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More on Estimating Catchment Productivity in the Valley of Mexico

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Finsten et al. (*AA* 85:124-129, 1983) take issue with my analysis of Formative sites in the Valley of Mexico, arguing that the methods I used to estimate catchment productivity were flawed, and that these methods largely predetermined the favorable results I obtained. Although the issues they raise are certainly important, many of their criticisms are unjustified.

Their first and most fundamental criticism stems from the fact that my catchment areas were initially defined by drawing a circle, the circumference of which was a constant distance from the edge of the site; in other words, the total radius of the initial circle was equal to the settlement radius plus the constant catchment radius. This method, they argue, automatically gives larger sites larger catchments, thereby ensuring that a positive relationship between site size and catchment productivity will be found. They attempt to prove that my results are spurious by computing partial correlations; their claim is that if the differences in settlement radius are controlled for, the residual correlation between site size and catchment productivity drops to zero.

This argument is beset with serious problems, not the least of which is that the partial correlations presented by Finsten et al. are statistically meaningless. Note that site size and site radius are not independently measured variables; rather, the value of the latter is always calculated directly from the former by means of a simple algebraic equation (Steponaitis 1981: 335; see equation 4 below). This lack of logical independence violates the basic assumptions of regression and partial correlation analysis, since the "control" variable is merely a transformed version of one of the variables being correlated. In effect, they have computed the partial correlation between productivity and size, controlling for size. The only reason they were able to obtain a value for the coefficient at all is that the exact relationship between site size and radius happens to be quadratic rather than linear. If the radius values are squared to make the relationship linear (a perfectly reasonable procedure in measuring correlations), the total correlation between site size and radius becomes a perfect 1.0, and the partial correlation between size and productivity, controlling for radius, becomes undefined (i.e., computing the partial correlation entails dividing by zero).

It is certainly true that, *other things being equal,* larger sites would tend to have larger catchment areas. Yet, in the Valley of Mexico case, other things were rarely equal. For example, whenever the catchment circles of adjacent sites overlapped, the shared area was partitioned more or less equally between the sites, thereby reducing the overall extent of each catchment. Thus, site packing—the relative position and proximity of sites—significantly influenced the size and shape of catchments, and did so in a way that was totally independent of the effects of site size.

Moreover, it must be emphasized that my index of productivity was not equal to the total catchment area, but rather to the number of hectares of *arable land* within the catchment. Needless to say, the latter figure was generally not equivalent to the former. The Valley of Mexico catchments often encompassed large areas of water and/or nonarable land, which greatly affected the values of the index. Much, if not most, of the variation in local productivity was attributable to this factor.

As a result, my estimates of catchment productivity were not nearly as determined by site size as Finsten et al. seem to imply. The value of the productivity index was affected by several factors, of which site size was only one, and not necessarily the most important. Under these circumstances, the key question is not whether site size had an effect on total catchment *area* (it obviously did), but whether this effect was primarily responsible for patterning observed when site size was plotted against catchment *productivity*. This question is best answered not by correlation coefficients, but by modeling and simulation. In other words, it is relatively easy to predict what the patterning in the data should look like, if the mechanical effects of site size on catchment area were principally responsible for the variation in estimated productivity. The predicted pattern can then be used as a kind of null hypothesis, against which the patterns actually observed in the Valley of Mexico can be evaluated.

The simplest way to construct such a model is to assume that catchment reduction due to site packing is nonexistent, and that arable land is uniformly distributed among all catchments. Under these conditions, the index of productivity (P) becomes directly proportional to the total catchment area, which in turn is a function of site radius (d):

$$P = k[\pi(d+r)^2 - \pi d^2] \quad (1)$$

$$= k[\pi d^2 + 2\pi rd + \pi r^2 - \pi d^2] \quad (2)$$

$$= k[2\pi rd + \pi r^2] \quad (3)$$

where k is a constant expressing the proportion of arable land in each catchment, and r is the constant catchment radius measured from the edge of each site.

As noted previously, site radius (d) is calculated from site size (S):

$$d = \sqrt{\frac{S}{\pi}} \quad (4)$$

Substituting equation (4) into equation (3), we arrive at an expression that describes what a purely mechanical relationship between site size and catchment productivity would look like:

$$P = k\left[2\pi r\sqrt{\frac{S}{\pi}} + \pi r^2\right] \quad (5)$$

$$= k[2r\sqrt{\pi S} + \pi r^2] \quad (6)$$

This relationship is graphed in Figure 1. Each point corresponds to a late Formative site; the vertical axis corresponds to the site's observed size, whereas the horizontal axis shows the predicted catchment productivity based on equation (6) (assuming a 1.5 km catchment radius and k = 1). Note that all sites, regardless of type or position in the settlement hierarchy, fall along a single, steep curve. The regression line that best fits these points has a slope of .242 and a highly negative y-intercept of −206.

One might, of course, regard this initial model as being overly rigid, since real catchments often differ greatly in size and in the proportion of arable land they contain. Hence, it is also useful to consider an alternative null hypothesis which allows for such variation but treats it as being entirely random. This condition can be mathematically simulated by regarding the factor k in the equation (6) as a random variate with a uniform distribution between 0 and 1. Introducing this stochastic element into the model has the effect of randomly "smearing" the points in Figure 1 leftward, thereby destroying any strong linear patterning, except that which might occur by chance.

The implications of this second null hypothesis can be illustrated by the results of a computer simulation, in which the observed sizes of Late Formative sites were regarded as given, and corresponding values of catchment productivity were randomly generated according to the model just described.[1] Two hundred sets of values were independently generated and the usual regression statistics were computed for each set. The mean values of these regression statistics are presented in Table I. The expected slope of the best-fit line for both centers and villages turned out to be positive but very close to zero; the expected y-intercept was slightly less than the mean site size; and the expected value of r^2 was consistently very low. In short, a slight tendency for positive correlation between site size and productivity was evident, but that tendency was extremely weak.

Thus, no matter which null hypothesis we prefer, it is clear that neither generates a size-productivity relationship that bears any resemblance to the patterns actually observed in the Valley of Mexico (Steponaitis 1981:340–358). In the latter case, centers and villages sorted out into distinct tiers, each with a positive slope. The line of centers was above the line of villages, and the y-intercept of the latter was close to zero (Table II). Note that the observed slopes are all at least five times greater than those predicted by the stochastic model, and nearly four times lower than those predicted by equation (6). Moreover, neither null hypothesis

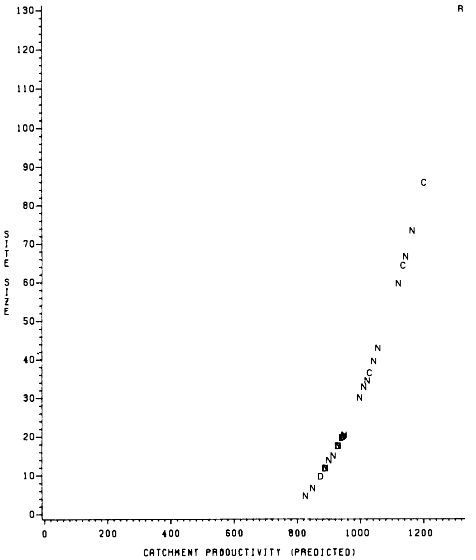

Fig. 1. The predicted relationship between site size and catchment productivity for Late Formative settlements, based on equation (6). (Key to symbols used in the diagram: C, center; D, dispersed village; N, nucleated village; R, regional center.)

can *simultaneously* account for (a) the tiered nature of the size-productivity scatter diagram, (b) the best-fit line for villages that passes near the origin, and (c) the consistently high values of r^2 obtained (never less than .61). All of these observations correspond very well to the predictions of the tribute-flow model I originally proposed, and there is absolutely no reason to believe that they are a spurious outcome of the way I measured catchment productivity.

Yet even if we accept this conclusion, a question will remains: Was the method I used to define catchments the most appropriate one available, or would another method have been better? Let us examine the alternatives.

Finsten et al. propose that a better way to define catchments is to draw a circle of fixed radius measured from the site's center, and to consider all the area within the circle as part of the catchment, including the area covered by the site itself. This method, they correctly point out, has the advantage of obviating any tendency toward mechanical correlation between site and productivity. However, it also has one glaring disadvantage, in that it is difficult to imagine how people could have intensively farmed the area on which their houses were standing. In a situation where sites are consistently small relative to the total catchment area, the error introduced by this factor is negligible, and the method can be used to good effect (e.g., Peebles 1978; Steponaitis 1978). However, one cannot ignore the fact that the Formative period sites I analyzed range from 4 to 130 ha in size. The largest site has an average settlement radius of .64 km. Given that my original analysis was conducted using catchment radii of 1, 1.5, and 2 km (Steponaitis 1981:335), the problems inherent in the method proposed by Finsten et al. become clear: at least 41% of the area within a 1 km catchment circle drawn around the largest site is covered by the site itself! With the catchment radius increased to 1.5 km, at least 18% of the catchment falls within the site—still an uncomfortably large proportion. An awareness of this serious problem is probably what led Finsten et al. to admit that their method of defining catchments was "not entirely satisfactory." I wholeheartedly concur with this assessment, which is why I avoided using the method in the first place.[2]

TABLE I. EXPECTED VALUES OF REGRESSION STATISTICS FOR LATE FORMATIVE SITES, BASED ON A STOCHASTIC NULL HYPOTHESIS.

Site type	Number of sites	Mean site size (ha)	Expected slope[a]	Expected y-intercept[a]	Expected r^2 value[a]
Centers	4	68.5	.008	62.3	.28
(excluding CH-5)			(±.068)	(±45.8)	(±.28)
Nucleated villages	16	30.7	.011	24.8	.09
			(±.020)	(±10.0)	(±.10)
All sites	26	37.5	.020	27.0	.08
			(±.021)	(±11.2)	(±.10)

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[a] This figure is the mean value obtained in 200 independent trials of the model. The observed standard deviation is given in parenthesis below each mean.

TABLE II. OBSERVED VALUES OF REGRESSION STATISTICS FOR LATE FORMATIVE SITES.[a]

Site type	Slope[b]	y-intercept[b]	r^2 value[c]
Centers	.069	14.7	.85
(excluding CH-5)	(±.020)	(±16.7)	(.30, .98)
Nucleated villages	.059	−13.3	.61
	(±.013)	(±10.8)	(.40, .76)

[a] Values are taken from Steponaitis (1981: Table V).
[b] The standard error for each estimate is given in parentheses.
[c] The 68% confidence interval for each estimate is given in parentheses.

As noted previously, the method I chose instead was to draw a circle of fixed radius from the *edge* of the site, and to *exclude* the area covered by the site itself. This method carries with it the possibility of spurious correlations, since, other things being equal, larger sites will tend to have larger catchments. Although the existence of this tendency should be treated as good reason for caution in drawing interpretations, it does not necessarily lead to tautology—a fact that my arguments above have demonstrated. Moreover, it should be emphasized that some degree of built-in correlation between site size and catchment area is not at all unrealistic from an ethnographic standpoint. If one is willing to assume that the size of an agricultural catchment is determined by the maximum distance that farmers are willing to walk to their fields, it follows that larger settlements *do* tend to have more land accessible from within their borders than smaller settlements. This is an inescapable fact of plane geometry, and I can think of no reason why prehistoric people would not have taken advantage of it.

In sum, after carefully considering the advantages and disadvantages of each method, I concluded then (and still believe now) that defining catchment circle from the edge of the site was the only realistic alternative, given the nature of the settlements being considered.

A further criticism raised by Finstein et al. is that my index of productivity relied only on the extent of arable land, and failed to take differential soil fertility and rainfall into account. This point is well taken, at least insofar as soil fertility is concerned. While doing the analysis, I was well aware that differences in soil fertility existed within the Valley, but could not find sufficient reliable information with which such differences could be quantified (Steponaitis 1981:334-335, footnote 6). Regrettably, the maps that Finsten et al. used for this purpose (Sanders et al. 1979: Maps 1 and 11) were not published until after my paper was written.

With regard to rainfall, Finsten et al. suggest that 700 mm of annual precipitation is a critical threshold below which maize yields are reduced by about 20%. It seems to me that the exact value of such a threshold (not to mention the magnitude of the correction factor) would depend a great deal on the nature of Formative crop varieties and irrigation systems—two things about which we know very little. But even if we were to accept these estimates as valid, problems would still remain in trying to apply them archeologically, for how can we determine where the critical 700 mm isohyet was located at 500 B.C.? Climatic studies strongly suggest that substantial fluctuations in the amount of rainfall have occurred in the Valley of Mexico over the last 3,000 years; unfortunately, the available data are so limited that the precise timing, direction, and intensity of these fluctuations have yet to be established with certainty (Sanders et al. 1979:406-409).

Despite these problems, Finsten et al. use the present-day 700 mm isohyet as a basis for estimating Formative period catchment productivities. This procedure is especially risky, since the present 700 mm rainfall contour runs right through the middle of the area where most of the sites are concentrated (Sanders et al. 1979: Map 2). Hence, a small error in the isohyet's assumed location—even on the order of a few kilometers—could substantially alter the outcome of the analysis. The uncertainties involved in correcting for rainfall are so great, and the potential errors so large, that reliance on such a procedure seems just as likely to distort the "real" size-productivity relationships as to reveal them. The simplest kind of productivity index based only on the amount of arable land may not be perfect, but at least it measures something that is directly observable, and permits us to have some confidence in interpreting the patterns that emerge.

The one other adjustment proposed by Finsten et al. involves correcting the productivity index for the presence of certain small sites—hamlets and small dispersed villages—that were ignored in the original study. Although a refinement of this kind is not unreasonable, the magnitude of the correction is so small that it is not likely to make much difference in the analysis. For example, taking account of this factor would not have reduced any of my Late Formative productivity estimates (1.5 km) by more than 6%.

What, then, is my overall impression of Finsten et al.'s reanalysis of the Late Formative data? In comparing their results with mine, the following points should be kept in mind:

1. Since the method they used to define catchments was somewhat unrealistic, I would not expect either of their productivity indexes to yield a pattern that is particularly clear. Hence, it is not surprising that their r^2 values are generally lower than the ones I obtained (see their Table III).

2. Their CPI 2 includes the highly dubious correction for rainfall, whereas CPI 1 does not. This probably explains why the latter index produced higher values of r^2 than the former.

3. In the original analysis, I explicitly noted that dispersed villages consistently failed to behave according to the model, and proposed a number of reasons why this may have been so (Steponaitis 1981:341, 345, 352);. Thus, in evaluating their results with respect to the ideal model, only the lines corresponding to centers and nucleated villages are relevant. The dotted lines which pertain to dispersed villages in Figures 2 and 3 should be ignored.

After taking all these considerations into account, I am a bit amazed at their confident assertion that the expectations of my model are not at all met in their results. Granted, their r^2 values are low and the fit is by no means as good, but the overall configuration of the size-productivity relationship they found still retains many of the elements predicted by the model. Note that the best-fit lines for centers and villages are consistently positive in slope, and that the line of centers is consistently above that of villages. The configuration obtained with CPI 1 is especially similar to that found in the original analysis—only the slope and y-intercept of the centers is substantially changed. If anything, these results seem to imply that the relationships predicted by the model are even more robust to the vagaries of measurement error than I originally believed.

A few minor points raised by Finsten et al. also deserve comment. In discussing the way my original findings were presented, they note that "the use of a smaller scale on the y-axis gives the *illusion* of greater slope than the figures would indicate (Table IV)" (p. 128; emphasis mine). This statement implies that the absolute value of the slope can be taken as some kind of measure of the strength of the relationship between the variables under consideration. Nothing could be further from the truth. The low absolute value of the slope simply reflects the units of measurement employed in the analysis. For example, if the index of catchment productivity is expressed in hectares, the slope of the line for Late Formative centers is .069; if we were to express the same index in square meters, the slope would "increase" to a value of 690, yet the underlying relationship would remain unchanged.

In fact, as an additional check on the plausibility of the model, it is relatively straightforward to estimate what the slope of the regression lines should be if site size was indeed determined by the agricultural productivity of the catchment. This can be done by means of the formula:

$$m = \frac{y}{cg} \qquad (7)$$

Where m is the estimated slope, y is the average annual maize yield (kg/ha), c is the average annual maize consumption (kg/person), and g is the average population density within sites (persons/ha). Archeologists who work in the Valley of Mexico have generally assumed that Formative sites had population densities ranging from 20 to 50 people per hectare (e.g., Parsons 1971; Blanton 1972; Sanders et al. 1979:34-40). The magnitude of Formative maize yields remains a matter of controversy, but most estimates fall within the range between 200 and 1,000 kg/ha (Kirkby 1973; Flannery 1973; Sanders 1976; Sanders et al. 1979: Table 9.1). Finally, it is reasonable to suppose that an average person's maize consumption may have been somewhere between 120 and 200 kg/year (Sanders 1976: 109). Plugging these figures into equation 7, we arrive at a minimum predicted slope of .02 and a maximum of .42. All of the slopes obtained in the original analysis fell squarely within this plausible range (Steponaitis 1981: Tables IV, V, VII).[3]

Finsten et al. also "question the validity" of applying the r^2 statistic to a body of data which, to them, seems "so small and spatially limited." Instead they suggest that Spearman's rank correlation statistic would have somehow served my purposes better. I find this statement puzzling. A major goal of my statistical analysis was to assess goodness-of-fit to a theoretical model which predicted a specific kind of linear relationship between two interval-scale variables: site size and catchment productivity. The r^2 statistic, derived from linear regression, is designed to do just this. As a descriptive device it is elegant and unambiguous, for it simply expresses what proportion of the variance in one variable can be attributed to a linear relationship with the other. Spearman's statistic, on the other hand, is a measure of association designed for *ordinal* rather than interval-scale variables. It takes on its maximum values, positive or negative, whenever two variables exhibit a monotonic relationship, yet a monotonic relationship need not be linear. Thus, to use Spearman's rho as a measure of goodness-of-fit to a linear model is potentially misleading. Why Finsten et al. consider the number of sites and the size of the region to be compelling reasons for using this statistic is never made clear.

To sum up, Finsten et al. fail to demonstrate that the results I obtained in the Valley of Mexico were spurious, and their reanalysis of the Late Formative data does little to undermine my original conclusions. This is not to say that I regard the tribute-flow model as being perfect or necessarily applicable in all nonmarket situations, for there are many potentially relevant variables that it does not take into account. There is no question that exchange relationships, mating networks, and other factors do sometimes influence community size, as Finsten et al. point out. However, the fact that the data *in this case* so closely fit the predictions of the model strongly suggests that only two variables were dominant in determining the size of settlements: local productivity and the position of the settlement in the regional hierarchy.

Notes

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[1] The simulation was written in PL/I. Copies of the source code can be obtained from me on request.

[2] Finsten et al. attempt to finesse the problem by claiming that their method of defining catchments is based on the assumption "that if community size is determined by catchment productivity, then it should follow that more productive catchments will *eventually* produce larger settlements" (p. 126, emphasis mine). This assumption is fundamentally at odds with the basic premise of my original model, that the size of any settlement is directly related to the amount of food it has available at the same point in time (Steponaitis 1981:325). One cannot legitimately evaluate a model by using an index whose underlying logic contradicts the model to begin with.

[3] For present purposes I have not tried to take fallow cycles into account. To correct for this possibility, one could multiply the denominator of equation (7) by a "land-use factor," generally equivalent to the number of years plus one that an average field is left fallow (Sanders et al. 1979:376). Incorporating this factor would tend to lower the predicted slopes, but it is worth noting that even if the average land-use factor were as high as 6, the observed slopes would still fall within the predicted range.

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