

History and quasi-history in physics education—part 1

M A B WHITAKER

Department of Physics, New University of Ulster, N. Ireland

Why do we teach physics? Our primary aim, presumably, is that the student should gain an understanding of physical principles and techniques, and their application. A secondary aim of most teachers would be that the student should obtain an appreciation of the significance of the scientific approach, the various revolutions in man's understanding of nature, the way in which science is carried on today and its place in modern life.

One might naively assume that it is in pursuit of this secondary aim that, in addition to being exposed to a large amount of strictly scientific information, the student will be asked to digest a smaller, but not insubstantial, amount of material which is historical, or purports to be historical, or seems to be historical. But this is far from true. In some cases there is certainly a genuine desire on the part of the author or lecturer to pass on the history of the subject. It is merely unfortunate that such attempts are often unsatisfactory. Marwick (1973) castigates the elderly scientist, devoid of historical training but urged to write an introductory 'historical' chapter to a scientific work, who produces the worst form of Whig history, a term used by Butterfield (1931) to describe historical writings which start from modern ideas and attempt to explain how they came about, rather than trying to understand the approaches of former generations of scientists in their own terms and according to their own preconceptions.

In this article I shall discuss another type of material which looks historical, but in which there is no attempt to convey history truthfully: the aim is solely to put over scientific facts, and the 'history' is there to provide a framework inside which the scientific facts fit easily, appear to 'make sense' and may be easily remembered for examination purposes. It also provides, maybe, a little light relief from the hard facts of the science itself. I shall call this 'quasi-history', and many examples will be given below, followed by a discussion of its effects.

First, though, let us consider the type of comment which merely seems to the student to be historical, without any intention on the part of the writer. This may merely be nomenclature such as 'Newton's laws' or 'Lorentz contraction', or such statements as 'Archimedes' principle states that ...' or 'From Huyghens' principle it follows that ...'. This seems unobjectionable enough, but may convey to the student completely the wrong impression of the connection between, for example, Huyghens' principle and its consequence. The student may infer that the consequence was obvious to Huyghens, or 'immediately obvious' to anybody of intelligence once Huyghens had produced his principle, whereas it was perhaps the result of great amounts of labour later (as for the full wave theory of light). The impressions gained by the student may be similar to those to be discussed in part 2 of the article, as results of quasi-history, but perhaps more insidious because they are gained subconsciously rather than directly. The problem will not be discussed here, but is worthy of further study.

Rayleigh and Planck

The example of quasi-history with which we start, the discovery of the Rayleigh-Jeans and Planck radiation formulae, has been discussed recently in this magazine in an interesting article by Dougal (1976). It will not be out of place here, however, since there are several further important points to be brought out, which I shall develop later.

In any case, despite the availability of excellent general accounts of the origins of the quantum theory (Jammer 1966, Hermann 1971), as well as particularly detailed study of Planck's work by Klein (1962, 1963, 1966), the grip of quasi-history is very strong. As an extreme example, a recent book review (Phillips 1973) actually 'corrects' the author of the book, asserting that the Rayleigh-Jeans law was known before Planck discovered his own law, and claiming that 'it was the failure of the Rayleigh-Jeans law of classical physics to accurately predict the law of black-body radiation that led to Planck's quantum hypothesis'. In fact Planck announced his law to a meeting of the German Physical Society on 14 December 1900 (Planck 1900). His method bore little relation to that used in most modern textbooks (e.g. Eisberg 1961); it was based on the relation between the entropy and energy of the system.

In June 1900 Rayleigh (1900) had published a short paper in which he outlined the method of computing the number of degrees of freedom of the black body with the method used by most textbooks today to obtain the Rayleigh-Jeans and Planck laws. He obtained an expression proportional to the energy density, but did not calculate the constant of proportionality. It was obvious to Rayleigh that the expression was unsatisfactory, as it diverged to infinity

for high frequencies and its integral over all frequencies was also infinite. He felt these defects were sufficiently obvious not to warrant explicit mention, but merely suggested including an arbitrary, exponential cut-off factor to lead to sensible results. There was no sense of catastrophe in the tone of Rayleigh's paper.

Little attention was paid to Rayleigh's work; no more, in fact, than to a number of purely empirical calculations published at roughly the same time (Klein 1962). While there is uncertainty as to whether it should be assumed that Planck saw Rayleigh's work (Klein 1962, Brush 1969a), it is clear that it in no way influenced him. It was not until 1905 that Rayleigh (1905) calculated the constant of proportionality. (Jammer 1966 is rather misleading in implying that Rayleigh obtained the complete expression in his 1900 paper.) By now he was, of course, well aware of Planck's work, and noted that in the long wavelength limit his result was eight times that of Planck. He wrote that it would be interesting to investigate this by a comparison of approaches, but that, since he had not succeeded in following Planck's reasoning, he was unable to perform this. Almost immediately Jeans (1905) pointed out that Rayleigh, by introducing wave vectors with negative as well as positive components, had produced an answer eight times too large, and the corrected version is the Rayleigh-Jeans law. Planck's law, of course, reduced to this in the long-wavelength limit, but by no stretch of the imagination could it be said that it had led to his quantum hypothesis.

The reviewer referred to above is obviously suffering from an excess of quasi-history, a result of the large numbers of books by authors who have felt the need to enliven their account of this episode with a little historical background, but have in fact rewritten the history so that it fits in step by step with the physics. Because the description of the physics is logical and orderly, the impression is necessarily given that this was also the way in which the ideas emerged historically. (Quasi-history, as defined here, is thus rather different from 'pseudo-history' defined by Klein (1972) to be the approach: 'This is what people thought in 1820, and this is what we know today'.) In this case it is convenient to present the physics by showing that classical theory yields the Rayleigh-Jeans law, but that the replacement of equipartition of energy by the condition $E = nh\nu$ gives Planck's law instead. Quasi-history asserts that this is the way things actually happened. (As pointed out by Jammer 1972, the distortion will be particularly severe when, as here, logical order is actually the reverse of chronological order.)

Various authors have discussed quasi-history. Holton (1969) shows that the widespread belief expressed, or at least suggested, in a large number of textbooks that Einstein's theory of special relativity was motivated by the null result of the

Michelson-Morley experiment is fallacious; he uses the term 'implicit' history of science. He brings out strikingly the point that Michelson's result could well be of decisive importance in convincing the physicists of the time—and the student of today—of the truth of relativity, and yet could have played little or no part in Einstein's own thinking. Brush (1974) discusses the relation between the history and quasi-history of science, and makes the tongue-in-cheek suggestion that the true history should be banned as being too dangerous for students. His ideas are discussed further later.

Klein (1972) notes that the historical sections of many physics texts are extremely shoddy, with the inclusion of errors in simple matters of historical fact, and goes on to claim that there is a fundamental problem in teaching the history of physics to physics students as an aid to their understanding of physics. As soon as the selection of items of history, and their presentation, are determined by reference to the physics being taught, it ceases being history, for, as stated by Lovejoy (1939), the historian's selection of subjects for inquiry should be determined by what seemed important to other men, and it is precisely this that differentiates historical studies from other types.

It may be of interest, finally, to describe briefly the concept of 'rational reconstruction' of history due to Lakatos, which has an apparent similarity to quasi-history. The possibility of objectivity in written history is widely discussed by philosophers and historians (Blake 1955); the view usually taken is that, if only by selection of material, the historian introduces personal bias (Popper 1960). Lakatos (1971) first contends that the bias must correspond to the set of theoretical ideas accepted by the historian. This must be particularly true for the historian of science, who must suppress (or declare irrelevant) anything that seems irrational in the light of his philosophy of science. He goes on to say that the historian may, in fact, 'radically improve' the account; this entails writing the history of the science as it would have been performed if the scientists had been strictly rational. In another work (Lakatos 1970) he writes his radically reconstructed version of history in the text, and adds footnotes which explain what actually happened as a result of the failure of the scientists to be rational. Kuhn (1971) criticises this approach, claiming that if the historian selects and interprets his material according to a prior philosophical position, there is no way in which his reconstructed data can react back and play a part in the assessment of the success of that philosophical position.

So the method of rational reconstruction appears to be of dubious validity for the philosopher. It may be useful in the teaching of science, provided it is pointed out at the outset that it is indeed a reconstruction: the process may, in fact, have an additional benefit of making explicit the fact that the way in which

scientists arrived at ideas does not usually seem, from our privileged viewpoint, the most straightforward. It should not be confused with quasi-history, which does not admit that reconstruction has taken place.

Further examples

A few more examples of quasi-history follow, starting with Einstein's theory of the photoelectric effect. It is a fair presentation of the physics of this to present Planck's work in a clear form and to give the modern experimental evidence, but it is quasi-history at its worst to imply that Einstein had access to these developed theoretical concepts and detailed experimental results. In fact, at the time of Einstein's work, Planck's ideas were not thoroughly understood or accepted, and the experimental evidence was meagre, and lacking in clarity and accuracy.

Among examples in the literature is the discussion of Bromberg (1967, 1968) on Maxwell's addition of the displacement term to the electromagnetic equations. The motivation for this addition was not to avoid the incompatibility of Ampère's law with the equation for continuity of charge (as stated in most modern textbooks, e.g. Jackson 1972). Neither was it to produce symmetry in the equations, in contrast to the claim of Campbell (1952), mentioned in Bromberg's paper, that Maxwell's work was 'one more illustration of the marvellous power of pure thought, aiming only at the satisfaction of intellectual desires, to control the external world'. In fact Maxwell was engaged in the more prosaic task of attempting to calculate the elasticity of the electromagnetic ether. His initial theory was in fact electromechanical rather than electromagnetic, his reasoning was laborious and his results ambiguous.

There is a fairly large number of examples of quasi-history in textbook descriptions of the evolution of modern physics. We may start with the frequently quoted statement that scientists at the end of the 19th century regarded the future of physics as lying in measurements to the 'next place of decimals'. While based on fact, in that Michelson did make comments of that nature, there is no justification for implying that it was a general view, nor was it necessarily Michelson's considered opinion (Brush 1969b). (This point has been further discussed by Badash 1972.)

Forman (1968) analyses the commonly expressed view that it was the failure of the Bohr theory to predict the spectrum of the helium atom that led to the belief, held in the years between 1923 and 1925, and expressed in particular by Born (1923), that the basic framework of physics required a far more radical reshaping than that provided by the Bohr model. Forman shows that this was not the case; the major difficulties were in reality felt to be the complex structure of spectral lines, and in particular the way these lines split in a magnetic field. At the time the

problem of atoms with more than one electron seemed linked with that of the complex structure. This was because of the rather strange circumstance that Sommerfeld's relativistic adaptation of the Bohr theory gave the doublet structure of the hydrogen spectrum precisely as found by experiment. Thus the problem seemed to be to understand complex structure in atoms heavier than hydrogen. It was only a rather bold return to the hydrogen problem by Heisenberg which led to the new quantum theory in 1925. The remaining problems of complex structure were solved by the introduction of electron spin by Goudsmit and Uhlenbeck.

Forman makes the interesting point that, from our privileged viewpoint, we divide the problems into two groups and imply that quantum mechanics was responsible for solving the several electron problem, and electron spin for solving the complex structure problem. Perhaps because quantum mechanics remains one of the most important theories of the century, while the concept of electron spin, although a substantial contribution, is seen, particularly after Dirac's work on relativistic quantum theory, to have more the nature of a useful device, we assume that those problems solved by quantum mechanics were the really urgent ones. As Forman points out, such a division of problems is possible only retrospectively.

Conventional modern physics textbooks regard Einstein's quantum hypothesis, based on the photoelectric effect, as triumphantly vindicated by Compton's discovery of the effect named after him. As Stuewer (1971) points out, this omits the very important question of whether the ultraviolet radiation considered by Einstein, and the x-rays used by Compton, were in fact just two different forms of electromagnetic radiation. The establishment of this point led to a lengthy controversy, from 1905 on, between Barkla, proponent of the wave theory of x-rays, and W H Bragg, who believed that x-rays were particles. After the discovery of the diffraction of x-rays by a crystal due to Laue in 1912, Bragg adopted the wave picture and, of course, gained a share in the Nobel prize for x-ray crystallography in 1915. He still felt, however, that the fact that an electron in a photographic plate may obtain, seemingly instantaneously, a large amount of energy from an x-ray beam meant that x-rays must have some of the properties of particles. In this belief he was proved correct by Compton. Once again the conventional account greatly oversimplifies the historical truth.

In the book by Stuewer (1975) on the Compton effect (and particularly in the review by Kuhn 1976 of that book) another piece of quasi-history, again concerning the relation between Einstein's theory of the photoelectric effect and Compton's work, is exposed. The textbook reader finds it difficult to accept that Compton's work was crucial, because the particle-like nature of electromagnetic radiation would be assumed

to be taken for granted ever since Einstein's first work, and particularly since his fluctuation theory of 1916, which demonstrated that the light quanta carried momentum as well as energy. In fact, despite Millikan's work of 1915, which is now taken to prove the correctness of Einstein's photoelectric theory, virtually nobody, not even Millikan, took the idea of light quanta seriously until the Compton effect was discovered in 1923. The Compton effect and its explanation were influential in the discovery of both Schrödinger's wave mechanics and, through dispersion theory, Heisenberg's matrix mechanics; it is, therefore, perhaps the link explaining these virtually simultaneous discoveries.

I shall mention briefly two further examples of quasi-history. First, in his history of the meson theory Mukherji (1974) comments on the commonly repeated story (e.g. Thorndike 1952) that Yukawa's prediction of 1935 meant that experimentalists were on the lookout for a particle of intermediate mass, and were thus readily persuaded that their observations of the μ^- particle were genuine. In fact the first accounts of the discovery of this particle by Anderson in 1937 do not mention Yukawa's work at all. Yukawa was separated geographically from the western physicists and his work was not studied carefully. Indeed, the reverse of the common story has considerable truth; Anderson's discovery encouraged theoreticians to take Yukawa's work seriously.

Finally we note briefly the explanation by Tonks (1967) of why the term 'plasma' was chosen by Langmuir. He states that it was merely related to blood plasma, and refutes stories that the name was intended to convey the ideas of oscillatory characteristics or seething movement in living cells, or to be an analogue of protoplasm.

As we see, the presentation of quasi-history is common, and it is interesting to note one case where, despite much temptation, it seems to have been avoided. Schrödinger's development of wave mechanics used de Broglie's ideas, and the Schrödinger equation can be produced by starting from the de Broglie theory and adding a number of additional, far from obvious, ideas and assumptions (e.g. Eisberg 1961). It is not clear to what extent Schrödinger himself followed this path, and a check of textbooks appears to show that writers have avoided the temptation to claim that the logical path that they have traced was in fact that taken by Schrödinger. They may thus be regarded as writing rationally reconstructed history rather than quasi-history.

Propaganda and myth

I have given above some examples of quasi-history which strictly obey our definition. The error is the responsibility of textbook writers, and perhaps the scientific community at large. I shall now mention two

cases, which are similar to those, in that it is claimed that erroneous statements are practically universally accepted as true, but differ, in that the scientist or scientists themselves are said to be responsible for the initial spreading of falsehood.

The first example is of what I shall call propaganda. In the proceedings of the Charles Lyell centenary symposium, Porter (1976) claims that Lyell's vision of himself as the 'spiritual saviour of geology, freeing science from the old dispensation of Moses', which has been accepted by scientific writers from Victorian times right up to the present day, is largely a result of Lyell's self-publicity. Porter calls for a reappraisal of Lyell's contribution to geology.

The second example is rather more interesting; its analysis is subtle, and it has been heavily criticised by one of those claimed to be principally responsible for distorting historical truth. In many of the cases described so far, the word 'myth' has been used in the original paper. It seems preferable to reserve the term for cases like the present one, that of the discovery of x-ray crystallography. Forman (1969) claims that here a myth has developed, to be compared with myths surrounding the creation of many religions. Analogous to the religious community is the world community of crystallographers, who have maintained their own grouping, where Forman says they would have been expected to merge with the various disciplines supported by crystallography.

According to the common description of the development of the x-ray technique (von Laue 1964, Bragg and Bragg 1937), an unlikely and highly fortuitous combination of circumstances made the discovery of the technique at Munich possible. These were the understanding that atoms in a crystal formed a space lattice (because Sohncke had worked at Munich), and advocacy of the wave theory of x-rays. Thus the 'birth' of the technique is given heightened significance. Forman attempts to debunk this, claiming to show that the assumption of the space lattice was widespread, and also that the wave theory of x-rays, in the form it was in at the time, which was actually that of a pulse theory, did not suggest that x-ray diffraction by crystals was possible. He also claims that the conventional accounts accentuate the difficulties of the initial experiments, again to enhance the mystique of the discovery. Forman generalises his case, maintaining that there is no reason to accept historical assertions of scientists, because they place no value on historical fact, apart from questions of priority. The historical accounts of scientists should be analysed, not for historical content, but for socio-cultural information.

In response to Forman, Ewald (1969) attempts to refute his analysis, criticising most of his assertions, with varying degrees of plausibility. The points brought out are that it is an extremely difficult task to decide from written evidence what was really believed

at a certain time, but also that, after many years of propagation of quasi-history/myth, even those involved in the events may come to believe it.

Conclusion

The origins of quasi-history have been discussed, and a large number of examples have been presented. In the second part of the article we shall discuss the results that this quasi-history may have on the attitudes of students and others towards physics and, indeed, science in general.

References

- Badash L 1972 *Isis* 63 48
Blake C S 1955 *Mind* 64 61
Born M 1923 *Naturwissenschaften* 11 537
Bragg W H and Bragg W L 1937 *Current Science* (supplement) 9
Bromberg J 1967 *Archs Hist. Exact Sci.* 4 218
Bromberg J 1968 *Am. J. Phys.* 36 142
Brush S G 1969a *Phys. Teach.* 7 271
Brush S G 1969b *Phys. Today* January p9
Brush S G 1974 *Science* 183 1164
Butterfield H 1931 *The Whig Interpretation of History* (London: Bell)
Campbell N 1952 *What is Science?* (New York: Dover)
Dougal R C 1976 *Phys. Educ.* 11 438
Eisberg R M 1961 *Fundamentals of Modern Physics* (New York: Wiley)
Ewald P P 1969 *Archs Hist. Exact Sci.* 6 72
Forman P 1968 *Isis* 59 156
Forman P 1969 *Archs. Hist. Exact Sci.* 6 38
Hermann A 1971 *The Genesis of Quantum Theory (1899-1913)* (Cambridge, Mass.: MIT Press)
Holton G 1969 *Isis* 60 133
Jackson J D 1962 *Classical Electrodynamics* (New York: Wiley)
Jammer M 1966 *Conceptual Development of Quantum Mechanics* (New York: McGraw-Hill)
Jammer M 1972 *History in the Teaching of Physics: Proc. Int. Working Seminar on the Role of History of Physics in Physics Teaching* (ed S G Brush and A L King) (Hanover: University Press of New England) p97
Jeans J 1905 *Phil. Mag.* (6th series) 10 91
Klein M J 1962 *Archs Hist. Exact Sci.* 1 459
Klein M J 1963 *The Natural Philosopher* 1 81
Klein M J 1966 *Phys. Today* November p23
Klein M J 1972 *History in the Teaching of Physics: Proc. Int. Working Seminar on the Role of History of Physics in Physics Teaching* (ed S G Brush and A L King) (Hanover: University Press of New England) p21
Kuhn T S 1971 *Boston Stud. Phil. Sci.* (ed R C Buck and R S Cohen) 8 141-3
Kuhn T S 1976 *Am. J. Phys.* 44 1231
Lakatos I 1970 *Criticism and the Growth of Knowledge* (ed I Lakatos and A Musgrave) (London: Cambridge University Press) pp138, 140, 146
Lakatos I 1971 *Boston Stud. Phil. Sci.* (ed R C Buck and R S Cohen) 8 106

- von Laue M 1964 *Nobel Lectures in Physics* (Amsterdam: Elsevier)
Lovejoy A O 1939 *J. Philosophy* 36 477
Marwick A 1973 *The Historical Perspective* (Milton Keynes: Open University Press)
Mukherji V 1974 *Archs Hist. Exact Sci.* 13 27
Phillips R A 1973 *Am. J. Phys.* 41 751
Planck M 1900 *Verh. Dt. Phys. Ges.* 2 237
Popper K R 1960 *The Poverty of Historicism* (London: Routledge)
Porter R 1976 *Brit. J. Hist. Sci.* 9 91
Rayleigh Lord 1900 *Phil. Mag.* (5th series) 49 539
Rayleigh Lord 1905 *Nature* 72 54
Stuewer R H 1971 *Brit. J. Hist. Sci.* 5 258
Stuewer R H 1975 *The Compton Effect: Turning Point in Physics* (New York: Science History Publications)
Thorndike A M 1952 *Mesons, A Summary of Experimental Facts* (New York: McGraw-Hill)
Tonks L 1967 *Am. J. Phys.* 35 857

A small wind tunnel made of polystyrene

JOHN DOWNIE†
*Department of Mathematics,
University of Edinburgh*
FRANCIS BARNES
*Department of Physics,
University of Edinburgh*

Schools have always shown an interest in wind tunnels, and in Scotland experiments concerned with air flow have been popular as topics for the certificate of sixth-year studies project. The usual difficulties experienced by the students are the amount of time, manual skill and money needed to build a satisfactory tunnel. This article gives a brief description of open-circuit wind tunnels of rectangular cross-section and of how expanded polystyrene can be used to build a small one. Polystyrene tunnels are cheap, very easily made and readily modified.

The simplest wind tunnels are open-circuit tunnels.

† Now at Department of Mechanical Engineering, University of Sussex