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Matters of Demarcation: Philosophy, Biology, and the Evolving Fraternity between Disciplines

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The influence that philosophy of science has had on scientific practice is as controversial as it is undeniable, especially in the case of biology. The dynamic between philosophy and biology as disciplines has developed along two different lines that can be characterized as 'paternal', on the one hand, and more 'fraternal', on the other. The role Popperian principles of demarcation and falsifiability have played in both the systematics community as well as the ongoing evolution–creation debates illustrate these contrasting forms of interdisciplinary engagement, underscoring the influence philosophy of science in shaping our contemporary understanding of biology in the North American context. However, a strict disciplinary distinction between philosophy and science may itself be a false dichotomy that risks hampering future development of the biological sciences. By actively engaging with philosophical considerations as an integral part of their scientific practice, nineteenth-century biologists offer an interesting counterpoint to current trends of overspecialization and provide a model of scientists who avoided extremes of antagonism with, or subservience to, philosophy.

> Now I think philosophy *should* influence science—and vice versa, of course—because both are reaching for knowledge about the same world. In saying this you'll probably notice I am taking sides already in several philosophical disputes. The disputes themselves may seem of little interest to scientists, who will want to argue with each other about other matters, but which side you take influences how you do science.
>
> – Tom Settle, philosopher (1979, 522)

> Philosophy is much better at obscuring reality than it is at explicating it, at confusing rather than clarifying, however much philosophers may justify their activity on the

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> opposite assumption … Skip the philosophy, pay little attention to the 'history' … and try to get down to cases.
>
> – Robert Trivers, biologist (1999)

1. Introduction

At times, the relationship between philosophy of science and research science appears similar to that of family members: intimately connected, mutually influential, and more than occasionally in a quarrel. If by this analogy the role that philosophers take on appears to be akin to that of a parental figure—with a presumption of authority on how science should behave and ideally be practiced—it is also true that it is a dynamic that scientists themselves often help engender. When disputes over scientific claims erupt scientists have been known to call upon philosophy of science to bolster their own claims as well as argue against competing schools of practice, and perhaps conveniently so: scientists are the ones dealing with reality, but it doesn't hurt to take advantage of whatever conceptual muscle that philosophy can offer to their cause. Under other circumstances, however, the adoption of an outright anti-philosophical attitude may be considered the admirable, productive, and perhaps defining quality of who serious scientists are and how they approach their work; they don't philosophize, they act. As one philosopher of science observed, 'scientists themselves just do it, they just operationalize their concepts without saying much about how they do it' (Hull 1999, 489).

While these characterizations are certainly overly simplistic, they capture some of the binary and equivocal sensibilities that philosophers and scientists alike hold about the disciplines and their practices. Although philosophy's influence on scientific practice cannot be denied, the exact nature of this influence and whether it has always been positive is a question that engenders considerable ambivalence as well as debate. The role philosophy has played in the biological sciences in particular has varied considerably through time as well as between its various sub-disciplines. Through the early twentieth century math and physics received the lion's share of philosophy of science's attention. During this period controversies of philosophical import in biology such as homeostasis, symbiosis, organicism, and the debates between Mendelian and biometric schools of genetics were argued and settled among biologists themselves, with no noticeable input from philosophers in any formal sense. Through the 1950s, 60s, and 70s, however, philosophy of science became increasingly important in the field of evolutionary biology in particular, and along two general veins. One vein grew organically out of a need for clarification as well as expansion of concepts central to evolutionary research as the methods, tools, and data available to the field advanced. A variety of topics such as adaptation, fitness, historical contingency, the definition of species, and the 'units-of-selection' question all found a growing community of biologists engaging with philosophical concerns seriously, and likewise a new discipline within philosophy itself—the philosophy of biology—developing to address the issues unique to these questions. From this arose what could be called a 'fraternal' mode of interdisciplinarity.[1]

Around this same time, another kind of interaction developed between philosophy and biology, growing out of quite a different set of concerns. As biology gained greater status among the sciences in the second half of the twentieth century, so too did a tension over what in fact biology's standing was as an autonomous scientific discipline (Munson 1975). Some molecular biologists, such as Francis Crick, posited that the 'ultimate aim' of science was 'to explain all biology in terms of physics and chemistry' (as quoted in Mayr 1996, 98). This was a view that many philosophers of science—whose own discipline had developed on the template of the physical sciences—were sympathetic to. The philosophy of science began playing an increasingly prominent and contentious role in these debates by challenging the scientific status of evolutionary and non-reductionist forms of explanation. This dynamic stands in contrast to the fraternal vein and represents a relatively 'paternal' mode of interaction between the disciplines, one best exemplified by the role of Popperian philosophy in the development of contemporary evolutionary biology. As significant as they have been to the sciences, the way that Popper's principles of demarcation and falsifiability have operated within the context of the biological systematics community as well as the North American evolution/creationism controversies epitomizes a paternal model of disciplinary interaction.

Examining the nature of these fraternal and paternal modes is useful because it can help address what the future of interdisciplinarity between various arenas of biology and philosophy of science might look like and what still possibilities exist. I will argue the more fraternal mode that has developed between evolutionary biology and the philosophy of biology is a dynamic worth emulation in other subfields of biology, proposing further that the practices of nineteenth-century biologists provide an important model for an integrated scientific and philosophic engagement that is relevant for the work of today's biologists.

2. Paternal Sensibilities

In 1974 philosophy of science put evolutionary biology on official notice. In that year Karl Popper had 'come to the conclusion that Darwinism is not a testable scientific theory but a metaphysical research programme' (Popper 1974, 134). This claim was taken by many as a serious and substantive challenge to the status of evolutionary biology as a scientific discipline. Although Popper's conception of a metaphysical research program was quite sophisticated and not necessarily pejorative, his principle of demarcation set out clear criteria for what could be considered genuine science versus merely pseudoscience. By Popper's initial estimation evolutionary biology did not make the grade on two counts: Darwinian theory was historical in nature and therefore lacking the universal and law-like qualities that his formulation of science demanded, and likewise natural selection did not appear to be testable in the ways genuine science necessitated. Popper's assertion provided a definitive form to concerns that had circulated about biology and its 'provincial' nature as compared with the universally structured laws of the physical sciences (Munson 1975).

However by the mid-1970s philosophy of science was no longer as homogenously physics-oriented as it had traditionally been. Philosophy of biology as a discipline had developed to the point that professional philosophers specializing in the biological were there to defend the field against such claims. Michael Ruse, future founding editor of the journal *Biology and Philosophy*, responded firmly that Popper's criticisms of biology were 'without force and his suggestions for its improvement are without need' because he was 'abysmally ignorant of the current status of biological thought' (Ruse 1977, 638). After two years and further consideration Popper made a 'recantation'; he reversed his position on the testability of natural selection and came to believe that evolution indeed had a scientific—rather than primarily metaphysical—character (Popper 1978). By this time, however, the die had been cast and the issues that Popper had raised about evolutionary biology, demarcation, and falsifiability were to persistently reverberate within the field for years to come. Just as importantly, a precedent had been set in terms of how the field of philosophy might assess science, with philosophers taking positions on both sides as authorities on the scientific legitimacy of evolutionary biology.

The implication of this authority extended beyond academia and into the wider public consciousness as well. In the 1982 American legal case, *McLean vs. Arkansas Board of Education*, the courts considered the teaching of creationism in public schools, with creationists arguing in support of the Arkansas law called the *Balanced Treatment for Creation-Science and Evolution-Science Act*. During this case the centre of creationist argument drew directly on Popper's earlier views on Darwinian evolutionary theory as non-science. It relied upon the reasoning that since neither 'creation-science' nor 'evolution-science' met Popper's criteria of genuine science that a 'balanced treatment' of both creationism and evolution in public school science classes was therefore justified (Overton 1982). The oxymoronic reasoning of the creationists and their out-of-date understanding of Popper notwithstanding, the case was significant for the very fact that a philosopher of science's views carried such weight as an authority on evolution's scientific standing in the popular consciousness. Just as significant, however, was the extent to which the evolutionists' position also largely hinged on Popper: as an expert witness for the pro-evolution plaintiffs, Michael Ruse actually invoked Popper's principles of demarcation and falsifiability as principles that *supported* the scientific status of evolutionary biology over that of creationism. In the end, the arguments of Ruse and other expert witnesses for the plaintiffs prevailed over the account of Popperian reasoning that the creationist defence put forth. The argument presented by Ruse and others was so compelling that the presiding judge not only concluded that creation-science was 'simply not science', but also deemed the Popperian criterion of falsifiability as one of the five 'essential characteristics' of what defines science (Overton 1982).

While it may be tempting to conclude that all is well that ends well for the cause of public science education, the Arkansas case raised a variety of troubling questions. Even though the judge was finally convinced of the scientific qualities of evolutionary biology compared to 'creation-science', it is significant that the final decision pivoted on philosophical argumentation of what counted as a science *in principle*, and not on the merits or demerits of the evidentiary accounts that evolution or creationism provided. Philosopher of science Larry Laudan articulated this point shortly after the Arkansas trial. He argued that the basis of the judgment against the Arkansas law should have been creationism's empirical failures as testable hypotheses (the earth, for example, turns out not to be 6,000 years old), and not on the philosophically determined 'essential characteristics' of science that creationism didn't appear to meet. Laudan strongly critiqued the central role philosophy played in the court case, writing, 'It simply will not do for the defenders of science to invoke philosophy of science when it suits them (e.g., their much-loved principle of falsifiability comes directly from the philosopher Karl Popper) and to dismiss it as "arcane" and "remote" when it does not. However noble the motivation, bad philosophy makes for bad law' (Laudan 1982, 19). He went further still, writing that in the court case victory had been 'achieved only at the expense of perpetuating and canonizing a false stereotype of what science is and how it works', concluding that, 'if it [the false stereotype] goes unchallenged by the scientific community, it will raise grave doubts about that community's intellectual integrity' (Laudan 1982, 19).

Given the weight granted Popper's opinion and critical role it played in the trial, would it have mattered to the Arkansas case if he had never recanted on his original view of evolutionary biology? Considering the line of argumentation and circumstances of the case, it is not entirely clear. One might argue that the stakes for evolutionary biology and goals of its defenders in Arkansas were very local and aimed primarily at the defeat of an individual, wayward school board or local government (Gross 1983). However, it would be naïve to think that the affirmation by philosophers and by the legal system alike would not be warmly welcomed by scientists who considered the case a victory for evolutionary biology and for its status as a scientific discipline, especially in the eyes of the public. Ruse and others defended the role of philosophical arguments in the Arkansas trial (Ruse 1982; Gross 1983), but to many it raised an unsettling implication that philosophy was in the position to adjudicate the status of what was, and was not, science in a way that science itself could not. On this point as well Laudan took exception and challenged scientists to defend their discipline against the encroachment of philosophers speaking on behalf of science. Nevertheless, the Arkansas trail signalled a shift in the relationship between the disciplines of philosophy and biology. As Steve Fuller, a philosopher of science and expert witness in the recent 2005 Dover evolution–creationism trial recounts, Ruse's role in the Arkansas case 'set a precedent, because up to that point, the only people allowed to testify on the nature of science were professional scientists' (Corbyn 2006).

If philosophy of science—and Popper's philosophy in particular—asserted undue influence in both establishing and safeguarding the credibility of evolutionary biology as a science in the context of creationism and the public understanding of science, then its function was taken to even further extremes among scientists in the biological subfields of systematics and cladistics. Concerned with the classification of and relationship between organisms, this field experienced a remarkable period of controversy and divisiveness that revolved around matters of Popperian philosophy. The titles of articles regularly published in a leading journal in the field, *Systematic Biology* (né *Systematic Zoology*) well illustrate the authority that Popper held among researchers in the discipline: 'Karl Popper, Verifiability, and Systematic Zoology' (Kitts 1977), 'Falsifiability, Consilience, and Systematics' (Ruse 1979), 'Probability, Parsimony, and

Popper' (Faith and Cranston 1992), 'Philosophical Conjectures and Their Refutation' (Kluge 2001), 'Popper and Systematics' (Rieppel 2003), and many others. On first consideration, this adoption of philosophical concerns by researchers would seem ideal, as scientists finally took philosophy seriously and made it both operational and relevant to how their science was actually being done—indeed, seriously enough that certain interpretations of Popper even helped inspire new schools of practice, such as the 'transformed' or 'pattern cladists'. Pattern cladists held that the branching diagrams and classifications deriving from cladistic analysis should not be assumed to reflect evolutionary relationships among organisms, but logically and most parsimoniously only represent the patterns the method itself produced. To many biologists, however, such a position seemed disturbingly contradictory, because it rejected the process of evolution by common descent, which most considered to be a foundational premise of the field in the first place. Striving to be 'theory neutral' in this context risked excluding the content of the science itself for some notion of methodological purity.

Despite the extended engagement that systematics has continued to have with philosophy of science, it may represent anything but a successful model for how the two fields might interact. For as much considered and extensive philosophical exchange that has taken place over 30 years time in the systematics and philosophy communities, the discussion has been dominated almost exclusively by questions of Popperian interpretation and standards, with 'falsifiability' functioning as a litmus test in assessing the legitimacy of various methods. Often debates devolved into what many felt were convenient and somewhat mercenary uses of philosophy by scientists to justify their own positions. Competing schools of systematics assiduously invoked Popper in an attempt to support their own position and undermine the opposition's in a pattern that has been described by some close observers like David Hull as nothing less than the 'use and abuse' of philosophy (Hull 1981, 1999). Coming from a philosopher of science and a former associate editor of *Systematic Biology*, such an assessment is sobering indeed. Other scientists walked away more convinced than ever that philosophy had little constructive role in influencing scientific practice. Following the more recent reappearance of Popper-centred papers in *Systematic Biology,* an editorial commented that although philosophy could 'influence future research directions to some unknown extent', the fact of the matter was that most of the important methodological innovations subsequent to the initial Popper debates had developed 'without regard for philosophical foundation' (Olmstead 2001, 304). A recent article by two biologists went even further to argue that systematics researchers should fundamentally change their approach and now 'allow science to play the part of the horse pulling the cart of philosophy' (Helfenbein and DeSalle 2005, 279).

The seemingly straightforward 'either/or' quality of Popper's demarcation principle may have predisposed it to oversimplification by scientists willing to use it as formula by which to make definitive judgments. Indeed, for working scientists in many fields methodological falsifiability is considered *the* standard of scientific practice and remains the primary exposure most have to the philosophy of science. As British physicist Sir Hermann Bondi expressed it, 'There is no more to science than its method, and there is no more to its method than Popper has said' (Bondi 1992, 363). And although badly implemented philosophy of science is not a reason to conclude philosophy is bad for science, the horse-and-cart metaphor is telling of the ways in which philosophy of science has come to be seen as paternalistic in reference to evolutionary biology. This impression is unfortunate to the extent it is inaccurate if one considers the field of systematics in its full scope. Work by philosophers such as Sober (1988) on inference and statistics in systematics, for example, has made very constructive and constitutive contributions to the development of many of the standard methods used today (Felsenstein 2003). Despite such positive outcomes, the high-profile vitriolics of the Popperian debates in systematics may have tainted many people's view of the philosophy/biology relationship, serving to reinforce negative and uninformed notions of philosophy among practicing biologists as well as a validating a sense commitment to the disciplinary divide between the two.

3. Fraternal Realities

Hull has commented that 'Professional philosophy of science rarely plays a significant role in scientific controversies. In the case of cladism, Creationism and evolutionary theory, it did—to the detriment of all concerned' (Hull 1999, 500). Discouraging as this conclusion may seem, it is important to acknowledge how both genuine differences of opinion mixed with the (sometimes faddish) adoption of views can often result in intellectual bickering within science, philosophy, or any academic community. Hull himself has been one of the most perceptive in recognizing the ways in which 'science is a process' (1988) that involves various forms of cooperation and discourse as well as conflict and quarrel, dynamics integral to how the social and conceptual character of scientific communities evolve over time.

The paternal character of the Popperian paradigm towards evolutionary biology in the 1970s and 1980s is certainly not the only form of engagement available to philosophy of science. Fraternal modes of interaction have also thrived and there are many indications that the dynamic between the two disciplines continues to evolve. As much attention as there has been to Popper and the evolution–creationism or cladism debates, it is equally true that a diverse community of scholars across both disciplines have been thoroughly engaging with a wide range of philosophical and biological concerns. From the 1960s onward researchers such as Ernst Mayr, John Maynard Smith, Stephen Jay Gould, Richard Dawkins, Stanley Salthe, and Michael Ghiselin were in the vanguard of biologists seriously exploring and incorporating philosophical consideration in their scientific work. Likewise, as philosophy of biology established its own unique scholarly identity, philosophers such as Robert Brandon, Philip Kitcher, David Hull, William Wimsatt, Elisabeth Lloyd, and Elliot Sober began to tackle seminal issues in the field such as adaptation, reductionism, the roles of chance vs. necessity, and the units of selection question. The topics of study and the number of philosophers working within them has only continued to grow to the point where, at least in North America, philosophy of biology is now arguably one of the most active areas in the philosophy of science overall. Within this more fraternal environment, philosophy and biology have worked as counterparts and collaborators in fundamentally shaping the trajectory of biological research. For example, philosophical considerations have been of central influence in framing our understanding of evolution's 'Modern Synthesis' and how seemingly opposing influences of 'chance' versus 'necessity' may be accounted for within it (Sober 1983). Nascent discussions in the late 1970s on the importance of selection-driven adaptation (functionalism), historical contingency (historicism), and the possibility of emergent properties that operate in biological systems (structuralism) set the conceptual framework that critically informed three decades of subsequent discussion into topics as disparate as the relation of micro- to macroevolution, the status of social insects as superorganisms (Mitchell 1992), and the adaptivity of human female orgasm (Lloyd 2005).

Likewise, this ethos of interdisciplinary research has not only resulted in extensive exchange between the disciplines, but also close philosopher/biologist collaborations. Elliot Sober is just one example of a philosopher who has worked extensively with biologists, including D. S. Wilson on the issue of group selection (Wilson and Sober 1989, 1994; Sober and Wilson 1998) and Steven Orzack on the uses of probability, optimality, and adaptation in biological research (Orzack and Sober 1994, 1996, 2001). These projects in turn have motivated direct responses from other biology/philosophy collaborations (e.g. Brandon and Rausher 1996), resulting in an ever-growing community of hybrid research directed towards questions central to both disciplines. Rather than simply being a matter of specialty exploration, these interdisciplinary efforts are also producing texts that are becoming standard references in their respective fields of evolutionary biology and philosophy of biology alike (Orzack and Sober 2001; Pigliucci and Kaplan 2006; Rosenberg and McShea 2007).

A telling indication of how this fraternal and more interdisciplinary mode of the Philo–Bio interaction has come to the fore is evident in comparing the recent 2005 evolution–creationism trial—*Kitzmiller et al. vs. Dover Area School District*—to that of the 1982 Arkansas case already discussed. In the Arkansas case I argued that Popperian philosophy of science was paternalistic in how it acted as standard-bearer and expert witness for both the evolutionary biology and creationist sides of the court argument. In the recent Dover trial philosophers of science again played a crucial role; however, the evolutionist and creationist sides stood in stark contrast in terms of how they employed philosophy in their arguments. Steve Fuller, a philosopher and sociologist of science, acted as expert witness for the Intelligent Design (ID) defence and saw his role very much in the paternal frame, akin to how Ruse and others utilized Popperian philosophy in the Arkansas trial. In his testimony Fuller argued at length for the critical function philosophy of science, and the principle of testability in particular, could and should play: 'this is where issues of testability get their legs, because there's a sense in which one can talk about testability in a way that is abstracted from what the dominant sciences are at the moment and provides, you might say, a kind of neutral court of appeal … in fact, it is a kind of quasi-judicial traditional discipline traditionally, which makes judgments about what is science and not science from a punitively neutral standpoint' (ACLUPA 2005, Day 15 AM, 30). From Fuller's point of view, the authoritative task of the philosophy of science as expert witness is as definitive as it is clear: 'So is it true then that the training you have actually makes you better equipped to answer that issue than a scientist that's practicing?', queried the defence attorney. 'Yes', responded Fuller (ACLUPA 2005, Day 15 AM, 33).

While the argument for the creationist defence that Fuller put forward was a multi-faceted one involving a mixture of sociological, historical, and philosophical claims, one key theme was his claim that a Popperian ideal of science-as-a-process is the standard to be emulated. To realize this ideal—one in which there is an 'open society' of competing ideas and theories that are tested on equal footing—Intelligent Design creationism must be given the opportunity to develop more fully as a research program, not judged by the current standards of the dominant evolutionary paradigm (ACLUPA 2005, Day 15 AM, 22). In this way Fuller's argument relies upon a normative ideal of what science should be as prescribed by philosophy. From this perspective, the merits of ID creationism do not stem from anything actual or empirical, but in *possibility*. For example, when asked whether ID creationism is inherently religious Fuller answered definitely 'no'. Why? 'Well, the point is, you don't have to be religious to be able to develop it', he explained. 'I mean, I think that's the key point here, that even though historically it's been associated with a lot of religious people, one doesn't need to be religious' (ACLUPA 2005, Day 15 AM, 107).

Fuller's position, with its commitment to normative possibilities, stood in sharp contrast with the approach of the evolutionist plaintiffs in this trail. While it's true that some hypothetical form of ID on a Twin Earth wouldn't necessarily have to be religious in nature, the fact of the matter is that the present ID movement is. The philosopher Barbara Forrest as expert witness for the plaintiffs thoroughly documented how Intelligent Design is a direct descendent of religiously motivated creationism, as well as how ID—far from being an organically developing scientific research community—is itself a movement orchestrated by one organization with the explicit intention to undermine evolutionary science and promote a Christian worldview (ALCUPA 2005).

Although Popper was not directly invoked by philosophers arguing on behalf of pro-evolution plaintiffs, this is not to say that the principles of demarcation and the logic of science more generally did not play a large role in the their arguments. They relied on the philosophy of science to explain the ways in which Intelligent Design is not a scientific proposition or how—even if certain claims within Intelligent Design or creationism were tested as hypotheses—they would fail the criteria the tests set forth. This said, it is striking to note how divergent the core strategy of the pro-evolution plaintiffs was compared to the earlier Arkansas case. This time evolution's arguments were centrally rooted on empirical evidence that countered ID's claims rather than primarily on the strength of philosophical positions. Laudan should be pleased, as this addressed his core critique of the earlier Arkansas trials: 'The correct way to combat Creationism is to confute the empirical claims it does make, not to pretend that it makes no such claims at all' (Laudan 1982, 17), '[...] we should confront their claims directly and in piecemeal fashion by asking what evidence and arguments can be marshalled for and against each of them' (Laudan 1982, 19).

This empirical turn among today's evolutionists can, in part, be accounted for by the greater amount of data supporting evolution now available after 23 additional years of research, and perhaps too in the need for the plaintiffs to respond directly to the scientific claims of Michael Behe, biologist and expert witness for the ID defence. More generally, however, I believe this shift to a more empirically informed argument is itself indicative of a real change in the way much of philosophy is now relating to biology. Rather than philosophy of science acting as an intellectual judge over what is or is not science in a categorical sense, philosophy informed and acted in concert and collaboration with biological research to confront the ID challenge in Dover. Expert witnesses like Robert Pennock—an established philosopher of science but also active in collaborative biological research—exemplify the fraternal vein that has come to characterize the interdisciplinary work of researchers in Philo–Bio. In contrast, creationism's latest iteration appeared to still be relying upon a paternalistic model of interaction in which philosophy is to dictate the terms of science. In this light, the Dover case provides a revealing look into how the relations between the two disciplines have evolved in the North American context. If considered very loosely in the conceptual framework of evolutionary epistemology (Popper 1984; Hull 1988), it may be worth considering whether the relative hybridity of the fraternal strain provides it a competitive advantage over the paternal, more ancestral mode of relation between the disciplines.

4. The Possibility of a 'Philosophic Physiologist'

To the extent that the interaction between philosophy and biology is currently moving in a direction with more of a sibling sensibility, it is important to acknowledge the suspicions that many biologists hold towards philosophy of science and its implications for how the disciplines may relate in the future. Unaware that various epistemic considerations underwrite the validity of their work, it is true that scientists pursuing certain kinds of research argue that there is no pressing need for them to engage with philosophy; philosophy is something that (at best) is simply not applicable, and at worst runs the risk of clouding the clear-eyed empiricism of basic research (as the Robert Trivers quote at the very beginning attests). Molecular biologist Robin Holliday's opinion of Popper's philosophy in relation to molecular biology also does well to illustrate a commonly held attitude when he writes, 'the [genetic] code is true; it is universal and scientists do not waste their time trying to refute or falsify it' (Holliday 1999, 890). Views like Holliday's are frustrating in the way that they misconstrue philosophical principles, nevertheless they are representative of the view many biologists subscribe to. Of course the argument that any scientific discipline can safely avoid involvement with philosophy of science runs the risk of defining itself out of the possibility of complexities *a priori,* thus closing off the ability to realize live philosophical issues of empirical significance. As unfortunate as it is, there may very well be some truth to Steve Fuller's opinion that he as a philosopher is better equipped to think through the foundational bases of science: 'Nowadays, to be professionally trained to be a scientist, is, in effect, to be a technical specialist in a very small area, a small branch even of your own science. And very often, these technical specialists have to take largely on faith what people from other branches of their own field are doing because they have only the most cursory understanding of it' (ACLUPA 2005, Day 15 AM, 32–33).

However I believe positive alternatives to this disciplinary myopia are evident in the kind of work now emerging in the fraternal vein of research exemplified by Pennock, Sober and Orzack, Pigliucci and Kaplan, and so many others besides. While I've been characterizing this more intimate form of interdisciplinarity as a new development, in fact the more distant past provides an important precedent for it. After all, there was a time when philosophy and science were not considered such autonomous disciplinary realms. In the mid-1800s, for example, biologists such as Agassiz, Geoffrey St. Hilaire, Owen, Lyell, and Darwin actively debated the role that teleological explanations should or could any longer play in biological accounts of changes in form over time (Ospovat 1978, 1980). In the process this scientific community did nothing less than restructure the epistemic basis upon which biology as a science would proceed thenceforth. Rather than biologists marshalling choice concepts from another discipline that were favourable to positions they were defending, the scientists themselves were well-versed in the philosophical considerations underlying their debate and understood them as coextensive and constitutive to the science that they were doing. At that time, biologist William Carpenter wrote in recommendation of an ideal biologist—what he called a 'philosophic physiologist'—who should 'follow precisely the same course [as the physical philosopher]' and not accept teleological explanations of final causes for phenomena, but instead seek out general laws to explain them (as quoted in Ospovat 1978, 39).

While the definition of 'philosophy' and its identity as an academic discipline distinct from the natural sciences is very different in the context of Carpenter's writings than from how we understand this relationship today, this contrast is exactly the point. It has been argued that 'Although science and philosophy of science as ideal types can be distinguished, their activities cannot be' (Hull 1988, 297). True as this is, it is just as important to then ask whether the notion of research science, on the one hand, and philosophy of science, on the other, as 'ideal types' has itself been given more deference than it is due. A philosopher like Fuller relies on the disciplinary distinction of these ideal types, believing that while scientists trained in certain fields may in fact be in a unique position to judge the epistemic status of certain scientific claims, at the same time they somehow are not equipped to proffer judgments about the second-order question of whether what they do is actually even science in the first place (Fuller 2007, 91). The Fullerian notion of philosophy of science seems to need a research science that lacks self-awareness, but this paternalistic perspective has increasingly little relation to a philosophy of science that is actually engaged with the practice of science itself—that is to say, contemporary philosophy of biology as we find it now. To in any way condone the idea of today's scientists as 'technical specialists' who cannot and need not assess the scientific (to say nothing of the ethical) standing of their work seems like the most dangerous of all outcomes for the practice of science.

The professionalization of academic disciplines may have contributed to a dangerous narrowing of intellectual horizons in science and philosophy both, but thankfully we see a growing number of researchers who are both scientifically and philosophically adept and who are making novel contributions to the synthesis in both areas. While the discussion here has focused on primarily evolutionary biology, it should go without saying that philosophical issues concerning causation, explanation, and methodology run like roots throughout all fields of biology, including molecular genetics (Kitcher 1984, 1999; Schaffner 1974, 1993; Keller 2002; Stotz and Griffiths 2004), developmental biology (Griffiths and Gray 1994; Oyama et al. 2001), and behavioural biology (Hacking 2001; Longino 2001). As the understanding of complex processes on the molecular genetic level increasingly necessitate holistic systems-level approaches to associate vast amounts of data with observed phenomena, biologists in many sub-disciplines will also need to confront issues of causal inference and explanatory reducibility that evolutionary biology has already grappled with for some time (Mayr 1996; Rosenberg 2001). The current cross-pollination between neuroscience and the philosophy of mind likewise stands as an excellent example of growing fraternity between historically disparate fields of inquiry that are fundamentally influencing each other in new and unique ways around reductionism, causation, and explanation that were borne from empirical advances in the field (Metzinger 2000; Bennett et al. 2007). Despite the copious literature and various communities engaged in these topics, it is notable that most of this cross-pollination still remains obscure to the general science community, which is preoccupied with generating the ever novel data that funding agencies insistently demand.

The ability to be a philosophically engaged scientist does not necessitate a degree in both disciplines, but it does demand a commitment on the part of science education and research training to take exposure and engagement with philosophy of science much more seriously than it does now. Undergraduate and graduate biology education would do well to invest considerably more in students' understanding of the context of biological discovery and explanation relative to the overwhelming focus on its technical details. As true as this is for issues in analytic philosophy I've focused on here, this is even more the case for the pressing bioethical issues that biologists increasingly must confront in their research and their teaching, as both scientists and citizens. The physicist Leonard Susskind recently wrote that, 'Good scientific methodology is not an abstract set of rules dictated by philosophers. It is conditioned by, and determined by, the science itself and the scientists who create the science' (Susskind 2004). For Susskind's claim to be as welcome as it is true, we must challenge and reformulate the false dichotomy implicit in his statement and reconsider what it even means to be a philosopher or a scientist in the first place and how we categorize ourselves as scholars. I am not suggesting a dissolution of the professional disciplinary training that philosophers and scientists receive, nor do I claim that their concerns should in fact always be one in the same. Rather, the goal would be an even fuller mutual engagement between the disciplines that deepens the work of each field while also contributing to the other robustly, fraternally, and in kind—as philosophic physiologists and physiological philosophers alike.

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Note

[1] Aware of the generally gender-specific nature of the term 'fraternal', I intend to use it here in a gender-neutral sense of 'sibling relationship', much like 'fraternal twins' who can be of any sex. 'Fraternal' was specifically chosen because of its additional connotations of amity and cooperation, which are not implicit in alternative terms.

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