Biology and Philosophy **15:** 301–310, 2000.

David Hull's Natural Philosophy of Science

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Introduction

Throughout his career David Hull has sought to bring the philosophy of science into closer contact with science and especially with biological science (Hull 1969, 1997b). This effort has taken many forms. Sometimes it has meant 'either explaining basic biology to philosophers or explaining basic philosophy to biologists' (Hull 1996, p. 77). The first of these tasks, simple as it sounds, has been responsible for revolutionary changes. It is well known that traditional philosophy of science, modeled as it was on theoretical physics, proved inadequate when philosophers turned their attention to biological science. Biological examples have driven major revisions of accounts of reduction (Hull 1974; Schaffner 1993, Ch. 9), laws of nature (Beatty et al. 1997), theories (Lloyd 1988) and natural kinds (Wilson 1999, Part III). Nor is explaining basic philosophy to biologists a task to be looked down upon. It is useful, not because philosophy has all the answers, but because scientists must think about how to do science, that *is* doing philosophy of science and scientists frequently reinvent philosophical views with known flaws. Early in his career Hull found biological systematists in the grip of a crude operationalism about scientific concepts and said so in the pages of Systematic Zoology (Hull 1968). For the next thirty years, as biologists debated the nature of species and the correct principles of classification, Hull added a philosophical note at the same congresses and in the same journals (Hull 1970, 1976, 1980, 1997a, 1999).

It is not only the philosophy of science that can benefit from closer contact with the biological sciences. The main branches of philosophy – ethics, metaphysics and epistemology – continue to draw inspiration from the sciences, as they have always done. The longstanding consensus that scientific findings have no direct moral implications does not prevent the findings of contemporary genetics, for example, from having dramatic implications when conjoined with existing moral principles. Biology is also used to support our 'philosophies of nature' – visions of humanity and its place in nature that underpin social and political perspectives (Godfrey-Smith 2000). Supporters of unfettered economic competition and of the green movement both find some biological claims more congenial than others and feel threatened when the biological consensus seems to be shifting against them. With respect to such debates, philosophers of biology play a role different from and complimentary to that of science communicators. The philosopher critically examines the connections that are drawn between science and these ethical and political issues, questioning both the science and the philosophical argumentation founded upon it (Sterelny and Griffiths 1999, Ch. 1). The biology of animal behavior, for example, has been a major source for the last fifty years of arguments about 'human nature' and its implications for our social and political arrangements. Philosophers of biology have been prominent amongst those who have tried to sort out the ensuing mess (e.g., Kitcher 1985). David Hull's most direct contribution to these debates has been to argue that the very ideas of human nature and 'normal' behavior sit very uneasily in a Darwinian world (Hull 1986; Horvath 2000). Less directly, Hull's seminal work in debates over the units of evolution, and particularly his replicator/interactor distinction, has helped structure the debate over the significance of the 'gene's eye view' of evolution for human nature (Hull 1980, 1981; Godfrey-Smith 2000b; Lloyd 2000).

Hull's opposition to the idea of a universal human nature is closely related to his main contribution to contemporary metaphysics. As late as the 1970s philosophers of language were citing biological species as obvious examples of 'natural kinds' – categories all of whose members share some underlying essential property that makes them what they are (Kripke 1980; Putnam 1975). The fact that essentialism about species had been largely abandoned by biologists thirty years earlier had not filtered through to the philosophical community.[1] A Darwinian species can exhibit unlimited variation in any of its characteristics without losing its identity as a species. Species membership does not supervene on the intrinsic properties of an organism but on a much wider class of properties related to the evolution of the population of which that organism is a member and an ensemble of related populations. Hence, at the same time that Hull has provided philosophical clarification in the scientific debate over the nature of species, he has provided an important biological corrective to philosophical views of the kinds of things that inhabit our world (Hull 1978b, 1984, 1987; see also Ghiselin 1974a, 1974b). While Hull and Michael Ghiselin's view that biological species are ontological individuals and hence not the subjects of genuine laws of nature has not been universally accepted, it forms the background to all later philosophical discussion of biological categories and, increasingly, of natural kinds in general (Wilson 1999).

Another way to bring the philosophy of science into closer contact with science is via the history of science. Hull has led by example in urging philosophers to learn and contribute to the literature in history of science (Hull 1973). There are many reasons to study history of science (Maienschein, this issue), but what precisely is the benefit to philosophy of closer interaction with history of science? It is certainly a *prima facie* problem for any philosophical account of science if major episodes of apparently successful science do not conform to its prescriptions (Feyerabend 1975). But Hull's own theory of the scientific process has a more intimate relationship to the history of science than this picture suggests. Hull does not elaborate a normative epistemological account of the scientific process, with the history of science functioning to suggest that there has been a mistake somewhere in his philosophical reasoning. Instead, Hull aims at a descriptive and explanatory theory of science derived from the empirical study of both historical and contemporary science. In fact, Hull's vision comes very close to that of some of the 'strong program' sociologists of science – a science of science itself (e.g., Bloor 1976).[2] One difference between Hull and the strong programmers is that Hull makes less *a priori* commitments concerning how science is to be explained. While Hull is entirely open to the idea that the sociological methods can contribute to the science of science, and collects sociological data to this end, he does not presume that these methods will be adequate to the task of explaining scientific change. History, sociology, behavioral ecology (Hull 1978a) and theories of cultural evolution are all possible sources from which we may be able to derive insight. The only criterion by which to judge an approach is its fruitfulness in yielding explanatory and predictive generalisations about science. Arguments about the strength of the analogy between classical economics and ecology carry little weight when compared to the productive use to which Darwin put that analogy. Similarly, arguments about the strength of the analogy between biological and scientific evolution are of little value compared to determined attempts to model scientific change on biological change (Hull forthcoming).

Hull's social epistemology

The theory of scientific change embodied in Hull's Science as a Process (Hull 1988) is driven by an analogy between scientific change and evolutionary change. The resulting account can, however, be interpreted in two ways. On the one hand, it can be read as a literal evolutionary theory of science – a contribution to that form of evolutionary epistemology which tries to explain scientific change as the outcome of fitness differences between units of conceptual evolution. On the other hand, Hull's work can be read as 'conservative social epistemology' (Grantham 2000). On this second reading, Hull has been inspired by some aspects of evolutionary thought to produce a particular kind of sociology of science. This is a 'conservative' sociology because it takes as part of what is to be explained the outstanding success of science in comparison to other intellectual traditions in yielding pragmatically successful empirical knowledge. This is another difference between Hull and the so-called strong program. Hull's sociology does not accept the 'symmetry thesis' according to which currently accepted theories are to be explained in just the same way as those currently rejected (Bloor 1976, 1981). The symmetry thesis purges the domain of facts to be explained of what in Hull's view is one of its most important constituents – the tendency of science to adopt theories pragmatically superior to their competitors. This asymmetry in the direction of scientific change cannot be explained by a fully symmetrical approach.

Hull's theory of science is discussed in greater depth in two of the later essays in this issue (Grantham 2000; Downes 2000) and exemplified in a third (Lloyd 2000). In essence, however, Hull proposes what Bruno Latour has labelled "a typical American myth" (Callebaut 1993, p. 315). He treats the social structure of science as something akin to a market mechanism. Intellectual credibility, which Hull calls 'credit', takes the place of profit and empirical knowledge takes the place of economic product. The reward structures of scientific institutions are such that to achieve their individual goals, whatever these may be, scientists must accumulate credit and to do that they must contribute to the production of pragmatically effective empirical knowledge. "Science works as well as it does because the selfish goals of individual scientists happen to coincide with the manifest goal of the institution, the increase of empirical knowledge" (Hull 1978a, p. 685). One of the advantages of this explanation of the pragmatic success of science is that it does not rely on identifying the 'scientific method' by which success is achieved. Indeed, within Hull's framework it is natural to think of the methods of science evolving as much as the contents of science. Scientists respond to the incentive system by altering their methods so as to gain greater rewards. Hull's theory thus allows an answer to those who argue from the fact that the methods of science are in a continual state of development to the conclusion that there can be no general theory of how science works.[3] For Hull, there is a general explanation of why science succeeds despite the fact that the means by which it succeeds are ever changing.

Commentators on Hull's theory of science have questioned whether it adequately explains the success of science. Kim Sterelny, for example, asks whether the pragmatic success of science would persist if scientists did not have appropriate intrinsic motivation, as well as the extrinsic motivation created by the career structures of science (Sterelny 1994). Would the social structures of science suffice to generate reliable knowledge from a population of scientists with no intrinsic interest in better science? Might not such a group generate theories as ineffective as the fraudulent medicines we know a market economy would offer up were the drug industry not highly regulated? Criticisms of this sort question Hull's explanation of the success of science but do not question the project of seeking such an explanation. Criticism of this more radical variety has come from, amongst others, Todd Grantham (1994). Grantham identifies a tension between modeling scientific change on evolutionary change and assuming that science is increasingly successful. There is a longstanding consensus that evolution by natural selection is not a directional process leading to better and better organisms. Organisms do not evolve up the great chain of being; they adapt to local aspects of an ever-changing environment. Organisms are always moving up fitness surfaces, but they do this in the context of multiple peaks in a fitness landscape which is itself changing. Furthermore, organisms do not simply adapt to fit their environment but rather co-evolve with it (Lewontin 1982). Organisms construct their environment both by physically changing it (Odling-Smee et al. 1996; Laland et al. 2000) and by changing which aspects of the physical environment form part of their ecological environment, or niche (Brandon 1990; Godfrey-Smith 1996). Paralleling these features of biological evolution are features of science which challenge the idea that scientists succeed by achieving better and better fit to the same reality. First, scientists to a significant extent physically create the reality they investigate, as work on the construction of model organisms and experimental systems has shown (Kohler 1994; Ankeny 1997; Rheinberger 1997). Secondly, when scientists set out to construct theories that are pragmatically successful they set out to make them successful by the standards of their day (Rasmussen 1993, forthcoming). What counts as a success at one stage in the history of science may not count as such at another stage. For example, Darwin proposed a new theory of the origin of species, but he also changed what counted as an adequate theory of the origin of species. A statistical historical narrative was not what the methodologist Sir John Herschel wanted when he called on scientists to explain this 'mystery of mysteries' (Herschel 1966 [1830]). Nor was it what Darwin set out to provide when he responded to Herschel's call (Depew and Weber 1995). Another example is provided by the changing status of explanations in physics involving action at a distance (Hesse 1961).

If the analogy between biological and scientific evolution is a just one in these respect, then the phenomena that Hull set out to explain – the increasing pragmatic success of science – is hard to pin down. If the systems whose behavior is to be successfully explained and the standards of successful explanation both change over time, then it becomes at least problematic to say what it is that science is increasingly successful at doing.

I suspect that Hull can redescribe what he is trying to explain in such a way as to sidestep this objection. Perhaps what strikes Hull and other 'conservatives' about science can equally well be described as the outstanding efficacy of science in allowing scientific communities to achieve their goals, without fixing what those goals have been and will become. This is an approach adopted by other authors broadly in the tradition of evolutionary epistemology, such as Wayne Christensen and Clifford Hooker (Christensen and Hooker 2001). Taking on board the criticisms of a simple 'lock and key' model of adaptation which I alluded to above, these authors have tried to develop models of evolution, and particularly of cognitive evolution, in which the organism adapts to achieve its goals while simultaneously adapting its goals. Extending these models to evolutionary epistemology, they allow epistemic agents like communities of scientists to change their epistemic goals at the same time as changing their theories and methods in pursuit of those goals. Conservative social epistemology can take the same route. The 'conservative' element in such a revised account would be that at each point in time the community can provide reasons grounded in their existing theories, methods and standards, for changing their goals. In such an account, the aspects of the social tradition of science that Hull uses to explain the success of science would serve to explain how the tradition has been successful in transforming its theories, methods and so forth in a way that meets its goals at each stage.

There is an aspect of Hull's theory of science, as yet unmentioned, that would make it easier for him to accept such a revision of his approach. Hull has always been careful to separate the task of defining an intellectual tradition from that of characterising its intellectual content. Just as a biological species is defined historically, in terms of common descent, Hull defines an intellectual tradition like Darwinism historically, in terms of sociological descent (Hull 1988). Being a Darwinist is like being a member of the Republican Party. It is of no significance how closely your beliefs resemble those of the party's founders. What matters is the sociological continuity of the organisation of which you are a sociological part. This strategy can be applied to science as a whole. Science is like Christendom – a social tradition ramifying out from certain historical events, in this case the 'scientific revolution'. The epistemological characteristics of science change over time and vary from population to population, just like the characteristics of a biological taxon. Because Hull defines intellectual traditions in this way, his claims about how science works do not have to do double duty as a definition of science – a demarcation criterion. This means that he can allow the goals of science to undergo open-ended change. The sociological character of the scientific tradition can be brought to bear to explain why science has been consistently successful in achieving its changing goals. The sociological character of science is itself, of course, a characteristic rather than an essential or defining property of that tradition. Thus it is an open question for Hull whether science will continue to be successful. Indeed, part of the interest of the project of 'conservative social epistemology' is to identify features of the social institution of science that it might be unwise to change.

Conclusion

David Hull has set new standards for close involvement between philosophers of science and the science they study. He has shown us that the philosophical analysis of science can be introduced into the actual process of science as critical and contestable commentary, rather than being produced after the fact and for a separate audience. In his search for an understanding of the scientific process he has taken a profoundly naturalistic course, eschewing on the one hand a prescriptive epistemology and on the other a descriptive approach that despairs of any general insight into the phenomena of scientific knowledge. In both these respects, Hull's work holds out the prospect of, if not a science of science, at least a natural philosophy.

Notes

1 Dupré (1981) is an honourable exception.
2 For an interesting perspective on Hull's convergence with other forms of sociology of science, see the exchange between Hull and Bruno Latour in Callebaut (1993).
3 See, for example, the debate between Nicholas Rasmussen and Sylvia Culp (Rasmussen 1993; Culp 1994; Rasmussen forthcoming).

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