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Introduction

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The breadth of Donald Campbell's scholarship was such that he made significant, enduring contributions to social psychology, anthropology, sociology, education, science studies and epistemology. A previous volume in his honor (Brewer and Collins, 1981) provided an admirable representation of this breadth, with chapters sampling many aspects of Campbell's research, from the technicalities of program evaluation to the abstractions of hypothetical realism. The present volume, in contrast, focuses on Campbell's studies of knowledge; on his elaboration of the evolutionary naturalistic epistemology which he ultimately preferred to call "selection theory" (Campbell, 1997).

Although he had been publishing on epistemologically relevant topics since the 1950s, in 1986 when I first met Campbell, he continued to see himself as a "pushy outsider" in relation to the community of epistemologists and philosophers of science. This collection of essays does not contradict that typically mocking and astute self-description, but it confirms that the outsider got a long way in; that his central epistemological claims were heard, and that they have

effected change and excited controversy that will continue to influence debate in the philosophy and sociology of science for generations.

This introduction has three purposes: to provide a brief biography of Donald Campbell, sketching the roots of his epistemological interests, and including references to his work in this field; to locate Campbell's selection theory in relation to traditional epistemological concerns and social constructivism; and to give an overview of the chapters in this volume.

Biography

Donald T. Campbell (DTC) was born in 1916 in Grass Lake, Michigan, to parents whom he often described as being ready to converse with their children and to answer sensible questions. His grandfather, uncle, and cousins were members of Appalachian Bible-belt free churches, and although DTC had "By high school or early college . . . drifted away from whatever belief in God [he] had had as a child" (Campbell 1988a, p. 21), he recognized in himself the zeal and persistence of an evangelical itinerant preacher.

DTC had great respect for research communities outside the spotlight of premier academic institutions. In line with this, he identified the start of his scholarly career, not with the beginning of his undergraduate years at Berkeley in 1937, but with his arrival at San Bernardino Valley Junior College in 1935. There he learned evolutionary theory from a squirrel hunter and developed his conception of the "worker scientist"; while at Berkeley, as an undergraduate and graduate student in psychology, he was influenced by Egon Brunswik, Edward Tolman, and Robert Tryon. He was involved in data analysis on Tryon's project examining inheritance of maze-running ability in rats, and attended a small seminar group with Tolman and Brunswik, both behaviorist psychologists with a strong interest in philosophy of science and commitment to logical positivism. In contrast with other behaviorists of his day, Tolman saw learners—rat and

human—as meandering explorers hoovering up information about their enclosing maze, only a fraction of which would ultimately be useful. So strong was this image in DTC's mind that fifty years later he would impersonate Tolman's intrepid rat, whiskers aquiver, to impress on me, a new post-doctoral student, the inevitably chanciness and wastefulness of knowledge acquisition.

DTC completed his doctoral dissertation on "The Generality of Social Attitude" in 1947, having interrupted his studies from 1941–46 to serve the U.S. war effort, first as a research assistant with Tryon in Washington, and subsequently commanding a naval gun crew on merchant ships sailing between the U.S. and England. In the former role he was exposed to German, Russian, and Italian home propaganda, providing an object lesson in cultural relativism. Having read social science discussions of Nazi ideology and Hitler's *Mein Kampf*, he expected Germany's internal presentation to emphasize the rights of the strong, but found that, like the home propaganda of the Allied nations, it presented the war as essential for defense. DTC said that during his time as a naval officer he "allowed professional content to drop out of [his] life altogether" (Campbell 1988a, p. 5), but that didn't stop him from using a 24-hour leave in 1944 to make a pilgrimage from Liverpool, where his ship was docked, to Cambridge to meet F. C. Bartlett, who was famous for his views on the dynamic, selective character of memory. DTC had traveled for 22 of the 24 hours and, unknown and unexpected, got a cool welcome from Bartlett, but he did not report the episode with any sense of folly. It implies that he was hungry for scholarly contact, and, like the Tolman link, that he was already drawn to selectionist models of knowledge in any available form.

During his first faculty appointment, at Ohio State University (1947–50), DTC taught social psychology, did research on opinion-polling and leadership, and developed his epistemological interests by joining a philosophy of science table each Friday at the faculty club. Kurt Wolff, who was also a regular participant in these discussions, introduced DTC to the sociology of knowledge, and the development of this inter-

est was reflected in his inaugural colloquium, "On the Psychological Study of Knowledge," at the University of Chicago when he became an assistant professor there in 1950. DTC was at the University of Chicago for three years, first as part of the Committee on Education Research and Training in Race Relations, and subsequently in James G. Miller's Committee on Behavioral Sciences. The latter affiliation led him to focus on cybernetics, information theory, and general systems theory, and thereby to examine the relationships between W. Ross Ashby's cybernetics and other natural selection analogues to learning theory and perception. This work appeared a couple of years later in "Adaptive Behavior from Random Response" and "Perception as Substitute Trial and Error" (Campbell 1956a; 1956b), Campbell's first published papers on what he would later describe as "evolutionary epistemology" and, later still, "selection theory."

In 1953, DTC moved from the University of Chicago to Northwestern and remained there for 26 years. Over this period he worked as a social psychologist on social attitude measurement, ethnocentrism, and, most famously, developed techniques of "quasi-experimentation"—research designs and statistical methods enabling rigorous investigation of the effects of manipulated variables under conditions in which random assignment of subjects to groups is impossible. This work made him one of the most frequently cited psychologists in history, and earned him numerous honors, including a Fulbright Professorship at Oxford (1968–69), the American Psychological Society's Distinguished Contribution Award (1970), Presidency of the American Psychological Association (1975), Fellowship of the American Academy of Arts and Sciences (1973), and Membership of the U.S. National Academy of Sciences (1973). Alongside these achievements, however, DTC maintained a strong interest in theory of knowledge, teaching a seminar course on "Knowledge Processes" throughout his time at Northwestern, and regularly publishing essays in which one can trace the development of his selection theory (e.g., Campbell 1956a, 1956b, 1958, 1959, 1960, 1965a, 1966, 1969, 1970, 1973, 1974a, 1974b, 1975b, 1977). High points in this series were the essay "Evolutionary Epistemology," which appeared in a volume honoring Karl Popper (Campbell 1974b), and his William James Lectures, given at Harvard in 1977 and circulated widely in preprint form until their publication eleven years later (Campbell 1988b).

For personal reasons, DTC moved to Syracuse University in 1979, and from there to Lehigh University in 1982, where he remained a University Professor of Social Relations and Psychology until the end of his career. From 1979 onwards, theory of knowledge was his principal focus and the majority of his publications were in this area (e.g., Campbell 1979a, 1979b, 1981b, 1981c, 1982a, 1983a, 1983b, 1984, 1985, 1986a, 1986b, 1987a, 1987b, 1988b, 1988c, 1988d, Campbell & Paller 1989, Campbell 1990a, 1990b, Campbell & Cziko 1990, Campbell 1992, 1993, 1994b). When his final illness began, he was in the middle of assembling an overview of his selection theory, reiterating its principal claims, and indicating those that he would now add or adjust. This final paper was published one year after his death in 1997 (Campbell, 1997).

Epistemology and Social Constructivism

Metaphysics and epistemology are two of the basic areas of philosophy. Metaphysics concerns the fundamental constituents of the world in which we live, while epistemology deals with how we come to know about this world. To a modern reader, metaphysics might sound like physics and epistemology like psychology. The basic constituents of our universe are quarks, prions and the like, while we come to learn about these basic constituents by the methods of science. In the early days of modern philosophy, the ambiguity implicit in the preceding descriptions would have gone unnoticed. Now that science has split off from philosophy, philosophers can be seen to be involved in quite a different activity. Perhaps knowing a good deal of science might help philosophers, but such knowledge is far from sufficient. As far as metaphysics is concerned, philosophers worry about whether a

metaphysics of entities and their properties is adequate or whether reference to fields alone will do the trick. Are mental entities part of the furniture of the universe, or can they be explained, possibly explained away, entirely in terms of properties of matter?

Epistemology deals with the justification of whatever beliefs we come to hold. In the early days of modern philosophy, unrealistic standards were assumed in deciding which of our beliefs are justified. Either we know that the earth is roughly spherical in shape with absolute certainty or we know nothing at all. A couple hundred years of philosophical debate resulted in philosophers abandoning such unrealistically high standards for justified knowledge. Another assumption at the time was that the place to begin establishing standards of justified knowledge is the individual human being and his/her interactions with the world in which we live. As it turns out, from this perspective, very little if anything can be known, even on a reasonable notion of justified belief. We do not learn in isolation from all other people. Just as languages are inherently social, so is the acquisition of knowledge. The rallying cry of recent social constructivists is that science is inherently social, and knowledge of social relations is absolutely necessary if we are to understand science. The more ambitious claim of social constructivists is that such social knowledge is also sufficient to explain all the claims made by scientists.

In the past twenty years or so, a variety of novel methods have been used to study science. Scientists deliver papers, discuss their research with each other, publish papers, etc. All of these activities require language. Rhetoricians study the use of language by such language users as politicians and film critics. They have also extended these same modes of explanation to scientists. No one denies that the use of language by scientists has a rhetorical dimension. The issue is whether science is nothing but rhetoric. In the case of all the novel methodologies that are being used to aid in our understanding of science, the conflict arises with respect to the "nothing but" clause preferred by reductionists.

All of us, scientists included, live in a variety of societies and subsocieties. What effects do these societies have on us?

One of the chief occupations of social constructivists is to point out assumptions built into the language that scientists use which seem obviously a feature of their society rather than the particular subject matter of the scientists. For example, evolutionary biologists ask how it is possible for organisms to cooperate in such a competitive process as gene-based biological evolution. They do not ask how it is possible to compete in such a cooperative enterprise. Could it be that we are reading a feature of our societies into the biological world?

Plotkin (this volume) uses the term "social construction" in the context of reductionism. Can descriptive statements made about societies be reduced to psychology or, even more ambitiously, biology? Reductionists claim that in principle, they can, while antireductionists claim that both in practice and in principle they cannot. A $50 note is more than a piece of paper with some printing on it because that is the way that people treat it. In principle, societies are nothing more than people and their interrelations, but the number of people in a society is so large and these interrelations so variable that claims about in-principle reduction may be pie in the sky. In practice, no such reductions can be carried out, nor is there anything gained by ignoring the complex of social relations that characterizes societies. Redescribing a $50 bill in terms of paper and ink accomplishes nothing. However, that Plotkin, a Briton, refers to a $50 "note," while an American refers to the same piece of paper as a $50 "bill," says something about differences in the societies to which the speakers belong.

Total redescription is one thing. Partial redescription is something else. Perhaps not everything about human beings, their psychology and social relations, can be redescribed so that the basic principles of behavioral psychology apply universally, but large segments can be, and these conceptual revisions add to our understanding of the human animal. Picture a behavioral psychologist trying to handle a Freudian dream interpretation. Similar observations hold for the relation between biology and both psychology and sociology. For a very long time both psychologists and sociologists have

been aware of what is now termed by evolutionary biologists "parent-offspring conflict." Such conflicts can be explained in part using the techniques of gene-based biological evolution. The point of contention is whether everything about such conflicts can be explained biologically without any reference to the basic principles of psychology and sociology. Conversely, can all of science, including the content of science, be explained totally as nothing but the effects of local, social phenomena? Did Darwin couch his theory in terms of competition among individual organisms because his own Victorian England was so individualistic and competitive? Can the beliefs of social constructivists be explained in the same way?

As Caporael (this volume) points out, Donald Campbell was both a Darwinist and a social constructivist of sorts. In its early versions, evolutionary epistemology was extremely literal. Those organisms that misjudge their environments pay the price of having few, if any, offspring. Some characteristics of organisms, including the human species, can be explained in terms of gene-based biological evolution, but a great deal cannot. Genes have had very little to do with the rise of formal logic in this century, especially since so much of formal logic is so strongly counterintuitive. Perhaps genes provide the necessary prerequisites for coming up with any sort of logic, but that is all. Genes have also had very little to do with the rise in quantum mechanics and relativity theory, both of which are very counterintuitive to ordinary people. As Hull (this volume) argues, Campbell was a "Darwinian" only in the sense that gene-based biological evolution provided one instance of a selection process. He was also a social constructivist in the sense that he thought that the justification of scientific knowledge can be traced to the social organization of science—from evolutionary epistemology via selection theory to a sociology of scientific validity. The status of facts as facts, even social facts, is determined in the context of particular social organizations. Individual scientists are objective in a variety of ways and to various degrees, but the sort of objectivity that counts in science is group objectivity. Because of the social organization of science, scientists are strongly pressured into behaving objectively, like it or not,

and enough scientists like it enough of the time for science to have the traditional epistemological characteristics that it is reputed to have.

Overview

Most of the chapters in this volume are concerned with Campbell's claim that all increases in "fit" between systems and their environments arise through the operation of "nested hierarchies" of mechanisms, each operating on the basis of "blind variation and selective retention" (BVSR). The entities involved in variation and selection are themselves various—for example, antibodies in the domain of immunology, hypotheses or ideas in the arena of creative thought, research groups in science, and genes in the seminal case of adaptation through selection—but within each mechanism there is a proliferation of entities, only some of which survive and further proliferate, yielding novelty and increased organization. Campbell's classic statement of this view is his 1974 essay "Evolutionary Epistemology." When we asked each of the contributors to this volume to pick the article by Campbell that had influenced their own work the most directly and substantially, they all cited this paper, a transcript of Campbell's William James Lectures.

In his chapter, Gary Cziko shows how Campbell's BVSR model can be applied to a broad spectrum of cases, from gene-based biological evolution and the reaction of the immune system to antigens, to learning and conceptual change in science. In addition to providing a broad and lucid survey of cases considered by Campbell, he discusses at greater length the operation of BVSR mechanisms in neuroscience, and thereby provides a valuable introduction to a growing field of interest.

In such a broad survey it is possible for Cziko to demonstrate only that, in any given domain and at any given level of organisation, a BVSR mechanism *could* be in operation. It is not possible to consider in detail alternative hypotheses and the kind of evidence that favors, or would favor, the

BVSR option. Campbell's assertion that BVSR is necessary for increases in fit may be taken to mean that there are no such alternatives, but this would be a mistake. His model implies that increases in fit will always be traceable to BVSR in some part of a system, but not that every subcomponent or level of a system will have BVSR characteristics. Thus, the empirical challenge presented by Campbell's selection theory is to identify *where* in any given system BVSR is occuring, to delineate the entities on which it is based, and to model the system-specific mechanisms of proliferation and selective retention.

Cziko also answers some objections to Campbell's evolutionary epistemology raised by Michael Bradie in the chapter that follows. Bradie expands upon his distinction between two programs that are subsumed by evolutionary epistemology: EEM investigates the evolution of epistemic mechanims, and EET the evolution of epistemic theories. In EEM, genetic variation and natural selection are taken literally. Certain ways of perceiving and thinking about the world in which we live are literally programmed into our genes. Bradie sees problems primarily with respect to EET, raising three principal objections. First, he believes that Campbell did not develop one of his central metaphysical concepts, that of "fit" between a system and its environment, in sufficient detail and in a fashion consistent with recent work in evolutionary biology. Second, Bradie claims that Campbell's application of evolutionary epistemology to scientific change is based on a flawed analogy between organism and belief evolution. Organisms, he argues, are selected by "the world in its brute facticity," while in science, beliefs are selected primarily by an environment consisting of other beliefs, data, and explicitly formulated selection criteria. Since by their very nature all analogies are incomplete, we must ask whether the differences between organism and belief evolution *make* a difference. According to Campbell, the principal significance of the *similarites* is that they demonstrate that BVSR is necessary for adaptation and knowledge acquisition, but it is the analytic character of this claim which is the focus of Bradie's third major objection to Campbell's evolutionary epistemology. He argues that if the

claim is analytic, then it is untestable, and that untestable claims neither have explanatory power nor a place in descriptive or naturalistic epistemology.

Rather than focusing on the BVSR evolutionary aspect of Campbell's epistemology specifically, two of the contributed chapters, by Ronald Giere and Michael Ruse, proceed from Campbell's commitment to "descriptive" or "naturalistic" epistemology. Believing that this is the only kind of sustainable epistemology, Campbell argued that the search for ultimate justification of knowledge claims must be abandoned, but in favor of "hypothetical realism," not relativism, instrumentalism, or pragmatism. Hypothetical realism posits the existence of an external reality which can be known through perception, learning, and scientific enquiry, but it acknowledges that we can never know with certainty whether specific beliefs arising from these processes are true or justify the global commitment to an external reality. According to Campbell, realism is a hypothesis favored not because it is justified or can provide justification but because it makes sense of, or coheres with, what we (hypothetically) know about evolution, learning and science (Campbell, 1997).

A potential answer to Bradie's third objection, and a great deal more, is provided by Giere in his paper recommending that naturalistic epistemologists take a "methodological turn." In taking such a turn, Campbell's claim that BVSR is necessary for improvements in fit would be recast as a methodological principle characterizing his evolutionary naturalistic epistemology, for example: *For any instance of fit between a system and its environment, seek a BVSR explanation*. Under such a formulation, the principle does not seem to require the kind of transcendental justification that is fundamentally incompatible with naturalism, nor, apparently, does it need to be testable. It could contribute to the production of scientific explanations by promoting the development of specific BVSR hypotheses—hypotheses about the identity of variant generating and selection mechanisms in a given natural domain—which are testable in that domain.

Campbell might have embraced this reformulation of his BVSR claim, and as an avid supporter of maps over words as

metaphors for knowledge he would almost certainly have shown enthusiasm for Giere's "perspectival realism," a subtle thesis emphasizing the partiality (incompleteness and interest-relevant character) of scientific knowledge. However, it sometimes seems that Giere is content to eschew ontological questions where Campbell insisted that they be raised. For example, Giere argues that we can use the concept of "fit" between a scientific model and the world without asking whether the model is true, in the correspondence sense, or whether it refers. Campbell, on the other hand, repeatedly insisted that correspondence truth and valid reference should be the explicit goals of science, alongside humble acceptance that whether or not these goals have been achieved can never itself be known.

In contrast with Giere, and in common with Bradie, Ruse takes testability to be the hallmark of naturalistic epistemology. He elaborates five hypotheses about the role of external cultural values in science, and, focusing on biologists' beliefs about progress, attempts to test them against a 200-year historical record of the development of evolutionary theory. After delineating the alternative hypotheses and taking the reader on a fascinating, lightning tour of the relevant history, Ruse concludes that theory development is accompanied by an increase in the influence of internal, epistemic values at the cost of external cultural values, and that the pressure for this transformation comes from scientists' efforts to attain professional status. This conclusion concedes a good deal of ground to the social constructivist portrayal of scientific change while holding fast to substantial remnants of a more traditional, philosophical view, and thus represents a blend of these perspectives very similar to that which Campbell favored.

Kyung-Man Kim addresses the character of variation in BVSR, and in the process responds both to Ruse's early criticisms of the BVSR model of scientific problem solving and some of Bradie's concerns about the "organism and belief evolution analogy." The debate centers on two questions: 1) What did Campbell mean when he claimed that variant generation in science, as in all other domains, is "blind"? 2) What is the

significance of the fact that scientists *try* to produce successful hypotheses; that they are intentional problem solvers? Kim argues that Ruse's objections to the application of the BVSR model to science are based on misinterpretation of "blind" to mean "random," and on the insupportable assumption that intentional problem-solving involves some kind of prescience. In this context, it is interesting to note that towards the end of his chapter on progress in evolutionary theory, Ruse emphasizes that contemporary evolutionists actively seek to suppress the influence of external cultural values on their theorizing, and apparently takes this effort as evidence that they succeed.

While Kim defends Campbell's BVSR model of science, Caporael challenges and extends it. Through a close reading of Campbell's work, Caporael first provides a vivid image of the tension that he believed to exist between human social dynamics based on genetic evolution, and the culturally evolved goal of objectivity in science. She portrays Campbell as having seen the (hypothetical) success of science as something which occurs in spite of human sociality, and seeks to replace this view with one that escapes gene-culture duality, emphasizes the similarity between the social dynamics of science and of hunter-gatherer societies, and casts these dynamics as more naturally cooperative than Campbell and other evolutionists are typically willing to accept.

A key component of Caporael's model of the evolution of sociality is the claim that "core configurations," types of social groups, are repeatedly assembled in cultural evolutionary time. The term "repeated assembly" indicates that formation of these groups, rather than being genetically determined, is influenced by cognitive abilities that have co-evolved with the configurations themselves, and by consistent environmental demands from the asocial environment. Although Caporael's model contrasts with Campbell's, she makes use of an important Campbellian "tool": the notion of "downward causation" (1974a, 1990, 1994b).

Plotkin also uses the concept of downward causation, but his project is very different from Caporael's. Phillip Kitcher has warned that culture will not come within the

explanatory reach of natural science "without a serious p chological theory onto which the considerations about (tural transmission can be grafted." Guided by this challer Plotkin tries to identify the kind of psychological the which could provide a basis for this grafting, and conclu that the best candidate is a nativist, Chomsky-esque the of language. Thus, in contrast with Caporael, who rega Campbell as having overestimated the role of genetic mec nisms in shaping cultural beliefs and practices, Plot emphasizes the degree to which BVSR at the genetic le influences cultural change.

In the final chapter, David Hull looks very carefully the temporal development of Campbell's naturalistic epi mology. The general view is that Campbell was reasoning analogy from gene-based selection in biological evolutior other forms of BVSR, but from his earliest writings Campl was attempting to develop a general analysis of selection t applies equally to all forms of selection. Hull also argues t in Campbell's later years he came to think that the epi mological warrant of science flows not from the involvem of selection *per se* but from its social organization, w Campbell called "the sociology of scientific validity."

Each of the authors of this volume acknowledges importance of Donald Campbell's contributions to episten ogy by criticizing, defending and/or extending them. In ac tion, and without any prompting from the editors, each c tributor chose to pay tribute to Campbell in a personal n at the beginning or the end of his or her chapter. These pa graphs describe the extraordinary warmth, generosity, playfulness of a great scholar.