

THE USE OF MORALISTIC STATEMENTS IN SOCIAL MANIPULATION: A REPLY TO ROY A. RAPPAPORT

by *Lee Cronk*

Abstract. Rappaport's comment includes several errors. First, he conflates manipulation and deceit. Second, he confuses the rationalism of the evolutionary biological analysis of organisms with the rationalism (or lack thereof) of the motivational and cognitive structures of the organisms under study. Third, his moralistic judgment of my focus on manipulation implies that scientists should not only not explore but should also suppress such unsettling ideas. We will make little progress in understanding morality and in fostering truly moral behavior if we refuse to acknowledge that moralistic statements may sometimes, and perhaps even often, be used in a manipulative way.

Keywords: communication; morality; signals; social manipulation.

It is gratifying when one's work inspires a leading thinker in one's field to make a new contribution to the literature, and I am pleased that my article has prompted Roy Rappaport to write his comment. Despite the critical tone of his remarks, Rappaport's approach and mine actually have much in common. For example, more than half of his comment is on the topic of religious rituals, and I find little room for disagreement with him on that topic. As I tried to make clear in my original article (Cronk 1994), my concern was neither with the origin of religion nor with how religions are used to enhance the solidarity of relatively egalitarian communities but with how they are used by elites and by colonial powers as a means of social control. Religion provides a forum and a vocabulary for moralistic discourse that lend themselves to this sort of use, but this observation is by no means the only interesting or insightful thing one can say about

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religion, and I did not mean to imply that it was. As Rappaport himself points out in his comment, he has made essentially the same observation in previous publications (for example, Rappaport 1979; 1984, 237). Rappaport's observations on the frequency and length of ritual performances are also interesting. Like many other phenomena, the length and frequency of rituals may be influenced by a variety of factors. My article pointed out only one such factor, the role of rituals in social manipulation and control, because it has heretofore been largely neglected, but, again, this does not imply that it is the only relevant factor.

Rappaport and I also share an interest in the application of evolutionary biological theory to human social behavior, which Rappaport and some others would label "sociobiology." In fact, the word *sociobiology* is used only once in the entire text of my article, and then only in a quote from Donald Campbell. I and many other people doing research on the evolutionary biology of human behavior have chosen other labels for our work, mainly *behavioral ecology* and *evolutionary ecology*, in part because of the pejorative way that many people, evidently including Rappaport, now use the word *sociobiology*. It is ironic that Rappaport would choose this apparently derogatory label for my work since, in a technical sense, Rappaport himself was a pioneer in human sociobiology. Wilson (1975, 595) defined sociobiology as the "systematic study of the biological basis of all social behavior." Rappaport's work on the ritual cycle of the Tsembaga Maring was just that: an application of evolutionary biological theory to human social behavior. Neither the fact that Rappaport was using group selectionist evolutionary biological models (for example, Wynne-Edwards 1962) that have since been largely discredited (see Williams 1966; Trivers 1985, chap. 4) nor the fact that the word *sociobiology* was not popularized until several years after the first publication of Rappaport's findings changes this.

As for areas of disagreement between Rappaport and myself, part of the problem is merely terminological. In my original article I tried to make clear that the term *manipulation* can include both honest and dishonest signals and cooperative and noncooperative ones. For example, I wrote, "For successful manipulation, honesty may sometimes be the best policy, even when there are conflicts of interest between signalers and receivers" (Cronk 1994, 88). Despite such statements, Rappaport repeatedly conflates manipulation and deceit. Once it is made clear that deception is just one route to successful manipulation, the basis of many of Rappaport's objections to my article disappears.

The clearest contrast between Rappaport's approach and mine is

in the area of old-age security and moralistic statements about the obligations of children to care for their elderly parents. Rappaport suggests that "a deep emotional connection" between parents and children may be "a concomitant of effective parental care of children" and that this emotional commitment may explain the willingness of children to care for their elderly parents. Several problems beset this idea. First, Rappaport's idea does nothing to help explain why the idea that children have an obligation to care for their elderly parents is so often expressed as a moral imperative. If an emotional commitment alone explained such behavior, then such statements would be unnecessary. Second, Rappaport attempts to contrast his emotional approach to this question with a caricature of modern evolutionary biology in which a "central place" is given to "rationality itself," implying that behaviors are subject to evolutionary biological analysis only to the extent to which they are rationally rather than emotionally motivated. The falsity of this position is shown in that his own suggestion can be phrased in cost-benefit terms. According to Rappaport, an emotional commitment to care for one's elderly parents is simply an extra cost of effective parental care in our species. This suggestion is every bit as subject to rationalistic, cost-benefit analysis as anything evolutionary biologists have come up with. What Rappaport's discussion obfuscates is that the analysis of the natural selection of any trait, even an emotional one, must necessarily be put in cost-benefit terms, as shown by a recent spate of research on the evolutionary biology of emotions (for example, Frank 1988; Tooby and Cosmides 1991; Nesse 1990). Rappaport confuses the rationalism of the evolutionary biological analysis of organisms with the rationalism (or lack thereof) of the motivational and cognitive structures of the organisms under study. Third, Rappaport's observation that old folks often "pay their own ways" simply repeats a point I made in my original article (Cronk 1994, 93; see also Turke 1988; Hawkes, O'Connell, and Blurton Jones 1989).

Rappaport repeatedly belittles the evolutionary biological study of behavior for having a "simple-minded and ugly view of human nature" and for having as a "fundamental limitation" an inability "to accommodate" the idea that "society is distinct from, but not separated from, the individuals making it up." On both counts I think Rappaport reveals that he is less than fully informed about current thought on the evolutionary biology of human behavior, culture, and society. Our conception of human nature is quite broad and, in particular, it is fully capable of including the non-rational, emotional side of human nature that Rappaport is keen to

emphasize. The evolutionary biological analysis of human social arrangements is also growing more and more sophisticated while it remains rooted in the hypothetico-deductive method and eschews the sort of imaginative storytelling that is becoming so popular in cultural anthropology.

The thing I find most disturbing in Rappaport's comment is his moralistic judgment of the emphasis in my article on the manipulative use of moralistic statements, which he finds "socially dangerous and morally repugnant." By indulging in this sort of criticism, Rappaport takes his critique of my piece out of the rational discourse of science and into the moralistic discourse of the humanities, where arguments are over interpretations and the personal qualities of the participants in the debate rather than over whether the real world fits various competing hypotheses (see Alexander 1988 for more on these different modes of discourse). The implication of Rappaport's position is that scientists not only should not explore but should also suppress such potentially unsettling ideas, and as a scientist I find *this* idea to be both dangerous and repugnant.

Finally, Rappaport makes a valid point when he writes that the manipulative view of signaling may create the erroneous impression that anyone who lives morally is a sucker, namely, that one has to be either stupid or naive to behave morally. To see why this is not an accurate interpretation of this view of signaling, we need to make a clear distinction between moral behavior and moralistic statements. As Nietzsche pointed out, to make a moralistic statement is not necessarily a moral thing to do, but it does not necessarily follow that to behave morally is a stupid thing to do. Indeed, much of the current work on the evolutionary biological bases of morality that I discussed in my article deals specifically with the question of how it can be individually rational to follow moral codes even when those codes may require emotional commitments and may call for individual self-sacrifice (for example, Alexander 1987; Frank 1988). Our progress in understanding moral behavior and moral codes and how to encourage true morality will only be hampered if we refuse to acknowledge that moralistic statements may sometimes, and perhaps even often, be used in a manipulative way.

NOTE

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