

Guest Editorial

## Confessions of a Closet Sociobiologist: Personal Perspectives on the Darwinian Movement in Psychology

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On reflection, I began to realize that I might be a latent sociobiologist as early as graduate school. Try as I did, I could not become aroused by secondary drives, variable-interval reinforcement, and the like. I felt there must be more to myself and to the universe of behavior than conditioned responses. Watching an infant make his first smile, or move smoothly into language from babbles and coos, implied to me that our species, as all others, enter the world with considerable baggage. Nevertheless, I read my Skinner and Hull as was expected of me but turned to Desmond Morris and Robert Ardrey for enlightenment.

Nor was it generally beneficial to raise the issue in the well-defended bastion of pristine environmentalism that defined psychology in the late 1950s and early 1960s. During a class on operant conditioning, I asked whether anyone had placed a rat trained to press a bar for food into a naturalistic setting to see if it would get on its hind legs to press twigs or similar protuberances. The instructor shrugged and said that it would probably begin to burrow. The issue was irrelevant.

Subsequently, I heard the esteemed ethologist, Daniel Lehrman, introduce his colloquium with a similar tale. He described how he had approached B. F. Skinner after a talk by the latter and asked, quite sincerely, about using operant conditioning methods to alter the courtship rituals of his ringdoves. Skinner replied, as casually as my instructor, that, "operant techniques didn't seem to work well with natural behaviors." Lehrman then asked pointedly of his audience, "Can anyone tell me then, why we should be interested in them?"

One indication to me that redemption might be at hand was, *The misbehavior of organisms* (Breland and Breland, 1961) depicting cases in which animals conditioned by food reinforcers had spontaneously reverted to species-specific, adaptive behaviors. For example, a chicken conditioned to propel plastic capsules with its beak tried to peck them open, as if they were food pods; a raccoon conditioned to place coins in a bank slot began to rub them together instead, as if

washing its food. The authors concluded that, "... the behavior of any species cannot be adequately understood, predicted, or controlled without knowledge of its instinctive patterns, evolutionary history, and ecological niche." (pp. 683-84)

*The misbehavior of organisms* is still one of the most widely quoted papers in introductory psychology texts, but the field remains largely oblivious to ultimate causation, working virtually exclusively in the realm of proximate causes and ontogenetic development. Ask psychologists why men tend to be more aggressive than women. A physiological psychologist will probably answer that male testosterone levels are higher. A social psychologist might say that males possess stronger tendencies to establish status hierarchies within their social groups. A developmental psychologist will tell you that males are socialized to be more aggressive by their parents and society. All are valid answers, as far as they go. None, however, deal with ultimate origins. Why is testosterone level linked to aggression? Why do some species, humans among them, form social groups and status levels within these and some not? Why do parents, across cultures and species, tend to treat male offspring differently than females?

Thus, psychology has yet to develop a general theory. A science cannot develop a general theory while eschewing the genesis of the phenomena it attempts to explain (which is why all other natural sciences deal with ultimate causation), and a science cannot meaningfully progress without a general theory. Psychology has *expanded* greatly in the last 40 years, laterally, into an increasing number of areas and sub-areas, but it has not meaningfully progressed.

For my own part, I stayed in the closet for quite a few years into my academic career – not because I was afraid to emerge, but because I couldn't find an exit. I did a fair share of research and writing, on topics as diverse as attitude change, aging, problem solving and phobias, but I did not have a sense of coherence about the body of my work and I felt more like a dabbler than a serious scientist. This feeling, in fact, led to a study by one of my graduate students and myself (Shulman and Silverman, 1972) in which we found that, on a yearly basis, close to 90% of the approximate 2400 references cited in *The Journal of Personality and Social Psychology* appeared one time only, and less than six percent appeared more than twice. Further, across the three-year period of the survey, there was a significant increase in the percentage of citations appearing just one time. Thus, the aimlessness that I felt characterized my own work seemed endemic to the field.

## **Coming out**

Devoid of the direction afforded by ultimate level questions and general theory, psychology continued to founder, drifting, as the social sciences are wont to do, into the domain of sociopolitics. This became particularly prominent in the 1970's, an era of intense sociopolitical activism in North America. In 1981, I

opined:

Psychology has become increasingly politicized, to the point where it seems to be all-encompassing. Trends in areas of public concern, such as the environment and women's rights, are now followed almost instantaneously by the creation of new sub-fields within the discipline, complete with journals and American Psychological Association divisions. Committees of the Association are forming for the expressed purpose of establishing inroads into public policy. The *APA Monitor* reads more like the newsletter of a Congressional lobby than a scientific organization. In our universities, basic research courses and programs remain stagnant at best while those of an applied nature seem to grow progressively, despite the absence of any breakthroughs to justify the transition. (Silverman, 1981, p. 86)

In contrast, evolutionary biology in the same era was witness to significant conceptual breakthroughs in the application of Darwinian theory to animal and human behavior, heralded by the works of Hamilton (1964), Williams (1966), Maynard Smith and Price (1973) and others. E. O. Wilson's (1975) comprehensive review and synthesis of the applications of Darwinian and neo-Darwinian theory to behavior marked the apex of the movement and provided its title of Sociobiology.

I had earlier made some forays into evolutionary based studies of human behavior, on the relationship of status and interpersonal spacing and on the nature and universality of female sex fantasies. These I presented in several conferences and colloquia, but the small frowns and quizzical looks of my colleagues deterred me from taking them any further or attempting publication. In retrospect, the *zeitgeist* for the assimilation of evolutionary theory into psychology was apparently at hand, but what was lacking was a model linking the theory to molar human behavior. Sociobiological concepts such as kin selection, evolutionary stable strategies, domain specificity, etc., provided the model and, on a personal level, enabled me to reconcile my vocation with my avocation and my science with my truths. Others had also found the grail, for I frequently found the names of colleagues, particularly in social psychology, in the sociobiology literature.

The surface topics of my studies remained as diverse as before (e.g. spatial perception, incest avoidance, ethnocentrism) but the theories driving the research were all bound, in some manner, to the larger question of the origins of human nature and capacity. This provided a stable framework from which to generate hypotheses, and a sense of conceptual unity and veridicality about the work.

## **Going back in**

One of my earliest contributions to sociobiology began as follows:

A sociobiologist evokes much the same responses from his traditional behavioural science colleagues as would a Marxist in a business school. Both may be tolerated if their ideas are kept in their place; small, agrarian societies, perhaps, for the Marxist; bower birds and beavers for the sociobiologist. If they venture beyond, however, they may readily elevate their statuses from minor oddities to dangerous demagogues. The Marxist becomes the usurper of freedom and progress; the evolutionist who casts his eye on humankind risks an array of assignations, including imperialism, colonialism, elitism, sexism, and racism. (Silverman, 1987, p. 205)

My prophecy became my reality two years later, in 1989. A representative of my University's *Sexual Harassment Education and Complaint Centre* (SHEACC) appeared in my evolutionary psychology classroom and informed me that she was there to observe my lectures, based on an anonymous student's report that I had created a "chilly climate" in the classroom. The specific complaint, I was told, was in regard to my statement that the female reproductive strategy of *hypergamy* (mating up in status) seemed to pertain to humans as well as animals.

A three-way standoff followed. SHEACC would not abandon its mission, the University administrators involved did not take it as their right or responsibility to insist that they do, and I refused to lecture under surveillance. The standoff was resolved in my favor two weeks later when my students petitioned for their own academic freedom to hear whatever I had to say (though the sudden interest of the news media in the matter may have also played a role). In response to my subsequent formal grievance, a University committee was struck to define SHEACC's proper role, and they eventually decided that it did not include visits to classrooms or review of course material.

The battle was won but the war continued. Asked five years later by the author of *Moral Panic: Biopolitics Rising* (Fekete, 1994) whether I thought I would prevail as readily then, I replied unhesitatingly in the negative (p. 239). In fact, my own encounter was rendered relatively trivial soon after it occurred by the events surrounding J. Philippe Rushton, a colleague in a neighboring university. Based on his theory about the evolution of the three major races, Rushton faced a serious threat of job loss, called for by the Premier of the Province, and was subjected to criminal investigation under Canada's "hate laws" (Gross, 1990). I am not aware of other sociobiologists whose experiences approached Rushton's. I do know, however, from informal contacts and from the literature of the

National Association of Scholars (NAS) in the United States, and its sister group in Canada, the Society for Academic Freedom and Scholarship (SAS), that a goodly number had been drawn into serious confrontations with their university administrations by their teaching and/or writing. Sociobiology had become a hazardous occupation.

Did my experience drive me back into the closet? Returning to my course with syllabus intact, I thought not. But I gradually became aware that I was looking over my shoulder, revising or qualifying or just dropping lecture material here and there. I considered that if I had not been a tenured full professor, I might have offered to teach methods courses and saved whatever I had to say about evolution and human behavior for my research papers. I have since wondered how many younger and more vulnerable colleagues are doing just that?

I realized also that I had retreated at least part way back into the closet before SHEACC came into my life, as had most of us in the field. The reader may have noted that my course was called Evolutionary Psychology rather than Sociobiology, the standard label for its subject matter at the time of its inception in 1984. This was not based on happenstance. I had realistic doubts about whether a course with sociobiology in the title would have been approved by the necessary academic committees. For the same reason, Evolutionary Psychology was also the title given the division of the Canadian Psychological Association I helped organize in 1986.

On a more global level, when the *Human Behavior and Evolution Society* (HBES), the most prominent group in the field, was formed in 1989, there was a brief, unsuccessful protest about the exclusion of the term sociobiology from the title. Defenders of the exclusion, including myself, offered one or another rationalization, but there was no serious doubt that the main reason was to try to keep the Society out of harms way. A more intense but equally futile protest ensued in 1996 regarding the change in name of the journal, *Ethology and Sociobiology* to *Evolution and Human Behavior*.

Finally, the *European Sociobiological Society*, the oldest formal group in the field, was incorporated into the *International Society for Human Ethology* in 2001. Whether prompted by political considerations or not, this probably marked the virtual total abandonment of sociobiology as a label, if not as a discipline. Of the 178 books relevant to evolution and human behavior published between 1999 and 2001 and listed on the HBES website, just five contain sociobiology in the title, with one of these being the anniversary edition of Wilson's original work and another, Segerstråle's (1999) history of the concept.

Was discretion the better part of valor? In his Presidential Address to the first HBES meeting in 1989, William Hamilton declared to the 200 or so present that we were a deme, striving to survive in a hostile surround. Taking Hamilton's analogy further, a deme must reproduce quickly and effectively in order to survive, and reproduction in the memetic realm of ideas requires placing students

in academic positions. Given the extent to which sociobiology had been besmirched by its detractors, that goal would have been exceedingly difficult without a change in label. Camouflage is a commonly used means of predator avoidance.

### **Out again, in disguise**

The new label was evolutionary psychology (EP). Colleagues and students often ask me whether EP is actually sociobiology reborn. This is not surprising, given that the claim has been made by both the founder of sociobiology (Wilson, 1998) and its most eminent critic (Gould, 1997). I usually beg the question, asserting justifiably that the question is unanswerable as posed because there have been so many diverse definitions of sociobiology used by its detractors (see Caplan, 1978; Segerstråle, 2000). It became a catchword for whatever was out of fashion in the fields dealing with evolution, genetics and human behavior.

Political exigencies aside, are there meaningful differences between sociobiology and EP? Cosmides and Tooby have defined EP as "... the application of adaptationist logic to the study of the architecture of the human mind" (1997, p. 14), based largely on their own model of evolved, domain specific, cognitive mechanisms (1992). Granted, the theory of domain specificity represents a significant theoretical breakthrough in cognitive psychology. Nevertheless, scientific theories should not be reified, and exclusionary approaches to theory building, particularly in the early stages of a discipline, discourage creative exploration and induction (Silverman, 1998a). As a definition of EP in its totality, I prefer something like. "Darwinian and neo-Darwinian approaches to the study of human behavior."

In that sense, EP is indeed sociobiology under a new banner. In fact, perusal of the *Handbook of Evolutionary Psychology* (Crawford and Krebs, 1998) or most any other contemporary review of the field, will reveal pretty much the same cadre of scholars or their students, pursuing the same topics, within the same theoretical frameworks, as we did when we all called ourselves sociobiologists.

Why did we have to hide behind a new label to survive? Who are our enemies and why?

I have written previously on these questions (Silverman, 1995, 1998b). My contention, simply put, is that the evolutionary approach is the only approach in the social and behavioural sciences that deals with why, in an ultimate sense, people behave as they do. As such, it often unmasks the universal hypocrisies of our species, peering behind self-serving notions about our moral and social values to reveal the darker side of human nature. Thus, evolutionary based studies of molar human behavior may readily be disquieting to those who tend, themselves, toward self-deception, or who believe that mass deception can and should function as a palliative for societal ills.

## **Acceptance**

Invited to contribute a chapter on recent influences of evolutionary theory on the discipline of psychology, Maryanne Fisher and I (Silverman and Fisher (2001) searched the PsychINFO database for the years 1970 to 1999. We assessed changes in the frequency of *evolution* as a keyword, compared to six control terms: *conditioning, personality, memory, psychotherapy, socialization and perception*. We found that the increase for *evolution* was more than 300%, significantly greater than for any of the other concepts.

We also sampled psychology introductory texts across the same time period, calculating changes in percentage of space devoted to evolutionary related content.<sup>1</sup> We found a marginally significant ( $p = .08$ ) increase in mean percentage from .1% to .3%, which roughly represents ½ to 1½ pages. Two other recent studies (Cornwall, et. al., 2000; Euler and Volland (2001), showed increases in magnitude similar to ours.

Though these differences may seem small in light of the PsychINFO data, it is likely that novel trends in the research literature require some time to become routinely incorporated into general texts. In fact, in all three studies the bulk of the increase came during the last five years. Encouraging also was the observation by Silverman and Fisher that the evolutionary material in introductory texts encompassed a wide range of topical areas, including learning, language, cognition, personality, emotion, sexual attraction, and familial and social processes.

In all, it appears that the deme is surviving well.

## **Note**

1. Texts specifically oriented to, or containing one or more chapters dedicated to evolution and behavior, were excluded.

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