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WHY FALSIFICATION IS THE WRONG PARADIGM FOR EVOLUTIONARY EPISTEMOLOGY: AN ANALYSIS OF HULL'S SELECTION THEORY*

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Contemporary empiricism has attempted to ground its analysis of science in a falsificationism based in selection theory. This paper links these evolutionary epistemologies with commitments to certain epistemological and ontological assumptions found in the later work of K. Popper, D. Campbell, and D. Hull. I argue that their assumptions about the character of contemporary empiricism are part of a shared paradigm of epistemological explanation which results in unresolved tensions within their own projects. I argue further that their claim to be doing a science of science is not defensible. Hull's selectionism is analyzed to show how this epistemological agenda has played itself out in late empiricism. I suggest some directions that Hull might take toward an historical epistemology.

1. Introduction. D. Hull's *Science as Process* (1988b) articulates a variant of the falsificationist accounts of scientific change that are based in selection theory. Such accounts are currently known as evolutionary epistemology. The naturalized falsification approach to scientific change is most clearly exemplified in the natural selection epistemology of K. Popper's more recent work (1972b, 1984) and D. Campbell's (1960, 1974, 1987) selectionist explanation of the origin of perception and science. I will define as falsificationist any model that follows the Popperian ex-

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plication of rational scientific practice as a process of error elimination founded in a mechanism of conjecture and refutation.[1]

All of these views are attempts to salvage an empiricism of a decidedly positivist sort from its modern and postmodern critics. I will show how evolutionary epistemology as falsificationism is explanatorily dependent on a set of epistemological and ontological commitments that actually undermine its project of naturalizing epistemology. These commitments can be stated as follows:

1. *The objectivity of science is connected to a nonintentional characterization of the process of scientific change*. That is to say, falsificationism claims that the global directionality of progressive scientific change is not a function of the aims, interests, or intentions of the individual scientist. The validity of scientific theories or practices is independent of specific mental characteristics of its practitioners, such as their particular commitment to truth seeking or their unbiased observations. Rather, objectivity is a characteristic of the methodological practice as a whole.

2. *Scientific change is described as teleological in a strong sense*. Science as science is globally progressive. Theory change in science is directional in the sense that, as one theory replaces another, some positive factor (such as empirical content or verisimilitude) increases over time. Science's success is like that of a "smart" missile homing in on its fixed target. For selectionists like Hull, the teleology of scientific progress is external since its goal exists outside the process itself. Given certain characteristics of the process, the goal (i.e., discovery of the fixed laws of nature) *causes* the global directionality.

3. *Some form of convergent realism is assumed*. The process of scientific inquiry brings us closer to the truth, or to the one best characterization of fixed laws governing physical reality. The commitment of evolutionary epistemologies to a convergent form of realism has been widely noted (Shimony 1981, Putnam 1983a, Clark 1984, Bradie 1986, Ruse 1989).

I will show how this set of commitments is connected with recent empiricism's attempts to reformulate itself on naturalistic grounds. Furthermore, this set of commitments can be shown to be incompatible with claims, like those frequently made by evolutionary epistemologies, to be doing purely descriptive epistemology, the "science of science", or epistemology without normative content. I will demonstrate how these com-

[1]M. Bradie has distinguished two programs within evolutionary epistemology. One includes those like Konrad Lorenz that give an evolutionary account of the origin of cognitive processes in animals and humans. These Bradie calls "evolutionary epistemology mechanism" (EEM). The type of interest to us here are Darwinian selectionist accounts of theory change like those of Popper, Hull and Campbell. These extensions of evolutionary explanations into epistemology proper Bradie calls "evolutionary epistemology theory" (EET).

mitments, especially to convergent realism, function within Hull's selection theory. I will argue that this set of mutually reinforcing commitments renders falsificationist evolutionary epistemologies unacceptable as either epistemology or as evolutionary science. I will suggest directions for a genuinely naturalized evolutionary epistemology that accommodates an historical and a realist construal of scientific theory change that does not need convergent realism to justify or explain scientific progress.

2. Field Work: Describing the Descriptive Epistemologist. I begin with some "descriptive" epistemology. I will describe the evolutionary epistemologists *at work* on their central task of explaining the success of science. We need to be clear about why evolutionary epistemologists are compelled to take the question of science's success as the central puzzle for a theory of knowledge. Furthermore, we need to understand how certain epistemic and ontological commitments determine which solutions are acceptable to evolutionary epistemologists.

Currently, evolutionary epistemologists of the falsificationist variety share a strong commitment to classical empiricism and realism. These commitments lead to (1) a strongly empiricist interpretation of scientific justification; (2) a progressive interpretation of scientific change as converging on some true or more adequate final description of the world; and (3) a correspondence view of truth. Constrained by these commitments, they must explain how science increases our understanding of the natural world. Thus Popper asserts that "the most important of the traditional problems of epistemology (are) those connected with the growth of knowledge" (1959, 22).

This explanatory task must be seen against the background of contemporary empiricism's struggle with various versions of the problem of induction. The recognition that any theory, no matter how well "confirmed" in the old empiricist sense, is radically underdetermined by perceptual evidence makes the success of an empirical science a puzzle. An orthodox foundationalist approach based on the justifying perceptions of individual scientists as the test of reality or truth is no longer seen as a viable explanatory possibility for late empiricists. As Hull puts it, "Some epistemologists have construed their task in such a way as to make it impossible. They view knowledge acquisition in terms of an individual subject who confronts objects of knowledge in total isolation from other knowing subjects" (1988b, 13). He argues that if science's status as a knowledge-generating enterprise is taken to be grounded on "private" individual observation then we are faced with unsolvable puzzles concerning the apparent progressiveness of science.

Furthermore, although correspondence to a mind-independent reality has been the central model of truth for falsificationists, a simple iconic approach is now universally regarded as naive. The invisible world remains invisible. Thus, the burning question is how the progressive correspondence of theory to the world is to be guaranteed, whether that correspondence is construed as representational or as nonrepresentational (Munz 1985).

The contemporary empiricist is left with several problems. First, given a commitment to a correspondence theory of truth, he or she must explain the success of science in a realist, not an instrumentalist, mode. Second, some passive, nondistorting and direct test of theory against the stable, but invisible and unimaginable, structure of reality itself must replace the individual perceiver.

Popper can be read as taking just this tack. Following Hume, Popper showed that, in spite of confirmationist hopes, no amount of observational evidence could ever increase the probability, let alone guarantee the truth, of a given hypothesis. Popper's (1959) initial problem, therefore, was to save the empirical grounding of scientific theory in the face of the failure of inductive logic. Popper shifted the explanatory problem from an analysis of justification to that of falsification, thereby sidestepping the problem of induction altogether. This gave him the advantage of grounding potential falsifications on single observations, regarded as self-validating. But, Popper realized that what counts as an observation statement is dependent in the end on community agreement and is thus conventional. He says in "Conjectural Knowledge" (1972a) that all observations and all observational statements are conjectural and theoretical in character. This led him to the view that "all languages are theory impregnated; which meant, of course, a radical revision of empiricism" (ibid., 30). This need for a radical revision led him to connect his theory of scientific falsificationist methodology with a Darwinian theory of natural selection. The failure of traditional empiricism to ground its understanding of the success of science in an observational base while simultaneously maintaining its basic assumptions about the character of truth and objectivity leads to the "naturalization" of epistemology.

The "naturalization" of empiricism might be more accurately seen as a move toward externalized empiricism (Gibson 1988, 54). In Quine's naturalized epistemology the relationship between sense data and theory is replaced by the "external relationship" between observational sentences and stimuli. As Gibson puts it, "Sentences, or their utterances, and stimuli are out in the open, accessible to the intersubjective techniques of study characteristic of natural science generally" (ibid.). So, too, Popper's naturalizing move is toward what Campbell calls an "epistemology of the other one" (1974, 141). Such a view does not seek an account of knowledge based on first-person observational certainty, but rather focuses "on the problem of how people in general, or other organisms, come to know". It becomes an empirical enterprise, a science of science.

3. The Selectionist Paradigm: How Can a Blind Process get Anywhere? Campbell, taking his lead from Popper, claims that the selection paradigm, a generalized version of the Darwinian mechanism of evolutionary change by random variation and selective retention, is the universal nonteleological explanation of teleological achievement (1987, 54): "Such a general selection theory can be regarded as the all purpose physicalist solution to puzzles of *design*. . . .For the epistemologist of scientific belief, the design puzzle is the apparent fit between belief and the invisible world to which the belief refers" (Campbell and Paller 1989, 232). The adaptive features of organic forms that appear to be deliberate achievements of the causal process from which they arose can be explained in totally mechanistic terms (nonteleologically) as the result of random variation (random with respect to the current environment) and selection. So, too, the movement of theory in the direction of verisimilitude, if not truth, is explicable from a selectionist perspective without explanatory recourse to the epistemic intentions or the genius of the individual scientist. For the evolutionary epistemologist, the success in question, namely, the fit of our perceptions to the referent, the fit of theory to the world, the development of increasingly powerful predictive capacity, is strongly analogous to the fit of an organism to its environment. It is explainable—just as the amazing and apparently "intended" mimetic adaptations of the stickbug to its bushy surround are explainable nonteleologically—as the result of the fortuitous fit of random variants to an existing environmental challenge.

For Campbell, selection theory is the only available solution to the puzzle of design of which the puzzle of the growth of knowledge is but an example. Selection theory is, he argues, the only appropriate candidate to "explain" the successes of science, "A blind variation and selective retention process is fundamental to all inductive achievements, to all genuine increases in knowledge, to all increases of fit of system to environment" (1987, 56). On Campbell's view, the only alternative to selectionism, either for evolutionary or epistemological explanation, is to assume a directive given by clairvoyant powers of some sort, a direct intellectual access to the noumena (Campbell and Paller 1989, 237).

What the evolutionary epistemologists notice in relation to their reading of the neo-Darwinian model of species evolution, says P. Thagard (1980), is that "variation, selection and transmission are also features of the growth of scientific knowledge" (p. 189). Thagard argues that these similarities are superficial and that the disanalogies between species evolution and theory change are too deep to make the comparison useful. I will not recount the details of his criticism here, nor do I concur with all of it.[2] Even comparisons which have a rather large negative-to-positive ratio of analogy may be exceptionally revealing in both analogical directions. The most telling of Thagard's objections is shared by other critics of the selectionist program (Skagestad 1978; Rescher 1977, 157). He denies that theory generation, like the generation of genetic variants in organisms, is random in relation to the demands of the environment, "One does not have to suppose there is some algorithmic logic of discovery to see that when scientists arrive at new ideas they usually do so as the result of concern with specific problems" (1980, 188).

Hence, unlike biological variations, conceptual variations arise in relationship to specific environmental conditions (ibid.). Nevertheless, for Campbell, this portion of the analogy is the most crucial. Conceptual variants are not taken as "answers" to a given environmental challenge. In the face of much criticism, Campbell is adamant on this point. He insists that selection processes, including visual perception and scientific theorizing, are "blind" in that "variations emitted are independent of the environmental conditions of the occasion of their occurrence" (1987, 57). For Campbell's hardline empiricism, perception provides a profoundly indirect access to the world. In his words, sight is an example of a vicarious process, like radar. Such processes allow for exploration of the world via a "profoundly indirect" method of trial and error. The tapping cane of the blind person, my sighting of the sycamore tree outside my window, the posing of an hypothesis, the random mutation of a gene, are *always* stabs in the dark.

In this regard, he follows Popper in taking observation itself to be conjectural. In a science structured as a process of conjecture and refutation, this "blindness" functions in creating an unbiased process in that it "maximizes the opportunity for 'physical reality' to optimally influence the new consensus" (Campbell and Paller 1989, 248). The *invisible* world itself must directly act to effect the differential survival of beliefs if the process is directed toward verity. Such a process cannot *depend* in any determinate way on a prior selection of any hypothesis as more likely. Scientists may, in fact, engage in the abductive behaviors suggested by Thagard. Nonetheless, on Campbell's view, since no hypothesis can be rendered more probable than any other within a strict falsificationist anal-

[2]Thagard's criticism takes for granted a fairly standard interpretation of neo-Darwinian selection theory. However, some of the features of organic evolution that he takes as disanalogous to theory change are just those features that are currently under debate in evolutionary biology. For instance, he claims that development of species is gradualist, whereas conceptual change comes in leaps. This is of course the point of contention between evolutionary gradualists and those like N. Eldredge and S. J. Gould (1972). They claim that the evidence of the fossil record is compatible with evolution having periods of stasis and periods of relatively rapid change, viz., punctionated equilibrium.

ysis, such prior selection must be seen as ultimately unjustifiable. Within a selection process, abductive inferences must be rendered insignificant in their effect on the mechanism of scientific progress. Therefore, Campbell's insistence on the blindness of hypotheses generation is required if he is to solve the puzzle of scientific progress as he and others like Hull have constructed it. Even heuristics of hypothesis generation, Campbell tells us, are themselves the end products of a selective process, as the vertebrate eye is the result of a process of blind variation and selective retention. They serve as more efficient ways of scanning a problem space as contingently generated and locally honed tools for the "vicarious exploration of vicarious representations of the environment" (Campbell 1987, 66).

4. Hull's Selectionism: The Methodology of Disillusionment. Hull's selection model of conceptual change in science explicitly accepts the problem of explaining the success of science, "The most puzzling feature of science is that it works so well in realizing its manifest goals" (1988a, 125). His account can be taken as an extended attempt to explain that success defined in relation to science's goal of developing an increasingly more adequate empirical understanding of the world. Hull begins by disclaiming any intention to answer traditional epistemological questions (1988b, 13). The project is to come up with a general account of conceptual change in science in terms of a selection process. Thus, Hull sets out to give an empirical account of empirical inquiry. His stated aim is to show "the interrelationship between social and conceptual development in science which results in science having the features it does have" (ibid., 12).

The feature of modern science around which Hull's account revolves is its "success" at achieving its goals. Hull explicitly disavows any interest in justifying the selection processes or deriving an epistemological *ought* from a descriptive *is* (ibid.). He claims that he does not mean to use a selectionist account to "justify" the conceptual process of science. Rather, he merely means to see how it works so well "given certain goals" (ibid., 13). In this disavowal of epistemological aims, Hull alludes to and tries to disarm in advance the classical objections to naturalized accounts of the sort articulated by Putnam (1983a).

Putnam argues that reducing normative considerations (such as rational explanation or justification) to naturalistic descriptions is incoherent, unless one is willing to give up the notion of knowledge, truth, and causality at the same time. This, he says, would make our statements about the world (including statements about science as a social process) mere "noisemaking" (ibid., 246). Yet, Hull clearly means his account to be both explanatory and causal. He seems to want to do a "normal" science of science, wherein epistemological questions about the character of rationality and justification are suppressed or bracketed. It becomes evident in the course of his detailed and careful account of science as selective process that these normative considerations cannot be kept external to the account of science as science. He can be taken to be doing "normal" falsificationist epistemology. The very things which he takes to be in need of "explanation" and "justification", and the reasons he gives for taking the current social arrangements of science as objective, and thus conducive to scientific progress, assume the cluster of epistemological assumptions mentioned above.

Let us look at Hull's externalized epistemological picture. His model is an evolutionary account of science based in the familiar Darwinian mechanism of variation and selective retention, with some debt to population biology and sociobiology. Hull has done an immense amount of "field work", studying in careful detail the mechanism of transmission and modification of concepts within the scientific domain of modern systematics. He gives us a wealth of information about the processes by which scientists get their work published, cited, and challenged. He traces the social arrangements and mechanisms that affect the acceptance or rejection of those ideas. He has given us a far more accurate "epistemological genetics" than was previously available.

Hull's theory of theory change is based in an ontology which he has argued for within evolutionary biology. The general idea is as follows. The results of selection processes are lineages of species in biology and of concepts or conceptual systems in science. Both Hull and M. Ghiselin (1974) have defended the view that species should be construed not as classes, but as individuals (Hull 1976, 1978, 1980). Hull extends this thinking to conceptual change in science. Scientific concepts or conceptual systems are individuals since they are historical entities rather than ideal types. For Hull, philosophers or historians of science are dealing with historical individuals when they examine a conceptual system like Newtonian mechanics or Mendelian genetics. The theoretical conceptual systems of science, like particular species (*Giraffa camelopandales, Ulmus americana*) of evolution, are temporally bonded historical individuals. Hull points out the conceptual confusion of treating either a species like *Dodo elangans* or a concept like *gene* simultaneously as an historical entity and as an immutable natural kind. Concepts and species both can function in Hull's selection theory just because they are the same kind of things, namely, spatiotemporal individuals related to one another by descent. For an ideal morphologist, one who takes species to be immutable natural kinds, a wing is a wing in the way that gold is always and everywhere gold, independently of its genealogical origin. Similarly within the idealist framework of conceptual history that Hull challenges, *gene* is the same concept wherever or whenever it arises. The conceptual ideal morphologist would say that "All those who have the same views about genes are using the gene concept the same way, accidents of terminology and history aside" (Hull 1988b, 17).

On Hull's view, these instances of the concept *gene* are merely analogous and not part of a single historical lineage. However, if we are to understand the process of science as a process of selection, related concepts must be taken as evolutionary homologies, like the forelimb of the reptile and the wing of the bird, traits that are possessed by two or more groups which derive with or without modification from their common ancestor. In such cases, it is possible to establish real connections in time for conceptual changes, a lineage. The totally independent use of the *same* concept in totally discrete historical contexts is, therefore, not possible. The Greek concept of the atom and the modern concept are not homologous, not related by clear descent. They are like the eye in the vertebrate and the eye in the octopus, similar in interesting ways, but not part of the same lineage. For Hull, conceptual history of science must be seen as an effort to trace the lineages of concepts showing actual descent.

Hull's powerful and useful point here promises to add needed historical depth to philosophy of science. He opens the possibility of an interpretation of scientific change that recognizes that the way we organize our experience conceptually is a deeply historical and social process. A concept such as *gene* as a discrete unit of inheritance (e.g., the Mendelian gene) is not there to be recovered again and again. Rather *gene* is a particular historically generated conceptual tool, a token of a type of thinking about generation which as it is refined over time and passed on from scientist to scientist may give us better and better access to a specific domain of experience. Given this analysis, if we could roll back conceptual history, it is at least imaginable that the concept of "a discrete unit of inheritance" might never have come into play, or played the central role that it has in the development and organization of our thinking in areas of inheritance, development and evolution. Our traditional way of looking at scientific progress, to which Hull still clings, coupled with convergent realist assumptions, would lead us to think that if science were to make progress in the domain of organic generation, something like the idea of a gene as a discrete unit might have been necessary. From a convergent realist perspective, this is a concern because being within a truly "progressive" lineage requires a conceptual structure that limns the eternal regularities of nature. Thus, without such an idea and its spawn, our current understanding would not be as successful as it is. Hull has built this tension between the historical nature of conceptual systems and a metaphysical realism into his account. If we do not embrace a convergent realist interpretation of scientific progress, we can allow for the possibility that we could have had significant cognitive success with a very different lineage addressed to the understanding of the phenomenon of inheritance.

Hull has given us a new way of talking about the deeply historical nature of our understanding of the natural world, one that connects in interesting ways with our current understandings of organic change. As I will argue later, however, in adhering to the falsificationist model of evolutionary epistemology with its commitments to an ahistorical final truth as a criteria of progress and a causally linear view of time, Hull overlooks much of the potential of his own analysis.

Given an ontology of concepts or conceptual systems as historical individuals, Hull needs to describe a mechanism which assures or at least makes probable that conceptual change in science is progressive, that is, leads toward a limning of the "eternal, immutable, regularities (that) exist out there in nature" (Hull 1988b, 476). He proposes a social mechanism of selection based in the replication of concepts transmitted and selected by scientists.

Hull argues that the social structure of modern science is demic, that is, it is organized into small research groups or cliques. In evolutionary biology, a deme is a small isolated interbreeding population that shares a gene pool. The scientific demes that Hull identifies tend to be relatively short-lived. While a scientific deme exists, those within it share a commitment to certain concepts and approaches, beliefs, definitions of problems, and promising directions for solutions. (The scientific deme seems to have much in common with a Kuhnian community of scientists working within a paradigm.) This overlap of conceptual commitment among those within the scientific deme is analogous, for Hull, to the way in which demic populations of organic species have many genes in common, and thus are close genetic relatives.

Demic social structure in organic populations has several features that are evolutionarily functional and are transferrable to Hull's social model of change in science. First, because the small population is isolated from others of its species, variations spread quickly and speciation or evolutionary change is relatively rapid as compared to species where populations are distributed in a more geographically continuous manner (panmictic). Hull claims that the demic social structure of modern scientific communities is a condition which contributes to rapid conceptual change. Within scientific demes, ideas are generated and exchanged rapidly, since it is relatively safe within the group to share and test one's pet hypothesis (1988b, 15, 433).

Another feature of demic social structure that is of functional interest to Hull is the close genetic relationship of those within a deme. In small interbreeding natural populations, individuals tend to be closely related to one another, that is, they have a relatively large number of genes in common. Therefore, it would favor the inclusive genetic fitness (likelihood of passing on a goodly number of one's genes) of each individual to be more cooperative and less competitive in a reproductive sense. If my sister or my cousin produces offspring, I share the genetic glory since a significant number of my genes are successfully reproduced.

Likewise, within small research groups the success of each scientist, defined now in terms of inclusive conceptual fitness (the passing on of concepts or "memes" instead of genes), is strongly connected with the success of the others within his or her group. The success of one member of the group is often shared with the others as reflected glory via joint publication or simply by association (as in, "She's a part of Jim Watson's research team").

Hull argues that just as an organism strives, albeit unconsciously, to increase its genetic fitness through certain adaptive behaviors (for example, refraining from eating its sibling's children), so too the scientist within the group strives to maximize his or her conceptual fitness by cooperating with, citing the work of, and sharing new ideas with those immediately within the group. However, a scientist striving for inclusive conceptual fitness may not be aware of what he or she is doing in those terms, "Flour beetles are unaware that such a thing as genetic inclusive fitness even exists. Scientists are not appreciably different in their quest for conceptual inclusive fitness" (Hull 1988a, 128).

These features of the scientific deme as Hull describes it allow for an arena of fertile and bold conjecture. The deme constitutes a context of discovery. As within Darwinian evolutionary biology, so also within falsificationist views of scientific change, the "mechanism" that generates the variations that will fund change is not directed toward an answer to the challenges being posed by the immediate environment. The *direction* of change is not determined from *within* the process itself. It cannot be taken as a "learning process" in that the acquired experience is not directly useful in seeing where we are, cognitively, in relationship to the actual structure of the world.

Like Popper and Campbell, Hull sets aside the problem of the source of viable hypotheses saying, "Without innovation scientific change would cease, but I have come up with little of interest to say on the subject" (Hull 1988b, 27). This dismissal of the creative process of hypotheses generation from the domain of explanation is totally consistent with the falsificationist paradigm. The puzzle around which it constructs its explanation is how science can be progressive (as Popper, Campbell and Hull take it be) and yet be "blind" in its generation of theoretical variants. The ironies are many. There is no objectivity without blindness and no valid directionality without nonintentional causality. That a particular variant also happens to be fit in relationship to a given environment or that a certain hypothesis turns out to be particularly fruitful is, from the perspective of the process, inexplicable in itself. Neither the individual organism nor the individual scientist enters directively into effecting the ultimate goal of the process. The appearance of creativity or genius, in this view, is a kind of epiphenomenon of the process as a whole, a process in which the individual is embedded. What is difficult to comprehend is what "knowledge" is supposed to amount to on this account.

The third aspect of the social organization of science that Hull finds significant in ensuring its effectiveness is the intensely competitive character of the practice. Scientific competitiveness arises because recognition of one's work is the ultimate measure of success, that is, inclusive conceptual fitness. What matters is passing on one's ideas by having them pass the tribunal of hostile testing and having them recognized as one's own. Fame and career success are the scarce commodities that fund this process. The unsung, unappreciated precursors just do not count, "Mendel's main problem was that he was too humble: a couple of papers, a couple of letters to a famous colleague and that was that" (ibid., 363). We must wonder how seriously to take Hull's remarks that if "we are genuinely interested in educating students who are most likely to contribute to the growth of science, we might well give applicants to graduate school aggressiveness tests as well as achievement tests" (ibid., 365).

Moreover, as things are arranged in contemporary science, recognition almost always goes to the first person to publish something new, no matter how much that innovation built upon the work of others. Recognition alone constitutes conceptual survival, which is measurable in terms of career success. In Hull's view, the overall success of science is built upon this tension between the greed of the individual scientist for total credit and the scientist's fear that if he or she does not connect his or her work with that of others by rapid publication and citing the work of colleagues, his or her own work will not find support, "Scientists want their work to be acknowledged as original, but just as importantly, they want it to be accepted. One cannot gain support from a particular work unless one cites it. . . .Scientists would like total credit and massive support but they cannot have it. Science is so organized that scientists are forced to trade off credit for support" (1988a, 127).

Science is cooperative in the sense that each scientist needs the work of the others to get on with his or her own. But for Hull, "Cooperation in science is behaviorally indistinguishable from mutual exploitation" (ibid., 131). One scientist dutifully cites another, does not steal his or her data, and does not publish fraudulent or sloppy results because such behavior tends to lead to a decline in inclusive conceptual fitness. Since the work of one scientist builds on that of others, especially those sharing conceptual lineages, the publication of fraudulent data or sloppy results is likely to be discovered and lead to a decline in inclusive conceptual fitness, or even to scientific extinction for the individual in question. Thus, cooperation is enforced by mutual dependence and a clear definition of evidence.

All of the interdemic competition and cooperation leads to a kind of derivative objectivity for science as a whole. For the individual scientist, says Hull, "a little objectivity goes a long way" (1988b, 27). The location of the objectivity that counts is at the level of the process of science itself. He says, "In addition, the sort of objectivity that gives science its peculiar character is a property of social groups, not isolated investigators" (1988a, 127).

The competitive-cooperative features of science give us another important functional aspect of a falsificationist picture: Rigorous testing eliminates error. Individual scientists are rarely inclined to test their pet theories rigorously, but the need for support and recognition forces rapid and careful publication. Public exposure will result in rigorous testing especially by those with rival theories (ibid., 131). The conceptual fitness of rival groups will be best served by their doing their best to prove their opponents wrong. Thus, for Hull, the objectivity of the scientific process, its self-corrective capacity, depends not upon unbiased scientists, but on scientists' having different biases and rival career interests (ibid.).

The clarity of the definition of evidence is unique to science, according to Hull. He does not go into detail about how this clarity is achieved or maintained. Apparently, what counts as evidence in certain fundamentalist sects is also very clearly defined. Equally, among warring scientific demes, the nature and worth of evidence may be just what is disputed. A case in point is the interpretation of gene frequency data by neutralists, who attribute much of the variation within species to the stochastic process of genetic drift, and by selectionists, who claim that gene substitutions occur as a consequence of selection for advantageous mutation (Li and Graur 1991). The current data as defined by either group does not promise to resolve this dispute. In fact, the question of what counts as evidence is a particularly difficult point for the reformed falsificationist because any easy appeal to a direct theory-independent empirical test has already been discounted.

Hull claims that fierce competition accounts for the factionalism, elitism and sometimes nasty behavior of rival scientific groups. He argues that this "bad behavior" is actually functional in ensuring scientific progress:

> [S]ome behavior that appears to be the most improper actually facilitates the manifest goals of science. . . . Although objective

> knowledge through bias and commitment sounds as paradoxical as bombs for peace, I agree that the existence and ultimate rationality of science can be explained in terms of bias, jealousy, and irrationality. As it turns out, the least productive scientists tend to behave the most admirably, while those who make the greatest contribution just as frequently behave the most deplorably, You pays your money; you gets your choice. (1988b, 32)

More ironies. Evidently the highest form of rationality is based in a rather nasty form of irrationality. Hull's favorite example of this "survival of the meanest" behavior is the notorious and sometimes heated controversy between the cladists and the pheneticists in current taxonomy. On his view, the dedication of the enthusiast that is required for scientific progress is often, and perhaps best, fueled by personal animosity, the desire to "get the son of a bitch" (1988b, 160). Campbell (1990) objects to Hull's failure to distinguish social conditions that increase validity of scientific beliefs from those that are simply effective in selecting a certain subset of beliefs for other reasons. However, Hull is in line with the falsificationist program in attempting to explain the surprising strength of the selection process in fixing "valid" belief, despite the biases and the less-than-admirable motives of its individual practitioners.

Given this system of replication and interaction and its demic competitive-cooperative structure, science progresses toward its goal, an increase in knowledge about the natural world. Hull claims that the "greatest strength of science as it is now organized is that it harnesses our baser motivations for more lofty goals" (Hull 1988a, 133).

5. Hull's Evolutionary Falsificationism as Epistemology: Internal Tensions. Hull's analysis of science as a process tied to the falsificationist model discussed above poses an irresolvable set of internal tensions.

First, it maintains and does not resolve the tension between the descriptive accounts and normative epistemology endemic to "naturalized" analyses of science. Hull's selectionist account of scientific progress works from within a clearly epistemological agenda. Yet, he says that if he is making normative claims, they constitute only an anemic form of epistemology that "merely urges the use of successful methods" given certain goals (1988b, 13).

To the contrary, he cannot have this hedge, for, clearly, if the goal he is talking about is an approximation of the fixed laws of nature (ibid., 476), then he has built normativity into his account from the outset. His is not a simple instrumental account defined in relation to goals that we could choose or forgo and still be doing science. Doing science is not like giving advice about the best or most efficient way of getting from Pittsburgh to Cairo, or the most efficient way to build a mousetrap. The objective pursuit of truth, however we might construe that term, is a radically epistemic process. Therefore, Hull's definition of science as a selection process is not an innocent sociological description. It stands clearly within an epistemological lineage of fallibilism which includes Hume, Peirce and Popper.

It is difficult to see how one could read his view as anything but an attempt to answer epistemological questions about the possibility of objectivity and rationality and how knowledge of the world is possible at all. It cannot be supposed that he believes that other and different accounts of these particular things are really epistemology whereas his account is innocent empirical description. His account, if adequate, is so in comparison to other accounts of what counts as knowledge, rationality and truth.

Unless Hull is willing to give up his external teleology for the scientific process, he cannot avoid a full-blooded normativity. Given his account, it is obvious that if we want to achieve this particular end, we ought to use this means or its functional equivalent. No anemic epistemology will pass. He is not merely describing how contemporary science operates (and I am not disputing his descriptive accuracy in that area). Rather, he is also saying that this is at least one way that science ought to go on if objective knowledge of the world is to be attained.

Second, a serious tension internal to Hull's model arises from his adopting an account of objectivity that relies on putting intentions of individual scientists off line in determining the global direction of the process, while, at the same time, maintaining that the process is cognitively progressive. He is left with the explanatory problem of giving a nonintentional account, namely, a causal account, of how it is that our cognitive structures can succeed in limning the laws of nature.

An individual limited to subjective experience has no criteria by which to judge, at any particular historical moment, how theory stands in relationship to the noumena. For the empiricist, only a direct intellectual intuition of the real could give validation of our current knowledge, or indication that we are headed in the *right* direction. However, the evolutionary epistemologist is confident that the process of inquiry itself is so structured that "the object of perception participates substantially in generating the momentary belief" (Campbell and Paller 1989, 236).

Hull argues further that, although scientists sometimes consciously seek truth or are motivated to help humanity in their work, the presence of good intentions does not make a discernible difference to the process of science. He suggests that such sideline intentions might even be discouraged since things are so arranged that the motor of science is run by the striving of the scientist for his or her own career goals, "Scientists adhere to the norms of science so well because more often than not, it is of their own best self interest to do so. The best thing that a scientist can do for science as a whole is to strive for his or her own conceptual inclusive fitness" (Hull 1988a, 129).

The structure of Hull's argument is reminiscent of that forerunner of naturalized views of reason, T. Hobbes ([1651] 1961). How, Hobbes asked, are we to get rational political behavior out of basically self-interested and not very rational beasts? His answer was to arrange things socially so that the behavior resulting from self-interested behavior would be, in its effect, more genuinely rational than that for which the individual's intentions could provide. One must devise a mechanism whereby the behavior required to attain the social good and the behavior necessary for the survival of the incurably self-interested individual coincide. So, says Hull, science has allowed us to harness our baser motives for a higher good.

Despite his fallibilist position, Hull contends that we can see a trend toward better and better theories or more and more competent reference that allows for the faith that the present method for error elimination will, in the long run, lead to comprehensive knowledge. For Hull, science appears to be more progressive globally than biological evolution, "Although scientists strive to solve unsolved problems, all that this striving influences is the relative speed of change, not its global direction" (Hull 1988b, 474). Thus the puzzle of scientific success, defined in this way, calls for a physical causal account of scientific progress. Such an external directive is provided by assuming a convergent realism. Hull claims that the possibility of global directionality depends on the goals staying put, "Conceptual evolution, especially in science, is both locally and globally progressive, not because scientists are conscious agents, not because they are striving to reach both local and global goals, but because these goals exist. Eternal immutable regularities exist out there in nature" (ibid., 476).

Hull and other falsificationists ignore the major disanalogy with organic evolution that this teleological structure presents for their model. In their analysis, only in the evolution of scientific theories do we see an external goal-directed mechanism as a central explanatory feature. Biological evolution is not going anywhere in particular. It has no predefined end, no external telos. Its successes are locally defined adaptations, not movement toward some fixed end. The same can be said of immune system responses (Hull's other example of a selective process). It seems gratuitous to allow an addition to the mechanism of change in conceptual evolution that would essentially negate the power of the other selectionist accounts by readmitting just what they were meant to explain away. (See Ruse 1989 for a similar criticism.) Moreover, as Shimony has pointed out, presuming the truth of realism to explain the success of science seriously begs traditional epistemological questions (1981, 117).

Ruse agrees with Hull that science does indeed progress in the global sense, but he points out that biological evolution has no such direction, and thus has no need for explanatory recourse to some external guiding directive. Ruse contends that this clearly shows the failure of the analogy of scientific theory change to organic evolution (1989, 210). Others have similarly argued against the analogy (Thagard 1980). Toulmin has simply denied that science is globally progressive in the way Hull takes it to be, allowing the analogy with organic change (Toulmin 1972). I tend to agree with Bradie that "judgements of progress in science are always local judgements" (1986, 427).

The claim that science is not globally progressive does not require denying that science does succeed. Science as it exists in twentieth-century Western cultures undeniably "progresses" in some sense. We do know more about the structure of cells, the mechanism of inheritance, and the dynamics of evolutionary change than we did fifty years ago. Less clear is that progress must have some linear directionality of the sort expected by Hull, that is, that understanding of some of the general and relatively stable features of the natural world is achieved by reference to "the fixed and eternal laws of nature".

Bradie has pointed out that most evolutionary epistemologists, including all of those discussed here, are unwilling to accept "the radical implication that the direction of conceptual evolution and change need not be leading anywhere in particular. They are unwilling to abandon the eschatological vision of a terminal-consensus in the long run" (ibid., 451). The radical implications of such an acceptance are almost impossible to deal with if one wants simply to "revise" positivist empiricism along the lines of Popper or Hull.

6. Conclusion. Three major kinds of objection have been raised to Hull's selectionist model of the process of science and to others of similar form.

First, his protests to the contrary, Hull is doing epistemology. Against the background of radical empiricism's need for a new model to explain the success of science, Hull responds with several normative moves. He attempts to give an account of the objectivity of science by putting the intentions and goals of the individual scientists off line and vesting objectivity in the social structure itself. He maintains a commitment to a correspondence theory of truth by giving a causal account of scientific progress that allows for the world to fix the direction of scientific change. If Hull had limited himself to explaining how the current social structure of science (in the United States, in late twentieth-century capitalism) results in certain kinds of pressures to rapid publication, and how competition among small groups leads to the survival of the most arrogant in the ranks of science, his empirical study would have remained empirical. However, slipping from the merely descriptive to the epistemological in this area seems inevitable if one is to say anything interesting about science as science. Hull claims that he is an internalist on matters scientific. Thus if he means to say something interesting about science as a distinct social institution, the epistemological must take a central part. He makes the same promise that Putnam claims modern materialists make but cannot keep, to lead us to the noumena and not be unscientific about it (1983a).

Second, in putting the intentions of individuals off line in determining the direction of the process, Hull and other evolutionary epistemologists have a self-referential problem of a peculiar sort. Their accounts are meant to be explanatory, to tell us about the real causes of scientific progress, to be a "science of science". They intend to pick out the "cause" of scientific objectivity, that which keeps science moving toward its real end, that which makes this process science and not something else. But as Putnam has pointed out, explanations and causes are inherently intentional. They do not exist in the world independent of our interests. Explanations are context-sensitive. What Hull takes to be the explanatory power of some fixed, but as yet unknown, laws of nature is the result of a negotiation the evolutionary epistemologist makes between the local epistemological situation in which he or she finds certain things urgently puzzling, and clear commitments to what an acceptable explanation must look like. In this case the puzzle is scientific progress. Hull chooses to negotiate his understanding of science as a teleological process in terms of a "causal" mechanism involving the fixed laws of nature. These laws seem to be involved in a strange double causation. They are both the explanation of the phenomena the scientist is investigating *and* the explanation of the course of the scientific process itself.

Furthermore, I am pragmatically enough inclined to ask what would we do with the fixed laws once we had approximated them? Even the idea of "approximation" here has ambiguities which need examination. Since these laws are independent of any of my interactions with the world, it is unclear what they would mean to me or anyone else except as exceptionally brutish facts. If the current relationship between the real and a subset of my beliefs (the scientifically successful ones) is *purely* causal (in some mysterious nonintentional construal of "causal"), and since I have no intentional access to this relationship, it is difficult to see how that would change as science gets closer and closer to a final account of the noumena. The idea of one fixed eternal set of laws of nature existing independently of our needs to explain certain things from within a context of expectation and action seem epistemologically irrelevant.

Evolutionary epistemology would do well to reach for a more pragmatic realism wherein what counts as explanatory is clearly connected with peculiarly human and historically mediated epistemic needs. Such a view could be evolutionary and realist as well, but it would be less weighted down with the internal problems of a faltering empiricist paradigm.

Although Hull's analysis of concepts as historical individuals allows him to take our present mode of inquiry as itself an inherently historical phenomenon, Hull opts to take the science we have as displaying the essence of science itself. For him, the science we have is a token of a type of social practice that displays a functional mechanism of a definable sort. As I have argued above, the functional form which science qua science takes for Hull has a lot in common with the falsificationist views of Popper and Campbell.

Hull says that if this type of social organization becomes extinct and then arises again in some other historical era, it would still be science. Thus science for him, like the eternal laws of nature, is ahistorical. Its essential character is to instantiate a mode of social interaction that results in the generation and selection of theories in such a manner that a closer and closer approximation of the fixed, eternal, immutable laws of nature is achieved. Science is essentially a homing device for these eternal regularities. A social organization that does this is real science, whenever or wherever it occurs. It is because of this "essentialism" about science that Hull can have a science of science. A science deals in laws, and laws, Hull tells us, do not deal in particulars or historically defined individuals (1984, 17).

As Dupré (1990) has pointed out, Hull stalks the "big game" of explanatory laws within which the elements of selection theory (interactors and replicators) stand as natural kinds. If social aspects of contemporary science are contingent historical features that arise from a particular historical lineage, then science is not the kind of thing over which genuine laws could range. If science qua science is more like the Baroque Period or *Dodo elangans* (an historical individual) than it is like "gold" or "species", then Hull can do historical epistemology, but not scientific epistemology. Very difficult and interesting issues are at question here. Is science an individual or a natural kind? It should be recalled that what most modern epistemologists are willing to classify as "real science" has existed for a very short period even relative to historical time. Is the whole period of mechanistic science from the seventeenth century to the present an historical individual or a token of a universal type? To make matters even more ambiguous, modern science is motley. Even at the end of the most scientifically *progressive* of centuries, there is no obvious methodological or explanatory unity in those social practices that call themselves science. Science hardly seems to be one thing, but "a loose and heterogeneous collection of more or less successful investigative practices" (Dupré 1990, 69).

Hull should look again at the possibility of an historical rather than a scientific epistemology. He himself argues for the validity of narrative explanation when dealing with historical entities. He says "there is more to nature than the sorts of regularities enshrined in laws of nature" (1984, 31). He might acknowledge, following Dewey, that since all natural existences are histories, "all knowledge is historic" ([1925] 1988, 130). Only a decontextualized empiricism which sees its validity determined by a set of eternal laws continues to seek a radically dehistorized image of science. Hull's important contribution in showing us the possibility of taking conceptual systems as individuals related to one another through descent might be broadened in exciting directions, though none, I suspect, would appeal to Hull.

For instance, the idea of conceptual lineages differs in interesting ways from organic lineages simply because their expression is often extrasomatic. They exist for indefinite lifetimes in texts and on computer disks. This allows for interesting backward loops in historical time that are not possible in evolutionary time. Clearly the octopus eye and the vertebrate eye are not and will never be related by descent. It is less clear that some contemporary evolutionary thinker is totally immune to influence by the thinking of Lamarck re-read, or by someone even further back in the "tangle bank" of historical time. Hull's analogy with a strict neo-Darwinian view of organic lineages requires that he deny this possibility. And so he claims that precursors, unrecognized and thus without influence on their contemporaries, are irrelevant. Precursors are analogous to the member of a species, however elegantly endowed, whose genes are not replicated in the next generation.

Replication and modification mechanisms in conceptual and biological evolution are significantly different. Conceptual *speciation* by hybridization and "back crosses" seem more explanatory of the direction of scientific theorizing than do the fixed laws of nature. In fact just about the time a field of inquiry begins to have a theoretical crisis, its practitioners start looking carefully at precursors, even the previously unrecognized ones, for possible clues, fruitful metaphoric shifts or data.

Finally, I contend that Hull's views on the split between scientific excellence of character versus lay judgements in that regard results from his commitment to what he calls an internalist view of science. The internalist regards science as a unique process that is not determined qua science by external social or historical influences, or by external criteria of moral relationships. Hull asks us to consider the possibility that if scientists behaved like gentlemen (or women) that science as we know it would grind to a halt. If we knew that this were a real possibility, would we not swallow our moral repugnance and accept the aggressive competition as a necessary evil? Within his analysis, science belongs in a different moral universe than the rest of society. This idea is at least as old as the discussion surrounding the founding of the Royal Society in 1662. On this view, social goods and scientific goods are different and attained by distinct skills and virtues. In fact, what is good and virtuous outside, Hull tells us, is often dysfunctional inside.

This view, however, depends for its justification upon the truth of convergent realism. Positing a fixed and transcendent goal toward which science qua science moves, in relative independence from the intentions of the scientists, supports this picture. Since the goal is fixed, any path to it is as good as any other. Comparison between means is only a matter of judging relative efficiency and speed. The contingent twists and turns in the actual historical path are in the end without consequence.

This aspect of Hull's analysis reflects his objectivist position that what we come to know exists independently of how we come to know it. As philosophers, we have all been raised to fear being accused of committing the genetic fallacy. How or even why we acquire an item of knowledge is supposed to have nothing to do with the justification of that knowledge. The intentions of the inquirer do not materially affect the end result. Worries over the genetic fallacy are a frequently successful defense against any challenge to the validity of the distinction between the context of discovery and the context of justification. Nonetheless, I would argue, a science red in tooth and claw will give only a certain kind of knowledge, namely, a science useful only for certain kinds of ends, including cognitive ends of understanding, and not for others.

Without convergent realism as the guarantee of scientific objectivity, and we have good reasons to reject it, then these rationalization of an aggressive and mean science rings hollow. If it is possible that the natural world is open to multiple forms of address leading to various kinds of genuine understanding, then it is possible for "us to become answerable for what we learn how to see" (Haraway 1991, 190) and with whom we choose to see it. Hull's account blocks that possibility and even makes it look foolish. In that sense, Hull's model is not merely epistemically normative, it is straightforwardly morally normative. He tells us that scientists qua scientists ought to be aggressively focused and self-interested people in order to serve the truth, to harness our basest motives for good ends.

I am not suggesting that the project of giving a naturalized account of our knowledge acquiring practices is totally misguided. In fact I see this as the most appropriate form of modern epistemological inquiry. This is where my criticism of Hull's analysis differs from others like Thagard. Unlike Ruse, I am not claiming that the analogy between conceptual evolution and organic evolution is false or even shallow. Rather I am suggesting that it is carried out too strictly both in terms of debatable views of scientific knowledge and evolutionary processes. The disanalogies may well serve as active points from which to develop an understanding in both directions.

However, if evolutionary epistemology is to give us a useful account of human understanding of this world, it must give up the need to support the "unnaturalizable" aspects of the old epistemological paradigms, for example, the dichotomies of object/subject, observation/theory, mind/world, fact/value, and the externalist/internalist versions of epistemology. It is upon these paired tines that the old empiricism keeps impaling itself, no matter how "descriptive" and "externalized" it becomes. The form of naturalism that epistemology might make better use of is more closely aligned with a pragmatic naturalism like that of Dewey (Gatens-Robinson 1991). Within such views knowledge is not a copying of reality that exists independent of us, but a form of transaction within the world itself. Progress in science is not a blind groping forward in the dark, but an emancipating and illuminating process through which we might come not only to know the natural world of which our inquiry forms a part, but also the real character of our own knowledge.

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