Metascience[1,2]

Leigh M. Van Valen
Department of Ecology and Evolution
University of Chicago
915 E. 57 St.
Chicago, Ill. 60637

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By metascience I mean the study of science, where science itself is the object being studied. Metascience has philosophical, historical, sociological, anthropological, scientific, and undoubtedly other aspects. These aspects intergrade with each other and are often combined in various ways, whence the need for a unifying label. Hull's book combines all the aspects named but is primarily a study in the science of science, a combined theoretical and empirical treatment of how science, as he views it, progresses. As far as I know it is the first such study (as distinct from more narrowly focused ones), and it sets a high standard for its successors. Nevertheless, I think the analysis is wrongheaded in an important if remediable way.

The main thesis of the book is nowhere clearly stated, although the writing itself is mostly lucid. Overbaldly put, Hull argues that science proceeds by a process of selection within and among conceptual systems of greater or lesser scope, and that evidence (with explanatory scope) and social interactions within the scientific community are both important in this selection. He supports this thesis with a detailed study of the vicissitudes of two schools of systematics, cladism and phenetics. Although he glosses over broader applications, presumably changes in nonscientific subjects and in other aspects of culture have similar causes, commonly with less or no emphasis on evidence and demonstrative argument and therefore with less or no real progress. (One can perhaps reasonably say that subjects controlled by taste progress to the extent that a greater diversity of options is available at a later time. No contemporary of Homer could have composed Bach's concertos.)

In addition to presenting his data-base on systematics (and there is now at least one anthropologist whose subject is anthropologists), Hull gives less detailed but fresh treatments of the history of evolutionary thought and of systematics as a whole. They may be the best summaries of their length available, although as usual the major contributions of Schmalhausen are ignored. This is despite oblique reference to earlier Russian work and despite the concordance with a main thesis of the book which is provided by Schmalhausen's small influence elsewhere. (Some actual errors: The term "gene" is said to have come into evolutionary biology from molecular biology. Norman Newell's name is consistently misspelled. An order of mammals is renamed "Primata". There is an odd misinterpretation of Darwin on species on p. 501. A dubious claim: Any functional definition is defective, so that "if hearts are defined in terms of pumping blood,

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[1]Contribution 94, Lothlorien Laboratory of Evolutionary Biology
[2]**Science as a Process.** An Evolutionary Account of the Social and Conceptual Development of Science.
David L. Hull. 1988 (31 October). Univ. Chicago Press. xiii + 586 pp. ISBN 0-226-36050-4. Hardbound. $39.95.
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then when they are not performing this function, they are not hearts" (p. 406). This is easily remedied by defining a heart in the conventional way as an organ whose function is to pump blood, and the remedy is immediately generalizable.)

As far as I know no one has treated the origin of the synthetic theory of evolution as an example of Hegel's dialectic (thesis, antithesis, synthesis). A careful consideration from this perspective might well be mutually illuminating; there is more than metaphor involved.

The data-base is detailed and makes delicious reading for a marginal participant. The emphasis is on social interactions rather than the science itself, and this is probably useful because the science is already published, if scattered. Is it accurate, though? Here I have doubts. There are four references to me, none adverse. Three of them, i.e. all except a straightforward citation, have small to large errors. I have no idea how representative this tiny sample is, but it doesn't foster wholehearted trust in the rest. Like other data, these need independent checking.

The low attention to the actual science may well distort the conclusions. Before the advent of either of the schools primarily discussed, systematics was practiced mostly as a craft. In this it resembled in some real ways molecular or developmental biology today. Apart from basic principles, one learned by doing, as an apprentice. This atmosphere was one in which generalization, even at the level of estimating phylogenies, was a bit off-color, despite the existence of notable exceptions. Into this theoretical vacuum came what was later called phenetics. It probably would have had more success if it hadn't required more mathematical ability than most systematists had or wanted to have, in addition to the social difficulties which Hull emphasizes. So an overall vacuum remained, despite some visible (but not socially pushed) theory in the predominant synthetic approach. Even in the past few years some adherents of the latter approach haven't understood either it or its alternatives, apparently from inertia. The vacuum was then entered from the other side by cladism, with a simple and explicit theoretical framework. A vacuum is easily invaded by aggressive expansion, and I think that this internal contrast was critical. The relative importance of this and the social groups, on which Hull concentrates, will remain unknown until these hypotheses are evaluated comparatively.

The book focuses on why the activities of scientists result overall in scientific progress, but most of the hullaballoo described has at best little to do with that. There have been some real, even major, advances in the time period discussed, but they are mostly peripheral to the political infighting. Numerical methods did indeed originate largely from pheneticists, but these have been incorporated, where useful, into all approaches. Explicit criteria for the estimation of phylogenies did indeed originate with Hennig, the founder of cladistics, but they also originated independently (partly the same and partly different) from several others, both earlier and later (but before Hennig's work became known to most of us), with various approaches to classification itself. Molecular methods have no visible relation to any approach. It isn't these things that the fighting has been about. The wars in systematics have concerned not the substance of science but its expression: having whatever information and methods one wants, what criteria should be used to incorporate the results into a classification? This question lacks an objective answer. There are metacriteria which one can appeal to, but these again are not unique and different metacriteria give different optima. None have as yet gained overall support. (I nevertheless think that the relationships of the metacriteria to the competing views are often misstated, so some rational progress may be possible.) Only power can arbitrate genuinely basic questions of taste, and that is why there has been war in systematics.

If so, the data set may be quite atypical and inferences from it may have low generality within science. Hull mentions sociobiology as a case where there was similar virulence; the exception probes the rule and the rule stands. The sociobiology war was also basically not about science. In this case it was about politics, the uses of science; I happened to be visiting the principals the day when it erupted. There are, and have been in the recent past, other and more basic controversies about which I know something, such as neutralism or punctuation or plate tectonics or evolutionary catastrophism or group selection. All have aroused strong feelings but none have led to ferocity. All are capable of being resolved objectively, and the participants are, at least mostly, willing to be convinced by evidence. That doesn't mean that they won't try to reinterpret apparently opposing evidence so as to defuse its opposition, but it does mean that they can rely on more than social power. I predict a general inverse relationship between the intensity of a scientific conflict and the perceived ease of its objective resolution, and a direct relationship with perceived importance of the subject.

Hull does have an implicit general model. It is based on the following definitions (pp. 408-409):

"replicator -- an entity that passes on its structure largely intact in successive replications."

"interactor -- an entity that interacts as a cohesive whole with its environment in such a way that this interaction causes replication to be differential."

"selection -- a process in which the differential extinction and proliferation of interactors cause the differential perpetuation of the relevant replicators."

"lineage -- an entity that persists indefinitely through time either in the same or an altered state as a result of replication."

It is possible to note difficulties with these definitions. For instance, just a few pages earlier Hull notes that "in many sorts of organisms, the distinction between [sic] individual survival, reproduction, and growth is not all that clear", and he returns to the topic at greater length a few pages after the definitions, even recognizing fuzziness among hierarchical levels. Yet his definition of selection is conventionally vertebrate- and individual-centered, ignoring selection by differential expansion. The definition of "lineage" applies to clades generally, but this is apparently not meant (pp. 504-505).

Anyway, for Hull "Identity by descent is necessary for an analysis of science in terms of selection" (p. 447). Conceptual systems and theories lack essences; their only general unification is historical. Views change, and the changes are often denied or minimized. There are both social lineages and conceptual lineages; they don't coincide, although borrowing is less than usually thought and borrowings are often transformed in the process. External forces have a minor influence except for "the sort of cooperative competition that characterizes the social relations among scientists."

"Just as in biological evolution, traits that 'twinkle' [appear sporadically] are not genuine traits, in conceptual evolution themes or unit ideas that crop up now and again cannot count as the 'same' in an evolutionary sense -- they are not [i.e., do not represent] conceptual evolutionary homologies" (pp. 515-516). However, I fail to see a requirement for identity by descent for selection in either biology or metascience. Phenotypes and ideas reappear, and even with positive selection most individual mutations are lost by drift. The supposed requirement is related to Hull's advocacy of treating species as individuals in a formal sense, a position which to me distorts the biology to conform to a rigid philosophical classification, rather than changing the latter to conform to its real data (Van Valen, 1977, 1988). Hull actually seems to argue (p. 505) against synthetic classifications because they don't fit the received philosophical categories of individual and class! He develops his position consistently, to the extent of explicitly denying that anthropology can have laws, even though it does.

The perspective creates a bias in Hull's historical interpretation. A striking example is his implication (pp. 213-214) that Simpson thought that species have universal characters, a view quite the opposite of Simpson's and merely a result of Simpson using the word "class" differently from Hull. Similarly, Hull thinks that in the traditional (pre-Ghiselin) interpretations of species, selection among them is impossible. Because Darwin, Schmalhausen, Simpson, and Wright, e.g., advocated such selection, might this not be a hint that their concepts of species don't fit the received class-individual dichotomy?

Even (polyploid) species sometimes have multiple origins, and this also happens to be the case for cladistic classification (e.g. Säve-Söderbergh, von Huene, Reed) as well as for explicit methods of estimating phylogenies (e.g. Gregory, Throckmorton, Van Valen). A relevant biological term is Lawrence's "polyclone", which refers to a clone-like group of identically determined cells for which the determination occurred in several predecessor cells rather than in one. Like social groups, viewpoints commonly have fuzzy edges, multiple origins, interchanges and overlaps to form networks, and fissions. None of this prevents selection on them, and a main value of Hull's book is documentation of such selection despite a rather poor fit of his data to his formal theory.

Hull doesn't see a useful sense in which sociocultural evolution is "Lamarckian". Surely, though, the variation of expressed ideas and social pressures isn't "random" (unrelated to adaptive need), even though it rather looks that way for the prescientific history of hypotheses on dinosaur extinction, which has been aptly described as "the multiple-working-hypotheses method gone mad" (Vogt and Holden, 1979). Such positive relation of new variation to adaptation is what advocates of Lamarckism in biology have meant by it, although this is indeed not true of all opponents.

"However, what matters most in science is not what a particular scientist intended to say but what others took him to be saying" (p. 205). "However, the more distant in space and time an undisseminated discovery is, the less important it is" (p. 376). What matters to Hull is not the quality of the science but its influence. To him the social effect is not just an important aspect, but rather all that need be considered. Quality comes in only as one determinant of influence; it has no independent value. Apart from the theory of conceptual change, the book is descriptive rather than evaluative. There is little consideration of what scientific advances actually occurred or even why individual systematists or groups took the positions they did. A closer look at the real science would show, as I have indicated, that it wasn't tied to social groups.

Yes, reputation is currency, as Hull emphasizes. There are people who are money-grubbers and there are people who are prestige-grubbers, but others have other values which they may place higher. The values which motivate scientists are varied and come in diverse combinations: money, power, reputation, joy of discovery, practical use. I know different scientists who have each of these as primary. The distributions of the relative importance of such scientific currency, surely different in different countries, times, and fields, would be an interesting if difficult empirical study.

Doing science is something radically different from the dissemination and expansion of its results. Historians have their own culture, one which focuses on power and influence. At least in this book, Hull accepts the values of that culture. However, to study scientists is not to study science except in a derivative perspective, one where science is regarded as a generally accepted body of knowledge. The study of how this body of knowledge evolves is an important subject, and Hull has written a major treatment of an aspect of it. A scientifically naive reader may, however be excused for concluding that science is just another form of influence. It isn't, and that it isn't is what underlies progress in science and in other rational fields of inquiry, including history. The importance of the distinction is difficult to overemphasize. Science is public discovery, not public relations.

I don't want to end negatively. The book is too good for that. For instance, it reviews critically, and thereby tests, several views on social effects of science. And there is even a new bit of interesting science: We usually think of meiotic sexuality as being overwhelmingly prevalent nowadays. By most reasonable criteria, though, it is still subordinate to ameiosis (p. 429).

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