to realise that if it is widely believed that there is scientific sanction for the idea that by space-travel death may be postponed indefinitely on our terrestrial time-scale, some very regrettable consequences may ensue. Still more serious is the reflection that experiments are now being undertaken in which reliance must perforce be placed on calculations from the theory of relativity. Such experiments may have the most tragic consequences if the calculations are unsound, and they may be in the hands of those who so misunderstand the theory that they believe it can contradict itself. In such matters we can do nothing except utter a warning : we do not know what is going on. But if anyone is inclined to regard the matter as merely academic, he should think again.

To return to our main subject, we may note that there are indeed signs that the need is being realised. Before the first World War, independent studies of the history of science and the philosophy of science were largely the work of individuals, ploughing a number of lonely furrows with neither hindrance nor support from organised scientific bodies, which were on the whole unaware of their existence and would have been indifferent to it had the knowledge come to them. Between the wars these efforts grew into something like viable organisms, which, however, remained independent, and such collaboration as existed arose from the spontaneous efforts of individuals rather than from systematic organisation. These bore some fruit after the second World War, and in 1947 an International Union for the History of Science was formed and affiliated with the International Council of Scientific Unions (ICSU). Thus for the first time the study of the history of science became formally recognised as a scientific activity. The organisation of the philosophy of science was somewhat less advanced, but this subject also generated an International Union, and in 1956 a formal liaison was established by which a joint Union for the History and Philosophy of Science came into existence, with separate Divisions for the History and the Philosophy of Science, and took the place of the former History Union as a constituent part of ICSU. In the Annual Report of ICSU for 1955—the latest available—twenty-two countries are listed as adhering to the International Union for the History of Science : the present number is thirty. This rate of growth gives some indication—and I think not a false one—of the growing realisation in the world in general of the status of the history of science as an integral part of science itself.

Among the comparatively few countries which have not joined the International Union for the History of Science—or, as it now is, for the History and Philosophy of Science—the most conspicuous is Great Britain. This distinction can scarcely give us satisfaction. It may well be thought that it is the duty of our Society to act as a representative of this country in order to secure its affiliation with the International Union, and if no other means to this end becomes available such action will be considered and, I have little doubt, will be taken. But no-one who has any feelings of national pride could regard such a step as anything but a regrettable necessity. The normal procedure in such matters is for the leading scientific organisation in a country to form a National Committee, through which the affiliation is made. That is the action taken in other countries —for instance, the Academy of Sciences in France and the

National Academy of Science in the United States have formed such Committees—and it is the action in this country with regard to all other Unions. The Report of ICSU to which I have referred shows that there are eleven Scientific Unions included in ICSU, to ten of which the Royal Society has adhered by the formation of National Committees : the single exception is our unfortunate subject. The matter is still under consideration, and I have hopes that a reproach to which we have for some time been open will shortly be removed.

But if this should come to pass, it could not be regarded as other than a small step towards meeting the need which grows ever more urgent as time goes on. It is not enough to give formal support to an association of national efforts. Such an association will be powerless unless the efforts of the individual nations are themselves effective. Our Society, with its Philosophy of Science Group, has been in existence for some ten years and has made some progress. We have little cause for self-complacency, but also, I think, little for self-accusation. We have done something to promote the study of the history and philosophy of science, and we may hope to do more. But no-one who has clearly realised the essentially progressive nature of scientific research, and the limited capacity of even the most potent human intellect, can remain unaware that sooner or later some positive action must be taken to ensure that the prosecution of new research is inevitably accompanied by regular provision for the examination and proper understanding of its necessary foundations. In the nature of things this cannot be effectively undertaken by a struggling voluntary association of individuals whose very existence is at the mercy of circumstances over which they have no control : it is the inescapable responsibility of the Royal Society itself. And the present organisation of the Royal Society is such that no discharge of that responsibility is easily possible. Devised at a time when the problem scarcely existed, when there was a sufficient proportion of the pioneers in science whose natural curiosity and comparative leisure allowed them to examine as well as to build on the foundations of their work, it is now quite unfitted for a situation in which those conditions have passed away. Consider the constitution of the Society. It is entirely in the hands of its Fellows, twenty-five of whom are now elected each year on the basis of their contributions to new knowledge. With the present competition and tempo of scientific work, and still more with that which we may expect in the future, no-one has the slightest chance of election who has not narrowly specialised in the particular department of a department of a department of a science in which he has achieved distinction. The chance that any such person has any interest, let alone faculty or opportunity of indulging that interest, in the history of science is almost negligible : if he was born with the necessary predilection, he must have effectively suppressed it in order to qualify for admission to the Society. The result is that in a very few years the Society will contain no-one who knows on what he is building so feverishly or, still worse, has any awareness that such knowledge is important. The magnificent library which the Society possesses will be unknown to its Fellows and will be used only by those—mainly visitors from abroad—to whom it may grant permission to examine its treasures. That this is only too accurate a picture of the situation may be seen by examining the present list of Fellows and noting those who have given any indication that they believe the history of science to be a worth-while study. They are few, and among the more elderly, and their number must inevitably decrease fairly rapidly. A similar examination of those elected to Fellowship since the last war would give a result too ominous to mention.

I do not think it can be other than obvious to anyone who looks at the present constitution and membership of the Royal Society with a sense of responsibility that the time has come to take some definite steps to ensure that the prosecution of science, in which we all rejoice, shall be accompanied by a continuous examination of the foundations on which the whole validity of the new work must necessarily rest. It would be an impertinence for one who is not a Fellow to suggest what those steps should be, but it is not only not an impertinence but a positive duty for anyone, Fellow or not, who has a clear view of the intrinsic necessities of scientific research, and of the inadequacy of the present organisation to provide those necessities, to point out the peril of the present position and to call for its consideration. That such a step as I have indicated must be taken sooner or later is beyond question: it is a natural imperative unless science is to perish or to destroy us all by failure to understand its own achievements. What we must call for is for something to be done now, before such a catastrophe comes upon us. If those who are in a position to act can be persuaded to look at the matter seriously I do not doubt that something will be done. There is, I believe, no defect of will or sense of obligation; it is the vision that is lacking, and that becomes harder to acquire with every advance in the specialisation which now has us firmly in its grasp.

Meanwhile, our Society, in its small way, may help to spread abroad an interest in the subject and keep it alive. We have survived and grown through a decade, not without difficulties or without support from bodies whose responsibility for support has been less than their readiness to give it. We go forward in thankfulness and in hope.