

**Purple People Eaters?: A Comment on Aztec Elite Class Cannibalism a la Harris-Harner**



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pectations derived from the qualitative description of the relationship between labor and capital" (pp. 162-163). No one can ever *prove* causation, and science does not rest on *proof*. One can, however, look for data that either can disprove causation, or, alternatively, can support an inference of causation. Such inferences vary in their reliability, and there is always some possibility that even our most strongly supported inferences are false. I offer the Spearman rank correlation and some case material because they support the inference that households with greater labor resources tend to accumulate greater wealth.

On the subject of wealth and household size, Magnarella offers his own view (in apparent contrast to my view): "wealth and household size can be better understood as part of a more comprehensive system of interacting factors" (p. 161). This implies that I overlook other factors influencing household wealth. In fact, I explicitly discuss drought, disease, severe cold, bridewealth, occasional high family expenses, and differences in the efficiency of individuals as economic producers and managers of household resources as other factors affecting household wealth.

Magnarella states that my occasional generalization of statements to "all Yomut, even all Turkmen" (p. 161) are unjustified, but fails to point out that I did a survey of the entire Yomut area of Iran (pp. 13, 18-19), traveled in the Göklen Turkmen area (p. 13), drew on a large number of written sources on the Central Asian Turkmen that provide information on groups I have not visited (*passim*, especially Chapter I), stated explicitly that "the word Turkmen is restricted in meaning to the Central Asian Turkmen" throughout the monograph (p. 5, fn 2), and specifically mentioned Turkmen groups in Anatolia, the Caucasus, and the Fertile Crescent as groups to which the word Turkmen did not apply anywhere in the monograph (p. 5, fn 2).

The bulk of Magnarella's review consists of cataloging what he sees as flaws in the monograph. He gives readers of his review almost no idea of what the monograph actually has to say. Some readers of the *American Anthropologist* might be interested in knowing something more about the contents of the monograph before deciding whether to read it. I suggest that they read the reviews by Philip Salzman (1977) and Michael Meeker (1977).

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### Purple People Eaters?: A Comment on Aztec Elite Class Cannibalism à la Harris-Harner

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In his latest work *Cultural Materialism* (1979), Harris has made a significant step toward setting out a workable "strategy" for anthropological study. I yield to none in my admiration for his efforts to pursue the nomethetic cause and smite the mentalist Philistines and other obscurantists, but to save the dignity of the effort it seems a little caution may be necessary. Sometimes the speculative flights of fancy bear an uneasy resemblance to the armchair period of archaeological reconstruction and such thinkers as Grafton Elliot Smith when wishing could often make it so. That "structure" and "superstructure" must be explained in terms of "infrastructure" I cannot dispute, but there is an unseemly eagerness to get on to the conclusions without crossing the intervening evidential ground.

My present grouse is Harris's espousal of the view put forth by Michael Harner (1977) purporting to explain "The Ecological Basis for Aztec Sacrifice." Basically it argues that the Aztec were not, as is usually reported, engaging only in ritual cannibalism but that in fact human flesh was an important item in the subsistence of the elite classes. Following in the Harris tradition this is expressed in my title

noting that it was only the wearers of the purple (nobles) who were allowed to engage in significant degrees of people eating. My objection to all of this lies in such sweeping final statements as Harner's "What is essential is to demonstrate that the sacrificial captives typically were eaten; and this has already been done" (1977:129) and Harris's "Anyone with a knowledge of how the Tupinamba, the Huron and other village societies disposed of their sacrificial victims should be able to come to the same conclusion: the victims were eaten" (1977:108). For a man who is much concerned with establishing anthropology as a scientific discipline the statement by Harris seems astonishingly to be based on the old quasi-Aristotelean principle: "If one dog has spots then all dogs have spots." If anybody eats sacrificed persons then everybody eats sacrificed persons?

Harner's is the "substantial" article which Harris merely endorses and so it is Harner's "proof" that is critical, but both Harris and Harner rear a mighty edifice of "explanation" on the supposedly important practice of people eating. Harner appears to believe that the practitioners of Meso-American ethnohistory have been engaged in some sort of Nixonian cover up saying that "The evidence of Aztec cannibalism has largely been ignored and consciously or unconsciously covered up" (1977:119). The causality of this is partly to be found in "European ethnocentrism regarding cannibalism" and with the Mexicans being "embarrassed" for "nationalistic reasons" and the rest of us because "of the desire to portray native peoples in the best possible light in order combat ethnocentrism" (Harner 1977:126-127). All of this purportedly demonstrates why everyone but Harner has come to different conclusions from the same data base. Harris says "He alone deserves the credit for solving the riddle of Aztec sacrifice" (1977:108). Indeed?

The fact is that Harner's proof is based on apparently credulous analysis of a few selected "eyewitness" documents most notably of course the letters of Cortes, the true relation of Bernal Diaz, and the writings of Sahagun and Duran. (Cortes 1962; Diaz del Castillo 1963; Sahagun 1951; Duran 1971). Rather than argue the case at a length inappropriate for a brief comment, I will simply point to the consistent theme that runs throughout the four volumes devoted to ethnohistorical sources in the *Handbook of Middle American Indians*—the need for caution and careful analysis in the use of these sources. J. Benedict Warren comments that "In

using Cortes' letters one must remember that they are the work of a man who is telling his own story to make a favorable impression on the Spanish Crown" (1973:66). Nicholson in his "overview" of major problems in Middle American ethnohistory repeatedly points out such pitfalls as in his comment regarding native histories: "There has been too much naive acceptance of the literal historicity of the earliest segments of these stylized histories" (1975:491). Bernard R. Ortiz de Montellano (1978) has also made this point and provided a general critical appraisal of other aspects of Harner's data.

I do not mean to suggest that there is no truth in the accounts that Harner cites, but I do suggest that he use a few grains of salt on his tales of people eating. I cannot say that it is proven that no such thing occurred but I do insist that he has not proven that it did. Certainly his estimates of the numbers of sacrifice is purely speculative, and it is the issue of quantity that is critical to his developed argument that the search for flesh in wholesale lots was central to the nature of the "cannibal empire." The archaeological evidence is not yet there for such numbers and the sources he cites are speculations like his own, often at variance with one another. All of this is not to say that such a hypothesis need not be explored but simply to object to its being presented as proved *a priori*.

With the proof in hand both Harner and Harris rack up considerable explanatory mileage. Given their fundamental assumptions, some of their conclusions seem relatively justified. In particular the contrast between the Meso-American states and others in terms of the implications of the lack of domesticated herbivores seems a fruitful line of inquiry. I suspect them, however, of some ethnocentrism of their own in their insistence on the desperation of the Aztec who were "reduced to eating the algae skimmed from the surface of Lake Texcoco" (Harris 1977:110). Harner lists a wide range of foods including iguanas and snakes as well as the algae and says, "The nobility and the merchant class, in contrast, normally had a rich diet . . ." (1977:127). Charles Gibson in his monumentally thorough work *The Aztecs Under Spanish Rule* notes that a variety of "aquatic foods" including the larvae and scum so repulsive to Harner and Harris "continued to be elements of native diet through the colonial period and into modern times" (Gibson 1964:341). I think that rather too much is made of the "desperation" that leads peoples to eat

foods not served at the Columbia University cafeteria.

I think that a height (literally) of explanatory excess is reached when Harner says, "Even such little touches as the steepness of the pyramid's steps becomes understandable if one keeps in mind the need for efficiency in rolling the bodies down from the sacrificial altars to the multitudes below" (Harner 1977:132). One rolled down the Pyramid of the Jaguar (Temple 1) at Tikal would probably impact with enough force to do away with anyone in the "multitude" who got in its path. If we use the "how steep was my pyramid" approach, we are left with the conclusion that the Maya were the truly voracious people eaters since it was they who built most appropriately for chuting the bodies along. While we have evidence of sacrifice and probable cannibalism (ritual or otherwise) from Teotihuacan perhaps we can safely conclude that it was not significant since those pyramids were not well designed for chuting?

While I do not want to return to the days of Boas, he did have a point in his warnings against premature generalization. It does the cause of scientific cultural materialism little good if its practitioners make statements that are difficult to separate from those of Erich Von Däniken.

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## Reply to Greenwood's Comment

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In a recent comment (*AA* 81:351, 1979) on my review of his *Unrewarding Wealth* (*AA* 80:442, 1978), Greenwood states:

... explanations of complex phenomena such as rural exodus require attention to a variety of kinds of interacting causes (in this case, profits; stresses in local social structure; regional, national, and international political economy; and local values).

I agree. Unfortunately, that is not the kind of explanation given in the book. Rereading *Unrewarding Wealth* (with special attention to chapter 4) leads me to conclude, once again, that primary emphasis is given to Basque values in explaining rural exodus (see especially pp. 144-160), and to reassert that the explanation falls short because power and class interests are not adequately treated.

Profit levels, he says, cannot explain farm abandonment: farmers are making good incomes. Stresses in local social structure emphasized by Greenwood have to do largely with inheritance and marriage: young people do not want to stay on the farms, because farming does not fit their view of a dignified life (see pp. 155-159). Tourist development, land speculation, highway expansion, and their effects on agriculture are discussed (see especially pp.