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A Rejoinder to Ackerly and Young's Comments on My Analysis of Formative Period Sites in the Valley of Mexico

VINCAS P. STEPONAITIS
Department of Anthropology
State University of New York, Binghamton

In a recent commentary ("Formative Period Political Differentiation in the Southern Valley of Mexico," *AA* 86:976–985, 1984), Ackerly and Young find fault with my analysis of Formative period settlements in the Valley of Mexico (Steponaitis 1981), and question the validity of certain inferences that I reached. These inferences originally stemmed from an argument in which I constructed an ideal mathematical model, deduced implications from this model, and showed that the prehistoric settlement data were largely consistent with these implications. Ackerly and Young challenge the last step in this line of reasoning and contend that their "rigorous statistical testing" fails to confirm an adequate fit between my model's predictions and the data in question. They further claim that my model contains a raft of "unstated" or "implicit" assumptions, from which they deduce additional implications that are not supported by the evidence.

Sadly, this critique is so fraught with misunderstandings that it never really hits the mark. Not only do Ackerly and Young misinterpret their own statistical results, but also they attribute to me various assumptions and statements that I never made. By injecting these assumptions, they distort my original argument beyond recognition. Thus, the model they purport to test and find invalid is largely their own creation.

I will set forth the major problems with their commentary, treating statistical matters first, and then discussing various substantive issues raised.

Statistical Problems

The theoretical model I presented was based on the premise that the population of any settlement was directly proportional to the amount of food it had available. The amount of food, in turn, was assumed to be dependent on two things: (1) the productivity of the settlement's catchment, and (2) the flow of tribute either into or out of the settlement. By formulating these and other assumptions into a series of mathematical equations, I derived a set of ideal predictions that can be briefly stated as follows. If site size is plotted against catchment productivity, each level in the political settlement hierarchy should be represented by a distinct line of positive slope, with the number of lines on the diagram corresponding to the number of levels in the hierarchy. Lowest on the diagram should be the villages, above them the local centers, and higher still the regional centers. Ideally, each successively higher line should have a larger y-intercept and a somewhat steeper slope (Steponaitis 1981:325–332).

In applying this model to the Valley of Mexico data, I plotted size against productivity for sites of the Middle, Late, and Terminal Formative periods separately. On the whole, patterns very similar to those predicted were observed. The Middle Formative diagram exhibited only villages, whereas the Late and Terminal Formative diagrams showed three-tiered hierarchies consisting of villages, local centers, and regional centers. When least-squares lines were fitted to these scatters, the nucleated villages consistently had y-intercepts close to zero, and the local centers had y-intercepts considerably greater than zero. Moreover, within each period the best-fit line for local centers exhibited a greater slope than that for nucleated villages. Although the correspondence between model and data was by no means perfect, it nevertheless seemed to me remarkably close, especially in view of all the potential sources of error. Given this correspondence, I was able to use the model as a basis for making empirical estimates of tribute rates, the relative number of nonproducers, and the degree of regional centralization in each of the periods considered (Steponaitis 1981:340–356).[1]

Ackerly and Young take issue with this analysis by questioning the reality of the empirical patterns I identified. To support their claim, they perform a series of statistical tests comparing the regression parameters of local centers with those of nucleated villages. While not denying that the computed values of these parameters differ in exactly the way the model predicts, Ackerly and Young show that many of these differences are not statistically significant at the 0.05 level. This finding applies to both the slopes and the *y*-intercepts of the Late Formative sites, as well as to the slopes of the Terminal Formative sites. These results lead them to argue that the model is not adequately supported by the evidence and that many of the inferences I drew from the model are unwarranted.

The weakness of their argument lies not in their statistics, but in how these statistics are interpreted. The key question is, what can we legitimately infer from their finding that the differences between certain regression parameters are not statistically significant? Contrary to what Ackerly and Young suggest, such a result does not necessarily imply that the observed differences are random or interpretively meaningless. Rather, it simply indicates that the available evidence is insufficient to formally reject a null hypothesis (H_0), which states that the sample of sites is drawn from an underlying population in which the parameters being compared are equal. This is a far cry from saying that H_0 must be true, or even that the evidence supports H_0 more strongly than some precisely specified alternative hypothesis (H_1). Indeed, one cannot justifiably argue that such a result provides convincing evidence in favor of H_0, *unless* one demonstrates that the test presents a reasonably high probability of rejecting H_0 when H_0 is false. This probability is referred to as the *power* of the test, and its value depends on both the sample size and the magnitude of the difference that one is looking for.

That Ackerly and Young ignore this point is regrettable, for it can easily be demonstrated that the power of their tests is so low that one would not expect a statistically significant difference in slopes, even if my model holds true. Calculating power requires that we specify what the predicted difference between slopes of local centers and villages should be. As shown in the Appendix, the magnitude of this expected difference is largely a function of tribute rates. Based on the tribute rates that have been estimated for the Late and Terminal Formative, my equations predict that the slopes should differ by only about 10% (Table 1). The probability that Ackerly and Young's tests would correctly detect a population difference this small is only about 0.06 (Table 2). Under these circumstances, it is ludicrous for Ackerly and Young to argue that a lack of statistical significance constitutes strong evidence against my model. If anything, exactly the opposite is true: given what is predicted by the model, a statistically significant difference in slopes is so unlikely that finding such a difference would be legitimate reason to question the model's validity. Ironically, by not contradicting the model, Ackerly and Young's statistical results tend to confirm it.

Indeed, when we examine the differences in slope observed for each period (Table 1), we find that they are very close to my predicted values, much closer, in fact, than they are to zero—the value stipulated by the null hypotheses that Ackerly and Young accept. This situation points up a major weakness in the usual method of significance testing: the procedure automatically insures acceptance of the null hypothesis unless the evidence against it is overwhelming. Although such an approach may be logically justified in certain experimental settings or in cases where there are strong *a priori* reasons to believe the null hypothesis is valid, its utility in the nonexperimental social sciences, especially archeology, has been questioned (Cowgill 1977; Henkel 1976; Morrison and Henkel 1970). In the present case I see no reason—logical, practical, or philosophical—to treat the arbitrary null hypotheses proposed by Ackerly and Young as being intrinsically more plausible than the hypotheses that stem from my model. If we are to compare these alternatives statistically, it makes much more sense to compare them on an equal footing. This can be done by computing the two-tailed probability (p) of obtaining the observed difference in slopes with reference to each hypothesis separately. As shown in Table 3, the observed values for the Late and Terminal Formative yield probabilities ranging from 0.60 to 0.73 in relation to Ackerly and Young's null hypothesis of no difference, and probabilities of 0.85 to 0.94 in relation to the hypothetical differences generated by my model. Since all these probabilities are rather high, the evidence is best viewed as ambiguous. However, if one *had* to decide on the basis of such a comparison, one would have to say that the evidence favors the predictions of my model over the alternatives accepted by Ackerly and Young.

Turning now to the matter of intercepts, my model stipulates that the *y*-intercept of local centers should always be greater than that of

Table 1
The predicted differences between slopes of local centers and nucleated villages.

	Observed slopes[a]		Estimated tribute rates[b]		Differences in slope	
Period	M_L	M_V	t_1	t_2	Predicted[c]	Observed
Late Formative	.0688	.0592	.157	.29–.46	.006–.008	.0096
Terminal Formative	.0514	.0438	.162	.43–.44	.005	.0076

[a]M_L is the slope of the best-fit line for local centers; M_V is the slope of the best-fit line for nucleated villages. The values are taken from Steponaitis (1981:Tables V and VIII).
[b]These figures are taken from Steponaitis (1981:Tables VII and X, n. 10 and 13). The rationale behind the alternative estimates of t_2 for each period is explained therein.
[c]Calculated using equations (8) and (9). Both equations yield virtually identical estimates. The different predictions for the Late Formative stem from using alternative possible values of t_2 in the equations.

Table 2
The power of Ackerly and Young's statistical tests for differences in slope.[a]

	Ackerly and Young's test results				
Period	Null hypothesis (H_0)	Two-tailed probability (p)	Decision on H_0 (α = .05)	Alternative hypothesis (H_1)	Power[b] with respect to H_1
Late Formative	$\Delta_1 = 0$	.73	not rejected	$\Delta_1 = .006$ $\Delta_1 = .008$	.06
Terminal Formative	$\Delta_1 = 0$	.60	not rejected	$\Delta_1 = .005$	.06

[a]The population parameter for the difference between slopes is represented by the symbol Δ_1. In Ackerly and Young's terms, this is equivalent to $\beta_{1\ (\text{Local Centers})}$ minus $\beta_{1\ (\text{Nucleated Villages})}$.
[b]Power is calculated as the probability of obtaining a t statistic whose value falls within the rejection subset of Ackerly and Young's test of H_0, *assuming* that H_1 is true. This probability was estimated by linear interpolation from Table 3 in Fisher and Yates (1957).

villages (Steponaitis 1981:326–332). The regression lines for both the Late and Terminal Formative showed exactly this relationship, and in each case the difference between intercepts was substantial (Steponaitis 1981:Tables V and VIII). Based on their statistics, Ackerly and Young conclude that the difference is significant for the Terminal Formative sites but not significant for the Late Formative sites. Once again, however, it seems that these results are largely a reflection of the power of the tests that were employed.

Unlike for slopes, there is no satisfactory way to arrive at a theoretical value for what the difference between intercepts should be. We do know, however, that the *observed* differences are perfectly consistent with the model, since the vertical separation between centers and villages on the scatter diagrams yielded estimates of the first-order tribute rate (t_1) that were ethnographically plausible (Steponaitis 1981:346–347, 354–355; 1984). Hence, it is both legitimate and instructive to calculate the power of Ackerly and Young's tests in relation to these observed values (Table 4). Note that the Late Formative test, which failed to reject H_0, had a power of less than 0.20, whereas the Terminal Formative test, which did reject H_0, had a power of nearly 0.70. In other words, the one test that had a reasonably high probability of successfully detecting a difference of the sort predicted by my model did, in fact, detect such a difference. Given the low power of the other test, the negative result it produced cannot be given much interpretive weight.

Ackerly and Young seem to believe that no hypothesis can be taken seriously unless it is found to be statistically significant at the 0.05 level. This notion, as a general proposition, has very little to recommend it. Not only is the 0.05 level a totally arbitrary criterion, but there is no reason to believe that it should be

Table 3
Sampling probabilities of obtaining the observed differences between slopes, computed with respect to alternative population parameters.

Period	Observed difference between slopes[a] (d_1)	Assumed population parameter (Δ_1)	Student's *T* statistic[b]	*df*	Two-tailed probability[c] (*p*)
Late Formative	.0096	0	.357	15	.73
Late Formative	.0096	.006	.143	15	.89
Late Formative	.0096	.008	.071	15	.94
Terminal Formative	.0076	0	.533	11	.60
Terminal Formative	.0076	.005	.200	11	.85

[a]Taken from Table 1. The standard errors of the estimates of d_1 are .0277 for the Late Formative and .0150 for the Terminal Formative. These values were calculated using the formula in Kleinbaum and Kupper (1978:100–101; also see Ackerly and Young's n. 4).
[b]Calculated as $(d_1 - \Delta_1)/S$, where S is the standard error of d_1.
[c]Estimated by linear interpolation from Table 3 in Fisher and Yates (1957).

Table 4
The power of Ackerly and Young's statistical tests for differences in *y*-intercepts.[a]

Period	Ackerly and Young's test results: Null hypothesis (H_0)	Two-tailed probability (*p*)	Decision on H_0 (α = .05)	Alternative hypothesis (H_1)	Power[b] with respect to H_1
Late Formative	$\Delta_0 = 0$	.24	not rejected	$\Delta_0 = 27.97$	.19
Terminal Formative	$\Delta_0 = 0$	.02	rejected	$\Delta_0 = 25.45$	.69

[a]The population parameter for the difference between *y*-intercepts is represented by the symbol Δ_0. In Ackerly and Young's terms, this is equivalent to $\beta_{0\ (\text{Local Centers})}$ minus $\beta_{0\ (\text{Nucleated Villages})}$.
[b]See note b in Table 2.

equally appropriate in all situations. When sample sizes are small, and the relationships of interest are subtle, significance tests have so little power that they rarely provide any real help in deciding between competing hypotheses. Of course, if the power of a test is low, some would argue that a lack of significance merely indicates that a larger sample is necessary before *any* conclusions may be drawn. Such an attitude is fine for experimental scientists, who can almost always expand their samples until the desired power is obtained. Archeologists, however, often deal with samples that are intrinsically limited by historical or geological factors and cannot be enlarged at will. The settlement data from my study area provide a good case in point: these data were obtained by means of a total survey that ensured discovery of virtually all Formative period sites large enough to be of interest (Sanders et al. 1979:1–32). Although the resulting sample may not be as large as one would wish, it comprises all, or practically all, of the relevant sites that exist. Clearly, the power of Ackerly and Young's tests cannot be easily increased, and to insist on statistical significance as *the* criterion for plausibility under these circumstances is both self-defeating and unnecessary.

Scientific hypotheses, even those subjected to statistical tests, should always be accepted or rejected on the basis of informed judgment, rather than blind adherence to an arbitrary decision rule. I do not mean to imply that inferential statistics are always useless or unimportant, but only that the results of such tests can never be translated directly into statements regarding the likelihood that a certain hypothesis is true or false (Cowgill 1977). In deciding on the merits of a given hypothesis, one may take into account the results of statistical procedures, but it is also important to consider factors such as its theoretical plausibility, its relationship to data not used in the statistical tests, and its consistency with other hypotheses that are thought to be true. Often, these other factors are far more compelling than statistical tests, especially when the results of the latter are ambiguous because of a lack of power.

In sum, I do not believe that any of Ackerly and Young's tests vitiate the evidence presented in support of my model. When writing the original paper, I considered using inferential statistics and deliberately chose not to. One reason was that my data came from a 100% survey of the study area and therefore comprised a complete population rather than a random sample. Whether the use of significance tests is appropriate in such circumstances is a thorny issue about which there is little agreement even among statisticians (Cowgill 1977:366–367; Henkel 1976:78–88; Morrison and Henkel 1970). Without going into the details of the controversy, suffice it to say that at the time of writing I was not persuaded that such tests were theoretically meaningful. And even if they were, I realized that tests having so little power would at best yield ambiguous results, and at worst create needless confusion.

Lest anyone remain unconvinced (or bored) by the statistical arguments, I suggest that they simply *look* at the scatter diagrams published in my original article (Steponaitis 1981:Figs. 5, 8, 11). The patterns in these diagrams are so clear-cut visually, and *so consistent for all three periods,* that it is hard to believe that they could have arisen purely by chance, as Ackerly and Young imply. Obviously, such patterns demand interpretation, and my model was offered as a start in this direction.[2]

Substantive Misunderstandings

So much for Ackerly and Young's statistical tests of the predictions that I myself put forward. As noted earlier, they also claim to find additional implications, not previously noticed, that are inconsistent with the available evidence. All this would seriously damage the credibility of my argument, were it not for one simple fact: none of the additional implications they propose are actually entailed by my model. In "deducing" these new implications, Ackerly and Young betray a profound misunderstanding of my original paper. Indeed, their argument is so full of incorrect and misleading assertions that it is impossible to deal adequately with them all. I will therefore confine my comments to those I consider most important.

To begin with, Ackerly and Young repeatedly claim that my analysis "assumes little or no technological innovation or agricultural intensification" over the span of time from the Middle to the Terminal Formative. Such statements are simply not true. My analysis relied on an index of catchment productivity that was equal to the number of hectares of arable land within a fixed distance of each settlement. As explained at length in the original article (Steponaitis 1981:334–358), this index was used to provide relative estimates of productivity for the sites *within each period separately.* So long as the intensity of production was approximately equal at all settlements dating to a given period, the index could be expected to yield valid results (Steponaitis 1984:143–144). Logically, there was never any need to assume that the level of intensity remained unchanged from one period to the next. I was quite convinced when writing the article that intensification had occurred in the Valley of Mexico from the Middle to Terminal Formative times (note that Sanders [1976] is listed in my original bibliography); and it is precisely for this reason that all between-period comparisons of catchment productivity were avoided in my analysis.

A second misconception is evident in their belief that my model contains "an unstated assumption that the ratios of the number of local centers to the number of nucleated villages are constant." Nothing in my article suggests that such an assumption was made, and nothing in the logic of the model requires that it be made. The equations I presented clearly postulate certain relationships among catchment productivity, tribute flow, and the *sizes* of settlements measured in terms of the relative number of people (producers and nonproducers) who lived there. The model implies that, for the region as a whole, the total ratio of nonproducers to producers is a function of the tribute rate (t_1), the value of which may change from one period to another. Even if one were to specify the tribute rate exactly, one could not from the model predict how the population would be distributed over the landscape. A given number of nonproducers, for example, could live in one large center or ten smaller ones, depending upon the average size of the districts from which they collect tribute. Similarly, a given number of producers could be found in 10 villages or 100, depending on the nature of the settlement pattern, and the way in which productive soils happen to be distributed. In short, my theoretical model contains no premises, and makes no predictions, regarding the *numbers* of settlements in a region, in either relative or absolute terms. For

Ackerly and Young to maintain otherwise is wrong.

Another mistake is made when Ackerly and Young attribute to me the assumption that only nucleated villages paid tribute to centers, and dispersed villages did not. Quite the contrary, I assumed that *all* producers paid tribute, regardless of the settlement types in which they lived. This point was made repeatedly throughout my article (Steponaitis 1981:326, 327, 332, 346, 354, Tables VII and X), and it is hard to imagine that a careful reader could have missed it.

In view of these misunderstandings, what can be said about the "additional implications" supposedly deduced from my model? Let us take a closer look.

The first implication proposed is that the observed slopes of nucleated villages should decrease through time. Ackerly and Young support this expectation with the following line of reasoning: as the number of levels in the political hierarchy increased, so too did the tribute demands on producers, thereby reducing (in an absolute sense) the number of villagers that could be supported per hectare of arable land. A statistical comparison of regression lines for nucleated villages dating to the Middle, Late, and Terminal Formative periods leads Ackerly and Young to conclude that the expected decrement in slopes did not occur. This, they contend, is sufficient reason to reject my model.

Although I do not deny that the settlement hierarchy became more complex from Middle to Terminal Formative times, and that tribute rates probably increased, it does not necessarily follow from my model that village slopes should have correspondingly decreased. The important point to realize is that the observed slope not only depends on the tribute rate but also is directly proportional to the intensity of production, that is, the absolute yield per arable hectare (Steponaitis 1983:133). All of Ackerly and Young's deductions are based on the premise (wrongly attributed to me) that the intensity of production remained unchanged through time. Because this assumption is patently unrealistic, it never entered into my analysis. Hence, I am not the slightest bit surprised that the slopes failed to diminish, and do not believe that such a finding invalidates my original argument. Indeed, the intensification that is known to have taken place during Formative times could well have offset any decreases in slope due to tribute, by increasing the agricultural output per unit land, thereby allowing more villagers to be supported.

The second implication offered by Ackerly and Young, partly related to the first, has to do with the relative numbers of centers and nucleated villages within the study area. Allegedly following my model, they argue that an increase in the number of centers should be accompanied by either a decrease in the slope of nucleated villages, or an increase in the number of these villages. Inasmuch as the former alternative seems not to be true, they focus their attention on the latter, showing that the change from Late to Terminal Formative involved a 50% increase in the number of centers, but a 40% decline in the number of nucleated villages. These figures, they claim, render my model implausible.

The flaws in this argument should by now be readily apparent. As explained earlier, the theoretical model I presented has no inherent premises or implications concerning the relative frequencies of different settlement types. In the course of deducing these implications, Ackerly and Young explicitly presuppose (1) that the intensity of production remained constant through all three periods, and (2) that nucleated villages paid tribute, but dispersed villages did not. Neither of these assumptions was contained in my original argument. Hence, any "test" of my model that relies on these assumptions is fundamentally misconceived, and cannot be legitimately used to evaluate my conclusions.

At a more general level, Ackerly and Young seem to be concerned that my interpretation of the Valley of Mexico data portrays a situation in which (to use an appropriate cliché) there are too many Chiefs and not enough Indians. This is certainly a valid consideration, and one that I explicitly addressed in the original study. The most straightforward way to approach this matter is not by comparing the relative numbers of centers and villages but by estimating directly the proportion of nonproducers in the region's total population. The information gleaned from the scatter diagrams, when interpreted according to the model (Steponaitis 1981:331–332, Tables VII and X), suggests that nonproducers constituted only 16% of the population during both the Late and Terminal Formative periods. Supporting such an elite contingent would require that the average producing household give up 16% of its annual yield as tribute. Such a rate is not at all implausible and conforms well with ethnographic data on tribute payments in societies of comparable complexity (Steponaitis 1981:n. 11, 1984:145–147).

My estimates, of course, presuppose that the tribute load was shared by all producers, regardless of the type of settlement in which they lived. At one point in their argument, Ackerly and Young entertain the notion of including dispersed villages as part of the tribute-paying population (apparently not realizing that I had already done so) but dismiss the possibility with the following statement:

> If the dispersed villages are grouped with nucleated villages as potential sources of tribute, the correlation between size and catchment productivity becomes tenuous. . . . This would effectively compromise the theoretical basis for Steponaitis's model of political differentiation. [p. 984]

It is true that dispersed villages, unlike other sites, consistently failed to show a positive correlation between size and productivity. I took note of this pattern in my original study, and suggested some possible explanations for it (Steponaitis 1981:341, 345, 352). Even within the framework of my model, a linear relationship between size and productivity does not result from tribute flow per se, but from the rules that link a settlement to its own catchment. Obviously these rules were different for dispersed as compared to nucleated villages, but this finding does not preclude the possibility that both kinds of settlements paid tribute. And so long as nucleated villages *do* exhibit the predicted positive correlation, the regression line fitted to these points can be used to estimate certain dimensions of political complexity and tribute flow, which was the principal goal of my analysis. In other words, the pattern of dispersed villages may well be anomalous, but this does not vitiate the utility of my model for interpreting the Valley of Mexico data in the way that I did.

To sum up, the problems that Ackerly and Young identify in my analysis stem largely from their own misunderstandings. In the process of deriving additional test implications, Ackerly and Young *change* my model by adding assumptions that I never made. Thus, the model they empirically reject is really theirs, not mine.

Appendix: Finding the Predicted Difference between Slopes

In order to derive an expression for the predicted difference between slopes of local centers and nucleated villages, we must make use of the equations for an idealized, three-level hierarchy that were presented in my original paper (Steponaitis 1981:327–330). These equations stipulate that, in a scatter diagram of site size versus catchment productivity, local centers and villages should fall along distinct lines having the following slopes:

$$M_L = K(1 - t_1 t_2) \quad (1)$$

$$M_V = K(1 - t_1) \quad (2)$$

where M_L is the slope of the line of local centers, M_V is the slope of the line of villages, t_1 is the first-order tribute rate (i.e., the average proportion of each household's subsistence production that is given up to the political establishment as tribute), t_2 is the second-order tribute rate (i.e., the fraction of all tribute collected at local centers that gets passed on to regional centers), and K is a factor expressing the number of units of population that can be supported per unit of catchment productivity (in whatever units these variables happen to be measured).

The difference in slopes (Δ_1) is found simply by subtracting equation (2) from equation (1):

$$\Delta_1 = M_L - M_V \quad (3)$$

$$= K(1 - t_1 t_2) - K(1 - t_1) \quad (4)$$

$$= K(t_1 - t_1 t_2) \quad (5)$$

As noted originally, both t_1 and t_2 can be estimated empirically from the size-productivity diagrams on which the analysis was based (Steponaitis 1981:331–332, n. 2). Inasmuch as these estimates are logically independent of the observed difference between slopes, they can be substituted into equation (5) without risk of tautology. This leaves us only to arrive at an empirical estimate for K, which can be deduced in two ways. From equation (1) we have

$$K = \frac{M_L}{(1 - t_1 t_2)}, \quad (6)$$

and from equation (2)

$$K = \frac{M_V}{(1 - t_1)}. \quad (7)$$

Substituting equations (6) and (7) into equation (5), we obtain two alternative expressions for the predicted difference between slopes, one based on the observed slope of local centers, and the other on the observed slope of villages:

$$\Delta_1 = M_L \frac{(t_1 - t_1 t_2)}{(1 - t_1 t_2)} \quad (8)$$

$$\Delta_1 = M_V \frac{(t_1 - t_1 t_2)}{(1 - t_1)} \quad (9)$$

Notes

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[1] I am grateful to Ackerly and Young for pointing out the inadvertent omission of site IX-11 from my published listing of raw data (Steponaitis 1981:Table III). For the record,

the missing information is as follows: Period, T.F.; Type, N.V.; Size (ha), 6.0; P(1 km), 204.0; P(1.5 km), 324.0; P(2 km), 482.0.

[2]At one point, Ackerly and Young propose an alternative model based on the premise that every village pays the same amount of tribute regardless of its size. This premise strikes me as being unrealistic in the Valley of Mexico case, since Formative villages varied greatly in population. The smallest villages are thought to have been inhabited by fewer than 200 people, and the largest by more than 2,000 (Parsons et al. 1983). It is hard to imagine that villages so different in productive capacity would have been required to make identical contributions to the political treasury. Besides, even if one were to use their model instead of mine, none of my substantive conclusions would be changed, since their model results in exactly the same estimates of the three political-economic variables I originally set out to reconstruct (cf. Steponaitis 1981:321).

[3]Ackerly and Young seem to have misread not only my assumptions, but also my conclusions. Time and again they portray me as believing that regional centers did not appear in the Valley of Mexico until the Terminal Formative period. In fact, I argued that such centers were established during the Late Formative—a point repeated in my article at least six times (Steponaitis 1981:344, 346, 358, Figs. 8 and 10, Table VIII).

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In Defense of *Sexual Practices*

DONA LEE DAVIS
RICHARD G. WHITTEN
Social Behavior Department
University of South Dakota

As two instructors who have attempted to both utilize and advocate the anthropological approach in teaching human sexuality (Davis and Whitten 1983), we were saddened and surprised to read J. W. Edwards's (*AA* 86:782–783, 1984) review of Edgar Gregersen's *Sexual Practices: The Story of Human Sexuality* (1983). We feel that the review is far from