

BIOLOGY AND FREEDOM: REPLY TO REVIEWERS

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Sociobiology

Sociobiology forms only a small part of *Biology and Freedom* (B&F). I begin with it because that part has caused most excitement. Dr Salthe even regards it as "the main point of the book" though it is not.

Several reviewers make the same criticism as I now do myself: that much of the analysis of human sociobiology in chapter 8 is of declining relevance. Unfortunately, a number of recent events suggest the contrary. Here are three. (1) I have just received a paper summarising two conferences, held in 1988, on evolution and human behavior (Low & Nesse, 1989). These authors refer with approval to several sociobiological works which have been justifiably mauled by many critics, but give no hint even that they have aroused controversy. (2) Since B&F was written, new, explicitly sociobiological works have appeared which represent human beings as *forced by their evolution* to be deceivers and self deceivers. Each begins with the *deceptive* appearances (cryptic and aposematic coloring, and so on) of many animals. (For documentation and further comment on *Homo mendax*, see Barnett, 1990.) (3) The only published reviews I have seen that dismiss B&F with scorn are by social scientists whose teachings are partly based on human sociobiology. But I do not know how widespread such teaching still is or to what extent it continues to influence public attitudes. These are matters for research.

Caplan

(1) Dr Caplan complains that I fail to deliver a promised alternative to pop biology. The "commonsense alternative" to which I refer on p. 227 is to reductionism, not to pop biology. On the same page I

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say that to end B&F with a supposedly complete portrait of *Homo sapiens* would be "pretentious and shallow;" and I still think so.

(2) Dr Caplan quotes part of a sentence from p. 238. The whole sentence is as follows: "The notion that everything that can be usefully said can be said in terms of physics is indeed incoherent, because the existence of physics itself rules it out." I continue that physics is a branch of knowledge, and so "implies a knower." This is not, as Dr Caplan suggests, an attack on physicists: it is part of an examination of the various kinds of explanatory reduction. Dr Caplan also comments that "almost no one believes this version of reductionism." I wonder whether he is right. Will most readers of B&F have worked out the implications of physicalism? In 1980, sixty-one advanced students of zoology were asked to comment on this sentence: *All biological phenomena can, in the long run, be explained in terms of the physical sciences*. More than half the students (and several of their teachers) accepted this proposition as an axiom (Barnett, Brown & Caton, 1983). See also Barnett (1990) for examples of prominent scientists who sometimes seem to espouse this view or something like it.

(3) I agree that my account of the history of German eugenics could well have been lengthened to match those of Britain and the USA (*cf.* Kelly, 1981).

Kaye

(1) Dr Kaye and I agree, I believe, on all matters of substance. I am sorry that his excellent book (Kaye, 1986) reached Canberra too late for me to refer to it.

(2) But I nowhere state that racism and sexism are prerogatives of "white, male capitalists"; B&F says little on the world distribution of these "abuses;" perhaps it should have said more. But see p. 287 on racist attitudes of Chinese and black Africans. (To suggest that sexism is confined to Whites would of course be ludicrous.) Most of my criticisms concern not capitalists but the works of scholars who are unlikely to be even rich. They are, I believe, male. About their color I have never enquired. I am not much impressed by the cry that sociobiologists are "often radically opposed to the existing social and moral order;" in B&F I repeatedly and explicitly point to the evidence of the unwelcome social *influence* of their works, which is *independent of the writers' intentions*. On pp. 137-9 I enlarge on their many inconsistencies. "The neo-Darwinians just quoted have evidently not fully worked out where their arguments lead . . . [and] faced with disconcerting implications, the writers recoil" (p. 138).

(3) Where does Dr Kaye get the idea that I regard the scholars I criticise as "political thugs and moral monsters?" On p. 139 I remark

on their undoubted altruism (in the primary sense of the term). The passage he quotes from p. 301 (about “the powerful, violent and self-ish”) comes from a summing up of the political obstacles to progress: it refers not to the writings of the learned but to the actions of rulers.

(4) Dr Kaye asks: “What biologist would disagree that ‘each developmental change is influenced by the interaction of genes and environment?’” Probably none. Nor, perhaps, would non-biological readers of B&F. But in chapter 7 I warn the reader against accepting this truism without considering its implications, and show why I find it necessary to repeat that principle, *and enlarge on it*, over and over again. The need is exemplified by the fact that Dr Kaye complains about that sentence.

Klopfers

Economic determinism and market politics are discussed on pp. 207-12 and elsewhere. The nearest to the statement about war, attributed to me, is a sentence on p. 296: “The foremost obstacle to enlarging our freedoms is war.” This is part of the discussion of obstacles to progress; it is followed by a passage on the economic and political effects of war and preparation for war. War and other kinds of violence are dealt with mainly in chapter 5. For political power, see pp. 73-6 and chapter 15.

Oyama

Differences from Dr Oyama are only of emphasis and terminology. But I do part from her on this: “it is hard to maintain the pleasant conviction that the ‘growth’ of abilities is necessarily a good thing.” The relevant passages in B&F are on each person’s need to develop skills and to perform useful and satisfying work. If Dr Oyama does not accept this principle, I wish she had used her “considerable skills” to say why!

Salthe

(1) Dr Salthe complains that I reject all behavioral homologies between human beings and animals; but he does not state what he would accept as a homolog. In several passages I not only contrast but also point to similarities between human and animal actions. See, for example, pp. 21-3 and 36 on infant behavior; pp. 37 and 251 on communication; p. 181 on conditional reflexes; pp. 190-194 on curiosity and exploration; pp. 271-3 on teaching. And on p. 117 I refer to the uses of comparing human and animal behavior. The definitive statements on homology, apparently overlooked by Dr Salthe, are on pp. 23-4 and in the glossary: in biology the term homology refers primar-

ily to *structures*. A reading of chapter 4, and of the account of exploratory behavior in chapter 10, will also show that I do not dismiss all animal behavior as species-typical and therefore invariable.

(2) Dr Salthe states that I do not question Neo-Darwinism. Modern evolutionary theory and its limitations are critically analyzed in some depth in chapter 6. If, as I might have done, I had omitted this rather knotty subject, Dr Salthe would have had valid grounds for his complaint.

(3) "Barnett argues that violence is learned rather than being a genetic predisposition. But he adds (p. 71) that this is true 'especially among boys.'" The sentence from which these three words are taken is part of an account of experiments on children: "seeing violence on television increases 'aggressive' conduct, again especially among boys." The experiments do not reveal how the difference between the sexes arises. (Incidentally, I would never use the expression genetic predisposition.)

(4) Dr Salthe writes: "To argue against a 'fixed human nature' is to argue against applying science to humans at all." I do not understand either the logic of this statement or its relevance to B&F. On p. 114 I write: "It may be objected that beneath and behind human variation and adaptability, there is still a fixed human nature. Such a statement is true in the sense that the species, *Homo sapiens*, is distinct from all other species: human beings have unique features . . ." Nearly every chapter of B&F gives examples of applying science to humanity. Nor do I say that the human species is "unselected"; I assume the opposite. And particular instances of effects of natural selection on human populations are on p. 86 (sickle-cell anemia) and on p. 89 (albinism). I do, however, object to groundless Neo-Darwinian interpretations of human action and to implications of fixity which defy the facts. So, evidently, does Dr Salthe.

THE BOOK

Here are some further criticisms and comments, many of which refer to the book, not to the reviews. B&F should have been subtitled, *The Political Implications of the Human Sciences*. It is an essay in transdisciplinary study. Each section deals briefly with a vast area of knowledge. The bibliography, of only about nine hundred entries, could well—as several reviewers imply—have been much longer.

Chapter 2, on the history of misanthropy and pessimism, should have given more space to its opposite—the belief in the possibility of human improvement. Chapter 3, on metaphor and analogy, does not deal adequately with the literary uses of imagery. I wrote these introductory chapters because both topics are fundamental for understand-

ing current ideas about “human nature;” and they are not given enough attention.

Chapters 4 and 5, on *Homo pugnax*, present both the errors committed under the heading of aggression and also achievements of ethology and of the social sciences. But they do not bring out the achievements strongly enough. I have also been criticized elsewhere for not stating clearly what I think should be the role of ethology in the study of the human species. The answer is that it can provide instructive descriptions and comparisons and, sometimes, testable hypotheses; but not conclusions.

Chapter 6, on evolution, is the only one (I think) which might be much shorter. Yet the peculiarities of the theory of natural selection, and of attitudes to it, are important. This was my worst dilemma.

Parts of Chapter 7, on nature and nurture, should (I sometimes feel) be printed in letters of fire. The whole field continues to contain pitfalls which regularly trap even cautious victims. I do not know how to prevent this.

Chapter 8 presents another kind of difficulty, already mentioned. I am accused of setting up straw men—an expression I use against myself on p. 139! But the passages I quote verbatim, and the works from which I take them, are not figments of an overheated pen. (For additional examples and critique, see Kitcher, 1985, regrettably not cited in B&F.) Granted, in sociobiological works, lack of logic, inconsistencies and banality are often more prominent than tone deafness to morals. Should I therefore have further softened my criticism of the outrageous gammon published by prominent persons, and added that they are really decent chaps and didn't mean what they seem to say? I admit I am more enthusiastic about exposing error than protecting the feelings of those who commit the errors. Perhaps this is wrong. Certainly, the few people I have met who advocate or teach some form of human sociobiology are charming, virtuous and willing to indulge in amicable argument. (They are also white and male.)

I wish I had said more about the extent to which current debates in this field seem compulsively to repeat those of the past (*cf.* Crook, 1987, 1989). And if I were writing B&F now, I should be able to refer to recent works in which sociobiology is allowed to retreat—as it should—into social ethology (*e.g.*, Betzig, Borgerhoff, Molder & Turke, 1988; Porter, 1987).

Chapters 10 and 11 are on the scope and social impact of behaviorism and the alternatives to it—a subject which deserves even more attention than *Homo pugnax* or *H. egoisticus*. I should have somehow put still more emphasis here, and especially on the importance of work and play.

The remaining chapters attempt “an incomplete sketch of humanity” (p. 227). Chapter 12, on reduction and determinism, arises

from the frequent unexpounded use of these terms in discussions of *H. pugnax* and their cousins. In writing it I was led into deep waters—even into the status of psychoanalytic ideas. Should I have ventured so far? I should like to have the comments, not so much of reviewers, as of the extraordinary readers of B&F. But they would probably all differ.

Language, the main topic of chapter 13, is central to any acceptable portrait of our species. We have no authentic biological account of human speech (or of music, about which I write a little, or of mathematics, about which I write nothing). The main shortfall in this chapter concerns the kinds of sentence we utter.

A large part of chapter 14 (on teaching and tradition) presents humanity as the teaching species, *Homo docens*: a favorite topic of mine partly because—despite the vast literature on education—nobody else seems to have written much about it. (I wish somebody would follow it up.) This part should probably have been shorter. I could then have said more than I did about the findings and ideas of workers such as Cavalli-Sforza and Boyd & Richerson and about the historical aspect of tradition.

The final chapter sums up the political implications of the argument. It is too condensed for rapid reading and ought to be at least two chapters. Expansion would, of course, not make consensus more likely. Many of the problems discussed are matters of life and death. They demand unremitting debate. This justifies the kindness of the editors of IJCP in arranging diverse reviews, and of the reviewers in writing about my book and allowing me to reply.

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