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History and quasi-history in physics education—part 2

M A B WHITAKER
Department of Physics,
New University of Ulster,
N. Ireland

In part 1 of this article the prevalence of quasi-history was demonstrated; here we discuss its effects. The only author to have discussed the effects on the student of reading quasi-history appears to be Brush (1974). He considers quasi-history to be an attempt to force accounts of scientific discovery to meet the standards of objective scientific method, as described by philosophers of science. Here hypotheses are based on experimental results, and critically tested by further experimentation. To show that this assumption of objectivity is far from the truth, he discusses various cases from history which demonstrate that, while theory is obviously influenced by experiment, a particular theory may become and remain prominent in the absence of, and even if it is contradictory to, experimental evidence. This he puts down to 'subjective' features, such as intuition, persuasion and conversion. For Brush, the importance of teaching true history, rather than quasi-history, is that these subjective elements are displayed, rather than swept under the carpet. He is not very concerned with the social aspects of scientific activity.

In this article the analysis is rather different from that of Brush. For a start, I do not assume that writers of quasi-history necessarily have any philosophical intent, even subconsciously. I see quasi-history more often merely as a result of a rather misguided desire for order and logic, as a convenience in teaching and learning. (A few colourful terms such as 'ultraviolet catastrophe' and 'sixth place of decimals' are included in an attempt to retain the enthusiasm of the student.)

A more important divergence from Brush is that I consider those factors which are ignored in quasi-history to be mainly those relating to the social interaction of scientists. Indeed I would view those features which Brush terms subjective to be largely a result of the public and social nature of science. I shall make much use of the ideas contained in the book by Ziman (1968), *Public Knowledge: The Social Dimension of Science*, where it is suggested that the progress of science is via a consensus of universally accepted knowledge, rather than through work of individuals in isolation. Ziman discusses scientific education, communication and institutional forms, as elements designed to contribute to this consensus. The need to communicate with other scientists, and to make one's work acceptable to them, is seen as pre-eminent. Indeed a scientist's work only becomes 'science' in these terms when it reaches the stage of acceptance by the consensus. (Of course, when Brush uses the word 'persuasion' in his list of subjective features, he is implying a measure of social activity, but he does not appear to view this activity as central to science.)

First we shall examine how quasi-history, by eliminating the social dimension, distorts its description of major scientific advances in one of two distinct ways, which will be represented here by the cases of Einstein's theory of the photoelectric effect and Planck's discovery of his law. (These, and the theory of relativity, will be mentioned frequently; they are particularly important examples, as students study them in detail and will assume that they are typical cases of scientific advance.)

Social dimension of science

In the case of the photoelectric effect we have seen that the ideas are made to seem almost trivial, because the motivation is presented with such retrospective clarity. (Similarly the emphasis on the Michelson–Morley result as a guide to Einstein in his development of relativity also diminishes the creativity involved, though here even quasi-history cannot make the ideas appear obvious.) Where even today the motivation cannot be described in such clear-cut terms, quasi-history goes to the other extreme of making the advance seem an act of unmotivated and instinctive genius. We can see this in Planck's discovery. We may well wish to describe Planck as a genius, but his discovery was certainly not, as implied in many texts, a spur-of-the-moment idea tossed out by sheer brain power, or even scientific instinct. It was the result of months of hard work, with Planck performing finally 'an act of desperation' (Hermann 1971) to produce the required result. This is brought out clearly in his *Scientific Autobiography* (Planck 1949).

So quasi-history presents the discovery of new scientific concepts as one of two extremes, either almost trivial or almost mystical. In both cases the lack of attention paid to social interaction is at least partly responsible. In the case of the photoelectric effect no attention is paid to the fact that Planck's work took a long time to become part of the consensus of accepted knowledge, and that during this time it had to be discussed and refined, and to have its implications fully developed. In the case of Planck's

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discovery itself, quasi-history ignores the processes which needed so much time and effort, the detailed examination of the evidence, the comparing and contrasting of different lines of thought, and the construction and testing of hypothesis after hypothesis, all of which led up to the creative act. While these are largely personal in nature, there is a social element which is crucial. The consensus idea implies that any scientist builds on the work of others. In this case Planck studied the work of, in particular, Clausius and Wien, he used the experimental results of Rubens and Kurlbaum, and he reacted unfavourably to the statistical ideas of Boltzmann, which, in the end, he was forced into using.

We shall now consider in a little more detail how quasi-history cannot accept the social aspect of science. Once a discovery is announced, in fact, quasi-history insists that there is instant understanding and agreement. The writer wishes to convince the reader of the undoubted truth of his case, and feels that the controversies of the past are not relevant. An example of this occurs even in such an excellent book as that by Eisberg (1961), where an account of the Michelson-Morley experiment is followed by an instructive discussion of the various theories put forward to explain the null result. It is still possible, however, to obtain the idea that, when Einstein put forward his theory of relativity, there was instant agreement and understanding.

This, of course, is very far from the truth. Even as late as 1921 Einstein's Nobel prize was given officially for his theory of the photoelectric effect, because his theories of relativity were looked on as still suspect. It would be nearer to truth to believe that, as Planck said of quantum theory, general acceptance of relativity had to wait for the deaths of many of the leading scientists of the time. There were obviously many exceptions to this rule, but we should remember that Lorentz himself, according to Born, 'never became a relativist at all' (Born 1971).

Neither can quasi-history accept the idea that even leading scientists can make mistakes. The writer is concerned to expound accepted ideas, and it is obviously no part of his job to mention errors. (On the consensus theory, of course, mistakes are to be expected in the work of individual scientists, but it is hoped that the vast majority will be detected by the scientific community before they become part of the consensus. Once they reach the consensus, they may be particularly hard to dislodge.) We see these effects very clearly in the way quasi-history treats the Rayleigh–Jeans law. It places it securely before Planck's law in time, as, once the latter was known, quasi-history assumes it would be completely understood and accepted, and there would be no need for further classical approaches. The fact that as late as 1905 Rayleigh was not only thinking classically, but could not even understand Planck's approach comes as a surprise to us. And the fact that Rayleigh had to be corrected by Jeans is suppressed to the extent that many students imagine that the law was discovered by a physicist named Rayleigh-Jeans.

Thus quasi-history has a considerable distorting effect on the presentation of physics. Does it matter? First it must be admitted that it can amount not just to a lack of accuracy concerning history, but to a complete disregard for historical truth. It is difficult to take seriously a passionate feeling for the importance of scientific truth, coupled with a lack of interest in the truth or otherwise of historical statements. This is in itself an important objection, but our chief concern here is the effect of the complete disregard of the social aspects of science on the reader, especially if that reader is a student, anxious to learn not only the facts of science, but also about the worth of science as a human activity, and about scientists and the scientific process. The attitude of the student will determine whether he continues his studies in science, and will affect what he thinks of science in later life.

The effect of quasi-history must be to repel the student. We have seen that it presents the scientist not as a hard worker, using all the insight and experience he possesses to solve his problem, but either as a solver of trivia or as a superman, conjuring up answers from thin air. The student will have little desire to join the ranks of the former, and little confidence to attempt to join the ranks of the latter.

On the other hand, the ideas of the social dimension of science are appealing to students, as they show human interaction and cooperation between scientists. There may be rivalry between scientists or scientific teams, but they may still be regarded as being in effect in partnership to unravel the mysteries of nature. So science is shown as a humanistic endeavour carried on by human beings, rather than, as would be gathered by the reader of quasi-history, an activity wholly of the intellect, totally depersonalised.

Use and misuse of historical approach

I have attempted to show that quasi-history plays an important part in the rejection, by many, of the ideas and values of science. Can anything be done to counteract it? This obviously raises in turn the question of whether the history of science has a part to play in the teaching of science, as discussed, for example, at the seminar mentioned in part 1, and also by Brush (1969). Brush categorises the different approaches to the teaching of science as 'logical' and 'historical'. It is a worthy aim to teach 'logically', but it is nearly impossible to avoid mention of names and occasional dates, and the insidious drift to quasi-history begins.

If the writer presents historical matter at all, it is essential that he teaches the history as it happened, not as it might have happened, or as he wishes it had happened. The teacher, teaching a 'logical' course, must be watchful for these tendencies to quasi-history and should be prepared to counteract them. Should there, however, be a deliberate attempt to teach science historically or at least to introduce some historical material? I shall first give two examples of the dangers of the use of history (although these could, of course, be called dangers of the misuse of history).

The first example concerns the development of the concept of the Kelvin temperature scale and that of entropy, as consequences of the second law of thermodynamics. Historically this development was performed by the use of the 'engineering method', by, in particular, Carnot, Clausius and Kelvin. The basic concepts of this method are connected with the working of engines and refrigerators, and the operation of the Carnot cycle is central to the development.

The physicist or chemist would prefer to discuss these fundamental ideas in terms of the properties of the physical substance itself, rather than its behaviour in a particular use in an engine or refrigerator. In 1909 Caratheodory developed an alternative method (Caratheodory 1909, Buchdahl 1958) which Zemansky calls the 'axiomatic method'. He replaced the conventional statements of the second law by a mathematical axiom from which all the consequences of the engineering method could be derived, by techniques which were not conceived in terms of the operation of any machine, but which were, however, rather too mathematical to be appreciated by most physical scientists. Much more recently this approach has been considerably simplified mathematically. The axiom itself is no longer necessary in the simplified approach, and the standard textbook by Zemansky (1957, 1968), which used the engineering method in its first four editions, changed to the simplified axiomatic method for the fifth.

This is a case where the historical treatment had become traditional, and, although the engineering method was not really suitable for teaching students of science (as distinct, perhaps, from students of engineering), there was little incentive to look for a better approach. This approach therefore took many years to arrive, and one suspects that it may be many more years before it becomes generally accepted by teachers.

As a second example we shall consider the wave-particle paradox. Physicists of the late 19th and early 20th centuries understood very well energy transfer of a wave-like nature, and energy transfer of a particle-like nature. Though few might have gone as far as Preston (1890), who stated that any other method of transferring energy was inconceivable, this very belief in fact became part of the accepted consensus. The terms 'wave' and 'particle' seemed to be 'in logical contradiction to each other' (Hanson 1963). It was taken for granted, not only that all mechanisms of energy transfer had to have either a particle-like or wave-like nature, but that none could have elements of both. Textbooks on quantum mechanics and modern physics almost universally follow this historical line, presenting the discoveries that light had a particle-like nature and electrons a wave-like nature as the 'wave-particle paradox'. This is, of course, a delusion. Very many mechanisms of energy transfer can be imagined with superimposed particle-like and wave-like natures, among them those described in Newton's theory of light and the present theory of quantum mechanics.

The fact that the 'paradox' was so difficult to unravel fits in with an idea of Ziman (1968) that the hardest beliefs to disprove are those that are held subconsciously; if the statement that entities could not have wave-like and particle-like properties had actually been made, it might have been objected to, but in fact it had come to seem so obvious that it was hardly ever thought necessary to make it. (Bacon too observed that 'Truth emerges more readily from error than confusion' (Spedding *et al* 1869).)

The fact that there was thought to be a paradox, and the way in which it was resolved, are, of course, important and interesting, and should be taught to students. It is extremely unsatisfactory, however, that it is seldom explained that the setting up in logical opposition of the wave and particle concepts was mistaken. Very often students are told, on the one hand, that an entity can be either a wave or a particle (with no possible alternative, and no possibility of shared attributes) and, on the other hand, and without correction of that idea, they are given the ideas of modern quantum mechanics. In this case it is confusing to follow the purely historical approach without a good deal of critical analysis.

Sources of material

Despite these examples of the dangers of following a strictly historical approach, it seems clear that to counteract the quasi-history which creeps into any 'logical' treatment, physics courses (and, indeed, science courses in general) need some historical content. In spite of the dangers mentioned before of studying those aspects of the past which seem interesting from our present point of view, it also seems clear that, to a large extent, this is unavoidable here. This need do little harm, provided that once a general area of study is selected all aspects of it are studied, not just those that seem of direct significance today. For instance, a student might be encouraged to study Bohr's original paper on the model of the atom; he would then proceed in a particular direction to study, perhaps, its influence on subsequent developments, or to compare it with other models. This type of study helps to show how simplified the textbook accounts nearly always are.

There are not many secondary sources which are particularly suitable for study at this level. Many popular histories of science are rather superficial, and popular biographies of scientists often concentrate on personal details or, where they discuss the work of the scientist, present him as a lone worker (the 'superman' image), thus ignoring the social dimension. There are a number of scientific autobiographies, however, such as those of Planck (1949) and Slater (1975), which help to show the scientist at work.

An interesting source of material is the collection of Nobel lectures in physics (1964, 1972). They contain accounts of important branches of physics, which are obviously authentic and very often at a reasonable level for a student. These accounts usually include a useful guide to the antecedents of the discovery, which may be investigated by the students, and the accounts of the prize winners' lives often contain further interesting material concerning the conduct of science. For instance, chosen almost at random, Maria Geoppart-Mayer's lecture of 1963 is an excellent introduction to the shell model of the nucleus; it gives an interesting account of how the discovery was made and the previous work it relied upon. Her biography shows the difficulty of being a wife of a well known scientist, even when you are yourself capable of excellent scientific work. Of course, this souce may be criticised as being limited to highly successful investigations and highly resourceful individuals. The lectures may be distorted by the desire of the lecturer to claim too much, or perhaps more often as in Laue's case, according to Forman (1969), too little of the credit, but at least they show how eminent scientists viewed their work.

As important as study of the history of science is study of its sociology, so that students get some idea of how science is carried out. I have already mentioned the book by Ziman (1968). Among others, which present different views on certain matters, are the books by Kuhn (1970), which discusses the way in which changes occur in scientific beliefs, and by Hagstrom (1965), which describes and analyses many aspects of the behaviour of scientists. The book by Price (1963) is a short, readable account of matters such as frequency of scientific publication, while that by Crane (1972) is a much more sober account of the informal structures of science. Lastly I would mention Polanyi's book (1958) on scientific knowledge and method, which combines sociological and philosophical modes of analysis, and a more recent book by Ziman (1976), in which the workings of the scientific community are discussed in relation to the pressure and needs of society.

A last recommendation is that teachers should make great efforts to present physics as a living discipline, rather than as a completed structure of knowledge. Project work is excellent from this point of view. While there is an inevitable tendency for most courses to present material which appears cut-and-dried, it is important to point out the difficulties and uncertainties that remain. And it may be worthwhile organising sessions where teachers and research students describe their own research. This will be not so much from the point of view of conveying knowledge, because that will lead to the tendency, noted by Ziman (1968), to present the results and ideas as if they are already part of the established consensus. The aim will be to show the difficulties of performing the research, and the doubts that may remain.

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