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From Evolutionary Epistemology Via Selection Theory to a Sociology of Scientific Validity

Edited by Cecilia Heyes and Barbara Frankel

Editors' introduction

This paper originated in a set of background readings assembled by Donald T. CAMPBELL (DTC) for a talk at a meeting entitled "The Epistemology Group: The Evolution of Knowledge and Invention". The meeting was organized by John ZIMAN, and took place on 24 May 1995, at The Royal Society of Arts, London. The editors of Evolution and Cognition encouraged DTC to develop the manuscript for publication, and in his notes on the version completed before his death, DTC described it as "in a halfway state of editing", and as "a personalized retrospective history of ideas". He also wrote that "The main goal of the paper is to provide a naturalistic epistemology for science. It turns out that this will have to also be a sociology of scientific validity. Fragmentary suggestions along this line take up much of the space." DTC pursued this goal first by identifying the type of evolutionary epistemology in which he was engaged as 'general selection theory' (section 1). Rather than focussing on the biological evolutionary origins of categories of perception and thought, exploring detailed analogies between biological evolution and scientific change, or examining the role of innate perceptual mechanisms in language acquisition, general selection theory asserts that variation–and–selective–retention processes operate at a number of hierarchically organized loci or domains in nature, and that all improvements in 'fit' between systems and their environments are attributable to the operation of these selection processes.

Section 2 provides further preliminary clarification. Therein DTC announces that he now believes it counter-productive to describe adaptive organic form, resulting from genetic selection, as 'knowledge'. Instead, the term 'knowledge' should be reserved for the products of vicarious selection processes, such as perception, trial-and-error learning, and scientific enquiry, that "short-cut selection by the life and death of genetic variants".

Sections 3–6 examine the implications of general selection theory for the status of scientific knowledge and justification. Throughout, DTC emphasizes that the realization that science proceeds through selection processes does not provide justification for scientific beliefs or theories, and, in this sense, general selection theory is "epistemologically irrelevant". He underlines this sceptical point in section 3 by noting that religious, as well as scientific, beliefs can be selection-based, and in section 4 by drawing attention to the many selection processes in science that are antagonistic to "competence of reference", i.e. correspondence truth of theories. Primary among these are the processes required to sustain science as a social system, as a "vehicle" or embodiment of knowledge. In sections 5 and 6, DTC continues to "side with the sceptics" by stressing the ubiquity and ineliminability of the problem of induction, and the oversimplification entailed by the use of language.

In the spirit of DTC's hypothetical realism, there is a switch of perspectives between sections 6 and 7. Having insisted in the first half of the text that general selection theory cannot justify scientific beliefs, and therefore that realism about those beliefs is necessarily hypothetical, or a matter or faith, DTC offers in the latter part of the text a selectionist rationale for greater trust in scientific than in religious beliefs (section 7), and argues that public commitment to the norms of science, even if it is hypocritical, functions to increase competence of reference (section 8). Thus, he shows that, if one assumes what cannot be demonstrated (using selection theory or by any *other means), that science is (sometimes) successful in describing nature, selection theory can explain how this success could be achieved, and provide the basis for recommendations about how science should be organized to maintain or enhance the (presumed) validity of the beliefs it generates. In other words, those used in the title of the paper: one type of evolutionary epistemology, selection theory, yields a descriptive epistemology for science, and, more specifically, a sociology of scientific validity.*

In editing this manuscript, we have sought merely to clarify the import of the original. To maintain the momentum of the text, we have inserted passages which try to impersonate DTC's style, rather than interrupting in our own editorial voices. However, all modifications and additions are printed in italics (as used here), and are therefore clearly identifiable.

The first editor (HEYES) performed the exacting task of cleaning up a scanner-generated computer disk, since the original disk could not be found after DTC's death. She then made a very able first pass at smoothing out rough transitions, clarifying sections that presumed too much familiarity with DTC's ideas on the part of the reader, and generally improving without abbreviating a very long manuscript. It was in that form that the paper was sent to the other authors whose papers are included in this special issue of Evolution and Cognition.

The second editor (FRANKEL) took a rather less reverent approach, and thus allowed herself the prerogative of overriding, to a degree, DTC's long-standing habit of cannibalizing his own earlier work freely, thereby saving himself the labor of rephrasing things he had said to his own satisfaction before. In his lifetime, Don CAMPBELL's lengthy self-quotes were a standing joke between husband and wife, and now that he can no longer defend himself she has taken shameless editorial advantage. Some very long quotes have been therefore abbreviated (using ellipses in the body of the text) and/or partially reduced to briefer summaries. The other authors in this volume will recognize that the paper has been shortened since they read it, however, every effort has been made to preserve all parts of the argument in the original.

Equally important, the changes are not so extensive as to make Don CAMPBELL's final publication anything other than his own work, in his own inimitable voice—or anything less than the persuasive document, the sermon to the unconverted that he intended it to be. DTC's lifelong dedication to science and his faith in the good that it ultimately produces were central themes of his career. Although he realized that all human beings and all human enterprises are imperfect, his own achievements stand as testimony to the intellectual and moral integrity that can be aspired to by members of the scientific community in which he so delighted.

1. Four types of 'Evolutionary Epistemology'

Its length notwithstanding, the present essay covers but a small portion of all that goes on under the 'evolutionary epistemology' rubric. (For bibliographies and surveys, see CALLEBAUT 1993, CAMPBELL & CZIKO 1990, CAMPBELL, HEYES/CALLEBAUT 1987.)

1.1 Evolutionary origins of Kant's a priori categories

Most completely in CAMPBELL (1974b), I offer documentation on the many independent discoveries of the notion that the a priori categories of perception and intuition are the products of biological evolution. The insight goes back to DARWIN's notebooks, Herbert SPENCER, William JAMES, and a hundred others. I am continually finding new predecessors previously missed, such as Hans VAIHINGER (1911), but modern attention to it has been primarily stimulated by the writings of Konrad LORENZ (1941, 1951, 1973). I first became aware of LORENZ's essays through WHYTE (1951) and BERTALANFFY (1955). I initiated and edited the widely reprinted English translation of LORENZ's 1951 essay on KANT's categories, first published in 1962. The recent vigorous developments in Austria and Germany are primarily of this type (e.g., RIEDL 1982, 1984, VOLLMER 1975, 1985, 1986, ENGELS 1989) and this is the only type of evolutionary epistemology endorsed by such naturalistic epistemologists as QUINE and SHIMONY. In a widely used typology, BRADIE (1986) designates this as EEM (Evolutionary Epistemology of Mechanisms). It is neglected in this essay.

1.2 Analogies between biological evolution and the 'evolution' of scientific theories

This type is central to the present essay. POPPER devotes a paragraph or so to it (1959 and presumably 1935) and it is central to the more recent works of TOULMIN (1967, 1972, 1981), HULL (1988a,b) and RICHARDS (1987). BRADIE (1986) called it EET (Evolutionary Epistemology of Theories).

1.3 Shared innate perceptual reification of middle-sized objects

This type accents a link between 1.1 and 1.2. It focuses on the phenomenon that makes language and the bulk of culture possible. It is because infants and their parents share unjustified but useful perceptual reification of external objects that a shared language can be transmitted through ostensions that are always to some degree equivocal (see 6 below, CAMPBELL/PALLER 1989, and CAMPBELL 1973). Also, in the ideology of the scientific revolution, 'proof' calls for 'demonstration' using the same level of ostensions usable in language learning.

1.4 General selection theory

Most of those who have elaborated on 1.2 above have employed too close an analogy between science and biological evolution, carrying over many details from the latter that are inappropriate (see section 3 below). This essay approaches an epistemology for science from the perspective of a much more general 'selection theory' in which biological evolution is just one nested cluster of exemplars. This biological cluster is, of course, to be mined for useful insights and analogies, but is not to be taken as a compulsory model to be followed in every detail. Among the exemplars of general selection theory is the JERNE–BURNET theory of acquired immunity.

According to this theory, the presence of a toxin stimulates a proliferation of potential antigens, some of which by chance immobilize the toxin molecules and are triggered into mass production. This metaphorical base has stimulated GAZZANIGA's (1992) belated independent invention of evolutionary epistemology, among others. Trial-and-error learning (of a blind animal for purists like me – see CAMPBELL 1956) was Karl POPPER's first metaphorical base. THORNDIKE (1898), ASHBY (1952), and PRINGLE (1951) were among those who have called attention to the common selection theory model shared by trial-and-error learning and natural selection. Early evolutionary epistemologists such as SIMMEL (1895) and BALDWIN (1909) have called this 'selection theory', and I should have done so too, instead of using 'evolutionary epistemology'. That 'selection theory' would have been a better term is shown by the titles of several of my historical bibliographic lists in the appendices to CAMPBELL (1974b): "Appendix I: Trial–Error and Natural–Selection Models for Creative Thought", "Appendix II: Natural Selection as a Model for the Evolution of Science", and "Appendix III: On the Ubiquity of Multiple Independent Invention." CZIKO (1995) provides an extensive survey of the many exemplars of a general selection theory.

2. My 1960 model and a proposed revision

2.1 The 1960 dogma

The following is rearranged from an early essay on what has come to be called "evolutionary epistemology"—or, more aptly, "naturalistic epistemology"—based upon a selection-and-retention model of knowledge processes.

Between a modern experimental physicist and some virus-type ancestor there has been a tremendous gain of knowledge about the environment ... This extended usage of 'knowledge' is a part of an effort to put 'the problem of knowledge' into a behavioristic framework which takes full cognizance of man's status as a biological product of an evolutionary development from a highly limited background, with no "direct" dispensations of knowledge being added at any point in the family tree. The bibliographical citation of the several sources converging on this approach to the problem of knowledge, and the discussion of its relation to traditional philosophical issues and to the strategy of science are presented elsewhere (CAMPBELL, 1959). [See also CAMPBELL 1974b.] Suffice it to say here that the position limits one to 'an epistemology of the other one.' The 'primitives' of knowledge can not be sought in 'raw feels' or in 'phenomenal givens', or in any 'incorrigible' elements. While man's conscious knowledge processes are recognized as more complex and subtle than those of lower organisms, they are not taken as more fundamental or primitive. In this perspective, any process providing a stored program for organismic adaptation in external environments is included as a knowledge process, and any gain in the adequacy of such a program is regarded as a gain in knowledge. If the reader prefers, he can understand the paper adequately by regarding the term 'knowledge' as metaphorical when applied to the lower levels in the developmental hierarchy. But since the problem of knowledge has resisted any generally accepted solution when defined in terms of the conscious contents of the philosopher himself, little seems lost and possibly something gained by thus extending the range of processes considered.

In bulk, [the knowledge gained between the virus-type ancestor and the physicist] has represented cumulated inductive achievements, stage by stage expansions of knowledge beyond what could have been deductively derived from what had been previously known. It has represented repeated 'breakouts' from the limits of available wisdom, for if such expansions had represented only wise anticipations, they would have been exploiting full or partial knowledge already achieved. Instead, real gains must have been the products of explorations going beyond the limits of foresight or prescience, and in this sense blind. In the instances of such real gains, the successful explorations were in origin as blind as those which failed. The difference between the successful and unsuccessful was due to the nature of the environment encountered, representing discovered wisdom about that environment.

The general model for such inductive gains is that underlying both trial-and-error problem solving and natural selection in evolution, the analogy between which has been noted by several persons (e.g., ASHBY 1952, BALDWIN 1900, 1909, PRINGLE 1951). Three conditions are necessary: a mechanism for introducing variation, a consistent selection process, and a mechanism for preserving and reproducing the selected variations.

The Basic Selectionist Dogma

2.1.1. A blind-variation-and-selective-retention process is fundamental to all inductive achievements, to all genuine increases in knowledge, to all increases in fit of system to environment.

2.1.2. The many processes which shortcut a more full blind-variation– and-selective-retention process are in themselves inductive achievements, containing wisdom about the environment achieved originally by blind variation and selective retention.

2.1.3. In addition, such shortcut processes contain in their own operation a blind-variation-and-selective-retention process at some level, substituting for overt locomotor exploration or the life-and-death winnowing of organic evolution. [Rearranged from CAMPBELL 1960, pp380–381]

2.2 A 1995 modification

In the above (and in CAMPBELL 1959 and 1974b), adaptive organic form is treated as 'knowledge' (e.g., above, "Any process providing a stored program for organismic adaptation in external environments is included as a knowledge process.") This broad inclusion I now reject. It is a needless obstacle in making contact with the traditions of philosophical epistemology.

Instead, I now wish to identify 'knowledge' with point 2.1.2 of The Basic Selectionist Dogma, i.e., with those 'vicarious' processes which short-cut selection by the life and death of genetic variants. Visual perception is the most important of these. Two levels of creative thought are others, as are linguistic transfer of belief, and the improvement of belief in dialogue.

This revision still includes as knowledge processes some strange items from a philosopher's point of view: Blind trial-and-error exploration (discovery and/or learning on the part of a blind person) is included. The resulting 'knowledge' is so similar to that provided by vision that it would be remarkable if no philosopher had attended to it. (I await help from historians of epistemology.) Echolocation by blind humans—dull sense that it is—is included, as well as exploration of local space with a cane. Echolocation by radar and sonar are also included, seen as mechanical protheses for humans. Visual perception by non-humans, and echolocation by bats and cave birds (NAGEL 1974) also count as knowledge processes under my revised view. (Undoubtedly, the reflections of supersonic squeaks are displayed on a brain map previously evolved for vision in protobat ancestors.)

3. The focal contrast: Analogies between biological evolution and the evolution of scientific belief do not help 'justify' scientific 'knowledge'

The many efforts to model the 'evolution' of scientific knowledge on the principles of biological evolution (e.g., TOULMIN 1967, 1972, RICHARDS 1981, 1987, HULL 1982, 1983, 1988a, 1988b) I now judge to be epistemologically irrelevant. MAYNARD-SMITH (1988) has made this point in his conspicuous review of HULL's (1988b) famous book. Referring to cooperation among scientists, he says

> [HULL's] explanation for such cooperation is that the replicators (genes) in the cells are identical... in different members of the group and will be transmitted to future generations only insofar as the group as a whole... is successful. Now an analogous argument might explain the loyal cooperation of members of a tightly knit research group, but would equally well explain the cooperation of the members of a religious sect or of a group bound together by a common political or artistic program. [MAYNARD-SMITH 1988, p1182]

HULL (1988b) does provide many important insights as to how the social system of science leads opportunistic and egotistical scientists to report their findings honestly (and why falsifying data is a much more major sin than plagiarism). But these contributions to a future 'Sociology of Scientific Validity' do "not" come from his analogies to biological evolution. Independently, and with more privacy (CAMPBELL 1988b), I made the same point as did MAYNARD-SMITH. My own insight came from the fact that the variation-selection-reproduction-speciation-genealogy model applied equally well to the Appalachian Bible-belt free churches in which my grandfather, uncle, and cousins participated. TOULMIN (1981) inadvertently made the same mistake in an evolutionary epistemology advocacy which used the speciation of the Romance languages as an exemplar. In no way is Portuguese adapted to Portugal. Romanian would serve just as well (if expanded with a dozen or so loan-words for use in cork gathering).

Here are more limited ways of making the same point, i.e., that evolutionary models of science are epistemologically irrelevant. ... the stance of the modern biological evolutionary epistemologist can be epitomized: "Natural Selection would not have left us with eyes that regularly mislead us." Thus, reference to natural selection can be used to 'justify' visually supported beliefs in the formula 'Knowledge is justified true belief,' in the weakened interpretation of 'justification' used by all modern epistemologists except skeptics.

This general program of evolutionary epistemology (or of providentialism) can only with great difficulty, if at all, be extended to the social processes producing scientific belief. I will give two brief epitomes of the problem. In the evolutionary epistemology program, any 'validity;' or usefully competent reference, is attributed to the selection processes which weed out, rather than to the competence of the generation processes producing the variations. We know of so many selection processes in the generation, publication, teaching, and believing of scientific truth claims that are irrelevant or inimical to improving the competent reference of beliefs that it becomes hard to argue for a dominant role for 'Nature Herself' in the selecting. This is in contrast to the case we can make for Her role in the biological evolution of the eye and brain.

A reflexive use of biological evolutionary theory provides a complementary perspective. Both CARTESIAN and evolutionary providentialists could plausibly say "(God) (Natural Selection) would not have given us untrustworthy eyes." But even if they noted that the social system of science requires great (albeit selective) trust of fellow scientists, neither the old providentialist nor the evolutionary epistemologist would find it plausible to argue that "(God) (biological Natural Selection) would not have given us untrustworthy fellow scientists." If we can in fact often validly trust fellow scientists, this is because of culturally evolved and fragile social systems, not because of innate honesty and objectivity. [CAMPBELL 1987b, 151–152]

4. Plausible co-selection of belief by referent

4.1 Justification of visual percepts

As I now see it, the beliefs about normal middle-sized objects and events which vision produces are 'justified' by two separate plausibilities.

4.1.1 Eyes are adaptive. If the theory of biological evolution is approximately correct, then eyes are adaptive in the normal ecology within which they evolved. Eyes that produced dangerously misleading beliefs would have been weeded out. (After all, as SIMMEL 1895, pointed out long ago, in evolutionary epistemology, 'true' and 'useful' become confounded.) This makes contact with Alvin GOLDMAN's, e.g., 1986, "reliability" theory of justification.

4.1.2 Co-selection by the objects of belief. It must also be plausible that the objects and events of the current belief-independent environment have been co-selectors. (This is a version of Alvin GOLDMAN's 1967, "causal theory of belief". GOLDMAN 1986, provides an integration of reliability and causal theory.) The belief in the selective reflection of the emitted radar beam makes it plausible that an airplane is over there (although a compact flock of birds might generate a similar reflection). Detailed pattern in the reflection increases the plausibility (CAMPBELL 1966), but pattern that can be plausibly explained by the characteristics of the radar machinery (rather than the presumed referent) reduces the experienced validity (CAMPBELL 1992 ms).

4.2 Only co-selected

Here I wish to make the point that, even when the referents of beliefs play a part in their selection, beliefs are also selected by processes that could be expected to detract from *their validity.* This is a point I wish to belabor from several perspectives. *In 4.2.1 I use a non-epistemological example illustrating the way that organic form is selected, not only by the environment in which it must function, but by history or 'historicity', the forms from which it descends. In 4.2.2, I argue that, far from being confined to biological evolution, co-selection is an inevitable feature of embodied knowledge, all knowledge recognized by a naturalist. The embodiment, the physical vehicle of knowledge – whether it consists of stone fragments, nervous tissue, or, in the case of science, a social system (4.2.3)—is itself a co-selector.* Bear with me or scan rapidly.

4.2.1 Co-selection by interests and history. It is plausible that beliefs 'fit' the environment because that environment has selected them. But that environmental selection, leading to 'competence of reference' (a.k.a. validity, truth), will have been only one of "many" selective forces, one of many 'co-selectors'. 'Competence of reference' selection will have been only one of several kinds of selection, although in biological and cultural evolution there may have been selection to maximize such selection, and to render minimal (or as stable background for contrast) the most dominant of the other co-selectors.

Let me offer a non-epistemological example. The asymptotic true and complete laws of hydrodynamics have been co-selectors of the shape of the killer whale and the shark (the killer whale and bear are closer relatives), and are 'in part' responsible for their similarity in shape, muscles, and fins. Other co-selectors making for similarity in shape have included their shared interests in predatory speed, and the prior adaptations which each was modifying (historicity). The shape and musculature of the squid has also been co-selected by the true laws of hydrodynamics, but co-selected from different prior adaptations (historicity) and perhaps purposes. The mathematical models and contextual language of the physics of hydrodynamics have also been selected by these same true laws. The latter have been co-selected by the traditions of paper and pencil mathematics, two-dimensional graphics, and historicity. In no sense is mathematical hydrodynamics a pure 'representation' independent of the products of other co-selectors. (Nor is it independent of selection by use, but it may have achieved greater multi-use possibilities.)

4.2.2 Vehicular requirements for embodied knowledge–general. A different tack on "only co-selected" comes from *my 1987 participation in a symposium on naturalistic epistemology:*

Even though naturalistic epistemology as a movement announces the relevance of the anatomy and evolutionary biology of eye and brain, most of its discussion, including much of my own (1959, 1966, 1974a, 1974b), employs philosophical concepts and vocabulary. In contrast, the present essay attempts to keep to a language of physical substances, placing 'knowing' in a framework of material things and systems, of physical objects and processes. Pure epistemology may often deal quite profitably with disembodied, unrealized, and abstract belief and knowledge. Not so, however, for the descriptive epistemology I attempt. Instead, the knowledge it studies will be physically embodied in some substance, some vehicle or carrier. This vehicle will have its own physical nature and limitations.

Let us make this more vivid by considering a mosaic mural done in stone fragments and picturing a street scene, as an example of embodied knowledge of the street, buildings, and persons depicted. The size of the stones, the thickness and color of the cement, the range of natural colors available, the restriction to a two-dimensional surface, the required rigidity, etc., all contribute to the substantialized belief or knowledge that is carried, all become a part of the picture, reducing its validity from any ideal of perfection, were such a conceptualisation feasible. The end product, knowledge, at its realized best, is some compromise of vehicular characteristics and of referent attributes. Where validity is our goal we of course minimize the vehicular contribution as much as possible, as by using smaller and smaller pieces of stone, and cements that are thinner and more transparent. But we can never completely eliminate vehicular restriction and bias for embodied knowledge. This also holds true for retinas made of rods and cones, for nerve cells, brains, memory processes, visual perceptions, innate reflexes, stimulus-response associations, thought and cognitive structure.

Without having done the logical analyses that might make them compelling, I have leaped to some general principles that will guide my explorations: the vehicular substance that carries knowledge is unavoidably alien to the referents of knowledge—it is a different substance with different structural characteristics. Complete flexibility in depiction, reflection, transmission, or recording, is precluded by the structural requirements of the vehicle. If the vehicle is completely flexible it lacks the rigidity to hold together the picture it carries. These vehicular-structure requirements produce not only restrictions on fineness of detail, but also bias and limitations of aspect. Keeping the vehicle intact becomes a requirement in rivalry with the requirement of validly mapping the referent.

This alien, limited, biasedness I extend to less obviously physical vehicles of knowledge, such as spoken, written, and remembered language, logical symbol systems, and mathematical notations. Their rigid structures of terms and syntax are vehicular requirements distorting the referents to some degree. This analysis can also be extended to the self-perpetuating social systems that are the vehicles for scientific knowledge (CAMPBELL 1979a). The social glue that holds such groups together has structure-maintenance requirements that limit and bias the portrait of the world such social groups sustain.

Descriptive epistemology will need eventually a physical theory of optimal vehicles. Think of plaster-of-paris casts, clay, magnetic tapes, photosensitive chemicals, and fixing processes in photography: do these always involve a two-phase process, one phase of maximal flexibility ... and a second phase of rigidity. Think of how we choose stone and wood for realistic sculpture: is it required that the physical structure of a good vehicle be fine-grained? Do nervous tissue and genetic codes conform to such principles insofar as they differ from other bodily tissues? How are these physical requirements for stable record related to revising, expanding, and improving embodied knowledge? Under what conditions are partial revisions possible? Is total substitution of a different portrait generally a more mechanically feasible procedure than retouching it?

A similar applied physics of structures is needed for detection and transmission systems. Fritz HEIDER in his 'Ding und Medium' (1926, 1959) was thinking about such issues. A 'transparent' medium seems to be one that contributes least of its own structure to the knowledge it transmits. But it must have some structure to transmit other patterns at all ...

Let us pause for a moment in this physicalization. Like most traditional and modern epistemologists, I, too, regard conscious experience, visual perception, memory of past events, language, and the mathematical formulae of modern physics, as prime exemplars of embodied knowing. I recognize that most epistemologists, descriptive or otherwise, will profitably stay within these bounds. But I also feel that it may be useful for some of us to try placing these prime exemplars in radically different conceptual frameworks. I have started such an exploration in adding the strange examples of mosaic murals and plaster-of-paris castings. [CAMPBELL 1987a, pp167–169]

4.2.3 Vehicular requirements in the social system of science . In a partially overlapping presentation, I extend this attention to vehicular maintenance to the social vehicles of scientific knowledge:

The requirement of vehicle maintenance becomes a structural requirement operating as a selective factor in the winnowing of knowledge representations. First, a biological example accepting the common metaphor of the gene as a code embodying adaptive 'knowledge'. Consider a gene bombarded by cosmic rays that disrupt and rearrange its prior structure. In order for the resulting material to compete as a mutant gene which might improve the fit of organism to environment it must first meet the structural selective requirement of being a gene at all, of being a stable alternate form of DNA molecule capable of duplication. The great bulk of the disruptions produce rearrangements that fail to meet this structural requirement, being incomplete or imbalanced. There are also other intraorganismic selection levels involved that could be separated out with profit, but which I will lump for now with the structural. Thus, the stable DNA molecule must be one around which a messenger RNA can form, with this RNA capable in turn of serving as a template around which a stable molecular alternative among the proteins can assemble. It also must be a DNA molecule that at times escapes the inhibitory influences that inactivate most genes most of the time. The proteins produced must form nonlethal composites with the preponderance of the proteins other genes have produced. If, after all of this internal, structural selection an adult, fertile phenotype is produced, this phenotype is then subject to an external natural selection. Of all of these many selective systems, only this last can involve an improvement in the fit of the organism to the environment, an increase in the 'knowledge' which the genome carries in the external world.

Similarly, before a scientific community can be a self–perpetuating social vehicle for ever-improving a set of beliefs about the physical world, it must first meet the social-structural requirements of being a self-perpetuating social system. The requirements of achieving this 'tribal' continuity come first, even if they compete and interfere with the cognitive task of increasing the validity of the image of the physical world carried by the 'tribe'. A scientific community must recruit new members and reward old members well enough so that young recruits will be attracted to a lifelong commitment to the field and will justify the drudgery and the painful initiation rites. Journals must be published, purchased, and read. Members must remain loyal to the group and not 'defect' to other tribes. Jobs must be found for loyal followers. Social facilitators are needed to keep the group together and must be rewarded for this role, even if this means giving them scientific honors not earned by their contributions. The requirements of leadership for coordination and continuity may produce leaders whose decision-making power is used to protect their own social positions and their own scientific beliefs against internal challenge from young rivals. The deeply ingrained social custom of building ingroup loyalty by mobilizing hostility and disgust toward outgroups may be employed as a convenience (and perhaps even occasionally as a necessity) in maintaining group cohesion and continuity. Without meeting these social-structural requirements, there can be no scientific community to serve as the vessel carrying scientific knowledge.

These social-structural requirements make it appropriate to 'accuse' a scientific community of being tribelike, that is, of having some basic similarities to other self-perpetuating social belief (and superstition) maintenance systems. This 'accusation' will be appropriate to single schools within a scientific discipline, to whole disciplines, and to coalitions of disciplines such as the physical sciences.

Calling attention to the functional requiredness of these shared tribal features may help to make them seem more compatible with a respect for science as a social system remarkably effective in its achievement of valid shared belief. This task is incomplete, however, until a sociologized version of the "demarcation" problem of POPPER (1959) and other philosophers of science is addressed: How does the social system of science differ from that of other self-perpetuating belief systems, and, more particularly, what, if any, are the social system features relevant to minimizing the interference with the validity of scientific beliefs coming from the necessary tribe–maintenance vehicular requirements? [CAMPBELL 1979a pp184–186 and CAMPBELL 1988a pp492–493]

4.3 Conclusion (Intermediate)

Analogies between theory change in science and biological evolution do "not" help justify scientific beliefs. Instead:

'A selectionist model for a scientific belief 'justifies' such a belief to the extent that it is plausible that 'the way the world is' has participated as one of the systematic selectors of that belief from among the historically developed rival beliefs.' Spelling this out will lead to rather orthodox conclusions: experimentation is important as, too, are competitions in the prediction of natural (e.g., astronomical) events. The ideology of the 17th century scientific revolution held out as an ideal a social-construction system that would plausibly increase the role of selection of scientific beliefs by their presumed referents.

In what follows I reexamine 'knowledge' and 'justification', then introduce the grounds for 'competence of reference' in ordinary language, and finally come back to give this conclusion in more detail.

5. An evolutionary perspective on what we can expect for 'knowledge' and 'justification'

5.1 'Knowledge' is more indirectly (but more precisely) selected than are biological structures

While some naturalistic epistemologists (e.g., KORNBLITH 1985, 1–13) see appeals to evolution as a mode of answering the skeptics, I do not and have never done so. In terms of traditional epistemology, I have from the first (CAMPBELL 1959) sided with the skeptics. The shift from my 1960 dogma, announced above in section 2.2, adds to my emphasis on the presumptiveness and indirection of the 'foundations' of knowledge. Vicarious selectors, such as vision, employ presumptive vicars for 'the environment', not the environment 'directly'. And, of course, the environment is not very precisely represented even in the natural selection of genes and, through them, of proteins and protein adjacencies. Selection at this level is a highly stochastic process based upon slight probabilistic advantages. The environment 'represented' is always a past one, with only a slight advantage being given to the most recent periods. Very strong are the implicit 'assumptions' concerning the regularity of nature, the representativeness of past samples, and the competence of accumulated models.

The strongest regularities will be crudely mapped first, with contingent modifications occurring in later adaptations (see CAMPBELL 1988b, 467–471, 1987a, pp182–185). *Take for example*

> *... the New England fruit tree: It has achieved its mapping of the seasons not by an inductive procedure in which records of thousands of years have been averaged, but rather by retaining the remnants of many tried-out seasonal rules. These no doubt began with single-contingency rules, perhaps based upon temperature alone, later superimposing contingencies based on the amount of daylight, etc. Within each of the contingencies, the homeostat setting or reference signal undergoes continuous editing as late frosts and missed warm springs affect differential survival. Both early and late in this sequence, the fruit tree's map is more simple than the seasons themselves. In certain years, dramatic evidence of misfitting occurs, as when a whole season's crop is lost to a late frost, but the species is well-advised to overlook most such anomalies and counterevidence, to avoid being overresponsive to a misfitting year or to overfitting a specific locale. [CAMPBELL 1988b, p468]*

Vicarious selectors such as trial-and-error learning, and especially vision, while much more presumptive than natural selection of genes, are nonetheless more competent for the immediate environment of behavior. While the presumptions that make them work are historically as deep as any aspect of bodily form, the 'inventions/discoveries' by which they work include aspects sensitive to a much narrower 'specious present', and their 'selection ratio' (were we to borrow such a concept from the statistical theory of evolution, population genetics) is much higher, more precise.

5.2 A perspective-free, context-free and interest-free embodiment of knowledge is impossible

This is the conclusion of section 4 above. The context of co-selectors will be there. These will include the vehicular co-selectors for sure, but also there will be far *less relevant historical* co-selectors. 'Dialectic indexical historicity' connotes the point that the language of new theories and the design of new experiments involve contrasts with predecessors, and are uninterpretable without such awareness.

5.3 'Knowledge is undefeated justified true belief'

This latest amendment of the Anglo-American orthodox definition of knowledge (LEHRER 1989, POLLACK 1974) acknowledges that 'justification' is always potentially defeasible, never complete. 'Knowledge' is 'not yet defeated' belief. HARMAN's (1965) account of 'justification' in terms of 'inference to the best explanation' acknowledges this same incompleteness and, moreover, makes 'justification' a comparative process, in which 5.1 and 4 are not denied, but hopefully 'held constant' by being shared by the competing beliefs. This occurs in a sort of "pragmatic eliminative induction" (DUNN 1995), in which elimination of rival explanations is only 'plausible', historically dated, and never complete.

5.4 The ubiquity of 'inductive incompleteness.'

It is convenient to make this overlapping point by quoting from a *1993* essay *(cited below) The point is that all induction is incomplete in that it disregards the existence of a multitude of interpretations or hypotheses that are, like the chosen one, consistent with the data. The passage that follows provides examples of this inductive incompleteness or 'underdetermination' in science and in visual perception, and argues that while it is ubiquitous and inescapable, it does not render useless labor to eliminate some plausible rival hypotheses in any given domain.* My own agenda for the past thirty-five years has been to relate the philosophers' epistemological problems to evolutionary theory, and to that more abstract model of discovery and adaption shared by trial-and-error learning, natural selection, cultural evolution, acquired immunity, radar, sonar, echolocation, and vision: 'selection theory' for short. Pursuant to that agenda, I would like to relay graphically in figure 1 what I take to be the consensus position of modern epistemologists and philosophers of science. It is a perspective that provides philosophical warrant (were any needed) for the symmetrical, relativist, social constructivist, sociology of scientific beliefs.

One of the 'scandals of induction' can be expressed by noting that science makes use of an invalid logical argument, making the error of the 'undistributed middle term', or of 'affirming the consequent'. But while invalid, the argument is not necessarily useless. The logical argument of science has this form:

If NEWTON's theory "A" is true, then it should be observed that the tides have period "B", the path of Mars shape "C", the trajectory of a cannonball form "D". Observation confirms "B", "C", and "D" (as judged by the scientific consensus of the day, QUINE-DUHEM cop-outs notwithstanding). Therefore NEWTON's theory "A" is 'true'.

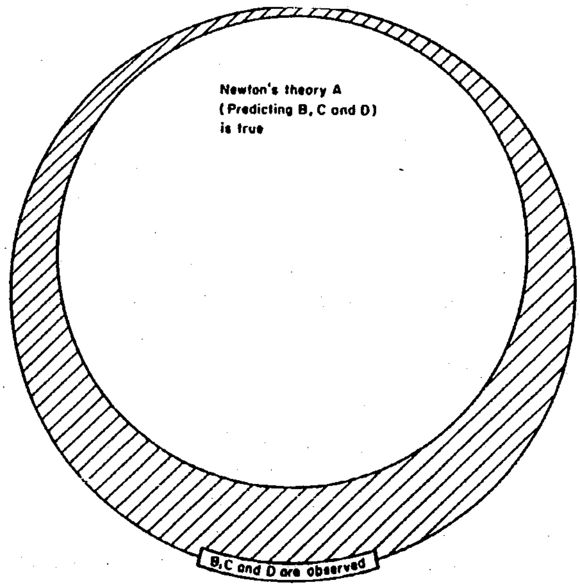

Figure 1: NEWTON's gravitational theory as an 'incomplete induction'.

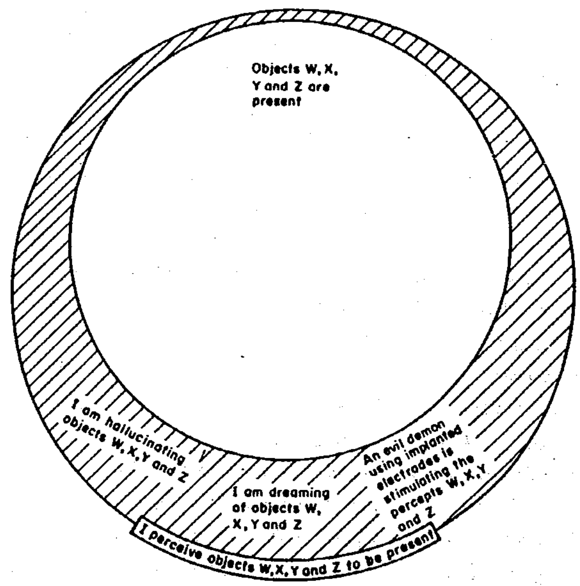

Figure 2: Visual perception as 'incomplete induction' (with apologies to DESCARTES).

We can see the fallacy of this argument by viewing it as a quasi– EULER diagram, as in figure 1. The invalidity comes from the existence of the cross-hatched area, that is, other possible explanations for "B", "C", and "D's" being observed. But the syllogism is not totally useless. If observations inconsistent with "B", "C", and "D" are agreed on by the consensus of participating scientists, these impugn the truth of NEWTON's theory "A". The argument is thus relevant to a winnowing process, in which predictions and social consensuses on observations serve to weed out the more inadequate theories. Furthermore, if the predictions seem confirmed by the consensus of current experimentalists, the theory remains one of the possibly true explanations.

All inductive achievements are 'incomplete inductions' (CAMPBELL 1990b), with an incompleteness such as is graphically illustrated in Figure 1. It is now generally recognized that this incompleteness is equally so for the so called 'facts' that test or 'falsify' theories. Any 'well-established' scientific fact which falsifies a theory is a socially negotiated consensus for which a diagram like Figure 1 could be drawn, with a fringe area of plausible rival interpretations: Thus, the reference to 'QUINE–DUHEM cop-outs notwithstanding' in the second term of the syllogism above.

The quasi-EULER diagram is also useful in presenting DESCARTES's skepticism about sense perception, as in Figure 2. DESCARTES was perhaps the best neurophysiologist of vision and physicist of vision of his day. In this role he took a reflexively realistic stance in taking the machinery of perception to be made up of real objects and events in the world, comparable to the ordinary objects of perception. He studied the physics of light rays, their propagation through pinholes and lenses (both of glass and from ox eyes). He posited a subsequent message transmission through nerves to the brain (hydraulically, by fluids in neural tubes). All of these mechanical links increased his awareness of the possibility of malfunction, of pseudotransmissions initiated at an intermediate link rather than by the 'perceived object' itself, and of intrusions into this mechanical sequence from tangential causal chains. All this increased the plausibility of the skeptic's argument from illusion. Trust in perceptions produced by such a mechanism required faith in powers or processes that would keep the vulnerable causal chain insulated and free of defect. Lacking selection theory, DESCARTES chose God. Those modern evolutionary epistemologists who invoke biological evolution (e.g., QUINE 1969, RESCHER 1977, GOLDMAN 1986, and the many others cited in CAMPBELL 1974b and CZIKO/CAMPBELL 1990) use dear old Mother Natural Selection to support a parallel trust in vision, albeit a more qualified trust, not providing incorrigibility. But many selectionists at the level of the evolution of the visual system tend toward a complacent foundationalism with regard to the momentary operation of vision.

> DESCARTES got to his skepticism about vision from what he took to be the illusory vividness of his own dreams, from an up-to-date knowledge of the physics, anatomy, and physiology of vision, and from a pathological need for certainty. But his analysis has been a part of the great tradition of perceptual skepticism back to the pre-SOCRATICS. PLATO's parable of the cave (bk. 7 of 'The Republic') has that theme. The "strange prisoners" are "like ourselves." "They see only ... shadows. To them, the truth would be literally nothing but the shadows of the images." In this allegory, "the prison house is the world of sight." Note how compatible this is with our modern physics and physiology of perception, in which the brain reifies objects from patterns of light indirectly and superficially reflected from them.
>
> From the epistemology exemplified by Figures 1 and 2, all knowing can be epitomized as guessing what is casting the shadows — the shadows on our retina or the shadows on our laboratory meters. [CAMPBELL 1993, pp90–93]

In the article from which this section is extracted (CAMPBELL 1993) two pages were devoted to an ambiguous silhouette which when seen alone looks like a gunman, but in the second figure is displayed as the shadow of a woman tennis player. These figures were

> ... an advertiser's illustration of the equivocality of shadows. But the more fine-grained detail of a photograph (or of a 'direct' perception), differs from the silhouette shadow only in degree, not in fundamental epistemology. Psychoepistemologically, the 'guesses' of direct perception are unconsciously automated, and the conscious experiential 'givens' are of external objects as though directly, unmediatedly, known. But this did not mislead PLATO or DESCARTES, and it should not mislead us as epistemologists. [N]ote that any transient belief that the shadow caster was a dark alley gunman, or the belief that the photo was of a tennis player, is only co-selected by the shapes of shadow and photo: Essential also to their formation are the culture and experiences providing the repertoire of possibilities, one or another of which was triggered by shadow form or photo contours.

As social animals, we acquire confident beliefs through the reports of others. The layers of equivocality are then more numerous, as shown in figure 3. As the process is diagrammed here, I may end up confidently believing that, in the next room, out of my sight, the cat is on the mat. This belief is compatible

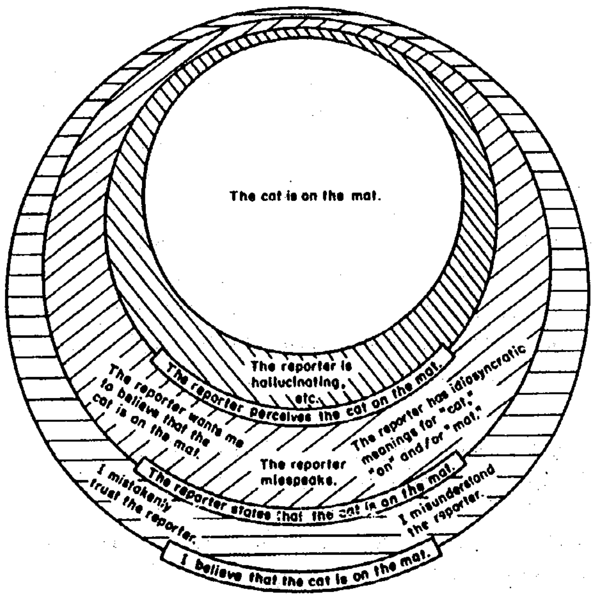

Figure 3: Verbally transmitted belief as 'incomplete induction'.

with the cat's really being on the mat (the inner clear, circle). But the reporter who supposedly is in a position to see the cat in the next room may have hallucinated, and the cat was not really on the mat. The inner two circles are as figure 2 (DESCARTES). The reporter speaks out, "The cat is on the mat." Now this may be due to the cat's being on the mat, and being perceived as on the mat, or it may be due to idiosyncratic semantics for "cat", "mat", and "on", or it may be due to the reporter's wanting me to believe that the cat is on the mat whether or not it actually is.

For example, the reporter may be my child, who sides with the cat's preference for sitting on the sofa and does not want me to go in and discipline the cat. This source of equivocality is one peculiar to social vertebrates, not shared by the social insects. For us social vertebrates, in our public truth claims, there are always two motives: (1) to report validly to the best of our own knowledge and (2) to influence the decisions we expect to be made on the basis of our report in a direction deemed favorable to us. These two motives are often in conflict. The second is often the stronger, particularly if our very lives and livelihoods depend on those decisions. Since 99% of the beliefs of a scientist are solely dependent on the observations of others, this makes social control of the validity of reporting central to an epistemology of science (as Hull 1978 has noted) and to ordinary knowing of social animals (even if not for a nonlinguistic solitary perceiver).

of the likelihood of being caught fudging the data, and the degree of humiliation it would entail, might also be involved. These factual beliefs are surely social system products. Probably the negative utilities of lying and shame, the degree to which one values one's honor among peers, how much one values HULL's (1988) "conceptual inclusive fitness", and so forth, are also social system products to a substantial degree.

Not all conversions from one partisan position to another are to be interpreted as symptoms of the second position's superior validity. In many cases of conversion, a sociological analysis may find that social power, within the scientific community or external to it, provides the most plausible explanation. In the case of DARBISHIRE'S conversion, however, it seems overwhelmingly more plausible that it was his own data and a social system which, among other things, made possible his being cross-examined on his data and gave him the freedom to change sides without loss of job or career. [CAMPBELL 1994, pp xv-xvi]

The goal and the approximative practice of 'rational inference' should be retained, but the conceptualisation of what 'rational inference' is, or could be, must be made more realistic. In other words, our conceptualisation of rational inference must be, sadly enough, relativised and contextualised. At least three compromises with the EUCLIDEAN ideal of rational deduction must be made if the deductions are to be relevant to the validity of descriptive beliefs about a belief-independent world.

First, it must be recognized that there are many more axioms, and they are much less secure, than the EUCLIDEAN ideal assumes. Under that ideal there are 'a few indubitable axioms', and many true deductions from them. But history has shown these axioms (some at least) 'are indeed dubitable, and none are of proven truth'. For rational inference in science, we need thousands of 'axioms', i.e., unproven presumptions we tentatively trust. Rational inference is possible only within a community of discourse that shares most of the same 'presumptive axioms', unproven but trusted beliefs about the nature of the world and science. All of these presumptive axioms are incomplete inductions.

For examples, look back at Figures 1 and 2 of section 5.4 above. The crosshatched areas of each contain infinities of potential rival hypotheses. 'Proof' consists of eliminating only those few plausible alternative explanations which our community has made explicit. 'Absurdly implausible' rivals are not considered. (Note that even in his small and tidy domain, EUCLID had to employ the illogical, non-entailing 'reductio ad absurdum'.)

Second, contrary to the EUCLIDEAN ideal, the correspondence rules between posited things in the world (objects, actions, events) and their logical or algebraic vicars are imperfect, with an imperfection that is context-dependent, and which may differ in various locations in the deductive sequence. For descriptive purposes, even the law of contradiction may not hold. The logical 'ps' and 'qs', and the algebraic 'xs' and 'ys', are pure and mono-attributional. The real-world referents of these are invariably loci in n-dimensional space, multi-attribute syndromes. Using 'ps' or 'xs' as their vicars in a logical or mathematical deduction is a very approximate affair, and, as a predicted experimental outcome, 'p' may not be quite the same as it was earlier in the deductive network.

5.6 Natural kinds and concepts

My third proposed compromise with the EUCLIDEAN ideal of rational deduction involves rejection of the idea that natural kinds have essences and are defined by necessary and sufficient conditions. The following passage (from CAMPBELL 1988b, pp457–460, a transcript of the William JAMES Lectures at Harvard University, 1977) makes this point by reflecting on the processes of language learning.

There is another aspect of those samples of the real world heavily utilized in the ostensive teaching of the initial vocabulary that enhances entitativity, cognizability and -talkaboutableness. This is the preponderant emptiness of the n– dimensional attribute space and the resulting discreteness of 'natural kinds'. Imagine a space of possibilities with a dimension for each possible descriptive variable that might be used to describe cats, dogs, squirrels, robins, ducks, geese, fir trees, oak trees, dandelions, grass, stones, clods, rivers, and other natural kinds. The attribute dimensions could include height, length, breadth, weight, redness, greenness, moisture content, carbon content, fuzziness, furriness, angularity, dendricity, compactness, location and mobility in latitude, longitude, altitude, etc. If we plot the location of individuals of one natural kind in such an n-space, they cluster tightly together. In contrast, the space between any two natural kinds is vast. Let me make my imagery clearer by locating a few natural kinds in a 2-space [see Figure 4]. Even though there are only two dimensions, this drawing illustrates the point that most of the space is empty. Even though kinds A and B overlap on dimension

The crosshatched areas of figures 1, 2, and [3] can never be entirely eliminated. Beliefs, and the best of current scientific theories, will always be underdetermined, underjustified. This ubiquitous 'inductive incompleteness' (CAMPBELL 1990[b]) leaves ample room for the influence of social and personal interests seemingly tangential to scientific inquiry. The research achievements of the symmetrical, social constructivist, relativist programs in the sociology of scientific knowledge amply document such influences. [CAMPBELL 1993, pp93–96]

5.5 'Rational inference' and the coherence strategy of belief revision

Those making efforts to refute the claims of relativist, constructivist sociologists of scientific knowledge sometimes argue that 'scientific rationality' adequately explains the adoption of new scientific beliefs in response to new evidence, *without recourse to sociological explanations. Such an argument has been made for the conversion to MENDELISM of DARBISHIRE. He was the major student of Weldon, who, with Karl PEARSON, militantly defended continuous variation biometric theory against William BATESON's MENDELISM. With this, I vigorously disagree. And, more generally, I believe that philosophers' use of rationality as a cause of belief change will not stand up under scrutiny.*

KIM (1994) examines the historic episode of belief change from biometry to MENDELISM, circa 1910, using a sociology of scientific validity to rebut an earlier externalist 'sociology of scientific knowledge' focussing on the same episode. In my introduction to KIM's book, I provide a critique of the use of rationality as a sufficient explanation for belief change in science. This 'rationality' cannot be understood as a context-free deductive system producing entailed conclusions. Thus we find that:

> DARBISHIRE's bias in his initial report bears a sociological similarity to episodes of fraud, but, as KIM (1994) dramatically reports, under BATESON's private cross-examination of DARBISHIRE's data in correspondence, and under CASTLE's published criticism of his analyses, he confessed his errors in print, even to the point of describing his previously published articles as attempting "to refute the MENDELIAN theory by all costs". This dramatic reversal is the result of a social persuasion process in which DARBISHIRE's own data played a major role.
>
> DARBISHIRE's behavior conformed to the norms of science as traditionally viewed. But neither DARBISHIRE's nor PEARSON's nor WELDON's behaviors are, however, explained by identifying their examples of, or violations of, 'scientific rationality'. Their behavior, instead, was the product of social system, social locus, and individual personality. From a future, more thoroughly developed sociology of scientific validity, one could generate recommendations for optimal individual behavior and optimal institutional norms for the goal of optimizing the validity of the consensus beliefs of a focal group of scientists. These recommendations, we may anticipate, will have much in common with the ideology of the early scientific revolution, and with what scientists refer to as 'the scientific method'. Conformity to such norms may be collectively 'rational' for a scientific community, but is not explained by so designating it. The term 'rational' at its best refers to ideal norms, not causes of behavior. But even as norm, the meaning of 'rational' is in flux. It can no longer be identified with 'logical'. Indeed, it is now generally recognized that where science results in belief in a theory's truth, it does so by way of an invalid, but pragmatically useful, syllogism ... [See above, the first section of 5.4 and Figure 1.]
>
> Microeconomics is based upon defining rationality in terms of an individual person's rational optimization of his or her own utilities (not a group's optimality), and this model is colonizing substantial segments of sociology. This model may or may not be appropriate. For a sociology of scientific validity, the goals of a collective, not an individual, need optimizing. But even were individualized rationality adequate for our purposes, it would not help us much. In the case of Lincoln STEFFENS's predecessor graduate student [CAMPBELL 1994, p xiii], falsifying his data was rational behavior, rewarded by career success. Given the parallels, DARBISHIRE's behavior might seem irrational. But to make that computation, we have to know for him the negative utility or pain of dishonesty, and many other personal utilities we can only speculate about.
>
> To employ a model of individual rationality, one needs to also know DARBISHIRE's information base. Rational actors never have complete information, particularly about the future. DARBISHIRE's behavior becomes selfishly rational if he believed that there was a 'truth' to the matter, that the consensus of fellow scientists would soon converge on that truth, that his own data indicated that MENDELISM was correct, and that it was best for his career if he joined that future consensus as early as possible. His quasi-factual estimate

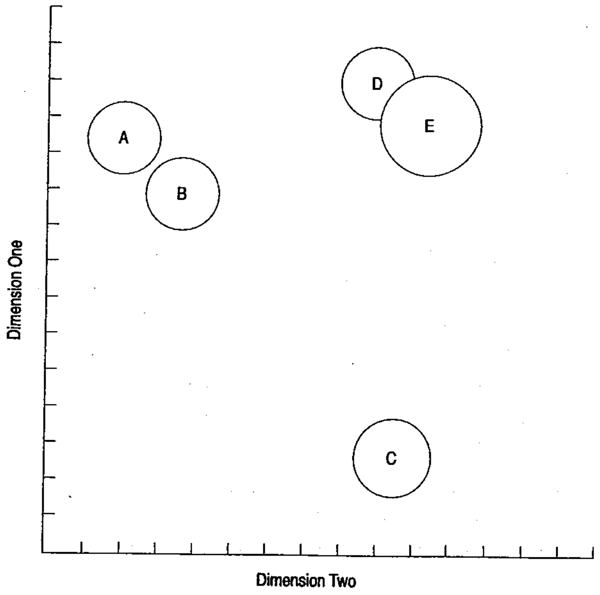

Figure 4: Some 'natural kinds'.

1 and dimension 2, in the 2-space there is empty space between them. The overlap between kinds D and E is not so resolved, but if we were to add dimension 3, you would find that kind D hangs close to the blackboard, while E is out here away from it. By the time there are five to ten dimensions most natural kinds are separated by wide spaces. Stephen GOULD (1980) has pointed out that even within the compact attribute space of the three dimensions generating mollusk shells from flat clams to helically coiled snails, the great bulk of the attribute space is empty and species tend not to overlap. Adding more attributes generally increases the empty space.

There are, of course, hundreds of usable attribute dimensions. It is characteristic of natural kinds that any two kinds differ on innumerable dimensions. (Is this a counterpart of the emptiness of the space?) Numerous small subsets of attributes have equivalent practical effectiveness as distinguishing features. The attributes a child uses to recognize a cat may have no overlap at all with those used by a paleontologist, and neither of these subsets need overlap with those used by a dictionary maker, yet all three may be effectively diagnosing the same natural kind. A natural kind once identified and named is rich with attribute characteristics yet to be discovered. Thinking in terms of definitions and essences, there seems to be rich redundancy of usable essence-sets or potential definitions, most yet to be discovered. On the other hand, the view can move one, and it moves me, to abandon the concept of definition for natural kind words, and to recognize that they are learned as ostensively as are people's names or proper nouns in general. Rather than 'definitions', we have workable short lists of diagnostic symptoms substituting for larger syndromes of such symptoms, mostly yet undiscovered. When we introduce a new proposed hypothetical natural-kind entity in the course of intellectual development, we are at the same time asserting the existence of a syndrome we have only very partially mapped.

One other feature of this n-attribute space model further undermines the use of essentialist definitions. There are so many attributes, and the spacings are so far apart, that any one of the usual attributes can be missing, can have a zero value, and the individual still will be closer in the n-space to its natural kind than to any other kind. Thus a black swan is still a swan if feather color is all that is out of line with the usual symptom set, prototype, or stereotype. Thus even before DARWIN, LINNAEAS had classified snakes as quadrupeds, that is with the reptiles, for which four-footedness was an essential characteristic. For anatomists and physiologists, they shared so many attributes that even though four-footedness was missing, snakes still obviously belonged. Thus the plucked duck ready for roasting, lacking feathers, ability to fly, webbed feet, long flat bill, fertility in breeding with other ducks, and any other of the usually used symptoms of duckhood, may be still much closer to ducks in the n-dimensional attribute space than to any other natural kind ...

KUHN's (1974, pp472–82 and 500–513) important discussion of learning natural-kind terms is both partially similar and has partially inspired this exposition. Note particularly his insistence on the importance of empty inter-type space in the world of objects (p475) if similarity perception is to operate and his insistence that learning to name ducks, geese, and swans inseparably also involves learning something new about the nature of the world. In his discussion with SUPPES, SUPPES, and SHAPERE (KUHN 1974, 500–513) he would, I believe, have been aided by a more radical rejection of ostension as 'defining' (in contrast to puzzle-setting), also by a rejection of the existence of 'definitional' (substitution-permitting) relations between observation terms and theoretical terms, and by an explicit introduction of a quasi-ostensive process of puzzle-setting (but not 'defining') in all learning of theoretical concepts in science, as will be described below. As I understand him, he and I are in agreement on these points. [CAMPBELL 1988b, pp457–460]

The 'deductions' in scientific reasoning are gross 'ceteris paribus' shorthands, and the 'other things' assumed to be 'equal' in the deduction include the effects of many as yet undiscovered laws.

These three modifications of the EUCLIDEAN ideal 'do not at all rule out useful rational inference dialogue' among a community of scholars. This dialogue will employ the strategy of raising rival hypotheses and evaluating their plausibility.

This *plausibility evaluation of rival hypotheses* is also the 'coherence' strategy of belief revision. LEHRER (1974) working within the 'knowledge is justified true belief' tradition, contrasts the coherentism which he advocates as a mode of justification with the perceptual foundationalism advocated by Chisholm (and many others). POLLACK, HARMAN, GOLDMAN, and QUINE (see CAMPBELL 1990ms) and many others are coherentists in this sense. This Anglo-American coherentism must be distinguished from the continental 'coherence definition' of the meaning of truth. Coherentism is compatible with a correspondence meaning of the word 'truth'. LEHRER himself (1989, p132) says: "Knowledge arises when there is the appropriate sort of match between all of what a person believes and external reality." But neither LEHRER nor the militant correspondence theorists *of truth*, such as QUINE and POPPER, have ever suggested that correspondence is available as a truth test for specific beliefs. Indeed their advocacy of the QUINE-DUHEM problem (or its equivalent) makes it clear that they reject this possibility.

5.7. Knowledge is always oversimplified, interest-relevant and superficial

A complete videotape with sound-track of one's past would be a useless form of memory. It would take longer to search it than it did to experience it. By way of my philosopher friend, Mark BROWN, comes this simplified version of a parable from Lewis CARROLL (presumably to be found in CARROLL 1898). There is a competitive conversation between two cartographers. The Englishman brags: "We now have mapped all of England one mile to the inch". The German replies, "That is nothing. We have a map of Germany one inch to the inch, but the farmers won't let us unroll it". The goal of completeness for knowledge is profoundly misleading. 'Knowledge' is implemented by superficial reflections.

5.8 Maps are better epitomes of knowledge than are propositions

They obviously employ interest-relevant selective simplification while still retaining the possibilities of validity and error (TOULMIN 1972, Chapter IV, GIERE 1995).

5.9 'Competence of reference' is primary: 'Representation', where approximated, is in the service of competence of reference, and is always very incomplete

As a naturalist, I take the history of animal adaptations (innate and/or learned) and nervous systems as relevant to 'knowing'. The great evolutionary divide (independently occurring in insects, crustaceans, mollusks, and vertebrates) occurs when sense receptors evolve into distance receptors in object-seeking locomotor animals.

Before that divide, knowledge represents 'if-then' rules, where the 'ifs' are single sense-organ cell activations, and the 'thens' are specific muscle contractions. While it is misleading to use the word 'reference' for knowledge at this level, I will do so as a temporary stopgap. 'Competence of reference' at this level is achieved when the activation produces a response that is on the average adaptive. *Through evolution,* the 'if-then' rules at this primitive level are *greatly elaborated* into contingent rules such as 'if then but only if'. These reduce the equivocality (the inductive incompleteness) of the 'belief', but of course do not eliminate it.

On our side of this *great evolutionary* divide, these complex contingency rules merge into the fallible re-identification of patterns. Consider the activation of a single retinal cell: the number of events and objects in the world which might plausibly have activated it is very large. But for a pattern of multiple retinal cell activations, this equivocality is greatly reduced (though still technically infinite). Note too, that it is now 'the pattern', not the same specific retinal cells, on each re-cognition. Competent reference (justified belief) still leaves unresolved the Gettier problem (our long-time friend just might have an identical twin we never knew about). Inductive incompleteness is ineliminable.

On our side of the evolutionary divide, it is not only the 'stimulus', but also the action or 'response' *that has* changed. It is no longer the twitch of a specific muscle. Rather it is a distal response, an act or achievement, with substitutable muscle movements extemporized to reach a subgoal that is itself a perceptual pattern.

"Pattern-matching is essential in distal knowing" (CAMPBELL 1966). Once again, a map rather than a proposition best epitomizes knowledge.

6. Competence of shared reference in language

We who have distance receptors have unjustifiably reified stable external space containing discrete objects. Although *it is* unjustifiable, we reidentify with considerable competence 'the same' objects and events on successive occasions. As a biologist and psychologist, I posit that these unjustified reifications of objects occur very similarly from individual to individual within the same species. It is this person-to-person similarity of reifications (not their validity per se) that make language possible. The following excerpts from CAMPBELL and PALLER *address these issues*

> *[Extending QUINE's model of radical translation, we promote]* an antifoundationalist emphasis upon ostension to explain shared reference. Such ostension cannot be definitional because ostension is unavoidably equivocal, as both QUINE and WITTGENSTEIN (1953) have emphasized. Nonetheless, ostension provides a selective restraint, whereby the referent has some likelihood of participating in the selection of the language learner's guesses. In subsequent ostensive instances, and the learner's use of the term, the mentor or learner may recognize that the learner's hypothesis as to the word's meaning is wrong, and new guesses as to word meaning can be generated. The occurrence of 'rational errors' on the part of children's language use are symptomatic of this process (e.g., CAMPBELL 1988a, pp462–464). From QUINE: "Such is the quandary over 'gavagai,' where one 'gavagai' leaves off and another begins.... The only difference is how you slice it. And how you slice it is what ostension or simple conditioning, however persistently repeated, cannot teach" (QUINE 1969, pp31–32). Instead, whatever shared reference is in practice achieved is a result of "hypotheses of translation – what I call analytic hypotheses... Insofar as the native sentences and the thus associated English ones seem to match up in respect of appropriate occasions of use, the linguist feels confirmed in these hypotheses..." (QUINE 1969, p33).
>
> These are precious passages *(cf. also CAMPBELL 1990)*. They deny foundational status both to ostension and to simple conditioning. There is a fundamental equivocality in the assumed shared reference that results. (The present authors would argue that this partakes of the equivocality that plagues all induction, all relationships between theory and data.) This situation holds for children learning to speak (and protohumans inventing a language for the first time) fully as much as for the radically uninformed translator.
>
> But usable translation is in fact achieved; ... children do learn to use words in effectively sharing reference ... And insofar as valid beliefs are verbally transmitted, ostension (equivocal though it is) has been absolutely essential in the language learning process.
>
> The improbable and tedious process thus described is speeded up and made nearly error-free by the shared innate and learned tendencies to reify middle-sized physical objects and boundable acts. Without hesitation or awareness of alternatives, the child and translator guess that "gavagai" means "rabbit," rather than rabbit-aspect, rabbit-part, rabbit-moment, transient sense data, direction-of-pointing, etc. The perceptual reification of independent objects and events, described in the previous section, will have been naturally selected for the usefulness available when stable discreteness, manipulability, and reoccurrence are typical, thus making possible approximately adaptive learning about them. It is around such pervasively shared reifications that the foundations for usefully shared linguistic reference can be built.
>
> The nature of the referents—their "entitativity" (CAMPBELL 1973)—has already participated in selecting the perceptual reifications of the language learners (and their pre-linguistic ancestors equipped with image-forming distance receptors). The nature of these referents (their "ostensionability") also operates as a strong selective restraint on word meanings that can become socially shared in the first-level ostensive vocabulary: ...
>
> The role of entitativity in guiding the boundaries of conceptualisations and words can be seen through examples of the kinds of designations that do not become words. Take words about fragments of trees, for example: A word for a tree-fragment including leaf may or may not be present in a given language. If it is present, it will divide leaf from tree at that point where leaves typically separate from tree-limbs. There won't be a word for the extreme three centimeters of leaf, nor a word for the whole leaf ... plus the adjacent three centimeters of branch ... [T]he language will follow PLATO's advice and 'cut nature at her joints'...
>
> Language evolves in a speech community. We can imagine there being a continual muta-

tion of new conceptualisations and namings. Only a few of these become part of the common coinage. The selection pressure of the learner's guesses reduces word meaning discriminanda identifiable by others in the language community. Words unattached to dependable discriminanda are lost from the start. Words utilizing subtle discriminanda where adjacent striking discriminanda go undesignated are rapidly vulgarized so that through the multiple confusions of common usage, the meaning drifts to the striking discriminanda.

Thus, through providing the referents utilized in the editing of trial meanings, and through providing the basis for the conspicuous and popular hypotheses as to word meanings,

interpersonal sharing of competent reference seems to me to be impossible (CAMPBELL 1989).

7. Why pre-scientific cultural evolution did not produce competence of reference to presumed invisible causes of visible effects

The short answer is that the pre-scientific, or religious, domain of believing had been co-opted for social solidarity purposes. Sociobiologists posit for us vertebrates (who have not eliminated genetic competition among the cooperators as the social insects have) selfish and nepotistic tendencies to optimize personal inclusive fitness by being 'free-riders', by cheating on the social contract, by avoiding self-sac-

craftsmen and later soldiers—specialists who did not feed themselves but had to be fed by the labor of others. Storage of food, usually in the form of cultivated grain, was always present. Urban residence was characteristic for at least some portion of the population.

It is on the moral orders of these city-states that I focus. I find in them several puzzles that seem to me to be solved by BOYD and RICHERSON's "conformist, frequency-dependent cultural transmission" (1985). All but one are puzzles of uniformity.

(1) All of these protocivilizations were accompanied by political centralization, coordination, leadership, and hierarchical downward– command structures headed by a single person. All were well-organized tyrannies or despotisms.

(2) Although independently socially evolved, all of these archaic city-states ended up with a very similar set of moralizing preachments. All preached the value of duty to the political organization and its customs. All preached the duty of self-sacrificial military heroism in defense of the state. All preached within-group honesty. All preached against self-interested deviations from duty (covetousness, jealousy, etc.).

(3) All supported their moralizing preachments with a supernatural cosmology that provided authority and sanctions for these preachments. (Why were not the force of custom plus interpersonal reinforcements sufficient without such cosmologies?)

(4) The details of these supernatural cosmologies were extremely heterogeneous, differing widely from city-state to city-state. (This is the puzzle of diversity. All others in this list are puzzles of uniformity.) This ... *fact* argues in favor of ... *their independent invention* ...

(5) Compared to the supernatural beliefs of their acephalous predecessor societies, the pantheons and cosmologies of the archaic city-states were more incredible (as judged from a modern secular viewpoint) rather than less so. While we can recognize in these archaic city-states a general cultural advance toward modern civilization, they were more superstitious, more credulous, than their predecessor cultures. *Were* these supernatural cosmologies ... *merely* perceived as myth and poetry? I judge that they were believed ... *in a manner* comparable to today's beliefs in magnetism, gravity, electromagnetic waves, atoms, genes, etc., that is, as invisible but physically real sources of observable physical effects.

(6) Ubiquitous in these religious cosmologies were rewarding and punishing heavens, hells, and reincarnations. These uniformly extended individual hedonic calculations beyond one's own biological lifetime ...

(7) Also ubiquitous were wasteful royal funerals, containing provisions for a royal afterlife. The commonsense, materialistic, calorie-counting, economic optimizing of modern sociobiology (fused in anthropology with optimal foraging strategy) has no tools to explain such wastefulness. Fully useful horses, soldiers, wives, weapons, jewels, and money were interred ... Were this a culturally isolated occurrence, no functional explanation would be called for ... The economic and biological wastefulness is undeniable ... [and] clearly a selective force continually selecting against such customs. Their ubiquity requires ... [an] overriding functionality, which I posit lies in their affirmation of the reality of the afterlife. The functionality of extending individuals' hedonic calculi beyond their biological lives probably needs no arguing ... *It contributes* to the survival of the social group as an entity, and perhaps also *to* the combined biological inclusive fitness of the members (although not *to* any single individual's inclusive fitness).

7.2 Anadaptive cultural transmission and two types of adaptive cultural evolution

We need as a background an anadaptive model of cultural evolution. This should probably be more like a model of nonadaptive genetic drift than a HARDY-WEINBERG equilibrium in which character frequency remains constant in successive generations (Beatty 1987a, 1987b). Adjacent generations in a contiguous lineage are more similar than in noncontiguous ones ... *due to* cultural borrowing from the previous generation. Across generations ... *cultures* change in a meandering way that should not necessarily be interpreted as 'adaptive' to a systematic selective environment. Against this background, we can distinguish two forms of possible cultural adaptation.

7.2.1 Individual level cultural adaptation. The first is exemplified by the cultural evolution of tools, weapons, *and* knowledge of ... materials ... *Change is due to* the fact that individuals can generate variations on the culturally received form, and ... can confirm *their* efficacy (satisficing, not optimizing) ...

The basic recipe for evolutionary adaptations is haphazard variation, selection, and blindly loyal transmission. All of the fit is achieved by selection.

The variations show no foresight ... they are 'haphazard,' 'blind.' The only requirement is heterogeneity ... [S]urviving variations (the genuinely adaptive plus the anadaptive and maladaptive variations not yet weeded out) are ... reproduced with blind loyalty, both the maladaptive and the adaptive, although selection reduces the frequency of the maladaptive ...

For both types of cultural evolution, there is an analogue to this blindly loyal retention. Uniquely flaked spear points remained constant for tens of thousands of years, testifying to the strength of cultural orthodoxy ... Cultural evolution has as its raw material of variations not only chance deviations from the inherited orthodoxy, but also the products of vicarious blind-variation-and-selective-retention processes at the individual level, such as vision (CAMPBELL 1956, 1974b) and creative thought (CAMPBELL 1960). These vicarious processes are not of entailed validity but depend upon the imperfect validity of their presumptions. These 'intelligent' sources of variation are indeed often adaptive for the wrong reasons.

I judge that the adaptiveness of cultural evolution at this 'individual' level is undeniable ...

All adaptive processes require powerful retention mechanisms for the cumulation of already achieved adaptions, as a base upon which fringe variations are explored. Blind cultural conformity is individually adaptive for this type of cultural evolution, increasing individual biological inclusive fitness.

For the theory that follows, we must posit that the individually adaptive products are so valuable that a general tendency toward blind conformity has a net individual inclusive fitness advantage ... Readers should be warned that this is one of the most vulnerable parts of the theory.

7.2.2 Group-level cultural adaptions. For our theory of archaic moral orders, we also need to posit group-level adaptiveness in cultural evolution. This is much more problematic, and for several reasons, to be specified below.

Let me illustrate from some classic small-group experiments, initiated by Alex BAVELAS (see GUETZKOW 1961). Sets of six persons were provided with communication links of contrasting form: circle, hue-and-spokes, and fully connected. Each member was given a few playing cards, and the group was to assemble the single best poker hand from the total of their cards. The spokes pattern was clearly superior to the fully linked and the circular pattern. This held true even where the hub, or communication clearinghouse position, was occupied by the least competent person. When fully connected groups played repeated rounds, there was spontaneous disuse of some links, resulting in a spokes pattern. This organizational pattern is an attribute of the group (unattributable to individuals in isolation) and with a group-level adaptiveness in this experimental ecology.

For the central theory ... we must posit such a group-level selection not only for moral norms, but also for religious-political ideologies. Adaptive cultural evolution at this stage is much more problematic than for 'individual' cultural evolution for many reasons: (1) There are, on the group level, fewer 'units' and fewer 'degrees of freedom' (proportionally to the size of the group). The basic statistical theory of adaptive evolution requires large numbers of quasi-independent units, and shared, consistent, selection pressures. (2) The time units of trait exhibition and selection are longer and fewer. (3) Complex, multiattribute 'objects' of selection for cultural complexes make it much less likely that a specific attribute be selected. In contrast, the selective pressures on the form of a spearhead are much more focused. (4) For those beliefs and organizational forms that are beneficial for the group as a whole, but costly for individual inclusive fitness (producing self-sacrificial altruistic behavior), there is individual-level selection pressure operating against the adaptive group selection. There are no doubt other obstacles. I should doubt that cultural evolution at the group attribute level had taken place were it not for the great obstacle to ultrasociality which I judge genetic competition among the cooperators to be, and were it not for the seven central puzzles of archaic city-states.

7.3 The Boyd and Richerson model: Intragroup homogeneity

Of the many important features of BOYD and RICHERSON's great 'Culture and the Evolutionary Process' (1985), I will make use of only one: conformist frequency-dependent nonlinear (multiple parenting) transmission ('conformist transmission' for short). Like their major predecessors (e.g., GINSBERG 1944, WADDINGTON 1960, reviewed by CAMPBELL 1965), BOYD and RICHERSON note that cultural evolution makes use of cross-lineage borrowing (they call it "multiple parenting") in sharp contrast with biological evolution (save for a few isolated exceptions). Under conditions of ecological diversity and migration, they find that it would be optimal for the learners to adopt the majority (or plurality) position of the mentors (i.e., the 'conformist' version of frequency– dependent cultural transmission)...

Add to conformist transmission the condition of stable small groups semi-isolated from each other. In a dozen generations, these groups will be moved to internal homogeneity on all traits ... In different groups the chance pluralities will be in different directions, in a cultural analogue of genetic drift.

Several things can be noted about this outcome. Cultural unity on a trait need not be interpreted as a product of adaptive selection. Cultural differences between nearby tribes need not be interpreted as adaptations to different ecologies. This is a great emancipation for the believer in cultural evolution. Previously (e.g., in my 1965 model) my anthropology friends would challenge me. "In our people, twins are put to death at birth. In the neighboring people, twins are given special treatment and reared for shaman roles. Both live in the same mosquito-ridden yam culture. Are you going to claim that this can be explained as different adaptations?" (Nancy LEIS and Philip LEIS, personal communication.) Cultural evolutionists have been at least as much burdened by excess adaptationism as the sociobiologists criticized by GOULD and LEWONTIN (e.g., 1984). Indeed, such excesses ... have been the major reason for the rejection of ... functionalism in sociology and anthropology.

The new functionalism which I advocate attempts to avoid this excess adaptationism by requiring for each functionality ... a plausible selection process at the organizational level of the function (CAMPBELL 1974c, 1990a). This new restrained functionalism is greatly helped by the nonfunctional, or afunctional, explanation of intracultural uniformities which the BOYD/RICHERSON (1985, esp. chap. 7) model provides. This new functionalism does, however, still retain the concept of 'latent' functions (functions not obvious to those who practice and transmit the custom, or rationalized by them in other ways) even though it was the concept of latent function that so relaxed the self-critical discipline of the old functionalists, making it possible for them to treat every feature of ... [*any society* as functional. Now with BOYD and RICHERSON's help, functional theorists are forced to distinguish between 'accidental' cultural uniformities and 'selected', or functional, ones. This distinction requires that a plausible theory of selection at that functional level be provided. The functional level upon which this essay focuses is that of the coordinated social group.

7.3.1 Parentheses on reciprocal altruism. At this point, I interrupt my presentation of the BOYD and RICHERSON model for an important aside. These 'neutral' homogeneities within groups, in the context of sharp differences between nearby groups, almost certainly have a function whatever the specific content of the homogeneity, and even if this function was not involved in the selection for the difference. TRIVERS (1971) in one of sociobiology's most important papers has presented the concepts of 'reciprocal altruism' and 'moralistic aggression'. Reciprocal altrusim is also the key to AXELROD's influential book (1984) on the evolution of cooperation. For TRIVERS and AXELROD, the tendency to form reciprocally altruistic cliques ... is explicable in terms of purely individual considerations. The reciprocal altruist pairs or cliques are precarious, and vulnerable to selfish defection. For them to emerge requires long-lived individuals, who are likely to encounter the same specific others again and again, and who have the capacity to identify ... the specific others. Given these conditions, an innate readiness to form such cliques could emerge. TRIVERS posits that under such conditions there would also evolve ... 'moralistic aggression' against partners who violated reciprocity ...

It has been pointed out (CAMPBELL 1979b, pp42–43, BREWER 1981) that a culturally-inherited membership in such a reciprocal altruist pact would reduce the risks involved in negotiating a new one. It would be in the biological inclusive fitness interests of ... parents to force such culturally-inherited membership upon their offspring. All group uniformities on trait-specifically neutral features would be useful signs of co-membership in such a reciprocal altruistic pact. Easily perceivable homogeneities in dialect, dress, rituals, and scarification would be particularly useful ... Moralistic aggression becomes death-to-traitors in this functional explanation of the roots of tribal ethnocentrism.

If we turn the phrase from 'reciprocal altruism' to 'clique selfishness', we note that the internally altruistic groups are exploiting unorganized persons, or organized out-groups. Here is an area in need of clarification. Some presentations of reciprocal altruism read as though it would be to each person's inclusive fitness advantage if all humanity were in a single reciprocal altruist pact, and that its only problem would be that of preventing anonymous free-riders. OLSON's pioneering study (1968) provides formal models and cites experimental studies showing that small groups are much more likely to achieve mutually altruistic cooperative relationships. But this does not provide a rationale for ... anti-out-group polarization which is so ubiquitous in human sociality. The concept of 'clique selfishness', emphasizing the exploitation of outgroups, comes closer. Each ingroup can plausibly accuse the other group of clique selfishness and use this accusation to mobilize their own in-group solidarity. From this point of view, the accidental ingroup homogeneities produced by conformant cultural transmission play a role comparable to that of the unique nest and hive odors of ants and bees. They provide signals as to who is to be admitted and who excluded...

This discussion of reciprocal altruism has been presented as a diversion from the BOYD and RICHERSON theory. But it may be an essential addition. If cultural group selection produces group functional, self-sacrificial altruism, as we shall argue it does, then this produces an individual selection pressure against it, which would tend to eliminate the conformant cultural transmission tendencies which produced it. The math modeling and computer simulations which BOYD/RICHERSON (1985) report have not yet covered this feature. The plausibility of their model would be strengthened by the explicit addition of individual-selectionist supports. The social inheritance of membership in a reciprocal altruist clique is one of them ...

Kin selection is the other individual-selection route to quasi-altruism. The reciprocal-altruist cliques are most advantageous when they are composed of close relatives and when individuals are less closely related to members of other nearby cliques. But status as a relative is predominately learned ... [H]omogeneities on neutral traits become a symptom of kinship. Thus conformist social transmission, and the in-group uniformity it produces, receives individual-selection support from kin selection also.

7.4 Cultural group selection in cultural evolution

BOYD/RICHERSON (1985) point out that it is this internal-group homogeneity and intergroup variability which set the stage for group selection ... [if] the traits ... provide a group-level advantage. This is a central concept for the Type 2 cultural evolution of group attributes, ideologies, organizational traditions, etc.

It is important to emphasize that this is an organized (or at least face-to-face) social group (rather than some nominal group, type, species, etc.). It is also important to emphasize that this is a selection of culturally transmitted attributes, not biological. (For biological evolution, this paper—at least tentatively—accepts the dogma of individual selection's dominance.) Groups (social organizations) can 'die' *when their biological individuals join other groups, or are converted to other ideologies and organizations.* ... The selective process could be pure emulation by unsuccessful groups of the successful. Or it could be the forcible imposition of the victor's culture upon the vanquished. Biological extinction of weak groups ... [or] fertility of successful ones, could also further the selective reproduction of ideologies, but ... are not essential. The 'group selection' posited is a selection of culturally transmitted beliefs, social-organizational structures, religious ideologies. It is not a 'group selection' of genes.

Where selection occurs at several organizational levels, the levels operate in part as competing organizations. *E.g.*, '[s]elfish DNA', reproducing itself without regard for whole animal functionality, is in rivalry with whole animal optimization ... So, too, individual biological person and social group are—to some degree—in competition. DAWKINS (1976) made famous the conception of "the selfish gene" (not referring to selfish DNA). In my judgment, he confused the unit of retention (the gene) with the unit of selection, and it is only the units of selection ... *that can have* purposes, including selfishness. Vis-a-vis individual interests, we need to keep in mind a 'selfish group' concept and recognize that effective selection at that level is selection for organizational and institutional self-perpetuation, at the expense of the individual if need be (and within limits).

7.4.1 Systematic selection pressures in the group selection of ideologies. John BOWKER (1973) ... argues that, if God existed as a part of the environment during the course of human evolution, then the human mind would be selectively attuned to that reality (as it may well be to quasi-EUCLIDEAN geometry and quasi-NEWTONIAN mechanics). I want to accept the general mode of the argument, but disagree if BOWKER sees it as justifying the specifically Christian origin myth, theology, and claims for revelation.

Considering the dozen independent evolutions among archaic human city-states, and the dozen times among the social insects that the syndrome has emerged of stored nonspoiling foodstuffs, full-time division of labor (including social roles that are well fed but gather no food), and professional soldiers in a ubiquitous role, I have argued (CAMPBELL 1965, 1974c, 1983) for the existence of common "laws of sociology" as part of the ecological niche of all twenty-four cases, insect and human.

It would be nice to be able to derive such laws from general principles, and then find them confirmed in the observations. But even in biology, discovery of the ecological niche often follows the discovery of the puzzling animal or plant form. Such confounding of theory and evidence is at least as great a problem in the present arena. However, conceptually, one might develop, on systems-analysis grounds, a model for human social behavior to optimize individual inclusive fitness in a central range of human environments ... If these analyses are appropriately general, then symptoms of universal conflicts should appear in all archaic city-states. We propose that the ubiquitous features appearing in all archaic moralizings are the symptoms of this conflict. Note that sociobiology presents a model of vertebrate social behavior optimizing individual inclusive fitness. Note also that the recurrent image of sinful, temptation-ridden human nature in worldwide moral systems is in remarkable agreement with the sociobiologist's picture.

With regard to the shared moralizings of archaic states, it seems to me plausible that any conformant transmission event that ended up containing part of the universal moral norm package would have some systematic tendency to be selected, however slight, and that the ubiquitous common set of moral norms is in general what is under selection pressure. Ideologies will be selected not for their own content, but incidental to their support of these norms. It seems that there are many specific cosmologies, origin myths, and pantheons that will support the moral norms ... explaining the great heterogeneity of such beliefs.

If we use universality as a symptom of recurrent selection pressures on content, then there seems to have been ... survival value in the belief in suprahuman invisible authority, gods, or a God ... Such beings, or one Supreme Being, are to be taken as real, as the invisible but real causes of visible physical effects, comparable to our beliefs in invisible causes such as gravity, magnetism, wind, and sunshine (i.e., 'natural' rather than 'supernatural). The BOYD and RICHERSON theory of adaptive conformist transmission requires this credulity, as do the group-level effects.

Explaining the ubiquity of invisible, transcendent authority is of course much more complex than the above paragraph explains ... SWANSON's brief, provocative 'The Birth of the Gods' (1960) is, in general, supportive of the latent-functionalism of this paper. But it offers a nonfunctional explanation for the ubiquity of the hierarchies of gods, and of one Supreme God. These pantheons, he argues, are metaphors for cultural-evolutionary truths at the organizational level for which there exists no 'literal' language. The local human political organization is used as a source of metaphor. The functional ubiquity lies at the political level ... The ubiquity of high and highest gods may be thus explained, without arguing the functionality of the theology per se.

Biological evolution has, presumably, selected our erogenous sense organs, our hedonistic sweets and bitters, pleasures and pains, in such a way as to increase genetic inclusive fitness ... It has no doubt also selected for ... long-term rational hedonic calculation, which weighs future rewards and punishments against present temptations. If cultural evolution ... can lead *credulous believers* to extend this hedonic calculus to include rewards and punishments in an afterlife (heaven, reincarnation), this supports obedience ... even in the face of death, and ... *sacrifice of* pleasures even in the absence of observers and sanction systems.

What I am arguing is functional augmentation, not necessary requisite ... *that would tend to* lead to more effective collective action. Hence, where the BOYD and RICHERSON belief-homogenization processes have produced such beliefs, the groups holding them may have functioned more effectively, their ideologies more imitated by other groups, etc.

Wasteful royal funerals may not be quite as ubiquitous in archaic city-states as I have claimed. But they are certainly too frequent and too independent to be explained by accidental belief–homogenization and nonfunctional diffusion ... *They would be more likely selected if they* had several latent advantages, so I need not seek a singular explanation ... [T]wo functions ... are plausibly related to overcoming the social-organizational problems created by the biological human nature produced by ... genetic competition among cooperators.

The explanatory principles central to this essay seem useful only for the archaic city-states ...

[A]lthough burials showing belief in an afterlife *and in ghosts and spirits* ... no doubt already existed in the simpler egalitarian predecessor societies ... other selective advantages must be found to explain these. But these precursors may have provided useful seeds for exaptation into city-state ideologies where they were selected by different functions.

These elaborately wasteful royal funerals usually had details testifying to the ruler's belief in an afterlife. They presumably ... also increased its credibility among the local population. Thus my first functional explanation is dependent upon the more obvious functionality of belief in after-death rewards and punishments.

The second possible function seems unrelated to the first, but not therefore incompatible. Covetous envy is biologically natural but *undermines* the division of labor, as is evidenced by the ubiquity of anti-envy moral preachments in division-of-labor societies. Envy is exacerbated by the unjust share of collective products which those occupying 'communication clearinghouse' roles are able to achieve for themselves and their offspring ... Conceptualizing rulers as divine, as a different order of being, and ceremonializing this difference in wasteful royal funerals may help reduce such envy.

I recognize these explanations to be weak. What I will persist in seeking are functional explanations. These seem to be most likely to be found in social-organizational functions, rather than in individual- person functions ... *or in surplus-disposal functions*. However, a seasonal need to keep a large labor force organized and occupied when its directly functional agricultural activities were not possible may provide one function for pyramid building. [CAMPBELL 1991, pp97–99, 102–112]

8. Ideology of the social system of scientific validity

In this section I am going to concede, to constructionist sociologists of science and others, that science has a great deal in common with social systems that generate and perpetuate superstitious beliefs, and much more than its explicit, anti-traditional norms would imply. However, I argue that these norms—including commitment to 'facts that speak for themselves' —should continue to be preached, however hypocritically, because they provide the opportunity for 'the way the world is' to co-select scientific beliefs via experiments and demonstrations, and thereby to enhance their competence of reference. This competence of reference, or validity, is necessarily hypothetical for the many reasons given in section 5. I am saying that if we assume, without justification, that scientific beliefs have greater validity than those of other social systems, then that difference can be explained by scientists' partial adherence to the norms that originated in the ideology of the scientific revolution.

The discussion that follows is borrowed from CAMPBELL *(1986a pp108–135 and 1988a pp513–522 and 1979a pp192–198 and 1988a pp498–503. See also* CAMPBELL *1986b.)*

8.1 The anti-tribal norms of science

[In 4.3.2 above, and in a non- excerpted case study (CAMPBELL 1979a and 1988a pp493–497), I have testified] to my conviction that scientific communities must meet the tribal requirements, the social structural requirements, of group cohesion and perpetuation. Many of the younger descriptive epistemologists, sociologists, and historians of science, supported by anthropological readings, have gone beyond this to the further conclusion that science is not different than other social superstition preservation systems. Instead, I want to assert differences as well as shared features: Among belief-preserving mutual admiration societies, all of which share this common human tribalism, science has different specific values, myths, rituals, and commandments. These differences are related to what I presume to be science's superiority in improving the validity of the model of the physical world which it carries.

My ontological nihilist friends do not deny that some of science's norms are different, but instead assert that these different norms are hypocritically preached—since, after all, the community of science does not live up to them, but instead behaves like other tribes. In addition, such critics point out that one cannot prove that the tribe of science is better than other tribes at sustaining a valid model of the physical world, so that I am working on a pseudo-problem. In deference to the presumptive nature of descriptive epistemology, I will confess that both the problems I work on and the solutions I offer are presumptive.

Focusing (as I feel we critical realists should) on the high quality epistemological-relativist challenges accompanying this ontological nihilism, the minimum we can do is generate a presumptive model of social knowing which could produce increased validity in beliefs about nature if the world were as we assume it to be ...

The charge of hypocrisy on the part of scientists is a charge that I concede in advance if it asserts only a descriptive inconsistency between what scientists do and the values they inculcate. But I do vigorously reject an implied conclusion that therefore science should stop inculcating these values and instead openly acknowledge its conformity to the tribal prejudices shared with the other superstition maintenance systems. Instead, I regard these special scientific norms as precious, and would sooner recommend their inculcation with increased vigor than recommend their abandonment.

All self-perpetuating belief communities are tradition-ridden, viewing current events through the spectacles of their pasts ... *But whereas most belief communities* locate truth in a long-past revelation or ... locate the ideals of life in some past heroic period, ... science's norms go explicitly counter to this, idealizing truth as lying in the future and decrying tradition as a burden and source of error ... Do these antitraditional norms ... not offer some advantage, however slight, to innovators ... *and* make the sciences less tradition-ridden than other tribal groups?

The organizational requirements of group continuity and career attractiveness give administrators and leaders power ... beyond what their declining competence and increasing rigidity merit. Such geriarchical and authoritarian biases scientific communities share with all other tribes. *Thus* off-the-record advice which young recruits to a thriving scientific laboratory receive indeed will usually be much like that received by an army recruit: "You'll find that if you want to get ahead in this lab, you'd better go along with the old man's ideas. He just doesn't know how to take suggestions or criticism." Yet, military communities and churches have explicit ideological support for this practice, while science's ideology explicitly decries it ...

In all social communities, narcissistic people with competitive egocentric pride are a problem. Cooperative people who defer to the majority, who get along and go along with others, and who hold the team together, get preferential treatment even if they are less competent. This is true of scientific communities too, contrary to scientific norms that encourage vigorous internal criticism even if feelings are hurt ... Yet, scientific communities no doubt differ somewhat from other belief tribes in the rewards given competent arrogance.

No cult, sect, or other belief community can isolate itself from the larger society. Science is influenced by the external social system in many ways counter to optimizing scientific truth. Thus, the status systems of the larger society, based on political and economic power and social class, contaminate the internal status system of science. Given equal ability, it helps a young scientist ... to be well-connected in the extrascientific real world ... *to have good manners, conventional social views, and to come from a high-prestige university.* All such contamination violates important norms of science which hold that the contribution to scientific truth should be the only determinant of status within science. Should this norm be given up as hypocritical? Or has it in fact some effect, making science less subject to this contamination than it would otherwise be? ...

These values of science I want to keep alive and available for use in the arguments that are made in the course of institutional decision–making. These values will, I believe, occasionally make a difference— a difference in favor of truth. Exposes demonstrating that science violates these values can go two ways: In shocked disapproval, we can try to advocate them more effectively. As a sociologist of science, I approve of this naive moralistic reaction and am thus sympathetic to the institution-preserving motives lying behind the outraged reactions KUHN and FEYERABEND have evoked ... The exposes can also have the opposite effect, in a call to give up the hypocrisy by ceasing to affirm these values. This I do vigorously oppose.

But I do not want to exaggerate the effect of preached norms. Institutional arrangements that provide selfish incentives for norm– supporting behavior are more powerful. Honesty, for example, is an important norm for science as for all other self-perpetuating social groups. But the exceptional honesty of experimental physical scientists where science is concerned is probably not due to their superior indoctrination for honesty (though the sciences may recruit persons who have an exceptional desire for an occupation in which they can be honest). Rather, it is due to science's exceptional punishment of dishonesty and to the possibility of ... exposure ... *which* competitive replication of crucial experiments provides ... [R]epeated failure of others to be able to *replicate* a given experiment is cause for fear, shame, and anxiety ... Fields lacking the possibility or practice of competitive replication thus lack an important social system feature supporting honesty ...

This brings us to the important scientific belief in 'facts', 'the hard facts', 'facts that speak for themselves'. This is such a pervasive normative belief-complex in the practice of science that I feel it must have great positive social system value, contributing to the objectivity and validity of science. Yet, descriptive epistemological analyses have been particularly debunking of this value. It is not only that scientists often fail to 'face up to the facts', as this norm says they should, but, perhaps more important, that the hard factuality of the facts disappears on closes examination ... Laboratory facts are only facts for those who share presumptions and background assumptions ... Disconfirming meter readings are regularly explained away as equipment failure or mistaken auxiliary assumptions. NEWTON, MENDEL, and DALTON are said to have doctored their data to unacceptable degrees in order to make the evidence for their theories more dramatically persuasive. In their cases the theories were right and replications approximately confirmed them, even if not again so elegantly. But such overediting processes must often go wrong and create pseudo-discoveries.

In spite of the theory-ladeness and noisiness of unedited experimental evidence, it does provide a major source of discipline in science. Thus, though in some sense literally untrue, the ideology of 'stubborn facts that speak for themselves, independently of any scientist's whim' seems to me an extremely important norm to preserve, and one that has a functional truth. (Though I have not taken time to explicate this, evolutionary epistemology leads to giving up the notion of literal truth while still holding onto the goal of truth [see CAMPBELL 1974a, and 1975b p1120]).

8.2 Experiments as divination rituals

In a final extension of the tribal model to science, I would like to argue that certain of its oracle rituals and magical divination ceremonies could contribute to the validity of scientific beliefs, even if these rituals were adhered to superstitiously and rationalized on inconsistent grounds. My

ing the outcome, thus providing an uncontaminated channel through which the supernatural powers can speak if they will ... These *and similar* divination rituals were used when *visiting* the well-known hunting sites had ... *yielded no game and* justified tedious explorations into ... regions that would otherwise have gone unexplored. They had the further social role of blaming no one group member for the frustrations of such exploration ... They also ... *rendered* the hunters' behavior unpredictable ... a *[subtle]* strategic advantage explicated in the theory of games of VON NEUMAN/MORGENSTERN (1944). None of these adaptive wisdoms is explicit in the beliefs that accompany the divination ceremonies. Instead, there is a quite incompatible rationale of supernatural beings that are potentially helpful but perverse and undependably placatable. The wisdom of the custom is hidden in its manifest justification.

In contrast to these, there is a second type of traditional ritual oracle so designed as to provide supernatural authority for the human wisdom of shaman or priest. In ancient Egypt some of the hidden voice tubes and mechanisms for getting statues to move indicate a priestly sophistication about the deception they practised. Perhaps the oracle at Delphi was also managed this way. But for many more, such as those described by MOORE and AUBERT, the procedures are, on the contrary, designed to keep the shaman's wisdom from determining the answer, and are performed by devout shamans sincerely dedicated to providing a channel through which the supernatural can speak instead of oneself. (Many more are mixed...)

It would be characteristic of the exciting and provoking new sociology and history of science, which takes as a duty the working hypothesis that science is no truer than other forms of tribal magic, to interpret the scientist's laboratory experiment as just another divination ritual (BARNES 1974). As perspective-expanding exercise, I believe this would be worth exploring in considerable detail. But I already know enough to insist that the experiment is a ritual of the first type, meticulously designed to put questions to 'Nature

The brilliant historians and theorists of science of recent years have convinced me that the galvanometer reading is not at all the 'solid fact that speaks for itself' we once imagined it to be. Instead, it turns out to be highly equivocal, interpretable only at the cost of many unprovable and revisable assumptions. Yet, the laboratory scientist's phenomenology is not altogether wrong: these stubborn laboratory facts are not speaking in the experimenter's own voice. Within the degrees of freedom the apparatus allows, they are out of the control of one's own hopes and wishes. [CAMPBELL 1979a pp192–198 and 1988a pp498–503]

8.3 Disputatious communities of 'truth' seekers

The title of this section denotes one sociological feature of scientific belief exchanges. I use it to introduce my version of the ideology of the scientific revolution. However, I have yet to integrate it with the history of the scientific revolution or with HABERMAS's concept of an ideal speech community, with which it probably has considerable communality (HABERMAS 1970a, 1970b, MCCARTHY 1973).

The ideology of science was and is explicitly anti-authoritarian, anti-traditional, anti-revelational and individualistic. Truth is yet to be revealed. Old beliefs are to be doubted until they have been reconfirmed by the methods of the new science. Persuasion is to be limited to egalitarian means, potentially accessible to all ... The community of scientists is to stay together in focused disputation, attending to each other's arguments and illustrations, mutually monitoring and keeping each other honest until some working consensus emerges ... [T]he ideology explicitly rejects the normal social tendency to split up into like-minded groups on specific scientific beliefs, but at the same time it requires a like-mindedness on the social norms of the shared inquiry. Sociologically, this is a difficult ideology to put into practice. MERTON (1973) has described the requirement as 'organized skepticism' ... *yet social* settings in which organized skepticism can be approximated are rare and unstable. Nonetheless, it may be regarded as a viable sociological thesis about a system of belief change that might improve beliefs about the physical world (including the not-directly-observable physical world) were such to exist ...

In terms of my model of variation, selection, and retention science puts greatest emphasis on the first two to the neglect of the third ... (e.g., CAMPBELL 1974a). To so stress variation and selection and neglect retention in the official ideology would be adaptive only if, at a particular historical period, retention were grossly overemphasized in the general cultural ideology and practice. At the time of the scientific revolution, retention had gotten entirely out of hand insofar as beliefs about unobservable physical processes and competence in negotiating with the invisible physical world were concerned. An anti-traditional counteremphasis was adaptive at that time ... With such plausible apologies for *certain* aspects *of early science* that in the seventeenth century did not need underscoring, I believe we should seriously consider the ideology of the scientific revolution as a useful, albeit contingent, thesis in an epistemologically relevant sociology of science.

From my perspective, the ideology and norms of science are not clearly distinguished from 'scientific method'. Scientific method is also to be seen as a product of cultural-evolutionary process on the part of a bounded belief-transmitting subsociety of many generations. With FEYERABEND (1975), I would agree that new criteria of method are developed as new choices provide new arguments. Like religious commandments, the 'rules' may be mutually incompatible in the sense that if any one were to be followed with complete loyalty, it would interfere with compliance with the others. Each is ... interpretable only against a background of prior and current norms and practices. While historically both methods and ideology have fed on concrete successes, it is convenient to regard the ideology and practice of cooperative truth-seeking as coming first and method as a rationalized summary of successful usage in the community. This is more obviously so for the hermeneutic methods, but I believe it also holds for MILL's canons of cause and FISHER's analysis of variance. [CAMPBELL 1986a pp108–135 and 1988a pp513–522]

The discussion that follows stresses the importance of 'demonstrations' in science. It should be expanded to emphasize that scientific 'demonstrations' are to involve objects and events that one can see with ones own eyes (and touch, hear, smell, or taste), which are of the same order as those ostensionables employed in teaching an infant language, i.e., basic to interpersonal shared competence of reference, *as discussed in section 6 above.*

8.4 Visual demonstration and assent to facts.

It is my belief that some version of the fact/theory distinction is essential to sociology of science (cf. STEG-

MÜLLER 1976, on KUHN and the theory-ladenness of facts). Here 'facts' are understood as shared, visually-supported beliefs introduced as demonstrations in a persuasive process. The terms 'demonstration' and 'experiment' have much the same referent in early physics, chemistry and biology. The early persuasive role of 'demonstrations' for both lay and scientific audiences was, I assert, more important than 'experiments' as a social grounding of scientific belief, even though current experimental science seldom relies upon them.

'Facts' were originally theoretical inferences supported by processes built into the nervous system by both natural selection and learning—ontological assumptions built into neural information-processing channels. At a more mature stage, facts may be micro- theories no longer controversial within the scientific community. [CAMPBELL 1986, p121 and 1988 p515]

8.5 Referential ecology

Successful science rests upon assent to agreed-upon facts (many implicit) as a background for demonstrating new facts. In the social sciences, difficulty in achieving agreement upon facts is certainly a major source of its failure to achieve genuinely scientific status.

Much of this is a referential-ecology predicament that is unavoidable, since it is intrinsic to social science topics. Some of the problem, however, is a larger societal ecology-of-support issue. Were social scientists to limit their work to topics on which factual assent could be readily achieved, it might be that society would not support their research, nor students attend lectures limited to their findings, because of their banality ... But some of the fact-assent problems might be alleviated through structural and ideological changes in the social science community, in publication practices, reward systems, funding priorities, and the like. [CAMPBELL 1986 p121 and 1988, pp515–516]

8.6 Replicability of fact

A crucial part of the egalitarian, antiauthoritarian ideology of the seventeenth-century 'new science' was the ideal that each member of the scientific community could replicate a demonstration for himself. *Thus, alchemy's* ideology of secrecy was an anathema to scientific exchange ... *and* COLLINS' sociological studies showing the absence of replication in current physics (1975, 1981a, 1981b) are to be taken very seriously ... [T] early study of electricity will show hundreds of Leyden jars, Voltaic piles, and static electricity wheels generating sparks in hundreds of labs ... *few of these experiments were published, but all figured importantly in the* social persuasion process. A healthy community of truth seekers can flourish where such replication is possible. It becomes precarious where it is not.

Replications can be attempted, but too frequently fail, in the most exciting fringes of experimental social psychology (a referential– ecology problem, at least in part). Perhaps as a result, social psychology has the custom (atypical of successful science) of trusting a single dramatic study in going on to the next experiment without explicit or implicit replication. The effort and cost of replications within a social system that regards them as unpublishable and of low prestige contribute to their absence. The lack of replications ... *in social psychology means that the discipline* lacks the social control that exists in those sciences in which replication is feasible and regularly succeeds.

In general, the absence of the norms and practices of replication ... are major problems for the social sciences. From the standpoint of an epistemologically relevant sociology of science, this absence makes it theoretically predictable that the social disciplines will make little progress. Can planned changes in science policy ... change the situation? [CAMPBELL 1986a pp121–123 and 1988 p516]

Hermeneutic approaches to history and other fields in the humanities create disputatious communities quarreling over the meanings of specific facts (including some that are theory-laden, i.e., resting on culturally-shared conceptions of human nature). The scrupulous mutual monitoring in such communities often generates a mistrust of theory as leading to disregard of facts. (Thus SPENGLER, TEGGART and TOYNBEE in history, and such early anthropologists as FRAZER, TYLOR, and WESTERMARK have been used as cautionary examples of the evils of theorizing).

8.7 The ecology of explanations and anticipations of facts

It is our ontological predicament that the events and stabilities we come to know lie at the intersection of innumerable forces, restraints, and causal processes, most of them unmapped at any given stage. This is true both of the biological evolution of sensing and predictive machinery and of culture or science. The survival value of perception and memory lies in those ecologies in which the highest order interactions of all of the variables are not significant, in which ceteris are approximately paribus. Similarly, the growth of science has required not only the accumulation of facts, but also the achievement of successful approximative theory relating facts to facts. This is most possible in those ecologies where powerful, oversimplified ceteris paribus laws can be invented ... to sustain the group's feeling of progress ...

Success in this regard must often be a matter of the referential ecology. Take, for example, the attitude-change research epitomized by dissonance theory in experimental social psychology. In most respects, the participants acted correctly in terms of my tentative sociology of successful science. The generally recognized collective fatigue and search for other models (documented by GERGEN 1982) was in my judgment due to referential ecol-

Insofar as the disputatious communities of scholars dispute about theory, they are likely to ... enter into arguments in which mutual persuasion becomes possible only where there exist degrees of freedom sufficient to make possible cross-validation ... Such degrees of freedom can come only from attempting generalization across instances, persons, provinces, times, or the like. Of course, we do not want to *reject* the 'one-shot case study' (as in CAMPBELL/STANLEY 1963) ... [I]nstead we want to join my later recognition [CAMPBELL 1975a] of the degrees of freedom available in a case study that come from ability to check multiple implications of a theory in that setting. Yet until we have successful cases of mutual persuasion converging upon an agreed-upon theory achieved by such methods, we should continue to regard the problem as serious.

munity of scholars. It can only be resolved by discretionary judgments of plausibility. Nonetheless, scientific communities often achieve working consensus, often against the interests of the established and powerful. The central mode of argument involved is closer to the hermeneutic methods than to some idealizations of scientific certainty. The strategy of trusting most of the fabric of corrigible benefits while you challenge and revise a few (the 1 to 99 doubt/trust ratio) is central ... *and* ramification extinction of rival hypotheses is ubiquitous ... It was thus (as MOYER 1979 has so well described) that the British community of astronomers and physicists changed between 1915 and 1925 from overwhelming faith in NEWTONIAN gravitational theory to complete acceptance of general relativity ... Something similar is described by CLAUSNER/SHIMONY (1978) for ten years of testing of BELL's theorem. Each particular experiment was flawed, but through ramification extinction of the alternative explanations, these flaws permitted even the hidden- variable theorists ... *to be* for the most part convinced... [CAMPBELL 1986a pp125–126 and 1988a pp518–519]

8.10 Insulation of the social system of science from that of the larger society

Thomas KUHN says of the physical sciences that:

[T]here are no other professional communities in which individual creative work is so exclusively addressed to and evaluated by other members of the profession. The most esoteric of poets or the most abstract of theologians is far more concerned than the scientist with lay approbation of his creative work, though he may be even less concerned with approbation in general. That difference proves consequential. Just because he is working only for an audience of colleagues, an audience that shares his own values and beliefs, the scientist can take a single set of standards for granted. He need not worry about what some other group or school will think and can therefore dispose of one problem and get on to the next more quickly than those who work for a more heterodox group. Even more important, the insulation of the scientific community from society permits the individual scientist to concentrate his attention upon problems that he has good reason to believe he will be able to solve. Unlike the engineer, and many doctors, and most theologians, the scientist need not choose problems because they urgently need solution and without regard for the tools available to solve them. In this respect, also, the contrast between natural scientists and many social scientists proves instructive. 'The latter often tend, as the former almost never do, to defend their choice of a research problem'—e.g., the effects of racial discrimination or the causes of the business cycle' chiefly in terms of the social importance of achieving a solution'. Which group would one then expect to solve problems at a more rapid rate? (KUHN 1970 p 164, emphasis added)

Here I argue that the dependency of scientists on support from the larger society makes it probable "that science works best on beliefs about which powerful economic, political, and religious authorities are indifferent (RAVETZ 1971) ... However ... visual demonstrations vary greatly in clarity and persuasiveness ... *and if they are* convincing enough, demonstrations can even overcome political relevance." (CAMPBELL 1986a, p127 and 1988a, pp519–520)

For example, 17th century Chinese emperors replaced their well-entrenched, politically important court astronomers with socially powerless Italian astronomers who could successfully predict lunar and solar eclipses (SIVIN 1980, pp25–26). "But the combination of perceptually unclear demonstrations with highly important political beliefs, such as is found in the applied social sciences, is ... unlikely to produce belief change in the direction of increased competence of reference." (CAMPBELL 1986a, p127 and 1988a, p520)

8.11 Critical mass and success experiences

Here I suggest that there are certain sociological requirements for maintenance of communities of truth-seekers. Most important are (1) a critical mass and (2) the appearance of progress (collective success experiences).

Fad phenomena in the natural sciences' choice of problems characteristically generate enthusiasm and intense informal communications. In turn, these ... supply the critical mass, mutual monitoring, cross- validation and sometimes sustained perceptions of progress. *However,* [w]ithout perceived breakthrough into further problem areas, interest dwindles and experimental energy becomes available for new perceptions of hot problems and promising techniques. Certainly there are many areas of the social sciences that lack critical mass at the mutual monitoring level ... Sociology-of-science studies might well ask scholars in various fields about a specific publication "If you are wrong about this, who will notice? Who will try to check by replicating? Who will publish (or formally publicize) their disagreement? Who will let you know privately about a successful or unsuccessful replication or other data that support or weaken your position?" These studies should focus both on the level of fact and on the level of theory ... Without having such studies available, let me nonetheless hazard some opinions.

There exist mutually monitoring communities in religious hermeneutics for such issues as who borrowed from whom in the New Testament gospels and the proper translation of crucial verses in the Old and New Testaments. There have in the past and may still exist such communities in HOMERIC scholarship. It might thus be reasonable for practitioners to claim that cumulative progress had been made.

In anthropological ethnography, no such communities exist, LEWIS (1951), BENNETT (1946), HOLMES (1957), FREEMAN (1983), FIRTH (1983), and BRADY (1983) notwithstanding. Instead, one seeks a region as yet unstudied on one's special topic and, once successfully published, may jealously try to prevent others from allegedly needless replication of one's work ... [T]he genuine collective interest in describing all vanishing cultures before they disappear provides justification. (Mutually monitoring communication networks seem better realized in anthropological linguistics and in ... archaeology.)

Contrast the ethnomethodology movement within sociology with the behavior-modification movement within psychology. Both are proud, self-conscious deviations from the mainstream of their disciplines. Both have social-solidarity needs that press for the inhibition of internal divisiveness, and hence for the inhibition of mutual criticism, in order to shore up the intramovement morale against the neglect or attacks of the dominant paradigm. The behavior modifiers withdraw to their own journals and within them pursue vigorous internal disputation. The ethnomethodologists, on the contrary, produce isolated illustrations of their method and theory but, owing to their lack of numbers and embattled status, never disagree with each other about matters of fact. Insofar as they disagree about matters of theory, they tend toward further sectarianism and reduced communication rather than mutual monitoring. The same can be said of ... those who identify their method and theory with Verstehen, hermeneutics, critical-emancipatory theory, dialectical materialism, phenomenology, and symbolic interactionism (except for its atypical 'labeling theory'). Mutual monitoring fails, not only within these movements but ... also in their roles vis-a-vis their parent disciplines. These are all movements of great actual or potential value for mainstream social science as penetrating criticisms and suggestions for revision. But this effect can only be achieved if both the radical critics and the mainstream scholars remain within a common communication network and listen seriously to each other. [CAMPBELL 1986a, pp127–129 and 1988, pp520–521]

8.12 Observations on belief selection by sociologists of scientific knowledge

I end this paper with yet another plea to the social constructionists in sociology of science. I do so as part of my long-term effort to persuade them to entertain the possibility, however remote it might appear to them, that the real world might play a role in selecting the beliefs we come to have about it. If so, it follows that scientific discoveries may not be made up entirely out of the whole cloth. Once more I quote what I have said elsewhere:

Let us return now to the sociologists of scientific knowledge *(SSK)*. Their case studies of the social construction of scientific consensuses report on the proposal and abandonment of many hypotheses about process and instrumentation. They offer microprocess studies of belief selection appropriate to selectionist accounts. Thinking of LATOUR/WOOLGAR (1979), KNORR-CETINA (1981), and PICKERING (1984), for example, we probably have a hundred or so instances. These could be tentatively classified as to the type of selection involved. Some of these episodes will be classified as purely social: An idea is not followed up because it would offend the laboratory head, or because it would give comfort to a rival research group, or because of lack of funding.

Other ideas are reported as being tried out and found not to 'work'. In such episodes it is possible that the way the world is participates in belief selection, even though there are vast negotiable resources to ... *deploy to settle* why it did not work, that is, QUINE–DU-

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HEM cop-outs. More borderline cases are those in which an idea is rejected because of reasons why it will not work, or because of rumors that a trusted researcher is known to have tried it and failed. If those reasons and rumors themselves have been coselected by the way the world is, then (still more indirectly) coselection by referent may have been involved. LATOUR's (1987) chapter "Laboratories" also provides several examples of beliefs being abandoned by the resistance encountered in laboratory practice.

Reflexively, we should of course use interest theory (BARNES 1977, 1983) to critique such a data set. The scientists being reported on shared an ideology probably leading them to exaggerate the role of the referent in belief selection, even in their apparently unguarded gossip and shoptalk. However, the SSK authors of these works might have had an opposite bias, in favor of the dramatic and more publishable message of 'social construction out of whole cloth'. It is conceivable to me that they did not start out with this bias, that it was for them a discovery in the research process. (If we add EDGE/MULKAY 1976, that initial bias was probably lacking.) Interview testimony on this point would abet our coherence-based discretionary judgment. However, since they were all pressed by the need to write brief, vivid, and publishable books and articles, in which only one-tenth of their notes could be used, a postresearch selection bias in the 'constructed out of whole cloth' direction is possible. Access to the full field notes might provide a less biasedly selected set of episodes. On the other hand, we might expect an opposite bias on the part of science ethnographers such as HULL (1988b) and GALISON (1987). While I have not done the systematic rereading and coding, I am sure that these properly venerated texts (LATOUR/WOOLGAR 1979, KNORR–CETINA 1981, PICKERING 1984) make it plausible that the purported referents of belief are participating to some extent in belief selection. [CAMPBELL 1993, pp103–104]

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