

Blindness in pursuit of science: A Companion to the Philosophy of Science
April 6, 2001

Book Review by Ray Scott Percival in the Times Higher Education.

A Companion to the Philosophy of Science is an ambitious project given the rapid growth and specialisation of science in the past 50 years. The essays are naturally short but sweet, wide-ranging, mostly well referenced, and serve as excellent aperitifs. There are 81 entries and a useful index.

The book has many distinguished contributors, but it would have been refreshing to have even one non-justificationist, especially as three well-known inductivists are allowed more than one broadside each without a rejoinder. A reader might be forgiven for thinking that no useful philosophy of science occurs outside government academe. But including independents aids debate and avoids mutual-admiration-society syndrome.

W. H. Newton-Smith as editor could usefully have included contributions from scientists, engineers or thinkers in other fields that touch on science. Naturalism, an attempt to blur the distinction between what is the case and what should be the case, is a powerful fashion in philosophy, as evidenced in this book, and it deserves a focused critical commentary. Thomas Kuhn notoriously claimed that "(no) process yet revealed by the historical study of science at all resembles the methodological stereotype of falsification by direct comparison with nature".

My reply is that no unqualified statement yet revealed by the historical study of Karl Popper's works at all resembles this stereotype of falsificationism. It was Kuhn who instigated (and Imre Lakatos who reinforced) the myth that Popper had begun with this "naive falsificationism". Lakatos also asserted that if a proposed method of science is contradicted by the best examples of scientific achievement, that proposed method should be rejected. Brendan Larvor, who explores the relationship between methodology and the history of science, though a recent proponent of this line, provides no example of scientific achievement that actually flouts Popper's method in his entry. This suggestion is scandalous in its pernicious, though no doubt inadvertent, elevation of science to ultimate authority. Science must be bold, but let us recall Newton's saying that he was like a boy playing on the seashore beside the great ocean of truth.

Even our best efforts to chart just the shores may utterly fail. To preserve the openness of science to new ideas and methods, we must accept the possibility that the whole of science may be wrong. Suppose Kuhn, Lakatos and Larvor had been writing at the time of Copernicus and had won over the population to their view. Any suggestion that it might be an improvement of method to predict unknown types of appearances instead of just explaining in more convenient and systematic ways known knowledge would have been mercilessly crushed.

Popper's proposed method of bold conjecture and refutation, though inspired by an examination of what scientists have actually done, is not a description of what they did or do, but rather a prescription of what a scientist ought to do if he wants to promote the growth of

knowledge. If we consider cookery and cooks, what cooks should do neither follows from nor is refuted by what the best of them have done, even though a study of their methods might lead to better methods. Ronald Giere concedes this point, but he suggests that if we had a theoretical explanation of scientists' behaviour (not just a description), we could devise better methods, just as explanatory theories in physics tell us how to build bridges. Here Giere, Newton-Smith and Mary Tiles in other entries, confuse science and technology. They overlook the fact that laws of science tell you only of a "given" set of precisely described bridges, what loads they can bear, but not how you should design or build one in the first place. Bridges, MP3 players, cars and such are not contained in scientific theories waiting to be logically derived therefrom. All technologies have to be invented.

Are proposed methods outside criticism? Just how can we judge our methods? We can check them for internal logical consistency and for their logical consistency with one another. We can also see if they are consistent with a logical analysis of the statements of science (laws of nature, for example). If it is a proposed method of inference (such as induction), we can check its logical validity.

In his stimulating entry on "discovery", Thomas Nickles seems, like Giere, to be also after an "explanatory" method that will lead one to justified and reliable new knowledge. Nickles discusses the Popper/Campbell point that the generation of new knowledge must be by totally blind forays into the unknown. It is important to note that there can be degrees of blindness. But Nickles overlooks this when - pointing to the incorporation of blind variation and selective processes into computer "discovery" programs - he seems to think that he has undermined the Campbell/Popper claim that this Darwinian-like process is not a method of discovery in the above sense. The fact is, these programs have specific designed constraints: specific initial populations of variants, and specific criteria for selecting variants for closeness to an optimum solution to a well-defined problem. All of this already embodies considerable "higher-level" conjectural "blind variations".

Despite these criticisms, *A Companion to the Philosophy of Science* is a delightful and clear book. It is highly recommended for critical undergraduate students and anyone with wide intellectual interests.

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A Companion to the Philosophy of Science

Editor - W. H. Newton-Smith ISBN - 0 631 17024 3 Publisher - Blackwell