

## *Commentary*

### Rejoinder to Kenneth Schaffner

*Richard D. Alexander*

SCHAFFNER DESCRIBES my opening remarks about the significance of evolution for human understanding as "an almost imperialistic claim for evolutionary biology." But knowledge advances through a competition of ideas not disciplines. It is not an imperialistic claim of physics that humans are subject to gravity. To doubt my arguments is appropriate; to imply that their acceptance would constitute a supplication to the discipline from which I operate is not.

Schaffner is disturbed by the "absence of argument" in my paper about the relationships between (1) parent-offspring interactions and other forms of nepotism, and (2) altruism to relatives and reciprocal altruism between nonrelatives. Actually, my footnote 31 cites several references in which these relationships are discussed. Here I especially refer the reader to my paper on social exchange where I argue that equity and exchange theory in sociology and psychology have encountered difficulties with what investigators in these fields have termed "deep" and "intimate" interactions because they have failed to recognize that these are either nepotistic or (because of novel social situations) surrogate-nepotistic in nature.<sup>3</sup> Thus, like the parent-offspring interaction, they have in the past involved genetic returns that nullified the requirement of *phenotypic* equity in exchange. For most organisms, parenthood is wholly an expense to the parental phenotype;

so is nepotism in general, with the payoff in both cases genetic. In reciprocal or exchange relationships the payoff is phenotypic. Nevertheless, in evolutionary terms the altruism in all of these interactions is similar in that (1) it represents an investment with risk, and (2) in parenthood or nepotism it may involve both genetic returns and phenotypic reciprocity.

Schaffner probably means to suggest that if social reciprocity can be shown to have no historical or other connections to nepotism, then perhaps the human social interactions that appear to be cemented by reciprocity (as I have suggested for the unity of large nations) can be inferred to be independent of biological history. All that is required to deny this suggestion is some degree of intrication of nepotism and reciprocity in human affairs. Scarcely anything shows better than does the structure of law that such intrication is the rule in everyday life. Moreover, social reciprocity can arise on strictly selective grounds in the absence of nepotism;<sup>2</sup> after all, one of the oldest social interactions ( $\sigma$ - $\varphi$ ) begins as reciprocity. Schaffner's objection parallels the incomplete view of social reciprocity expressed by Sahlins,<sup>3</sup> in which he asserts that nonrelatives cannot gain biologically from social interactions; I have explained Sahlins' error elsewhere.<sup>4</sup>

Schaffner attempts to weaken my argument about the development of nation-states by describing it as "heavily indebted" to a single account—that of Flannery.<sup>5</sup> But Flannery's paper is a review article which the reader can use to direct himself to the anthropological literature on this subject. Flannery does not suggest that his reconstruction is unique; instead it is solidly backed by the work of numerous anthropologists and archaeologists.

Contrary to Schaffner's implication, the concept of "reproductive effort" is firmly established in biology and is essential to the explanation of the finiteness and the patterning of individual lifetimes.<sup>6</sup> For a review of the concept and its theoretical basis I suggest Hirshfield and Tinkle;<sup>7</sup> for a discussion of the kinds of effort shown by organisms, their variations between male and female and between species with different kinds of breeding systems, see Alexander and Borgia.<sup>8</sup>

Schaffner says that my use of the idea of "balances of power" is vague. But I see this notion as pervading every imaginable

aspect of human existence, with its role understandable by ordinary humans in everyday circumstances that involve individuals as well as groups of all sizes, compositions, and purposes. I cannot imagine anyone who understands the evening news or who functions satisfactorily in ordinary social interactions viewing the concept of balances of power as vague.

In connection with the problem of levels in the organization of life at which natural selection is most effective, Schaffner seems to have interpreted the rule of parsimony to mean that we must always invoke selection at the lowest *possible* level. This is a misinterpretation: On parsimony we invoke selection at the lowest *acceptable* level, that is, the lowest level at which, on the basis of available information, it seems to explain things. The reason for using the *lowest* (in this sense) level is simply that fewer assumptions are involved.

Schaffner castigates me for preferring singular explanations.<sup>9</sup> But all valid principles and generalizations amount to "singular explanations." Accordingly, I seek them unabashedly, and when they work I prefer them.

Schaffner accepts Wilson's argument against "advocacy" of one among several possible explanations. But Wilson's argument is inconsistent because it is itself an advocacy of a particular hypothesis about procedures. More than this, it is advocacy of a silly and wasteful procedure. He indicates that merely because one "explanation" is more probably correct (one can only presume that this means "superior on the available evidence") is no excuse for "advocating" (using?) it, and that we must instead, each time, review all possible explanations and eliminate them. But hypotheses cannot really be eliminated and conclusions are never absolute. All one can do with hypotheses or conclusions is to continue to the next step in analysis.

The problems of "levels" and "units" of selection is an extraordinarily complex one with a growing literature. I have been involved and interested in it since 1968.<sup>10</sup> But I have never "accepted wholeheartedly," as Schaffner says I have, that selection only occurs at the gene level or denied that it ever occurs at the level of groups of individuals.<sup>11</sup> Instead, I am the first, last, and most explicit of the modern authors to argue (1968-78) that humans are an excellent model for group selection. They alone have the traits of conscious foresight and ability to cooperate in

planning that Wynne-Edwards<sup>12</sup> seemed to impute to other animals in arguing for adaptive population regulation; and, because of their ability to cooperate and compete in groups, humans seem to have been their own principal selective force for a very long time.<sup>13</sup> I cite Lewontin in regard to levels of selection not because I am implying that he argues for the gene as the unit of selection, as Schaffner indicates, but because he states, in the paper I cite, that: "The primary focus of evolution by natural selection is the individual." Franklin and Lewontin's conclusion<sup>14</sup> that genes are inherited in various kinds of groups is well understood in biology, not disputable, and in no way contrary to any of my arguments. Lewontin's arguments<sup>15</sup> included the first discussion of precisely what determines the levels at which selection is effective in different circumstances, and they showed that significant variations in this regard are a certainty; thus, his conclusions are not accurately reflected by Schaffner's comments.

Williams's conclusion against group selection is not "a methodological assumption" but an argument now almost universally accepted in biology because of both logic and empirical evidence. Lewontin said it correctly—that population regulation, on which the argument for group selection was based, simply does not occur (at least outside humans). Other topics, like sex ratio selection, can be used to show that empirical evidence is sufficient to refute group selection in a very large realm.<sup>16</sup>

I end with a critical remark against myself. Since submission of my manuscript, I have continued to delve into the literature of law in relation to anthropology, sociology, and philosophy, and I am increasingly astounded and dismayed at its immensity and complexity. Anyone deeply immersed in these topics has to view my paper as naive and with numerous failures of reference. If the paper turns out to have lasting merit it can only be in regard to the approach, and the generalizations from that approach. I am thankful that I have found nothing yet which seems to me to obviate this possible virtue.

## NOTES

1. See my note 11, pp. 283-84.
2. See my note 31, p. 286.

3. M. Sahlins, *The Use and Abuse of Biology* (Ann Arbor, Mich.: University of Michigan Press, 1976).
4. *American Anthropologist*, 79 (1977): 917-20.
5. See my note 14, p. 284.
6. See Fisher and Williams, cited in my notes 2 and 5, pp. 280, 281, and Alexander, in *Goulden*, cited in note 4, p. 281.
7. M. Hirshfield and D. W. Tinkle, "Natural Selection and the Evolution of Reproductive Effort." *Proceedings of the National Academy of Science*, 72 (1975): 2227-231.
8. R. D. Alexander and G. Borgia, "On the Origin and Basis of the Male-Female Phenomenon," in M. F. Blum, ed., *Sexual Selection in Insects*. (New York: Academic Press, 1978).
9. See also my note 9, p. 283.
10. R. D. Alexander and D. W. Tinkle, "A Comparative Review [of *On Aggression* by Konrad Lorenz and *The Territorial Imperative* by Robert Ardrey]," *BioScience*, 19 (1968): 245-48; R. D. Alexander and G. Borgia, "Group Selection and the Hierarchical Organization of Life," *Annual Review of Ecology and Systematics*, 9, 1978; also, all of my cited papers.
11. For example, see my note 5, p. 281.
12. V. C. Wynne-Edwards, "Self-regulating Systems in Populations of Animals," *Science*, 147 (1965): 1543-548.
13. See my note 11, pp. 283-84.
14. See Schaffner's note 4, p. 302.
15. See my note 3, pp. 280-81.
16. See my note 2, p. 280.



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# **Morals Science and Sociality**

**Edited by**  
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