mathematics is, and to assist it to realize more completely its own potentiality. To the question "What is mathematics?" Bernays is able to give a succinct answer: "Mathematics is a study of structure"—words that might well be echoed by any present-day mathematician, influenced as he must be, to a greater or less extent, by the Bourbakist tradition. Perhaps the word 'structure' does not mean quite the same for Bourbaki as for Bernays, but there is certainly a very substantial overlap; and this is no mere coincidence, since the Bourbaki enterprise itself has been said by one of its early participants to have developed from the influence of the Göttingen ambience on certain young French mathematicians who felt themselves trapped within a tradition, moulded in the great age of the *Cours d'Analyse*, that had eventually become moribund. And so it is that we are able to sense, in this early thinking of Bernays, a noteworthy harmony of outlook between the mathematician and the philosopher of mathematics. This is one of the reasons why these essays make such stimulating reading.

Mathematics, as the study of structure, is at one and the same time intuitive (*anschaulich*) and conceptual. It is securely grounded in intuition, and yet it goes beyond intuition into a world of conceptual idealization, itself reciprocally involved with the world of direct experience. Bernays sees the philosophy of mathematics as part of a much more comprehensive study, and as a philosopher he is strongly drawn to the views of Ferdinand Gonseth, whose thesis that scientific theories are schematic representations of nature rather than embodiments of nature itself has something in common with his own understanding of mathematics as a conceptual study of structure.

Bernays does not believe in the possibility of a reduction of mathematics to logic, as something yet more general and more fundamental; for theoretical logic, being structural, is properly an application of mathematics to the subject-matter of logic, just as mathematical physics is an application of mathematics to that of physics. Nor can he accept the claim of Brouwer's Intuitionism to be a more valid conception of mathematics than the classical one. Intuitionism is just too restrictive to do justice to mathematics as it already exists. The various schools of thought in the philosophy of mathematics each offer valuable insights into the nature, or potential nature, of mathematics, but every such insight is partial. No theory of mathematics and its foundations can ever be definitive, and no ultimate solution to the great foundational problems is to be envisaged. Once again we may detect a Bourbakist echo, recalling a famous paper read by André Weil in 1948, and to be found at the beginning of vol. 14 of the *Journal of Symbolic Logic*.

Bernays's fundamental belief that we must take mathematics for what it is, and seek by philosophical criticism to understand, to improve, and to develop it, comes out particularly clearly in his article on Wittgenstein's *Remarks on the Foundations of Mathematics*. He is plainly unsympathetic to Wittgenstein's approach, and feels that Wittgenstein cannot really get to grips with mathematics because the examples which he considers are too trivial to exhibit the real nature of mathematical assertions or proofs. As he observes somewhat testily, "Wittgenstein goes on as if mathematics served for hardly anything but household management". The remark may not altogether do justice to Wittgenstein, but it certainly shows how much mathematics matters to Bernays, the philosopher of mathematics.

G. T. KNEEBONE

Can Theories be Refuted? Essays on the Duhem-Quine thesis. Edited by SANDRA HARDING. (Reidel Publishing Company. 1976. Pp. xxi+318. Price Dfl. 110.-.)

This is a book of readings designed to take the reader through the convoluted turnings of the "Duhem-Quine thesis" and out into the light of post-"naive falsificationist" methodology.

Popper has claimed that there is an asymmetry between verification and falsification.

Theories may not be conclusively verifiable but they are ("conventionalist strategems" aside) conclusively falsifiable. This claim is spelt out in some excerpts from *Logic of Scientific Discovery* and *Conjectures and Refutations*. However, Popper's claim had been contested some twenty years before he had first made it by the French physicist/philosopher Pierre Duhem. A chapter from his *Physical Theory: Its Aim and Its Structure* is reprinted. More recently Duhem's ideas have been revived and given new life. Quine's "Two Dogmas of Empiricism" is the most obvious place, but Hempel's ideas on the status of theoretical entities and Kuhn's views on the incorrigibility of paradigms are other examples. Quine's essay is reprinted, as are Hempel's paper "Empiricist Criteria of Cognitive Significance" and a chapter from Kuhn's *Structure of Scientific Revolutions*. Adolf Grünbaum has attacked Duhem's view in a number of places, and his criticisms are represented by one of his essays and a chapter from *Philosophical Problems of Space and Time*. However, Grünbaum's arguments have themselves been attacked and are dealt with here in papers by Laudan, Giannoni and Wedekind.

At this point the balance of arguments is in favour of Duhem. This raises the obvious question "If theories can be neither conclusively confirmed nor refuted, what basis can we give for our knowledge?". The book ends with three important papers suggesting new approaches to the problem. Mary Hesse develops and expands Quine's "network theory" in her "Duhem, Quine and the New Empiricism"; Lakatos explains his methodology of scientific research programmes in his paper reprinted from *Criticism and the Growth of Knowledge* and Feyerabend gives us a dose of methodological anarchism in an excerpt from "Against Method". Thus, as the Duhem-Popper controversy disappears into the background, the reader is left pondering the possibilities of the new landscape.

Now the topic of the book is certainly of central importance in the philosophy of science and the book contains the arguments of most of the major philosophers of science of the last twenty years. It goes without saying, therefore, that the material in the book is well worth reading. However, I do wonder at the rationale for having the book in this form. Firstly, there is no new material in the book at all (with the exception of a previously unpublished half-page letter from Quine to Grünbaum). Even the introduction merely makes a précis of the rest of the volume. Further, anyone who is interested in the subject will already have (or at least will want to have) the important books from which most of these snippets are taken. Hence it would seem that the only point of the book is to provide a collection of readings which will introduce students to the topic. But why then include excerpts from books such as *Logic of Scientific Discovery* and *Structure of Scientific Revolution* which are standard students' books anyway, and papers such as Hempel's or Quine's which are reprinted in numerous collections already? (If "Two Dogmas of Empiricism" is reprinted again, it should be in the *Guinness Book of Records*.) Admittedly there is a certain convenience in having the papers collected together. However, the main effect of reprinting all this standard material is to push up the price of the book well beyond the price range most students can afford. Finally, if the book was intended as an introduction, then a bibliography or a guide for further reading is indispensable. Yet it seems to have been dispensed with.

Hence I find myself in the apparently paradoxical situation of recommending the contents of the book, but not the book itself.

GRAHAM PRIEST

Explanation. Edited by STEPHAN KÖRNER. (Oxford: Basil Blackwell. 1975. Pp. viii +219. Price £5.50.)

This book assembles the contributions—four principal papers, each accompanied by two comments from other hands and a rejoinder—to a conference held it is not said just where or when but which from internal evidence I hypothesize to have been held at Bristol during 1973.