Methodologies and myths

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Method and Appraisal in the Physical Sciences: The Critical Background to Modern Science, 1800–1905. Edited by Colin Howson. Pp. vii+344. (Cambridge University: Cambridge and London, September 1976.) £10.50.

"THIS volume constitutes the first collected edition of work so far done in illustrating an important new development in the philosophy of science, 'the methodology of research programmes' ". So claims the editor of this collection of seven monographs which exemplify and examine the ideas of the late Imre Lakatos (who is the posthumous author of the opening paper) regarding the philosophy of science. Five of the papers deal with selected case histories, three in physics (kinetic theory/thermodynamics, wave theory of light, and relativity) and two in chemistry (oxygen/phlogiston, and Avogadro's hypothesis).

The self-styled 'importance' of the new development may make the reader bridle: and he may well ask what a research "program" is. It suggests a deliberate plan by an individual or a group, and it ought therefore to exclude those developments in science where there has been no conscious planning; any attempts by subsequent philosophers to fit such events into a plan will be falsely based. With these prejudicial observations I have attempted, as an experimenter interested in the history of science, to read the book.

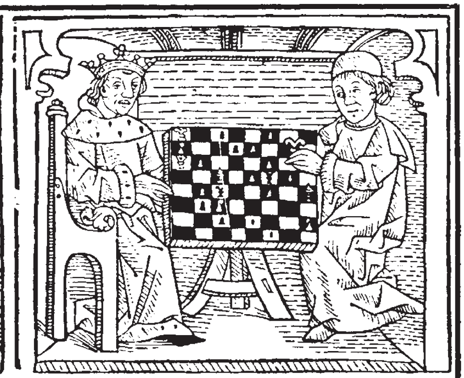
As for the philosophy, there is so much unfamiliar jargon that I can only grasp at clues to determine whether or not it is worth mastering. Lakatos discerns four "logics of discovery", of which one is "methodological falsificationism", and his exposition is strewn with terms of comparable ponderosity. So, rebuffed and bewildered, I have retreated to sample two of the case histories to see how his followers have applied his methods in instances where the language and the events are more familiar to me.

In the course of asking "Why did Einstein's programme supersede Lorentz's", Elie Zahar claims that his particular ideas of how physics may be furthered by translation into mathematics are illustrated by an account given by Peierls of the way in which Maxwell arrived at his famous equations. Although Zahar credits Peierls with this account (1963) it had appeared in various textbooks of physics long before, and—as Peierls himself took pains to state—there is no evidence in Maxwell's writings that the latter thought, consciously or subconsciously, in the way described. Briefly, the invention of the displacement current is credited to a desire by Maxwell to balance one of his equations of the electromagnetic field. But in his original (1864) paper Maxwell himself gave his own, quite different, line of thought. He said that he assumed that the aether was a medium of "small but real density" capable of being polarised in a similar manner to a material dielectric; and that while polarisation was taking place there was a displacement of charge which "does not amount to a current because when it has attained a certain value it remains constant, but it is the beginning of a current". Peierls discounts this, by saying frankly that "Maxwell arrived at the extra term by using a picture that we do not accept today". But the question is not what Maxwell might have done if he had present-day knowledge, but by what route he in fact arrived at the outstandingly imaginative concept of a displacement current *in vacuo*. Maxwell himself recorded his approach clearly; and this was by physical analogy rather than by mathematical analysis. So, although most of us are aware that mathematics may suggest creative steps in physics, Maxwell's displacement current is not an example, and its history would have to be bent to fit Zehar's philosophy. Perhaps he is not to be blamed unduly for relying on so eminent an authority as Peierls, but he fails to quote the reservation, made by the latter himself, which completely destroys the argument.

I have to emphasise that I have only sampled Zehar's monograph and it would require much effort to treat it in detail; it has 141 footnotes and 82 references. Peter Clark's study of "Atomism Versus Thermodynamics" runs to 244 footnotes and 196 references, and John Worrall's "Thomas Young and Newtonian Optics", 252 footnotes and 83 references. Such assiduity cannot be reviewed at length, but one further sample will illustrate why to me the book is dangerous. It concerns the famous two-slit interference experiment of Thomas Young.

Worrall says: "There are, I claim, sufficiently many suspicious aspects of Young's account of the two-slit case to support the belief that he never performed the experiment"; and "However, as I have said, there is evidence that Young never performed the experiment"; and once again "Moreover, the dearth of details in Young's account makes it seem unlikely that Young ever did successfully perform it, and certain that he did not give sufficient information about the experiment to ensure its repeatability by others".

What are the facts? Worrall makes play of the point that Young did not give a full account of the experiment, which he described only briefly in his course of lectures at the Royal Institution between 1802 and 1806. Worrall says that when Young really did an experiment he usually gave enough



Illustrations taken from *The Game and Play of the Chess* by Jacobus de Cessolis. Pp. 174. (Scolar: London, September 1976.) £12.50; special edition £30. Facsimile edition of Caxton's translation (printed 1482) of an allegorical work "on human morals and the duties of noblemen", which uses the chess pieces to represent the stations and duties of life. To mark the quincentenary of the establishment of the first printing press in England, the British Library, Great Russell Street, London, has mounted a major exhibition of Caxton's work as printer and publisher. (Closing date: January 31, 1977.)