THREE MAYA SETTLEMENT PROJECTS AND SOME IMPLICATIONS

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My paper describes three settlement/household projects in which I have participated: Copan, Tikal, and Piedras Negras. Each of these centers was a major Classic Maya royal capital, and each has an abundant corpus of dynastic inscriptions and iconography and a well understood architectural history. Each center and polity is quite different in its natural and social environments. I briefly describe the character and results of these projects, along with their strengths and weaknesses, and then discuss how they have affected my thinking about basic issues of Lowland Maya archaeology: demography, sociopolitical and territorial organization, adaptation to regional landscapes, and the methodological aspects of research design.

A benefit of working in several such places is that one is forcibly struck by differences of many kinds that are sometimes glossed over by the general concept of "Maya civilization". Each project had different research designs and goals, operated at different scales, and utilized different methods. Two of the projects were "evolved" as well as "designed", an important methodological distinction.

COPAN

Copan is an example of a project that was both "evolved" and "designed". The work we began there in 1981 evolved out of earlier research directed by Gordon

Willey and Claude Baudez. Their work established a firm understanding of settlement distribution in the Copan urban core and the surrounding Copan pocket - the 24 sq km area immediately around the Copan Main Group (Fash and Long 1983). Our own efforts (Freter 1988; Webster, Freter, and Gonlin 2000) were designed to supplement the earlier work in three ways:

1) Intensive survey of the outlying or "rural" Copan drainage. Much of the approximately 460 sq km Copan drainage beyond the Copan pocket consists of very rugged topography. We sampled such terrain but mainly focused on the main valley floor and its adjacent foothills, and also on two major tributary valleys. The landscape had been heavily deforested so we used 1:4000 aerial photos to situate each site with an accuracy of about 10 m. Complete coverage extended over 38 sq km of the original photo universe of 129.8 sq km (i.e., that outside the Copan pocket), within which we identified 550 sites with 1106 structures. We also gathered information on soils, natural vegetation, land use, and water sources. John Wingard made a detailed soil model of 200 sq km of the landscape (Wingard 1996). Adding the earlier Copan pocket surveys) to our own brings the total coverage up to 62 sq km – a very large proportion of the landscape in the valley useful for habitation and farming.

2) Test excavations in sample of rural sites stratified by location and site type. Artifacts were not conspicuous on the surface, so extensive test-pitting was necessary. Fewer than 1% of sites in the Copan pocket had been tested by earlier projects, so we made 688 test excavations in 252 rural and Copan pocket sites, stratified by location, scale, and complexity. Some 29.5% of all sites outside the Copan pocket and 13.9 % within it were tested, so 18% of all known Copan valley sites (as of 1988) were sampled by our project. The vast majority of rural sites tested – 84% -- are remains of residences or field huts, and the rest are ritual sites or artifact scatters.

3) Complete household excavations in small, outlying sites. Details of site character are rarely revealed by survey alone. Extensive horizontal exposures of both buildings and ambient spaces were designed to check the accuracy of our impressions of site character based solely on surface mapping and test-pitting. Such information is essential if one is to develop algorithms to make demographic inferences from surface features. They also provided more representative artifact samples than test-excavation did, as well as detailed chronological information and better opportunity to detect any specialized economic activities. Finally, our research at small outlying sites complemented the excavations of elite households earlier carried out by the Harvard/Baudez projects and our own. Such excavations, although commonplace today, were rare in the mid-1980s.

We excavated all or most of eight outlying small sites with 27 structures. Eleven of the exposed structures were undetected on the basis of surface examination. With the exception of one field hut, all sites were residences. None exhibited complex architectural stratigraphy and there were only faint signs of occupation predating AD 600-650. Burials were few (17) and were recovered from only two sites. Human bone collagen yielded a set of AMS radiocarbon dates, one as late as the 15th century in a site with an ostensible "Late Classic" Coner phase assemblage (Webster, Freter, and Storey 2004).

Real or Potential Biases and Limitations: The Copan landscape has been heavily altered by natural processes and human activity. Early settlement (before about AD 400) on the valley floor is deeply buried by alluviation, and many later sites have been destroyed or buried both on the valley floor and the northern slopes of the Copan pocket. Only one major tributary stream was heavily surveyed. We did not survey the lower reaches of the Copan River in Guatemala although we have some information about it from the research of Carson Murdy (1991). Our several models of Copan settlement history and demography take these deficiencies into account and we think our characterizations are generally

accurate. In any case we have nothing comparable to the Copan data set from other parts of the Maya Lowlands.

Our Copan work represents an "old fashioned" surface survey, based partly on methods refined in the Valley of Mexico and elsewhere. We operated without GIS, GPS, or any capacity for computer recording or analysis of spatial data in the field. Our success is due to the work of previous projects, to generous funding that allowed us to operate at a large scale, and to ancillary work that enriched our field data – notably the soils study by Wingard and the obsidian hydration dating research of AnnCorinne Freter (Freter 1988; Webster, Freter and Storey 2004). A final advantage is that natural topography and hydrology pretty clearly define the core of the ancient Copan polity.

TIKAL

A recent example of settlement research that was both evolved and designed is our Tikal project (Webster et al. 2007). Previous work there by the University of Pennsylvania – particularly the Sustaining Area Project supervised by Dennis Puleston -- set a standard for Lowland Maya settlement survey (Puleston 1973).[1] Puleston's methods and the data he recorded, supplemented by household archaeology and test-pitting, were for many years *the* model for how to conduct such fieldwork, and his results long influenced perceptions of what a Classic Maya social landscape might look like and how the Maya made a living. With the exception of Anabel Ford's Tikal-Yaxha transect (Ford 1986), no large-scale settlement survey was done later at Tikal for almost 40 years. Such neglect is unfortunate because Tikal is an excellent region for this kind of work. Colonization of the northeastern Peten has been so recent and Tikal's landscape

[1] Puleston himself benefited, of course, from the pioneering Uaxactun surveys of the Wauchopes and from the Carr and Hazard (1961) map of the 16 sq km core of Tikal.

has been protected as a national park so that small sites are well-preserved and accessible on the surface (visibility aside). Nor have ancient settlement remains been as heavily affected by erosion or hydrological processes as at Copan. If Puleston revisited his survey landscape today he would find it almost unchanged. In sharp contrast, I observed major and disorienting transformations of the Copan archaeological landscape within a decade.

Puleston surveyed settlement on four survey transects radiating out from Tikal's monumental core about 10-11 km in each direction. Total coverage was about 25 sq km, of which 10.7 sq km was uninhabitable *bajo.* He recorded 2192 structures and from this sample, plus the previously-mapped 16 sq km zone of Central Tikal, he extrapolated a total of 13,948 structures for the "site of Tikal" -- the 120 sq km region bounded by a combination of earthworks and bajos (Puleston 1973: 229; Puleston and Callender 1967). Fry (2003) concurrently excavated 97 test excavations in Puleston's north and south transects. His sample amounts to about 4.5% of all mapped rural structures and about 0.7% of the projected structure total for the site. Haviland, Becker, and others carried out similar tests, along with household excavations, at sites in the central zone (none of these can really be characterized as "rural" -- see Haviland 2003: 113 for a list).

Our own 2003-2006 research evolved from Puleston's and was designed around specific issues:

1) How accurate was Puleston's mapping?
2) Were the earthworks as he described them, and could they have functioned as demographic and defensible territorial boundaries?
3) Would additional settlement survey and test-pitting confirm the work of Puleston and Fry?

We also carried out a soil/vegetation survey to the southeast of Tikal.[2]

Our most important goal was restudying the Tikal earthwork system interpreted By Puleston and Callender (1967) to be Early Classic defensive boundary defining Tikal's 120 sq km agricultural hinterland. Conventional aerial photos were not useful because of the dense forest, and we also lacked a detailed topographic map. We accordingly depended on modern GPS equipment and discovered that it worked well under the forest canopy. We remapped the earthwork and found Puleston's map to be extremely accurate. We surveyed a strip of land on either side of the earthwork and made block surveys to check and expand Puleston's settlement sample, and followed up with test-pits in the newly-recorded sites. We mapped approximately 113 groups with 474 structures over an area of 7 sq km. Most sites were small, and test-pitting yielded basic household assemblages of Late Classic (or later) date, but no signs of any specialized activities. Pre-Late Classic ceramics are extremely sparse. We concluded that the earthwork was not an effective fortification, that it was probably incomplete, and that it was much longer and differently laid out than Puleston thought. We found no drop-off in structure density along the "outer" side of the northern ditch as Puleston imagined. The earthwork probably did not function effectively as a boundary or territorial marker and had little influence on where people settled in the 8th century or later.

Pertinent at this point is that regional settlement research is usually problem-oriented only in the most general sense. It is often undertaken to fill in gaps in previous research (as in our work at Copan) or to address very general agendas – Puleston's agenda – at least as the Sustaining Area Project developed -- was the issue of how the ancient Maya supported apparently dense populations in

[2] This study enlisted Prof. Richard Terry of Brigham Young University as sub-contractor to the main project. Tim Murtha, along with Terry and his students, did the fieldwork.

Late Classic times. In part to address this question we retrieved 844 soil samples, now under study, from about 200 soil profiles in excavations at or near the earthworks and from the surrounding landscape. Puleston (1973) envisioned intensive ramon cultivation in the Tikal hinterland, but preliminary analysis of our soil samples shows stable carbon isotope enrichment consistent with the cultivation of C4 plants – probably maize.

Real or Potential Biases and Limitations: The most obvious limitation at Tikal is limited surface survey. Puleston's coverage is smaller than ours at Copan, but in a much larger potential survey universe. We added 7 sq km more, but Tikal's total coverage (including the 16 sq km of Central Tikal) still amounts to only about 77% of Copan's in *absolute* terms (48 sq km vs. 62 sq km). At Tikal we thus have a smaller sample of a larger inhabited landscape or polity. The problem that Puleston faced, as we did, is how to define limits to Tikal's "rural" or "intersite" area. Puleston thought (incorrectly) that the earthworks might solve this problem, but our work shows otherwise. The Tikal Sustaining Area surveys radiated out from Tikal's monumental core about 10-11 km. Copan surveys extended much farther from the Main Group – following the major river valleys about 19.3 km to the northeast , 8.4 km to the south, and about 16 km to the north. We lack a good grasp of what the overall population around Tikal was in a region of 400-500 sq km – a reasonable estimate of its core Late Classic sustaining area. Ford's work partly compensated for this limitation on Tikal's eastern periphery, but surveys should be extended to the south and the west. The latter direction is particularly important because there is a natural corridor leading in this direction to El Zotz and its neighbors (now being studied by Stephen Houston and his colleagues) and then to El Peru.

One glaring deficiency of Tikal settlement research is extremely limited rural test-excavation. Even when our own sample is added to Fry's there are only about 159 rural test excavations, a woefully inadequate number, and there are no extensively excavated outlying sites as at Copan. Comparing Tikal to Copan,

then, we have a much smaller and more imbalanced sample (both in terms of survey and excavation) of a much larger sociopolitical system and landscape.

Piedras Negras

Piedras Negras is well-known from the University of Pennsylvania work during the 1930s, but no surveys or household excavations were done outside the site core at that time. The inception of the joint US –Guatemalan Piedras Negras Archaeological Project led by Stephen Houston and Hector Escobédo offered the opportunity to begin to rectify this situation (Webster and Houston 2003). Between 1997 and 2000 a Penn State project examined the "near periphery" of Piedras Negras --- three survey blocks within maximal distance of about 2 km from the Acropolis and covering about 3 sq km. We also mapped and tested the secondary center of El Porvenir, about an hour's walk to the northeast, and recorded a number of sites along the path to that site.[3] Our survey was focused on narrow valleys that approached Piedras Negras from the north and the south.

Our project was one of pure design because there was no earlier research to build on. In essence it was a miniature version of our Copan project, carried out on a very different landscape and with much more limited resources. We combined surface survey with test excavation, followed up by extensive horizontal stripping of five outlying sites. Human presence has been minimal since ancient Maya times, so the archaeological landscape was quite undisturbed.

By 1997-1998 GPS equipment was available and we got decent signals under the forest, although often only with prolonged effort. Signals were still scrambled prior to 2000 so site locations are less precise than either at Copan or at Tikal.

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[3] Fieldwork was done by Jennifer Kirker, Amy Kovak, Tim Murtha, Zachary Nelson, and David Webster.

We had no accurate contour maps or aerial photos to help situate sites visually, nor was modern surveying equipment generally available. Only in 2000 did situational access to total stations allow us fix the locations of the fully excavated sites and make contour maps of adjacent topography.[4]

We recorded 89 sites with 254 identifiable structures, and got GPS fixes on 84 of them. None (except Porvenir) was more than a 25 minute walk from Piedras Negras. Sites were typically small and located on ridge tops (34%), on hillside spurs or artificial terraces (40%), or on valley-floor locations (26%). Configurations range from single, low structures to multi-plaza groups, but many lacked any formal plaza or courtyard arrangements. We also counted as sites small residential terraces that lacked house platforms. Only four sites were of elite scale -- the biggest one had at least 24 buildings and apparently was not known to the Penn project.

We placed 28 test excavations in 20 sites (a 22% sample). Most settlement spans the Yaxche and Chacalhaaz phases (625-825 AD), but some sites had earlier Naba/Balche phase (AD 350-625) material. Soil samples were routinely taken and examined for phosphate and carbon isotope concentrations by Prof. Richard Terry and his colleagues (Fernandez et.al 2005). Phosphate concentrations were usually low, indicating sparse midden build-up and refuse disposal at peripheral residences, and soil isotope signatures suggest maize cultivation.

We extensively excavated five rural sites ranging from a single mound to an impressive elite group.[5] All of these are plausibly part of a single neighborhood -- at least were arranged along the same access route. All sites produced

[4] Thanks to the efforts of Tim Murtha and Zachary Nelson.

[5] These excavations were supervised by Webster and Amy Kovack.

considerable occupational debris. These peripheral household excavations were supplemented by similar exposures at Piedras Negras proper (Nelson 2005).

Limitations of our research were limited scale and too close proximity to the Piedras Negras site core. We have no firm grasp of territorial bounding for the Piedras Negras kingdom at any time, apart from inscriptions that refer to smaller regional centers such as El Cayo, and some new insights into a probable political frontier with Yaxchilan (Golden et. al 2008).

ISSUES AND IMPLICATIONS

Thinking back over all these projects, what strikes me is the discrepancy between the view from the centers and that from the peripheries. Inscriptions and architectural stratigraphy at all three centers suggest much longer histories than the outlying settlement remains. Judging from the former, all of these kingdoms had active, vital capitals long before their hinterlands were heavily settled. We found no single-component sites outside the Copan pocket that had pure occupation predating the Coner phase (before AD 600-650), and even traces of early occupations were rare. Our Tikal sample is similarly late, but test-pitting is so minimal that its results are untrustworthy. The same is generally true for Piedras Negras. Settlement research in all these regions historically proceeded out from the site cores. If we had proceeded in the other direction, we would be astounded by the much greater time depth and complexity revealed at the centers.

These patterns convince me that early populations (pre-550-650 AD) were either very small and/or very concentrated. This seems true of Piedras Negras at all times; even at its 8th century apogee when it had *at the m*ost 2600 inhabitants, including elites (Nelson (2005: 124). We might add a similar number of people within a radius of 2-3 km on the Guatemalan side of the river. The core population of the kingdom was thus surprisingly small, but presumably able to provide most of the necessary subsistence resources, political support, and labor

for the polity as a whole. This modest estimate is agrees well with that for Late Classic Palenque – 4147-6220 people (Barnhardt 2007: 111). Those impressed by the architecture at these centers will insist that labor, food, and other resources must have been pulled in from dependencies at greater distances. Until proper surveys have identified them, however, this is just speculation, and in my opinion unnecessary given reconstructions of Maya labor capacity (Webster and Kirker 1995).

Based on John Wingard's soil and land-use model (Wingard1996; Webster 2005) I now think that the whole Copan river drainage in Honduras probably had *at most* 20,000 people in the mid-8th century.[6] This population was artificially concentrated in the Copan urban core and adjacent parts of the Copan pocket rather than being rationally distributed vis-à-vis its agricultural resources. Either kings and elites restricted where commoners settled, or else commoners were attracted to the capital zone. Piedras Negras (and Palenque) show the same pattern -- outlying populations were not big enough to feed themselves and the core. In both regions most farmers probably lived at or very near the royal centers, and some peripheral sites probably did double-duty as seasonal facilities for labor moving out from the respective site cores. If this inference is correct we have to deflate population estimates based on structure counts.

Our Copan sample amounted to 4553 mapped structures, so the structure/population ratio is not quite 1:4 (23:100). Two widely accepted estimates for the 120 sq km Tikal core at AD 700 are 45,000 (Haviland 2003) and 62,000 (Culbert et al. 1990). I personally doubt Puleston's projected total of almost 14,000 structures for this whole landscape, but if we accept it and apply the Copan ratio the result -- about 56,000 people -- falls squarely between

[6] In various publications we give maximal estimates of 26,000-28,000 people for Copan at AD 800. I now think that about 20,000 is more realistic.

Haviland's and Culbert's figures. Either estimate is believable, although I think both are probably too high.

Settlement reconstructions typically postulate large Late Classic populations. My research has made me rethink the issue of earlier (pre-A.D. 550-600) demographic patterns. We all know how impressive such centers look on the maps and photos we are accustomed to seeing, and we also know that they were much smaller places prior to Late Classic times. Nevertheless, I think we fail to scale back our demographic reconstructions in a consistent manner, which leads us to evaluate much early behavior and many important events against the backdrop of our population and landscape impressions for the 8th century.

We estimate roughly 5500 people for the whole kingdom at AD 550-600 (Webster, Freter, and Gonlin 2000: 161); Wingard's soil model yields almost exactly this figure. Population thus doubled just about twice to bring the total up to the 20,000 people living in the mature polity in AD 800 – a rate reasonable for a thriving Maya kingdom and well within those known for other pre-industrial populations. If we back down at some similar rate to AD 400 there were only a few thousand people in the whole valley. The founder K'inich Yax K'uk' Mo' played out his royal drama in a demographic context comparable to a poorly attended college basketball game.

Some archaeologists will object to such tiny numbers, but we cannot have it both ways. If Maya kingdoms thrived for centuries their populations must have increased significantly, either through intrinsic fertility or other kinds of recruitment. If we alternatively imagine that early populations were almost as large as their later ones we are faced with an absurdity – thriving kingdoms that were demographically stable, but whose centers and inhabited landscapes only achieved maximal size in Late Classic times.

With regard to Tikal, Culbert et al. (1990: 112) remark that " ….Early Classic population …… is unusually dependent on factors that are poorly controlled". This applies even more so to the Preclassic population. Nevertheless, they calculate relative frequencies of Early Classic occupations for central Tikal (33% of the AD 700 population) and the sustaining area (50%). If we apply these reductions to Haviland's AD 700 estimate (45,000) Tikal would have had somewhere in the range of 15,000-20,000 people at AD 550. Back up another 150 years and the number dwindles down to around 6000. Sihyaj K'ahk, the leader of the famous Teotihuacan-related entrada of AD 378 would thus have encountered a basketball-game size core polity. How many stalwart companions did he require to impose himself on this little kingdom with some internal help? A few score? We have to rethink the dynamics of many early political and warfare events if we accept such numbers.

What about political or territorial boundaries? The most tempting view is from the center outwards, but any conception of territoriality we attribute to kings must take into account that many other people had diverse interests in territorial definition – other kings, sub-royal elites, and ordinary people. Any Maya ruler no doubt aspired to a firm grip on some reasonably compact core hinterland within at least a couple of hour's walk of his dynastic seat. Sometimes topography dictated territorial logic – Copan's rulers must have seen the 24 sq km Copan pocket as an irreducibly strategic landscape. At Palenque the flat shelf of land on the escarpment was similarly strategic to Pakal's dynasty. If the emic conception of Tikal's earthworks dates to the mid-6th century, then Puleston's "artificial" 120 sq km hinterland makes sense for about 20,000 people (see Table 1). Beyond such "natural" limits I think territorial conceptions took a back seat to political considerations – the "kingdom" was wherever kings could go with impunity (and obviously Quirgua was a step too far for Copan's 13th ruler) or over which they

could exert some reasonable influence or acquire resources.[7] Examples of such political dynamics are easy to find – this pattern was traditionally true of the "kingdom" of Afghanistan in the 19th century, and still obtains there today.[8] To be sure, features that reflect political tensions and frontier facilities are being detected, as between Piedras Negras and Yaxchilan (Golden et. al 2008), but even these might have fluctuated with the vicissitudes of conflict or alliance. The viable territory was where most of the politically "attached" people were at any particular time. This is apparent at Copan and Piedras Negras, but not at Tikal.

By *attachment* I mean how the bulk of the population – common food producers – was connected to a political center, a dynastic line, or a set of sub-royal nobles. Hirth (2003) and Gutierrez (2003) suggest the widespread existence of a basic Mesoamerican political form that we know best from the Central Mexican *altepetl*. According to this model commoners and elites were bound together in a kind of ethnic polity within which they shared common political identity, recognized bonds of "legitimate" traditional leadership, and inhabited a compact "contiguous" territory (Hirth and Gutierrez recognize regional variants of this model, which can include more complex systems with non-contiguous territorial segments). Restall (1997) thinks the 16th century Maya equivalent was the *cah*, but there seems to be no Classic Maya term for such a polity.

I suspect that the Classic Maya departed from the Mexican model in that commoners were weakly attached to kings or nobles and that there was considerable potential for defection or detachment. The great burgeoning of

[7] We should also probably include here various places, most still mysterious, that were of mythic or ancestral importance to specific dynasties, as reflected in many of the toponyms that epigraphers increasingly identify.

[8] There is a macro-level intimation of this pattern in Classic geopolitics, in which lesser kings are "possessed" by "overkings" who sometimes lived considerable distances away.

Caracol detected by the Chases after Tikal's mid-6th century military defeat might reflect such population movements. I also suspect that the explosive growth (and abbreviated political career) of La Milpa, which followed immediately on Tikal's defeat of Calakmul in AD 695, is another case in point. Maya commoners often voted with their feet. If this conjecture is true, it would explain many of the regional demographic shifts so apparent going back to Late Preclassic times, and also many political vicissitudes. Such volatile attachment would be extremely variable – much more feasible at Tikal or Piedras Negras than at Copan, which existed on a more isolated political landscape.

But how did commoners produce food? This became Puleston's basic question in his Sustaining Area research. One thing evident from all our work is that there are few signs of agricultural intensification.[9] Particularly unexpected is the almost complete absence of agricultural terracing.[10] There are many residential terraces on all three landscapes, some of which could have supported limited houselot gardens, but nowhere are there features like those documented at Caracol. At Copan we only found terraces in one limited locale and they have not yet been investigated. Some patterns of land tenure might have discouraged farmers from making such investments, but their absence might also reflect comparatively low population densities.

On this score it is worthwhile to consider some density figures. I estimate that overall densities in the Late Classic Copan polity (the drainage) were on the order of 55 people per sq km and that densities on the valley's core cultivated landscape were about 134 per sq km (maintained for only a century or two).[11] In

[9] Neither the Copan nor the Piedras Negras landscapes have much potential for the intensive bajo cultivation envisioned by some archaeologist for the northeast Peten.

[10] Tikal has the least topographic relief, but terrace-like constructions were documented by Turner (1983) around Becan, where the topography is even flatter.

[11] By "overall" I simply mean people on the landscape, regardless of whether there were local concentrations.

Table 1 I have applied Haviland's and Culbert's Late Classic Tikal estimates and their reduction figures for earlier times to Puleston's 120 sq km hinterland. Overall and upland Late Classic densities are unconvincingly high. We have no soil/agricultural model for Tikal, nor have we general models that convincingly account for such numbers, especially on a landscape already cultivated for over 1000 years. These densities far exceed those reconstructed for other early civilizations (Trigger 2003) which include farmers cultivating rich alluvial soils in Egypt and Mesopotamia, and those for the Late Aztec period in the Basin of Mexico (the highest in Trigger's sample). Either the Tikal figures are too high or by AD 700 intersite areas far beyond the earthworks were cultivated to support her core population.[12]

Table 1

Time	Central Tikal	Sustaining Area	Total Pop.	Overall Density 120 sq km	Upland Density 120 sq km
AD 700	16,280	45,720	62,000	517	861
AD 700	11,700	33,300	45,000	375	625
AD 550	5372	22860	28,232	235	392

Even the scaled back 550 AD densities appear much too high. They far exceed the overall densities that Turner (1990) attributes to the Late Classic Maya Lowlands – 145 people per km^{2} ¨ and to the Early Classic (AD 300-600) -- 40-50 people per km^{2}. Puleston's solution was his ramon hypothesis, which has not been widely supported. I think the simplest solution is that we have routinely overestimated populations for most time periods. Parenthetically, Scarborough (1996, 2003) singles out water as another limiting resource at Tikal, and suggests that control of the large reservoirs at the site core conferred dry season

[12] The other possibility is that *bajo* cultivation was much more extensive and productive than present evidence seems to indicate.

political leverage on elites and kings. Puleston's survey, as well as our own, noted many peripheral reservoirs and aguadas, which tends to dilute this argument.

I expected that domestic assemblages would be quite similar at Copan and Piedras Negras because people had to do the same basic tasks. At Piedras Negras, though, I was struck by the differences from Copan. The paucity of obsidian blades was predictable, and they are also noticeably smaller than Copan tools (we also found very little obsidian in the Tikal household tests). People at Piedras Negras heavily depended on chert, from which they made a broad range of retouched tools. Copan rural sites yielded very weakly developed lithic technology – particularly scarce were bifacial tools of any kind. Effigy figurines of humans, supernaturals, and animals were very common at Piedras Negras, and very rare at Copan. Finally, if anyone imported basalt grinding stones (a la Rathje) almost none trickled down to Piedras Negras commoners, who used the most wretched local raw material imaginable for this purpose. Some of these differences are of course attributable to environmental or spatial factors – Copan is much closer to obsidian sources than Piedras Negras. Figurine use must reflect some culturally-based proclivities, and it is even possible, as archaeologists have often speculated, that rural commoners in the Copan Valley were ethnically non-Maya, or at least that they retained non-Maya household traditions.

In all of our large household exposures we searched for burials under benches and interior floors, and along wall lines. At both Copan and Piedras Negras burials were very scarce (25 in total) – and most sites lacked them entirely. Our standard expectation is that Maya cemeteries were few and that the dead were usually placed in and around household facilities. At both Piedras Negras and Copan most people instead were disposed of in some way that we do not understand --- a challenge to bioarchaeologists who hope to retrieve unbiased samples for skeletal analysis from household contexts.

SUMMARY

Thinking back over all these projects, what sobers me are the deficiencies of each, and how non-comparable they are in many essential respects, even when designed by the same researchers. As usual, the concept of “design” here hides the very real, on-the-ground decisions that were made as various episodes of fieldwork unfolded. Our block survey sample at Tikal grew in size, for example, as it became apparent that our GPS equipment allowed more rapid movement over the landscape and more accurate mapping of sites than anticipated. Similarly, the number and character of rural sites tested or excavated at both Copan and Piedras Negras was determined by survey results, by previous test-pitting, and by the pace of individual excavations. In such projects there is a powerful element of “rolling design”, with each phase of work contingent on earlier ones, and on available time and resources.

For me the most important methodological consideration is the *scale* of research – and by this I mean not the absolute amount of landscape surveyed or number of sites tested, but rather the relative scale with respect to the size and complexity of the system being investigated. Copan, by this standard, is very well understood and Tikal is not. Second, surface mapping must be supplemented not only by ambitious test-pitting, but by extensive lateral stripping of household remains. Lacking the latter, we cannot get a firm grasp of site functions, chronology, and character, all of which are essential to modeling demography and sociopolitical organization. Third, research projects should be multi-faceted so that independent lines of evidence are available. Obvious examples are the soil/agricultural simulation of Wingard and the obsidian hydration research by Freter that complement our settlement data at Copan.

I will not address the dreaded and controversial topic of the algorithms we devise to extrapolate population estimates from our settlement data, except to say that they are only as good as our samples and that archaeologists seldom take a

census perspective using comparative demographic and household figures, as we did for Copan (see Webster, Freter and Gonlin 2000: 153-160). Failure to do this leads to over-inflation of population.

Copan and Piedras Negras have made a demographic minimalist of me These two small polities have dynastic histories of almost exactly the same length and I would bet that each, at its height around AD 800, had similar populations of *no more* than 20,000 people – a scale that I would also extend to the Palenque kingdom. Tikal is a big polity, but I think its Late Classic sustaining population (in an area of *at least* 200-300 sq km) was *maximally* about 30-40,000 people, and for a very brief time. Although some archaeologists will regard my reductions as heretical, such demographic scale is no disrespect to the Maya – quite the contrary. They were clever and industrious enough to accomplish all that they did with far fewer people than we imagine.

I think our own overestimates have created a problem we have wrestled with for 50 years but that might not really exist – how did the Maya sustain so many people on their landscapes? The big advantage of my heresy is that it effectively eliminates this issue. At all times maize was the staple crop. While there were some forms of intensive agriculture, much of the landscape remained under rainfall maize (albeit short-fallow) cultivation and lacked major *landesque* investments even in the 8th century. Many such “improvements” in fact represent coping behavior, not Bosrupian innovations that raised overall carrying capacity (see Murtha 2002). A corollary proposition is that even with low populations and population densities the Maya were capable of creating and sustaining the institutional complexity we have documented and its associated programs of art and architecture.

Finally, I’m painfully aware of all the things we did not do, all the things we might have done better, all the things that must still be done, and all the shortcomings of the existing data, whether from surface mapping, test-pitting, or large-scale

excavation. Of the three projects Copan is by far the most complete and reliable, and Piedras Negras is just a beginning. The Tikal data are the least useful in many important respects, despite how influential they are in the literature. Given the environmental differences in the three regions and their very different culture histories it is, moreover, dangerous to generalize from Copan to the others. In fact this is a general problem with the many surveys that have been done in the Maya Lowlands – their methods and data bases (and the interpretations derived from them) are disparate enough so that one can pick and choose whichever one suits one's personal scholarly preconceptions. So long as we do this, we will be like the several blind persons all feeling different parts of the elephant, and we will never agree on many of the basic questions we face concerning the ancient Maya.

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