Biology and Epistemology

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Introduction

RICHARD CREATH AND JANE MAIENSCHEIN

Epistemological issues have always been at the heart of philosophy of science. After all, science considered as a product is a set of organized knowledge claims. Considered as a process or institution, science is the (more or less) organized attempt to devise and defend – that is, to justify – such claims to know the world around us. Just as scientists find and refine their evidence and articulate their arguments so that this bit of data bears on that bit of theorizing, so philosophers have sought general accounts of what scientific observations and arguments can be. Taken together, these accounts comprise a theory of scientific knowledge. And while it may not be the only way to look at science, the attempt to say what makes it *science* is bound to remain a central concern.

Within the past two decades or so, philosophy of biology has emerged as an important and recognized specialty within philosophy of science. For the first time, large numbers of scientifically well-informed philosophers (often joined by historians and reflective biologists) have systematically examined the vast domain of biological work. Are biological claims to knowledge justified in some *different* way (perhaps because those claims are historical, or about life, or about systems unmanageably complex, or perhaps because we have an emotional or political interest in the outcome)? Or is biology fundamentally similar to nonbiological sciences, with similarities that would have gone undetected but for the examination of biology on its own terms?

For the most part, the studies that have appeared so far in the philosophy of biology have concentrated on the character of biological theories (especially evolution) and concepts (e.g., species and individuals as units of selection). As interesting as these interpretive, semantic, and ontological questions are, they do not focus on specifically epistemic concerns. There have, of course, been volumes written about so-called evolutionary epistemology. But these have sought the structure of an answer to the question "why do some theories survive?" in the structure of evolutionary theory. However interesting, this is a long way from answering the epistemic questions about evidence and argument and a long way from the consideration of the full range of biological sciences.

Certainly there has been epistemological work about biology, and from a rich variety of sources, too. What has not appeared is a single volume that focuses on biology and epistemology, that brings together and into focus epistemological work on the full range of domains within biology, and that likewise brings together work focused on biology that illuminates a range of epistemological issues. This volume places the individual efforts within the context of the general and common concerns, and we believe that the result will be of value to philosophers and historians of science, to biologists, and even to those interested in some aspects of science policy.

The volume is organized into three sections, though there are many overlaps and a number of the chapters could have fallen into more than one section. The first focuses on a central idea of the nineteenth century: evolution and its contemporary philosophy of science. What view did Darwin and leading evolutionists hold concerning the nature of evidence and its relation to theory? What was the relation of philosophy of science to biology? *Michael Ruse* considers "Darwin and the Philosophers," by which he means primarily John Herschel and William Whewell. Philosophical ideas played an important role for Darwin, Ruse contends, and we can even see Darwin's work as an attempt to respond both to Herschel's British empiricist demand for knowledge of *verae causae* through direct experience and to Whewell's rationalist understanding of those same *verae causae* as accessible through a process of consilience of inductions. Indeed, Ruse sees Darwin as constructing his "one long argument" to build a theory with *verae causae* at its very heart. Darwin's efforts to take the middle ground and to satisfy both sets of criteria left him satisfying neither of the philosophers of science he sought to follow. In the "twilight" of their respective careers, Herschel could not accept natural selection as the mechanism for evolutionary change, and Whewell rejected the enterprise altogether. Even younger men more sympathetic to the evolutionary view, like Thomas Henry Huxley, remained unimpressed by the rationalist elements of the approach. In order to understand the reception of Darwin's ideas, Ruse argues, we need to understand the interplay of alternative contemporary philosophical ideas.

Jon Hodge denies that Whewell had a significant impact on Darwin's own standards of scientific acceptance and argumentation. Even from the *Beagle* years Darwin was a disciple of Lyell and Herschel, holding that a science must explain by reference to *verae causae* – that is, causes for which there is evidence independent of the facts they are invoked to explain. By the summer of 1838 Darwin held the commonplace views that it is a virtue to connect disparate phenomena as well as to allow successful prediction and that purely hypothetical conjectures should be replaced where possible. At the same time he planned a book which would separate evidence for the theory from its explanatory use in unifying many different facts.

Darwin then carefully studied Whewell's *History of the Inductive Sciences* and assimilated some views on the a priori. But Whewell made no dent in Darwin's methodological loyalties to Lyell. Certainly Darwin did not learn then about Whewell's proposal for an alternative to the *verae causae* ideal, the consilience of inductions, which appeared only in 1840 after Darwin's theory of natural selection and the arguments for it were already firmly in place. The consilience idea denies a distinction between evidence for a theory and its explanatory use, a distinction that Darwin continued to maintain. According to the consilience idea we get strong verification when (a) the theory explains many different facts and (b) the theory explains many facts unknown when it was first conceived. The first condition is not new, having been defended by Herschel and even Darwin. The second is new, but Darwin must have ignored it, for he never discussed the relevant issues. This was true of Darwin's writing on through the *Origin.* Thereafter he sometimes backpedaled a bit on the *verae causae* ideal, suggesting that it may be too demanding and putting more emphasis on explanatory unification. But Darwin never fully repudiated *verae causae;* Whewell was not his inspiration on explanatory unification; and he never addressed the concerns of (b).

In order to explore the larger question of what effects philosophy of science and science have on each other, *David Hull* also focuses on the case of Darwin and his philosophers. He thus tackles a set of questions similar to Ruse's, but introduces John Stuart Mill into the mix. Mill, as the first truly inductivist philosopher of science, was more representative of the time, when both the "hypothetical" and "deductive" cores of Herschel's and Whewell's philosophies remained suspect. Mill's *System of Logic* provided a method for science, and according to Mill's interpretation of the strict logical standards, Darwin had provided a theory that was logical and that could be true. It could be. But, Mill believed, it was not. A logical and possibly true theory was not a proven theory, and Darwin did not have proof. Instead, a theory based on intelligent design was more defensible for Mill. Thus, the fact that Mill endorsed Darwin's method of constructing a theory did not help Darwin to establish the validity of, or to justify the belief in, evolution by natural selection. What does this tell us about the relation of philosophy to science? Well, Herschel, Whewell, and Mill all rejected Darwin's theories, even though Darwin sought to base his views on their philosophies. Surely, Hull concludes, this calls into question whether philosophy of science and science really help each other very much.

Robert Richards steps back and suggests that most scholars to date have adopted an unacceptable essentialist view of scientific theories. Instead he offers an historical approach, where individual theories are individuals with developmental histories and lineages to which they belong. Thus, any study of theories should proceed historically, and with full awareness of the changeability of individuals over time. In particular, he cites study of Darwin as a problem. Scholars have taken Darwin's theory of evolution by natural selection as if it were one thing, and they have sought to discover its origin and to consider its reception. In particular, they have found it important to deny any progressivist thinking on Darwin's part and to deny that Darwin endorsed embryological recapitulation. That is simply wrong, Richards argues. Darwin was a recapitulationist and a progressivist – in particular ways and in ways that changed over time. Richards follows Darwin in suggesting that "when we regard every production of nature as one which has had a history . . . how far more interesting, I speak from experience, will the study of [the history of science] become!" (p. 84, this volume)

The second set of papers moves to this century and to the virtual explosion of laboratory and experimental research. The papers here explore the nature and use of evidence, considering such central questions as what counts as data, when data counts as evidence, and what role experimentation plays in revealing knowledge about living nature.

David Magnus revisits the naturalist-experimentalist distinction that Garland Allen and others have outlined on many occasions, and explains that what we have is really a case of competing epistemologies. Arguments by Hugo de Vries that changes in populations occur because of mutations, and that those mutations can even be large, conflicted with David Starr Jordan's conviction that it is isolation of parts of the population that leads to difference and change. This, and disputes like it, have been taken as indications of differences in theory about nature. And they have been interpreted as evidence for the existence of a naturalist approach to nature (with a more descriptive, qualitative, speculative, and evolutionary perspective) in contrast to an experimentalist approach (with a more quantitative perspective, more narrowly focused topics, and interest in reduction and micromechanisms). Magnus shows, by contrast, that what lies at the root is a disagreement about what counts as good science.

For Jordan, the naturalist, what matters in science, what he values, is consilience of a variety of lines of evidence, drawing on a diversity of methods. Breadth of theory and holism also are highly prized. Alternatively, de Vries and experimentalists value repeatability. It is not so much the experimentation per se as the possibility of repetition and hence definiteness that matters here. Experimentalists also look for parsimony, hence exhibiting a tendency to reduction, and the resulting rigor that they see in their approach and not in the naturalists' studies. This dispute is not, then, about whether to experiment or not. They all experiment. Rather, it is about the epistemological value of the experimentation. And this is not a tale about conflicting theories or debating individuals but rather a story about competing epistemologies, about what counts as knowledge and as good science.

What it means to be "right" in selected cases in developmental biology serves as the central question for *Jane Maienschein*. She argues that epistemological concerns actually drove the discussion in cases that have usually been taken as classic examples of theory conflict. Caspar Friedrich Wolff and Charles Bonnet in the eighteenth century, Wilhelm Roux and Hans Driesch at the end of the nineteenth, Camillo Golgi and Santiago Ramon y Cajal all argued about versions of preformation and epigenesis. Is the organismal form or the nerve already formed from the very first stages of development, or do they emerge later? In the case of Thomas Hunt Morgan, did he change his mind and move from an essentially epigenetic view to a genetic preformationism when he discovered the white-eyed male *Drosophila* fly, as the story has typically been told? No, these are not primarily conflicts over theory. Rather, they are more interesting stories about what should count as knowledge and about how to achieve "rightness" in biology; these are cases of competing epistemologies. Wolff and Bonnet argued about whether the senses can be trusted; Roux and Driesch about how much can be concluded from one counterexample; Golgi and Ramon y Cajal about which examples are decisive; and Morgan actually held to a consistent set of epistemic values, even while his theoretical interpretations changed significantly.

The electron microscope and PET (positron emission tomography) technologies raise further questions about how much observation can warrant belief. And which observations can we count? *William Bechtel* discusses these two examples and shows that ordinary perception is just the same. In the early stages of using new technologies, there is much concern about artifacts and about the reliability of both the techniques themselves and their interpretations. But the same concerns hold for ordinary perception, and we nonetheless have ways to deal with the concerns. We look to consiliences from other sources of evidence, and for consistency. Thus, we move beyond skepticism to a greater certainty of belief in our interpretations of what we are seeing. In general, then, Bechtel concludes that seeing really is believing – or that it becomes so as the field and technology develop.

The third section examines the nature and role of argument, including issues of object and styles. When there are competing epistemological frameworks, whether because of different styles of work or because of divergent ideologies, how do those differences play out in the science done as a result? The section considers issues of objectivity and other goals in science, and the way those have changed over time in response to a diversity of factors.

Larry Holmes reflects on the unpopularity of the logic of discovery in science among all but a few scholars such as Kenneth Schaffner. He wonders why, and asserts that historians need to work both to remain open to such considerations from philosophy and sociology of science and to retain their strong standards of historical study when looking at actual cases in detail. He focuses on Hans Krebs and his experimental work on the ornithine cycle of urea synthesis and on the citric acid, or Krebs, cycle. These represent "middle-range" theories that neither hold for all cases universally without exception nor are unique single instances. To what extent, Holmes asks, was the ornithine cycle discovered through logical processes? Was there a tight logic like the philosophers' propositional logic? No. But the process was logic and it was "intelligible to reason."

Holmes has followed through every step of Krebs's process, working from exquisitely detailed notebooks recording daily details of reasoning and experimentation. He records those details elsewhere, but here he explores the meaning of such work. What he sees is a process in which discovery and justification remain closely linked at all steps. There is, he explains, a tremendous interplay of thought and operations on a daily basis. Tiny steps of insight and creativity give way to the overall logic and reason. The process is effectively self-correcting, so that we see none of the great "breakthroughs" or revolutionary flashes or leaps that usually count as "discovery." Discovery we have, nonetheless. And it is that prolonged, reasonable process of daily experimental exploration that amounts to discovery.

Richard Lewontin provides a survey and substantial rethinking of issues from his book *The Genetic Basis of Evolutionary Change.* He concentrates on the difficulties standing in the way of knowledge in evolutionary genetics. To use a philosopher's word (one that Lewontin does not use), his subject is underdetermination. There is, he notes, a large number of basic biological mechanisms (forces), each of which enters in quantitatively quite different ways into the historical trajectories of different populations, or into the same trajectory at different times. This is due to differences in the organisms themselves, to external conditions, and to the stochastic nature of the operations of the mechanisms. The direct measurement of the forces is generally impossible, and such measurements of the populations as are available and that provide indirect information dramatically underdetermine the estimates of the quantitative values of those forces.

In view of this, it is perhaps unsurprising that the prospects for knowledge in evolutionary genetics are inversely proportional to the ambitions of the program designed to provide it. A maximal program aiming to provide the correct, universally applicable quantitative account of the forces of mutation, migration, selection, and breeding structures has little or no chance of success. More detail, but of less generality, is possible if we restrict ourselves to a model system. Still narrower is the demonstration that the limited evidence in a particular case is consistent with the occurrence of some theoretically possible process. In many cases the evidence is sufficiently weak that various parameters of a system cannot be independently measured. And where one is measurable, the others can be calculated only with the help of substantial assumptions sometimes amounting to the whole theoretical apparatus itself. Given such constraints, knowledge in evolutionary genetics will be a singular achievement and one that is likely to be highly restricted.

Marga Vicedo uses an example from early twentieth-century genetics to test claims by Ian Hacking (that we can know that certain theoretical entities exist because we can manipulate them in the lab) and by Nancy Cartwright (that we can know that certain theoretical entities enter into causal interactions in the lab and hence exist, even though we do not know the theories into which such entities figure). The example in question concerns William Castle's hooded rats. Castle bred tens of thousands of them at Harvard in order to determine whether observable differences depend on a malleable unit or factor of inheritance. He concluded that because he could breed the rats to exhibit a wide range of hoodedness, there were such malleable factors. E. M. East, by contrast, looked at the same data and concluded that no such malleable unit existed and that the phenomena were due instead to the simultaneous operation of many stable genetic factors. Vicedo shows that not only do the data of the lab get variably interpreted by scientists, they get variably interpreted and reported by historians of science as well. Vicedo concludes that the data do not speak for themselves. We cannot separate the question of whether the supposed entities enter into causal interactions from the question of whether the theories describing the interactions are acceptable. This is because on different theories different entities will be so involved. Similarly, we cannot evaluate the claim that we are indeed manipulating specific unobservable entities without choosing among theoretical accounts of such processes.

The core question of developmental biology, how the zygote becomes a multicelled organism, provides the focus for *Evelyn Fox Keller.* She argues that explanation is not self-evident in science but also functions locally and contingently as it meets the needs of the particular experimental system. Even funding interests may shape the formation of explanations. To make the point, she looks at genetic and epigenetic approaches to development. There are limits to the notion of genes as causes for development, notably the paradox that individual cells having the same genetic material (or information) develop differently. The discourse for gene action takes redundancy as a problem. Yet redundancy manifestly occurs, and epigenetic accounts take it as necessary for an acceptable explanation. Redundancy helps to produce the reliability and stability manifest in developmental systems. Through the examples, we see that the character of the quest for explanation, like the explanation itself, is both local and global, both contingent and contextual

Helen Longino examines five examples of apparent pluralism in biology. In each case, not only do different theories coexist, but the very questions at issue and the epistemic approaches addressing those questions command no consensus. Different data are taken as admissible or relevant; different epistemic or cognitive values are appealed to; and different arguments linking assumptions and practices to aims and goals are accepted. Rather than writing off biology as immature or unscientific, or writing off such epistemic concerns as sociologically irrelevant, Longino sketches a community-based picture of scientific justification which makes room for such pluralism. In this picture, which builds on her earlier work arguing for community-based objectivity, communities are constituted by a selection of substantive and methodological assumptions where the latter can be called the "local epistemology" of the community.

Finally, *Kenneth Schaffner*'s "Afterword" ties together some of the emerging themes of the volume and relates them to recent literature in the history and philosophy of biology. Among the themes he highlights are controversies surrounding forms of empiricism and experimentation as well as issues of pluralism, discovery, and explanation in biology.

Taken together, these essays show the richness and diversity of studies in the biological sciences. They get at core questions in – and about – biology and show a range of philosophical concerns. They do not reveal a need for a philosophy of biology that is fundamentally unique, nor do they demand that study of life requires a special biological epistemology. Yet they do make clear that a close look at such diverse and focused cases within biology is important to allow us to reflect on the role of epistemology and other philosophical concerns. In addition, these studies, taken together, help to develop and deepen our understanding of how biology works and what counts as warranted knowledge and as legitimate approaches to the study of life.