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BOOK REVIEW

Marcel Weber: Philosophy of Experimental Biology

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Philosophers have committed sins while studying science, it is said—philosophy of science focused on physics to the detriment of biology, reconstructed idealizations of scientific episodes rather than attending to historical details, and focused on theories and concepts at the expense of experiments. Recent generations of philosophers of science have tried to atone for these sins, and by the 1980s the exculpation was in full swing. Marcel Weber's *Philosophy of Experimental Biology* is a zenith *mea culpa* for philosophy of science: it carefully describes several historical examples from twentieth century biology to address both 'old' philosophical topics, like reductionism, inference, and realism, and 'new' topics, like discovery, models, and norms. Biology, experiments, history—at last, philosophy of science, free of sin.

The mere existence of this book is a service to the discipline. One of its strengths is the amount of philosophical and scientific territory it covers. With the vantage of several detailed case studies, the nine chapters serve as both accessible introductions to core problems in philosophy of science, and as demonstrations of how these problems can be freshly addressed by paying attention to historical details of experimentation in biochemistry. For instance, in Chap. 4, after discussing the 'oxidative phosphorylation controversy', Weber has a section called 'Why Biochemists Are Not Bayesians' (p. 108). Moreover, this book is a contribution to the re-emergence of excellent work in general philosophy of science. One often hears that philosophical inquiry into scientific realism, methodology, conceptual change, and explanation have lost momentum compared to philosophical inquiry into conceptual matters in the special sciences. This book challenges such a trend.

Weber confronts a tension in the discipline of philosophy of science (and philosophy more generally) between descriptive and normative accounts of its subject matter. The cartoon dialogue goes like this: Positivists and Popper said "scientists should do it this way"; Kuhn and other historically-minded philosophers

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and naturalized epistemologists said "but scientists don't actually behave that way"; and recently more daring philosophers respond by saying "perhaps not…but they should!". Weber falls squarely in the descriptive camp. The trouble is that it is hard to see how a philosophical methodology so committed to a descriptive approach can be used to refine or refute normative philosophies of science; this limits the philosophical projects to which the book can contribute.

Fiery debate has already ignited from the second chapter, on reductionism and explanation. Weber argues: molecular biology attempts to provide reductionist explanations, physiochemical laws are needed to undergird such explanations, and Cummins-style functional analyses are a good way to think about explanations in experimental biology. Critics have elsewhere responded—see for example the December 2008 issue of *Philosophy of Science*, in which Weber debates with Craver (2008) and Bogen (2008)—and so I will not discuss this chapter here; nevertheless, the fact that it has sparked such debate speaks to the book's importance. This chapter can be usefully read with Chap. 8, on developmental biology, where Weber discusses the main criticisms of a reductionist account of genetics, in which deoxyribonucleic acid (DNA) is given a special causal status. With a sharp argument, Weber concludes that we should accept the claim by developmental systems theorists that DNA is not a master molecule, but not for the reasons that developmental systems theorists have given; rather, if we accept John Mackie's account of causes as INUS conditions (insufficient but necessary part of an unnecessary but sufficient condition), then DNA must be construed as an INUS condition, along with many other INUS conditions.

The tension between descriptive and normative accounts of science is exemplified in Chap. 4, on scientific inference; this is also the weakest chapter of the book, which is unfortunate, since it is probably of most interest to readers of *Erkenntnis*. Weber criticizes previous theories of scientific inference, and gives an account of his own. For example, he has two answers to the titular question of why biochemists are not Bayesians: one is familiar, the other is back-to-front. The familiar criticism is that Bayesianism requires too much subjective guess work to estimate prior probabilities. The back-to-front criticism of Bayesianism is historical: in the case of the oxidative phosphorylation (ox-phos) controversy, "the Bayesian approach could be used to show that chemiosmotic theory ought to have been accepted *much earlier*" (pp. 110–111). That is, had biochemists been Bayesians, they would have accepted what is now considered to be the true theory (the chemiosmotic theory) much earlier than they actually did. This is supposed to be a problem for Bayesianism, rather than a virtue, because the biochemists did not in fact judge the chemiosmotic theory to be true until years after the point at which Bayesianism could have rendered a positive verdict for the chemiosmotic theory. Since the normative Bayesian account contradicts the actual historical details of biochemistry, the problem according to Weber is with the normative Bayesian account and not with the science, since "the philosopher of science is well advised to take the actual judgments of the scientific community very seriously" (p. 111). But Weber himself argues that Bayesianism could have rendered a correct verdict on the hypothesis quicker than the correct verdict was actually reached, which, one might reasonably think, is not a vice but rather a virtue of a normative account of science. Veracity to the behavior of the objects of scientific study, rather than correspondence to the behavior of scientists, should be a standard for *normative* accounts of science. The converse is true for *descriptive* accounts of science. Weber has blurred this distinction. One could still say: Biochemists Should be Bayesians.

Resolving the ox-phos controversy was possible only when there was a "*combination* of all the reconstitution experiments done in Racker's laboratory that provided the crucial evidence" (p. 108). This is one of the many interesting ideas that Weber peppers his book with. A table lists the various modes of evidence in favor of the chemiosmotic hypothesis and the various modes of evidence in favor of its competitor, the 'chemical hypothesis' (p. 104). The evidence from the various modes was discordant (call this *discordant multimodal evidence*), with several modes favoring one hypothesis and several modes favoring the other. In the end, strictly speaking, there was never a "combination" of the multimodal evidence, since scientists, then as now, do not know how to systematically combine evidence from different kinds of experiments. Rather, it was a *consideration* of evidence from different kinds of experiments that compelled most biochemists to accept the chemiosmotic hypothesis. Weber calls these reconstitution experiments "crucial", but he notes that there was not a *single* crucial experiment. In an earlier discussion of the ox-phos case, Allchin (1992) describes the eventual evidence as an "ensemble of empirical demonstrations" rather than crucial. Weber does not further pursue the question of how multimodal evidence can be assessed concomitantly to decide between competing hypotheses; he "does not think that there exist sound methodological principles that would allow this" (p. 105), though he does not argue for this. My view is that, given the ubiquity and epistemic importance of discordant multimodal evidence in contemporary science, policy, and law, the task of developing and justifying methodological principles to assess and amalgamate multimodal evidence should be a priority for theoretical scientists and philosophers of science. Nevertheless, the discussion of discordant multimodal evidence is one of the many gems of this book, and the idea deserves greater philosophical attention. A similar gem is placed in an endnote: the resolution of the above controversy came only after a "plausible mechanistic explanation" of the victorious theory was available (p. 305), which is yet another idea for future philosophers of science interested in methodology to further investigate.

The normative-descriptive tension is also prominent in Chap. 3, on discovery. Weber reviews the reasons why Reichenbach, Popper, and others held the distinction between the context of discovery and the context of justification and why they thought that philosophical analysis was only suitable for the latter. Discovery later came to be deemed fit for philosophical inquiry because "the concept of rationality includes more than just formal logic" (p. 54). Other processes that came to be seen as part of the purview of rationality include the generation of scientific theories and the patterns of scientific discovery. Of course, one might ask: if such processes are inconsistent with formal logic, are they still rational? If your answer is no, then perhaps, contra Weber, there is nothing more to the concept of rationality than formal logic; if your answer is yes, then perhaps our concept of rationality has become deracinated. Three recent philosophical accounts of discovery in biology—by Kenneth Schaffner, Lindley Darden, and Frederic

Holmes—support Weber's claim that philosophical analysis can try to "exhibit the *rationality* of the mental procedures used by scientists when generating new theories" (p. 86), despite the fact that, according to Weber, these procedures are not general across scientific disciplines.

Although a strength of the book is the amount of philosophical territory it covers, this is also its major weakness. Much of it reads like a summary literature review, both of the philosophy and of the science. Occasionally we are served choppy one-paragraph histories of rich episodes in early molecular biology (see p. 159, for example). Complicated questions like scientific realism and robustness get a small handful of pages. Each chapter has a review and short critique of arguments from history and philosophy of science, and then Weber's own view as a replacement. Some of these replacements are brilliant, while others are uncompelling. An example of the latter comes after criticizing Bayesianism and Deborah Mayo's notion of severity testing: Weber suggests that biological experiments involve controlling qualitative error to eliminate experimental artifacts. Controlled experiments allow scientists to employ J. S. Mill's "method of difference" (p. 120); Weber is perfectly correct, but: (a) Bayesians can account for the method of controlled experiments; (b) Mayo's severity testing can account for the method of controlled experiments; and (c) many other methodologists not discussed in the chapter can, in their own ways, account for the method of controlled experiments. An example of an exciting replacement comes in Chap. 7, on reference and conceptual change, in which Weber introduces the notion of "*floating reference*"—discussing the various concepts of the gene in classical and molecular genetics, he writes "changes in experimental methods and in theory continuously altered both the concept's modes of reference and its extension" (p. 227). Despite lacking an essential and unchanging reference, the gene concept was extremely fruitful. Weber modestly claims that the notion of floating reference does not generalize to the physical science, but one wonders.

Model organisms are the subject of Chap. 6: the history of their use in laboratory research, how we can understand what model organisms are with respect to more traditional modes of experimentation, and the limits of possible knowledge from experimenting on model organisms. This chapter is both interesting and frustrating. There is not much good work by philosophers on model organisms, and Weber (here and elsewhere) has done more than anyone to begin such an inquiry. This chapter is full of interesting ideas; for example, Weber writes "if we look at the history of twentieth-century biology, we find many examples of biologists who overgeneralized findings that they obtained with a particular organism" (p. 156). He considers and rejects work by Robert Kohler and others who consider model organisms to be tools, instruments, technological artifacts, or systems of production. His reason for doing so is curious: living organisms cannot be artifacts, simply in virtue of the fact that they are alive. Calling living things 'tools' is simply a "metaphor", according to Weber, and somehow it follows from the fact that they are alive that model organisms cannot be, strictly speaking, tools. Weber has a different word for the role that model organisms can serve: "*research materials*" (p. 186). It is hard not to see this as a poorly-founded quibble. However, this allows Weber to introduce the concept of "preparative experiments", which are not aimed at directly testing a hypothesis, but rather are aimed at developing research materials and knowledge of these research materials, which will be subsequently used to test hypotheses. Weber suggests that "a great deal of experimental work in biology is preparative", and so without the notion of preparative experiment we would be missing much of biological practice (p. 175).

The chapter on model organisms includes an insightful discussion of a contrast between inferences based on parsimony compared with inferences based on inductive enumeration or extrapolation (pp. 180–191). Scientists think that many mechanisms are shared amongst all species, or at least most. But these mechanisms have been described in only a tiny fraction of species. Thus, enumerative induction cannot justify the belief that most species share the same mechanisms. Weber draws on previous work by Elliot Sober, Matt Haber, and others, and suggests that an argument from parsimony supports the assumption that mechanisms are phylogenetically conserved, which is a necessary assumption to extrapolate knowledge inferred from model organisms to general knowledge of all species.

One of the more interesting sections comes at the end of the book, in Chap. 9, on realism. Weber discusses 'The Argument from Independent Determinations' and illustrates the argument with the controversial example of 'mesosomes' (p. 281). Mesosomes are artifacts created by techniques of sample preparation for microscopy, but were once thought to be real cellular structures. The argument from independent determinations has also gone under the name 'robustness' or 'concordant multimodal evidence', and the argument has the structure of an inference to the best explanation or a no-miracles argument: it would be a miracle if multiple modes of evidence confirmed x (where x is an entity, or a process, or a constant, or a relation), and x is not true; we do not accept miracles as compelling explanations; thus, when multiple modes of evidence confirm x, we have strong grounds to believe that x is true. In a series of papers in the 1990s, mesosomes served as an example for philosophers who argued for and against the veracity of robustness arguments given the vicissitudes of the purported reality of mesosomes, and vice versa, mesosomes served as an example for philosophers who argued for and against the reality of mesosomes given the vicissitudes of the purported veracity of robustness arguments.

Profound insights have been made by philosophers engaged in careful descriptive enterprises—think of Thomas Kuhn, Hans-Jörg Rheinberger, and Hasok Chang. Weber is in good company. He describes this company throughout the book, and especially in Chap. 5, on experimental systems, which can be usefully read together with the fourth chapter, on scientific inference. However, let me restate my worry. Some traditional philosophical questions are primarily descriptive, and thus a descriptive approach to them is appropriate. Candidates in this class could include reductionism and reference change. Other traditional philosophical questions are primarily *prescriptive*, and the prescriptions are made with the force of logical arguments, and so a descriptive approach, in which counter-prescriptions have force derived from empirical conclusions, is less appropriate. Candidates in this class could include arguments for and against realism, and norms of scientific inference. Of course, this distinction between the normative and the descriptive has been vigorously disputed, and exemplifies a tension in all of philosophy—consider

Quine, or consider the recent 'experimental philosophy' movement. Not all will agree with this distinction, but it is, nevertheless, why some chapters of Weber's book are uncompelling, while other chapters are insightful.

I would recommend this book to senior undergraduates, graduate students, and to any philosopher of science interested in experimental biology. Despite several flaws discussed above, this book achieves a fine balance between being a valuable research manuscript and being a valuable text for undergraduate education—akin to Ian Hacking's (1983) *Representing and Intervening*. However, the price of this book, as with most texts in the Cambridge Studies in Philosophy and Biology series, is high; let us hope for a paperback edition. It will serve as a valuable introduction to problems in philosophy of science illuminated by a focus on experimental science, and as an introduction to several important historical episodes in molecular biology and biochemistry. Gems are strewn throughout the book, unpolished, ready to be picked up, admired, and with hope, to be further polished so they can really shine.

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