Reply to "A Critique of Some Recent North American Mortuary Studies"

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REPLY TO
"A CRITIQUE OF SOME RECENT NORTH AMERICAN MORTUARY STUDIES"

Joseph A. Tainter

Braun's critique is characterized by misleading statements, by misrepresentations of my work, and by repetition of cautions that I have already voiced.

As one who has called repeatedly for the development of objective, quantitative methods for measuring characteristics of past societies, I am heartened to see that others share this concern. Theoretical and methodological refinements in this area are certainly to be desired. Unfortunately, through flaws in logic, lack of understanding, and poor scholarship, Braun's critique offers little in this regard.

I will not discuss in detail the coding of the data used in my studies, for I do not believe that the pages of American Antiquity are the proper place to discuss such local concerns as the significance of limestone slabs in Woodland graves, or whether an obscure mound group in the Midwest does or does not have a crematory. I would, however, like to mention two points. First, the criteria used to classify Middle Woodland grave associations are fully described elsewhere (Tainter 1973:49–50), and second, there is not, as Braun believes, any auto-association in the coded data. Thus, not all facilities classified as central features had ramps, while at least two disarticulated Middle Woodland burials were rearticulated as part of the mortuary ritual. Parenthetically, I find Braun's criticisms concerning auto-association in the Klunk-Gibson data puzzling in light of the fact that Braun's own study of the same data contains variables which he admits suffer from this problem (Braun 1979:72). I will also mention that, if Braun had contacted me concerning those aspects of my coding criteria which he found unclear, I would have been happy to explain my rationale.
As I read Braun's paper, he makes a number of points: (1) that the classification procedure I used did not produce reliable results; (2) that the measures of social characteristics I employed were not appropriate; and (3) that my general assumptions and theoretical arguments are at fault. These matters are easily dealt with.

CLASSIFICATION

Braun raises the following objections to the clustering procedure I employed: (1) that binary data can only recognize qualitative, not quantitative, distinctions; (2) that redundancy in mortuary attribute combinations, which seems to be a universal feature, suggests that polythetic classification procedures are more appropriate than monothetic ones; (3) that several classification techniques should be compared to obtain the most reliable results; (4) that collapse of information into binary categories results in loss of information; and (5) that I found it necessary to subdivide my terminal clusters to obtain the results I needed for further analysis.

The notion that binary data cannot recognize quantitative social distinctions is easily dismissed. I will mention that the recognition of quantitative social variation has been the major purpose of my work. But more to the point, in one of the few cross-cultural studies of ethnographic mortuary systems ever conducted, Saxe (1970:112, 231–233) showed that analysis of binary data has the power not only to isolate quantitative social variables, but even to discriminate between continuous and graded social distinctions. Saxe derives from this a powerful measure of sociocultural complexity (1970:112).

Braun suggests that because it seems, from Saxe's ethnographic study, that mortuary ritual employs a highly redundant code, polythetic classification procedures would be more appropriate than monothetic ones. Braun forgets, however, that more than five years ago I pointed out this potential for redundant data sets, but I noted at the time that Saxe's (1970:102–109, 230–231) ethnographic cases were not perfectly redundant (Tainter 1975a:13). More tellingly, I showed that in one experiment, polythetic cluster analysis simply did not give suitable results. The goal of such studies is to classify the burials so that socially distinctive individuals are segregated into different classes. Polythetic clustering procedures could not achieve this. Braun's suggestion that several classification techniques be compared for individual data sets is precisely what I did (Tainter 1975a), with results indicating that only one technique yielded usable results. Finally, I would point out that if Braun is correct in his belief that mortuary rituals may be perfectly redundant, then the need for multivariate classification is obviated, as is his protracted critique. But the world is never so simple.

Braun's complaint that collapse of data into binary categories results in loss of information may in some cases be correct. I would never advocate monothetic-divisive analysis where binary coding would do damage to the data. Indeed, I said as much in 1975, when I wrote that such techniques "are far more appropriate for mortuary data which can be coded binarily" (Tainter 1975a:13). More basically, Braun shows no awareness of the fact that classification always results in loss of information. This must in turn be evaluated against the information gained.

As for the contention that I manipulated the final output of my monothetic-divisive classifications, I gladly plead guilty. Braun seems to feel that I have violated a serious taboo when I rationally evaluated my results! Braun concludes from this that the criteria I employed were "insufficiently precise." I would remind Braun that I only claimed the technique gave superior results (Tainter 1975a), not perfect ones. No classification technique can ever give "perfect" results, in part because the purposes of classification vary. Braun's complaint, that the clustering technique I used wasn't perfect, is startling in its naiveté.

SOCIOMETRICS

Braun makes the following points in this section: (1) that some of the measures I advanced cannot distinguish between an egalitarian society with a few social outcasts and a hierarchical society with a few high status persons; (2) that some of these measures only quantify degree of centralization; (3) that such things as length of cemetery use may confuse results; (4) that these socio-
metric measures should be standardized by population size; and (5) that burial age cohorts may skew quantitative results.

It is quite true that some of the measures I employed cannot distinguish between an egalitarian society with a few outcasts and a hierarchical one with a few high status persons. But what Braun does not seem to realize is that equations never distinguish anything; people do! I never intended that these equations would be mindlessly applied in situations where they were inappropriate. Indeed, I have already said as much in an earlier paper. Braun cites this paper repeatedly, but does not tell his readers that in it I stressed that the concepts I discussed were "not meant to be taken as a tool kit for archaeological interpretation, but rather as a base for deriving interpretive principles appropriate for each individual case" (Tainter 1978:110). Since Braun has apparently overlooked this statement, or perhaps chosen to ignore it, I will rephrase it more forcefully: the interpretive framework I have presented is not meant to be used as a substitute for thinking!

Braun goes on to assert that "$D_1$ and $RD_1$ will measure the degree of centralization in prestige and authority rather than the degree of vertical differentiation in decision making." Actually, I have never claimed that these measured "the degree of vertical differentiation in decision making." All that I did claim was that $D_1$ and $RD_1$ measured constraints on access to status positions. I went on to suggest that, in hierarchically ordered societies, a measure of such constraint would indirectly monitor a social system's requirements for centralized decision making and for centralized control of behavior (Tainter 1977a:74–75, 1977b:335). Braun appears to be in agreement about this last point, but when he says that "$D_1$ and $RD_1$ provide only an indication of average degree of centralization" (emphasis added), he implies that measuring centralization is somehow a negative accomplishment. To the contrary, I believe that measuring even this much is a major positive accomplishment in the quantitative analysis of past societies.

Braun’s suggestion that the results of my analysis may be affected by the duration of cemetery use applies, in fact, to nearly all archaeological studies, including Braun’s (1979). If he has a solution to this recurrent problem, I would like to hear of it. I did attempt, in my study, to separate out components which were clearly temporally anomalous, such as the Middle Woodland component at the Joe Gay mound group (Tainter 1975b:162–163). Braun apparently does not feel it necessary to mention this to his readers.

Braun proceeds to suggest that my quantitative measures should be standardized by cemetery population size. He suggests that if my measures are divided by population size, the results indicate increasing relative complexity through time. This is a most curious conclusion. Braun has devoted many pages and obviously considerable effort trying to convince the reader that my measures are spurious. But he then turns around and uses those same measures to support his own conclusions! Surely we cannot have it both ways!

In a discussion I had with Braun in the fall of 1976, he suggested that the reason for standardizing sociometric measures by population size is that social complexity increases with population level. This point has been clearly shown by both Carneiro (1967) and Blau (1970). Carneiro in particular demonstrated that when population size and his measure of organizational complexity were transformed to logarithms, the relationship between them was strongly linear (Carneiro 1967:236). Of course, this may present some problems for Braun’s approach. If organizational complexity and population size co-vary linearly, then standardizing the former by the latter will result in a measure which is trivial, for it will not monitor any variation! Surely Braun does not wish to propose such a meaningless measure. In fairness I will note that perfect covariation between population and organizational complexity does not seem to happen. But the demonstrated correlations are of such strength (Carneiro 1967; Blau 1970) that Braun’s approach appears to be pointless.

Braun correctly points out that the age cohorts of burial populations do not directly reflect the sizes of corresponding cohorts in living populations. In societies where authority resides in persons of adult age, the proportion of adult leaders will be exaggerated. And since mortality schedules may vary among members of different rank levels, quantitative comparison may be difficult. I would point out initially that this consideration applies to all mortuary studies, whether quantitatively based or not. And I would also point out (since Braun does not) that this consideration pertains only to egalitarian systems. Where hereditary ranking is present, as in the Wood-
land cases of my study, the effect of burial cohort discrepancies does not apply. Indeed, despite Braun's cautions, this effect may not always apply to egalitarian situations. I have applied these measures in a carefully controlled study in which the burials of an egalitarian and a stratified community on the Island of Hawaii, both contemporaneous, were compared. The results confirmed the usefulness of the sociometric measures, for both the egalitarian and the stratified systems (Tainter and Cordy 1977). Curiously, Braun does not mention this study in his critique, perhaps because its results were so positive.

THEORETICAL FRAMEWORK

In regard to my theoretical framework, Braun raises two objections: (1) that different dimensions of organization respond to different kinds of adaptive constraints, while the equations I proposed measure only abstract structural form, and (2) that I did not test whether differences in energy expenditure in mortuary ritual always reflect status distinctions.

Of course different dimensions of organization may respond to different kinds of adaptive constraints. It is also true, however, that different kinds of processes may lead to similarities in the general, structural form of different societies. Much of social anthropology rests upon this principle. In any event, I have recently called upon the profession to pay greater attention to the behavioral significance of the social models we develop and have proposed one method of doing this (Tainter 1980). Again, in the interest of fairness, I will mention that Braun did not have access to this last study when his critique was written. He did, however, have access to an earlier version of it (Tainter 1973:134-146).

Braun contends that my ethnographic test of energy expenditure "does not consider, let alone dismiss, the possibility that differences in burial ritual energy expenditure could occur among individuals who do not differ in social importance." This is preposterous. I not only did consider this possibility. I have even given ethnographic examples of it in papers which Braun cites (Tainter 1975b:59-61, 1978:128)! Braun has misled his readers by making a demonstrably false claim. I will not accuse Braun of intellectual dishonesty, but I will accuse him of at least poor scholarship. Perhaps Braun has not read so carefully the work he proposes to criticize.

CONCLUDING REMARKS

Braun does raise some interesting and important questions. I hope that I have dealt with these to the reader’s satisfaction. His critique is marred, however, by misleading statements, by misrepresentations of my work, and by his having claimed as his own cautions that I have already voiced. What is most disappointing is that, in responding to Braun, I have been forced to repeat so much of what I have written before.

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WEBSTER vs. CHAMPLAIN

Bruce G. Trigger

Webster’s (1979) thesis that the Huron were prepared to hunt deer at a considerable energetic loss is vitiated by a serious factual error.

Gary Webster’s recent discussion (1979) of “Deer hides and tribal confederacies” is vitiated by a serious error of fact that has gone unmentioned in Gramly’s (1979) reply. Webster’s calculations are based on the assumption that in November, 1615, Samuel de Champlain witnessed 500 Huron hunters kill 120 deer over a period of 28 days. In fact, Champlain informs us that on October 28, having finished their raid against the Iroquois, the war party of 500 men that he was accompanying broke into small bands to return home. Some planned to return to the Huron country directly, others to hunt and fish along the way. Champlain accompanied the Huron chief Atironta to a campsite located some distance east of Rice Lake. There a band of about 25 men erected two or three lodges to live in during the ensuing hunt. Over the next 10 days, some of the men seem to have gone fishing while the rest built a hunting trap or enclosure into which deer could be driven. The group then hunted deer in this location for approximately a month.

Even if we were to use Champlain’s vexatiously loose sentence structure as a warrant to assume that those who went fishing were not originally among the 25 men who established the hunting camp (and familiarity with Champlain’s narrative style suggests to me that it would be wrong to conclude this), there is still no reason whatever to assume (as Webster has done) that they constituted the rest of the now scattered war party (Biggar 1922–1936 (3):81–92). In addition, Champlain’s briefer description of a hunting episode that he witnessed en route to attack the Iroquois makes it clear that if 500 men had been present, a hunting enclosure would not have been needed (Biggar 1922–1936 (3):60–61). Also, if the hunters in the returning group initially were stationed 80 paces apart, as Champlain states they were Biggar 1922–1936 (3):84), 25 men would be enough to use an enclosure for a successful drive, whereas 500 would be absurdly too many. In the course of 28 days, the Hurons are reported to have killed 120 deer and to have kept the skins and fat, but only a little of the meat, to take home with them. Yet when they left for the Huron country, each hunter was carrying a heavy burden, which Champlain estimated weighed 100 pounds (livres). This again suggests a party of far less than 500 men.

Webster appears to have been misled by Heidenreich’s (1971:207) erroneous description of this hunt, a flawed passage in an otherwise admirable and authoritative book. Proper historiographic