Reply to Paul J. Magnarella

Marvin Harris

American Anthropologist, New Series, Vol. 84, No. 1 (Mar., 1982), 142-145.

Stable URL:
http://links.jstor.org/sici?sici=0002-7294%28198203%292%3A84%3A1%3C142%3ARTPJM%3E2.0.CO%3B2-D

American Anthropologist is currently published by American Anthropological Association.

Notes

Acknowledgments. I wish to thank Leslie Sue Lieberman for reading and commenting on an earlier version of this paper. She is in no way responsible for the ideas expressed.

[1] This criticism parallels the one made by Harris of dialectical materialism ("The Hegelian Monkey"). Harris (1979:145) writes:

> The central weakness of dialectical epistemology is the lack of operational instructions for identifying causally decisive "negations." If every event has a negation, then every component in that event also has a negation . . . Which negation is the crucial "contradiction"? . . . Since there are no instructions for identifying the properties or components that are the crucial negations, dialectical relationships can never be falsified.

References Cited

Douglas, Mary
1966 Purity and Danger: An Analysis of the Concepts of Pollution and Taboo. New York: Praeger.
Freund, John E.
1967 Modern Elementary Statistics. 3rd ed. Englewood Cliffs: Prentice-Hall.
Friedman, Jonathan
1974 Marxism, Structuralism, and Vulgar Materialism. Man: 9(3):444-469.
Harris, Marvin
1968 The Rise of Anthropological Theory. New York: Thomas Y. Crowell.
1974 Cows, Pigs, Wars and Witches. New York: Vintage.
1979 Cultural Materialism. New York: Random House.

Reply to Paul J. Magnarella

MARVIN HARRIS
Department of Anthropology
University of Florida
Gainesville, Florida 32611

Magnarella has raised several important points about cultural materialism (henceforth C.M.). I am grateful for his insightful comments and for this opportunity to identify and clarify sources of misunderstanding. In order of appearance (but not necessarily of importance) these points are:

> [1.] Cases where cultural materialist do resort to the emic superstructures for causal explanations would tell us a great deal more about their theories and stated logic than we now know.

There are two kinds of independent emic determinations. First, there are innovations in religion, ritual, and art, which may neither add nor subtract in any differential cost/benefit sense to or from the fulfillment of the biopsychological imperatives that drive cultural selection processes. It is unlikely, for example, that C.M. can account for the British rule that traffic must keep to the left instead of the right or for the rule that boy babies get blue blankets and girl babies get pink blankets, by referring to differential etic cost/benefits. The trouble is, however, that one cannot know a priori which superstructural traits are genuinely neutral. Hence, the irritating habit (to some) of talking about the possibility of superstructural variables that are independent of structural and infrastructural conditions without any accompanying display of instances which C.M. admittedly cannot explain. In any event, one learns more about C.M., not by the superstructural puzzles it cannot explain, but by those which it can explain better than rival strategies (e.g., Vaidyanathan, Nair, and Harris, in press; Ross 1980).

The second type of independent superstructural determination constitutes a more serious challenge to C.M. This involves major upheavals in political economy brought about by the influence of charismatic ideologues and revitalization prophets. Some current examples are the Iranian Islamic and Chinese cultural revolutions. C.M. admits that such movements can exert an autonomous effect on microevolutionary trends, but C.M. predicts that where the infrastructural conditions are functionally inappropriate, or especially if they are antagonistic, to the "top-down" changes, the superstructural innovations will be short-lived.

I have deliberately excluded from discussion cases such as the sacred cow in India, where the force of superstructural determinations lies in their functional resonance with the infrastructural conditions (or seem to lie there—an empirical issue still being studied). As has been stressed in the "cow controversy," ideology plays a crucial functional role in maintaining, optimizing, and changing various forms of sociocultural systems and subsystems (Harris 1981a).

> [2.] . . . the type of causality being called upon here is of a nonfrequency nature. . . . Lacking an explicit basis for quantification, such posited probabilities are not open to rigorous scientific testing and cannot be falsified.

On the contrary. The only measures of causality admitted by C.M. are the observed frequencies, or correlation coefficients linking the components of sociocultural systems in predicted or retrodicted ways in samples (random if possible) of such systems. This measure is eminently operationalized and thoroughly falsifiable (although the samples may be small and the statistics may get hairy) (e.g., Divale and Harris 1976). Perhaps I should also point out that in dealing with a single sociocultural system through time, it is possible to identify the *direction* of causality by the order of appearance of innovations; but it is not possible to derive a probability measure for the causal effects in question.

The probability estimate proposed by Magnarella ($\cdot 8 \times \cdot 8 \times \cdot 8 = \cdot 51$) is wholly his invention and has nothing whatsoever to do with measuring the predictive powers of the principle of infrastructural determinism. There is no way that probability measures can be assigned to cultural phenomena in the abstract. Only when specific theories are tested can such probabilities be stated.

The source of this misunderstanding is the conflation of the untestable theoretical *principles* of C.M. with the testable *theories* derived from those principles. The principle of infrastructural determinism does not include a probability measure; it merely serves as a guide for the construction of plausible and testable theories which will always be probabilistic (either because the *ceteris paribus* clause cannot be fulfilled, or because we lack sufficient knowledge, or because our objects of study have something called "free will"). I shall refer to the *principle* of natural selection to clarify this matter. This principle states that biological selection takes place as a result of differential reproductive success; it does not state for any particular lineage of organism or for organisms in general what the probability of a particular change will be; nor does it state what factors account for differential reproductive success in any particular case. Hence it is essentially untestable. Yet it merits our confidence (in lieu of anything better) as a guide to the construction of theories because of the extensive interconnected corpus of tested theories developed under its auspices. Ditto for the principle of infrastructural determinism. It is productive, but we cannot say a priori what its success rate will be.

> [3.] . . . Mary Douglas's (1966) structural theory is more nomothetic than Harris's because it applies to all the prohibited animals of the book of Leviticus.

First, it is not true that I "deal with only one" of the prohibited animals. In fact, in an article not cited by Magnarella, I dealt with all of them and offered cost/benefit explanations for all of the prohibitions on domesticated species and for some of those on the feral species as well (Harris 1973). If I concentrated on the pig, it was because the cost/benefits associated with this particular *domesticated* species (unlike the domesticated cat, dog, donkey, horse, camel, and cattle) seemed most problematic, not because C.M. has nothing to say about the other interdicted species.

Apparently Magnarella is not familiar with my contention that taboos on domesticated species and on potentially *useful* feral species can be subsumed under a single theory of sacred interdictions which applies to incest as well: "Total interdiction of [a potentially useful or satisfying act] by appeal to sacred sanctions is a predictable outcome in situations where the immediate temptations are great, but the ultimate costs are high, and where the calculation of cost/benefits by individuals may lead to ambiguous conclusions." (Harris 1979:193; cf. Ross 1978). Now this theory may be unsatisfactory, but it is certainly nomothetic.

As for Mary Douglas's theory, there is nothing nomothetic about it because it does not state the general conditions under which a species will be regarded as a "taxonomic anomaly" or when a culture will capriciously decide it doesn't like to eat "raptors." In order for one to have a nomothetic theory, one must be able to specify the general conditions under which specific phenomena will recurrently appear. Douglas's structuralist explanation is so far from being nomothetic that were the same group of pre-Leviticus Israelites plunked down once again in precisely the same set of cultural and natural conditions, there is not the slightest reason to expect from structuralist principles that they would develop the same set of taxonomic distinctions (e.g., between animals that chew and don't chew the cud, etc.).

> [4.] Harris's treatment of the [unique] Aztec case appears to contradict his own statement that "at the heart of the cultural materialist theoretical corpus is a set of theories dealing with . . . a *type* of institution under a set of recurrent conditions."

I think that I can best respond to this point by quoting from the *same page of the same book* (Harris 1979:78ff.):

> I do not wish to propose that nomothetic strategies can deal only with events that occur more than once. The origins of Christianity and Buddhism, for example, are unique localized events associated with the personal lives of two discrete individuals. Yet it is possible to give nomothetic explanations of the origins of Christianity and Buddhism which contrast strongly with common idiographic explanations. The difference is this: in the idiographic explanations, the personalities of Jesus and Gautama impose themselves as unique forces twisting events along unpredictable pathways; in the nomothetic approach, the forces characteristic of the imperial periods in which Jesus and Gautama lived create their personalities. The events unfold along predictable pathways, and the particular individuals involved respond in ways typical of messianic reformers during periods of corruption, exploitation, and widespread misery.

Furthermore, in the Aztec case, it should be remembered that the question to be answered is not only why the Aztec state sanctioned the sacrifice and eating of prisoners of war, but why a taboo against cannibalism was adopted by all other known imperial systems. It is the generality of the taboo among imperial states that carries the logical burden of the claim to nomothetic status. The Aztec do not satisfy the theoretical conditions for the appearance of the taboo. (i.e., they lack domesticated sources of animal protein). Therefore, the explanation for this single negative case is as nomothetic as the explanation for the two dozen or more positive cases. If I drew Sahlins's attention to the "uniqueness" of the Aztec case, it was only for the purpose of showing that explaining the existence of cannibalism in general could not explain the occurrence of Aztec cannibalism in particular. It is cannibalism among imperial states and not cannibalism in general that is at issue. Cannibalism in general poses no problem; it is the interdiction of cannibalism that poses the problem, just as it is not copulation between brother and sister that poses an intellectual challenge, but rather the interdiction of it.

> [5.] This [low density of band-organized populations] is not a prediction but a plausible explanation of what is already known . . . any nomothetic prediction is impossible if there can be no expectation of a sufficient number of new cases against which to test the prediction.

I do not share Magnarella's lack of enthusiasm for the structure and superstructure correlates of hunter-gatherer modes of production. Several kind of confusion seem to be involved here. First there is the problem of the distinction between a theory and a corpus of theory. I included the theory about low population densities among (certain kinds of) hunter-gatherers as part of the corpus of C.M. theories, not to claim credit for the banal and established, but to show how nomothetic processes leading to low population densities also probabilistically determine such additional and scarcely trivial items as the labile structure of local groups, the bilateral bias in kinship, the use of lactation to control fertility, the absence of political coercion, sexual parity, low incidence of warfare, band exogamy, reciprocity as the dominant form of exchange, and even the restraint on boasting as a brake on intensification. Nor does the nexus stop there, because there are in addition predicted microevolutionary and macroevolutionary consequences of the relative abundance and types of flora and fauna exploited, the effects of depletion, the transition to domesticated plants and animals, and so forth up to the evolution of the state and beyond.

While some of these retrodictions have been tested more than others, all are controversial and each would benefit from additional ethnographic fieldwork, more archeology, more ethnohistory and history, and more research in existing ethnographic collections including last but not least, HRAF. Magnarella's statement that "the distribution of various 'traits' such as band organization, are already known" seems to be derived from a prescience that bears no discernable relationship to the noisy lack of consensus everywhere evident in the anthropological literature on even the most elementary "facts" about bands, villages, chiefdoms and states, let alone their causal explanations (e.g., Service 1971; Lee 1979).

Surely Magnarella does not mean to say that because the information necessary to the testing of a theory is already present in HRAF, that the theory must be trivial. That this is a non sequitur need not be belabored. The logical status of retrodictive tests of theories is precisely the same as that of predictive tests. For example, consider the theory that prolonged lactation among hunter-gatherers is correlated with a low incidence of warfare and high protein diets. Does it make any difference to the logical-empirical status of the testing procedure that the test must rely mainly on data collected in the past and which may be available in HRAF? I cannot see how.

> [6.] Predictions of previously unknown distributions when a sufficient number of test cases exist or will exist are, of course, possible and should be the focus of those wishing to demonstrate the nomothetic and predictive powers of C.M.

Although predictions, including predictions about industrial society—for example, the prediction that the declining U.S. fertility rate is not a temporary aberration—are of central concern to C.M. (Harris 1981b:89), I cannot agree with the implication that predictions, as opposed to retrodictions, are more useful for testing theories. Many of the most important theories under consideration by anthropologists today refer to archeologically known or knowable data. Although some of these theories may soon seem rather self-evident, I hope that Magnarella does not regard them as unworthy demonstrations of the nomothetic and predictive powers of C.M. For example, a generation ago, most anthropologists thought that the Classic Maya population was supported by slash-and-burn agriculture. C.M. has long recognized a theoretical incompatibility in claims that Maya agriculture was dispersed and extensive while Maya sites were city-states or parts of states. The growing evidence for the existence of intensive forms of agriculture, including raised fields, canals, and tree crops (Adams, Brown, and Culbert 1981), validate C.M.'s position regarding the infrastructural basis for the rise of states. The Maya are one of the best-studied groups in the anthropological literature, yet important new discoveries are being made about them every year. Hence it is incorrect to suppose that we already know the "distributions" of traits in the past—even familiar traits. We shall learn about the past, just as we shall learn about the future: by constructing plausible theories under the auspices of coherent research strategies; by relating separate theories to an organized corpus of theories; by testing individual theories through predictions and retrodictions; and by modifying the corpus of theories with new theories that parsimoniously explain more than their rivals explain.

References Cited

Adams, R. E. W., W. E. Brown, Jr., and T. Patrick Culbert
1981 Radar Mapping, Archaeology, and Ancient Maya Land Use. Science 213: 1457-1463.
Divale, William, and M. Harris
1976 Population, Warfare, and the Male Supremacist Complex. American Anthropologist 78:521-538.
Harris, Marvin
1973 Riddle of the Pig, II. Natural History, February: 20-25.
1979 Cultural Materialism: The Struggle for a Science of Culture. New York: Random House.
1981a Reply to Freed and Freed. Current Anthropology 22:492-494.
1981b America Now: The Anthropology of a Changing Culture. New York: Simon & Schuster.
Lee, Richard
1979 The !Kung San: Men, Women and Work in a Foraging Society. New York: Cambridge University Press.
Ross, Eric
1978 Food Taboos, Diet and Hunting Strategy: The Adaptation to Animals in Amazon Cultural Ecology. Current Anthropology 19:1-36.
1980 Patterns of Diet and Forces of Production: An Economic and Ecological History of the Ascendency of Beef in the United States Diet. *In* Beyond the Myths of Culture. Eric Ross, ed. pp. 181-225. New York: Academic Press.
Service, Elman
1971 Primitive Social Organization: An Evolutionary Perspective. New York: Random House.
Vaidyanathan, A., N. Nair, and M. Harris
In press Bovine Sex and Species Ratios. Current Anthropology.