Sociology, Selection, and Success: A Critique of David Hull's Analysis of Science and Systematics

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David Hull's *Science as a Process* (1988) is an extraordinary book. That's not to say that I agree with all of it — in fact, I have my doubts about some of the central arguments. Nevertheless, it is remarkable in piecing together virtually all of the ideas that Hull has made so much his own. Individuality, essentialism, the Darwinian revolution, and even sociobiology and historical explanation, are woven into one long argument about the way science works. What's more, we're presented with the results of Hull's long-term "field" project — a detailed description and analysis of the activities of his "fruit flies", the pheneticists and cladists. There are other ways to get acquainted with Hull's philosophy, and this task has become even easier with the publication of a volume of selected papers (Hull, 1989) and a collection of commentaries on his ideas (Ruse, 1989). But here we're treated to Hull's own vision of the significance of his work, and for this there is simply no substitute. This book provides a powerful insight into the ideas of a pioneering philosopher of biology, and whether one likes or dislikes Hull's conclusions, it deserves to be read and taken seriously.

NARRATIVE HISTORIES

This is challenging reading, however, because so many ideas are packed in and, as Hull explains, his is a difficult argument to make. Rather than structuring the presentation around the central thesis, with illustrations interspersed, Hull presents a series of historical narratives in the first half of the book, and then, in the second half, provides his general analysis. A staggered arrangement is necessary, he says, because the general arguments would be obscured if detailed historical accounts were interjected. This is a special problem here because Hull maintains that choosing among competing views on the nature of science requires that it be studied "just as minutely as evolutionary biologists study changes in gene frequencies in local populations" (p. 21). This kind of scrutiny entails a significant

Biology and Philosophy **5**: 459—472, 1990.

cost, namely that so "much narrative is required for so little message" (p. 21).

I think the ratio of narrative to message could have been favorably adjusted by omitting some details. For example, what difference did it make that the swimming pool at the Hennig meeting in Grand Forks was "built in the shape of a cowboy boot" (p. 263)? And did it really matter that Gareth Nelson wore white socks (p. 144), that Ed Wiley sported a "chipmunk grin" (p. 254), or that Dan Brooks looked like "the well-groomed sort of man that pharmaceutical companies employ to urge doctors to use their products" (p. 258)? I admit to laughing out loud on occasion, and I suppose that was the intent, but I wonder how all of this strikes the outsider. Come to think of it, Colin Patterson's "resonant voice" (p. 144) just might have made a difference — he really is very convincing!

The narratives themselves are presented in an odd sequence. Hull follows two stories — evolutionary biology and systematics — but neither is devolped in a linear fashion. He begins with the early history of each, from Darwin through the "modern synthesis" in Chapter 2, and from Aristotle through the "new systematics" in Chapter 3. In Chapters 4 and 5 he details the clash between pheneticists and cladists, and he returns to post-synthesis developments in evolutionary theory in Chapter 6. Then it's back to cladistics again in Chapter 7. Considering Hull's emphasis on conceptual lineages, this switching back and forth seemed distracting, but perhaps it does have the desired effect: "eventually, after enough back-tracking, the outlines of the actual course of events begin to emerge" (p. 35). In fact, if Hull is right in viewing science in phylogenetic terms, then following one branch and doubling back to pick up another one may be *the* way to present the account (O'Hara 1988). The trick is to find the real branch points. Hull's account of the recent history of systematics is loaded with minutia that will amuse (or perhaps infuriate) those who were directly involved, and it may be of some interest to younger systematists who didn't live through it all. Apart from a few minor inaccuracies, Hull's chronicle is basically correct — at least it is generally consistent with my own recollections.

I wasn't entirely convinced that all of the narratives were needed. The sections on Darwinism and sociobiology, though fascinating (especially the tidbits on individual personalities), could have been omitted entirely, or some of this material might have been drawn into the discussion by way of comparison to developments in systematics. It is, after all, only in the case of systematics that Hull is able to present the kind of detailed account that he argues is necessary. In recounting the radiation of Darwinism he gives in to a cruder form of analysis, which he admits "cannot serve very well as evidence" (p. 207).

Why, then, did Hull include accounts of Darwinism and the synthesis? He suggests that these contain material needed to develop his evolutionary model, and in the case of sociobiology he hoped to avoid confusion with his own position. But I suspect he was worried that the systematists whom he had studied in such detail were simply too obscure, and he wanted to insure a broader audience: "at least readers are liable to have heard of it [the Darwinian revolution] and be interested in it" (p. 23). [This fear was apparently well-founded: an early review of Hull's book warned that readers would have to "grapple with such scientific buzzwords as 'phenetics' and 'cladistics'" (Angier 1988, p. 16).] Furthermore, although Hull defends his focus on systematics against the "inevitable criticism" that it is an "abnormal, atypical area of science" (p. 24), I sense that he is a bit embarrassed for having devoted so much attention to it. After all, he feels that systematics is "not one of the currently 'hot' areas of science" (p. 24) — that it's "a handmaiden at best" (p. 25). If only he had chosen instead to study mainstream evolutionary biology, "the focus of intense activity by some of the most brilliant scientists working today" (p. 25).

Being a systematist myself, I'm a bit offended by this. There's no need for embarrassment — systematics *is* "hot". And there's no need to worry about generalizing from systematics — it differs very little from other sciences, at least with respect to the attributes that matter most to Hull's theory, especially the desire for credit. On the other hand, Hull may have lavished too much attention on a few individuals who are anything but normal scientists (*whatever* that means) — Leon Croizat, for example.

GENERAL ARGUMENTS

I'll return to systematics and to Hull's perceptions of it, but first it's necessary to consider the arguments he develops in the second half of the book. I see two very general themes here, which are intimately connected; one concerns the analysis of science and the other concerns science itself. The first message is that philosophers and historians of science have paid insufficient attention to "the small research groups that periodically crop up", which he views as "the most important focus of rapid, though usually abortive, change in science" (p. 112). Instead, they have tended to focus either on gross external factors, such as social class, or on internal factors, such as the weight of evidence and the cogency of argument. Hull contends that in so doing "the most they can hope to discern are the distal effects of the causal factors operative in conceptual change in science" (p. 507). Much more attention should be devoted to analyzing the pervasive influence of "the sort of cooperative competition that characterizes the social relations among scientists" (p. 514).

In line with this position, Hull is critical of logical empiricist analyses of science that emphasize inference above all: "what scientists actually *do* is also relevant to an activity counting as being genuinely scientific" (p. 344).

Likewise, he is discontent with interpretations that ignore the temporal and social dimensions of science, and with the science fiction of contrary-to-fact conditionals that still characterizes philosophical discourse on science. Philosophers of science had better be getting their hands dirty taking a close look at what scientists are actually saying and how they behave, and "if such hard work is incompatible with a theory counting as a genuine 'philosophical' theory, then so be it" (p. 29) — "so much the worse for philosophical analyses of science" (p. 302).

But the strength of this first general message hinges critically on Hull's second major claim, namely that he has identified a mechanism "adequate to explain a great deal about the way in which science works, a mechanism which flows naturally from viewing science as a selection process" (p. 285). If Hull's mechanism actually can explain why scientists are organized into research groups and how this organization influences both the behavior of scientists and the nature of conceptual change, then his first point is well taken indeed. That is, attention really would have to focus on analyzing, in gory detail, the social relations among scientists. If, on the other hand, his mechanism seems inadequate, then it may not be necessary to engage in the sort of research that Hull recommends, as titillating as this may be.

Hull points to three elements as underlying the scientific process: "a desire to understand the world in which we live, the allocation of responsibility for one's contributions (both credit and blame), and the mutual checking of these contributions; in short, *curiosity*, *credit*, and *checking*" (p. 305). He is not especially concerned to explain these factors; they're simply taken for granted. Instead, he argues that together they largely account for the social organization and behavior of scientists and, in turn, the process of conceptual change. Other motivations, such as the desire to help humanity, and other attributes, such as humility, are dismissed in no uncertain terms.

As far as I can tell, it is the desire for credit (along with the desire to refute those with whom one disagrees) that is central to Hull's argument. To be sure, curiosity and the "wow-feeling of discovery" (p. 305) are necessary, and testing, which arises as a means of checking one's speculations, is required more or less by definition. But it is the need for credit that drives the system and that most directly accounts for the attributes Hull wishes to explain. Here it is important to clarify that in Hull's view the form of credit that really matters to scientists is not public recognition (he uses Raup's (1986) term "saganization"), nor awards and honors. Instead, it is "the credit that accrues when one scientist makes use of another scientist's work, in particular, its incorporation in his or her own research, preferably with an explicit citation" (p. 283).

Given their desire for credit, Hull contends that "scientists tend to behave in ways calculated to increase their own conceptual inclusive fitness" (p. 319). But increasing one's conceptual fitness also requires support, which entails citing the work of others, and this results in a conflict of interests: "citation automatically confers worth on the work cited and detracts from one's own originality" (p. 319). The upshot is that scientists give credit and cooperate only when they think it will be useful to them. The resulting blend of competition and cooperation fosters the formation of research groups, with positive citations promoting social cohesion and negative citations helping to demarcate one group from another. Furthermore, research groups serve to enhance a scientist's conceptual inclusive fitness by providing receptive audiences — "conceptual niches for the development of new ideas" (p. 395).

If we accept that all of this is basically correct, where does it get us — what exactly does it explain? Hull argues, first of all, that the desire for credit and the resulting demic structure of scientific communities accounts for a great deal about the behavior of individual scientists, their professional relations, and the structure and function of scientific institutions. Secondly, he believes that it substantially influences the development and the content of scientific concepts. I think his claims of the first sort are basically on the right track, whereas I find his arguments on the substance of science less convincing.

SCIENTIFIC NORMS AND INSTITUTIONS

I agree with Hull that the desire for credit "adds a pervasive tension in the professional relations among scientists" (p. 286) and accounts for "the polemics so characteristic of science" (p. 287), including the inattention to new ideas and their inevitable misunderstanding or distortion. Credit is obviously at the root of priority squabbles and the endless wrangling over the use of words, which seem so senseless to outsiders. And it helps explain the evident lack of correlation (perhaps even the negative correlation) between success in science and personable, helpful behavior — why "some of the most impossible people have been among the most productive scientists" (p. 305).

In turn, the social structure of science explains a lot about why scientific institutions function the way they do. For example, confidentiality in the review process may "allow scientists who are working in the same research program to offer criticisms while maintaining a good working relationship" (p. 326). Testing such propositions is a tricky business. For one thing, appropriate controls are seldom available. Thus, comparing the tone of confidential reviews with published book reviews isn't entirely valid, since "it is too late to do much about books that have already appeared" (p. 334). Even more importantly, the outcome of such tests depends critically on the initial assignment of scientists to camps. There is always the danger that results will be explained away as an artifact of not having properly delimited research groups at the outset. In the case of cladists this is a very real issue, since they became increasingly diffuse and eventually, according to Hull, "speciated and began to argue with each other over proper taxonomic method" (p. 339). This last point is highly controversial (e.g., Mishler 1989). Whether or not cladists have actually speciated, I do think Hull is right as regards fundamental conceptual issues — John Beatty (1982) was picking up on something real in recognizing "pattern cladism", heated denials notwithstanding. In this regard, I'm fascinated to see that some of those accused of being pattern cladists have now taken up this label in defending the divorce of pattern recognition from considerations of evolutionary process and the primacy of characters over common descent in defining groups (Patterson 1988).

Hull also addresses the apparent effectiveness of the norms of science — why it is that misconduct is so rare. His argument is that the goals of individual scientists by and large coincide with institutional goals: "scientists adhere to the norms of science because it is in their own self-interest to do so" (p. 394). But Hull's model also accounts for a more specific observation: "it explains why lying (publishing fabricated research) is so much rarer than stealing (failing to give credit where credit is due)" (p. 302). Lying is punished more severely because everyone is damaged by it. Stealing, which harms only the victim, is kept in check solely by the need for support. Corollaries are numerous; for example, it's much safer to steal from a graduate student than from someone with an established reputation.

Some of Hull's arguments take the same form as arguments for adaptation in the biological literature, and this raises the usual epistemological issues (see Sober 1984). In particular, Hull tends to interpret common behaviors as adaptive: "if generations of scientists behave in certain ways, then possibly there is something to be said for this behavior" (p. 302). Possibly so, but are we to believe that such behaviors were specifically selected *for* the function — that they are, in this sense, adaptations — or could they be fortuitous consequences of individual selfishness, possibly balanced by selection against the behavior at the level of the research group. I wasn't always sure what Hull had in mind. I can even imagine that some behaviors are now positively maladaptive, but exceedingly difficult to replace; the persistence of sexual reproduction in some groups of organisms might be a biological parallel.

Many of these issues have traditionally been dismissed by philosophers of science, but Hull makes a compelling case that they deserve more attention. It would be well worth following up on some of the leads he provides. How much social cohesion and competition are enough, and how much are too much? Can scientists be too aggressive in seeking credit? Under what conditions should they claim originality versus explicitly bow to a predecessor? There are many other questions, as well, that might be illuminated by Hull's model. I'm curious, for example, about the correlates and the significance of coauthorship, and about relations among coauthors in the face of criticism.

CONCEPTUAL CHANGE AND CONTENT

While I find Hull's approach promising as regards the individual and collective behavior of scientists, I find his discussion of conceptual change and the content of science much less satisfying. By way of illustrating my concerns, I will outline Hull's model in more detail and then concentrate on his analysis of conceptual change in systematics. Hull begins his argument by developing a general model of selection in biological evolution, with the aim of demonstrating that things are not as simple as they might seem. His primary observation is that selection appears to "wander erratically from level to level" (p. 401) in the traditional organizational hierarchy. Hull's idea is to remedy this situation by delimiting entities in terms of their function in the process of selection. This is achieved by formulating selection in terms of three sorts of entities — replicators, interactors, and lineages. Selection results from the interplay of replication and interaction, and lineages evolve as a consequence. If comparing like to like (e.g., replicators with other replicators, whether these happen to be genes, chromosomes, or sometimes even organisms) facilitates the discovery of regularities that might otherwise be missed, then any damage done to commonsense ontology is a small price to pay.

This material will be familiar to those who have followed Hull's work (e.g., Hull 1980). I liked it when I first heard it, and I still like it. In fact, I think it's the best outcome yet of all of the reflection on individuality in recent years. There are a few items here that I think need more work, but Hull's basic message is not likely to be affected. For example, I'm still not sure that Hull provides an adequate formulation of drift: "differential replication in the absence of interaction . . . replicators but no interactors" (p. 410). It seems to me that there must always be interactors — it's just that changes in replicator frequency are independent of the nature of their interactions. Hull waffles a bit on the relevance of identity by descent versus independently derived similarity in the selection process (pp. 406–407). This issue deserves much more attention, especially as it relates to such things as gene flow and population size, variables that are touched on only briefly by Hull. Perhaps the most significant residuum of confusion in the model revolves around the phrase "transmission of structure" (pp. 414–415). Hull's discussion centers on a few simple contrasts — reproduction by simple division in *Paramecium* versus sexual reproduction in multicellular organisms. I wonder how one would deal with a variety of intermediate cases (reproduction in volvocine "green algae", for example), and how the notion of passing on structure would fare in the end.

Hull's primary motive in developing a general model of selection is to apply it to conceptual change in science. He argues that scientific theories and research programs evolve in much the same way that organisms do, although the specific mechanisms may be rather different (e.g., conceptual change needn't involve DNA). The analogy is extended by specifying what entities function as replicators and interactors in conceptual evolution. Ideas are replicators, and these are transmitted by a variety of vehicles, including books, diskettes, and brains. Of these vehicles, scientists are the primary interactors: "replicators are generated, recombined, and tested by scientists interacting with the relevant portions of the natural world" (p. 436) and, as Hull emphasizes, interacting with one another in research groups.

Having developed the basic mechanism, Hull then takes on a series of objections to viewing conceptual change as a selection process. He considers the potentially rapid rate of conceptual change, the supposed need for discrete entities and biparental inheritance, the problem of cross-lineage borrowing, and claims to the effect that conceptual change is Lamarkian, progressive, and/or intentional. For the most part I find his arguments convincing that there are no major obstacles to considering conceptual change in evolutionary terms. Some of the arguments are almost too easy. For example, the view that biological evolution requires biparental inheritance is simply mistaken, so it is hardly a problem that there is nothing analogous in socio-cultural evolution. The analogy seems a lot sloppier, however, in the case of cross-lineage borrowing. Hull's appeal to plants — they hybridize and lineages might merge — doesn't do much to calm my fears, and I'm a botanist. In contrast to hybridization in plants, the situation in the case of concepts seems to be more like the transposition of elements of all sizes from all sorts of donors. If so, then it may be simply wrong-headed to think in terms of the usual form of gene flow and the level at which borrowing ceases. Perhaps the analogy could be fleshed-out, but it needs more work.

The final link in the argument is to specify what evolves by virtue of the conceptual selection process. Hull develops the view that both social and conceptual lineages undergo change by virtue of the process he has described. Like populations, these are not defined by attributes — neither has an essence. Instead, they are integrated historical systems that can potentially change forever and in every way. Being heterogeneous at any one time and variable through time, Hull argues that such systems are best individuated by something analogous (very roughly) to the taxonomists type method; for example, selecting a particular scientist (G. Nelson) at a particular time (1971) as a node through which to enter the complex web of social relations that integrate a particular research group (the American Museum cladists). The sticky issue of how, exactly, the development of social and conceptual lineages interrrelate, is even further complicated by the fact that they also interface with observations of the world. This observation raises a series of traditional philosophical concerns about operationizing concepts in order to test them and the theory-ladenness of observations, which Hull addresses by way of commenting on Kuhn, Kripke, and Kitcher.

Where does all of this leave the central issue of the influence of the social organization of science on conceptual change and the content of scientific theories? Hull contends, and I agree, that the social structure of science helps maintain conceptual variability and encourages testing. Beyond this, he maintains that "the formation of research groups affects conceptual change in science in two ways. One is generic – it increases the speed of conceptual change regardless of the specific content of the accompanying research program – but these allegiances and alliances can also have at least short-term effects on the content of science" (p. 507). I'm not entirely sure that the rate of change is always increased. In fact, it occurs to me that social factors could slow it down in some cases. Cladistics might even be an example, with personal squabbles and dogmatism (on all sides) impeding its acceptance and further development. And regarding content, I wonder whether there is ever anything more than a short-term effect.

What would really help at this juncture is a concrete example of a long-range effect. Hull suggests several possibilities, but doesn't provide enough details to back these up. For example, he believes that "the animosities that developed between Sokal and Farris strongly colored the subsequent development of cladistics and numerical taxonomy" (p. 507), but he doesn't say exactly how. Maybe this could be done by reference to the detailed accounts presented earlier in the book, but it isn't obvious. There were certainly short-term repercussions – some issues were addressed before they might have been and attention might have been temporarily diverted from other questions. But what long-lasting effects have there been on the content of cladistics, for example? Lacking this connection the argument seems incomplete.

THE "SUCCESS" OF "CLADISTICS"

Fortunately, in the final chapter Hull returns to the major conceptual shift that he studied in detail, namely the choice by systematists of cladistics over phenetics. A convincing explanation of this shift in terms of his model would go a long way toward insuring the acceptance of his entire argument. In fact, so much hinges on this case that it bears very careful examination.

Hull offers two reasons why phenetics "lost" and cladistics "won", or at least is "succeeding beyond anyone's early expectations" (p. 24). First, "numerical taxonomists branched out too quickly . . . they dissipated their energies in applying quantitative techniques in too many areas" (p. 519), whereas cladists "largely confined themselves to biological systematics" (p. 519). Second, and more importantly, pheneticists "offered systematists a plethora of techniques" (p. 519) whereas cladists "presented them with a method — one method — and they could use it without becoming experts" (p. 520). I will return to these points in a moment, but first I want to consider whether cladistics has, in fact, been successful.

This is a trickier issue than Hull lets on here. It is clear enough that there has been a shift away from phenetics — it's been dead in the water for some time. But what has the shift been to? "Cladistics" is the easy answer, but what is this exactly? This isn't a minor quibble because, as Hull has carefully documented, the differences among "cladists" are so great that they might well be considered two (or more) groups in so far as underlying conceptual issues are concerned (if not socially). In fact, it has gotten to the point that if some "cladists" feel that they've won, others will feel positively defeated. For example, if the view becomes accepted that taxa are defined by characters (e.g., Nelson 1989; Patterson 1988), I will personally feel that the revolution has been lost or that it never really got off the ground (e.g., Donoghue and Cantino 1988; de Queiroz and Donoghue 1988), whereas if Hennig's view wins out that taxa owe their existence not to characters but to the process of descent (Hennig 1966; de Queiroz 1988), then I imagine that some "cladists" will feel that "cladistics" has failed (compare Nelson 1989, to de Queiroz and Donoghue, forthcoming).

To a considerable extent, whether one thinks that cladistics has won (and how it won) depends on which people and issues one chooses to focus on. In this regard it is worth noting that Hull made an important set of decisions at the outset of his analysis. He chose, for example, to concentrate on Gareth Nelson and the American Museum of Natural History, and on the issues that captivated that group at the time and that have attracted Hull himself (e.g., the ancestor problem; Hull 1979). These are fine choices in many respects, and it certainly makes the job a lot easier, but there were certainly other people in other places that just might have affected the choices made by systematists at large, especially graduate students. There were, for example, little groups of cladists in Michigan, Kansas, Massachusetts, California, and throughout Europe, developing semi-independently of the group in New York. The southern Californians were even named — "hot tub cladists" — and perhaps had a style of their own. Developments in botanical circles are barely mentioned, despite the influence of Herb Wagner, and the early contributions of Kåre Bremer and Hans-Erik Wanntorp that presage more recent attention to species concepts and reticulation.

There were other issues in the air, as well. For example, I was surprised that Hull hardly mentioned the heated controversies surrounding the use of parsimony (versus distance methods, maximum likelihood, and character compatibility) in phylogeny reconstruction (involving Steve Farris, David Swofford, Joe Felsenstein, Elliott Sober, and George Estabrook, among others), especially considering the juicy stories this would have provided. As a matter of fact, I think this issue did have an important impact on the community — at least I know that I was worked-up over it as a graduate student. I'm not trying to build a case that Hull really missed the boat in making the choices he did, but it is important to recognize that he was rather selective and that this could make a difference. This selectivity problem is likely to be more serious in applying Hull's approach to other cases. Developments in systematics might reasonably be traced by following just a few research groups and journals, but this may be somewhat unusual.

Even having decided which people and issues to follow, it isn't obvious how best to measure success. Hull takes the widespread use of cladistic methods to be a measure of its success. This is an obvious and convenient measure, but it doesn't necessarily capture whether there have been any deep and long-lasting changes in the way systematists see the world. To be sure, everyone uses cladistic methods, but how do we know this isn't just a fad, lacking an underlying intellectual commitment on the part of most users? I may be wrong, but I don't think that most users have experienced a profound change in their view of the goals and principles of systematics. For example, taxa are still taken to be classes defined by traits rather than systems defined by ancestry (and diagnosed by traits). In fact, most systematists, including most users of cladistic methods, have long since managed to convince themselves that they needn't be especially concerned with such issues, with the result that cladistic classification (or, better yet, phylogenetic systematization; de Queiroz 1988) is hardly a popular item. In this respect, at least, "cladistics" has yet to be very successful.

If we ignore these complications and focus only on the shift in the use of methods, has Hull provided an adequate explanation for the success of cladistics, or is there more to it than the factors he identifies — remaining focused and providing an easy method? Hull's explanation seems to imply that systematists are basically a feeble lot, easily overwhelmed by too many extensions and unable to cope with anything but the simplest methods. I think this does a real disservice. On the contrary, budding cladists were grappling with an exceedingly complex set of issues and were faced with a wide variety of methods (several of which I mentioned above). They were also busy extending phylogenetic thinking in all sorts of new directions — to the study of linguistics, biogeography, speciation, and adaptation, to name a few. More importantly, Hull's explanation effectively leaves out the interests of systematists and their ability to reason — it leaves out the power of ideas themselves. Why couldn't it be that systematists opted for cladistics because the ideas made more sense in view of what they wanted to achieve? They wanted to know how things were related phylogenetically, and they realized, with the help of Hennig and others, that overall similarity was simply the wrong tool for the job. If they wanted monophyly then they needed synapomorphy. Maybe it's as simple as that.

The strangest thing about all of this is that Hull himself, in the literature and elsewhere in the book, provides plenty of reasons to reject phenetics (and pattern cladistics) and to opt for phylogenetic systematics instead. For example, he argues at length that there is no such thing as theory-free observation, and that any approach founded on such a premise is badly misguided. Does Hull suppose that systematists are incapable of thinking such issues through for themselves? And if they actually are contemplating such things, why is this left out of Hull's explanation? Do ideas really make so little difference?

Hull doesn't deny the worth of ideas altogether, but his analysis is intended to play down its importance: "one of the main themes in my own view of science is that the actual impact that a scientific work has is at least partially independent of its 'inherent worth,' assuming that the latter notion makes sense" (pp. 111—112). He concentrates instead on which strategies will maximize and which ones will minimize impact. If you want to get ahead in science, then join a research group (or start one), be an enthusiast, state your position again and again, and be willing to fight for your ideas. This, I would say, is a recipe for short-term success. If you're interested in long-term success — in having a real impact on the direction that the science takes — then you'd better have some powerful ideas. Social skills might help (perhaps more so in some fields than others), but they're no substitute.

The reason why ideas themselves play an important role in the long run is that they can be evaluated in relation to the world. It seems to me that Hull grants insufficient importance to the attention scientists pay to the world. In fact, I think he misinterprets their primary goal in testing their ideas. Their intention, according to Hull, "is to bring as many scientists as possible to accept the same general formulation of a particular view" (p. 511). In contrast, I would say the ultimate goal is to find out whether they are on the right track in understanding the world. As idealistic and naive as this sounds, I think many (perhaps most) scientists really do care about truth. The truth certainly isn't all they care about, but it has to figure in the equation somehow, and I suspect that its importance increases as a function of the time-scale under consideration. In the long run it's likely to be a decisive factor.

CONCLUSIONS

Having written a somewhat critical review, I want to reemphasize what a tremendous book this really is. Hull set his sights almost unimaginably high and he's taken a courageous stab at many of the biggest issues confronting philosophers of science. The ideas are bold and loaded with wisdom, and it is genuinely fascinating to see him develop the argument. And besides grand ideas, Hull has provided a detailed account and interpretation of a fascinating period in the development of systematics. Regardless of my disagreements, I have enormous respect for David Hull and for his monumental effort to come to grips with how and why science functions as it does.

Unpacking a complex functionally organized system is difficult because cause and effect inter-penetrate so extensively. Many elements in the system seem to be both a cause and an effect, and they function in both capacities somewhere in the analysis. The result is that it is sometimes difficult to pick out what's driving the system. In fact, I'm not entirely sure that I've given an accurate account of Hull's model — that I've emphasized the points he feels are most important. Hull observes that "when new ideas are first introduced they are rarely expressed with sufficient clarity. Scientists do not know what it is that they intended to say until they find out what other scientists think that they have said. Misconstrual verging on parody is part of the process of clarification" (p. 288). I suspect this applies to philosophers as well. Hull's ideas will undoubtedly be reworked and clarified by his critics, his followers, and by Hull himself, and I'm certain that a lot of good will come of this.

In the end, I come away convinced that Hull has identified a set of factors, related principally to the desire for credit, that do influence — or even largely explain — the behavior of individual scientists, their organization into research groups, and many attributes of science as an institution. However, I'm not satisfied that these factors also have a significant impact on the content of science and the choices that scientists and scientific communities make among alternative ideas. In fact, I think Hull's explanation for the major conceptual shift he considered in detail — from phenetics to cladistics — misses the mark. At least, it is incomplete. The rate of change may be influenced by social factors and the direction of change might be affected temporarily. But over the longer haul my guess is that the worth of ideas plays a more significant role in conceptual change than Hull is willing to admit. Scientists are as attracted to reason and are as swayed by it as philosophers are — and this just might make a difference.

ACKNOWLEDGEMENTS

I am grateful to John Beatty, Kevin de Queiroz, Shawn McLaughlin, Nancy Moran, Michael Ruse, and Elliott Sober for helpful comments, and to David Oldroyd for patiently awaiting my manuscript.

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