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Impressum

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Werner Callebaut/Rupert Riedl

Preface

This issue of *Evolution and Cognition* is dedicated entirely to the memory of Donald Thomas CAMPBELL (1916–1996), one of the most remarkable scientists and humanists of his generation. (CAMPBELL never pursued scientific aims for their own sake, but was always motivated by deeper humanistic concerns. See, e.g., his Presidential Address to the American Psychological Association [CAMPBELL 1975].) CAMPBELL ended his scholarly career as University Professor at Lehigh University, Bethlehem, Pennsylvania, after having taught at the University of Chicago (1950–53), Northwestern University (1953–79), and Syracuse University (1979–82).

A social psychologist by training (he was a student of Edward TOLMAN and Egon BRUNSWIK at Berkeley), CAMPBELL's uniquely creative blending of methodological rigor and daring theoretical speculation about the individual and collective human mind (cf. De MEY, this volume) was to make him highly influential in many other fields as well. Many psychologists and social scientists know him best for his contributions to methodology, which include measurement issues (e.g., convergent and discriminant validation by the multitrait-multimethod matrix), experimental design (most notably, the method of *quasi-experimentation* [COOK/CAMPBELL 1979]), and evaluation research.

Biologists and philosophers tend to regard him as the major American evolutionary epistemologist of our century. Scholars in the booming but divided new field of STS or Science and Technology Studies remember him most as the grand old man who sympathized with *all* of the warring factions: philosophers speaking in the name of rationality and correspondence truth, relativistic and constructivist sociologists doing their thing, cognitive scientists painfully turning naturalistic, *verstehende* historians…His personal contribution to this ongoing debate, which in his inimitable manner he dubbed ERISS, for "Epistemologically Relevant Internalist Sociology of Science" (cf. his later "sociology of scientific validity"), was an attempt to reidentify the "*something* special about science, which gives it some greater legitimate claim to objectivity than other social systems" in the light of post-KUHNIAN relativism, or, as he called it, "'cult-solipsism', portraying sciences as self-deceiving social systems incapable of distinguishing truth from tribal myth" (CAMPBELL 1979; cf. KIM, this issue). ERISS convinced many STS scholars that a dialogue with 'the enemy' remained possible and could even be profitable (cf. RESTIVO in press). CAMPBELL's *descriptive epistemology*, as developed in his William JAMES Lectures at Harvard University in 1977, relies on physiological, psychological, and sociological approaches to deal with epistemological issues, for it includes the (fallible) theory of how the processes studied by these approaches "could produce truth or useful approximations to it". Isn't it somewhat ironic or paradoxical that CAMPBELL-the-naturalistic-psychologist increasingly stood for a *respectful* conversation with the epistemological tradition (including its skepticist offshoot, which he deeply admired) at a time when many a philosopher (including such influential evolutionary epistemologists as Ronald GIERE or David HULL), following the QUINEAN suggestion that psychology and sociology should *replace* epistemology, surrendered to "nihilistic naturalism" (Susan HAACK), a position which CAMPBELL himself more graciously described as "epistemologically vacuous"?

CAMPBELL for all his modesty (CAMPBELL 1981) could do all this, and more, because he was a true encyclopedist in Otto NEURATH's sense, one of a handful of genuine generalists of the post-World War II generation who transformed cybernetics into General Systems Theory (cf. HEYLIGHEN, this volume). Contrary to a common perception, CAMPBELL's EE is not neo-DARWINIAN in the dominant, reductionist sense of the orthodoxy promoted by, say, George C. WILLIAMS or DAWKINSIAN sociobiology: Not only did he consider a *hierarchy* of levels on which DARWINIAN mechanisms operate (which allowed him to highlight the phenomenon of "vicarious selection"); he also and increasingly took into account internal ("structural") selection factors and mechanisms.

The concept of a *nested hierarchy of vicarious selection processes*, which was in part inspired by systems-theoretical considerations (cf. CAMPBELL 1973), may well be CAMPBELL's single most important contribution to EE and evolutionary theory more generally (see, e.g., CONSTANT, DE MEY, and KIM, this issue). RICHERSON/BOYD (in press) pinpoint its meaning and relevance against the background of the inadequacy of DARWIN's theory of inheritance in The *Descent of Man* (1871). As a consistent naturalist, philosophically speaking (see, e.g., CAMPBELL 1988), DARWIN took great care to suggest *continuity* between humans and other animals, which he secured by attaching great importance to imitation and other forms of the inheritance of acquired variation:

> His theory allowed him to account for the essential similarity of all living humans, while accounting for the vast diversity in human behavior, by attributing the underlying similarities to conservative traits and by attributing variation between human groups mostly to labile traits strongly influenced by inherited habits. DARWIN's distinction between more conservative and more labile traits did the same work for him that the modern gene-culture distinction does for us. (RICHERSON/BOYD in press)

One problem with DARWIN's view is that while human *culture* operates as a 'use inheritance' system, such a ('LAMARCKIAN') system is rudimentary or lacking in animals without culture. DARWIN thus left a major gap in evolutionary theory that contemporary workers, and CAMPBELL in particular, tried to fill. RICHERSON and BOYD argue that the idea of *vicarious selection* is crucial in this respect:

> How could it be that animals could acquire adaptive variations that anticipate what natural selection would favor? DARWIN certainly believed that humans and animals could acquire adaptive variations, but gave no clue as to how this neat trick could itself evolve. CAMPBELL noted that if variations are acquired other than randomly, it must be because organisms have the capacity to use some sort of rules to modify behaviors or structures. Indeed, plants and animals have many such capacities, the most familiar of which is ordinary trial and error learning, itself an example of blind variation and selective retention in CAMPBELL's view. Natural selection has arranged sensation of reward and punishment so that learning normally favors behaviors that are useful to survive and reproduce. CAMPBELL termed such processes "vicarious selection"; the rules of learning select behaviors on behalf of natural selection. (RICHERSON/BOYD in press)

CAMPBELL's insistence on the "blindness" of variation (and retention, but that is a different matter) has generated quite some misunderstanding. Most importantly perhaps, is that this has prevented people in the Artificial Intelligence community from appreciating the useful complementarity of CAMPBELL's views and those of that other great contemporary polymath, Herbert SIMON, on levels ontology, adaptive ("satisficing") behavior, and reduction of complexity in general (THAGARD 1988 is a case in point). Yet as CONSTANT (this issue) points out, the point CAMPBELL wanted to drive home may be formulated as a simple reductio ad absurdum.

The U.S. and Austrian varieties of Evolutionary Epistemology (EE) turn out, then, to share some of their intellectual ancestry, for BRUNSWIK (who coined the term "ratiomorphic apparatus") influenced Konrad Lorenz and Karl POPPER as well, and a major feature of the 'Vienna–Altenberg' conception of evolution is its systems-theoretical orientation (cf. LUHMANN 1997, 1, p431). In a way it is sad that the rapprochement which the publication of CAMPBELL's paper, "From Evolutionary Epistemology Via Selection Theory to a Sociology of Scientific Validity" in this issue consolidates is a posthumous one, but such is the vagary of biological and intellectual existence.

A first draft of the comprehensive paper *Evolution and Cognition* is honored to publish here was prepared by Don CAMPBELL for an invited talk at one of the seminars on "The Evolution of Knowledge and Invention" of John ZIMAN's Epistemology Group, on 24 May 1995. He was so kind to prepare a second version for this journal early in 1996. The additional time this most conscientious and generous of men had kindly required to complete work on the paper was not granted him by Life as he suddenly died on 6 May 1996. We are grateful to Barbara FRANKEL, his widow and a sociologist herself, and to his favorite student, Celia HEYES, a psychologist, who kindly accepted to edit the manuscript so as to give it a shape they and we hope CAMPBELL would have found suitable. More detailed information about the editorial process, and a summary of the paper—which CAMPBELL characterized as "a personalized retrospective history of ideas"—are provided in HEYES' and FRANKEL's Editors' Introduction.

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A number of CAMPBELL's younger colleagues and friends have accepted to write brief contributions highlighting one or another aspect of CAMPBELL's EE *sensu lato*, as developed in this ultimate paper of his. Their disciplinary backgrounds are as diverse as social anthropology (Linnda CAPORAEL), cognitive science (Andy CLARK, Marc DE MEY), complexity theory (Francis HEYLIGHEN), economics (Elias KHALIL), philosophy of science (W. D. CHRISTENSEN and Cliff HOOKER, Aharon KANTOROVICH, Erhard OESER, Massimo STANZIONE, Franz WUKETITS), sociology and social policy (Steve FULLER), history of technology (Ed CONSTANT), and experimental psychology (Henry PLOTKIN). These statements speak for themselves; there is no need to summarize them here. Although several of these comments are quite critical of one or another aspect of CAMPBELL's stance, they all reflect a deep sympathy for the man and his work. We like to think of this not as a closed project, but as ongoing work. CAMPBELL liked to challenge us, and many of the fascinating ideas in his rich paper could not be dealt with in sufficient depth at this occasion. But there will be other occasions: special conferences, books, journal issues, etc., devoted to his oeuvre, which will continue to inspire many of us.

Don CAMPBELL was such a kind and brilliant man.

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Donald T. Campbell

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 counterproductive 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:

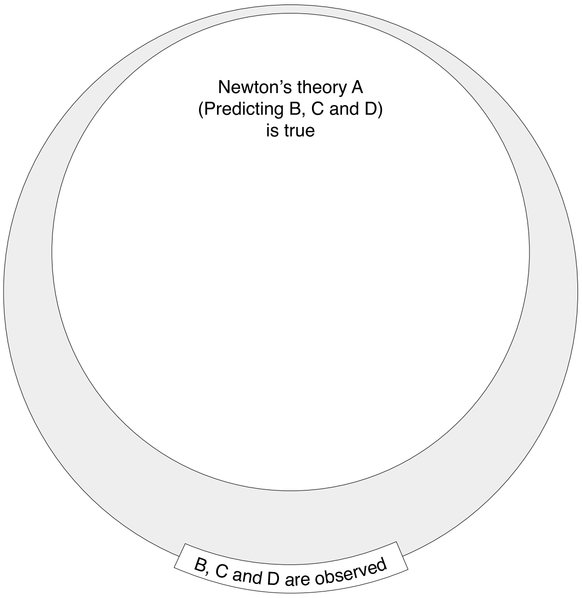

Figure 1: NEWTON's gravitational theory as an 'incomplete induction'.

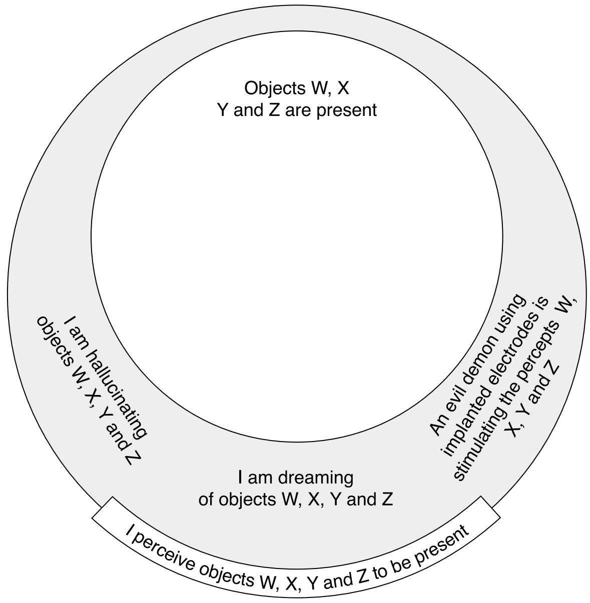

Figure 2: Visual perception as 'incomplete induction' (with apologies to DESCARTES).

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'.

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.

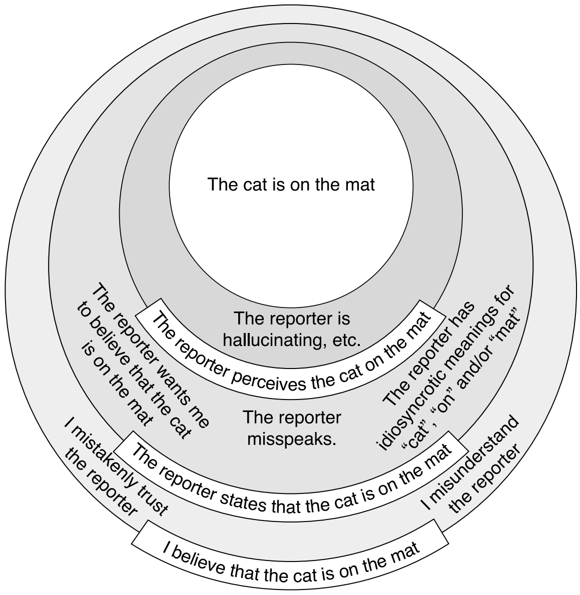

Figure 3: Verbally transmitted belief as 'incomplete induction'.

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 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, partic-

ularly 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).

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 infor-

> mation, 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 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, longi-

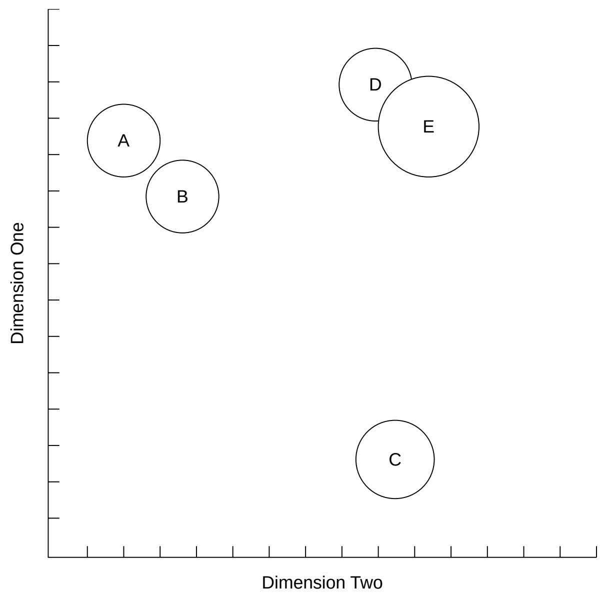

Figure 4: Some 'natural kinds'.

tude, 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 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 reidentification 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 mutation 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, the common objects of the 'real', non-linguistic world to some extent edit any language. For the basic vocabulary that makes language learnable, language is limited to the talkable-about. A common preverbal evolutionary background strongly biases human beings in the direction of finding 'stable' 'entities' highly talkable-about. (CAMPBELL 1973, pp1050–51).
>
> The shared perceptual reifications thus become socially 'foundational' in achieving linguistic transfer of valid beliefs from person to person. Even at the later non-ostensive stages of language learning – when new words are learned by describing their meaning in terms of other words already known, the term 'definition' is not quite right. Instead, verbally produced exemplars are used, and once again the learner has to guess at the meaning, the guesses being disciplined but not entailed by the exemplars. This is particularly clear for new terms in scientific theories. In the process, the degree of shared reference may be severely reduced, and also the selective role of the real nonlinguistic referents. (Expanded coherence networks may cure this degeneration to some degree.) [CAMPBELL/PALLER 1989, pp238–240]

(In future presentations I wish to give more stress to the proposition that the ostensionable objects of mutual 'demonstration' in the ideology of the scientific revolution are of the same class that is employable in teaching language to an infant.)

This model of language learning presumes that prelinguistic reifications of objects precedes language learning, and is indeed shared by dogs, pigeons, apes, and pre-linguistic infants. There is a rival model, the SAPIR, WHORF, CASSIRER, BORGES, and COLLINS (1985) model, in which language comes first and determines the reification of perceptual entities. From this model, 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-sacrificial altruism. Cultural inventions *such as* supernatural sin-recorders and after-death rewards and punishments have curbed these selfish tendencies by *means of* belief-systems involving invisible causes of visible effects, preempting this domain of belief. *When they are adaptive, these prescientific beliefs may have some* metaphorical competence-of-reference, but science posits much more specific and nearly literal invisible causes. To do so, it has had to declare independence from traditional religious belief. The ideology of science is incompatible with traditional ideology even in its Euro-Mediterranean homeland as well as in other areas of the world.

Elsewhere I have put forward a lengthy argument concerning the role of certain religious beliefs in the social orders of ancient civilizations (CAMPBELL 1991, pp97–99, 102–112). In it I suggest that, at least in the case of archaic city-states, supernatural or "transcendent" religious beliefs functioned to promote intragroup cooperation by encouraging group members to believe that cooperative behavior would yield personal gain after death. Groups holding such beliefs were favored by group-level cultural selection. Referring to BOYD/RICHERSON (1985), I argue that this process is likely to be rare and fragile compared with both individual-level cultural selection, and cultural transmission in which selection plays no significant part.

7.1 Some puzzles in the sociology of the beliefs of archaic civilizations

Ancient 'high' civilizations such as those in Egypt, Sumer, and Babylon, along the Yangtze and Yellow Rivers of China, along the Ganges and Indus Rivers of the Indian subcontinent, in ancient Peru along desert rivers and in the highlands, and in pre-Columbian Mexico show certain interesting uniformities. All were organized around full-time division of labor, with priests, rulers, skilled 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, l990a). 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 imagery for this comes from papers on traditional divination ceremonies by MOORE (1957) and AUBERT (1959). Caribou hunters roast a shoulder blade on the fire and use the cracks resulting to choose the direction the hunting party should take. The ceremony … has many features designed to prevent human hunches from influencing 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 Herself' in such a way that neither questioners nor their colleagues nor their superiors can affect the answer. The supplicants set up the altar, pray reverently for the outcome they want, but do not control the outcome … A narrow window has been provided through which 'Nature' can speak, free from the scientist's control.

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 antitraditional 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. STEGMÜLLER 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 mis-

trust 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 ecology. Major effects were replicated far beyond chance, but unevenly and with no emergence of dependable laws as to why and when. Some critics I respect believe that the waning interest was due to faintheartedness, boredom, and careerist topic changes and that we now have for dissonance theory dependable interaction laws quite sufficient to support a major scientific edifice. But these loyalists do not agree on which dependable laws result. [CAMPBELL 1986, pp123–124 and 1988 p517]

8.8 Degrees of freedom and historical/contextual uniqueness

Classical physics and chemistry sought laws which were universal and timeless—indeed, time-reversible—so that provenance and history became irrelevant.

Whether or not physics can ever quite achieve this, the biological and social sciences are much less likely to be able to do so and if they did would be shifting to a different set of scientific puzzles than they have at present undertaken (CRONBACH 1986). This difference in referential ecology rightly motivates social science methodologists and metatheorists to be wary of borrowing uncritically a theory of science based solely on the physical sciences. The methods of the humanities appropriately become attractive alternatives to be considered. But it is intrinsic to understanding … that the principles invoked are to some extent transtemporal and transcontextual, however conditionally hedged. Complete situational and historical uniqueness eliminates not only theory but also any grounds for 'understanding' or shared 'meaning'.

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.

This issue is not at all specific to the humanistic methodologies. It exists in extreme degree for quantitative economics … *which focuses* on national economies, where only short runs of thirty or forty 'comparable' years are available, rather than on the economics of, for example, neighborhood laundries, where degrees of freedom for testing hypotheses on not-yet-used samples abound. [CAMPBELL 1986, pp124–125 and 1988a pp517–518]

8.9 Eliminating rival hypotheses through discretionary ramification-extinction

The social crux of science, so it seems to me, is the ability to render rival hypotheses implausible. This focus, exemplified in the quasi-experimental tradition (CAMPBELL/STANLEY 1963, COOK/CAMPBELL 1979), seems to me now more central than experimental isolation or experimental controls … *all of which are devices for* rendering implausible the rival hypotheses that … disputative colleagues have effectively raised. Randomized assignment to treatments does not prove the hypothesis under test, nor disprove rival hypotheses, but instead renders many rival hypotheses improbable … The narrowing of experimental comparisons by specialized control groups clearly illustrates the role of the current contents of the disputatious dialogue … *Contrasting examples here are the abandonment of sham-operation controls to rule out surgical shock as the cause of changes in behavior following experimental lobotomies, versus retention of the double-blind strategy for drug trials.*

The QUINE-DUHEM equivocality of any experimental result is a very real problem for any community 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:

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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–DUHEM 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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Linnda R. Caporael

Vehicles of Knowledge: Artifacts and Social Groups

Abstract

CAMPBELL identified artifacts and human social organization as "vehicles of knowledge." This comment further develops these two concepts. Artifacts may represent transformations of nature, making it accessible to the human mind. The social organization of science is an effective vehicle of knowledge because it bears close parallels to "primitive" social organization. What differentiates science from other forms of belief-maintaining organizations is the rejection of supernatural cause.

Prolegomena

When Werner CALLEBAUT asked me to write a response to Don CAMPBELL's last paper, I accepted without hesitation. Don and his student, Marilynn BREWER, were major forces shaping my interest in evolution when I was a graduate student. Then, evolution was as popular in psychology as original sin. CAMPBELL (1975) was the only person who could talk about both—at the same time! To my surprise, writing this paper has been impossibly difficult, and not just because of Don's unexpected passing. I was reminded of when, some years ago, he asked me why I admired his work when he was a sociobiologist. I said, lamely, that he wasn't like other sociobiologists. (Not long ago, Barbara FRANKEL told me that he described himself as a "left-wing sociobiologist.") But the fact of the matter was that CAMPBELL, "militantly ambivalent" as he could be, worked in at least two, to my mind, contradictory, evolutionary modes. One was reductionistic and recalled the early days of sociobiology; the other was systems oriented and contributed to an expanded evolutionary perspective. This writing was arduous because CAMPBELL's writings on human nature were drawn primarily from his sociobiological mode. Yet his influence on my own efforts at a naturalistic epistemology were shaped by his views on evolutionary processes. CAMPBELL had created two distinct standpoints for a psychologically informed epistemology, but he stood on one, while I stood on the other.

CAMPBELL explictly advocated nature-nurture dualism. He saw a profound conflict between a biologically-based, selfish human nature and the "unnatural" (yet necessary) demands of culture for cooperation. He believed that human nature could be distinct from culture, and that this nature, fundamentally self-interested, was "in the genes". A well-known slogan of CAMPBELL's was "cooperation between genetic competitors". Culture held selfish human nature in check through moral preaching, the exercise of power, and the appeal to costs and benefits in a future afterlife. CAMPBELL rejected "biological" group selection, demanding that any adequate theory would have to provide a genetic explanation for self-sacrificial heroism. Yet, one could hardly argue that CAMPBELL favored a simple-minded genecentrism.

Many of CAMPBELL's insights about evolutionary processes, as well as his empirical work, lead to a different perspective, one that most influenced my own views about selection and design. CAMPBELL was one of the first to discuss "downward causation" (CAMPBELL 1974) in hierarchically organized systems. He recognized the group level of organization as a selector on individuals (CAMPBELL 1982). He knew well that humans were social animals with strong proclivities to forming groups. He acknowledged individual and group selection could go in the same direction. He recognized multiple levels of social organization and the potential for synergy and conflict between levels (HEYLIGHEN/CAMPBELL 1996). In 1965, CAMPBELL rejected self-interest as the engine for psychological explanation, argued for an "innate ambivalence" between social and selfish motives, and hypothesized advantages of cognitive efficiency for groups. Yet, just a few years later, he recanted this vision of human psychology because it implied biological group selection (CAMPBELL 1972).

Although I shared CAMPBELL's ambition for an embodied, material and descriptive epistemology and have started down paths he cleared, there have been significant detours. My comments recall CAMPBELL's earlier vision of psychology, reject the dualism that he later defended, and are sympathetic to group selection (CAPORAEL 1995; CAPORAEL in press.). The differences between CAMPBELL's emphases and my own inform the background of this comment on the physical and social vehicles of knowledge. These are subjects where CAMPBELL emphasizes antagonism between the medium and knowledge, and where I will suggest more focus on coordination.

Vehicles of Knowledge

In his target paper, CAMPBELL described two kinds of vehicles, artifactual and social. According to CAMPBELL, the "vehicular substance that carries knowledge is unavoidably alien to the referents of knowledge." Any map, for example, is unavoidably alien to the shoreline it represents. Similarly, the tribal structure of science is unavoidably alien—of a different substance—to the knowledge it represents. Consequently, the representation of knowledge is limited and biased by the structure of the representational medium, which may be artifacts or social organizations. If a vehicle of knowledge is too flexible, it would be unable to hold together the picture it contains. At the same time, keeping the vehicle intact conflicts with its function to represent knowledge. Ultimately there is a tension or conflict between the referents of knowledge and the media of its representation. While not denying that conflict exists, I suspect that it is the coordination between parts that enables vehicles of knowledge—artifactual and social—to be selected for the functions they perform.

In the case of artifacts, I will suggest that they transform nature to make it more usable. A similar claim could be made for social vehicles, but my comment on that subject will be directed to expanding on CAMPBELL's tribal model of science. In a highly speculative mood, I suggest that the tribal model of science bears a resemblance to a tribal model of knowledge among hunter-gatherers. I conclude by suggesting that the demarcation between science and other belief systems might be simply the result of rejecting anthropomorphic modes of explanation.

Artifactual Vehicles

CAMPBELL's prototypical example for an artifactual vehicle was that of a mosaic mural of a street scene. The thickness of mortar, the size of the glass pieces, the available colors and the restriction to two dimensions limited the validity of the picture represented. A photograph of the street scene would still be limited (although not as much as the mosaic), but it would have higher validity. The street scene could be carved in ice or painted in icing on a cake. These media would be both lower in validity and too flexible for most knowledge purposes: the ice melts and the cake is eaten. CAMPBELL's examples of physical vehicles, such as mosaics and plaster-of-paris casts suggest he conceived of structural vehicles, and of validity, as *copies* of knowledge. However, his inclusion of maps suggests a somewhat different story. As the joke about the farmers who will not allow the cartographers to unroll their 1:1 map over the fields indicates, a "true" copy of nature is not necessarily useful at all.

In many cases, representing knowledge in an artifact demands a radical transformation—not a copy—of the knowledge. In these "cognitive artifacts," the transformation reduces the computational complexity of problems. Knowledge is re-represented in an alien medium *in order to change* the cognitive tasks that must be undertaken. As an example, consider finding the square root of the product of 54 and 23. For most people, paper-and-pencil must be used to keep track of the intermediate calculations. The calculations can be considerably simplified if they are re-represented in an old-fashioned slide rule. One lines up two scales, shifts the hairline indicator, and reads out the number. The paper-and-pencil calculation has been transformed into a perceptual judgment task. The slide rule does not require that the user even know how to take the square root of a number. All the user needs to know is when "square root" is relevant and what scale to use when the square root is needed. Given the user knows the scale, the possibilities for errors are greatly reduced by using a slide rule compared to solving the problem on the back of an envelope or even with a calculator.

The slide rule is not perspective-free, context-free, or interest-free. It is the embodiment of historical contingencies, trial-and-error learning, and tinkering, which has accumulated over a period of time greater than that of the lives of the individuals who contributed to its invention and development. An abacus can be used to do the same task, but it, too, is the result of diverse historical and cultural contingencies. "Same task", however, needs to be qualified. From a computational perspective, the task is the same: whether one uses the different artifacts of a slide rule or abacus, the result of our computation will be the same (barring error). However, from a naturalistic perspective, the calculating tasks are *not* the same. Abacus and slide-rule demand dramatically different background assumptions, motoric behavior and perceptual judgments. Inheriting the cultural framework enables the use of the device.

We may also wish to designate a category of "nature's artifacts." Here there is neither copying nor direct transformation of materials. The artifact is jointly a product of culture and nature, a cultural-cognitive system. For example, Micronesians, using traditional methods of navigation, routinely embarked on voyages that took them out of sight of land for several days (HUTCHINS 1995). Yet the navigator was able to accurately indicate his point of departure, destination, and islands over the horizon and out of sight. (This following description of Micronesian navigation is highly oversimplified, but serves to make the point.) The navigator uses a *sideral compass*, based on the night sky. The compass is based on how humans experience the relationship between earth and the heavens. Relative to each other, stars are fixed in their positions. But as the earth turns, the constellations of stars appear to move in a path across the sky, rising at the eastern horizon and setting at the western horizon. A star path is a set of constellations that follow each other in a fixed arc across the sky. Position can be determined by noticing where one is relative to the place on the horizon where a constellation is rising (or setting). The navigator imagines a reference island, below the horizon and beneath a specific star. In the Micronesian conceptual framework, the canoe is still, and the invisible island moves from a place in front of the canoe to one behind the canoe as the navigator approaches his destination. By observing the location of the star above the invisible island, the navigator is able to fix his location and others, as well as determine how long the trip will take.

This "artifact" is different from the slide rule. The stars do not have the accumulated knowledge of humans. They are not transformed to make the knowledge available. In one sense, the knowledge *is* the stars themselves, their patterned regularity. In another sense, the knowledge is the cultural traditions that make the stars usable, just as cultural traditions make abacus and slide rule usable. The sideral compass also puts into high relief a central problem for any naturalistic epistemology. From the Western perspective, Micronesian sailing is based on invalid knowledge, a conception of the heavens revolving around the earth. Yet it worked for thousands of years for the purposes of Micronesians. In Micronesian and Western navigation, techniques are selected by the rigorous test of making a landfall. Today Micronesian sailors use Western navigation methods, not because of their greater validity, but because the Western sailing tools greatly extend the available ranges for sailing. More knowledge can be stored and distributed more widely when it is mediated by physical vehicles (e.g., charts and computational devices) than by oral traditions and face-to-face interaction alone.

Social Vehicles

CAMPBELL is well-known for his characterization of the "tribal structure of science." By tribal structure, CAMPBELL alludes to the practices, beliefs, ideologies and in-group loyalties that make science a self-perpetuating community. The community itself, in his view, is the embodiment or "vehicle" for scientific knowledge. Like physical vehicles, however, social vehicles are also limited and biased. They are unavoidably alien to the knowledge they attempt to represent and transfer between generations. Many of the processes required to sustain science as a social system, that is, as a vehicle or embodiment of knowledge, are antagonistic to competence of reference. I suggest that it may be the case that the processes required to sustain science are also *necessary* to whatever competence of reference exists.

Humans are obligately interdependent, unable to survive and reproduce reliably in the absence of a group. This interdependency, as CAMPBELL (1983) recognized, would result in a feedforward process selecting for psychological mechanisms that would enhance group solidarity. For CAMPBELL, the advantages of solidarity were primarily in the realm of predator protection and group conflict. I part company with CAMPBELL at this point. In his view, groups could be described as a homogeneous aggregate of innately selfish individuals with self-preserving social sentiments. In my view, groups have come to serve as an interface between individuals and the habitat (CAPORAEL/DAWES/ORBELL/VAN DE KRAGT 1989)

CAMPBELL's description of a "tribal" structure to science, rather than, say, a market structure or bureaucratic structure, was especially insightful. There are parallels between the typical organization of nomadic hunter-gatherer groups (JARVENPA/BRUMBACH 1988) and of scientists (HULL 1988). Both have a nested hierarchy composed of work groups (research groups), bands (primary face-to-face groups), and seasonal macrobands (tribes). In both instances—in science and among hunter-gatherers—group-size numbers at these levels are fairly constant; groups of about 3-5 individuals (hunters or lab research group), 30-50 individuals (bands, conceptual demes, or primary social groups), and 300-500 individuals (tribes of hunter-gatherers (BIRDSELL 1972) and of scientists (HULL 1988)).

In some sense, both hunter-gatherers and scientists work in the (hypothetically) "real world." Hunter-gatherers must find resources for survival; scientists search for explanations of phenomena in the world. Scientists and hunter-gatherers have a variety of techniques directed, with considerable effort, to reducing the unpredictability and uncertainty of their experience. In both instances, isolated individuals have reduced viability. Individuals must be part of a subgroup with sufficient "critical mass" to persist. Isolated hunter-gatherers are vulnerable to many dangers; isolated scientists are not very productive. Similar to hunter-gatherers groups, which reproduce by fission, scientists do not usually begin a new research group as social isolates; they must bring some of the old group members with them or attract new recruits.

Similarities of function also exist in the subgroup configuration structure of scientists and hunter-gatherers. Learning to fashion specialized tools, in stone or other materials, or to use specialized laboratory techniques or "primitive" food preparation techniques is a hands-on, situated activity (like golf or tennis). Such learning requires feedback from materials, equipment, and an experienced mentor if novices are to develop finely-tuned sensory-motor microcoordination. (Remember, too, that the equipment has knowledge embedded in it.) Frequently this learning takes place in dyadic interactions, not unlike the parent-child interaction through which children learn shared reference in language by ostension.

For both lab groups and small hunter-gather groups ambiguity is a major attribute of the task environment. Small groups of 4 or 5 people distribute cognition; they share tasks such as perception, classification, inference, memory, and contextually-cued responses when they interpret data or interact with uncertain features in the habitat. Hunters must detect an animal equipped with natural camouflage and distinguish predator from prey. Gatherers must distinguish toxic from nourishing plants. Scientists in the lab must detect the signal in their data from the noise.

Primary social groups function as "staging communities" (JARVENPA 1993), which serve as "general processing and maintenance centers" for resources and information retrieved from the smaller dispersed groups; they are also loci of shared group identity. In hunter-gatherer workgroups, food from the hunt is often brought back to the domestic base and shared in the band. Among scientists, research results and their interpretation are brought back to the primary social group in the form of workshops and small intimate conferences.

For both scientists and hunter-gatherers, seasonal macroband meetings (or yearly conventions) are important for the exchange of myths, gossip, and information about more distant areas and groups. Macrobands meetings are also arenas for competitive games as well as the affirmation of common worldviews, the maintenance of languages (hunter-gatherers) and specialized professional terms (scientists), and the exchange of people (mates, new PhDs, or disgruntled members).

The Demarcation Problem

The parallels between hunter-gatherers and scientists seem to suggest no genuine demarcation between science and other belief systems. CAMPBELL, of course, believed it was impossible to justify any differentiation, so he recommended a course of believing (without justification) that science produced more valid knowledge than other belief systems. If we make that concession, then science's higher validity can be explained by the anti-tribal norms of science. I am rather skeptical about this last point. Many areas of inquiry have anti-tribal norms (cf. BARZUN/GRAF, 1992) and the case could be made that many of science's notable achievements may require tribal norms (GRIFFITH/MULLINS, 1972). "Tribally normative" background assumptions are essential for stabilizing the interpretation of data. Nor am I convinced that the late appearance of science can be explained, as CAMPBELL says, by the co-opting of belief systems by religion for solidarity purposes.

Many factors contributed to the ideological shift in the 17th century origins of mod-

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ern science. A relatively small one from the modernist perspective may have had quite substantial results. An important element, which we almost take for granted, was the exclusion of anthropomorphic appeals in explanations. If humans are social creatures, with minds geared to interacting with other, unpredictable social creatures, it would not be surprising if we "defaulted" to anthropomorphic explanations (CAPORAEL 1986; CAPORAEL /HEYES 1996). Even modern humans casually attribute human characteristics to artifacts and nature. The modern automobile owner coaxes the car to start. The computer user begs her machine to produce the desired output instead of pages of zeros. If humans typically default to anthropomorphic explanations when they have no alternative account for a phenomena, then excluding anthropomorphism (gods, ghostly beings, elves, etc.) from the domain of possible explanation is a truly surprising, novel, and radical concept that forces the search for a material explanation.

If we make the concession CAMPBELL urges, there are probably many factors entering into an explanation for the higher validity of scientific knowledge. Central to these, as CAMPBELL points out, is that phenomena in the world are co-selectors of beliefs. This is the case for scientists and hunter-gatherers, but would not be so for other communities, say, the world of the American Kennel Club, where beliefs are organized around aesthetic considerations. Self-organization is likely to be another factor, which is crucial for natural phenomena to operate as a co-selector, and is characteristic of most scientific and hunter-gatherer communities. State organization of science can result in episodes such as Lysenkoism, or simply lower rates of discovery.

It would be a mistake, I think, to continue to preach the anti-tribal norms of science knowing them to be hypocritical. Hypocritical preaching invites scientists to cloak themselves in ignorance (and arrogance). For the less arrogant, the extent to which the traditional norms of science are compromised by culture and context comes as a rude, possibly disillusioning shock to the young men and women who go into science. It would be far better to develop new norms and expectations more suited to a post-modern world on the edge of environmental and social crisis. Such norms would recommend humility in the face of what science does not know; an appreciation for the cultural and social context of scientific work; a critical stance within a scientific community and a generosity of spirit across the boundaries of knowledge communities—norms, in fact, embodied in the work and life of Donald T. CAMPBELL.

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W. D. Christensen/C. A. Hooker

Selection Theory, Organization and the Development of Knowledge

A Selective Critique of D. T. Campbell's Evolutionary Epistemology[1]

Abstract

Donald CAMPBELL has long advocated a naturalist epistemology based on a general selection theory, with a major qualification (made in the present paper) restricting the scope of knowledge to vicarious adaptive processes. But being a vicariant is problematic because it involves an unexplained epistemic relation. We argue that this relation is to be explicated organizationally in terms of the regulation of behavior and internal state by the vicariant, but that CAMPBELL 's selectionist account can give no satisfactory account of it because it is opaque to organization. We argue the need for a general theoretic framework for understanding organized systems, and the need to understand epistemic capacity in terms of systems with high-order globally constrained regulatory organization.

I. Campbell's theory of knowledge

Donald CAMPBELL develops a naturalist theory of epistemology based on a general selection theory which posits three major components to selectionist processes (section 1): the production of variants (V), selection across these variants (S), and the retention of those variants which survive the selection process (R). The major intuition underlying CAMPBELL's theory, which he describes as the "1960 dogma" (section 2) is that knowledge emerges through, and only through, VSR adaptation to the environment. In this respect the epistemic sophistication of a scientist compared with his virus-type ancestor simply reflects the cumulative inductive achievements of millennia of DARWINIAN evolution (blind-VSR).

CAMPBELL combines this selectionism with a theory of vicariance: there are intrasystemic processes which function to shortcut evolution by vicariously anticipating characteristic environmental conditions (so either avoiding or encouraging them); the knowledge of these processes is derived from the VSR processes which produced them, they themselves operate according to VSR principles, and the scope of knowledge is restricted to them (section 2.2). This leaves us with the following account of knowledge. State I of system S is knowledge of environmental condition C iff: (a) I is correlated with C because of a selective history, and (b) S treats I as a vicariant for C.

This last qualification is important because it rules out many problematic cases and potential counterexamples. For example, contemporary viruses have apparently been no less structured by VSR processes than has the scientist, but they are no more epistemic than their ancient ancestors, possessing no organized vicariant structure. This is in sharp contrast with the lineage of the scientist, which is marked by a general tendency towards increasing epistemic sophistication realized through organized neural structure supporting complexly nested conditional response structures whose genetically inheritable component arose, we shall suppose with CAMPBELL, through VSR processes. Thus we have VSR processes for the refinement and conditionalizing of the products of earlier VSR processes, i. e., second and higher order VSR processes. (It is at least plausible that the enhanced anticipatory capacity these processes conferred created for the lineage a selective gradient favoring refined and deepened vicariance processes.) So both the contemporary virus and the scientist have experienced first-order VSR structuring, but only the scientist has experienced second- and higher-ordered VSR structuring, and it is this high-order VSR structuring which is constitutive of the scientist's massively enhanced epistemic capacity. Since a VSR process is common to both cases, VSR structuring in itself is at best a necessary feature of epistemic systems, and the virus case makes it look a relatively uninformative requirement.

The distinction between a system feature which is merely adaptive and a feature which is a vicariant plays a critical role in CAMPBELL's theory, but it is itself somewhat problematic inasmuch as being a vicariant involves an unexplained epistemic relation. That S uses I as a vicariant for C means roughly that S treats I as an indicator of C; however, what the indication relation comes to is unclear. It cannot simply mean that I is correlated with C because any system feature which is an adaptation carries some degree of mutual information with the environment (though see CHRISTENSEN/COLLIER/HOOKER 1997 section V.2), and consequently would meet the criterion irrespective of its epistemic significance. The least question begging, and from our perspective most interesting, interpretation of the indication relation we can make is that I indicates C for S if I plays a role in regulating S's processes so that they are appropriate to C. This means not only that I has been selected for correlation with C, but that I is embedded within a more general regulatory context in which S has a teleological-control relationship with C.[2]

However, although a regulatory interpretation of vicariance saves CAMPBELL's VSR epistemology from being question begging, his relative neglect of it raises several further problems for him. (1) CAMPBELL repeatedly emphasizes that 'competence of reference' is the hallmark of a vicariant state qua knowledge, where he seems to understand this condition in a purely correspondence manner. However, competent process regulation involves much more than competence of reference in this sense, which is useless in itself unless the system can make appropriate use of the vicariant. The extent to which I is a vicariant of C for S depends as much on the systemic context of I—in particular on the way C interacts with S and S's goals—as it does on I's referential competence. Indeed, treating competence of reference as the primary epistemic relation is misleading: to function as a regulatory vicariant for C, I must modulate S's processes in ways that are appropriate to S's interaction with C, but I needs only limited competence of reference to do this, it simply needs to control certain aspects of S's interaction with C. The mosquito flying up the CO_2 gradient does not need reference to it even as an extended stream, it only needs to control its flight direction by response to local CO_2 gradient. CAMPBELL's theory is somewhat vague concerning the epistemic relations involved in vicariance, because it almost entirely neglects the contextual regulatory aspects which must be understood to properly account for vicariance.[3] (2) Selection theory is fundamentally unable to address regulatory issues. Despite widespread attempts to the contrary, it is simply the wrong kind of theory. Consequently, whatever the role of VSR processes may be in knowledge development, contra CAMPBELL's central thesis they do not represent a sufficient condition for its occurrence (cf. also HOOKER 1995). We now turn to elaborating this point.

II. Why selection theory is unsuited as a foundation for natural epistemology

The basic problem with selection theory as a theory of natural epistemology is that it is radically impoverished as an explanatory model of organized systems, but understanding organization is central to epistemology. Selection theory washes out or glosses over almost all of the actual dynamics of the system being modeled, being focused just on frequencies of outcomes. This is not a problem if the primary objective is just to understand the dynamics of population statistics. However, we contend that many of the dynamical features which selection theory washes out play important roles in understanding the epistemicly significant features of complex organized systems.

Standard evolutionary theory takes as its primary unit the population, and measures changes in gene frequency within the population over time. Much of the usefulness of selectionist models derives from being able to choose conditions such that the adaptiveness, or mutual information, of a particular gene is determined by its relative change in frequency as compared with other genes within the population. This occurs when the population is treated as a decomposable, near-to-equilibrium system. These linearizing conditions are often only implicit in the models, but only under such conditions can outcome distributions be treated as independent of dynamical path from their starting values (e.g. as in the HARDY-WEINBERG law). Moreover, these assumptions are central to the explanatory capacity of the theory: If the population is not decomposable into quasi-independent systems (organisms, genes), because it displays holistic dynamical constraints, then relative frequency measures may not be well defined. There are many systems of this kind, and plausibly organisms and social systems—the epistemic systems in question—are amongst them (see below). Further, even if the system is decomposable or nearly so but the population is not near-to-equilibrium with its environment, then relative change in gene frequency cannot be assumed to be a good measure of adaptiveness because, beside correlation with the environment, path-dependency effects (e.g. founder effects in migration) will also play a role in determining frequency distribution. Note that we are not claiming that there cannot be nonlinear population-genetic models in these latter cases, just that in these cases relative frequency distribution will not directly measure adaptiveness.

However, just noting the limitations in the applicability of selectionist models is only part of showing that they represent an unsuitable foundation for natural epistemology—more important are the reasons for these limitations. Although a selection model compares the relative frequency changes of stable traits over time, such a model provides no insight into the dynamical processes by which the frequencies evolve. Moreover, by only comparing intra-populational differences, the contribution to adaptiveness of the shared organization of the members of the population drops out of the picture—in effect, it is suppressed by the high level abstracted structure of selectionist models. However, it is precisely such dynamical and organizational information which we need if we are to be able to make distinctions between various sorts of adaptive processes, e.g. between gene-based change of fixed first-order traits to improve environmental fit and high-order modification of adaptable traits in organism-learned fit.

The problem cannot be rectified simply by extending selection theory to intra-organismic or social processes. Although any system with stable traits can have a black-box frequency counting model imposed on it and may sometimes be useful, the basic explanatory gap remains—there is no account of process organization. Organisms and social systems are marked by hierarchical, holistic organization where such modularity as occurs (cells, organs, institutions) is heavily constrained by global functional organization. These kinds of systems are not dynamically decomposable because: (a) their components are highly interdependent, and (b) their dynamics tends to amplify this holistic organization. Consequently they can only be adequately modeled by a theory which explicitly treats dynamical and organizational factors.

If we are to successfully model knowledge development as an adaptive process—the root intuition behind CAMPBELL's evolutionary epistemology—we must distinguish amongst different kinds of adaptive processes. As CAMPBELL is all too aware, features of social systems (such as religious ideas) may be adaptive without being epistemically reliable. On the other hand epistemic reliability does seem to be a strong feature (if not the only one) of the adaptiveness of scientific ideas. Clearly there must be some difference between the adaptive processes of the respective systems, but as we have seen, although selectionist models may in certain circumstances be sensitive to differences in adaptiveness, they are not sensitive to the underlying reasons for these differences. Consequently selection theory cannot distinguish between adaptive processes which are distinctively epistemic and processes which are adaptive for other reasons. A theory of vicariant processes may well be an important step towards understanding distinctively epistemic adaptive processes, but it cannot be developed from within selection theory.

III. Organization and the generation of knowledge

These problems result in CAMPBELL's chronic inability to discriminate in a principled way between science and other cultural systems, and in his misguided account of the constraints associated with embodiedness, the so-called 'vehicular' and 'co-selection' constraints on competence of reference. To deal with the latter issue first, organized systems characteristically face global constraints, and these constraints may function both in an inhibitory fashion, by ruling out certain otherwise available complexions of the system, and in a functionally amplifying fashion by making available certain capacities which the system would be otherwise unable to achieve. We refer to the latter as enabling constraints (see HERFEL 1997). For example, the cell membrane is a global enabling constraint with respect to intra-cellular biochemical organization because it provides a quasi-isolated environment within which critical parameters such as ionic concentrations, enzyme activity etc., may be maintained and organized so

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as to perform useful work. Without the cell membrane the energy gradients and physical organization necessary for work would relax, and complex biochemical functionality would be impossible. CAMPBELL, however, models all such constraints as limitations on epistemic capacity. For example, he characterizes 'vehicular' constraints in terms of resolution and coloration limitations on reference capacity (section 4.2.2). There is no hint that some constraints may play an enabling function.

Similarly, CAMPBELL struggles to distinguish science from other types of cultural systems, resulting in the recognition that science like all cultural systems faces 'tribal' constraints, together with the peculiar recommendation that science should hypocritically ignore these constraints in its rhetoric (section 8.1). The problem derives from the fact that VSR structuring and embodiment constraints are common to both scientific and non-scientific cultural systems—the differences will lie in organizational features of science which play an enabling role in knowledge development.

For example, both the immune system (IS) and central nervous system (CNS) have VSR functional characteristics (as well as origins), but they have very different organizational—and epistemic—characteristics. If the IS is properly characterized as epistemic (and we think that in some respects it is), it is a very low-order epistemic system whose 'object reification' capacity is virtually nonexistent. The CNS, on the other hand, is an 'object reifier' par excellence (though it is also much more than this), but not because it has 'more VSR' than the IS. Fundamentally, the CNS is capable of object reification because it has far greater control depth than the IS. If we consider the organization of a primate retina, to take just one instance, we may observe that there are approximately 100 million photoreceptors which detect light signals which synapse onto a mere 1 million axons in the optic nerve (CHURCHLAND/SEJNOWSKI 1992, p148). Connections amongst receptors are highly organized, showing both mutual inhibition and activation. In other words, the behavior of individual retinal neurons is heavily constrained by the local and global organization of the retina, and it is precisely these constraints (together with those of the visual cortex) which enable the general pattern recognition capacity of the primate visual system.[4] The CNS also has more VSR functional structuring than the IS, but this is parasitic upon its greater organizational depth, and it is the increased control depth in particular which is the critical enabling constraint which permits object reification.

The organization of science parallels the features of the CNS in many respects. The theory-ladenness of facts, far from being a competing constraint to objectivity, is evidence of the extremely high level of control depth in science. A single experiment in physics, e.g., may involve appeal to all or most of physics in its design and data interpretation, including the theory under test (HOOKER 1975). In this respect, theory ladenness is an enabling constraint for science which ought to be embraced (with due caution) rather than suppressed.

Conclusion

As a very rough approximation we believe that the characterization of the scientist in terms of high-order VSR structuring is on the right track. However, CAMPBELL's account of vicariance—the conferrer of epistemic content to the theory—is fundamentally deficient because vicariance is an organizational property which selection theory per se cannot illuminate. If we are to understand epistemic systems, and the role of VSR processes within these systems, we need a general theoretic framework for understanding organized systems. In particular, we need to understand systems with high-order globally constrained organization which has embedded parallel competitive features.

Notes

1 All unnamed section references are to CAMPBELL's paper in this volume. The conceptual background of this paper is indebted to the complex systems ideas developed over a number of years by the highly interactive research group at Newcastle (Australia) led by C. A. HOOKER and including J. D. COLLIER, W. E. HERFEL and W. D. CHRISTENSEN. Our ideas on the metaphysics of natural epistemology have also greatly benefited from discussions with M. H. BICKHARD.

2 BICKHARD (1993) presents an analysis of indication (and representation) which is of this general type (see also BICKHARD/TERVEEN (1995)). The details need not concern us, the important point for the present argument is the introduction of a regulatory context to analyze an epistemic relation. Part of the significance of this is a shift in focus from the upstream causes of a signal to the downstream modulatory effects of a signal. For a detailed theory of the epistemics-relevant metaphysics of complex organized systems see CHRISTENSEN/COLLIER/HOOKER (in preparation),

COLLIER/HOOKER (1997), and for a regulatory theory of fundamental epistemology see CHRISTENSEN/HOOKER (in preparation), and HOOKER (1995).

3 CAMPBELL does discuss some contextual issues under the banner of 'co-selection' which we shall discuss below, but this does not address the regulatory context of vicariance.

4 CAMPBELL does refer to the role of mutual interaction in equivocation reduction (section 6), but with respect to VSR epistemology this stands as an ad hoc insight without systematic basis.

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Andy J. Clark

Evolutionary Epistemology and the Scientific Method*

Abstract

What is the proper attitude of the evolutionary epistemologist towards science? Should he regard science as disclosing (or aiming to disclose) information concerning the way the world is in itself, independently of the species-specific needs, bias and cognitive orientation of the human life-form? Or should he conceive it as intrinsically limited and indelibly marked with the stamp of his own humanity? Either way there is a problem. If he adopts the first, objectivist, interpretation he faces the charge of hypocrisy; why does he not extend the results of his conjectures concerning cognition in other species to the enquiring animal, man? To make that extension, and to regard our scientific knowledge as biased and limited in ways analogous to those attributed to the lower animals, is, however, to breed a deeper discomfort. For if he adopts a species-specific, non-objectivist account of scientific knowledge then the status of the evolutionary conjecture itself is brought into question. For by what right does the evolutionary theorist then quantify over all evolved life-forms in formulating his general picture of the relation between cognition and reality?

1. An evolutionary epistemologist[1] extends to mind and knowledge that account of adaptive nature and purpose usually reserved for the explanation of gross physical characteristics; characteristics such as sharp teeth, long necks or acute hearing. This extension is hardly unwarranted. Obviously, neither sharp teeth nor sharp ears will contribute to the proliferation of the genes concerned if they are not combined with an appropriate control system (call it 'mind') translating input into survival-en-hancing action. Sight of prey or sound of predator must be suitably processed or interpreted to result in use of teeth or use of feet accordingly. Nature must be red not *just* in tooth and claw, but in instinct and desire also if the teeth and claws are to be put to good use.

On the plausible assumption that basic cognitive orientation or instinct is as adoptively strategic as gross bodily form the evolutionary epistemologist brings his selective paradigm to bear on the issue of the relationship between an animal's environment (as we recognize it) and its knowledge of that environment (as expressed in its observable behaviour). He is concerned to account for the form and content of that knowledge in terms of the process of random mutation, recombination and differential survival to reproduce; a process known familiarly as natural selection.

By imposing the selective paradigm onto the matter of basic cognitive orientation the evolutionary epistemologist can explain, at a stroke, both startling achievements and apparently perverse inaptitudes. The location of food sources is understandably high on the honey-bee's 'list of priorities' so we can make good (albeit post facto) sense of the evolutionary development of the amazingly intricate dance routines and mutual interpretative capacities used to communicate information concerning the distance and direction at which food is to be found. Bee-dance, we conjecture, is choreographed by the selective process itself. Attention to the details of this process helps to explain not only what various animals *can* do, but also what they cannot do, cognitively speaking. The watershrew, for example, is distinguished in the literature (LORENZ 1941/1962 trans. p32) mainly in virtue of its incapacity to find a shortcut (literally) to save its life. For having once laid down a route to B from A via C, it can never progress to a direct route A → B even if the trip to C involves a long, looping detour. This cognitive 'deficiency' is explicable too (in a way more precisely detailed below) once we consider that it lives and reproduces perfectly successfully in default of any capacity to sustain the complex internal representations of its, environment necessary to determine a short-cut.

A theoretical model adequate to the explanation of both cognitive achievements and inaptitudes is available to the evolutionary epistemologist. For the process of natural selection is characterised by a pervasive dual aspect. On one hand, there is the pressure to survive; this brings the species into mental and physical contact, over evolutionary time, with the environment in which it is competing. On the other hand, there are the limitations inherent in the rather minimal goal (viz. survival and reproduction) ascribed to the selective process and responsible for the 'contact with reality' which it can support. For a process geared solely to survival may be expected to yield limbs and cognitive strategies alike which are geared to the special needs of a given being in a given niche. Further slack with any notion of absolute veridicality enters with the observation that the whole process is *blind* where by this is meant that the options among which selection takes place are random mutants; beings whose particular mutated nature stands in no causal relation to the nature of the environment in which they are to be 'tested'. And, finally, the selective process is to be deemed sensitive to the non-optimising demands of cost-efficiency. If a neat approximation is both effective and economical it will be selected for against a more detailed but energy-intensive rival.[3] Considerations of cost-efficiency, species-bias and random generation may thus explain the various inaptitudes of lower animals in the same theoretical context which explains their successes.

The selective model thus briefly sketched, we may now ask after the appropriate attitude of the evolutionary epistemologist towards his own (human) conception of reality. Is he simply to extend the account of basic cognitive capacities (instinct, degree and nature of internal representation of the environment, input-action transformation strategies) in lower animals to man himself? Or is science supposed somehow to be exempt from any repercussions of the constraints on basic cognitive content discussed in 1 above?

In his dialings with the knowledge of other species the evolutionary theorist stands committed to what Donald CAMPBELL has called 'an organism-environment dualism' (CAMPBELL 1974, p449). This dualism is both ontological and epistemological. It is ontological insofar as the world must be conceived as physically independent of mind; it is a mind-producing, not a mind-produced, system. And it is epistemological insofar as it involves a dualism of knowledge and reality; how the universe is (in itself, as it were) may always transcend how a given type of being knows it to be. One way to put the present question is to ask whether science can intelligibly aspire to transcend this latter basic epistemological dualism and leave behind the random, species-biased and cost conscious character of the process which made the brains which *do* science. To suppose it cannot is -to call into question the objective validity of the evolutionary model (itself a branch of the biological sciences) itself and hence to intimate that the naturalised angle on knowledge is a self-undermining one. To suppose it can is to invite the accusation of ignoring our own epistemic situation as human beings[3], for whence the phylogenetic discontinuity between the knowledge of the lower animals and that attained by man? Is there safe water between the Scylla of cognitive imperialism and the Charybdis of cognitive relativism? And if there is, can it be consistently occupied by an evolutionary epistemologist? To reach a decision we must take a closer look at the scientific method itself.

2. On any plausible view of the scientific method the conduct of science involves the performance of some range of cognitive operations upon some choice of data. The cognitive operations may include some kind of ranking of competing explanatory hypotheses in terms of the delicate balance between simplicity and comprehensiveness and utility (SOBER calls this the trade-off between simplicity and fruitfulness). And the data may be in the form of direct observational reports or it may be more or less impregnated with theory depending perhaps on the extent to which previously accepted hypotheses are assumed in the construction of the evidence upon which some current claim is to be based. But no matter how intricate the web of intervening theory it will remain at root true to say that science takes observational reports as inputs, generates explanatory laws and models as outputs, and decides amongst competing laws and models by employing considerations of simplicity and fruitfulness. The explanatory laws and models which get accepted are therefore subject to two sources of constraint. The first source lies with the observed phenomena themselves; a theory must be true to the facts. The second source lies with the structure of human (and perhaps all) rationality; a good theory should be simple, beautiful, comprehensive, suggestive and so forth.

It would be natural to think that if some species-based epistemological infection were to afflict science, the site of the infection would be with this second source of constraint. SOBER, indeed, has suggested that it might be unwarranted to believe that any cognising being must share the kind of human rationality evinced by reference to the 'parochial feature(s) of our own adaptive machinery' (SOBER 1981, p117). And this could well include the kind of heuristic constraints mentioned above. Against this it may be held that some features (such as the desire for simple hypotheses) may naturally result from demands of informational economy derivable from the broad evolutionary bias towards cost-efficient and prompt processing of data. This option too is signposted by SOBER. I think, however, that it is a mistake to see the scientific issue as essentially bound up with our attitude to the heuristics at all. For the prime site of empistemological infection must lie, I shall now argue, with the range and nature of our access to phenomena and hence with the first source of constraint on scientific theories. Even if we are obectivists about the heuristics (taking them as essential to any rational approach) this will not be sufficient to insulate science from the shock waves of the evolutionary account of our sensitivity to observational data. And if we hold the heuristics to be contingent, biased and unprivileged too, then so much the worse for a traditional scientific realism.

The observation that one of the two major constraints on scientific theory-building is to keep faith with the phenomena (to save the phenomena, as DUHEM puts it[4]) ought to be enough to transmit some of the basic evolutionary infection of bias and limitation to the body of scientific knowledge itself. For to admit that science aims to explain and systematise the phenomena is to tie the possible content of science to the range and nature of the phenomena accessible to the particular biological organism designated 'man'. It is at just this point that any thoroughgoing scientific realism which would see science as penetrating to the unique noumenal roots of nature must founder against the evolutionary rocks. For what is accessible to man (the bare observational data to which all theoretical constructions must answer) is determined by the very same contingent, species biased and limited modes of sensory access and basic processing to which the evolutionary scenario of section one unequivocally applies. Even the instrumental augmentation of human sensory capacities must answer to some checks in gross observational accuracy or we would have no cause to accept such augmentation as in any way veridical. Science, for all its sophistication, thus looks unable to transcend completely the humanity of its observational base.

Science, thus conceived, partakes of the dual aspect of all evolved cognitive modes (albeit by a more indirect route). In being faithful to the phenomena it maintains the original tie established by the selective process between the phenomena as known by a being.And the real world in which the being must live. Yet by dealing only with the phenomena which happen to be experienced by human beings it inherits also the species-specific interests and random caprices of fate which combined to render accessible those particular aspects of reality in that particular way. According to which of these two aspects of the phenomena are stressed we get a more or less realistic picture of the activity of science.

3. Just how much realism does the evolutionary account require if it is not to collapse under its own weight? To get some idea we may consider a typical evolutionary claim. The claim is that

> The hydrodynamics of sea-water, plus the ecological value of locomotion, have independently shaped fish, whale and walrus in a quite similar fashion … But the jet-propelled squid reflects the same Hydrodynamic principles in a quite different … shape. (CAMPBELL 1974, p447)

For such claims to be intelligible the evolutionary theorist must claim some right to employ our scientific account of the hydrodynamics of sea-water as descriptive of the common reality to which both fish and squid are adapted. In some sense then the world revealed by science must be justifiably taken to describe the mindindependent environment in which adaption has occurred.

Is such a role for science compatible with the evolutionary epistemologist's account of cognitive limitation and bias, supposing that account to be extended to include our own sensitivity to phenomena? I think it is, and one way to show how this is so is to focus on the idea of science as *modelling* an extra-experiential reality.[5]

The notion of a model seems a particularly apt one for the evolutionary epistemologist to employ. For there is no implication that a model is a perfect replica of what it models. Rather, we conveive a model as bringing out particular features of some realworld entity, perhaps to the exclusion of other features. And just *what* features are stressed will depend (a) on what information the modeller has at his disposal and (b) on the particular needs and interests which the model is designed to serve. These two features correspond satisfactorily to the contingency of the range of realworld phenomena to which man has direct observational access and to the particular kind of interest which man's needs and the nature of the human brain allow him to have regarding the accessible realm.

We may now clarify the nature of the proposed linkage between our scientific models and extra-experiential reality by introducing a special relation of tolerance. Thus we may call a basic cognitive strategy or sensory modality (let P stand for this disjunction) *tolerated* by the adaptive environment iff

> P affords a means of classifying, predicting or reacting to things and events which, when applied by beings of a given biological constitution in a given niche makes for successful (= survival enhancing) action in the world.

And we may call a scientific theory P' maximally tolerated by extra-experiential reality iff

> P' affords a means of conceiving of things and events which, when applied by beings of a given biological constitution, enables them to account for successfully (= explain and perhaps predict) all the phenomena accessible to a being so constituted.

P' is then to be conceived as an ideal scientific model in the sense of model outlined above. Such a model is then related to the real world it models by virtue of the relation between the phenomena it explains and the world, such links being constituted by the original tolerance relation between P and the environment. The justification for calling P' a model of the real world thus rests squarely on the evolutionary justification for taking the phenomena which are modelled to be appropriate (if partial and biased) representations of the world they cope with.

A true scientific theory, we may now say, would be one that is maximally tolerated by the reality accessible to man.[6] And there will be an infinite. gradation of tolerances between the minimal (accounting for only a small number of phenomena) and the maximal (accounting for *all* the phenomena). No maximally tolerated theory has yet been found, and perhaps none ever will be. But the crucial point is this; even if one *were* found, still the reflexivity of the formulation of the tolerance relation (its relativisation to *human* and contingent capacities) would rob it of any claim to be the one unique metaphysical truth fated to be agreed by all rational beings.

The intelligible goal of science, we may now say, is *not* the description of the world-in-itself but the production of more and more highly tolerated models of the world we find around us. And a model is, ultimately, nothing more or less than a useful arrangement of information. Just *what* arrangements of information we find useful will depend on our human needs and capacities and the particular cognitive orientation we happen to possess. Thus, to give a simple example, a program written in Cobol would not prove a useful arrangement of information for a computer which could only process commands coded in Basic.

The strong conclusion to draw from the picture of science as aiming at tolerated models would be that even at the ideal limit of human enquiry there might be a plethora of available models all of which are observationally and Heuristically adequate (such a conclusion is endorsed by PUTNAM 1983, pp1–25). For our purposes, however, something weaker will do. We may conclude simply that scientific enquiry is still not the only possible 'correct' representation of reality even if relative to our cognitive constraints and observational accessthere are no visible alternatives. In other words, given the natural possibility of alternative life-styles, needs, capacities and cognitive structures it makes no sense to identify our ideal scientific model of reality with the ultimate nature of the world-in-itself. A model is still just a model, it is not the ore true description worshipped by the metaphysical realist. Just because we do not regard our models as unique or necessary, however, does not mean we may not regard them as valid representations, in the light of our interests and structure, of the available information. It is this combination of cosmic contingency and limited objective validity which allows the evolutionary theorist his scientific account of the common adaptive enviro nment while admitting the cognitive bias and limitations implied for man by the adaptive account itself. One interesting consequence of this analysis is that we must accept the possibility of alien epistemologists (perhaps even alien evolutionary epistemologists) working successfully with a different model of the 'common reality' to our own! Such epistemologists may even diagnose man's models as a natural and explicable outcome of our own biological nature as it appears to their science. We, of course, might do the same for them! Each scientific model would therefore be sufficiently powerful to embrace the working of the other. The questionas to which model is the correct one would never be raised.

4. The question finally arises whether the spectre of the world-in-itself, apparently attendant upon the epistemological dualism diagnosed in 1 above, has been successfully exercised or merely relocated? For to adopt the quasi-realistic notion of science as aiming to produce tolerated models is to invite the philosopher's retort 'models of what?'. Two courses are open to the evolutionary epistemologist here. He may allow that all such models are models of the one

(alas indecribable) objective, mind-independent reality to which all beings are variously adapted. Or he may dig in his heels and refuse to countenance any conception of reality save that of whatever is said to exist by some successful model (be it a human or non-human one). So *either* we ghive up the very idea of the world-in-itself (as RORTY and DAVIDSON urge us to do[7]) and replace it with the notion of multiple valid species-specific descriptions whose objects are determined by the descriptions themselves, or we retain the idea of the world-in-it self as a bare noumenal something = X which somehow supervenes (or maybe transcends) the totality of possible descriptions of it. Whichever we choose, the divorce of science from the description of noumenal reality is ratified. Of the two oprtions suggested, I find myself attracted to the more austere alternative of dropping the notion of the world-in-itself entirely. The dualism of organism and environment would then remain as a part of the theoretical model of biological science, which model itself would be regarded as non-unique and cosmically unprivileged. Bot there would be no need to assume, in addition to this, that all the possible models of reality *themselves* stand on one side of a dualism of models and the world-in-itself. Aside from the general thought that the idea of the worl-in-itself can now be seen as theoretically spurious to the evolutionary account (which requires only the acceptance of an organism-environmet dualism within a given explanatory model which takes *science* to provide the necessary account of the environment) there are two reasons which tell in favour of abandoning the notion. The first is the recent and influental polemic launched by Hilary PUTNAM (see notes 3 and 6) against the notion of there being one true (if unknown) description of how the world is. Such a beliefe, PUTNAM argues, can be shown to be false on model-theoretic grounds alons. The second reason has to do with the intelligibility of the very idea of the world-in-itself. For such a world looks to be necessarily indescribable (description implying point of view, cognitive bias and so forth). But to claim that something about which we can necessarily say nothing exists may be to claim nothing which we can properly grasp at all. For any such claim looks distinctly dubious in the light of DUMMET's recent investigations into meaning. If we accept, with DUMMET[8] , that meaning attaches to statements in virtue of our capacity to recognise when the circumstances described by the statement actually obtain, we may still make sense of the minimal evolutionary claim viz. that various models may succeed in coping with reality (we may observe the survival and achievements of beings employing such models). But what *further* evidence could there be to warrant us in assenting not just to a plethora of models but to there being one, ultimate, unknowable way the world actually is beyond how it appears in the various models we, or any other sentient being, might construct?

In choosing therefore to give up the notion of the world-in-itself the evolutionary epistemologist must simultaneously 'resist RORTY's alternative description of reality as whatever human beings can agree at a given time exists. (See RORTY 1972, pp661–663). For human beings, we have seen, can recognise the bias and contingency of their own descriptions of reality from a position *within* biological science. To simply *identify* 'the world' with the world of man is, we may be sure, mere anthropomorphic conceit. The alternative, recommended in this paper, is to embrace the difficulty of admitting multiple valid descriptions and to assert that to be is to be perspectivally.

Finally, let us observe that the denial of any privileged status to the model of human science renders our whole account of the tolerance relation itself harmlessly self-referential. For our theoretical models are ultimately justified by keeping faith with observable phenomena. These observable phenomena are, on the theoretical model of evolutionary theory, accessed and characterised by sensory capacities and basic forms of processing which have stood the test of survival. They are hence assumed to constitute a species-valid arrangement of information concerning the external world. Theory is thus justified by theory in a cosy epistemological circle of the kind sometimes described as 'virtuously'.[9] A direct consequence of this is that our belief in the relation of tolerance is *itself* justified only as a tolerated belief. It is thus an acceptable representation, for beings of our knowledge and constitution, of the relation of sense and thought to an external reality. But we may not elevate the scientific model which employs the idea of tolerance to the level of a unique or metaphysically privileged representation of the relation of thought and sense to the world. The evolutionary epistemologist dare not claim to possess the one true account of the relation between mind and the

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material realm. The best he can do is to say that it is *an* account, acceptable to us, and one which avoids the metaphysical excesses of a traditional scientific realism. As Clive JAMES once observed:

"There are limits to the altitude that can be achieved by hauling on one's own bootstraps" (JAMES 1981, p35).

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Notes

* Reprinted by courtesy of Philosophica.

1 Examples of work in Evolutionary Epistemology would be CAMPBELL (1974), LORENZ (1941). Or TENNANT (1983).

2 These constraints are signposted by both TENNANT (1983) and CAMPBELL (1974) and also by SOBER (1981).

3 The very same accusation is made by H. PUTNAM against his earlier metaphysically realist self in the introduction to PUTNAM (1983).

4 See DUHEM (1974).

5 This kind of account of science is most fully developed in VAN FRAASSEN (1980).

6 This corresonds with PUTNAM's idea of truth as the ideal end-point of the series of warrantedly assertible claims concerning the nature of reality which human beings could in principle come to make. See PUTNAM (1981).

7 See RORTY (1972 and 1980). Also DAVIDSON (1974).

8 See especially DUMMETT (1978).

9 The terminology is, I think, due to Rescher. A virtuous circle is one which provides an improvement in understanding in spite of any element of self-reference involved. Thus, in the present case, we learn, by the application of our understanding, something of the reasons why we might trust our understanding to reveal something of the world in which we evolved.

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Ed Constant

Comments on Donald T. Campbell's "From Evolutionary Epistemology Via Selection Theory to a Sociology of Scientific Validity"

Abstract

In this comment I discuss CAMPBELL's insistence on the 'blind' character of variation in all evolutionary variation-selective retention processes, which has often been misunderstood, and the similarities and differences between communities of scientists and other believing communities.

First, let me say that I am deeply pleased that the editor's of Evolution and Cognition have chosen to publish this penultimate expression of Donald CAMPBELL's oeuvre. It is a pleasure profoundly compromised by the realization that this is the last of his remarkable work we are likely to see.

In my comments, I'd like to focus on two aspects of this synthetic statement of the issues and themata that have concerned CAMPBELL over the last forty years. One aspect, the importance in all evolutionary variation-selective retention processes of some form of "blind" variation, is from near the beginning of his work in evolutionary epistemology. The other, the similarities and differences between communities of scientists as knowers, and other believing communities, especially archaic religious communities, is a relatively recent development. Both, oddly, are somewhat muted in this presentation.

Probably no other single issue so plagued, or, in my view, generated so much misunderstanding of, CAMPBELL's evolutionary epistemology as his use of "blind" variation as the source of the raw material on which selective retention (or, later, a selection process) could operate. The long list of synonyms and circumlocutions CAMPBELL tried—"blind," "random," "unforesighted," "going beyond the limits of foresight or prescience" (this volume, p8)—bears witness to the difficulty of making this notion clear. Critics, unfairly I believe, and for reasons I still cannot fathom, set the idea of blindness or randomness in the generation of variants in sharp juxtaposition to "rationality" and "intentionality," and therefore labeled CAMPBELL's model "irrational." But as used in CAMPBELL's theory of a "nested hierarchy of blind variation-selective retention processes," "blindness" entails no such juxtaposition: the only conception of "rationality" it might contradict is one claiming that valid knowledge can only obtain from absolute deductive completeness, a view that no naturalized epistemology dare hold.

The key to CAMPBELL's true position is the idea of a nested hierarchy of variation-selective retention processes. The structure of this hierarchy permits vicarious exploration and selection, that is, "processes which shortcut a more full blind-variation-and-selective-retention process," and "are themselves inductive achievements, containing wisdom about the environment achieved originally by blind variation and selective retention." (ibid., p8) Such vicarious exploration and selection can be executed by many means, have many expressions: (fallible) vision itself, our beliefs (proofs) about Euclidean space, Boolean logic, the theory of the conservation of energy, taste and smell perceptions, HAMILTONIAN Quarterions (PICKERING 1995). All serve us as means to vicarious exploration and vicarious selection: they permit a nested hierarchy of *conscious* variation and *vicarious* selection processes that approximate "rational" deduction, intentional means-ends analysis, and rigorous evaluation.

All that CAMPBELL insists upon is that for *genuine* novelty—that which has not been produced by prior blind variation and selective retention—to enter such processes, some form of blind variation must occur at some, usually very low, level in the hierarchy. His proof of this contention is a simple *reductio ad absurdum: If* not blind or random variation, *then either* some form of foresightedness, which only can result by extrapolation or deduction from previously achieved knowledge, *or* supernatural revelation.

I suspect part of the difficulty in explicating the character of these blind variation processes was

CAMPBELL's reliance on introspective accounts of creativity, most notably Henri POINCARÉ's (1921). POINCARÉ, and also Jacques HADAMARD (1945)—whose theory CAMPBELL saw as simply a rehash of POINCARÉ's—, both postulated, via introspection, some unconscious, random association or combination of "ideas," which were then subjected to some equally unconscious preliminary selection, perhaps on "aesthetic" criteria. Only after this unconscious random variation-selective retention process might "thoughts" or concepts be presented for conscious evaluation. Such introspective accounts are quite naturally suspect, for CAMPBELL's evolutionary epistemology as for the self-reported verbal "protocols" of people's problem-solving activities that are so often cited as justification for artificial intelligence simulations.

In a sense, CAMPBELL's necessary reliance on introspective accounts parallels DARWIN's plight in simply having to postulate some mechanism for the inheritance of advantageous mutations without knowing precisely what that mechanism might be, which of course, later, reconstructed MENDELIAN genetics afforded (BRANNIGAN 1979). It may well be that connectionist models of cognition, or neuron nets, with their strong probabilistic foundation, will provide CAMPBELL's MENDELIANISM—or they might not. What matters, however, is that above this minimal level in the nested hierarchy of variation-selective retention processes lie strata upon strata of *conscious* variation and *vicarious* selection processes, the only rationality a naturalized epistemology can have: as CAMPBELL argues, all "rationality" is itself an inductive achievement, forever corrigible and hypothetical.

My other comments relate to the other end of the scale, as it were, both temporally and dimensionally: from the neuronal to the communal. CAMPBELL long argued that all the objectivity to be found in science was to be found in its communal practices, not in the cognition, morality, or behavior of individual scientists. In his extended discussion of the parallels and non-parallels between archaic forms of belief (religion), which he, correctly I believe, sees as serving the latent function of socially producing cooperation among genetic competitors, and the social system of science, CAMPBELL focuses mostly on comparative belief systems, their propagation (from BOYD and RICHERSON, density-dependent replication), and their epistemology. An alternative, surely latent in CAMPBELL's own discussion, is to focus on the social character of believing collectivities rather than on their beliefs.

Such an alternative emphasis reveals much stronger parallels, to the point of identity, not only between archaic religions and science, but also among religious communities, science, and all other quasi-altruistic social collectivities. The basic structure for all such collectivities (although he does not make such a broad claim) is laid out by P. Steven SANGREN (1988, 1991) in his studies of the MA TSU cult in Taiwan and adjacent mainland Fukien province. According to SANGREN's description, MA TSU, a presumably historical figure who saved Taiwan from invasion in the ninth century, serves as an "alienated representation" of group solidarity and efficacy: the ancient victory is attributed to the intervention of the goddess, not to the altruistic cooperation of her devotees. Thus the power and solidarity of the present-day community of believers is alienated to some remote, and, not coincidentally, incorruptible realm.

In temple rituals, but especially in pilgrimages, devotees learn the ritual production of selves in appropriate communal idiom, thereby socially producing themselves, and socially reproducing the community itself and its alienated representation. Only specific, temporal reifications of the deity's will or beneficence are subject to real-time human interpretation, or misinterpretation or misuse, not her transcendental existence or power. This social robustness in the face of human frailty and mendacity is, of course, the answer to CAMPBELL's parenthetical query about archaic religions, "Why were not the force of custom plus interpersonal reinforcements sufficient without such cosmologies?" (ibid., p22) More strikingly, a little reflection clearly reveals that what specific *forms* these alienated representations might take is completely irrelevant to their latent social function: as CAMPBELL puts it, "The details of these supernatural cosmologies were extremely *heterogeneous* … (This is the puzzle of diversity…)." (ibid., p22)

This form-independence of alienated representations suggests that the form does not necessarily even have to be supernatural. *Any* belief system that provides an alienated representation capable of sustaining communal solidarity, that performs the latent function of inducing quasi-altruistic cooperation among community adherents, will do, including ideologies: "the market," MARXIST-LENINIST dialectical materialism, or "*the* scientific

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method." The ideology of science, its exaltation of method, and its normative structure, all historical, contingent products of the scientific and social revolutions of the seventeenth century, albeit extended in later centuries far beyond the bounds of proper English "gentlemen," (SHAPIN 1988, 1994) are identical in structure and function to those in any other social collectivity.

CAMPBELL, in his characterization of "experiments as divination rituals," (ibid., p29) properly recognizes the ritual nature of "producing" science. What he doesn't articulate quite as clearly is that these rituals produce both science—reified bits of knowledge—and scientists. On the model sketched above, scientists, in their professional rituals—running experiments, writing reports and papers, refereeing others' papers, reading papers at professional society meetings, serving on advisory panels—learn as though pilgrims to produce themselves in communal idiom. In so doing, they also reproduce the community itself and its alienated representation: the awesome power of "*the* scientific method." And it is, of course, that very deity, that method, that, in SANGREN's happy phrase, "mutually authenticates" the bits of science, and the scientists, so produced.

Because the alienated representation of scientific method is conjured anew in each scientific ritual, the fidelity of the conjuring, like the orthodoxy of religious ceremony, is always contestable. Novel rituals—new science, new experimental methods—quickly set off disputes about fidelity to the ideal: witness the many versions of MENDEL, or the ugly fights among morphologists, pheneticists, and cladists about who was "really" the proper DARWINIAN (HULL 1988). If one were skeptical about the real existence of some spatiotemporally universal, immutable, invariant scientific method, as distinct from its ritual reifications in scientific practice (as one might be skeptical about the supernatural existence of MA TSU or any other god), then quarrels about what constitutes "good science," what specific behaviors and phenomena are to be acknowledged by the scientific community as expressing *the* veridical scientific method, are both to be expected and absolutely critical to the survival of the enterprise—and to a sociology of scientific validity.

I think it is the great virtue of Donald CAMPBELL's work, that all the while acknowledging the quintessentially social character of science, he also hewed hard and fast to the argument that the spatiotemporally particular rituals of science do include validity-enhancing practices through which the world as it is can at least co-select our beliefs about it: it is his always presumptive, corrigible and hypothetical, "fallibilist realism."

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Campbell's Failed Cultural Materialism

Abstract

CAMPBELL's evolutionary epistemology had the potential to treat the 'selection environment' for science as bearing more than a metaphorical resemblance to the material environments as it features in biology.His concern for knowledge 'vehicles' and 'co-selectors' pointed in that direction. However, CAMPBELL failed to follow through on his materialist promise because he could not envisage that science may become worse as it comes to dominate its material environment. I trace this to a prejudice he inherited from SPENCER. Here guidance could have been sought in economic history's grappling with the concept of 'development', especially in a world where some develop at the expense of others. I trace some of the implications of this 'path not taken', while granting that CAMPBELL's evolutionary epistemology was much more on the right track than, say, KUHN's more famous version.

The longer that Donald CAMPBELL developed his evolutionary epistemology, the more he was inclined to regard his reliance on DARWINIAN natural selection theory as a suggestive metaphor rather than a commitment to biology as the metatheoretic framework for explaining the emergence and growth of scientific knowledge. CAMPBELL (1997) is quite explicit on this point. However, I would urge that CAMPBELL let himself off the hook too easily and that in fact he implicitly accepted enough of the materialistic assumptions of contemporary biology to be seen as treating the 'evolution' side of evolutionary epistemology at least as literally as, say, Marvin HARRIS (1979) does in his pursuit of a 'cultural materialist' paradigm in anthropology. This point is easily obscured if we immediately confer metaphorical status on appeals to evolution as soon as the 'selected' entities or properties are not straightforwardly related to entities or properties recognized by current genetic theory (hence, the vigorous but inconclusive efforts by Richard DAWKINS and Daniel DENNETT to promote 'memes' as genetically respectable units of cultural selection). However, this requirement is needlessly strict. Like HARRIS, CAMPBELL accepted a literal form of what might be called 'ecologism', namely, that selection occurs on the material—or, in the case of knowledge, what CAMPBELL tended to call 'vehicular'—characteristics rather than ideational ones, which in turn reflects the fact that, in the final analysis, the environment provides the non-negotiable context to which prospective inhabitors must conform or else simply not survive. In this respect, 'selection' for CAMPBELL is not merely a synonym for 'decision' but refers literally to background constraints on the efficacy of decisions of which the relevant agents may be only dimly, if at all, aware.

I shall argue that CAMPBELL fell far short of his materialist promise. Nevertheless, CAMPBELL's latent ecologism should be commended for enabling us to see more clearly the strengths and weaknesses of his evolutionary epistemology. A good point of contrast here is with the somewhat different appeals to evolutionary theory that have been made by Thomas KUHN and Niklas LUHMANN, both of whom have been associated with 'autopoietic' or 'self-organizational' models of scientific change. Central to both KUHN's and LUHMANN's conceptions is an irreversible insulation, or 'autonomization', of science from the larger social environment at a certain point in its history (say, with the founding of the Royal Society)—not merely in the sense of science being protected from ambient social pressures but more importantly in terms of science's development being defined exclusively in the scientists' own terms. In effect, autonomization enables science to simulate the frictionless medium of thought that philosophers since PLATO have considered ideal to the pursuit of knowledge. Consequently, neither KUHN nor LUHMANN have much to say about the vehicular requirements of scientific knowledge or even those aspects of society that have been, in CAMPBELL's terms, 'co-selected' with scientific theories. Rather, one is presented with a seemingly irreversible, if strictly non-teleological, story of functional differentiation of the scientific enterprise, as inquirers encounter impasses in the day-today business of puzzle-solving that force them to divide their efforts in order to clarify the overall direction of their inquiries. Here evolution becomes little more than a metaphor in which 'selection' results from the expectations of inquirers being confounded by their experiences, which in turn cause them to take a fateful decision.

An important sign of evolution's metaphorical status in these autopoietic models is that it is never clear whether the process in question is supposed to be *unique* or *repeatable*. For example, is KUHN's (1970) theory of scientific change supposed to model the stages through which *the* history of science as a singular global phenomenon passes, or rather the phases through which specific sciences pass in any of a variety of times and places? Were KUHN to have intended the former, his model would have approximated a literal application of evolution to epistemology. But in that case, he would have been forced to confront how the initial scientization of certain fields at certain times and places—specifically, experimental physics in 17th century Western Europe—set constraints on later developments, even in remote fields, times and places. In other words, how does the prior existence of certain forms of inquiry constitute the environment against which subsequent forms of inquiry are selected? Clearly, KUHN has not been read in this way, but rather as having advanced a multiply repeatable, perhaps even universalizable, model of scientific change that is just as relevant to, say, sociological inquiry in the 1960s as physical inquiry in the 1660s. It is ironic, given KUHN's reputation for having 'historicized' the history and philosophy of science, that his model should be applied in such a mindlessly ahistorical fashion. However, precisely because CAMPBELL built in the relevant material constraints into his evolutionary epistemology, his model is unlikely to suffer from the same degree of popularity. Indeed, it is amazing that CAMPBELL never seriously pursued the implications of his materialism. Had he done so, we would have seen much greater discussion of the demographic, economic and technological dimensions of the scientific enterprise.

Today it is commonly assumed that because science does not follow the same path in all times and places, it must therefore follow different, unrelated paths that can be explained only by citing local factors. This inference, characteristic of the 'ethnographic' or 'postmodern' turn in science studies, constitutes a false dilemma. It overlooks that the evolution of scientific knowledge may be exactly like biological evolution in being a single and unrepeatable trajectory that encompasses the entire world. This is, of course, compatible with a considerable degree of variation across local environments, which can in turn be explained by the differential impact of earlier events of common ancestry. CAMPBELL's evolutionary epistemology is most naturally read in this fashion, but his own rather SPENCERIAN cultural prejudices prevented his model from serving as an exemplar of this 'third way'. Before revealing the nature of these prejudices, let us first turn to the field of economic history, especially its long-standing concern with modeling 'uneven development' (see e.g., BLOMSTROM/HETTNE 1984), which indeed does offer guidance on the third way.

The concern in question is an outgrowth of LENIN's theory of imperialism, according to which the logic of capital expansion enables the first industrializing nations to force latecomers into a position of subservience, either as mere providers of raw materials or as consumers of goods produced by the early industrializers. In this way, a 'core-periphery' relationship is perpetuated—that is, until the Communist Revolution occurs. Whatever its own shortcomings, LENIN's theory managed to eliminate the 'metaphysical residue' in Marxist political economy, namely, the idea that the essence of economic development was first manifested in Western Europe's transition from feudalism to capitalism and would subsequently be reproduced by the rest of the world, largely without change. (The most systematic scholarly elaboration of LENIN's perspective—albeit without LENIN's apocalyptic outcome—is *world-systems theory*: see e.g. WALLERSTEIN 1991). Even LENIN's 'bourgeois' critics, while questioning the inevitability of imperialism, nevertheless conceded that one needs to consider the world-historic environment that defines what often turns out to be a narrow band of possibilities within which development can occur in a given time and place. For example, while late developers have often industrialized more quickly, they have typically to supply local substitutes for the institutions of the originators (GERSCHENKRON 1962).

KUHN's theory of scientific change—at least as interpreted by his admirers—retains just the sort of metaphysical residue that LENIN endeavored to purge from MARX's theory. Not surprisingly, KUHN has been eagerly embraced by the most famous recent metaphysician of economic development, Francis FUKUYAMA (1992, pp80–81, 352–353), who regards the inviolate 'logic' of scientific inquiry as described by KUHN to be the motor of economic progress that will eventually lift all nations to the standard of living currently enjoyed by the United

States. One need not be a LENINIST to find evolutionarily relevant reasons for doubting FUKUYAMA's prognosis. The average American already consumes six times more energy than the average person. Barring a breakthrough in the design of artificial environments, that alone precludes the US from providing the basis for an ecologically sustainable political economy. Interestingly, the same applies to American science as a model for world science—and not just at the level of metaphor. However, compared with the idea of limits to economic growth (ARNDT 1978), limits to scientific growth has been rarely touched upon (RESCHER 1984). However, a telling precedent was indirectly set by the man who first drew the Little Science/Big Science distinction, Derek de Solla PRICE.

In his tireless search for 'science indicators', PRICE (1978, p87) found that the economic indicator most closely correlated with scientific productivity (papers per scientist) was electrical energy consumption (kilowatt-hours per capita), which is in turn related to economic productivity (GNP per capita) by a power of 3/2. In a nutshell, the US consumes 25–35% of the world's energy resources and produces a comparable portion of its scientific papers. (See OLSEN 1992 for recent confirmation.) The finding highlights the extent to which science itself consumes resources, thereby reversing the economists' image of science as exclusively a 'factor of production', a perpetual motion machine. By drawing attention to consumption patterns, PRICE unwittingly brought into the focus the question of *waste* in the scientific enterprise. Consider Michael POLANYI's famous definition of free inquiry as the ability to waste resources with impunity in the name of truth. These resources include not only money, paper, machines and energy but also *people*. Here I am alluding to what Robert MERTON (1973, pp439–459) has euphemistically called science's 'principle of cumulative advantage' (also known as the 'Matthew Effect'), which, like capitalism's 'invisible hand', is presented as a situation that appears cruel to individuals but eventuates in long term systemic benefits. Accordingly, in science, those who are rewarded earlier (say, in terms of elite training, early posts and publications) are rewarded more and longer, with the difference between the initially advantaged and disadvantaged increasing rapidly over time, such that two-thirds of scientists give up on publishing altogether after their first article. However, Social DARWINISTS of science policy that they were, neither MERTON nor PRICE ever suggested that this tendency should discourage the recruitment of new scientists, thereby downsizing the scientific enterprise so that more of its participants would have a better chance of having their contributions recognized. On the contrary, PRICE stressed 'economies of scale' as improving the overall quality of science. In other words, an ever widening field of competitors would keep all scientists on their toes, which would in turn ensure their best performances, leading to an increase in the overall percentage of quality publications—not to mention an increase in the absolute number of unrecognized scientists 'wasted' by the system. MERTON went so far as to argue that if indeed genuine contributions to knowledge are likely to go unrecognized, disadvantaged scientists should try to get advantaged scientists to promote the findings as their own.

PRICE and MERTON clearly envisaged a 'selectionist' sociology of science, albeit a crude one that let environmental vicissitudes do most of the work that rationalist philosophers of science would assign to explicitly formulated epistemic aims and normatively acceptable constraints on their pursuit. The closest that PRICE got to specifying the 'ends of science' was in terms of the indefinite accumulation of highly cited scientific articles. This metric of scientific progress is about as primitive as measurements of wealth prior to Adam Smith, when it was commonly thought that a nation increased its wealth simply by accumulating more bullion in its treasury than (and often at the expense of) its neighbors. Smith proposed that the range of living standards and the organization of labor be taken as alternative indicators of a nation's overall wealth. We have yet to take the 'SMITHIAN Turn' in evolutionary epistemology, whereby the progressiveness of science would be judged more by the quality of its people than its papers. Taking the turn would require merging the 'selection environment' for science with that of the surrounding political economy. Consider the case of science's 'invisible' tendency toward elitism, noted above. Behind it may be the fact that as research becomes more 'advanced', in the sense of specialized, it becomes more expensive in almost every respect and hence subject to various problems of scarcity (of skills, equipment, etc.), which are then sociologically resolved by concentrating resources in a few researchers and institutions. Yet, when this relatively unproblematic observation is set against the backdrop of science's contributions to labor-saving devices and other forms of technical efficiency, it would seem that science helps economize in the rest of society in order to make room for more lavish expenditure on its own activities, as measured by not only the number of researchers but also the time the general population must invest in acquiring credentials for any line of work. Certainly, this is one (uncharitable) way of interpreting the growing presence of 'knowledge-intensive' activities in the economy at large (STEHR 1994). It may also capture Moritz SCHLICK's (1974, pp94–101) brand of evolutionary naturalism, whereby the advance of civilization is marked by science gradually reversing its role from being a means to other ends in life to an end pursued in itself, and ultimately the only end worthy of unconditional pursuit.

From an economic standpoint, to regard science as an 'unconditional pursuit' is to forgo any scrutiny of its return on investment. Many would argue that this is as it should be, since science's returns are typically 'intrinsic', 'deferred' or 'diffuse' in ways that defy normal accounting procedures. However, the very use of the enquoted terms concedes the elusiveness of the boundaries that define science's selection environment. But there is more for the economist to worry about here. Consider the expression 'functional differentiation', which is supposed to structure the ever expanding scientific enterprise. When the metaphorical grounds shift from embryology to political economy, we are asked to imagine an increasingly specialized division of labor in science. At first glance, this suggests that the pursuit of inquiry is becoming more efficient, as each scientist does what only she can do and then collaborates with her fellows to get the entire job done. Adam Smith would be pleased. And while such an image may well capture the microdynamics of the research team, it has virtually no bearing on the sense of 'division of labor' that operates at the systemic level of scientific activity. At this level the issue turns more on *entitlement* than efficiency, namely, the settlement of jurisdictional disputes between competing inquirers, which then becomes the basis for relations of mutual deference and trust. In some evolutionary models, the importance of resolving these disputes is made explicit, as when a KUHNIAN paradigm subdivides to conclude a period of 'crisis'; in other models, such as CAMPBELL's, its importance remains implicit in, say, the 'fishscale model of omniscience' or his preoccupation with 'competence of reference' as a leading aim of science. This image of science as an abstract sort of real estate presupposes that over time the access that any individual scientist (or scientific discipline) has to reality becomes more circumscribed and mediated, so that most of what a scientist knows about a given field may be based almost entirely on whom she trusts for reliable information. Again seen in strictly economic terms, this puts us in the realm of brokered exchanges, whereby potential buyers no longer know first-hand what is for sale and hence must rely on third parties. A situation of this sort arises in the 'informal economies' of the former Soviet Bloc countries, which have experienced the breakdown of socialism without yet installing market mechanisms that reliably transmit PRICE information. Do we really want to call such a state-of-affairs 'functional'?

Earlier I alluded to 'SPENCERIAN prejudices' that prevented CAMPBELL from following through on the materialism implied by the significance given to 'vehicles' and 'co-selection' in his evolutionary epistemology. These prejudices appear most clearly in section 7 of CAMPBELL (1997), which portrays religious authority as a principle of social solidarity that enables a robust form of 'adaptive cultural evolution' that is nevertheless distinct from one that encourages science. Like Herbert SPENCER, CAMPBELL held that the coercively induced need to conform with tradition was the ultimate obstacle to genuine scientific and social progress. This belief informed not only CAMPBELL's reading of history but also his design of experiments, such that he could treat the problem of intergenerational knowledge transmission as a species of small group conformity studies (JACOBS/CAMPBELL 1961). Moreover, also like SPENCER (though less reflectively), CAMPBELL seemed to believe that scientific and economic rationality were co-selected, so that the growth in wealth that is characteristic of economically rational regimes—that is, capitalist ones—becomes the 'natural' environment for sustaining the growth of science. (The giveaway here is CAMPBELL's *en passant* remarks about the 'wasteful funerals' in religiously based regimes.) Thus, in neither SPENCER nor CAMPBELL do we find ecologically inspired concerns about whether science could become 'too expensive' or that we could have 'too much' knowledge or, for that matter, whether our investments in scientific inquiry might become 'unproductive' or their returns 'maldistributed'. If one can never be too rich, neither can one ever be too smart: In both science and capitalism, more is *always* better. However, a trajectory of endless accumulation and growth does not sit well with classical images of inquiry having a clear aim (e.g. 'Truth'), whose achieve-

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ment would presumably bring science to an end (FULLER 1997). CAMPBELL resolves this tension the usual way by gesturing toward the 'asymptotic approximation' of the Truth, which neatly embeds the idea of endless inquiry in a framework that still appears, in some sense, goal-directed.

Because of these SPENCERIAN prejudices, CAMPBELL could never envisage an evolutionary epistemology that features the decline or corruption of science as a long-term consequence of its having been co-selected with the capitalist ethic. However, the pressures on governments today to divest its massive funding commitments to science suggest that we may not be so far from a world that would force this awareness upon us. CAMPBELL would perhaps recognize its precedent—and recoil in horror. In a fascinating essay on the collapse of the great ancient civilizations, the archaeologist Joseph TAINTER (1988) argued that these societies eventually became too complex for their own good, as the cost of governance exceeded the benefit that accrued to either the governors or the governed. Collapse came in the form of either a simplification of the administrative apparatus or a fragmentation of the regime itself. If our highly 'functionally differentiated' science system is like one such ancient civilization that let its material development go unchecked, then we might expect *epistemic collapse* to come from, on the one hand, the elimination of disciplines and specialties (say, through the reorganization of the university) and, on the other, the privatization of knowledge in the form of intellectual property. Perhaps future strains of evolutionary epistemology will incorporate these very material changes in the selection environment for science.

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Francis Heylighen

Objective, Subjective, and Intersubjective Selectors of Knowledge

Abstract

It is argued that the acceptance of knowledge in a community depends on several, approximately independent selection "criteria". The objective criteria are distinctiveness, invariance and controllability, the subjective ones are individual utility, coherence, simplicity and novelty, and the intersubjective ones are publicity, expressivity, formality, collective utility, conformity and authority. Science demarcates itself from other forms of knowledge by explicitly controlling for the objective criteria.

Introduction

It is with great pleasure that I use this opportunity to comment on Donald T. CAMPBELL's last paper (1997). I came into contact with Don's work at the beginning of my research career, in 1984, during a conference on evolutionary epistemology at the University of Ghent. Since then, his writings have been a constant source of inspiration. After we met, in 1990, we started to regularly exchange publications. Each time I received a bunch of his papers, I began reading them with much pleasure, because I knew that I would find every paragraph teeming with deep insights and surprising observations. We finally decided to collaborate, producing an ambitious review paper about the evolution of social systems (HEYLIGHEN/CAMPBELL 1995). We were planning to write more papers together, but that has been made impossible by his untimely death in 1996. I see the present paper as an opportunity to somehow continue my collaboration with Don, adding my insights to his in a collective publication.

As may have become obvious, there is virtually no disagreement between my philosophical position and the one of Donald CAMPBELL. The differences in approach have more to do with theoretical background and methods than with aims or convictions. While CAMPBELL (1974) called his philosophy of knowledge "evolutionary epistemology", I would characterize mine as "evolutionary-*cybernetic* epistemology" (HEYLIGHEN 1993). "Cybernetic" refers here to the broad domain of cybernetics and general systems theory (ASHBY 1956; VON BERTALANFFY 1968), and its transdisciplinary study of organization, communication, control and modeling. This epistemology is part of the larger evolutionary-cybernetic philosophy which, together with others, I am trying to develop in the Principia Cybernetica Project (JOSLYN/HEYLIGHEN/TURCHIN 1993). Compared with a purely evolutionary approach, a cybernetic epistemology puts more emphasis on the structure of cognitive systems, on the processes by which they are constructed, on the control they provide over the environment, and on the communication of knowledge. Such a cybernetic analysis, for example, allows the reinterpretation of CAMPBELL's (1974) "nested hierarchy of vicarious selectors" as the result of a series of *metasystem transitions*, producing subsequent control levels (HEYLIGHEN 1995).

The ideas of cybernetics inspired much of CAMPBELL's work, as illustrated by his recurring references to the work of ASHBY, his long time support for the perceptual control approach of POWERS (1973), and his enthusiastic endorsement of the Principia Cybernetica Project. However, I guess it was a lack of expertise in the mathematical and computational models of cybernetics which kept him from using the "cybernetics" label more explicitly.

Different Co-selectors of knowledge

A large part of CAMPBELL's (1997) paper, which provides the focus of this memorial issue, is devoted to a discussion of the different selectors which together determine the evolution of knowledge. The main thrust is that the *referent*, i.e. the external object which the knowledge is supposed to represent, only plays a relatively small part in the selection of a particular idea or belief. In spite of its ideology, scientific knowledge too is the product of multifarious selective forces, most of which have little to do with objective representation of the referent. Of the other co-selectors, CAMPBELL pays special attention to the vehicle through which the knowledge is expressed and to the need to maintain the community which carries the knowledge. In addition to these primarily social selectors, he discusses the selectors of interests and historicity, which function on the individual level.

Whereas CAMPBELL analyses these selectors *structurally*, that is, by the specific object or component responsible for the selection, I will here try to classify them *functionally*, that is, by the role they play in the evolution of knowledge. The class of selectors that promote the same type of characteristics can be said to determine a *selection criterion*. Implicit in CAMPBELL's examples, we can find three superclasses: objective criteria (selection for fit to the outside object), subjective criteria (selection for assimilation by the individual subject) and intersubjective criteria (selection for sharing between subjects). These superclasses can be divided into more fine-grained subclasses. The resulting classification will allow us to highlight the differences between scientifically derived knowledge, which supposedly privileges the objective criteria, and other types of knowledge and belief, where subjective and intersubjective factors play a larger role.

However, as CAMPBELL emphasizes, it is impossible to really *separate* the different selectors. All the different types of selectors will affect the evolution of knowledge, scientific or other. Therefore, the overall probability for a belief to be selected will be a kind of weighted sum of the degrees to which it fulfils each of the criteria. For example, an idea that scores high on the objective criteria and low on the subjective ones, is less likely to survive selection than an idea that scores high on both counts. In this view, no single criterion can guarantee selection, or provide justification for a belief. We can only use the simple heuristic that the more criteria an idea satisfies, and the higher the degree of satisfaction, the "fitter" it is, and the more likely to win the competition with rival beliefs (HEYLIGHEN 1993).

In such a view, there is in general no single "best" idea. An idea may score high on certain criteria, while another idea scores high on other criteria. Such ideas are in general incomparable. The one is likely to win the competition in certain contexts, but to lose in others. This is similar to the natural selection of organisms: sharks are not more or less fit than seaweed or than shrimps. Each species is adapted to its particular niche within the larger shared environment. However, within the shark niche, some shark designs will be fitter than others. In both organisms and beliefs, "being fitter than" is a *partial order*, not an absolute one (HEYLIGHEN 1997). Such a philosophy synthesizes the relativism of philosophers who emphasize the "incommensurability" of theories, with the more traditional belief in the objectivity of scientific progress.

Objective Selection Criteria

Since, as CAMPBELL (1997) reminds us, we have no direct access to the "Ding an Sich", we can only use indirect means to determine whether a belief corresponds to an objective reality. Like the constructivist cyberneticians VON FOERSTER (1981), and MATURANA/VARELA (1987) note, in the nervous system there is no fundamental distinction between a perception and a hallucination: both are merely patterns of neural activation. However, subjectively most people have no difficulty distinguishing dreams or fantasies from perceptions.

To find out whether a perception is real, you should determine whether it is caused by an external referent, or by an internal mechanism (e.g.,, imagination, or malfunctioning of the perceptual apparatus). According to attribution theory (KELLEY 1967), people attribute causes of perceived effects to those phenomena that *covary* with the effects. External phenomena will covary with their external causes, but not with changes that only affect internal, subjective variables. This leads to the following criteria for judging objectivity or "reality":

1. *Invariance:* phenomena should not disappear when the way of perception is changed. The larger the domain over which it remains invariant, the more "real" it will be (cf. BONSACK 1977). KELLEY (1967) proposes the following more specific types of invariance:

a. *invariance over modalities*: if the same phenomenon is perceived through different senses (e.g., sight and touch), points of view, or means of observation, it is more likely to objectively exist.

b. *invariance over time*: a perception that appears or disappears suddenly is unlikely to be caused by a stable referent.

c. *invariance over persons*: a perception on which different observers agree is more likely to be real than one that is only perceived by a single individual.

2. *Distinctiveness*: different referents produce different perceptions (KELLEY 1967; cf. CAMPBELL 1992). A perception that remains the same when the attention

is directed elsewhere is likely to be produced by the perceptual system itself (e.g., a particle of dust in the eye). Moreover, "real" perceptions tend to be characterized by richness in contrast and detail (imagined or dream perceptions typically are coarse-grained and fuzzy) and to exhibit "Gestalt qualities", such as regularity, closure and simplicity, thus proposing a distinct, coherent pattern, rather than an unstructured collection of impressions (STADLER/KRUSE 1990). CAMPBELL (1966 1997) too notes that detailed pattern increases the plausibility of percepts.

Extending this logic of covariation, I would like to add the criterion of *controllability*: a phenomenon that reacts differentially to the different actions performed on it, is more likely to be real than one that changes randomly or not at all. This criterion underlies the method of preparation-detection, which characterizes scientific experimentation. Controllability, however, is to some degree dependent on the observing subject: although I am not able to influence the trajectory of a far-away plane, its pilot is. This leads us to the subjective criteria.

Subjective selection criteria

For beliefs to be accepted and retained by an individual, it is not sufficient that they correspond to distinct, invariant and controllable phenomena. A relativistic quantum field model of the beryllium atom may fulfil all objective criteria to be valid knowledge, yet very few people would ever assimilate, remember or pass on such knowledge. Therefore, from a selectionist point of view, the model is rather unsuccessful.

Most obvious among the subjective selection criteria is *individual utility*. People will only do the effort to learn and retain an idea that can help them to reach their goals. From a long-term evolutionary perspective, such goals and values derive from inclusive fitness. Organisms that assimilate knowledge which increases their fitness are more likely to survive and pass on that knowledge to their offspring. Assimilating useless knowledge, on the other hand, only burdens the subject.

Indeed, the capacity of a cognitive system is limited. Therefore, knowledge should be easy to learn. The most straightforward determinant of learning ease is *simplicity*: the more complex an idea (i.e. the more components and more connections between components it has, see HEYLIGHEN 1997), the higher the burden on the cognitive system. Simplicity is listed as a subjective criterion, because it is relative to the concepts and associations which the subject already knows. For example, the model of a beryllium atom may seem simple for a physicist well-versed in atomic models, but hopelessly complex for a layman.

More generally, the ease with which a cognitive system assimilates new ideas depends on the support they get from ideas assimilated earlier (this is an example of CAMPBELL's (1997) "historicity"). This requirement for ideas to "fit in" the existing cognitive system may be called *coherence* (THAGARD 1989). Coherence encompasses connection and consistency. Since learning is based on strengthening associations, ideas that do not connect to existing knowledge simply cannot be assimilated. The preference for consistency follows from the fact that a fit individual must be able to make clear-cut decisions. Mutually contradictory rules ("cognitive dissonance") will create a situation of confusion or hesitation, which is likely to diminish the chances for survival.

Complementary to the conservatism promoted by the coherence criterion is the criterion of *novelty*. New, unusual or unexpected ideas or perceptions tend to attract the attention, and thus arouse the cognitive energy which will facilitate their assimilation. This is another adaptation, which helps organisms to cope with unusual situations. It shows itself in the exploratory behavior of animals. The corresponding human emotion is curiosity.

Intersubjective selection criteria

Most of the beliefs a subject has were not individually constructed, but taken over from others. This process of diffusion plays an essential part in the selection of ideas. Only ideas that are transmitted frequently are likely to be assimilated frequently. Each time an idea is communicated, it replicates, i.e. is copied into another cognitive system. Thus, ideas can be modeled as replicators similar to genes: *memes* (cf. HEYLIGHEN 1992). The conversion of an individual to a new belief is in a way similar to an infection, i.e. the passing on of a "cognitive virus".

The first criterion which will determine how often an idea is transmitted is the amount of propaganda or *publicity*, that is, the effort the subject carrying the idea invests in making it known to others. That motivation largely depends on the other criteria: you will be more inclined to spread an idea if it is simple, useful, novel, etc. However, some beliefs include their own motivation. This is most visible in religions, cults, fashions and ideologies, which often include explicit rules that believers should go and spread the word. This may be explained by "selfish meme" selection (HEYLIGHEN 1992): selection at the level of the meme, which benefits the spread of the idea, but which is useless or even dangerous for the individual carrying the idea. Such ideas can be compared to cognitive parasites, which "hitch a ride" on a cognitive system without caring for the well-being of that system.

All memes, "selfish" or not, need a communication medium in order to be transmitted. Ideas that are easy to express in a particular language or medium will be propagated more easily. This is the criterion of *expressivity*. It depends on the medium: some ideas are easier to formulate in one language than in another. Thus, like CAMPBELL (1997) notes, the medium will co-select the idea. For example, it is difficult to imagine the evolution of physical theories without the mathematical language in which they are formulated.

The expression of an idea in an intersubjective code or language does not yet guarantee its accurate transmission. All expressions are to some degree indexical: their meaning depends on the context. Different people are likely to interpret them differently, thus assimilating an idea different from the one that was expressed. However, some expressions are formulated in less context-dependent way. The resulting lack of equivocation may be called *formality*. The more formally an idea is expressed, the better it will survive repeated transmissions. For example, ideas are more likely to be communicated accurately through logic and mathematics than through poetry or painting.

The group equivalent of usefulness may be called *collective utility*. Some forms of knowledge benefit the collective, while being useless for an isolated individual. Languages, traffic regulations, technical standards and moral codes are examples of cognitive entities that have value only for intersubjective purposes. Such collective ideas will be selected at the group level: groups having such beliefs will be more fit than groups lacking them. This how the supernatural cosmologies characterizing archaic civilizations discussed by CAMPBELL (1997) have been selected.

However, as CAMPBELL emphasizes, such group selection often runs counter to the more powerful and direct force of individual selection. Therefore, he proposes a mechanism that suppresses individually selfish deviations from these collective beliefs: conformist transmission. As illustrated by the mathematical model of BOYD/RICHERSON (1985), all other things being equal, it seems evolutionarily optimal for subjects to adopt the majority or plurality belief rather than a minority idea. Thus, already popular ideas tend to become even more popular, leading to an eventual homogeneity of belief within a closely interacting group. This selective pressure may be called *conformity*.

Complementary to this homogenizing influence, we find the diversifying effect of the division of labor. Because of their limited cognitive capacity, individuals within a complex society tend to specialize in a particular domain. As illustrated by GAINES's (1994) computer simulation, this process of cognitive differentiation is driven by a positive feedback mechanism: individuals who were successful in solving a particular type of problem will get more of these problems delegated to them, and thus develop a growing expertise or authority in that domain. The backing of a recognized expert will contribute to the acceptance of a particular idea. This is the criterion of *authority*.

The integration on the level of norms and codes fostered by conformity and the differentiation on the level of expertise fostered by authority together produce a complexification (cf. HEYLIGHEN 1997) of the social system. The process is similar to the metasystem transition which produced the differentiated organs and tissues in a multicellular organism (HEYLIGHEN/ CAMPBELL 1995; HEYLIGHEN 1995).

Scientific Validity and the Demarcation Problem

The selection criteria we discussed are summarized in table 1. When we look at the evolution of scientific knowledge, it is clear that *all* these criteria play a role in the selection of ideas. The objective criteria obviously underlie the experimental method: new concepts are operationalized by specifying the observations that will distinguish their referents, by

Objective Criteria	Invariance
	Distinctiveness
	Controllability
Subjective Criteria	Individual Utility
	Simplicity
	Coherence
	Novelty
Intersubjective Criteria	Publicity
	Expressivity
	Formality
	Collective Utility
	Conformity
	Authority

Table 1: summary of the proposed selection criteria

subjecting them to controlled experiments, and by trying to find results which are maximally independent of place, time, observer or means of observation (as CAMPBELL (1997) notes, the latter is often difficult in the social sciences). Moreover, subjective interpretation is minimized by formalization of the theories and concepts. However, from otherwise equivalent ideas, scientists will still tend to prefer those that may bring fame and fortune, that are simple, coherent with what they already know, and novel. In addition, the social system of science will prefer ideas that have vocal advocates, are strikingly expressed, benefit the community, and are supported by the majority, or by authoritative experts.

In what way, then, can science be demarcated from other knowledge producing systems, such as religion, fashion or tradition? The difference is that science *explicitly* promotes the objective criteria. (To a smaller degree, as CAMPBELL (1997) notes, science also tries to neutralize the criteria that are likely to detract from objectivity, such as authority which is not backed by expertise, conformity for conformity's sake, and—at least in the pure sciences—utility.) The objective criteria have been built into the scientific method. They have become part of knowledge itself, rather than an outside force to which knowledge is subjected. The scientific method, in CAMPBELL's (1974) terminology, is a vicarious selector, an interiorization of external selectors.

This vicarious selector functions at a higher hierarchical level than the knowledge it produces. Because other forms of knowledge are not selected at this higher level, they will evolve in a less efficient way, and are therefore likely to be of lower quality. The difference between scientific and other knowledge is not an absolute one, between objective and subjective, or between justified and unjustified, but one of degree, between the products of a systematic process of improvement, and those of a slow, haphazard process of trial-and-error, where neither trial nor error are consciously controlled.

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Aharon Kantorovich

The Rationality of Innovation and the Scientific Community as a Carrier of Knowledge

Abstract

It is suggested that epistemological considerations may not be restricted to the process of selection, as CAMPBELL maintains. Psychological and social mechanisms of generating new variations in science are shown to yield some conclusions about rational ways of creating scientific ideas and theories. Further conclusions are drawn from the function of the scientific community as a carrier of knowledge. Departure from tradition is made possible by unintentionality and by exploiting existing ideas for solving new problems. The norms of the scientific tribe should encourage this patterns of behavior.

1. The Balance Between Selection and Generation: Socio-Evolutionary Mechanisms for Generating New Variations in Science

CAMPBELL adopts the view that the social dimension is essential to science. Scientific beliefs are beliefs produced by a social system. If they have greater validity than those produced by other systems, this is due to the norms of the scientific community. Thus, science has different specific values, myths, rituals and commandments. The question is therefore what are these norms and what is their epistemological significance.

In CAMPBELL's text we can find the following characterizations of science as a social system: science is one of the "superstition preservation systems" and the scientific community is "among belief-preserving mutual admiration society". However, the scientific community differs from other tradition-ridden groups by recognizing that "tradition is a source of error". If there is a way of eliminating error, it would guide scientists in departing from tradition. This is the point where Natural Selection enters into the picture: it supplies the model for creativity.

The three main ingredients CAMPBELL borrows from the DARWINIAN evolutionary dogma are: variation, selection and retention. But he typically concentrates on selection. He confronts "selection by nature" with "selection by the scientific community". This is portrayed almost as a dichotomy: "It becomes hard to argue for a dominant role for 'Nature Herself' in the selecting." But how does the scientific community select? Or what are the criteria for "the generation, publication, teaching and believing of scientific truth claims"? It seems that in natural science (but not in the humanities and perhaps also in social science) the scientific community interacts with 'Nature Herself' via reliable means, such as experiment and logic. So the criteria for publication, for example, reflect predominantly the scientists' broad ('universal') interest of exposing truth rather than local or sectarian interests or personal tastes.

Selection is according to CAMPBELL the main epistemological tool in science, since "validity is attributed to the selection process" rather than to generation. Indeed, a message preached by Karl Popper and adopted by CAMPBELL is that in science, as well as in DARWINIAN evolution, generation or variation is 'blind', namely does not have any methodological implications. However, generation is too important to be left to mere chance. Generation should yield a limited number of plausible variants from which to select, otherwise selection would be a futile game and progress will be impossible. Scientific rationality thus depends on both variation and selection.

CAMPBELL diminishes the role of variation in line with the spirit of logical empiricism which expels discovery from the realm of rationality. As the distinction between the context of justification (which is considered to be governed by logic and reason) and the context of discovery (thought to be beyond the reach of reason) has been blurred we have to deal with the contribution of both contexts to the rational conduct of science. Scientists are not engaged with criticism for its own sake; criticism goes hand in hand with generating or discovering new ideas or theories. Often the overthrow of an existing theory occurs only after a better theory is found. Furthermore, there is a widely accepted view that a replacement of a theory by a new one is the only viable way of 'refuting' the theory (LAKATOS 1970). Thus, discovery cannot be treated separately from selection and if "validity is attributed to the selection process", it should be attributed to discovery as well.

Moreover, as we shall see, there are ways of generating new ideas which enhance the chances for these ideas to become discoveries, regardless of questions about their validity. For example, there are certain ways of combining ideas, or ways of making associations between different ideas, which lead more often to discoveries. Hence, generation is not entirely beyond the scope of reason. It was suggested (KANTOROVICH/NE'EMAN 1989) that within the framework of evolutionary epistemology 'blind' discovery be interpreted not just as random variation but as serendipitous discovery, specified in the following way. When scientists generate, or look for, ideas they are not engaged with sheer gambling. They are supposed to exploit intrapsychic and interpsychic mechanisms (SIMONTON 1988) which yield involuntary mental and social processes respectively. These processes can be characterized as 'blind' since eventually scientists solve via these mechanisms different problems than those they intended to solve originally. The intrapsychic creative mechanisms are not exclusive to scientific thinking. They appear also in ordinary creative thinking. SIMONTON describes such a creative process as generated by chance association or combination of mental elements (such as ideas, formulas or images). The process may end up with the emergence of a stable configuration. The latter is the product of creative thought, i.e. a discovery or an invention. This process can be represented as a semi-blind variation and selection, where the principle of selection is related to stability.

Thus, rationality in the domain of discovery would require that scientists follow some norms of conduct in order to take advantage of the creative mechanisms. These norms can be derived from what we know about the nature of these mechanisms. Rationality in this domain would therefore reflect our established theoretical knowledge about these phenomena which take place in our minds. For example, the above theory of forming chance configurations would imply the following rules for fostering or cultivating creativity. Scientists should be engaged simultaneously in several problems; they should not be one-track-minded. Thus, in their efforts to solve a given problem, they may generate or encounter an idea or mental element which was missing in the process of forming a stable configuration for solving a different problem. In this way their creativity will be enhanced. And more generally, they should be alert so that if in the course of their efforts to solve one problem they come up against an idea which gives a clue for solving another problem, they should seize this opportunity for solving the other problem instead of insisting on solving only the original one. Or, if scientists are not successful in solving a given problem they may turn to another. And after acquired the skill of using a given (experimental or theoretical) tool, they might look for other fields where it can be exploited—this is the basis for the phenomenon of intellectual migration.

The scientific community has at its disposal another source of creativity: the interpsychic process of epistemic cooperation. Here the combination of ideas takes place in the social arena. To the extent that science is essentially a social phenomenon (see for example ZIMAN 1968), this process perhaps distinguishes science from all other human activities. A set of social norms and codes of behavior would guide scientists in exploiting this extra dimension of creativity. Epistemological cooperation fosters unintentionality and serendipity. Thus an idea or theory which were generated by one scientist (or group of scientists) in order to solve a given problem may be employed by other members of the community in order to solve an entirely different problem. Again, we interpret this process as another kind of 'blind' variation since the original discoverer of the idea was not aware of the problem which is eventually solved by his colleagues or predecessors; the initiator of the process was blind to the problem solved. The coupling between both kinds of variation—the intra- and the inter-psychic—is what makes science such a powerful novelty-generating evolutionary system.

The following codes of behavior would guide scientists in exploiting the interpsychic mechanisms of discovery. Scientists have to use the communication network of the scientific community; to attend conferences, to read papers published in the field and to submit papers for publication in scientific journals, and so on. And there are further recommendations to the scientific community: epistemic cooperation should be encouraged by establishing appropriate norms of conduct through the scientific education system, the award system and other institutions.

Thus, according to this view, it is not true that validity or rationality is attributed only to the selection process. The mechanisms of generating new variations in science yield some conclusions about the rationality of creat-

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ing scientific ideas and theories so that the context of discovery or generation does contribute to the rationality of science. Rationality here is not full-proof; it does not necessarily lead to true ideas or theories. However, it complies with the nature of our knowledge-acquisition system. And in an atmosphere of post-positivism and naturalism we cannot ask for more than this.

2. Carriers of Knowledge

CAMPBELL distinguishes between embodied (endosomatic) and disembodied (exosomatic) knowledge. Referring to embodied knowledge he says: "the vehicular substance that carries knowledge is unavoidably alien to the referents of knowledge—it is a different substance with different structural characteristics". Thus, the horse's hoof embodies knowledge or information about the earth's surface. We have here one material system (the carrier of knowledge) which has been adapted to another (the referent of knowledge) through the evolutionary process.

Light will be shed upon the relation between the carriers and the referents of knowledge if we treat the first as hypotheses (theories) and the latter as the facts to be explained or accounted for. An organ, such as the horse's hoof, embodies a hypothesis about a portion of the environment, i.e., about the earth's surface. The fact that the vehicular substance here is alien to the substance which roads are made of corresponds to the methodological requirement that an explanatory hypothesis should be expressed in theoretical vocabulary which is alien to the observational vocabulary. For example, the story of the kinetic theory of gases is told in terms of particles in motion, whereas the observational story is told in terms of volumes, temperatures and pressures.

To continue the analogy, the relation between an organ and a species corresponds to the relation between a theory about a particular kind of phenomenon and a general theory explaining a wide range of phenomena.

Disembodied or abstract knowledge consists of ideas, beliefs and theories. This is the kind of knowledge traditional epistemology is engaged with; abstract knowledge detached from its material carrier. The following parallelism exists between the two kinds of knowledge. There are restrictions imposed on the way one material system (an organ) represents another. The restrictions are determined by the nature or structure of the representing system. In a similar fashion, the restrictions imposed on disembodied knowledge are determined by the set of categories or concepts available to the representing system.

A reptile's belly, the horse's hoof and the human foot are organs representing different facets of the earth's surface. The rhinencephalon of lower mammals, the neocortex of humans and the scientific community do not embody representations of phenomena, rather they are different ways or methods of coping with reality: they guide the different species in controlling their internal and external environment, constructing representations (such as theories) and explaining the phenomena. The brain and the scientific community are both producers and carriers of knowledge. CAMPBELL discusses the function of the scientific community as a vehicle of knowledge. As a vehicle of knowledge it should have some rigidity "to hold together the picture it carries". Rigidity means being faithful to tradition. The opposite alternative is that the vehicle is completely flexible. A transparent carrier of knowledge "contributes least of its own to the knowledge it transmits". This means that the social structure of a scientific community, or its 'tribal' nature, which contributes to the rigidity of this vehicle of knowledge is what holds together the scientific world picture. And CAMPBELL raises the important question: what makes a tribe of scientists capable of effectively generating "valid" scientific knowledge, what demarcates it from other "superstition preservation systems" or what makes it superior to every other "belief-preserving mutual admiration society"? CAMPBELL maintains that the selectionist model provides the answer. But this is only a partial answer. It seems that the model of unintentional or serendipitous innovation completes the answer. Departure from tradition is made possible by unintentionality and by exploiting existing ideas for solving new problems. The norms of the scientific tribe should encourage this patterns of behavior.

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Elias L. Khalil

Can Artificial Selection Remedy the Failing of Natural Section with Regard to Scientific Validation?

Abstract

Given the difference between natural and artificial selection, CAMPBELL finds artificial selection the more appropriate of the two for the depicting scientific validation. Natural selection, which emphasizes the context, entails a relativist view of science. Artificial selection, insofar as it presents selectors as interested in empirically supported theories, implies that the scientific enterprise is superior to other belief-preserving tribes. However, artificial selection, like natural selection, still adheres to the tenet of blind mutation. This tenet defies the fact that scientists—contrary to the core of neo-DARWINISM—propose new theories in light of their ex post effect.

This commentary is concerned only with one aspect of Donald CAMPBELL's (1997) multifaceted essay. Namely, CAMPBELL advocates an evolutionary epistemology theory which somewhat differs from natural selection. Section 1 finds that CAMPBELL's theory remedies the relativist connotation associated with the natural selection account of scientific validation. Section 2 argues that, like natural selection, CAMPBELL's theory is inadequate to account for the origin of variety of scientific theories.

1. Is the Criterion of Selection of Scientific Theories Self-Referential?

CAMPBELL recognizes the limits of natural selection when applied to the question of scientific validation. The theory of natural selection postulates that a population, abstracting from secondary hurdles, ends up with a mix of traits which is optimum given the selective environment. Such optimization, called 'adaptationism', guarantees that the mix of traits which become dominant is the most useful mix given the initial variety from which a population starts. Given that usefulness is defined by the environment, one cannot apply such a scenario to depict the process of scientific validation. It would entail that the dominant theoretical paradigm is dominant because of the peculiar, faddish tastes of the selectors, i.e., scientists. If such a relativist view of science is correct (KUHN 1970), the dominate paradigm could not, in opposition to alternative paradigms, claim superiority with respect to the approximation of truth. Paradigms would be incommensurable; they would be ultimately justified on self-referential grounds, i.e., internal criteria similar to the ones which sustain belief-preserving tribes.

CAMPBELL maintains that, at least according to professed norms, the scientific tribe does not operate according to what is *useful* but rather according to what is *true*. The disjuncture between usefulness and truth has prompted CAMPBELL to argue for a "general selection theory," where natural selection is only one variety while evolutionary epistemology is another. For CAMPBELL, natural selection (or what he calls "biological selection") is governed by the usefulness criterion, while epistemological selection is governed at least partially by the truth criterion. The latter selection is evident when members of the scientific tribe appeal, at least formally, to 'the way the world is' as the ground upon which to adjudicate among competing theories.

In support of CAMPBELL's thesis, the disjunction between usefulness and truth informs the difference between technology and scientific theorizing or, in general, between practical knowledge and theoretical knowledge. The ultimate concern of practical knowledge, which informs the foundation of vocational training from medicine and engineering to business administration, is how to fix a situation and make it more conducive to human welfare. The ultimate concern of theoretical knowledge, which informs the foundation of disciplines from sociology and biology to physics, is why are things the way they are. Obviously, producers of practical knowledge may use theoretical knowledge, and producers of theoretical knowledge may use tools and technologies afforded by practical knowledge. Nonetheless, each one pursues a different end. While the end of theoretical knowledge is the satisfaction of curiosity, the end of practical knowledge is human welfare.

The criterion employed to judge a technology is whether it affords the actor an advantage over competitors and, hence, advances human welfare. The assumption that actors are rational does not mean that they have the same technology. It only entails that they use the resources and technology they possess in the most efficient fashion. It is up to natural selection, which can be dubbed "market selection" in modern human production activity, to select the most useful technology. Like biological selection, market selection involves consumers acting as selectors. The dominant technology is the one which fits the tastes of consumers. As in the case of biological traits, one cannot determine in the abstract which is the fittest technology. It is possible that the most productive technology, measured by units of output per labor-time, is not the fittest. The cost of development and maintenance of the most productive technology may not justify it *vis-à-vis* more simpler technology. A technology which can be justified under certain circumstances can become less fit under new circumstances.

Therefore, market selection, similar to biological selection, justifies technology or practical knowledge according to the usefulness criterion. Both market and biological selections involve the natural selection mechanism: The dominant trait or technology is not valid in the abstract sense, but rather valid only in relation to the peculiar selective environment. In contrast, according to CAMPBELL, the scientific enterprise is about validation in the abstract sense. The market of scientific theories is not supposed to generate theories according to fashion or practicality but rather, at least formally, according to truth. If one applies natural selection to the market of scientific theories, i.e., treating theories as no different from tables and tools, one would be subscribing to a relativist theory of science (KHALIL 1995).

To counteract the connotation of relativism, CAMPBELL argues that selection theory should not be identified with natural selection. Evolutionary epistemology, according to CAMPBELL, posits that the selectors do not weed out theories which are least useful, but rather weed out theories which are least corroborated by the world of facts. Despite the innovation that selectors are committed to the norm of truth-seeking, evolutionary epistemology still adheres to the three conditions which characterize natural selection, viz., "a mechanism for introducing variation, a consistent selection process, and a mechanism for preserving and reproducing the selected variations." First, the blind mechanism for variation is trial and error, where scientists propose diverse theories to illuminate particular facts. Second, the selection process consists of a scientific tribe whose members, despite their pre-commitments and prejudices, practice at least partially the norm inherited from the ideology of the scientific revolution, viz., to choose the theory which best depicts 'the way the world is'. Although CAMPBELL sympathizes with the claims of the advocates of the relativist view of scientific validation, he maintains that the commitment to judge theory according to 'facts that speak for themselves' ultimately enhances the "competence of reference" of theories. The competence of reference makes the scientific community different from other, belief-preserving, mutual admiration tribes. Third, the scientific community reproduces the successful theory through the instruction of new practitioners.

I concur with CAMPBELL that evolutionary epistemology modified as such still preserves the main elements of natural selection and, hence, can be seen as a variety of a "general selection theory." To wit, such evolutionary epistemology can be characterized as "artificial selection"—similar to the selective breeding of domesticated animals and plants according to the preferences of the breeder. Charles DARWIN commenced the *Origin of Species* by an extensive discussion of artificial selection as a way to elucidate the theory of natural selection. However, there is one important and obvious difference between the two. The difference revolves around who is the beneficiary of the selection. In artificial selection, the beneficiary is the selector, whether it is the breeders who demand a more refined trait or the scientists who demand a better theory. In natural selection, the beneficiary is the population which is being selected so that the succeeding generation is more adapted to its environment.

2. What is the Origin of Variety of Scientific Theories?

CAMPBELL's recognition of the failing of natural selection with regard to scientific validation is refreshing. CAMPBELL's theory of evolutionary epistemology *qua* artificial selection goes a long way to remedy this failing. The question pursued in the rest of this commentary is whether CAMPBELL's theory of artificial selection is adequate with respect to another front, viz., the origin of variety of scientific theories.

CAMPBELL's model of artificial selection is based on a core assumption which is shared by all selection theories within the DARWINIAN tradition. DARWIN theory distinguished itself from other theories of evolution by its repudiation of teleology (GHISELIN 1969; see KHALIL, 1997): According to DARWIN, a species did not evolve by the volition of organisms in the face of environmental challenges. Rather, it evolved irrespective of the intention of organisms, i.e., as a result of the weeding out of the less fit organisms. For such weeding out to take place, the population should be characterized by variation. DARWIN's theory had to wait for the development of genetics in the 20th century to give meaning to the origin of variety. DARWIN's notion of pre-existing variety became sharpened in the neo-DARWINIAN synthesis with the notion that if variety changes, it arises from blind mutation of genes. The notion of blind mutation justifies the modeling of change of variety as exogenous. That is, the change in variety is not considered to be a function of environmental stresses. The idea of blind mutation amounts to the thesis that the genetic variety generated by mutation is independent of its *ex post* effect given the selectors.

The temple of neo-DARWINISM rises or falls with its central tenet of blind mutation. There are diverse approaches critical of neo-DARWINISM. However, what determines whether an approach is within or outside the neo-DARWINIAN program is whether it subscribes to the hypothesis of blind mutation. Soft neo-DARWINIANS may be critical of the reductionist tendency of orthodox neo-DARWINIAN (WILSON/SOBER 1989; ELDREDGE 1996) or the treatment of mutation as random, as if there is no 'structures' which persist through evolutionary change (GOULD 1977; GOULD/LEWONTIN 1979). However, such soft neo-DARWINIANS refrain from repudiating the central tenet.

The question is whether blind mutation is the appropriate description of the origin of variety of scientific theories. If selectors, following CAMPBELL, subject theories, at least partially, to the test of whether they are empirically supported, the suppliers of scientific theories must also have subject such theories to the same test prior to advancing them in public. That is, suppliers do not supply scientific theories in a blind fashion. Suppliers rather 'direct' the generation of theories, i.e., fashion them in order to meet the external criteria used by selectors. The advocates of scientific theories develop their hypotheses in light of the anticipated criticism of peers, the selectors. Even if there are no selectors, suppliers of scientific theories subject their products to the same criterion used by the selectors. Suppliers of scientific theories are also interested in the pronouncement of statements which they can accept on the same ground which they use to assess all other theories.

If this point is granted, it would be disturbing for any kind of selection theory in the DARWINIAN tradition. It would mean that variety, at least to some extent, is endogenous; the variety arises from the anticipation of *ex post* benefit. It would also entail that the selective environment does not play the crucial role portrayed by neo-DARWINISM.

CAMPBELL would probably have responded by admitting that originators of scientific theories do not advance them in a blind fashion. He would, however, probably continue and argue that such evident *directed* mutation on the part of originators of theories is the outcome of a "vicarious process." Similar to the vicarious processes of visual perception, creative thought, belief development through dialogue, and so on, the faculty of directing mutation is short-cut selection, i.e., originally achieved by blind-variation-and-selection-retention.

Even if we grant that the vicarious processes which CAMPBELL has identified had originated from natural selection, this does not mean that directed mutation has also risen from natural selection. And even if directed mutation has been occasioned by natural selection in the past, it does not attenuate the anomaly facing CAMPBELL's artificial selection. The idea that theories are directed mutations has a radical ramification. It amounts to the negation of the artificial selection account as if, to paraphrase Ludwig WITTGENSTEIN, scientists have kicked off the neo-DARWINIAN ladder of evolution (viz., artificial selection) once they have started to anticipate the *ex post* effects of their theories. To begin, why should one be concerned about the origin of directed mutation? The origin could be the result of an act of God, angels, natural selection, or whatever. What matters is the *consequence* of directed mutation, viz., the negation of selection theory as the primary explanation of evolutionary change.

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In short, CAMPBELL's theory of artificial selection succeeds in ameliorating the problem of relativism which connotes the natural selection account of scientific validation. However, artificial selection, similar to natural selection, adheres to the central tenet of neo-DARWINISM, viz., mutation of scientific theories are not correlated with *ex post* effects. Given that originators of scientific theories are self-critical—i.e., advance theories with *ex ante* expectations—selection theory, even of the artificial variety, fails to capture the core issue of scientific validation.

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Kyung-Man Kim

D. T. Campbell's Social Epistemology of Science

Vicarious Selection, Epistemological Fallibilism, and Sociology of Scientific Validity

Abstract

The concept of a 'nested hierarchy of vicarious selectors' occupies the central place in Donald CAMPBELL's evolutionary epistemology. After showing why vicarious selection process is so important in all knowledge processes, I will proceed to show how CAMPBELL incorporates this concept into his more recent vehicular theory of knowledge which stresses the embodied character of knowledge. Explicating these two main pillars of CAMPBELL's evolutionary epistemology will help us to understand how CAMPBELL develops a sociological model of scientific knowledge called a sociology of scientific validity which can counter the excessive relativism espoused by the relativist sociologists of scientific knowledge.

Introduction

Unlike his contemporaries Robert MERTON and Edward SHILS who polemically isolated themselves from the newly emerging sociology of scientific knowledge community, Donald CAMPBELL always has been eager to engage in a critical dialogue with the relativist sociologists of science. And during the last fifteen years before his death CAMPBELL has been concerned with developing a sociological model of scientific knowledge, called the "sociology of scientific validity". In contrast to the relativist model, it is able to explain how science can produce valid beliefs about the external world. CAMPBELL often calls his model a 'sociologized version' of POPPER's demarcation criterion since it seeks to demarcate science from non-science by asking how the scientific community, in contradistinction to the other belief perpetuating communities, can improve its "collective" goal of validly mapping the physical world. It thus comes as no surprise that developing such a sociological model of scientific validity takes the first priority in CAMPBELL's research agenda. As he wrote in his notes, his naturalistic epistemology of science eventually "will also have to be a sociology of scientific validity" (editors' introduction to CAMPBELL 1997).

In this short article, I will first briefly discuss the concept of the 'nested hierarchy of vicarious selectors' which is central to CAMPBELL's evolutionary epistemology. After showing why vicarious selection processes are so important in all knowledge processes, I will proceed to demonstrate how this concept can be fruitfully related to the current debates in the sociology of scientific knowledge. Examining CAMPBELL's writings on sociology of science, I will specifically argue that CAMPBELL's sociology of scientific validity can counter the excessive relativism espoused by the relativist sociologists of scientific knowledge.

Knowing as a Vicarious Selection Process

For CAMPBELL, knowing is a profoundly indirect process in which a punctiform certainty or accuracy in the mapping of the organism's environment is never warranted. Rather, gaining knowledge about the external world requires the organism to inevitably adopt a trial-and-error method. Let me illustrate this by citing CAMPBELL's favorite example. The leg of a lizard, when amputated, regrows to a length optimal for locomotion and survival. Such an optimal adjustment process, however, never implies the 'direct fit' of the organism with its environment. Rather, the leg length is controlled by the internalized 'vicarious' selector' which in turn is the product of previous natural selection. This vicarious selector embodies the 'already evolved wisdom' about the environment obtained through the trial-and-error of the whole mutant organism. Such a vicarious selector is only contingently related to the environment since the 'typical' environment against which such a vicarious selector has been developed can change. In a changed environment, therefore, the current vicarious selector will no longer be optimal. Take another example; while our eyes seem to give us a 'direct' and reliable knowledge in the normal environment, they delude us in different environments. Consider fog and glass as two such abnormal environments. While glass is transparent, it is nevertheless impenetrable; and while fog is opaque, it is nevertheless penetrable. The presumption that anything transparent is penetrable (and vice versa) is violated in such abnormal circumstances. Our vision— itself a vicarious selector—sometimes deludes us in an ecology which is *not* typical of the history of human evolution (CAMPBELL 1974).

CAMPBELL argues that such a vicarious selector—or shortcut—is obtained through the following three steps:

1. 'blind variation'
2. selection of variations
3. retention of variation.

Thus the 'homeostat' setting or 'reference signal' built into the developmental system of the lizard has been *originally* obtained through an *inductive* process which CAMPBELL dubbed the blind-variation-and-selective-retention process. In a changing environment, however, such a vicarious selector becomes inappropriate and becomes subject to the *more fundamental* natural selection process. Regarding such a 'nested hierarchy of vicarious selectors', CAMPBELL (1970, p55) thus argues that "What are criteria at one level are but 'trials' of the criteria of the next higher, more fundamental, more encompassing, less frequently invoked level".

Starting from the genetic adaptation as the most fundamental level in such a hierarchy, with increasing order, CAMPBELL enumerates various levels of vicarious selectors. They include non-mnemonic problem solving (i.e, direct locomotion without memory), vicarious locomotor devices, habit-instinct, visually guided thought, mnemonically guided or supported thought, social learning, language, and finally science, which is but an aspect of sociocultural evolution. From this discussion, we can find that the vicarious selector has at least three important implications regarding the process of knowledge acquisition. First, vicarious selectors are 'shortcuts' which abbreviate more 'wasteful' search, and in that sense are cost-effective; second, there will be a certain systematic bias resulting from the mismatch of the vicarious criteria with the changing environment; third, the increase of vicariousness of selectors in going up the hierarchical ladder means that, in a certain sense, the higher-level selectors presuppose, although presumptively, the validity of the lower level selectors in guiding generation and selection of variations at that level. This in turn means the presence of 'constraints' on the possible range of variations at that level. Thus, in science, as I shall elaborate below in more detail, while operating with the currently available vicarious selectors, scientists are groping for more 'adequate' or more 'valid' vicarious selectors which can better represent the physical world lurking out there.

Vehicular Theory of Knowledge

In his William James Lecture delivered at Harvard University in 1977 (CAMPBELL 1988), CAMPBELL spelled out some of the implications which his vicarious selection theory has for the naturalistic epistemology of science. Plugged into the naturalistic epistemology, the nested hierarchy of vicarious selection process implies the dependence of the higher level knowledge processes on the general validity of the lower level ones which are interpreted as developed earlier in the evolutionary time scale. In science, we can distinguish at least two kinds of vicarious selectors (KIM 1992). First, in any given period of time, we can find certain standards of scientific rationality or acceptability usually represented by the models, theories, inscription devices (LATOUR 1987), and even metaphysical principles (GALISON 1987), and these constitute the vicarious selectors which contribute to the selective propagation of specific ideas from a larger pool of ideas. These standards or vicarious selection criteria are in fact the results of the *previous* selections, and in that sense, have what CAMPBELL calls "historicity" (1997, p15). Having been selected and retained because they have done better than their conspicuous rivals in the past, these criteria or standards are believed to represent the world in a fairly valid way. But, as we have seen above, these criteria are at most imperfect, and has only a heuristic value for the representation of the physical world. They are only tentatively held as true until some anomalies or counter-examples are encountered. Also, as noted above, they tend to have a systematic bias in representing the physical world.

The second type of vicarious selectors are those which have been used by the sociologists of knowledge to explain the vicissitudes of beliefs observed in different cultures and scientific paradigms. In contrast to the first type, the second type is historically conditioned by the factors *inimical* to the selection of valid beliefs. In his first paper on the sociology of science entitled, "The Tribal Model of Social System Vehicle Carrying Scientific Knowledge" (1979), CAMPBELL introduced what he called the 'vehicular theory of knowledge'. In this theory, knowledge is conceived to be the compromise between the physical or vehicular requirements of the system that carries knowledge and the properties of its referents. In such a 'descriptive epistemology' (CAMPBELL 1977, 1979, 1988), scientific knowledge is conceived as 'embodied' in a physical vehicle or carrier which has its own physical nature and 'limitations' dictated by its structure. Just as the knowledge embodied in an individual organism is a 'compromise' between its structural and physical requirements and the external referents which are alien to the organism, so is the knowledge embodied in a social vehicle called scientific community. In introducing this 'vehicular theory' of knowledge, CAMPBELL shows his subscription to the hypothetical realism in which the operational independence of knowledge and that-which-is-to-be-known is emphasized. Such structural requirements that are necessary for the continuity of the tribal solidarity are however at the same time inimical to the selection of valid beliefs. For CAMPBELL, not only individual scientists have their own biases and preferences which tend to distort the valid mapping of the physical world, but also a group of scientists has its 'organizational requirements' which dictate the 'trade-off' between keeping the vehicle intact and validly mapping the physical world. In fact, KUHN's (1962) stress on the conformity pressure among the members of the paradigm well illustrates such vehicular requirements. Thus CAMPBELL argues that:

> [T]he 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 [knowledge] it carries. These vehicular-structure requirements produce not only restrictions on fineness of detail, but also bias and limitations of aspect (1979, p184).

According to CAMPBELL, the 'strong/constructivist' sociologists of scientific knowledge (BARNES 1982; BLOOR 1991; COLLINS 1985; LATOUR 1987) in fact demonstrate that the 'tribal' requirements such as local laboratory culture and practices, social and professional interests, training, and power structure of the scientific community rather than Nature herself play an important role in the maintenance and change of collective beliefs in science. But in his debate with David BLOOR (1989), CAMPBELL (1989) argues that the relativists fail to specify the role played by the natural world, however small, in the selection of beliefs. Thus, against BLOOR's argument that, given a particular need of a group, there is no reason why such a social group cannot invent a word for the "three centimeters of a leaf", CAMPBELL argues that the "entifiability of the natural world edits words for a socially pragmatic degree of shared reference" (1989, p154; see also KIM 1992).

Epistemological Fallibilism

The vehicular theory of knowledge discussed above is indeed a 'descriptive epistemology' delineating *how* the current social system of science produces knowledge. But, as CAMPBELL repeatedly stresses, his descriptive epistemology never precludes the possibility of a specific kind of normative epistemology. As I shall discuss below, this is what CAMPBELL attempts under the heading "sociology of scientific validity". Given a descriptive picture of science, the next step is to "design" a social system of science in which the role of the natural world as reflected in the research data can be 'maximized'. Thus, CAMPBELL argues that, "Descriptive epistemology will need eventually a physical theory of *optimal* vehicles … How are these physical requirements … related to the revising, expanding, and improving embodied knowledge? Is total substitution of a different portrait [of physical reality] generally more mechanically feasible procedure than retouching it? (CAMPBELL 1997, p17). He recommends the latter strategy.

To illustrate what he has in mind, CAMPBELL introduced the notion of *doubt-trust ratio* by using a biological example in which the mutations and recombinations of genes producing new variations in the modal inherited form represents the 'doubt' (or innovation) and the retained ancestral genes and gene complexes represent the trust (or tradition). Now, in any given generation, the doubt-trust ratio is overwhelmingly in favor of trust. Of course, it is clear that, over a number of generations, all parts of the original gene complexes may be replaced. The point here is that, if progress is to be made, both in the biological and in the scientific realm, the doubt-trust ratio should be very small. As is well known, this view of scientific change is widely endorsed by philosophers including QUINE, POPPER, and HESSE. QUINE's (1964) rejection of the analytic-synthetic distinction, POPPER's (1959) background knowledge in theory testing, and HESSE's (1974) concept of degree of 'entrenchment' of predicates in a theoretical network all attest to their conviction that *any parts* of the theoretical system is in principle revisable. They all concur that only by a *fallibilistic trust* in the large part of the system, can science grow.

Given this, we can ask: at what level of such a hierarchy of vicarious selectors the most adaptive modifications to improve the fit with the physical world are likely to occur? CAMPBELL's nested hierarchy suggests that when an anomaly is found at a specific level, it is fed into the more general, deeply entrenched, and less falsifiable level. And if the solutions cannot be found at that more fundamental level, one should check the still more fundamental vicarious selectors and so on until the most basic and deeply entrenched level is checked out for its validity. Let me quote CAMPBELL (1977, p51) on this point:

> Many of those who study and theorize about conceptual change in science observe sets of presumptions and decision rules which vary hierarchically in the reluctance with which the scientific community can or will change them. The conceptual changes within KUHN's normal science are lower in such a hierarchy than the basic paradigm changes of scientific revolutions. Of course, many more levels are needed than just these two… Perhaps the scientist's hierarchy of reluctance to change presumptions works backward down this developmental sequence, with the evolutionary earliest being the most tenaciously adhered to. I also feel some value in thinking about change in science in terms of the metaphor of a nested hierarchy of vicarious selection processes. Applied to science, most scientists can be seen as exploring local alternatives holding a large number of beliefs fixed as vicarious selectors. Occasionally there are periods of varying some of these tentative selectors, and still less frequent variations on still more reluctantly given up selector-beliefs— and so on up to the least frequent most fundamental variations called scientific revolutions.

Such an emphasis on the hierarchy of vicarious selectors— or presuppositions— leads CAMPBELL (1986) to adopt the metaphor of a "hermeneutic circle" or "spiral" to characterize the "validity-enhancing" aspects of science. Granted that knowledge process starts from a certain kind of prior understanding— represented by the presumptive and fallible vicarious selection criteria—more and more hidden presuppositions and assumptions become known and articulated in the process of developing and improving knowledge. Of course, we can neither make all of these presuppositions explicit nor can we go back to the 'absolute' beginning of them. However, something can be gained when we move freely between different levels of presuppositions and assumptions. For CAMPBELL, the social system of science must be designed in such a way that it can facilitate such a hermeneutic tacking procedure. In such a community of 'truth seekers', CAMPBELL argues, scientists with different theoretical orientations stay together in a focused disputation, attending and monitoring each other's arguments and illustrations and keeping each other honest. Such a process of argumentation, CAMPBELL argues, will maximize the role of the natural world reflected in the research data, and eventually will contribute to the selection of beliefs in the validity-enhancing direction. Selection theory, when translated into the sociology of scientific validity, posits that reality operates "as one of the selectors among the human socially generated category systems and hypothetical beliefs that are in the contemporary pool of contenders" (CAMPBELL 1994, p xii).

The Conversion of Scientists and the Sociology of Scientific Validity

While social constructivists have been quite successful in explaining the sustenance of numerous controversies in various fields of science by causally linking different interpretations on observational evidence to the starkly different social/professional interests and ideology (i.e., what CAMPBELL calls the requirements of the tribal continuity) held by opposing theory groups, they are much less successful in explaining how and why such controversies were closed down. I have argued elsewhere (KIM 1991, 1994) that this is inevitable since the relativists, being preoccupied with analyzing how the 'tribal' leadership and the loyalty to the group determine scientists' behavior and arguments, are

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deprived of any resource that might be used to explain the *conversion* of scientists from one theoretical position to another which is supposedly capable of more validly representing the physical world. Assuming that scientific change takes place in a 'referential vacuum', these relativists fail to demonstrate how and why disputants eventually come to a consensus regarding the superiority of one theory over another.

To demonstrate how 'reality' operates as one of the 'co-selectors' of beliefs (CAMPBELL 1993), and to show how collective belief change in science can be made comprehensible, CAMPBELL argues that the sociologists of scientific validity should attend to:

> the hundred or so instances (by implication, a sampling from thousand) in which ideas, procedures, and strategies are proposed and rejected, because they won't work. This "not-working" is often decided by thought and argument, and often by laboratory efforts. Some of these rejections no doubt involve blind social conformity to locally preferred belief. But probably only a small proportion. We need an expanded, still more microprocess, sociology of such idea-winnowing (CAMPBELL 1986, p118).

Such a proposal for a microsociology of idea-winnowing has been actually taken up by several historical sociologists of science (e.g., RUDWICK 1985; KIM 1994, 1996). In these studies of consensus formation in 19th century geology and early 20th century genetics, authors argue that what is lacking in the relativist/constructivist analyses of scientific controversy is the detailed explanation of the *restructuring* process of the previous network of allies and enemies in terms of the *changing beliefs* of the individuals involved in the controversy. Though such a restructuring process of the network necessarily involves a description of the changing pattern of allegiance, and hence an account of the *converts* who shift their opinions in the course of time, in the relativists' case analyses, numerous scientists who changed their positions and hence contributed to the consensus change are simply brushed aside. It is true, as the relativists argue, that some scientists may dogmatically adhere to their intellectual offspring. However, these studies show that many geologists and biologists *did change* their beliefs in the course of time, in response to the persuasive rhetoric of others based on experimental observations. By challenging the relativists/constructivists to come up with a plausible causal scenario as to the 'role' played by the natural world in the collective belief change in science, CAMPBELL's selection theory teaches us that any plausible social theory of science should include a careful *internal* description of how scientists with different locations in an interacting network emerge as converts as a result of the focused disputations on purported observations and mutual monitoring of each other's arguments and illustrations.

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Vision as Paradigm: From VTE to Cognitive Science[1]

Abstract

Central concepts of CAMPBELL's "evolutionary epistemology" or "selection theory" go back to his years as a psychology student when behaviorism was very much in fashion. However, VTE (vicarious trial and error) is already a cognitive notion from TOLMAN expanded into a general epistemological category. Linking the paradigmatic case of vision to action even maintains central concepts of an older profound tradition in the combined study of knowledge and behavior. This illustrates a remarkable steady line of research rising above the fluctuations of fashions while remaining sensitive to new findings.

PIAGET (in BRINGUIER 1977, p37) once complained about fashions and fads dominating the study of psychology in big countries like the US or the Soviet Union. Apparently like flocks of birds, researchers in those countries all seemed heading for the same direction, closely sticking together. All of sudden, one bird would change direction and, somewhat surprisingly, all the others would promptly follow and pursue the new direction. He felt these rather abrupt changes, coupled to reorientation cycles of roughly ten to fifteen years, as very detrimental to science. A meaningful line of research, once taken, should not be prematurely ended and should be followed through tenaciously until fully explored and thoroughly assessed. Though D. T. CAMPBELL might occasionally seem to explore apparent evolutionary advantages connected to the type of collective behavior that upset PIAGET , it does not seem to apply to his own line of thinking. From the early time of his graduate study at UC Berkeley during the heydays of behaviorism until the post-cognitivism of connectionism, he remained dedicated to a very specific line of research in the study of the phenomenon of knowledge. Despite its very wide range, his approach to this phenomenon preserved a very personal character, combining the rigor of his behaviorist education with the daring speculation of an evolutionist interested in that ephemeral organ called "mind". Can one remain a loyal behaviorist and be fascinated by the mind during a whole lifetime? Donald CAMPBELL managed to do so. Out of the tension between these two opposites he forged his remarkable "evolutionary epistemology" that he would like to be remembered as "selection theory". Selection of what by whom?

A recurrent theme in Don CAMPBELL's "personal retrospective history of ideas" is the role of vision. In his visionary view of knowledge processes, the case of apparently simple visual perception has always had an important position but in this paper its prominence seems more strong than ever. It is promoted into the role of prototypical knowledge: "I now wish to identify 'knowledge' with point 2.1.2 of the Basic Selectionist Dogma, i.e. those vicarious processes which short-cut selection by the life and death of genetic variants. *Visual perception is the most important of these*." (p10, italics ours) If vision is so important, do recent findings allow us a better appreciation of this claim?

In a forthcoming book on visual perception, Stephen PALMER (in press) introduces the explanation of vision as the most successful achievement of cognitive science. Current research on vision owes much to David MARR. MARR represents for the study of vision what CHOMSKY means for the study of language: an increase in both depth and scope. It was his ambition to develop a general theory of vision that would accommodate both natural systems, the eyes and brains of men and animals, as well as vision devices developed within artificial intelligence. His posthumous book Vision (1982) brings together the results of various disciplines dealing with vision, from neurophysiology to artificial intelligence, with the aim of developing an all encompassing scheme for the study of visual perception. Though criticised and amended at various points, MARR's model remains a powerful integrative model to deal with vision as it ranges from retinal sensitivity to conceptual interpretation. Two major features stand out as characteristic for his approach, one peculiar to MARR, the other widely shared by almost all current models. The generally accepted feature is the modularity of the visual system. The feature peculiar to MARR is the role attributed to an intermediate integration stage based upon perspective. Supported by neuroanatomical and neurophysiological findings, the prevailing notion nowadays is that vision comprises a conglomerate of many relatively autonomous subsystems working together in intricate ways. The actual count runs into the thirties! MARR's achievement is to provide a sequential orchestration for these subsystems. The skeleton of his system comprises three stages, 2D, 2.5D and 3D, characterising the various intermediate results in the sequential and progressive assemblage of the percept. These stages show some resemblance to established schemes for drawing and painting with units of analysis such as lines, oriented surfaces and volumes. Whether one accepts or rejects MARR's particular model, the main challenge in vision studies seems to be to account for the integration of these many relatively independent modules. Despite its apparent simplicity, what is seen in a single eyeglance is the result of a complicated combination. Visual perception involves a plurality of processes. Is it in those terms still suitable for its pivotal role in selection theory?

In the daring scope of his original hierarchical classification of knowledge processes, CAMPBELL (1959) inserted "perception" almost in the middle of a series of embedded "vicarious" processes (level 5). The impressive character of that classification came from the power of coupling random variation and selective retention with "vicariousness". From the second level onwards of "sexual reproduction" up to level 11 of "modern science" and 12 of "machines" (the early AI promises), each new level is reached through some substitute for the variation mechanism that reduces risk and secures preservation of what has already been achieved. Level 5 perception is a substitute for level 3 trial-and-error. Instead of running deadly risks by running around in a potentially dangerous environment (level 3), visual exploration (level 5) allows for the exploration of that environment by just looking. The gaze is a safe substitute for actively walking around in a spurious space. Ultimately, this is a combination of THORNDIKE and TOLMAN.

Trial-and-error is linked to THORNDIKE's historical research on problem solving by cats. Just before 1900, THORNDIKE studied how cats managed to escape from puzzle boxes with complicated locking devices. Trial-and-error is the apparently random behavior that those cats produce to free themselves from the encaged situation. They turn around and push at various handles and buttons, jump up against the bars of the cage, etc. until, by accident, they successfully hit the opening mechanism. At the most primitive level, neither the unsuccessful nor the successful behaviors are memorised. When the annoying situation would be installed again by locking up the same cat in the same case, it would mean that an entirely new sequence of random behaviors would be produced again until ultimately freedom is reached once more. As is known, THORNDIKE found out that cats do not demonstrate insight but they do exhibit learning. The behavioral segments just preceding the escape will increase in probability and the time needed to get away will gradually decrease. The Law of Effect corresponds to CAMPBELL's level 4: learning. On level 3, the selector is still the environment. The lock yields only to a specific way of handling it. On level 4, memory becomes a vicarious selector, imposing a specific probability distribution upon the behaviors tried. But where is the vicarious selector in vision?

Obviously, the notion of vicariousness came from TOLMAN, CAMPBELL's mentor at UC Berkeley. A rat learning a maze is not just chaining responses as THORNDIKE's Law of Effect might suggest. That is manifest from the flexibility in behavior when alternative actions are imposed. While being trained through various alternatives, the rat constructs a "cognitive map" of the maze. Confronted with new challenges (a barrier on an otherwise unobstructed path) the rat will demonstrate VTE (Vicarious Trial and Error). That means that the trial and error is to a substantial extent executed on the cognitive map. But to what degree is this perception and to what degree action? CAMPBELL has always been intrigued by an imbedding of perception into action. Contemplation is not the prototypical perceptual act. Perception is subordinated to action. To the degree that the environment resists specific actions, it is forced to yield information. The favorite model is the ancient image of the blind man with the stick. To perceive his environment, he has to poke around with the stick and he only "knows" the environment to the degree he is hitting it. CAMPBELL is even more fond of the echolocation paradigm. Through some behavior, the organism is acting upon the environment and specific knowledge is derived from the way the environment reacts to it. In a more basic scenario, the primitive organism will apply its action to whatever it encounters, say eat it, and be vulnerable to the radical selection by either staying alive or dying. In perception, especially for the distal receptors, the action of the organism is less risky, and the response of the environment provides the segments of the vicarious selector that is knowledge. But where is the action component in vision? How is echolocation applied in this celebrated case?

Vision is the most remarkable sense because it makes use, in the truly opportunistic way evolution always seems to rely on, of a very reliable component in our environment: the sun. Given the regularity of its behavior, life on earth can be sure of the action of this ultimate battery also as an echolocation emitter. There is no need to poke around with the stick if an omnipresent object is immersing the environment in light at predictable time intervals. The sun pokes the environment with its light and organisms with eyes figure out how that light affects their world. Reflected light is the echo resulting from the action of life's most reliable ally. Vision might be so powerful because of the lavish and inexhaustible power of this giant companion. If this "vicarious" action of the sun is ignored, vision data become epstemologically arcane and shaky. CAMPBELL quotes the case of a pioneering vision scientist:

"DESCARTES got to his kepticism 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 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 prisoner" 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."

But what else can CAMPBELL mean apart from the emitter action when emphasising vision's affinity with blind trial and error in the revised version of "evolutionary epistemology":

"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.)" (this volume, p8).

History of science can provide some of the requested help. The more convincing models of vision like EUCLID's and PTOLEMY's started out as extramission theories. In their views vision had to be conceived of as a dynamic force, unfolding with rays originating from the eye and propagated like sticks extended into the world until they hit the objects of vision. But modern robot vision systems too maintain a close affinity to echolocation models with range finders based on reflected beams of laser light. When Arab and late medieval scholars found out that vision could be better seen as based on intromission, rays coming in rather than leaving from the eye, visual perception maintained its status as prototypical knowledge process. There is a remarkable continuity throughout an erratic trajectory of epistemologies from Saint Augustine and thirteenth century theologians to the Age of Enlightenment up to our own time. All of them long for light as a means for obtaining more knowledge. Enlightenment and enlargement of cognition are equated. Even the emblem of CAMPBELL's university, UC Berkeley carries the phrase "Let there be light". The metaphor couldn't be that powerful unless it would in some deep sense be true.

As indicated, a characteristic of modern theories of vision is the emphasis on modularity. The action of the light is not only powerful but also multiform. Various modules capture various aspects of the reflected light so as to compose a perceptual object with genuine "entativity". Whether along the sequential line of MARR or some more ad hoc combination, the current challenge for theories of vision is to find out how the modules combine to yield an integrated percept. One wonders to what degree the co-operation between the modules could be handled in terms of the social epistemology for which CAMPBELL became increasingly interested. While relatively solid, the entativity of visual objects is ultimately also fallible. As the Magic Eye illustrations show, a few stereoscopically integrated modules can make illusions look veridical. The suggestion is that what accounts for the entativity of perceptual objects

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and for the solidity of scientific theories should somehow be comparable. It is a remarkable coherence that makes the various visual features captured by the various modules agglutinate into a single perceptual object. It is a remarkable coherence that keeps together the achieved in new combinations toward even superior adaptability. Figuring out the principles of this grouping, the ability of keeping together what has been selected, either directly or "vicariously", remains a major challenge for evolutionary approaches to cognition. In contrast to PIAGET, CAMPBELL has not explored logic but social epistemology along the pursuit of this question. Nevertheless, they have much in common.

Though CAMPBELL was quite conversant with PIAGET's work and his "biological epistemology", his name doesn't figure in the final paper. However, the work of James Mark BALDWIN is mentioned and he has undoubtedly been an inspiration for both of them. Before the onset of behaviorism, it was clear that extending psychology with the study of animals and children would ultimately increase the epistemological scope of psychology and the understanding of cognition. At the time behaviorism was proclaimed, this was understood by Edwin HOLT who accepted behavior as the basic observational category but, in contrast to WATSON, did not do away with knowledge as a psychological entity. PIAGET's genetic epistemology might have been tributary to BALDWIN, but CAMPBELL's "evolutionary epistemology" took the line of Edwin HOLT and TOLMAN who, while they adhered to behaviorism, never dropped cognition. That is the line of thought to which Donald CAMPBELL remained loyal and to which he contributed so much by including the social epistemology that cognitive science still has to discover.

Notes

1 This comment was prepared while the author was visiting scholar at the Institute for Cognitive Studies at the University of California Berkeley as Peter Paul RUBENS professor during the spring semester of 1997.

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The Two-stage Model of Evolutionary Epistemology

Abstract

The evolutionary or "phylogenetic" epistemology, as it was called originally by Konrad LORENZ, deals with the hereditary mechanisms of cognitive organismic systems—valid for man or animal—and is a logical consequence of the biological theory of evolution. Called EEM (Evolutionary Epistemology of Mechanism) by BRADIE (1986), it was set against EET (Evolutionary Epistemology of Theories) merely as a concept of analogy, as developed by POPPER and later by TOULMIN and others. Here, however, evolutionary philosophy of science should be recognized as a second stage of phylogenetic epistemology, which doesn't deal directly with the cognitive organismic systems, but with the scientific method as a goal-orientated cognition activity of man. In this respect MACH and BOLTZMANN had already made use of the evolutionary-epistemological approaches, found previously by Darwin himself, and applied them to the ontogeny of physics, intending to show the evolution of natural scientific methods of cognition as a self-infringement of the fundamental cognitive mechanism, which originally merely served survival.

As Donald T. CAMPBELL already showed in an essay (1974) dedicated to Karl POPPER, it is not new to apply the theory of evolution to the cognitive capacity of man. A succession of philosophers and natural scientists speculated (right after the publication of DARWIN's *Origin of Species*) that *a priori* forms of perception and categories of thinking are the product of phylogenetic development. Besides, DARWIN himself has ascribed this point of view in a most general way to the intended uses of his theory of evolution, as can be understood clearly nowadays from his notebooks (BARRET 1980).

Since DARWIN it was clear, also, that here a base can be found which establishes in a natural scientific way the evolutionary dynamics of scientific theories. It was not the biologists, but physicists like MACH and BOLTZMANN, who consequently expanded the evolutionary approach to the ontology of their own discipline. This first attempt, however, became stuck in a metaphorical view, because biology itself, and in particular genetics and behavioral science, had not developed sufficiently to enable it to recognize hereditary mechanisms of cognition as genetically conditioned ways of behavior, these being species-specific characteristics in the same way as anatomical structures.

In our century the renewal of this attempt emerged from POPPER's fundamental objection that the really interesting dynamic aspects of evolution and variation in science escape the mere logical analysis of evidential systems. For a long time POPPER was reluctant to accept the theory of evolution as a natural scientific theory and accused it of having a circular character. He considered DARWINISM to be a medium of battle against CARNAP´s Inductivism, putting forward an analogy between DARWINISM and LAMARCKISM, as well as selection and instruction (instruction through repetition), on the one hand, and deductivism and inductivism, as well as critical elimination of mistakes and positive justification, on the other. To characterize his own position POPPER added that his logic of research contains a theory of knowledge-growth through trial and error—that is, through the elimination of mistakes. This means, however, knowledge-growth through DARWINIAN selection, instead of LAMARCKIAN instruction.

POPPER elaborated this analogy at the end of his life into a four-phase-scheme of the dynamic of theories (POPPER, 1994, p33):

1. Problem (not observation)
2. Attempts for solution = hypotheses
3. Elimination = refutation of hypotheses or theories
4. New and more sharply defined problem that develops out of the critical discussion

As POPPER noted himself, this scheme was supported by Konrad LORENZ at the deeper level of comparative behavioral research. Thus it can be said that the old ideas of the evolution of science (as anticipated by MACH and BOLTZMANN) have not only been renewed by POPPER but that modern behavioral science, too, has delivered a natural scientific base which is far more than a mere analogy.

Therefore the real history of evolutionary epistemology as a program of research founded on natural science starts with Konrad LORENZ, who showed through his ethology, based on evolutionary theory, the way back to the unequivocal fundamental similarities of the cognitive mechanism of all beings to a homological base, that is back to a real relationship. In this sense three stages of argument, clearly separated from each other, can be differentiated:

1. The biological theory of evolution
2. Evolutionary epistemology
3. Evolutionary philosophy of science

The evolutionary epistemology that deals with the hereditary mechanisms of cognitive organismic systems—valid for man or animal—is in a narrower sense a logical consequence of the biological theory of evolution. It was called EEM (Evolutionary Epistemology of Mechanism) by BRADIE (1986) and put in opposition to EET (Evolutionary Epistemology of Theories) merely as an analogy, developed by POPPER and later by TOULMIN 1963 and others. Here, however, the evolutionary philosophy of science should be recognized as a second phase of phylogenetic epistemology, which doesn't deal directly with cognitive organismic systems, but with products of epistemology of a certain biological species, that is homo sapiens, who is the only living being able to practice science. In this respect the evolutionary philosophy of science is linked only indirectly with the biological theory of evolution. The logical-systematic link between biological theory of evolution and evolutionary philosophy of science is evolutionary epistemology in the narrower sense of a "phylogenetic" epistemology, as it was understood by LORENZ.

Every attempt to transfer the biological theory of evolution directly to the phenomenon of "science" has a merely metaphorical or at best analogical character. Historical as well as recent examples of such metaphorical viewpoints are phrases like "evolution of sciences" (TOULMIN 1963) or "evolution of scientific hypotheses and theories", whereby single fields of knowledge or theories are looked upon as biological species, which are able to live and die, compete with each other (POPPER), change themselves or are even able to fight against (MACH) and kill each other. Such metaphoric is harmless in this field, but can become a dangerous ideology in other fields like law, state and economy—as already demonstrated by Social DARWINISM.

The evolutionary philosophy of science as a second phase of evolutionary epistemology however is not merely a metaphorical view of the dynamics of theories in evolutionary disguise, but a further development of the basic trial-error-mechanism related to scientific methods of cognition. Therefore its object is not the evolution of theories (as already recognized clearly by BOLTZMANN) but the evolution of the scientific method (OESER 1984) as man's goal-oriented activity of cognition, that hastens the experience-scientific theories.

In this respect MACH and BOLTZMANN have already employed the evolutionary-epistemological attempts, previously traceable to DARWIN himself, for the epistemology of physics. Their goal was to show that the evolution of natural scientific methods of cognition is a self-infringement of the fundamental "cognitive mechanism" which originally merely served survival. Evolutionary epistemology as phylogenetic epistemology, however, can merely offer a necessary, but not a sufficient, explanation of the functioning as well as the failure of the human perception mechanism in all fields of scientific knowledge. Therefore, the following must be added:

1. EE has to be extended by an ontogenetic epistemology (in the sense of PIAGET), which should provide the fundamental explanation of the constructive enlargement of hereditary perceptional ability in man. This enlargement can be found in all scientific processes of cognition, especially in the natural sciences.
2. Furthermore, as both epistemologies are linked through the relation of phylogeny and ontogeny and refer primarily to the macro-level of directly observable behavior, they have to be completed by a causal explanation of the macro-phenomenon-creating mechanisms on the micro-level of neural structures, that is through a "neuroepistemology" (OESER/SEITELBERGER 1988). In this way the alleged contradiction between adaptionism and constructivism in the sense of competing natural scientific research programs of epistemology (LORENZ and PIAGET) can be abolished. A concrete neurobiological approach is supplied by the "Neural Darwinism" of G. EDELMAN (1987), which is expandable in the field of evolutionary epistemology to a "selection-theory of meaning". With such a theory it can be established that in "learning

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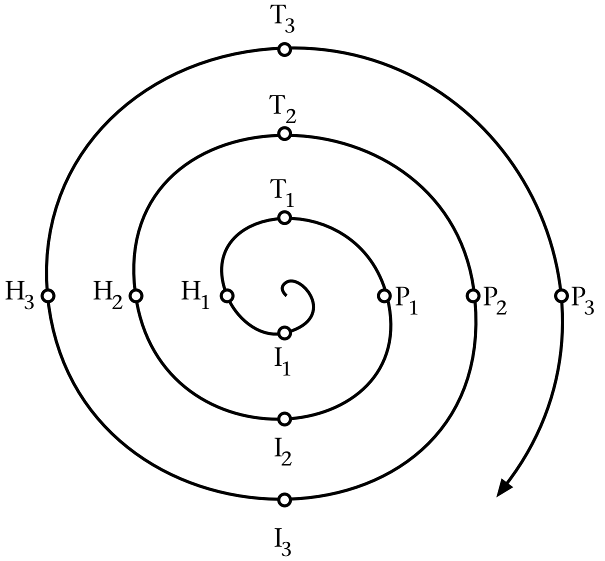
through experience" it is not the environment which instructs the cognitive system, but, on the contrary, it is the cognitive system that achieves meaning or internal so-called mental representations through constructive selection (that is, selection and linkage of neural activity which is meaningless on its own).

Completing the phylogenetic epistemology in that way, the simple but fundamental trial-error-mechanism (which is characteristic for all living beings "from amoeba to EINSTEIN" (POPPER) can be expanded to this model, which mirrors the interplay of theory and experience in the empirical sciences:



Proceeding from empirical information (I), which is, however, always determined by the already existing structural information of our phylogenetically created perception mechanism, scientific (that is expanding the everyday-experience) hypotheses (H) are created that lead to theories (T). From these theories derive prognoses (P), that can be tested reductively and can be improved on a higher level through new hypotheses and theories.

This model of scientific progress, which develops spirally through self-correction, mirrors the self-organization of information in the human perception process, where trial and error, theory and experience are linked together narrowly through formal, more or less exactly definable, methods like induction, deduction, construction and reduction.

Such a model is—in the sense of a distinction between the internal and external history—an internal model of the development of problems, hypotheses and theories, where abstraction is made from socio-psychological as well as social-political contexts (in contrast to CAMPBELL´s "Sociology of Scientific Validity"). There is an old saying of ARISTOTLE: "Who abstracts and knows that he abstracts, doesn't make a mistake." But the point is not—as POPPER says—to construct a personal PLATONISTIC world of problems, ideas and theories way above the individual-social reality but just to demonstrate the proof of an internal logic of scientific knowledge that is independent from individual social and political decisions only in its result, not in its real temporal course. Whoever denies this kind of independent scientific cognition, denies the autonomy of science and with that, in the end, the truth of scientific cognition, which, in the context and on the foundation of evolutionary epistemology, in the first as well as in the second phase, primarily has a functional pragmatic character.

To the extent that empirical constructs, i.e, hypotheses and theories, are used in instructing human actions, they will always be confirmed or disproved. That is to say that there is always a majority of hypotheses and theories, the better functioning theory with the better explanatory and predicative power will be chosen.

This means that science, primarily, is nothing other than a second-order survival machine which is built on the fundamental phylogenetically acquired reaction scheme and leads to more and more complex, theoretically established instruction schemes for successful action, which exceeds indeed the survival of the single individual, but which, in the end, is always linked to the reproductive success of the biological species homo sapiens. However scientific progress should be judged, one thing is absolutely clear: the enormous reproductive success, that is, the explosive increase of the biomass of the biological species homo sapiens, can be traced back to the application of natural scientific knowledge in technology, medicine and economics. This success is the only solid guarantee for that which we call the "truth" of scientific theories.

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Henry Plotkin

Knowledge and Adapted Biological Structures

Abstract

In this, one of his last pieces of writing, CAMPBELL argues for restricting usage of the terms knowledge and knowledge processes to vicarious processes that short-cut natural selection. The counterargument is presented that it is scientifically counterproductive to do this, whatever virtues it may have for communication with philosophers.

It is a tribute to the extraordinarily productive life of Donald CAMPBELL that *Evolution and Cognition* should be able to publish posthumously so stimulating a paper by him. As always, and right to the end of his life, CAMPBELL made us all think. Here I consider just one of the key points to which CAMPBELL addresses himself. This is what CAMPBELL now considers to be the overinclusive usage of the terms knowledge and knowledge process. Adaptive organic structures, he asserts, should not be' treated as knowledge because "it is a needless obstacle in making contact with the traditions of philosophical epistemology" (section 2.2). As I understand it, and as CAMPBELL concedes, this is a major shift from his writings of an earlier period.

Well, it depends surely on what one wants from evolutionary epistemology or selection theory. It must, incidentally, be an error to assume that these are the same thing. Selection theory is a specific form of evolutionary epistemology, which I take to be the view that the human ability to know is a consequence of evolution. The former, selection theory, is not mandated by the latter. But that is to digress. To return to the main point. Evolutionary epistemology can be made to serve different purposes. For those who want to make the crucial point that a complete *scientific* understanding of what we as humans know, and how we come to have knowledge, must be rooted in evolutionary biology, contact with philosophical epistemology is not relevant. The reason for this is that since evolutionary biology is science, then so too must be evolutionary epistemology. From DARWIN and T.H.HUXLEY onwards it has been presented as science, and if it is not science, then surely it is nothing. If this is accepted, then philosophers will not, in any event, be swayed *philosophically* by what it has to say. On the other hand, fellow scientists, especially social scientists, are open to persuasion by an argument that is rooted in science. That is the audience with whom evolutionary epistemology should be seeking contact, not philosophers for whom the science is irrelevant.

The notion that makes the contact is a hierarchy of knowledge-gaining processes with knowledge as a series of nested products of such a hierarchy. This is a most elegant conception, whether the hierarchy be that favored by CAMPBELL or one of some other form (for example, PLOTKIN/ODLING-SMEE 1979; RIEDL 1980). It is at once a conceptually simple scheme, and yet it also provides us with an architecture of appropriate complexity for something as complicated as human knowledge, whether it be that of the individual or of a collective kind, for instance science itself. But an essential part of the conception is seeing adaptative organic structures as knowledge and the processes that gain such knowledge as knowledgegaining processes.

Evolutionary epistemology as science also makes the crucial point that knowledge is a life-or-death matter. Why humans have the capacity for acquiring so much knowledge, much of it seemingly trivial, is an interesting issue. One possible answer is that it is only seemingly trivial and not actually so. Whatever the answer, it should not obscure the fact that without knowledge of physical and social causation, without knowledge of spatial relationships, and without knowledge of motor and linguistic skills, we would not survive as we do, and may not survive at all.

The central importance of our capacity to gain such knowledge for human survival is mounting from the direction of developmental psychologists. There is strong evidence that we come into this world with *a priori* knowledge (see ELMAN et al., 1996 for a minimalist nativist position; and SPERBER et al., 1995 for examples of a more explicit content to the knowledge of infants at birth or soon after), evidence that must have delighted CAMPBELL as a proponent of evolutionary KANTIANISM. The point of this developmental evidence is that no matter how minimal cognitive predispositions are at birth, they must point to genes as repositories of information leading to such predispositions. Such genetic information would not exist were it not for a history of selection for such cognitive predispositions, and the selection could only have occurred if the consequence of such predispositions was an increase in fitness. If human knowledge is so complicated as to depend upon genetics and the embodiment of such genetic information in neural networks that are tuned by appropriate developmental circumstances, then why be enslaved by philosophical conventions that knowledge only begins with the "vicarious processes which short-cut selection by the life and death of genetic variants". Do that and we are back before the enlightenment of CAMPBELL's great papers of the 1960s and 1970s with very little gained.

More interesting, if less substantive, is another approach to the same point which follows on from disagreement with that basic distinction that CAMPBELL now seeks to make between adapted biological structures and knowledge deriving from the workings of vicarious processes that short-cut natural selection. In this case, he expresses the distinction with the assertion that "… 'knowledge' is more indirectly (but more precisely) selected than are biological structures" (section 5.1). Given acceptance of a hierarchical scheme as the appropriate causal explanation for any instance or human cognitively-derived knowledge, it being more indirect than adapted organic structure must be true, but it is only trivially so. The structure of my hand is only marginally more indirectly an expression of the knowledge of a structure capable of fine manipulation of small objects, as filtered by way of countless selection and transmission episodes and coded in genes that partdetermine hand form and which are expressed in phenotypic form in a specific environment of development. than are the neural network states that represent "mental" or "psychological" knowledge of my hand.

Furthermore, and closely related in his account, the distinction that CAMPBELL now seeks to maintain between adapted organic structure and knowledge as he now defines it in terms of competence for the "immediate environment of behavior" as opposed to organic structure where "the environment 'represented' is always a past one" should also be challenged. Nature is *never* prescient, and while all knowledge constitutes a hopeful projection into the future of an experienced past, in many vital respects, for example, the direction of the arrow of time and hence the order of cause-effect relationships, or the need for energy resources in the environment to be exploited in order to maintain a state of negative entropy, both organic structures and knowledge structures are at once assumptions about the uniformity of at least some aspects of and hence adaptations to both the past and the present. That is why many, perhaps all. human cognitive processes are genetically and developmentally constrained, hence predisposed. Predispositions are constraints that work in the present, not the past.

But what is really interesting in CAMPBELL's paper is the question of precision. Leaving aside the difficult matter of an appropriate metric by which comparisons could be made, knowledge, according to CAMPBELL, has a greater precision to it than does adapted organic structure. But is this correct? There is, of course a wonderful precision, in both simple (the tuning of auditory receptors in some species of moth to just that frequency of ultrasound used by the echolocating bats that prey upon them) and complex (famously, the eye) forms of adaptive biological structures. After all, the whole of the argument from design, the designer being a supreme Creator, took its strength from the exquisite precision of adapted biological forms. Reject creationism, as virtually all scientists of course do, and what we are left with as an account of such adapted forms is evolution by selection. No one now believes otherwise. Yes, there is "misfitting" (CAMPBELL's term in this paper) or satisficing (SIMON, 1969). But given that evolution is a giant statistical machine that crunches numbers on a massive scale across geological time, where there are conserved features of the world, like the properties of light or sound, they are adapted to with wonderful precision.

On the other hand, the degree of precision of knowledge generated by cognitive processes is owed in large part, as CAMPBELL himself argues, to sensitivities to "a much narrower 'specious present'." My point is that the sensitivities to a narrower present derive from those self-same constraints on cognition whose origins lie in past selection and the constitution of human genes in the present. However it is described, as computational explosion or frame problem, the search space available for cognitive mechanisms to roam in is so huge that the only way of getting cognition to work rapidly and adaptively, which is what it does. is by pointing it at the right part of the search space (PLOTKIN

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1997). Scramble that part of the genome that codes for the central nervous system and human cognition if it survives at all, would be reduced to a slow, shambling and utterly imprecise instrument for generating knowledge. The precision is a result of the genetic constraints on cognition.

As I see it, the crux of the matter is this. Human cognition, and the knowledge that it gives rise to, is so dependent upon a causally larger, hierarchical structure, that it can only be a concession to a different constituency, in this case a philosophical one, which leads one to carve off one or several levels of that hierarchy and say that these constitute knowledge, whereas the others do not.

In one respect, though, I do completely agree with CAMPBELL's final judgement, which is to caution against running too hard the analogy from biological evolution when knowledge is partly a product of human social interaction and influence. In other words, when culture enters the picture, the analogy with biological evolution becomes tenuous. Partly this is because the rate of transmission of information by way of language between individuals is so great, partly because the transmission routes are much more diverse than occurs by way of genetic transmission of information, and partly because humans are subject to social forces that significantly affect the way information is received and stored. As a great social psychologist, Donald CAMPBELL had a better and deeper understanding of the importance of these differences than would the rest of us.

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Massimo Stanzione

Campbell, Hayek and Kautsky on Societal Evolution

Abstract

CAMPBELL's and HAYEK's evolutionary views are confronted, in order to understand their different stances towards methodological individualism. The many similarities between their interpretation of social cultural evolution are referred to the ideas of the Austrian School of Economics (particularly VON MISES) and to the ideas of an important figure of the Austrian Marxist School, Karl KAUTSKY. Methodological individualism appears as legitimate but non-unique result of the evolutionary premises posited by all these authors. Yet CAMPBELL's latest works reassert the principle that at the biological level, "groups are real", and that there is a nested hierarchy of selective levels which goes from individuals to groups as recently stated by E. SOBER and D. S. WILSON.

In CAMPBELL's work, along with the main lines of a new evolutionary epistemology, there is a substantial effort to reintroduce the evolutionary way of thinking in the social sciences (see his selected papers in OVERMAN 1988 plus many others such as CAMPBELL 1961, 1963, 1965, 1972, 1975, 1979, 1981, 1981b, and 1995). Almost independently, the same direction was taken by the Nobel Prize winner Friedrich August von HAYEK. The Austrian liberal economist soon became acquainted with CAMPBELL's ideas, thanks to a suggestion by Konrad LORENZ. HAYEK, in his monumental trilogy (HAYEK 1973, 1976, 1979) as well as in HAYEK (1988), approvingly quotes many of CAMPBELL's papers (i. e., CAMPBELL 1963, 1965, 1974, 1974b, 1975, 1977). CAMPBELL, on the contrary, neither in his intellectual biography (see OVERMAN 1988, pp1–26) nor in his last contributions discussed HAYEK's fundamental statement on the role of tradition as the real power behind the moral, legal and economical development in ancient and modern societies. Why? I'll try to give a provisional answer to this apparently simple question, because it has many different theoretical implications, going far beyond the political level.

In fact, there are many similarities between CAMPBELL's and HAYEK's conceptual frameworks. They both agree :

1. on the possibility to explain the fundamental transition which leads from the first human groups of hunters and gatherers to the "extended order" of modern (industrial and capitalist) societies *only* by adopting a selective evolutionary model, *inspired by* DARWINIAN *natural selection* (i.e. *analogous* but not *identical* to the genetic process).
2. that, during this transitional process, the older 'natural' control mechanisms (resuming into solidarity) which assured the fitness of ancient societies had to be progressively replaced by different and 'artificial' (i.e., cultural) constraints, as ethics, law and market.
3. that the peculiar character of this replacement is a result of human *action*, but not of human *design*. Therefore, it will be based more on 'traditions' than on rational projects about the best social and economic order. Traditions spontaneously (self-organizationally) emerge from human practices and effectively mediate between instinct and reason, without any precise and justified knowledge on the part of agents.

Yet CAMPBELL and HAYEK aren't really in complete agreement. They disagree:

1. on the evaluation of methodological individualism (and, more recently, on the applicability of group selection);
2. on the possibility of favoring social change through an experimenting society;
3. on sociobiology. There are also other topics that would be interesting to analyze, such as the role of self-organizational processes in HAYEK's view, but I haven't sufficient time to discuss them.

In HAYEK's view, the collective forms assumed by a tradition result *only* (and *automatically*) from (egoistical and selfish) actions of *single* individuals. These actions have *intended* and *unintended* effects, which are in turn selected through "invisible hand" mechanisms (such as the market's mechanisms). According to a central tenet of HAYEK's (and POPPER's) "methodological individualism", it would be a categorical error to affirm the 'ontological' reality of collective (institutional) entities. In other terms, *there is no space for the reality of groups as proper actors*. Groups can only be *selected a posteriori* according to the rules they have traditionally adopted, but they can never be the real subject of real action. Therefore, they cannot figure in any causal-explanatory reconstruction of the social realm. The ideological consequences of this assumption are well-known: any 'rationalistic' effort to project (construct) novel political, and economical forms of (present and future) society would be fatal conceit (see HAYEK 1988).

Already in CAMPBELL (1958) we find that groups are asserted to be 'real', as opposed to methodological individualism. On this matter he never changed his mind. Very recently, in readdressing the same problem, CAMPBELL wrote that methodological individualism "is at the base of most social sciences, including economics, psychology and much of sociology. It starts from the *dogma* that all social processes are to be explained by laws of individual behavior, that social systems have no separate ontological reality and that all references to social systems are merely convenient summaries for patterns of individual behavior" (CAMPBELL 1995, my italic).

Nonetheless, in accordance with the prevalent convictions of many evolutionary biologists, for a long time CAMPBELL restrained the validity of group selection mechanisms to cultural evolution (i.e., to the group selection of ideologies, social organizational traditions, moral indoctrination, and religious cosmologies—see CAMPBELL 1973, 1975, 1983, 1991). It seems to me that in so doing, he weakend his views of society to the point where it was insufficient to contrast HAYEK's refusal of social planning. In fact, CAMPBELL's constant attention to the problem of ameliorating social intervention was directly "affected by (his) nation's experience with the great society, the war on poverty, and social programs during the 1960s and early 1970s" (OVERMAN 1988, p240). Also CAMPBELL's proposal of an "experimenting society" (characterized as active, open, honest, accountable, challengeable, due process, decentralized, voluntary, etc.) couldn't be really adequate. This broad and ambitious linkage of his general theory of social experimentation with a specific political system had a prevalent ideological character. Some skepticism is evident in CAMPBELL himself when he withholds full advocacy of the utopian experimental society: "In the social sciences", he says, "we are scientific by intention and effort, but not yet by achievement" (ibid.).

To prove my point, let me summarize the social evolutionary assumption of what CAMPBELL here calls "my 1960 dogma".

CAMPBELL's evolutionary theory of sociocultural development took into consideration (as HAYEK also does) some typical characteristics that the variation-and-selective-retention process acquires when it is transferred from the biological to the cultural sphere. These differences are due to the active presence of substitutive and vicarious levels assumed by the three basic functions provided by the selective model.

CAMPBELL is quick to maintain that social variation can, and in fact is, not casual (not completely blind). For him, the real advantage of maintaining the term 'blind' in characterizing the sources of sociocultural variation (which may be an alteration of the social group on the whole *or* an individual deviation internal to a single group) lies in the possibility of explaining the rise of certain social organizations without resorting to conscious planning of the change and only pointing out their adaptive value—as HAYEK also says. On the other hand, the study of social change cannot exclude the analysis of those forces that explicitly intend to condition social change itself—as HAYEK admits only partially.

Turning to the selective mechanisms, CAMPBELL honestly admits that in sociocultural evolution, "the potential selective systems are … numerous and … intertwined, and the selective criteria … difficult to specify" (CAMPBELL 1965, p29). However, he was able to identify six different selective systems, partially integrable and approximately arranged according to a scale of increasing specificity.

The first one is represented by the selective survival of complete social organizations (groups, tribes, nations). But this selective mechanism, resorting to the conservation or extinction of the social organism itself, is evidently inadequate to explain either the internal existence of conservation systems, or their participation in partial adjustments. The second selective system, applicable to the multilinear variations in the social field, is constituted by the selective diffusion of traditions and by the selective adoption of these traditions on the part of other social groups.

The other four systems are: selective propagation of temporal variations; selective imitations of interindividual variations; selective promotion to leadership and educational roles; rational selection. They are all applicable to more restricted units and even to individuals and are based on vicarious and/or substitutive selection mechanisms, such as pleasure-pain, memory, imitation, conformity (that is, the tendency to imitate even those acts where it is not possible to observe their success), and, finally, social knowledge itself.

In societal evolution, retention substitutive systems are, on the whole, less rigid than in biological evolution. Yet we can expect "the greatest rigidity, the greatest demand for conformity, on the part of those societies with the more elaborate adaptive systems, particularly when these systems demand restraint upon individual hedonistic impulse" (CAMPBELL 1965, p34), as illustrated, for example, in a tillage society (with its priesthood castes and religious-ritualistic beliefs) in comparison with a hunting society. Other novelties, such as the greater amount of time devoted to passing on the cumulated tradition (i.e. the longer educational process) and the invention of writing, decrease the chances of the potential conflict between freedom to change and the selective value of retaining tradition. Nevertheless, the retention of some cultural forms (as ethnocentric loyalty and willingness for voluntary self-sacrifice in warfare), positively selected in the course of preceding sociocultural evolution, may prove to be inconvenient when new selective forces (such as the development of larger national units and new weapons) intervene.

The most important implication of the use of a selective model for the study of sociocultural evolution is the possibility of analyzing convergent multilinear evolution. "The blind-variation-and-selective-retention model would seem to make plausible the multiple independent invention of tools, rope making, fire, spear, bow-and-arrow, monogamy, the avinculate, and headship in social organization" (CAMPBELL 1965, p38). Among the results of convergent evolution of social forms we find conservation of food surplus, division of labor and urbanization, all due to their clear adaptive advantages.

Many years later, in an ideal confrontation with WILSON's sociobiology, he goes on to treat the subject of the character and role of ethico-social values (CAMPBELL 1975, 1979, 1983). There is a real conflict, according to CAMPBELL, between the ethical values inherited through phylogenetic evolution and the values of modern social organization. As in all societies composed of superior vertebrates, in our modern society there is genetic competition between group members, which imposes serious limitations on the survival value of certain ethical tendencies, such as altruism, formerly favored by biological selection. Furthermore, in the case of human societies evolved (through a process more social than biological) beyond the urbanization stage, genetic competition gives rise to egotistical and familial tendencies that conflict with the group's need of self-preservation. These tendencies must therefore be controlled, even if not completely eliminated, by means of a social system of moral rules. These moral rules should be based on the substitution of group altruism for individual altruism. Moreover, there are easier tendencies that an individual might adopt, such as choosing a social group where all its members show altruistic behavior (to the point of offering greater possibilities of procreation to the altruists), where selfishness is self-inhibited and where there is maximum cooperation.

As I already said, these assertions are insufficient to extend CAMPBELL's general rejection of methodological individualism to HAYEK's version of it. CAMPBELL's constant reassertion of the reality of group beliefs-forming mechanisms notwithstanding, his evolutionary scheme of moral and social development remains very similar to the one by HAYEK. Therefore, HAYEK appears to me completely justified in asserting that our moral tendencies toward cooperation and within-group solidarity are only residual products of the genetic fixation (probably through the so-called 'BALDWIN effect') of some behavioral schemes which were adaptive only in a prehistoric (cultural) selective environment. In the new "extended order", that came about by the selective elimination of alternative group traditions, these instinctual, pro-social attitudes will therefore lack any validity. They suffer from what CAMPBELL has called "clique selfishness"; therefore they will be not capable to produce a universally valid set of moral (and legal) rules.

To put it succinctly: both HAYEK's liberal theory of the economic and state order and his individualistic ideology can truly be presented as logical consequences of these premises, but (as CAMPBELL's different orientation suggests) they are not the only possible ones. In fact, they are a critical reaction against the use that some of the most famous Austrian socialists made of these same evolutionary premises. Let me briefly comment on this point before turning to CAMPBELL's new idea about reciprocal altruism and the reality of group-selection at the biological level.

When HAYEK was a young student at the University of Vienna, MENGER's original theories (exposed in his *Grundsätze der Volkswirtschaftslehre* 1871) were being reformulated and updated by the main figures of the Austrian School of Economics: Eugen BOEHM-BAWERK, Friedrich WIESER, and Ludwig VON MISES. WIESER was a FABIAN socialist and HAYEK became his pupil, because he found VON MISES' anti-socialist position too radical. In 1913, MISES attended BOEHM-BAWERK's famous seminar on the theory of value. The marxist interlocutor in the seminar was Otto BAUER, with whom von MISES become familiar. In 1906, Otto BAUER had written a short paper (BAUER 1905-1906) which criticizes the socialist (and social-evolutionary) ethic proposed by his friend Karl KAUTSKY (1906). KAUTSKY's short book was in fact a reassertion of his former interests in DARWINISM and positivist biological thought. He resorted to this intellectual armament to oppose Eduard BERNSTEIN's attempt to mix a (neo-)KANTIAN ethic and MARXIST ideology (see BERNSTEIN 1988). The fourth chapter of KAUTSKY's book is titled *DARWINIAN ethic. It contains, almost step by step, the same, identical reconstruction here exposed as 'CAMPBELL's 1960 dogma' (which is, in turn, very similar to HAYEK 1973, 1979 and 1988).* Quoting DARWIN, ESPINAS, AGASSIZ and FOREL, KAUTSKY accords moral consciousness and moral virtues to the animals themselves, soon adding that the development of human societies changes both the *strength* and the *domain* of the application of our ancient moral instincts. Furthermore, he says that human societies, in order to control better old instincts, produce new cultural inventions which improve group fitness. These novelties (language, division of labor, money, customary law, property) derive from human intentions, but are not the result of human conscious planning. They are always provisional and imperfect. "Moral law…", KAUTSKY says, "isn't a product of wisdom (which here means "conscious knowledge") and does not generate wisdom". "It is an error to believe that man could and must follow all his instincts without limitations … because they often limit themselves reciprocally". In the fifth chapter, second paragraph, KAUTSKY describes in evolutionary terms "the organism of human society" as a new and distinct level of evolution *interacting* with the previous ones constituted by individuals, kins and small groups. Because human society is different both from individual organism and from animal societies, its development cannot be considered analogous to individual development. There is no single social-developmental law which could predict the ongoing process in terms of universal and necessary stages. In it there will be always "effects that were unintended by the inventors, because they couldn't be intended". Together with the division of labor, which occurs also in some animal societies, the emergence of language "develops on a higher level what encephalization had already started", that is, the capacity to accumulate and elaborate information. "Something similar occurs in the economy … with the emergence of an element which mediates circulation. This element is money. Now it is possible to sell without immediately buying, not unlike the brain permiting excitations to act on the organism without immediately determining a movement". And so on, until the emergence of property, religion, customary law and the fundamental human rights. To sum up: if we identify what KAUTSKY calls "abitude" with HAYEK's (and CAMPBELL's 1960) "tradition"; if we refer his (fertile) idea of interlevels selection to the cultural group selection, and, of course, if we eliminate any call for the new "proletarian ethic", the conceptual framework becomes the same.

That can be curious, but isn't too surprising, if we think to the wide circulation which the same sources (social DARWINISM and MENGER's theory) obtained in the Austrian cultural climate. In 1922, HAYEK turned away from FABIAN socialism and began his collaboration with VON MISES, of whom he stated "There is no single man to whom I owe more intellectually". The concept of 'praxeology', methodological individualism, evolutionary epistemology and antisocialism were the main gifts of this heritage: a big research program which HAYEK's extraordinary mind contributed to develop in many details. Nonetheless, his conclusions aren't completely indisputable.

Let me quote from CAMPBELL/HEYLIGHEN (1995): "Libertarian theorists and laissez-faire economists recommend using market mechanisms as the overall control, while avoiding all centralized interventions except for the protection of private property and inherited wealth. Yet, we believe that market mechanisms cannot operate effectively without the contribution of the other social control mechanisms … In particular, legal control … is necessary to ensure that positive feedback loops do not get out of hand, and that investments are made in areas where the lack of local or short term benefits precludes pri-

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vate investment." How does the new CAMPBELLIAN theory of these social control mechanisms differ from his old one? In his first essays CAMPBELL adopted the orthodox way of thinking that:

1. individual selection always dominates group selection at the biological level;
2. this notwithstanding, groups are real and self-sacrificial altruism in the service of human social groups genuinely exists;
3. therefore self-sacrificial altruism has to be seen as a result of cultural group selection of ideologies, social-organizational traditions, moral indoctrination, and religious cosmologies.

A "simple point of view" that CAMPBELL, in the last part of his life, seemed willing to modify, retaining its relevance for secondary groups, but reinterpreting the role of group selection for social control mechanisms within primary groups. Why? Because, at the biological level, SOBER/WILSON (1994) proposed that group selection and individual selection could be concurrent, "producing an ambivalence on group preservation vs. individual preservation dimension". To understand CAMPBELL's new perspective, we have to remind ourselves that two classical problems are here clearly intertwined:

A. The individuation of the real units of selection. SOBER/WILSON (1994) reacts against WILLIAMS' (and DAWKINS') theory according to which the gene is the fundamental unit of selection because it is a replicator and higher levels of selection are only theoretically possible but unlikely to occur in nature. They point out that in WILLIAMS' scheme, individuals aren't real units of selection either. On the contrary, both individuals and groups are environments of genes. SOBER and WILSON realize that "WILLIAMS' case against group selection was strengthened by two other theories in evolutionary biology: … inclusive fitness theory (also called kin selection; HAMILTON 1964, MAYNARD-SMITH 1964) … [and] evolutionary game theory (AXELROD/HAMILTON 1981, MAYNARD-SMITH 1982, TRIVERS 1971, WILLIAMS 1966), which explained how altruism could evolve among non-relatives" (see SOBER/WILSON 1994). According to WILLIAMS and DAWKINS, however, even sexually reproducing organisms do not qualify as units of selection because they, like groups, are too ephemeral. Therefore DAWKINS (1976) proposed a completely new concept (except for KAUTSKY), i.e., "vehicles of selection", which is very similar to HULL's concept of "interactors" (see HULL 1980). According to the metaphor of vehicle selection, we can say that genes in an individual are like "the members of a rowing crew competing with other crews in a race. The only way to win the race is to cooperate fully with the other crew members". Yet, if individuals can be vehicles of selection, what about the groups?

B. The plausibility of hypothesizing a nested hierarchy of different levels upon which the selective process could act. "Taking vehicles seriously requires more than acknowledging a few cases of group selection, however; it demands a restructuring of the entire edifice … There is one theory of natural selection operating on a nested hierarchy of units, of which, inclusive fitness and game theory are special cases… Adaptation at any level of the biological hierarchy requires a process of natural selection at *that* level." (SOBER/WILSON 1994, italics mine). *HEGEL is vindicated*. In a nested hierarchy of units of selection a dialectic process is going on. A few pages later SOBER and WILSON say: "Since group selection is seldom the only force operating on a trait, the hierarchical theory explains both the reality of groups that CAMPBELL emphasizes, and the genuinely individualistic side of human nature that is also an essential part of his thinking".

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Franz M. Wuketits

Four (or Five?) Types of Evolutionary Epistemology

Donald Campbell and the Constructivist Approach

Abstract

This contribution aims at giving a brief discussion of CAMPBELL's attempt to systematize the different approaches to evolutionary epistemology. It is argued that, as CAMPBELL repeatedly stated, 'evolutionary epistemology' covers various conceptions and comprises different views that hold in common only the assumption that all types of cognition and knowledge result from evolution and that, therefore, the study of evolution is relevant to an understanding of cognitive phenomena of whatever kind. While CAMPBELL distinguished between four types of evolutionary epistemology, I think that there are more and that CAMPBELL himself, in his later work, showed some sympathy to the so-called constructivist approach.

The meaning of CAMPBELL's work for the evolutionary-epistemology enterprise is undisputed. In his seminal paper (CAMPBELL 1974) he systematically outlined—and commented on—theories that deserve the name 'evolutionary epistemology' and are rooted in the evolutionism of the last century. To be sure, this is not to say that CAMPBELL was just compiling the work of other scholars, naturalists and philosophers, who had contributed to the development of evolutionary epistemology. He himself contributed so much to this development that it would be risky to evaluate his efforts in a short paper. However, the aim of the present paper is not to give a comprehensive survey of his thoughts concerning evolutionary approaches to cognition and knowledge. I shall concentrate on his attempts to systematize these approaches and try to mark his own place in the evolutionary-epistemology movement.

Theories of Evolution and Evolutionary Epistemology

If one takes the theory of evolution really seriously, then one will come, sooner or later, to the conclusion that cognitive phenomena too are results of evolutionary processes. This is trivial. CAMPBELL (1974, p413) was of course right that "an evolutionary epistemology would be at minimum an epistemology taking cognizance of and compatible with man's status as a product of biological and social evolution". However, there is not simply *the* theory of evolution; we have to take into account different explanations of the processes and mechanisms of evolutionary development and thus to distinguish between different theories (WUKETITS 1988). Hence, it should not come as a surprise that evolutionary epistemology is not a monolith, but a wide field of ramifying theories (WUKETITS 1990), so that we are confronted with different types of this epistemology.

In his posthumous paper, CAMPBELL speaks of four types of evolutionary epistemology and characterizes his own model which is a selectionist one. Indeed, from the beginning on he was engaged in the natural-selection program of evolutionary epistemology. As he once stated: "I began my career as an 'evolutionary epistemologist' by applying selection theory to trial-and-error learning, incorporating gestalt models of problem solving…, to visual perception…, and to creative thought" (CAMPBELL 1990, p7). For natural-selection theories have predominated evolutionary thinking since DARWIN, one should expect their impact on evolutionary reflections with regard to cognition and knowledge. However, this type of theories ha.s always been compatible with—and has in fact supported—the *adaptationist* program of evolutionary epi.stemology (see e.g., LORENZ 1977) according to which cognitive structures of organisms are to be understood as adaptations to external reality, so that a correspondence between the 'inner' and the 'outer' world of any organism can be supposed (see e.g., WUKETITS 1984 for discussion).

General-selection models of cognition—that are not necessarily always in tune with strict adaptationism—have also been proposed to explain the analogies between organic evolution and the evolution of scientific ideas or what HULL (1988) calls the *conceptual development of science*. CAMPBELL is entirely right that elaborating an appropriate evolutionary theory of science requires a broad selection theory in which both, organic and conceptual evolution, are just 'clusters of exemplars' (see also CAMPBELL 1988), and that simply applying the organic-evolution model to the development of science does not suffice. Anyway, his own reflections on these problems and his proposed solutions are best examples for a refined type of *naturalism* and have helped us 'taking the naturalistic turn' (CALLEBAUT 1993).

The Constructivist Approach

There is one type of evolutionary epistemology that is not mentioned by CAMPBELL in the present paper, although he himself has contributed much to its development: the constructivist approach. This is, in a nutshell, the view that organisms do not simply 'represent' or 'reconstruct' their environment, but also—the radical constructivist says: only—construct and interpret it. CAMPBELL (in CALLEBAUT 1993, p298) takes as an example the salamander's leg: "When it regenerates when broken off, does it regenerate until it reaches the ground? No! It regenerates until an internal vicarious monitor for leg length is completed." Clearly, there must be something like 'internal selection' for living systems are not marionettes hanging on the strings of their environment.

Those who have recognized the importance of this kind of selection—with organismic constraints and internal building blocks in the organization of living beings—have pleaded, then, for a constructivist extension of evolutionary epistemology though their starting point was adaptationism (RIEDL 1995). Also, it has been argued that the correspondence theory of cognition/knowledge has to be replaced by a *coherence theory*, this is to say a view according to which getting to know reality is strongly connected to success in life—and not so much to a 'true picture' of what is out there (cf. OESER 1987; RUSE 1986; WUKETITS 1995). A stricter constructivist approach to (evolutionary) epistemology has been formulated by DIETTRICH (1994, p57) who states that "within the constructivist evolutionary epistemology … the regularities which we condense to the laws of nature are seen as the invariants of phylogenetically formed cognitive operators".

All these interesting extensions of evolutionary epistemology can not be detailed here, for my point of reference is CAMPBELL. He refers to his earlier model (CAMPBELL 1960) as to the '1960 dogma' and is presenting, in the recent paper, a modified view. He wishes "to identify 'knowledge' … with those *vicarious* processes which short-cut selection by the life and death of genetic variants". But already in one of his previous papers, published jointly with PALLER, we can find the following statement: "In the evolutionary epistemology movement, there is too much uncritical passing-the-buck to an omnipotent 'Dear Old Mother Natural Selection'. But clair-voyant vision is mechanically impossible and could not have been produced by evolution. Instead, evolution has opportunistically exploited marvelously effective proxy-variables of limited appropriateness" (CAMPBELL/PALLER 1989, p236). Unpacking this passage and CAMPBELL's posthumously published manuscript, we can formulate the basic assumptions of a constructivist approach as follows:

1. The cognitive capacities of any organism are limited;
2. these limitations are mainly the result of organismic, functional constraints;
3. as active systems, organisms explore those aspects of 'reality' that are relevant to their survival;
4. the act of perception always includes an interpretation of the perceived object;
5. this interpretation is based on the organism s own experience that is constrained by the evolutionary pathways of its species.

This approach to evolutionary epistemology is, of course, not *anti*-adaptationist, but it is *non*-adaptationist. It can meet one of the standard counterarguments which reads as follows: "The fundamental error of evolutionary epistemologies as they now exist is their failure to understand how much of what is 'out there' is the product of what is 'in here'. Organism and environment are codetermined" (LEWONTIN 1982, p169). Yes, they are, and the constructivist version of evolutionary epistemology

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is in fact at minimum an epistemology taking cognizance of and compatible with any organism's status as an active system that *actively* interacts with what is 'out there'.

Conclusion

CAMPBELL's evolutionary epistemology has a forty-year history and underwent some changes and improvements within this period of time. His present paper is the best example of these changes. As a personal retrospective history of evolutionary epistemology it shows that CAMPBELL himself was aware of some shortcomings of the traditional version of this epistemology, i. e., the adaptationist approach, and that, after all, he developed another type of the evolutionary approach to understanding cognition and knowledge. This is a remarkable and essential development in a field that some already tend to consider obsolete. One question, however, remains: Will—and can—there be a *unified* evolutionary epistemology?

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