

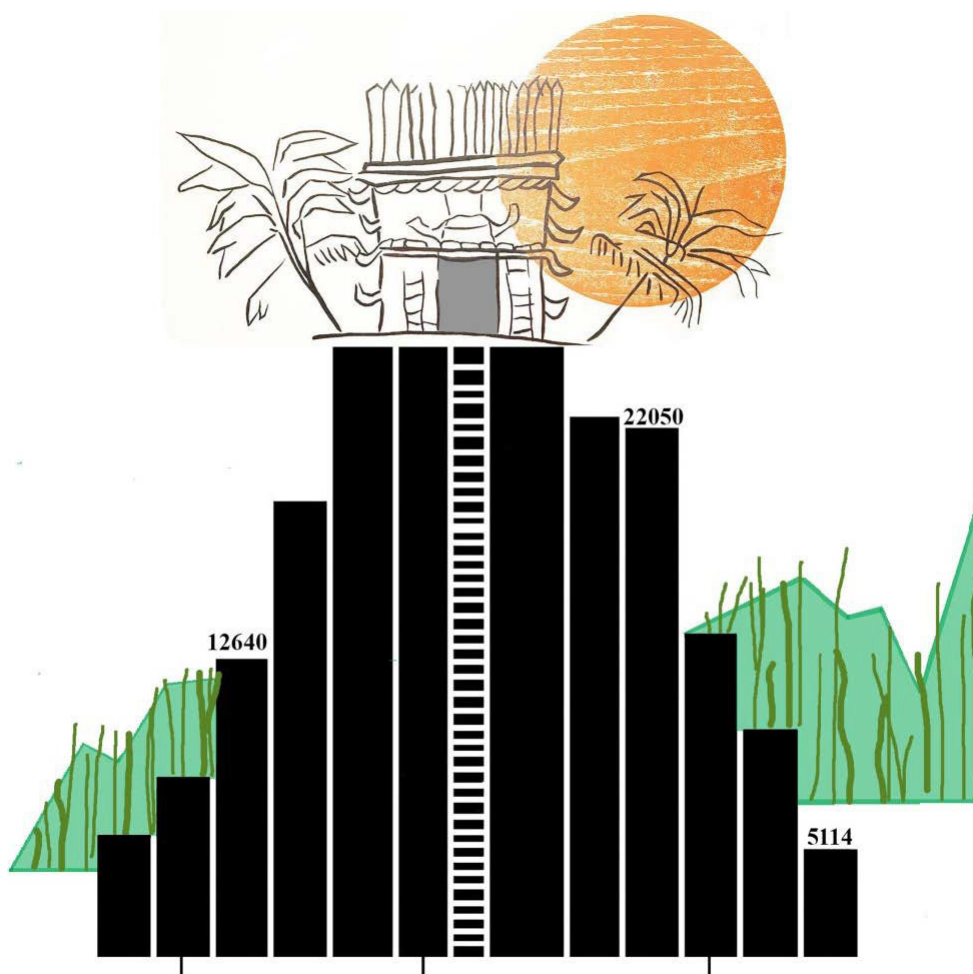
The Population of Tikal

Implications for Maya Demography

David Webster



PARIS MONOGRAPHS IN
AMERICAN ARCHAEOLOGY 49



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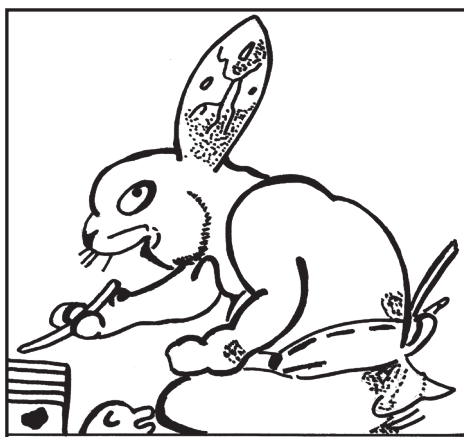
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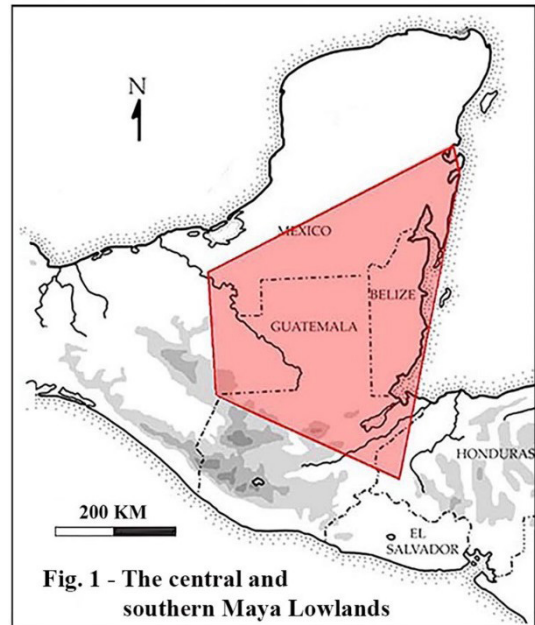
People believe unbelievable things because it's self-flattering to think that you are intellectually daring enough to accept what others find preposterous (Christopher Buckley 2003: 124).

INTRODUCTION

Tikal has long been central to our conceptions of Classic Maya demography, so I jumped at David Stuart's invitation to write an overview of its settlement and population for his 2014 symposium in Guatemala. Bad weather and hapless airlines precluded my participation, but the manuscript has since mushroomed into this much longer rumination, partly stimulated by my conviction that many Maya population estimates are overblown and sometimes preposterous. It consists of several main parts: a brief history and critique of population estimates (especially high ones), a section on the comparative demography of some ancient non-Maya civilizations, and finally a review of Tikal itself and the implications of research there for the demographic history of the central and southern Maya Lowlands. Three issues are conceptually separate. The first is the scale of the core populations that sustained Tikal and other Maya centers at their height

in the eighth century, the second is the distribution of people on their landscapes, and the third is how farmers made a living.¹ With regard to the first, there has long been agreement that the core supporting population of the mature Tikal kingdom was in the range of 45,000 to 62,000 people distributed over an area of about 120 sq. km. Because Tikal is widely identified as a Classic Maya "supersite" (Morley 1946: 318, Culbert 1973), this estimate, if plausible, provides a good benchmark for estimating populations on the wider Late Classic landscape, as well as those of earlier times. I am mainly concerned with the area shown in Fig. 1, which includes the Rio Bec/Chenes regions. B. L. Turner (1990) did an excellent review of three sub-regions in what he calls the central Maya zone, which pretty much equates with the territory discussed here, except that I include Coba in the north and Copan and Quirigua on the southeastern Maya frontier.²

That population size, density, and dynamics are essential to understanding the Maya was a common archaeological presumption of the 1960s and early 1970s. Research at that time usually focused on the Late Classic because important revelations about earlier periods, and especially the Preclassic, were still in the future, despite some promising starts at Uaxactun and Tikal. Since then, our knowledge of the early Maya has exploded, even as the pulse of large-scale settlement research has unfortunately waned and fallen out of fashion, with some notable exceptions in the Rio Bec/southern Campeche region (Šprajc 2014; Nondédéo *et al.* 2013). Literature on the pre-AD 700 Maya often fails to consider the demographic contexts of early processes and events, or presents them in perfunctory and unconvincing ways. I went through the recent overview volume *The Origins of Maya States* (Traxler and Sharer 2016) and found it paid practically no attention to population scale, although there was much talk of related issues such as monumental construction and intensive agriculture. As a more specific example, I have never seen a good analysis of Tikal's population during the famous intrusion of "foreigners" in AD 378, which is surely relevant to our understanding of these events. Reconstructing pre-Late Classic populations is



admittedly difficult. Below, I use Late Classic Tikal as a benchmark to extrapolate back in time, which leads to some unexpected conclusions, especially with respect to what some see as the first big Maya collapse at the end of the Late Preclassic.

Few things are as boring as Maya population estimates except the varied and often confusing algorithms we use to generate them. One young Mayanist of my acquaintance dismissed all such attempts because, he complained, one could generate any numbers one wanted. I do not subscribe to his gloomy perspective, but I will only touch lightly on methods, many of which date back to the University of Pennsylvania's Tikal project of the 1950s and 60s. Turner (1990) gives a good overview of methods, as do chapters in Ashmore (1981). Instead, I emphasize the population estimates made for Tikal during the last 50 years, the strengths and weaknesses of research there, especially in comparison with Copan, and my own conclusions about reasonable population scale at the polity's peak in the eighth century and during its Preclassic/Early Classic florescence. My review of all the Tikal material, along with our recent Penn State work at the site, has led me to rethink basic issues of Classic Maya demography. My colleagues and I recently finished an NSF- funded project focused on Tikal's landscape, soils, and hydrology.³ I am indebted to Timothy Murtha, director of our project, for some of the data discussed and displayed below. Parts of this paper derive from a chapter that Murtha and I published in a 2015 book on Tikal edited by David Lentz, Nicholas Dunning and Vernon Scarborough. I cite other parts of their book, but my paper was largely finished long before I read the final version. Human adaptation to a tropical forest environment is the basic theme of the 2015 volume, but only one chapter besides our own (Chapter 8 on agroforestry) pays much critical attention to population estimates.

Five major patterns in the demographic history of the central and southern Maya Lowlands require explanation:

- 1) The very long period of low population size and density extending from 2000 BC or earlier up through the Early Classic in most sub-regions.
- 2) The population dynamics of some sub-regions, such as the Mirador Uplands and the northeastern Peten, which experienced Preclassic peaks and then (apparently) sudden declines.
- 3) The major spurt of population growth that began around AD 500-550 and that lasted for several centuries.
- 4) The widespread demographic decline (or as some would have it collapse) that began around the eighth century, preceded by another possible decline at the end of the Preclassic.
- 5) The failure of Terminal or Postclassic populations to recover to anything like their eighth-ninth century peak.

Of these, the Terminal Classic collapse/decline phenomenon has consumed by far the most attention, and we are only beginning to understand Middle and Late Preclassic population dynamics. I discuss all of these patterns below, less to explain them than to place them in firmer demographic contexts. My main concerns are population size and density, and their relationship to food supply. These are only subsets of the wider field of human demography that includes age and sex structure, fertility and mortality regimes, relationships among kinship, inheritance, wealth, and fertility, demographic impacts on health and fitness consequences, and a host of others. I briefly touch on some of these subjects, but we know too little about the ancient Maya to analyze them in detail. Lurking behind what follows is the perennial anthropological debate about whether pre-modern humans effectively controlled fertility and population growth to sustain some reasonable balance with resources.

Demography is linked to several background issues and concepts. I discuss these in seven long appendices, essentially mini-essays, in order not to clutter up the main narrative. In many respects, these appendices, along with the numerous endnotes, are the heart of this paper and were the most satisfying parts to write. Population density is a slippery term. It is often used in the literature in varying, imprecise, and non-comparable ways. Readers are challenged to figure out what published estimates mean, and so sometimes were the archaeologists who purveyed them. Archaeologists usually generate Maya populations using data from small survey areas contiguous with impressive epicentral architecture, and it is often unclear to which parts of a landscape or political unit they refer. Recurrent problems are 1) surveys are sufficiently localized so that outlying populations attached to the densely-settled cores are poorly known, 2) extrapolations are often made carelessly from surveyed to un-surveyed regions, and 3) reconstructed densities are in excess of any plausible systems of staple food production.⁴ I try to be consistent by using the density definitions presented in Appendix A. Appendix B examines the proposition that large populations were necessary for Maya construction efforts. I review agricultural intensification in Appendix C, and Appendix D assesses the feasibility of supplying food to a kingdom such as Tikal should it experience local subsistence shortfalls. Appendix E addresses the thorny problems of agrarian land tenure, inheritance, and agency. Appendix F discusses sociopolitical implications of my population reconstructions, and the final Appendix G reviews the models of Thomas Malthus and Ester Boserup, whose purportedly disparate views have long framed theoretical perspectives on Maya demographic history.

The Classic Maya (and to an extent their Preclassic predecessors) are famous, or notorious, among ancient civilizations, as exemplars of two conflicting views of human adaptation. The first and oldest is that overpopulation and its associated anthropogenic landscape effects were major causes of the Classic Maya political and demographic “collapse.” The more recent view is that the Maya were canny tropical ecologists who managed their environments in ways that supported dense populations and still guaranteed resilience and sustainability.⁵ The first perspective is Malthusian (or neo-Malthusian) and the second reflects the alternative model of Ester Boserup (1965: 21). These seemingly opposing positions have in common the presumption of population growth, population pressure, and high population densities, and they resonate strongly with our current concerns about the human role in ecosystem change and resilience. Best that I stake out my own position right away, based on my experience at Becan, Copan, Piedras Negras and Tikal: *by overestimating Classic Maya populations, we have created problems that do not exist.* These include:

- 1) Why were Maya populations so large and dense compared to those of other early civilizations?
- 2) How did Late Classic landscapes support so many people?
- 3) What accounts for the huge population decline of the Late and Terminal Classic?

The big advantage of my heresy, as many colleagues will no doubt regard it, is that it eliminates or simplifies these demographic and agrarian issues. I believe that low populations for Tikal and other Classic kingdoms are still consistent with monumental construction, environmental deterioration and the Classic collapse. They also place the Maya more in line with what we know about other ancient civilizations. William Haviland, a veteran of the old Tikal project who shares my concerns about inflated population figures, kindly read drafts of this paper and provided many comments, some of which are included below. He also sent me his most recent (and very extensive) compilations of Tikal household data (Haviland 2014a and b, 2015). Although much of the Tikal information is now more than 50 years old, it is still relevant both for the Classic Maya and for their forebears.

Two caveats are necessary before going further: first, I have never seen the recent LiDAR imagery for Tikal and adjacent regions that is guarded like some old treasure map. What follows is a pre-LiDAR product that might need considerable change in the near future. I predict that the new LiDAR imagery for Tikal

and other regions, when widely available, will stimulate another pulse of even more unlikely population estimates. Second, much of analysis and data discussed below derives from Dennis Puleston's superb work at Tikal. Puleston's widow, Olga Stavrakis Puleston (2015), has edited and revised her husband's archives and notes, some previously unpublished, in a recent BAR volume. I did not have access to her update while writing this paper and none of its contents is included. Whatever new revelations derive from the BAR volume are in any case irrelevant to my review of the historical significance of the Tikal Sustaining Area Project.

A SHORT HISTORY OF MAYA DEMOGRAPHIC ESTIMATES AND THEIR IMPLICATIONS

Assertions of Classic Maya exceptionalism have a venerable history.⁶ The Maya were variously claimed to be the inventors of Mesoamerican writing, of complex calendars (particularly the Long Count), of innovations in art and architecture, and to have been inordinately peaceful. Archaeological research has long since deflated these claims and/or assigned these innovations to other cultures. One such extravagant assertion stubbornly persists, however: that the Maya achieved and maintained unprecedentedly high population densities on their tropical landscapes, especially during Late Classic times. These are often expressed as *overall densities*, either for the Lowlands as a whole or for large segments of them. Densities are usually said to peak during the eighth and early ninth centuries just before the collapse, even though by that time the foodscapes of the Maya Lowlands, anthropogenically constructed and inherited niches, had been utilized by farmers for at least 2000 years. A problem with overall densities and especially their correlate, the absolute numbers of people attached to particular polities, is that warfare was a conspicuous process on the Maya landscape by Late Classic times. Long-standing enmity and outright conflict resulted in the establishment of lightly used buffer zones, thus skewing carrying capacity estimates.⁷ We shall see below that the large corpus of Classic Maya toponyms is not much help in understanding population distributions and territorial scales.

The high density perspective partly derives from the fond conception, shared by many of my colleagues, that the Classic Maya effectively managed their agricultural resources to minimize risk and degradation and to ensure resilience in the face of uncontrollable stresses such as droughts, at least until the last really big ones hit them (see Ford 2008; McNeil, Burney and Burney 2009; Ford and Nigh 2014, 2015). Mayanists have struggled for decades to reconcile putatively high populations and ecological savvy with patchy evidence for agricultural intensification, with obvious signs of environmental wear and tear, and with the big fact of Classic Maya culture history, the dramatic political and demographic collapse that began in the eighth century. We are increasingly aware of earlier and more localized ones as early as Preclassic times – see Webster (2002), Houston (2007) and Estrada Belli (2011, 2016). Demographic dimensions of the Classic collapse (as opposed to earlier ones) have preoccupied Mayanists since the mid-nineteenth century.⁸

The widespread conviction that there were (or must have been) lots of Maya has several sources. One is the sheer volume of infrastructure -- temples, tombs, palaces, ball courts, *sacbes* and monuments -- that the Maya left behind in their site cores beginning in Middle Preclassic times. I call this the Big Stuff argument. Surely, so the logic goes, there had to be very large populations to supply the labor for these constructions. Remember that J.E.S. Thompson (1954) made excessive labor demands central to his conception of the collapse, although as we shall see shortly his population estimates were sober and reasonable. Franz Termer (1951: 101), anticipating my skeptical colleague, understood long ago how subjective extrapolation from architectural mass to population could be:

“It is assumed that such undertakings could have been carried out only by organized groups of multitudes of men. This line of reasoning leads to variable results in estimating former population density. It becomes a matter of the inclination of the particular scholar, as well as of his appreciation

for the imposing, highly artistic buildings of the Mayas, whether he populates these remnants of settlements with cities of hundreds or thousands or even sees the Maya territory crowded with millions of inhabitants.”

Such arguments have a long history: see for example the contending views of Ursula Cowgill (1962) and Ruben Reina (1967). More recently, William Fash and Robert Sharer (1991:180) objected that my colleagues and I underestimated the Middle Classic Copan population because our numbers were incompatible with the scale of early (AD 400-500) constructions on the Acropolis there (see Paine, Freter and Webster 1995 for a response).⁹ Similar assumptions persist, Mayanists being prone to impressionistic awe or outright misstatement about construction, especially that found at big centers such as Tikal. Years ago, Culbert (1973: 64) marveled that at Tikal “... immense sums of energy and resources were expended for religious structures and accouterments.” As I worked on this paper, a well-known Maya scholar (who shall remain nameless) wrote me that “... I have usually assumed a substantial working-class was necessary to support a large output of art, particularly architecture and public sculpture. This is why I have tended toward higher population estimates, just based on the sheer volume of man-hours required to build and decorate, say, the Acropolis of Tikal.”

Unfortunately, this supposed correlation between big structures and lots of people doesn't hold up. Long ago, Mayanists were fully aware of the monumental scales of Tikal, Copan, and other major centers, but until the 1950's most of them had no problem imagining that the necessary labor came from small, low-density, swidden-supported populations (see, for example, Hester 1954: 130). Moreover, since Termer's day, systematic experimental archaeology and related simulations (Abrams 1994; Webster and Kirker 1995; Wingard 2013) reveal that construction consumed much less time and energy than commonly imagined. Kirker and I used Tikal Temple 1 as a construction currency because W. Coe (1990) described it in admirable detail. This imposing building, with a mass of 18,260 m³, required the labor of 130 people working for three dry season months during seven successive years.

But what about the even bigger Maya efforts of the Preclassic? I defer detailed discussion of this Big Stuff to Appendix B, but I will give one example here. Michael Coe (2012: 22) says that the Late Preclassic Danta pyramid-temple complex at El Mirador has a mass of some 200,000 m³ “... far outstripping Egypt's Great Pyramid in bulk.”¹⁰ If his figure is roughly correct, Danta, to the contrary, is dwarfed by the Great Pyramid of Khufu, at about 2,760,000 m³ (Lehner 1997: 206).¹¹ Khufu's father Sneferu takes the prize though, having built four pyramids that together amount to some 3.7 million m³ of construction volume (Verner 2001: 586). His pyramids alone (never mind his other constructions) have 202 times the mass of Tikal Temple 1.¹² All monumental construction at Tikal built over more than a millennium is only a fraction of that generated by Sneferu in his single lifetime. So much for the big-buildings = big populations argument (see below for Egypt's population size).¹³ If anything, there are surprisingly few buildings on most Maya landscapes, even given the low population estimates I champion.¹⁴ Maya efforts were so modest in proportion to the available labor that I have long characterized them to my classes as “cheap signaling.”

A second reason for high estimates might be the general cross-cultural correlation between population size and density (in a region, polity, community, etc.) and complex socio-political organization and urbanism.¹⁵ This correlation is sometimes called the *size-complexity rule* (see Feinman 2013: 38).¹⁶ V. Gordon Childe, perhaps the most influential cultural evolutionist of the mid-20th century, stated baldly that, “... a certain size of settlement, and density of population, is [sic] an essential feature of civilizations” (Childe 1950: 4). He thought that the concepts of comparative urbanism and civilization were “... substantially reduced and impoverished by the inclusion of the Maya” (1950:9), stimulating in some Mayanists what I call “city-envy”. Other early Mayanists probably also chaffed at assertions that low population densities inhibited political centralization: “Slash and burn cultivation usually implies

a scattered population, a population unwilling to pay homage to a center of control” (Wolf 1959: 77). Milton Altschuler (1958:194) emphatically made the same point, as did Ester Boserup (1965: 70-71).¹⁷ In the absence of other kinds of evidence, large Maya demographic numbers by themselves could be made to imply complexity, contra Wolf, Altschuler and Boserup. Such assumed correlation was particularly tempting before we understood Classic inscriptions. My early professional engagement with the Maya occurred during this antediluvian epoch and I remember well the consequent arguments about Classic sociopolitical organization. There have been enormous revelations from epigraphy and iconography since 1980, not to mention from settlement and household archaeology, but much uncertainty remains. Those with a penchant for Maya hierarchy, centralization, and complexity can champion large populations as their presumed correlates or proxies: “Since the earliest comparative studies of global ancient civilizations, the presence of monumental buildings has consistently been a marker of complexity” (Doyle 2017: 27).

Contributing to early, freewheeling estimates for the southern Maya Lowlands was the lack of well-established indigenous populations throughout much of the region until very recently, especially in the Peten of northern Guatemala and adjacent parts of Belize, southern Campeche, Quintana Roo and Chiapas in Mexico. Population estimates for northern Yucatan based on ethnohistoric documents vary considerably, but at least have order-of-magnitude implications. Ralph Roys, the dean of Yucatec ethnohistorians, calculated that “... the population at the time of Spanish contact may well have been over 300,000” (Roys 1965: 661). Nancy Farriss (1984: 57) later advocated a much higher figure of 2.3 million, but only if one assumed some kinds of intensive cultivation in addition to maize swidden. Cook and Borah (1974: 39-40) exhaustively reviewed documentary records and put the population of northern Yucatan at about 800,000 or a bit more at Spanish contact. Sanders’s (1962: 91) synthesis of ethnohistoric sources put the Conquest period population of northern Yucatan at 535,000-592,000, and maximal overall density at 34 people per sq. km.¹⁸ I have always thought a population in the 600,000 to 800,000 range is most plausible (see Hogarth *et al.* 2017 for a discussion of Colonial populations).¹⁹ As we shall see shortly, demographic patterns associated with the living Maya in northern Yucatan served as a brake on extravagant claims derived from archaeological surveys.

Almost all pre-1965 Classic Maya population estimates were general and impressionistic, often based on opportunistic trail surveys (Uaxactun and Mayapan were exceptions).²⁰ Many result from back-of-the-envelope calculations of agricultural potential (carrying capacity), themselves heavily influenced by ethnographic observations. Ursula Cowgill (1962), for example, suggested the ancient carrying capacity of swidden agriculture in the Peten was 39-78 people per sq. km based on her agricultural studies, but bemoaned the lack of good settlement data that might provide an independent check on her calculations. Only a few such studies were then available to her, and archaeologists sometimes used them to assign population numbers to presumed house mounds in simplistic ways.²¹ One exception is the broader regional surveys of the Rio Bec region done by Carnegie Institution archaeologists in the 1930s (Ruppert and Dennison 1943). Their coverage was excellent, but their goal was finding and mapping big sites like Becan or Rio Bec rather than systematically recording small residences. During the 1950s, Carnegie archaeologists did record house forms at various scales at northern Maya sites (Ruppert and Smith 1957), but their data were highly descriptive and too sketchy to have demographic import. Here I highlight what I consider excessive estimates. Many fine summaries are available. Haviland (1963), Puleston (1973a), Willey *et al.* (1965: 7-14), and Ashmore (1981) all give overviews of pioneering fieldwork and interpretations, much of which was anecdotal or focused on individual small structures. Detailed descriptions of early work in the Peten are found in Rice and Puleston (1981) and Rice and Rice (2016). Rice and Culbert (1990) provide a synthesis of demographic estimates, along with a particularly useful set of comparative charts. Turner (1990) contributes a thorough and thoughtful summary of data, methods, and interpretations to their volume.

One early intimation of large populations came not from archaeology, but from *chiclero* operations in the early 1920s. Corporation field managers such as Paul Schufeldt cleared vast amounts of forest to grow maize to feed their men and mules. Schufeldt (1950: 226) observed that, "... these clearings, upon being burnt, showed every evidence that practically all the land had been occupied by small house sites and land under cultivation ..." Sylvanus Morley, the dominant Mayanist of the time, was initially confused by Schufeldt's report and dismissive of it. Soon Morley (1923) changed his mind, reporting to the Carnegie Institution that intersite areas between big Maya centers must have been virtually continuously occupied. Two years later, he enthused that "... it is not improbable that the southern Maya Lowlands was one of the most densely populated areas of its size in the world during the first five centuries of the Christian era..." (1925: 63). Unfortunately, no archaeologist at the time thought to take systematic advantage of Schufeldt's natural experiment in detecting house remains. A decade later, the Uaxactun surveys of the Ricketsons (1937) revealed higher than expected frequencies of such remains and reinforced Schufeldt's observations.²² The Uaxactun research impressed Morley and generated for the first time reasonably reliable, systematically obtained settlement data that offered a different approach from carrying capacity calculations. Maya demographic reconstructions ever since have derived from both kinds of information, often with disparate results (see Hayes 2013 for a recent effort).

What to make of the Uaxactun surveys depends on what one thinks the transects measured, because they only extended out for about 1.6 km in each direction from the site center. Did they measure an unusual concentration of people around Uaxactun's core, or was the roughly 2.25 sq. km transect sample part of the kingdom's agricultural hinterland? Morley (1946: 313-316) addressed this problem by assuming that Uaxactun's hinterland included all territory within a 16 km radius, and that a reasonable population for this whole area was 50,000 people (271 people per sq. km upland or productive density).²³ To correct for contemporaneity, he calculated upland densities around Uaxactun at 106 people per sq. km if 25% of structures were occupied simultaneously, and at 424 people per sq. km with 100% occupancy. He extrapolated the population of this small, local sample to the whole Classic Maya Lowlands, claiming 13,300,000 to 53,300,000 people, which I regard as truly preposterous estimate.²⁴ The low end of Morley's range yields an overall density of 53 people per sq. km that barely falls into the levels he and others envisioned for long-cycle swidden cultivation. The high-end density, 213 people per sq. km, is quite extraordinary. It not only exceeds swidden capacity, but it does not (or could not at the time) take account of unusable and uninhabitable land or actual population concentrations on the landscapes around large centers, which must have locally been much denser. Remember, though, that the then dominant "ceremonial center" perspective held that the cores of sites like Tikal or Copan lacked large numbers of permanent residents. Because Morley regarded Uaxactun as a second-rank "city", he guessed that "metropolises" such as Tikal or Copan might have had 200,000 attached farmers.²⁵ By 1946, Morley was apparently trying to accommodate the density implications of the observations by Schufeldt, the Ricketsons, and others, but still somehow retain his long-cycle swidden conception of Maya subsistence.

As usual, there was considerable disagreement and some scholars imagined much smaller populations and densities.²⁶ A decade after Morley championed high numbers, George Brainerd (1954: 78-79) calculated the *maximum* population of the whole Maya Lowlands at 5,000,000, with a corresponding overall density of about 20 people per sq. km – a reasonable figure that allowed him to retain the swidden interpretation.²⁷ In the same year, Joseph Hester wrote a very comprehensive if little-known dissertation on ancient Maya subsistence (Brainerd was on his committee). He calculated (Hester 1954: 130) that the Lowlands as a whole could have indefinitely supported 3,000,000 people who used extensive rain-fed agriculture, although he doubted that they ever reached this number. Around the same time, J. Eric S. Thompson (1953: 29) thought that all the Maya-speaking parts of Mesoamerica

had a maximum population at 800 AD of 3,000,000 people, thus putting the overall density of the Lowland sub-region on the order of 10 people per sq. km.²⁸ Termer (1951: 103-106) weighed in with a very low estimate of 800,000 people for the whole Lowlands, and an “Old Empire” total of 300,000 (6.6 people per sq. km). The latter is a distinct underestimate, but we shall see later that it is probably closer to the mark than Morley’s 13,300,000, although skewed in the opposite direction. Termer was also a hardheaded minimalist about the populations of Classic Maya ceremonial places (as he conceived them), imagining that 3,000-5,000 people inhabited them. Recent work at Palenque and Piedras Negras suggests just such magnitude (Liendo 2013; Liendo, López and Campiani 2014: 111); Nelson 2005, 2016). The important point is that amid the varied opinions about population scale, there were some skeptics concerning Maya multitudes, and some of their estimates turn out in hindsight to be highly plausible, as I argue below. Low estimates of Brainerd, Hester, Thompson and Termer were drowned out, however, by those of more influential Mayanists such as Morley. Although no one says it outright, I think the glamour of the Classic Maya collapse contributed to inflated estimates. If there were indeed even 13,300,000 Maya, much less 53,000,000, then the demographic decline of the eighth and ninth centuries was more massive and catastrophic, and its causes more mysterious, than if populations were smaller.²⁹ As we shall see, my alternative demographic perspective considerably diminishes this mystery.

What really complicated (but did not resolve) Maya demographic estimates was the burgeoning of field projects beginning in the mid-1950’s, particularly those that included systematic settlement surveys at Mayapan, the Belize Valley, Tikal, Dzibilchaltun, Becan, Coba, Sayil, Ceibal, Copan, Piedras Negras and several other sites.³⁰ In all of these cases, survey and mapping began with the monumental site cores and (in most cases) worked outward, although seldom far enough to get a good sample of distant settlement. In virtually all the surveys listed, with the exception of Copan, good density figures tend to apply to the central precincts, to nearby, adjacent settlement zones, or to narrow transects, and are insufficiently backed up by test-pitting and especially by extensive lateral excavation of small residences. Willey *et al.* (1965: 575-581) struggled to make population estimates from their pioneering Belize Valley sample and very sensibly realized that it was too localized to allow reliable extrapolation to a larger landscape.³¹ By the mid-1960’s, there were for the first time (excepting at Uaxactun) systematically mapped “rural” household remains from which to draw inferences. Review of all these projects would require a much longer paper. I will briefly discuss only two influential examples (both close contemporaries of the Tikal research) and their associated demographic reconstructions, which appear to me to be serious overestimates.³² Both, revealingly, come from the northern Maya Lowlands where household remains are highly visible on the surface. The main concern of this paper is the central and southern Lowlands, but surveys at Dzibilchaltun and Coba strongly influenced views of southern demographic patterns. Culbert and Rice (1990) provide additional case studies of the history of settlement surveys, including a good overview by B. L. Turner.

Dzibilchaltun

As early as the 1890s, Eric H. Thompson (1892) remarked on the numerous household remains evident around Labna, in northwestern Yucatan. A half-century later, mapping by the Carnegie Institution (1950-55) demonstrated the extremely high density of household structures within the walls of Mayapan (essentially a spot density – see Pollock *et al.* 1962).³³ These in turn stimulated the Middle American Research Institute’s work at Dzibilchaltun, which began shortly thereafter (see Kurjack [1974], Kurjack and Garza [1981], and Andrews IV and Andrews V [1980] for summaries). Although demographic implications of the Tikal surveys loom larger in the literature, the work at Dzibilchaltun was exactly contemporary and was very innovative and high profile. Because Dzibilchaltun is in the northern Maya Lowlands and lacks impressive monumental architecture comparable to that at “supersites” such as Tikal, it tends to get short shrift from archaeologists focused on the southern Maya and the related problem of the “collapse.” Despite its historical significance, for example, Dzibilchaltun is not included

in the 1990 demographic overview volume edited by Culbert and Rice. This is unfortunate because the MARI research resulted in some very extravagant and influential population estimates, as well as a household model later adopted by other archaeologists.

Mapping and associated excavations began at Dzibilchaltun in 1957 and continued until 1966. An arbitrary region of 19 sq. km was surveyed (out of an estimated 1000 sq. km for the sustaining area as a whole), and some 8398 individual structures were identified. Residential remains were highly visible on this landscape of shallow soils and low forest, so Uaxactun and Tikal-style transects were unnecessary.³⁴ Edward Kurjack (1974: 94) calculated an overall density of more than 2000 people per sq. km for the 19 sq. km zone after about AD 700. E. W. Andrews IV extrapolated 21,000 structures for a 50 sq. km zone, and claimed that it had a population in excess of 100,000 people. Imagine the implication of these numbers for the population of the whole 1000 sq. km sustaining area of the Dzibilchaltun “polity”! I still remember how skeptically other Mayanists regarded such estimates at the time, especially William T. Sanders. By 1981, Kurjack himself found his own estimates “exaggerated”, having been sobered by the low contemporary population densities on the surrounding 1970s landscape (40 people per sq. km, well within the limits of swidden agriculture).³⁵ He concluded that the mapped portion of Late Classic Dzibilchaltun had about 10,000-20,000 people (Kurjack and Garza 1981: 304; Andrews and Andrews (1980:17). Kurjack’s lower range still yields densities of 526-1052 people sq. km, and did not resolve the disparity between estimates derived from structures and those from observed agrarian capacity. Revealingly, however, Kurjack felt compelled to jettison his original structure-count estimates for ones that took more account of agrarian productive capacity, an early example of the tension between the two analytical approaches.³⁶

However convincing (or not) these figures are, Kurjack (1974) was among the first Mayanists (Haviland at Tikal was another), to intuit from a huge settlement sample that striking social distinctions were embodied by its residential architecture: “Buildings of any kind are a highly visible form of wealth, manifesting the status of the social groups that control them” (Kurjack 1974: 83).³⁷ This insight informed much later work, including that at Coba and the several surveys of the Copan Valley. Residential architecture aside, and even with the reduced estimates for Dzibilchaltun, the construction capacity of its population was absurdly under- used given the small scale of its various civic/ritual buildings, further undermining the big populations = big buildings perspective.

Coba

Following closely upon the Dzibilchaltun research, and clearly influenced by it, were surveys at Coba (1974- 1975) by William Folan and his colleagues. They mapped settlement features in 13 polygonal zones (total area = 21.3 sq. km) radiating out from the site core (Folan, Kinz and Fletcher 1983: 6-7). Zones were mainly delimited by adjacent causeways. The longest of these was about 5.5 km, but the surveys extended out much shorter distances in most directions, and only Zone 1 was intensively mapped. It is not clear to me how the archaeologists conceive of these survey areas. They variously refer to the site, or “city”, or “civic-ceremonial core” of Coba. They also refer to “inner and outer suburban zones”, with “middle-class” people mainly in the former. They identify a continuously occupied area of approximately 63-70 sq. km, almost all habitable, of which they surveyed about one-third (Folan, Kinz and Fletcher 1983: 51-54). They also envision a “Greater Coba” zone at least as large as the 120 sq. km zone at Tikal supposedly bounded by earthworks (see below).

To their credit, Folan and his colleagues considered a wide range of variables in their population simulations and made their assumptions clear. They concluded that the total population of the “city” (the 63-70 sq. km area) ranged between 40,000 and 60,000 people during its Late Classic peak (Folan, Kinz and Fletcher 1983: 207). Maximal overall densities thus range between 634 and 952 people per sq. km.

Although they characterize Coba as a “garden city”, it is not clear what the productive densities were, or what supported such a population.

Many other sites or regions could be discussed, but these two (setting Tikal aside for the moment), along with Mayapan and Uaxactun, were foundation studies of Maya settlement and resulted in some improbable demographic claims. Enthusiasm for high densities was firmly entrenched in the literature after 1970, prompted not only by new survey data, but also by various forms of agricultural intensification detected in some regions (Harrison and Turner 1978). There remained notable dissenters to the idea that there were large numbers of Maya. William Sanders (1973: 331-332) thought that overall density for the Lowlands as a whole was only about 20 people per sq. km, although he admitted concentrations as high as 200 per sq. km in some restricted areas. Nevertheless, by the 1990s, large populations were conventional wisdom for most archaeologists. New insights from inscriptions revealed unexpected dimensions of institutional complexity and seemed to bolster the extravagant density claims. B. L. Turner (1990) reviewed the accumulated literature and estimated the overall population density in a region of some 22,715 sq. km of the southern Lowlands at 145 people per sq. km by AD 800.

High population estimates were heralded as products of the “New Archaeology” by Jeremy Sabloff (1990), who concluded that “... the new Tikal settlement surveys indicated densities on the order of over 600 per sq. km” (Sabloff 1990: 79). A year later, in one of a series of overview articles about the Maya, Don Rice wrote that the carrying capacity of “full-fallow” swidden maize agriculture reached saturation about 300 AD at levels of 150-250 persons per sq. m (60-97 per sq. km). Then, “By the end of the Late Classic period (the ninth century) when many great centers had been formed, the population had reached densities as high as 2600 persons per square mile in the centers, and between 500-1300 persons per square mile in the more rural areas” (Rice 1991: 12).³⁸ As we shall see later, Rice’s estimate for site cores (1004 people per sq. km) is modest for some centers, but his rural estimate (193-502 per sq. km) is implausibly high. Rice might have very different opinions today, but his figures, along with Sabloff’s and others, are still cited, often in garbled fashion. Oglesby *et al.* (2010: 1) cite Rice this way: “At the time of their collapse, the Maya had attained one of the highest population densities in human history with 6700 people per square kilometer in the center and 1300-3400 per square kilometer in the more rural areas. This population density is today rivaled only by China and Java...” Such hyperbole, in this case enormously bloated by incorrect conversion of Rice’s English measures to their metric equivalents, remains commonplace.

Table 1 lists some of the density figures discussed above, as well as others that I will not review in detail. I will come back to them after discussing estimates from several other ancient civilizations, but in the meantime note the high numbers – almost all are well above 200 people per sq. km.³⁹ These estimates are keyed into the comparative histogram shown in Fig. 2 below.

COMPARATIVE DEMOGRAPHIC ESTIMATES FOR OTHER CIVILIZATIONS

I have a simple rule of thumb about scholarly interpretations. If you assert something that falls well outside our expectations from other sets of data, your job is to convince the rest of us. My expectations about the Maya derive heavily from my own fieldwork, but also from comparative anthropological and archaeological perspectives on other cultures that I know something about. Above, I called many Classic Maya estimates unprecedentedly high, and several brief non-Maya examples will show what I mean.⁴⁰

What follows is partly an update of a set of comparisons made by economic geographers (Whitmore *et al.* 1990), supplemented by recent information and some additional examples. I regard their article as one of the most sensible and elegant comparisons of long-term population dynamics in ancient civilizations. They based their analysis on published literature and were primarily concerned with relative patterns

Table 1: A sample of Maya density estimates.*

Source	Zone/region	Density/sq. km
A. Thompson 1954	Maya Lowlands	10
B. Brainerd 1954	Maya Lowlands	20
C. Sanders 1973	Maya Lowlands	20
D. Schwartz 1990	Dept. of Peten	28
E. Cowgill 1962	Peten	39-78
F. Turner 1990	22715 sq. km of Maya Lowlands	145
G. Morley 1946	Maya Lowlands	53-213
H. Rice and Rice 1978, Rice 1990	Sacnab-Yaxha	221
I. Bullard (cited in Rice and Puleston 1981: 144)	Dos Aguadas	222
J. Morley 1946	Uaxactun realm	271(upland)
K. Puleston 1973: 230	NE Peten intersite zones	200-300
L. Hammond 1975: 88-92	3.1 sq. km around Lubaantun center	200-400
M. O'Mansky and Dunning 2004	Petexbatun intersite areas	496
N. Chase and Chase 2015	200 sq. km of Caracol realm.	500
O. Rice 1991	Maya generally	193-502
P. Sabloff 1990	Tikal hinterland	600
Q. Adams 1981	Central Yucatan and SE Campeche	450-690
R. Chase and Chase 1998	Caracol realm	847
S. Folan et al. 1974	Coba	634-952
T. Kurjack and Garza 1981	Dzibilchaltun	526-1052
U. Chase and Chase 1996	Caracol realm	1846
V. Andrews IV 1981	Dzibilchaltun	2000+

*Spot densities for site cores at Mayapan, Copan, Palenque, Piedras Negras and elsewhere are not included.

of demographic scale and change, rather than the respective plausibility of the several quantitative examples they used. When I first read their article, I was surprised that they did not comment more about how the Classic Maya stood out as an anomaly among their sample regions. As will become clear later, I think they vastly overestimated the number of Maya on the Late Classic landscape.

I chose the examples discussed below because: 1) Whitmore *et al.* (1990) used several of them; 2) each represents a climax period of complexity and infrastructure development in its respective part of the world; 3) most are in non-tropical environments and thus less vulnerable to deforestation, erosion, nutrient depletion and other frailties than the Maya Lowlands; 4) each region had effective long distance bulk transport using animals and/or watercraft, and 5) each of the intervals examined was preceded by many centuries of previous human agricultural exploitation, as in the Classic Maya Lowlands, so landscapes were anthropogenically constructed niches.⁴¹ None of these regions is characterized by the thin tropical soils characteristic of the Lowlands that are so susceptible to erosion and leaching. Each also differs from the interior Maya Lowlands in that water is not a serious limiting factor for drinking, household use or for agriculture. Droughts caused situational agrarian shortfalls, but irrigation provided reasonable buffering in most areas. By contrast, much of the interior Maya environment, especially around Tikal, is a seasonal desert with no substantial streams, annual rainfall is quite variable, and irrigation was unknown or very limited.⁴²

Egypt

The Nile Valley was fabled in ancient Greek times as the most bountiful of agricultural landscapes, watered by regular floods, and enriched by annual deposits of fertile silt. Then there was the river itself, enormously productive of fish, waterfowl, and other resources, and a great conveyor belt for the

movement of people and cheap, bulk goods. Large parts of the valley provided pasture for domestic animals, especially cattle, which formed an important adjunct to staple grains, as did many kinds of garden and orchard crops. The valley was a very risky environment because uncontrollable high or low floods periodically caused desolation, starvation and political upheaval. Offsetting such risks was the landscape's relative immunity to deleterious anthropogenic degradation such as deforestation, erosion, soil exhaustion, and salinization. Because of its tight environmental and social circumscription – almost like an island – the Nile Valley serves as a revealing test case of population scale and dynamics. I focus on the interval of 2600-2500 BC because it is squarely in the middle of the Old Kingdom, the first mature phase of Egyptian civilization when the “Great” (and other) pyramid complexes were built in a century or so (see previous comments on construction scale).

The Nile delta and Upper Egypt (south of the delta to Aswan) together have roughly 32,000-34,000 sq. km of potentially cultivable land, some two-thirds of which is in the delta (Baines and Malek 1989: 16; Hassan 1993: 165; 2001: 18; Wenke 1997: 36). The annual flood watered about half of this landscape. Butzer (1976: 83) estimates that 17,000 sq. km were cultivable in 2500 BC, and supported a population of 1.6 million people.⁴³ Overall density works out to about 47 people per sq. km, and productive density to 94 per sq. km. By Roman times, when many more documents are available and more land had been put under cultivation, the population was probably in the 4 to 5 million range, somewhat larger than when Napoleon invaded Egypt in 1798 (about 3.9 million; see Bagnall and Frier 1994: 56; Whitmore *et al.* 1990: 31). At the turn of the 20th century, just before World War I, it had risen to about nine million, largely because of modern agrarian and infrastructural reforms including the early Aswan dams. Today the Nile Valley is among the most densely settled parts of the world. Its population is approaching 90 million (roughly 2500 people per sq. km) and is hugely dependent on external economic subsidies.

One lesson is that although Egypt's population had repeated intervals of waxing and waning, there were, until the mid-nineteenth century, distinct constraints to its size, probably related to uncontrollable agricultural risks that sharply limited sustained demographic growth over the long run. A second lesson, more pertinent here, is that a centralized sociopolitical system of great complexity and capable of infrastructure investments that dwarf those of the Maya was based on modest demographic scale and density.

Mesopotamia (Sumer)

Southern Mesopotamia (Sumer) developed impressive urban-centered polities on a deep-soiled, irrigated alluvial floodplain after about 4000 BC. Gardens and orchards surrounded settlements perched on rich levee lands, and extensive marshes provided fish, waterfowl, wild pigs, reeds, and bitumen, and were probably used for flood-recession agriculture (Pournelle and Algaze 2014). Water meadows and marshes were pasture for domestic animals, most importantly cattle, along with goats and sheep. Rainfall was too low for predictable dry-field grain production, but it produced sufficient browse away from the wetlands to support a component of specialized pastoralists. The biggest risk factors in the short run were uncontrollable flooding and the shifting of river courses, and in the long-run salinization of the soil, claimed to be the principal dimension of deleterious human niche construction for the region (but see Powell 1985 for a critique). Documents begin to record aspects of the Sumerian economy just before 3000 BC, although surprisingly little about the basic institutions that supported it. Based on his extensive surveys, Robert McC. Adams (1981: 90) concluded that during Early Dynastic times (3000-2350 BC) productive densities on the northern and southern alluvium were about 26 people per sq. km of arable land. Whitmore *et al.* (1990: 27-30), relying heavily on Adams's work, calculated carrying capacities for the southern alluvium of the Tigris/Euphrates, a region of some 55,260 sq. km. They reconstructed low overall population densities of less than 11 people per sq. km between 4100 and 1900 BC, and a population peak of 630,000 at the end of the Ur III period around 1900 BC, when overall densities rose to 11.4 people per sq. km.

Recently Piotr Steinkeller (2007) gives a much more detailed calculation for the Ur III period (ca. 2100-2000 BC). Ur III developed a complex and centralized bureaucracy and its texts provide good economic and settlement data for a few sub-regions of its vast empire (for a good overview of bureaucratic complexity, see Englund 1991). Steinkeller analyzed the Umma region, whose boundaries are well known and for which there are extraordinary textual resources (15,000 archive tablets covering 40 years). Umma and its associated communities occupied a territory of roughly 2000 sq. km. This whole district had about 100,000 inhabitants, with some 20,000 to 25,000 in Umma itself.⁴⁴ Overall population density works out to 50 people per sq. km, considerably higher than what Whitmore *et al.* (1990) envisioned. Steinkeller takes pains to point out that Ur III was an interval of unprecedentedly high population. The impressive Mesopotamian developments of the previous 1500 years, including the first cities, emerged under the more modest overall densities reconstructed by Adams (1981: 90), on the order of 10-20 people per sq. km.⁴⁵

The Basin of Mexico

The Basin of Mexico and its northern fringe regions were the location of three successive urban-centered societies – Teotihuacan, Tula, and Tenochtitlan.⁴⁶ In AD 1519, the Basin was arguably the most densely populated large region in the New World, with the possible exception of the Andean heartland of the Inka empire around Cuzco (see Marquez and Storey 2017: 188-194 for the most recent demographic overview). Its inhabitants practiced a complex mix of rainfall and irrigation agriculture, and made landesque improvements such as terracing. Soils developed on rich volcanic and lacustrine deposits, and the Basin's shallow lake was the source of many resources, including maize and other crops grown after about AD 1450 on the celebrated *chinampa* fields of its western and southern zones. Water transport was well developed, but otherwise the paleotechnic tradition of the Basin, which lacked effective metal tools and large animal energy sources, more closely resembled that of the Maya than it did either Egypt's or Mesopotamia's.

William T. Sanders and his colleagues calculated that at no time prior to the final 500 years of the Basin's Precolumbian history was the population more than 250,000 people (Sanders, Santley and Parsons 1979: 219, 378). Overall densities never exceeded 65 people per sq. km even during Teotihuacan's florescence, and usually were in the 30-50 range. This latter calculation excludes the urban population of Teotihuacan and its immediate environs. If this population is figured in overall densities might about double.⁴⁷

But how many people lived in the Basin in AD 1519 when population peaked? A long-standing estimate has been 1,200,000 people (Whitmore *et al.* 1990: 33), a downward revision of earlier estimates of around 1,500,000 people by Sanders (1972: 116). Central Mexican specialists today often give very general figures. For example, Deborah Nichols (2013: 49) simply says "... more than one million people ...", while Marquez and Storey basically accept the Sanders-Whitmore figures. Specific estimates depend partly on how large one considers the core region to be. Sanders used a 6000 sq. km area. I put the area at 7700 sq. km, including much of its northern fringe and the lake itself, which covered roughly 1000 sq. km.⁴⁸ In 1979, Sanders and his colleagues calculated a population for AD 1519 of between 800,000-1,100,000 people depending on what method they used (Sanders, Santley and Parsons 1979: 378). Their preferred overall density was 200 people per sq. km (Sanders, Santley and Parsons 1979: 219).

Almost every year until his death in 2008, I debriefed Sanders about the Basin's population, which he constantly refined using a mix of archaeological, ethnohistoric and ethnographic evidence. During our last discussion, he put *overall* densities (including the lake and the Basin's northern fringe, which roughly equates to my 7700 sq. km) at about 156 people per sq. km.⁴⁹ I personally think this figure is a bit too high, as does Susan Evans, who, like Nichols, is an Aztec specialist who got her PhD with Sanders.⁵⁰ Caught as I am amongst colleagues, I use an AD 1519 overall density range of 100-150 people per sq. km

– this at the time when northern Italy, the richest and most populous region of Europe, had a density of just 35 people per sq. km (Livi-Bacchi 1997: 58). The Basin's population was heavily concentrated around the southern lakes where productive densities were on the order of 500 people per sq. km in some areas (see chart in Sanders, Santley and Parsons 1979: 379).⁵¹ Something dramatic obviously happened to elevate carrying capacity during the last few centuries before the Spaniards arrived. A major factor was the infrastructural initiatives begun in the mid-fifteenth century to manage water levels in the southern and western lakes. Although the main intent was to control the flooding of Tenochtitlan, a spin-off consequence rendered much of the southern lake system suitable for expanded *chinampa* cultivation.⁵²

Bruce Trigger (2003: 307), in his rankings of the agrarian systems of early civilizations (which include Egypt and Sumer), listed the Basin of Mexico as the most intensively farmed region of them all.⁵³ Worth remembering is that if the above figures are correct, the Basin had roughly the same population as Old Kingdom Egypt on a landscape about one-fifth the area. Trigger unexpectedly ranked the Classic Maya second, not because he (or we) understands Maya subsistence practices very well, but largely, I think, because he bought into the inflated estimates that are so common in the literature. Some Mayanists would clearly rank them first, given the numbers listed above and others that we will shortly consider for Tikal.

Hawai'i

Hawai'i is often not rated as an ancient civilization or state in the same league as Egypt, Mesopotamia, Central Mexico or the Maya, and so might seem inappropriate here. I have long thought, to the contrary, that comparisons between the Maya and the complex societies of Polynesia have much to tell us (Webster 1998) and that Hawai'i, just before the arrival of Europeans in 1778, is particularly pertinent. Whether Hawai'i was very state-like has long been debated (I have my doubts about the Maya themselves in this respect – see below).⁵⁴ Many anthropologists have long considered it an archaic or proto-state because impressive internal processes of cultural evolution led to striking political centralization and stratification (e.g. Seaton 1978). In fact, Hawai'i figures in much ancient state literature that conspicuously omits the Maya (e.g., Claessen and Skalnik 1978). Two recent summaries (Kirch 2011, 2012) strongly assert that Hawai'i ranks with other archaic or proto-states. I agree, although European contact certainly accelerated the emergence of state-like institutions in Hawai'i and elsewhere in Polynesia (Sahlins 1963: 288). I suspect that by the late eighteenth century, if not before, Hawai'ian polities were as complex as those of the Classic Maya and established on much the same demographic scale.⁵⁵ Hawai'ian monumental constructions, although not as famous as Maya buildings, hold their own in scale as well. For example, one major complex on Maui required about 127,000 person-days of labor (Kirch 2012: 206) compared to 90,000 for Tikal Temple 1.

Hawai'i also serves as a good analogue because its people had broadly the same paleotechnic limitations as the Maya, the exception being the use of very sophisticated watercraft for coastal communication, transport and war. The islands are also better documented in terms of archaeology, ethnohistory and paleoecology than any of the examples considered so far. All this information has contributed to very sophisticated systems modeling (Kirch 2011).

Migrants from the Marquesas might have made the initial landfalls on the archipelago (area =16,729 sq. km) as early as the seventh century. The first well-attested arrivals came later from Tahiti in what are called the Long Voyages, sometime between AD 1000 and 1300 AD (Moore 2015: 5-10). When Europeans arrived in 1778, the population had swollen in distinct pulses to an estimated 450,000 (Kirch 2011: 17).⁵⁶ Overall density at the time of contact was thus about 20 people per sq. km. Fertile soils and sub-tropical climate were well suited to the imported Polynesian complex of plants and animals: sweet

potatoes, taro, yams, bananas, coconuts, breadfruit, sugar cane, pigs, dogs, and chickens.⁵⁷ Terrestrial wild fauna were sparse, but there were abundant marine resources – fish, shellfish, seabirds and sea mammals. Hawai’ans built landesque investments of many kinds, both to grow crops and as fishponds. Vast expanses of the islands were agro-engineered to extend and stabilize food production. Permanent and intermittent streams on the geologically older islands of O’ahu and Kaua’i fed irrigated pond-fields especially suitable for growing taro. Extensive leeward parts of the younger islands of Maui and Hawai’i were devoted to rainfall or rain-fed irrigation for yams and sweet potatoes. Only Hawai’i among our comparative examples has an agrarian economy based on staple root crops rather than seed crops, so yields per hectare were predictably higher (Kirch 2011: Fig. 3-1). Productive densities in favored zones with permanent irrigation eventually reached 150-250 people per sq. km (Kirch 2011: 17), placing Hawai’i squarely in line with the Basin of Mexico.

Europe and Haiti

Two more brief examples round out our comparisons as summarized in Table 2 and Fig. 2. The first is Western Europe in the mid-eighteenth century, by which time written records are much more informative than anything we have for the first four cases considered.⁵⁸ The historical demographer Massimo Livi-Bacci (1997: 27) reports that overall densities of France, England, Italy, Germany and the Low Countries were then roughly 40-60 people per sq. km.⁵⁹ Finally, modern Haiti is one of the most heavily populated countries in the western world, with an overall density, before the 2010 earthquake, of about 324 people per sq. km (see Webster 2014 for comparisons with the Maya). Its population, like Egypt’s, depends heavily on external subsidies because it far exceeds the degraded landscape’s agrarian potential.

Table 2: Summary of comparative densities.

Region	Overall density/sq. km	Productive density/sq. km in most favored zones
Egypt – 2500 BC	47	94
Mesopotamia 2100 BC	50	-----
Basin of Mexico AD 1519	100-150	500+
Hawai’i AD 1777	20	150-250
Western Europe AD 1750	40-60	-----
Haiti 2010	324	-----

With the exceptions of Europe and Haiti, all the estimates in Table 2, like those for the Maya, are unsupported by census data or other kinds of standard demographic information. Mayanists devoted to the “any numbers one wants” perspective can certainly dismiss or ignore them, but by this same logic one can simply ignore the high population numbers claimed for the Maya as well, which gets us nowhere. The comparative estimates (with the exceptions of Haiti and Europe) are certainly prone to significant error and subjective judgment, or one might say informed imagination. Each region therefore has a range of figures given by various scholars.⁶⁰ Nevertheless, there has long been considerable agreement among the experts, and in each case the high estimates are quite modest. Archaeologists and historians working in regions like Sumer and Egypt have not yielded to the temptation to imagine the big density numbers many Mayanists use, despite the productivity of their landscapes and the impressive constructions found on them. Such consistent conservatism seems incompatible with a mere numbers game.

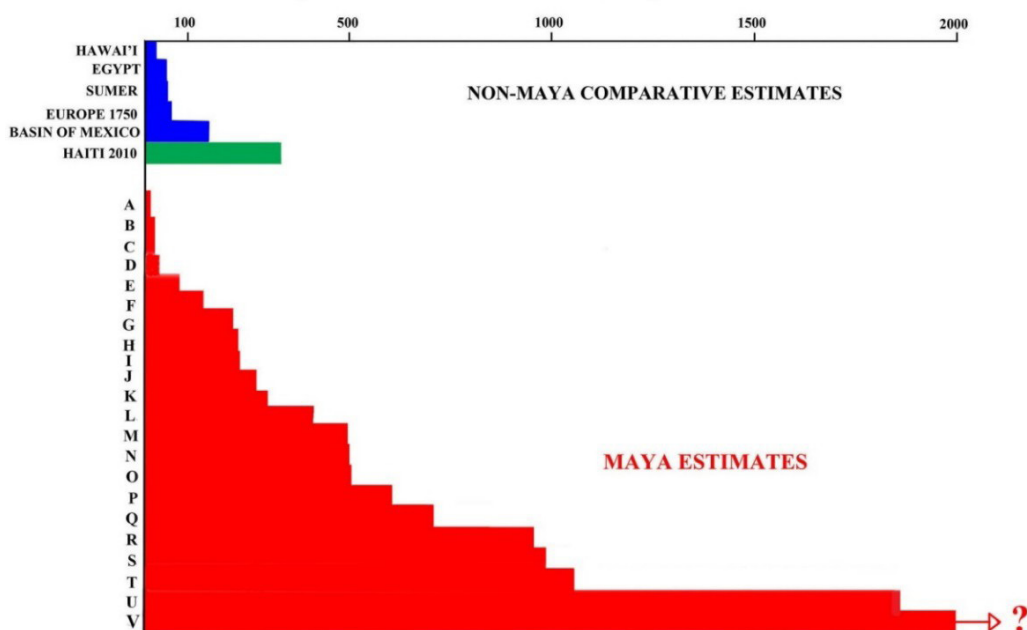
I could add many other comparative examples, but my point has hopefully been made. Fig. 2 shows that density estimates for large regions of the Classic Maya Lowlands are frequently far in excess of those for other parts of the preindustrial world, even those much more attractive and stable for agrarian exploitation, and where huge infrastructural investments were made. All the Maya estimates listed in

Table 1 are larger even than that for the Basin of Mexico except for Sanders's and the low end of Morley's range.⁶¹ I find the high Maya estimates without precedent and unconvincing in comparative perspective. We shall shortly see that Tikal archaeologists are no slouches themselves in their enthusiasm for high densities.

I realize, as do all anthropologists, that population size and density are only loosely correlated with sociocultural complexity. In his recent review, Gary Feinman (2013: 47) remarks: "In human groupings, scale is related to societal complexity, but not in a strictly finite or mechanical way." Broad cross-cultural correlations certainly become messy when we refocus on specific cases. For example, the clan-based Enga and Chimbu of New Guinea had overall contact-period densities of 44-112 people per sq. km (Roscoe 2013: 69), a range that compares nicely with those cited above for Egypt, Sumer, the Basin of Mexico and Europe, all of which were institutionally much more complex and hierarchical.⁶² Anyone devoted to Classic Maya exceptionalism can certainly argue that the Maya were the exception that proves the general rule. I disagree with this perspective, especially given the rich agrarian landscapes associated with the foregoing examples. One can also reverse the argument. Sanders (1973: 344) noted years ago that low-density populations similar to those he envisioned for the Late Classic Maya supported some impressive African states.⁶³

Alert readers will notice the apples/oranges quality of the comparisons shown in Fig. 2. All the non-Maya examples are true overall estimates that apply to regions of thousands of sq. km. Some of the Maya estimates refer to the Lowlands as a whole, or large sections of them, but others are for smaller survey zones. We already saw, however, that there is a strong tendency to extrapolate high survey zone figures to much larger areas as Morley did using the Uaxactun data. Another example, as we shall soon see, is the Tikal survey, which simulated population for a much larger but poorly known zone of 12,600 sq. km. B. L. Turner's (1990) estimate for three regions of the southern Lowlands cited above encompasses some 22,715 sq. km, almost 70% the size of the whole Nile Valley and three times the size of the Basin of Mexico. Puleston envisioned densities of 200-300 people per sq. km for very large intersite areas of the northeastern Peten, based on his limited surveys at Tikal.

Fig. 2: Overall population density comparisons. Graph of Maya estimates derived from numbers listed in Table 1.



For any Maya site or region, there is usually some point when reconstructed populations peak, and these are what I show in Table 1 and Fig. 2. Peaks commonly occurred in the eighth or ninth centuries, and lasted briefly before populations dropped to very low levels, or are said to disappear entirely. Admittedly, there are many exceptions. The peak at El Mirador might have been at AD 100 or 150 and at Caracol at AD 650. Nevertheless, the general pattern still holds pretty well and contrasts with other world regions discussed above. Egypt experienced numerous short-term fluctuations, but for almost 5000 years, the population of the Nile Valley hovered between about 1.5 and 4.5 million people. The Basin of Mexico experienced a slow, if somewhat fitful, increase in overall population from about 1500 BC to the sixteenth century, with a big pulse at the end. So far as I can see, the Maya trajectory as traditionally reconstructed – few people for a long time, lots of people for a short time, and then few people again for a millennium – is unusual and perhaps unique.

Algorithms based on structure counts of visible surface architecture did not generate the population estimates listed above for Egypt, Sumer, the Basin of Mexico and Hawai'i.⁶⁴ None of these regions have visible household remains comparable to those found at Mayapan, Dzibilchaltun, Coba, Sayil, Tikal, Copan, Palenque, Piedras Negras, or on other well-surveyed Classic Maya landscapes. We Mayanists are fortunate to have such data, but the very richness of the archaeological record might be deceiving us. I have long thought that excessive estimates derive from the ways we extrapolate population numbers from residential structures (see Webster, Freter and Gonlin 2000: 153-174). This was apparently Kurjack's conclusion back in 1981 when he adjusted his original Dzibilchaltun estimates drastically downward. Impressively high numbers cause us to wrestle, often unconvincingly, with simulations of agricultural productivity, which must account for what supported such putatively large, dense populations. This issue heavily motivated the Tikal surveys and demographic interpretations, to which we finally turn.

UNIVERSITY OF PENNSYLVANIA TIKAL PROJECT POPULATION ESTIMATES

This section summarizes population estimates and density implications made by members of the University of Pennsylvania's Tikal project and others beginning in the 1950s and 1960s.⁶⁵ Almost all estimates made for Tikal emphasize its population peak, usually but not always, placed at AD 700. I conclude that the Tikal archaeologists generated very convincing Late Classic population figures for the core polity, although they misunderstood the distribution of this population and accordingly its density and its agrarian adaptations. Following sections present demographic data from our recent Penn State University projects at Tikal, address pre-Late Classic demographic patterns, and provide comparisons with Copan.



**Fig. 3: Robert Carr and Filepe Lanza survey at Tikal in 1960.
Photo courtesy of Robert Carr.**

The Carr and Hazard map

Carr and Hazard published their celebrated 16 sq. km map of Tikal in 1961 (Figs. 3 and 4). The highly accurate map was the first showing a big Classic Maya landscape in such detail in the southern Lowlands, and it attracted considerable attention.⁶⁶ The mapped zone was an analytical construct imposed by the surveyors, and did not correspond to any distinctive and delimited spatial pattern such as known at Teotihuacan or Mayapan. Some 4.48 sq. km (28%) of the map's landscape consisted of bajos that were either unsurveyed, or showed few structures. Because the map recorded only the monumental and adjacent residential precincts of a much larger polity, its main effect was to stimulate arguments about comparative urbanization and consequent demographic scale in the spot density sense.⁶⁷ William Haviland (1963; 1965; 1969; 1970; 1972; 2008) used this map to present extremely comprehensive and thoughtful overviews of Maya settlement, urbanism, and social organization.⁶⁸ The urban issue at Tikal mainly focused on the monumental core and the zone of high residential density shown in Fig. 4. Haviland concluded that a minimum of 10,000 to 11,000 people, mostly non-farmers, lived in the mapped zone, although many practiced arboriculture (Haviland 1963: 523-528; 1965: 19).⁶⁹ His figures compared very well with Kurjack's estimate for a slightly larger area around Dzibilchaltun. We shall see that there has been consistent confusion concerning three landscape components: the "city" of Tikal, a core hinterland with purported emic boundaries, and the larger Tikal polity.

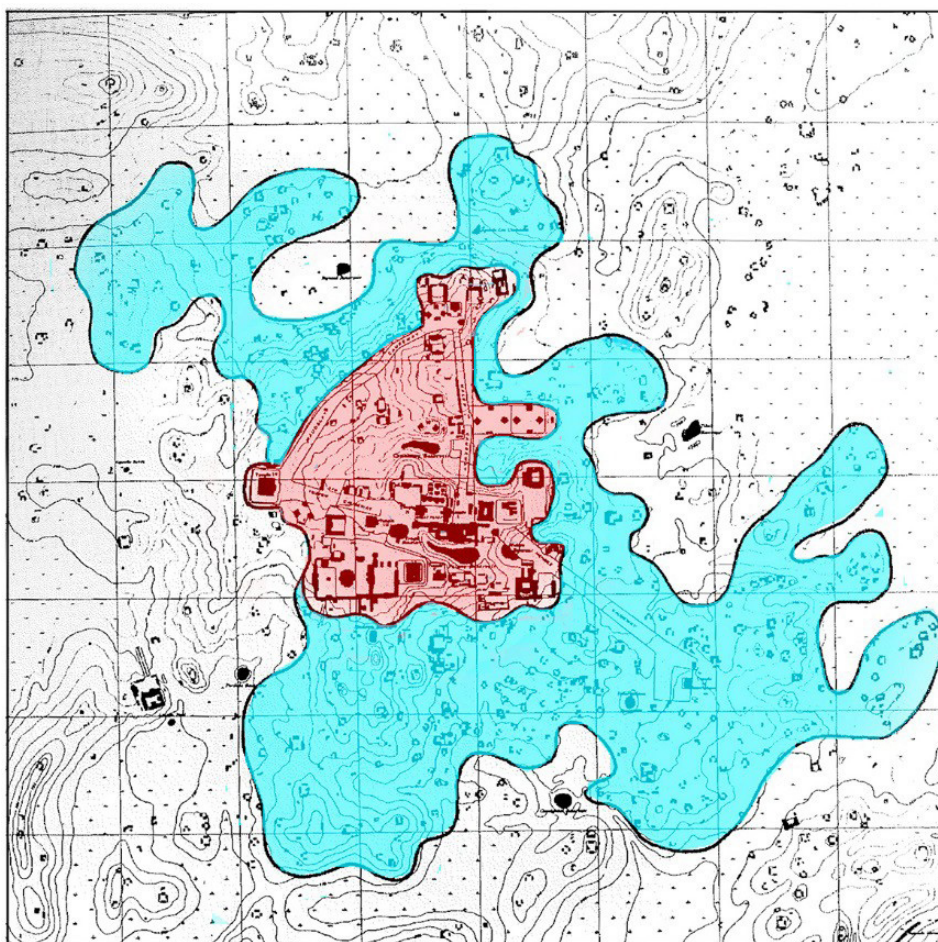
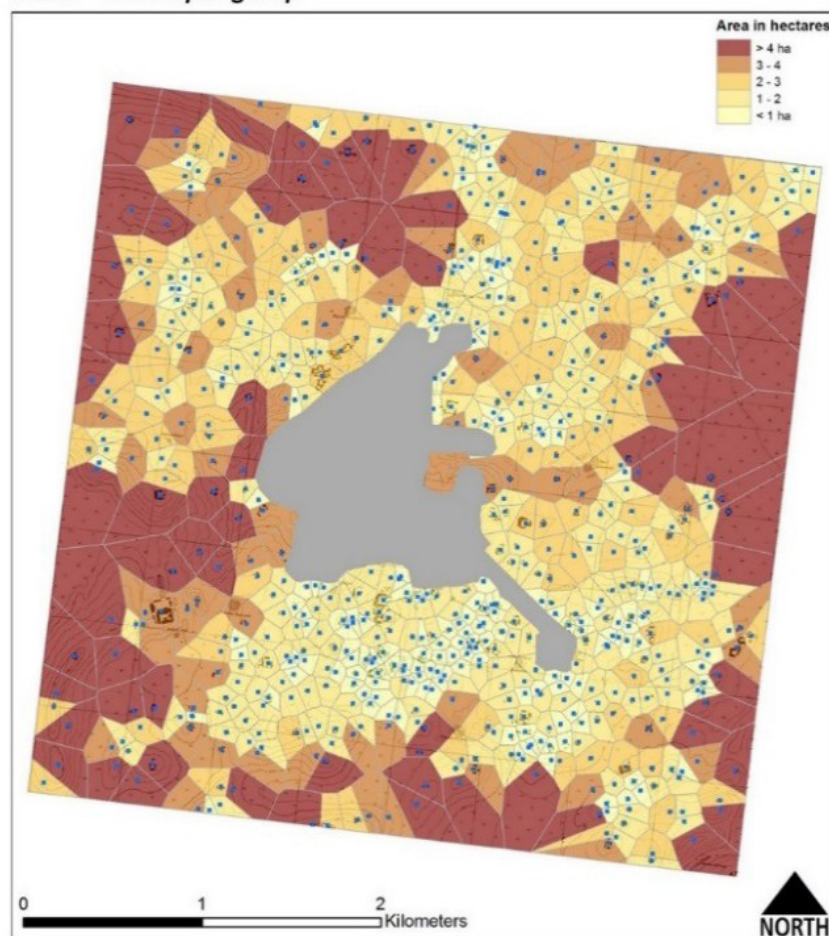


Fig. 4: The Carr and Hazard map showing the Epicenter (red) and the zones of highest residential density (blue). Adapted by Webster from the map in Coe (1967).

When Haviland made his estimate, the only other sites with settlement data from which he could calculate population size were Uaxactun and Mayapan (Haviland 1963: 533-534). Although his research is now 50 years old, Haviland brought an enduring and sophisticated sociological sensibility to his pioneering excavations, which focused on twenty-four “small” structures at eight groups in the northeastern part of the mapped zone (he recognized that some were more properly of “palace” scale and configuration). He particularly grappled with the problems of site function and contemporaneity, and was influential in establishing the nuclear family factor of 5.6 people per house.⁷⁰ In many respects, Haviland’s work, along with that of other colleagues such as Marshall Becker, who also excavated small structures, represents the foundation of settlement and household studies at Tikal, a distinction often attributed to Dennis Puleston.⁷¹ William Sanders (Sanders and Price 1968: 163-170) contrasted Tikal with Teotihuacan, as did Haviland in his 1969 and 1970 articles. Sanders noted that the density and distribution of the approximately 2200 small platforms shown on the map closely approximated patterns known for rural settlement elsewhere in the Peten (see Bullard 1960).⁷² He thought that there was a permanent residential component of about 2000 to 3000 specialists and/or elite people (archaeologists generally did not use the term “nobles” at the time), along with some 2000 to 8800 commoners of various sorts, depending on what calculation he used. He calculated that these “lower class” residents had densities in the range of 312-548 people per sq. km, from which he drew two conclusions: 1) Tikal was a “macroceremonial” center, not

Non Epicenter Plaza Groups using Thiessen Polygons
Mean = 1.93ha per group

Fig. 5



Ratio of land to non-Epicentral groups. Analysis and image by Timothy Murtha.

an urban place like Teotihuacan, and 2) no swidden system could support such numbers. Either a much larger sustaining area supplied resources, or the population was greatly overestimated. At that time, he did not envision intensive forms of food production in the mapped zone. Sanders's final range for the Late Classic population of the Carr and Hazard zone was 4000 to 11,800 people – a reasonable match for Haviland's total.⁷³ This range also agrees well with Rice's figure (cited above) of 1000 people per sq. km for the central cores of big Maya polities. We shall see later that core densities at Copan and elsewhere dwarf those for Tikal. Gordon Willey, who knew Copan and other Maya centers well, agreed with Sanders that the dispersed and comparatively low-density population of Tikal "... makes for not only a quantitatively, but qualitatively different entity ..." than Teotihuacan (Willey 1978: 335).⁷⁴ He went on to remark that "In my opinion, the Classic Maya failure of the ninth century A.D. might be described as a failure to proceed far enough on the ceremonial-center-to-true-city continuum."⁷⁵

Other archaeologists gave intuitive estimates. William Coe, who headed the Tikal Project, weighed in (probably just following Haviland) with a minimum figure of 10,000 people for the Carr and Hazard zone and struggled, like Willey, with the issue of Tikal's urban character (Coe 1967: 107). A later synthesis (Jones, Coe and Haviland 1981: 305-307) called the earthwork-bounded area of Tikal (120 sq. km – see below) a "functioning multi-activities city", but gave no population estimates.⁷⁶ These authors did echo Sanders in noting the small amount of land available per structure. As part of our recent Penn State project, Tim Murtha digitized the whole Carr and Hazard map and generated a model of groups based on Thiessen polygons (Fig. 5). About 1.93 ha of total land is available per structure group outside the epicenter, or about 1.7 ha if we count only uplands. Whether groups were occupied contemporaneously is another question.

The Carr and Hazard survey stimulated two of the first detailed spatial analyses of a large Maya settlement. Jeanne Arnold and Anabel Ford (1980) scrutinized residential units in the most heavily occupied 9 sq. km shown on the map. Their goal was to test a widely accepted concentric model of Maya settlement drawn heavily from ethnohistoric and ethnographic accounts. According to this model, the residences of elite Maya people were concentrated near the monumental site cores. Arnold and Ford ranked 630 residential units in terms of labor investment and measured their distances to the center of Tikal, concluding that there were no such concentrations.⁷⁷ Haviland and Moholy-Nagy (1992: 52) later argued to the contrary that "... the bulk of the nobility resided at the civic and ceremonial heart of the ancient city."

J. Jefferson MacKinnon (1981) published a detailed study of residential sites in several carefully chosen sections of the Tikal map and came to the following conclusions:

- 1) The map contains some errors, but is generally accurate with regard to Late Classic settlement distribution (see Carr *et al.* 2015 for a recent GIS re-evaluation of the map).
- 2) Groupings of house mounds were regularly distributed and show no signs of significant clustering; most parts of the core settlement, which he saw as basically non-urban, were unplanned.
- 3) There is no consistent pattern of large sites associated with smaller ones, as would be expected if there were strong hierarchical dimensions to neighborhood distributions.
- 4) There is no significant correlation between site scale and topographic elevation, although there is a rough orientation of sites to the cardinal directions.
- 5) Size of groups and associated building materials are best explained by occupational history rather than social rank, an assumption very different from that of Arnold and Ford, who associated volume and character of construction with rank.

6) There is no evidence in residential patterns of kin grouping above the level of the extended family.

7) Some 93 to 98% of structures were used during Late Classic times (here he follows Haviland, Culbert, and others – see below).

8) The Maya generally avoided slopes greater than 15 degrees for settlement and planting.

MacKinnon calculated the area of cultivable land available per person and believed his findings conformed to Stierlin's (1964) earlier conception of Tikal as a "garden city." In the fashion of the times, he firmly rejected large-scale dependence on maize/milpa cultivation, and instead thought that the Tikal Maya employed infields, kitchen gardens and orchards to raise various tropical plants, semi-domesticated animals, and trees. He particularly favored Puleston's ramon hypothesis (see Appendix D), remarking that "Puleston's figures for ramon production do show that the spaces between the house mounds could produce enough food to feed the population and leave a surplus besides" (MacKinnon 1981:231).⁷⁸ Despite such generalizations, he oddly avoided population assessments, except to assert that there was plenty of adjacent land to support the occupants of residential groups.

Like Arnold and Ford, MacKinnon identified the presence of palace populations, but thought that there was ambiguous evidence for powerful elites at Tikal. He recognized that there was some central authority able to direct construction of large civic architecture and features like the earthworks, but he did not think elites managed agricultural production or that farmers were reduced to some kind of agrarian proletariat. All this led MacKinnon to doubt that overpopulation and consequent social upheaval were responsible for the Classic collapse, a perspective vigorously argued a year earlier by Hamblin and Pitcher (1980).⁷⁹

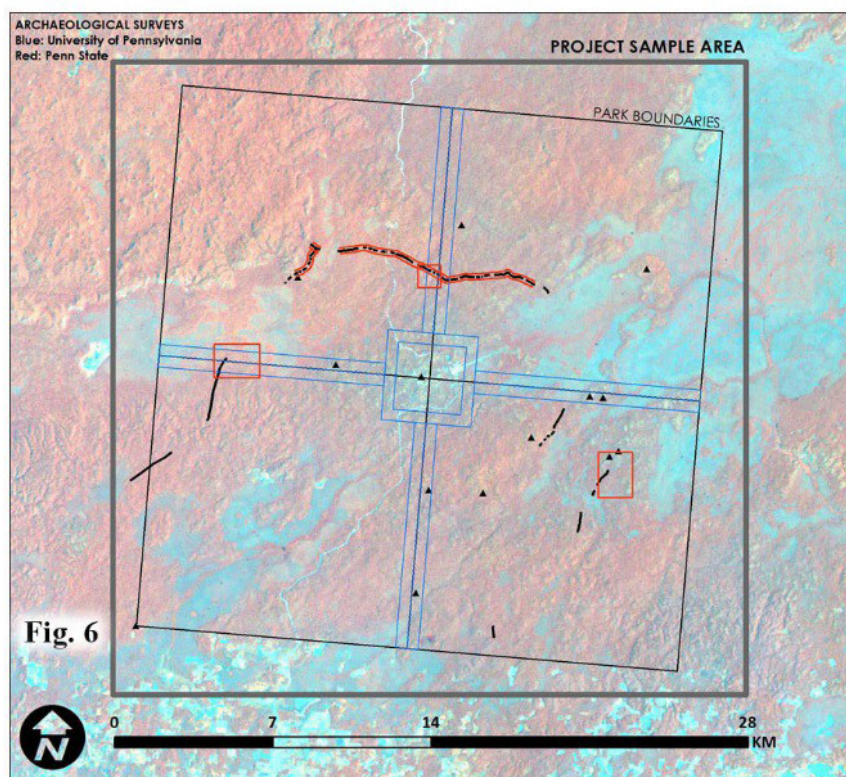
Before leaving the subject of this influential map, it is worth noting that Tikal settlement distributions are often wrongly or confusingly expressed in the literature. Glassman and Anaya (2011: 69-70), for example, describe Tikal settlement this way:

"The core area of the city, occupied by great temples and other elite constructions, took up six square miles. An additional 19 square miles, 25 miles altogether, were occupied by buildings. Another 22 square miles – 47 square miles altogether – was passively defended by walls, moats and natural boundaries. These 22 square miles were farmed intensively. They were the granary that the city made sure was never destroyed by enemies and may indeed have been another of Tikal's many reasons for rising to prominence." (Glassman and Anaya 2011: 69 – 70).

One wonders just what map these authors were looking at. Almost all the major core architecture at Tikal is scattered over a core area of only about 4 sq. km. I say scattered because there is a lot of empty space even in the core. "Great temples and other elite constructions" have a collective footprint of 1.9 sq. km according to Murtha's digital measures, much less than the area implied by the quote. Around these great buildings is Central Tikal, the 8 or 9 sq. km residential concentration that we call the *demographic core* (Webster *et al.* 2008: 369). There is only faint and scattered evidence for intensive farming, at least in the form of durable landesque investments, within the 47 sq. mi. zone (i.e., the area purportedly delimited by earthworks).⁸⁰ Finally, years before Glassman and Anaya published their book, our understanding of the earthwork system, including its size and function, had dramatically changed, rendering their "passively defended" description incorrect (see below).

The Sustaining Area Project

Virtually all post-1973 estimates of Tikal's larger demographic patterns derive from the University of Pennsylvania's Sustaining Area Project directed by William Haviland and Dennis Puleston (Haviland *et al.* 1968; Haviland 1969).⁸¹ This effort vastly enlarged the landscape perspective and set a standard for Lowland Maya settlement survey (Puleston 1973a, b, 1983).⁸² Puleston's 1973 dissertation is remarkable for its comprehensive thinking about Lowland Maya settlement, demographic, subsistence and landscape issues. The methods he used and the data recorded in his four survey transects, supplemented by the household archaeology of Haviland and Becker and the test pitting of Fry, have served for many years as models for how to conduct such fieldwork, how to reconstruct a Classic Maya social landscape, and how the Maya made a living.⁸³ This last point is particularly important because Puleston was as much preoccupied with issues of subsistence (thus following up on Sanders's concerns) as with what settlement remains might tell us about population scale, distribution, urbanism, or social organization.⁸⁴ Dennis Puleston sadly died before he could refine his data and interpretations, and Haviland (1983) later condensed his work in Tikal Report No. 13. Puleston's results were so influential that, with the exception of Anabel Ford's Tikal-Yaxha transect (Ford 1986), no large-scale settlement survey was done at Tikal for almost 40 years.⁸⁵ 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 so well protected as a national park, that small sites are well preserved and accessible on the surface (visibility in dense forest aside). Nor have erosion or hydraulic processes affected ancient settlement remains as drastically as they have at Copan. If Puleston could revisit 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 of our own surveys there, and the recent construction of a new airport is responsible for many more.



The Carr and Hazard map showing Puleston's transects in blue, known earthwork sections in black, and the Penn State survey zones in red. Small triangles are minor centers. Image by Timothy Murtha.

Tikal's dense vegetation precluded the kind of surface coverage possible at Dzibilchaltun (and to a lesser extent at Coba). Puleston accordingly followed the Uaxactun strategy of surveying along four *brechas*, or transects (Fig. 6) cut through the forest and radiating out from Tikal's monumental core.⁸⁶

These strips were 500 m wide and extended about 10-11 km in each cardinal direction. The staff of the park keeps them open, and Fig. 7 gives a good idea of the vegetation Puleston's crews had to contend with. Total survey coverage was not quite 25 sq. km, of which Puleston calculated that 10.7 sq. km (roughly 43%) was uninhabitable *bajo*.⁸⁷ He recorded some 2192 structures in his survey zones.⁸⁸ From this sample, plus that on the Carr and Hazard map, Puleston extrapolated 13,948 structures for the "site of Tikal" (Puleston 1973a: 229). By "site of Tikal", he meant the roughly 120 sq. km region presumed to be bounded by the celebrated earthworks and their associated *bajos* discovered in 1967 (Puleston and Callender 1967).⁸⁹ These earthworks seemed to be a unique emic feature that defended and delineated the main site of Tikal and its core agricultural hinterland: "... we suggest that the inhabitants of Tikal, including the upper echelons of nobles and priests, were interested in protecting the agricultural resources upon which they ultimately depended" (Puleston and Callender 1967: 48). No one knew at that time about Maya toponyms such as *kab ch'en* (or *ch'en*; see below), but had these been understood, Puleston and Callender probably would have applied them to the earthwork-delimited zone. Worth mentioning here is something not found by the Sustaining Project (Puleston 1973a: 289, 291): systems of terracing like those mapped around Caracol and Minanha on the Vaca Plateau in Belize (Chase and Chase 1998; Murtha 2002, 2015; Lamoureux-St-Hilaire *et al.* 2016) and Becan in southern Campeche (Turner 1983), or other signs of landesque agrarian features such as drained fields.⁹⁰

Haviland (1969: 430) swiftly accepted the earthworks and *bajos* as the "clear-cut boundaries" of Tikal, but then rather confusingly went on to calculate a total of 39,000 people for a larger area of 162.78 sq. km (a rough circle with a diameter of 14.5 km). To this zone, he added a 99 sq. km periphery with another 10,000 inhabitants, resulting in a population of 49,000 people with an overall density of 187 people per sq. km for the whole 262 sq. km region. Because Puleston was less concerned with urbanization than with population and subsistence, exactly what the "city" of Tikal might be was less important to him than the emically- bounded hinterland. Nevertheless, some archaeologists began to call that entire bounded area the "city", which at 120 sq. km was roughly six times the area of Teotihuacan. We shall see



The author standing at the south end of Puleston's north transect (2015). Photo by Elijah Hermitt.

later that the earthwork/hinterland implication is incorrect. Puleston's surveys greatly benefitted from the Uaxactun research and improved upon it in several ways, mainly by much larger spatial coverage (25 sq. km vs. 2 sq. km) and the greater distances his transects extended from the site core. Don and Prudence Rice (1990) later surveyed a series of similar transects in the Peten lakes region, about 20 km south of Tikal, and their data supplemented Puleston's in important ways.

Bedeviling most archaeological estimates of Maya population density is the generally poor partitioning of the landscape by either natural or settlement features, at least in etic ways that we can appreciate. It is largely for this reason that Puleston, Haviland and their colleagues seized so eagerly upon the earthwork as a supposed emic territorial boundary. Mayanists have identified many toponyms since Puleston's time (Stuart and Houston 1994; Tokovinine 2013; Velasquez 2010), but Classic Maya texts provide only ambiguous abstract terms for "city", "kingdom", or "polity" in our own territorial/political senses.⁹¹ Nor do place names in the large corpus of emblem glyphs have clear implications for territories or political domains (Tokovinine 2013: 71-85). Their main function seems to be to situate a ruler and dynasty on a landscape of other kings, not primarily in terms of place, but with regard to origins and identities: "The dominant identity in Classic Maya landscape is one's membership in the category of *ajaw* – lords or rulers -- or the lack thereof" (Tokovinine: 2013: 85). Tokovinine's analysis of Classic inscriptions echoes Sergio Quezada's (2014: 9) conclusion that among the Conquest-era Maya in northern Yucatan "... political ties among the ruling elites were of a personal and not a territorial nature..."⁹² As we shall see later, some models of Maya political behavior developed since Puleston's time downplay the importance of territory and suggest other forms of jurisdiction.

Territorial statements do appear in Post-conquest Maya documents. The term *kab* refers to earth or land itself, but also to the territory belonging to a *kaaj*, or town (Tokovinine 2013: 44). For some reason, Quezada does not mention *kab*, possibly because he emphasizes jurisdiction in an expansive political sense rather than a local one. Houston (2000: 173) notes that the *kab ch'en* couplet in Classic inscriptions might refer to the land or the property of rulers, or even to some broader political concept such as the central Mexican *altepetl* (see Webster *et al.* 2008 for a discussion of the *altepetl* in relation to the earthworks).⁹³ Tokovinine (2013: 36, 45) echoes this view, surmising that *kab ch'en* might mean "... the holy grounds and the land, the polity" -- some kind of landscape entity that always seems to be possessed by someone, as in a hereditary political domain. He goes on to review the many complex ways the Classic Maya named their landscapes, places, and jurisdictions. One meaning of the expression *ch'en* seems to refer to "... the homes of kings and gods, which matter most in the Classic Maya political landscape" (2013: 38). He further suggests (p. 45) that the term *k'uhul kab* might mean the specific domain of a "holy lord." Despite the existence of many Classic Maya expressions for groupings and networks larger than the local place or kingdom, "... territories are noticeably absent in the contexts when protagonists identify themselves or others as members of certain geopolitical collectivities" (Tokovinine 2013: 123). Expressions with spatial, directional, or other kinds of associations occur early in Maya epigraphic history, but they seem to proliferate and take on new meanings on the complex and crowded Late Classic landscape of the seventh and eighth centuries.

The Tikal mapping and surveys began from the center, the spatial entity so essential to Maya royal identity in the inscriptions and the core *kab ch'en* of its kings. They progressed outward to zones that were less well known and less well sampled, although this did not discourage enthusiastic extrapolation from known to unknown, as will become clear shortly. Using site density and earthwork data from his transects, Puleston (1973a: 17-23) defined a series of roughly concentric settlement zones (I slightly rephrase them below):

1) Epicentral Tikal: The space occupied by most of the monumental architecture at Tikal. It extends out for 0.5 km on the east survey strip, 0.4 km on the south strip, 0.75 on the west strip, and 1 km on the north strip. Notice that this does not coincide with the 16 sq. km Carr and Hazard map (see Haviland 2008: Fig. 2).

2) Central Tikal: The space within which there is high settlement density on cultivable land; bounded at 1.5 km on the north strip, 0.75 km on the west strip, 1 km on the south strip, and 0.75 km on the east strip. This *includes* Epicentral Tikal. It is an irregular area of mainly high ground (see Haviland 2008: Fig. 2).

3) Peripheral Tikal: the area outside Central Tikal, defined by points on the north strip at 0.5 km. on the south at 6.5 km.

4) Residential Tikal: The combined areas of Central and Peripheral Tikal, but excluding Epicentral Tikal. It essentially corresponds to almost all parts of the 120.5 sq. km within the presumed earthwork boundaries.

5) Tikal: Residential plus Epicentral Tikal – in other words everything within the earthworks, and thus a general term for the central core of the polity and its presumed emically-bounded hinterland.

6) Tikal Zone: The total area hypothetically under the jurisdiction of Tikal, as opposed to its neighbors such as Yaxha or Uaxactun.

7) Intersite areas: Various zones and sites not clearly attached to a dominant center.

However impressionistic the scale and distribution of these zones was (and this was even true of the earthwork-delimited area), at least they served useful descriptive and heuristic purposes. One generally knew which part of the landscape Puleston was talking about, and his conception of its significance, which is not always true about some other surveys or population estimates. For example, shortly after Puleston completed his dissertation, Culbert (1974: 41) said that Tikal population densities reached 618 people per sq. km, but did not specify what zone he meant.⁹⁴ Similarly, the recent overview by Lentz *et al.* (2014: 1) focuses on sustainability “... at the ancient Maya city of Tikal”, but what they mean by “city” is vague. They accept Haviland’s Late Classic population of some 45,000 people but are unclear about its distribution.⁹⁵ This population apparently inhabited their study area of some 1100 sq. km. This zone is the local polity, not just the “city”, which is presumably a more restricted area otherwise we have a Maya “city” fifty times the area of Teotihuacan.

Tikal (Zone #5) is by far most important in the subsequent demographic literature, although various presentations seem to conflate it (reasonably) with Residential Tikal. The poorly known earthwork (Fig. 8) became the default, emic hinterland boundary: “An area of 120 sq. km is defined by the combination of lower-structure density, earthworks, and *bajo* ... This is what we consider the site of Tikal” (Culbert *et al.* 1990: 115). More recently, “The city’s limits were marked to the north and the south by the presence of two defensive trenches or moats during the Early Classic (Valdez and Fahsen 2004: 156; emphasis mine). Here the striking confusion of “city” (whatever that might be) and agrarian hinterland is apparent. As the latter quote shows, the Tikal archaeologists thought that the earthworks were constructed late in the Early Classic (around AD 550), even though there was little evidence for this date, and they were most often used to analyze Late Classic Tikal population distribution.⁹⁶ Had Puleston lived, he might have been more cautious about the supposed configuration and demographic significance of the earthworks than subsequent Mayanists and more sophisticated about urban patterns.

My own main concerns are Late Classic Tikal (everything within the earthworks as Puleston imagined them), and the larger Tikal Zone – which includes sites beyond the earthworks that can reasonably be ascribed to Tikal’s jurisdiction, and hence have implications for its territorial scale, political support and available labor. These together comprise the Tikal kingdom or realm, which of course waxed and waned in size and sometimes exercised influence over other Maya polities and populations at considerable distances (although whether it could depend on them for abundant subsistence resources is doubtful –see Appendix D). Puleston chose to emphasize individual structures as his analytical focus, so he downplayed the sociological significance of residential building clusters. Nowhere in his dissertation are there tabulations of site numbers and character (although these can be seen on his maps, and much information could probably be retrieved from his field notes).⁹⁷ In contrast, at about the same time Kurjack at Dzibilchaltun (1974) and Tourtellot (1983) at Ceibal developed just such typologies. Some members of the Tikal project did have strong group perspectives (e.g. see Haviland 1963; 1969; 2003; and 2015 for summaries, and also Becker 2003), but these tended not to extend over the whole survey landscape beyond the 16 sq. km zone mapped by Carr and Hazard. The result is that there is no comprehensive Tikal group typology and distribution such as developed by Willey for Copan, and so well presented in the survey maps and catalogues later published by Baudez’s Copan project (Baudez 1983). Our own Penn State survey projects at Copan continued this group perspective (Freter 1988; Webster *et al.* 2000). Haviland (2008: 273) says of Tikal that “... unlike Copan, one cannot neatly divide up residential compounds into a hierarchy of a few relatively discreet types ... the situation is more complex.”

Robert Fry (1990: 287) listed 719 mapped structures in 283 groups in Puleston’s north and south survey corridors. He completed 97 test excavations in these two corridors, each one near a mound, for a sample of 13% of all structures and 33% of all groups in these survey zones.⁹⁸ Fry (1969; 1990; 2000)

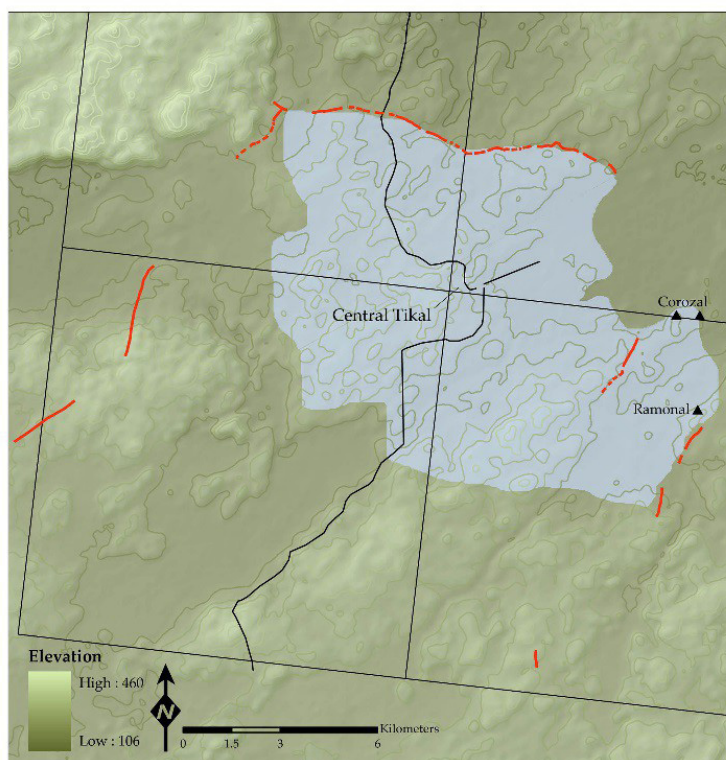


Fig. 8: Puleston and Callender thought the blue zone was protected by earthworks and bajos. Red lines show known sections of the earthwork and its much greater spatial extent. Image by Timothy Murtha.

also acquired artifact collections from the outlying minor centers of Jimbal and Navajuelal. The main objectives of these excavations were chronological control and detection of specialized activities. Fry's test sample amounts to about 4.5% of all structures mapped by the Sustaining Area Project (and a larger percentage of all groups), and about 0.7% of Puleston's extrapolated structure total for the whole 120 sq. km earthwork-delimited landscape. Haviland, Becker and others carried out similar tests, along with more ambitious household excavations, in sites in the central Carr and Hazard zone (see Haviland 1963; 2003: 113; and 2014a, b for lists, and Becker 1983). Haviland (1969: 429) argued that his own household research near the site core showed that residences were seldom abandoned, a perspective that informed his population estimates. The more distant tests of Puleston and Fry, he thought, demonstrated much less residential permanence, complicating his own methods of calculating overall population.⁹⁹ Test pitting showed that some visible structures predated the Late Classic, so he adjusted demographic estimates accordingly.

Contemporaneity of occupation aside, two other deranging factors must be taken into account. My research at Copan and Piedras Negras leads me to suspect that many outlying household sites were not permanent habitations. Instead, people who otherwise lived in or near the site centers occupied them seasonally or opportunistically. A second problem is structures that are invisible on the surface. Kevin Johnston (2004) investigated such structures at Itzan, to the west of Tikal, and postulated a hidden universe of residences that we cannot easily detect. To their credit, the personnel of the Sustaining Area Project paid considerable attention to the problem of "hidden" and "vacant ground" structures at Tikal (Haviland 1963; Bronson 1968; Puleston 1973a: 164-171).¹⁰⁰ We do not fully understand these deranging factors, but their effects on population estimates are diametrically opposed. If Johnston is right, we consistently underestimate population and the figures shown in Table 1 are too low. If, on the other hand, many structures were only intermittently used our estimates are too high. If hidden structures are a serious deranging factor for the Late Classic, we have to increase our population estimates, many of which are already excessive. The most hopeful scenario is that they cancel one another out.

Although Puleston heavily depended on Fry's ceramic identifications for his settlement history and demographic reconstructions, he identified several shortcomings in the way these data were analyzed and recorded. Most serious was that ceramic assessments relied on relative proportions within a collection rather than actual sherd counts (Puleston 1973a: 137). Some curious reasoning also occurred in the interpretation of Tikal test pit ceramic data as opposed to collections from larger excavations. If a test pit yielded one sherd of a particular ceramic phase, then Fry counted the associated structure as occupied throughout that phase (Puleston 1973a: 143). On the other hand, when some test-pitted sites were later more completely excavated, small numbers of sherds not found in the earlier test pit samples were *not* counted as evidence of occupation. There is obviously great potential for distortion of settlement chronology and demography here, mainly in the direction of inflated population numbers.

As an aside, test excavations are often discussed in settlement reports, but not always described in detail. Archaeologists typically mean one of three things by the term:

- 1) Test pits, usually 2x2 m but often smaller (or even shovel or core tests), are placed in or adjacent to structures or sets of structures. This was Fry's strategy. Haviland (2014a: 2) thinks a resulting problem was that "... minimal attention was usually devoted to architecture and the relationship of artifactual material to specific structures."¹⁰¹

- 2) Mounds are partly explored by large trenches dug through their short or long axes and/or around their peripheries. Most of Haviland's household explorations at Tikal used this strategy; see plans presented in Haviland's (2014a) Tikal Report 20A, Figs. 1-152.

3) Mounds or (better) whole mound groups are mostly or completely stripped by large lateral excavations, which include their plazas and other ambient spaces. We used this strategy at Copan and Piedras Negras, and Haviland used it occasionally at Tikal (see Haviland 2015). By far the best (if limited) such evidence comes from Ceren (Sheets 2006).

Having done a lot of this kind of work, I am convinced (as is Haviland) that only complete excavation that includes ambient spaces provides the well-controlled samples and insights into site configuration and function necessary for good simulations of population.¹⁰² Because one can only obtain very limited site samples this way, extensive test pitting must complement the strategy, as we did at Copan. In any event, Puleston's surveys and Fry's excavations helped Culbert to refine his ceramic phasing, which is shown in Table 3.

Table 3: The Tikal ceramic sequence.*

Lowland Maya Periods	Ceramic Complexes	Calendar Dates
Early Postclassic	Caban	AD 950-1200
Terminal Classic	Eznab	AD 870-950
Late Late Classic	Imix	AD 700-870
Early Late Classic	Ik	AD 550-700
Early Classic	Manik	AD 250-550
Terminal Preclassic	Cimi	AD 150-250
Late Late Preclassic	Cauac	1-AD 150
Early Late Preclassic	Chuen	350-1 BC
Middle Preclassic	Tzek	600-350 BC
Middle Preclassic	Eb	800-600 BC

*Members of the Tikal project have slightly adjusted the dates of the original ceramic chronology laid out by T. Patrick Culbert. This one is from Moholy-Nagy *et al.* 2013: 74.

Based on his surveys and Fry's work, Puleston (1973a: 201-203) calculated that Late Classic Tikal had a total population within the earthworks of 657 people per sq. km on uplands and *escoba bajo*, for a total of 79,168 people. He regarded this as a "median" estimate, and settled (p. 207) on a range of 65,000 to 80,000 (density range = 539 to 634) for the uplands in the whole 120.5 sq. km region. Later (p. 303) he says the population of Residential Tikal on the *escoba*/uplands, or even just the uplands, " ... could have ranged from 658-921 people per sq. km." Remember that because these figures refer to the core agricultural zone, they are upland *productive* densities. Puleston (1973a: 231) observed that these densities made Classic Maya food production capacity exceptional in comparison to other parts of the world, including some already discussed above. One reason why he entertained his controversial *ramon* hypothesis is that it helped to explain how so many people were supported (see Appendices C and D below). He thought that *ramon* cultivation allowed the astounding density of 724-1849 people per sq. km, although occasional shortfalls were made up by food brought in by outlying farmers. This idea is one source of the "managed fallow" hypothesis discussed below and in Appendix D.

Veterans of the Tikal project since Puleston's time have long presented reasonably consistent estimates for peak Late Classic population size, generally settling on smaller figures than Puleston's. Culbert *et al.* (1990: 106) and Haviland (2003: 129; 2008: 259) respectively proposed populations of 62,000 and 45,000 people for the 120 sq. km of contiguous territory.¹⁰³ Their estimates have become conventional wisdom, and Lentz *et al.* (2014) used them in their recent overview of Tikal's agrarian

adaptation. Exactly *when* Haviland and Culbert think the peak population occurred is not always clear from the literature. Culbert (1973: 67) observed that: “The site is literally covered with Imix pottery, almost everywhere in quantities large enough to indicate actual occupation” (1973: 67). He went on to say that the apparent peak interval of monumental construction was between AD 692 and 751 (Culbert 1973: 72). Later Culbert *et al.* (1990: 112) simply put the peak population sometime during the Ik and Imix phases, which seems odd given their insistence on the preponderance of Imix ceramics. Fry thought population peaked at roughly AD 600-650. Haviland originally placed the population peak sometime during the ninth century (1965: 19) and later at AD 770 (1970: 192). A decade ago he put it at AD 700 (Haviland 2003: 128), and most recently, he thinks that instead of a sharp peak, there was a lengthy plateau of population that began around AD 550 and lasted until at least AD 770 (personal communication Oct. 2014; Haviland 2014: 146.) I find the logic of this long Tikal plateau problematical in ceramic terms. If it were the case, then well-tested domestic residences should yield similarly high concentrations of both Ik and Imix ceramics, but I cannot tell from the reports if this is so. The general emphasis appears to be on Imix preponderance.¹⁰⁴ For the present, I use the round figure of AD 700 for the peak population, given its prominence in the literature (including Lentz *et al.* 2014), the ceramic chronology in Table 3, and Fry’s relative reconstructions shown below.

Mayanists have accepted the Culbert-Haviland estimates for so long that they are about as non-controversial as any could be. Below I will argue that the 45,000-62,000 range is plausible, but only if people were distributed differently on the landscape than the older settlement models envision. Table 4 shows the overall densities for the 120 sq. km region using the Haviland/Culbert figures (productive densities based on Puleston’s assumption of a 57% to 43% ratio of uplands to *bajo*). The problem is immediately evident. Even the overall densities dwarf those in our comparative examples, including that of modern Haiti. Haviland’s low overall estimate alone is more than twice that of the Basin of Mexico population in AD 1519. Remember also that Puleston considered the earthwork-bounded zone to be the agricultural hinterland of the polity – i.e., it produced all or most of the food for the population of the core kingdom. No reasonable subsistence model accounts for such numbers in a region of this scale -- much less on a landscape already subjected to human wear and tear for almost 2000 years and that has few signs of landesque agricultural features.

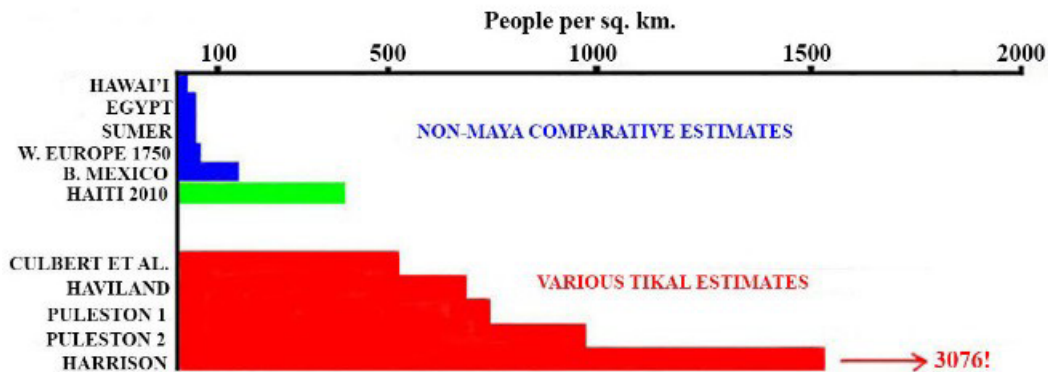
Culbert *et al.* (1990: 116-117) knew that by Late Classic times Tikal’s political control extended in some fashion beyond the earthworks, so they devised two more spatially-extensive estimates. First, they calculated population for all of the landscape within a 12 km radius of the Epicenter (452 sq. km) at 120,000 people. The corresponding overall density works out to 262 people per sq. km, or about 460 per sq. km upland density using Puleston’s ratio.

Next, they envisioned a larger “realm” of some 1963 sq. km that included other major sites and that in some sense was under Tikal’s political influence. They postulated that this vast region had some 425,000 people, for an overall density of 216 per sq. km. Peter Harrison (1999: 9) later claimed that “By the time of its collapse in the tenth century, Tikal covered roughly 65 sq. km ...The population reached a figure of somewhere between 100,000-200,000, although arguments are entertained for even greater numbers.” What he means by the 65 sq. km area is unclear to me, but the overall density range is an extraordinary 1538 to 3076 people per sq. km.¹⁰⁵ Turner (1990: 321) reconstructed population for an even larger “Tikal region” of some 12,600 sq. km, which he thought supported 1,520,107 people at AD 600-800. This is roughly the same number who lived in the whole Nile Valley in 2500 BC, but on a landscape only one-third the size and of much poorer agrarian capacity!

Table 4: Implied densities of the Haviland/Culbert estimates.

Source	Central Tikal	Sustaining Area	Total Pop.	Overall Density 120 sq. km	Productive upland density
AD 700 (Culbert et al.)	16280	45720	62000	517	861
AD 700 (Haviland)	11700	33300	45000	375	625

Fig. 9: Comparative overall density estimates for Tikal and other civilizations



The population figures long championed by Tikal project archaeologists (and others influenced by the Tikal literature) are thus often very high when compared with our non-Maya examples (Fig. 9). In the absence of widespread evidence for systems of intensive agriculture on the Tikal landscape – and especially permanent, landesque capital improvements – I find these densities implausible. They approach those of rice cultivators in East and Southeast Asia, which had (and have) the world’s most densely inhabited traditional agrarian landscapes (see Bray 1986: 144). Notice also here the seductiveness of extrapolation from what known to unknown. The Carr and Hazard map and Puleston’s transects together amount to about 41 sq. km of reasonably well-surveyed landscape.¹⁰⁶ The extrapolation progression reviewed above is 41 sq. km to 120 sq. km to 262 sq. km to 452 sq. km and to 12,600 sq. km, with each successive increment less well documented than the former.

THE LATE CLASSIC TIKAL POPULATION: PENN STATE PROJECT ESTIMATES

Our first Penn State Tikal research between 2003 and 2006 (Webster *et al.*, 2005, 2007) evolved from the Sustaining Area Project in two ways. First, we restudied the Tikal earthwork system. Puleston and Callender were careful to admit that they knew only parts of it and that its chronology was obscure. Because later conventional wisdom identified the earthworks and connecting bajos as the default emic boundaries of Tikal in Late Classic times, they became central to successive demographic estimates. Within two years of the discovery of the earthworks, Haviland (1969: 430) confidently said it was “... apparent that clear-cut boundaries existed for Tikal in Late Classic times ...”

Second, we surveyed a 250 m wide strip of land on either side of the northern earthwork, along with several other block sections, to check and amplify Puleston’s transect sample and his conclusions about settlement density and distribution. We followed up by excavating test-pits in fifty-two of the newly recorded sites to complement Fry’s earlier test excavations. We also dug eighteen long trenches through the earthwork. Mindful of our previous Copan experience, our focus in mapping and analysis was the group, not the individual structure.

Regional settlement research is usually problem-oriented only in the most general sense. It is often undertaken to fill in gaps in previous research or to address very general agendas. Puleston's main concern, along with population history, was the issue of how the ancient Maya adapted to Tikal's landscape and how it supported the apparently dense populations of Late Classic times. Our own work at Tikal also focused some very specific questions:

- 1) Are the earthwork layout and dating properly understood?
- 2) Did the earthwork function as a demographic and territorial boundary, especially during the Late Classic?
- 3) Was the defensive interpretation plausible?
- 4) How did outlying settlement relate to the earthwork?
- 5) What could we learn about soil and vegetation patterns from remote sensing or from field testing?

Our basic goals were to remap known segments of the earthwork and locate new ones, to dig sections of ditch and parapet, and to date them if possible. We devised the survey corridor along the northern earthwork to check Puleston's proposition that there was a distinct fall-off in structure density right at this "boundary". We also carried out a soil/vegetation survey to the southeast of Tikal.¹⁰⁷ We retrieved 844 soil samples from some 200 profiles in excavations at or near the earthworks and from the surrounding landscape. They are currently being analyzed to identify soil types and measure stable carbon isotopes.

The biggest methodological challenge confronting our survey in those pre-LiDAR days was that dense forest precluded use of conventional aerial photos, nor did we have a detailed topographic map. We accordingly utilized GPS equipment, which turned out to work well under the forest canopy. We reversed normal field strategy by doing the ground survey first, and then later viewing available remote sensing images. Various bands of satellite imagery failed to show the long, linear feature of the northern earthwork, much less even large sites, despite the fact that we know exactly where to look for them. Such images did prove useful for the vegetation/soils survey. Perhaps the recent LiDAR coverage for Tikal (early 2017) shows the sections of the earthwork that we surveyed and will reveal others as well.

Operations and results

We remapped the earthwork and found several previously unknown sections of it as shown in Fig. 8. Puleston's own mapping of both earthwork and settlement turned out to be impressively accurate, but incomplete. Our research indicated that the earthwork was not an effective fortification, that it was probably never finished, and that it was longer and differently laid out than expected. We found no signs of the southern earthwork so long imagined to parallel the northern one.¹⁰⁸ Old buried soils from beneath the earthwork's embankment and from our extensive soil testing show stable carbon isotope enrichment consistent with the cultivation of C⁴ plants, probably maize (Balzotti *et al.* 2013; Burnett 2009; Burnett *et al.* 2013). Puleston's idea of staple ramon cultivation in the Tikal hinterland is not supported. Like Puleston, we found no signs of intensive agriculture or landesque features.

Our settlement survey mapped approximately 113 groups with 474 structures over an area of 7 sq. km in the three block survey segments and the northern "corridor." Most sites were small ones, and test pitting yielded basic household assemblages of Late Classic (or later) date, but no clear signs of any specialized activities. Late Preclassic and Early Classic ceramics are extremely sparse (see below). Puleston (1973a: 227) estimated structure densities on the order of 49 per sq. km in his "intersite areas"; we found more --

about 67 per sq. km -- partly because our eastern survey block encroached on the large satellite centers of Ramonal and Chalpate where settlement is virtually continuous. More importantly, there is no drop-off in structure density along the “outer” side of the northern ditch as Puleston expected. Haviland (1981: 89) is of course correct that settlement density is generally lower outside the earthwork than within it, but Puleston’s interpretation of an abrupt earthwork threshold is incorrect.

We found no clear evidence to pin down the date of the earthwork. The hegemonic struggles between Tikal and the Kanuul kings of Dzibanche/Calakmul are obviously relevant to this issue if the earthwork were conceived as a fortification/boundary. Stanley Guenter, in his detailed study of the hiatus and the Tikal/Calakmul conflict, proposed that “... Tikal’s earthworks date to around 650 and were constructed either at the end of the reign of K’inich Muwaahn Jol II or at the beginning of the reign of Nu’n u Jol Chaahk, when Tikal had lost Uuxactun to the forces of the Snake lords to the north. The enormous amount of energy that went into this work could well explain why no constructions in the center of the city have been attributed to these kings” (Guenter 2002: 175-176). Guenter regards the earthworks as the single greatest construction project ever undertaken at Tikal, and reasons that they must have been built during a short interval in the face of a major threat.¹⁰⁹ Unfortunately his own analysis makes clear that critical events in this complex rivalry occurred over almost two centuries, so they provide no specific chronological anchor such as he proposes (see Appendix F for discussion).

Recent research in the Buenavista Valley and at El Zotz to the west of Tikal (Doyle, Garrison and Houston 2012) reveals increased defensive posturing by Early Classic elites there. El Zotz was probably an ally of Tikal’s enemy El Peru during the Late Classic. These new data reinforce Puleston’s and Callender’s original guess that at least parts of the earthwork were built around the end of the Early Classic. Fig. 8 shows how the impressive western section of the earthwork we found meets the northern one just where the low country around Tikal abuts the rugged hills of what is now called the Mirador Uplands (see Houston *et al.* 2015: Fig. 1-2 for a really good image of the El Zotz-Tikal corridor). On the other hand, Tikal was later also threatened by the ascendancy of Naranjo, barely a day’s journey away, which became a regional power after the defeat of its patron and ally Calakmul in AD 695. Tikal contested its eastern and southeastern approaches during wars with Naranjo and its allies throughout most of the eighth century. Yaxha, located between the two rivals, was also embroiled in these conflicts. Such protracted struggles make just about any date for different earthwork sections plausible. My own hunch is that the inception of the earthwork system dates to the Late Manik or very early Ik phases, or sometime before AD 562 when the Kanuul kings tightened their noose of allies, including Naranjo, Calakmul, El Zotz, El Peru and Holmul around Tikal (Estrada-Belli and Tokovinine 2016: 163-164). We did find tantalizing hints that one section was constructed considerably later, perhaps in response to Naranjo’s post-Calakmul rise as a regional kingdom in its own right.¹¹⁰

All these uncertainties aside, there is no reason to think that the earthwork functioned in any meaningful sense as a fortification or territorial marker for the mature Tikal polity in the late eighth or early ninth centuries.¹¹¹ It probably long predates the eighth century, and had little influence on where people settled on the landscape then or later. We conclude that Tikal Project archaeologists conceived of the earthwork system incorrectly and that all calculations of its density implications for a bounded Late Classic agricultural hinterland are suspect. Nevertheless, the old Puleston/Callender earthwork interpretation remains remarkably tenacious (e.g., see Dahlin and Chase 2014).

Tokovinine’s (2013: 91-98) able summary of Classic Maya quadripartite conceptions tempts me into an uncharacteristic digression about emic geopolitical perceptions, sometimes called *cosmovisions*. Puleston and Callender emphasized the supposed pragmatic functions of the earthwork system -- boundary and defense. In the late 1960’s, epigraphers were just beginning to detect the Maya penchant for

quadripartite (or quincunx) geo-spatial order, so the Tikal archaeologists knew little or nothing about it. Given the interest in such patterns evinced by epigraphers since that time, I am surprised that no one to my knowledge has discussed the original conception of the Tikal earthworks from this perspective. Although they do not quite say it this way, Puleston and Callender postulated a four-sided boundary, or at least territorial configuration, roughly aligned with the cardinal directions and with Tikal in the center.¹¹² Tokovinine (2013: 97) reviews inscriptions from the Tikal corpus that reveal directional associations. He suggests that “... Tikal rulers saw themselves at the center of this quadripartite world order.” I bring all this up with some trepidation, simply as a curiosity and not as a serious interpretation, given the goofiness it might provoke in some quarters.

Demographic implications

Having jettisoned the 120 sq. km earthwork zone as unrelated to the Late Classic population or to urban conceptions of Tikal, we followed the earlier example of Culbert *et al.* (1990: 116-117) by imposing a 12 km radius (452 sq. km) around the Epicenter (Fig. 10). This region incorporates almost all known segments of the Tikal earthwork, Puleston’s survey corridors, and about a dozen impressive minor centers such as Ramonal, Jimbal and Navajuelal that were probably subordinate to Tikal. We think it is a reasonable approximation of the core Tikal kingdom or “realm” in the eighth century. Haviland (1963: 529) initially suggested a 13 km radius in his much earlier consideration of the sustaining area issue, and in 1969, he used one with about a 7 km radius.

Tim Murtha calculated the size and distribution of basic landscape components as shown in Table 5, and keyed by color in Fig. 10. While the bajo and largely un-usable sections of the landscape (Land zone 1) are still significant, their distribution only represents 15% of the total region, a smaller proportion than Puleston estimated for his survey strips. Conversely, upland areas shrink to 56% of the landscape, with 11% on slopes that are highly susceptible to erosion. These areas, although probably initially attractive to early farmers, had slopes greater than 15%. Their soils would have eroded rapidly if cleared and might not have been usable by Late Classic times. Land zone 2, which essentially bridges lowlands and uplands, makes up 30% of the landscape. While its soils cannot be considered as fertile as those of Land zone 3, they probably represent the most sensitive areas associated with niche construction and inheritance. These margins, footslopes and toeslopes today exhibit evidence of re-deposition of soils from the uplands, and also have the clearest isotopic signatures of maize production. Because of their slope and topographic position, they represent some of the more stable pockets of soil resources throughout the Classic period, especially as upland soils in Land zone 3 begin to erode and decline in fertility.¹¹³

Table 5: Tikal landscape components (slope + topographic position).

Zone	Hectares	% Of Landscape	Topographic Description
1	6695	14.8	Bajo areas with standing water during the rainy season; typically less than 1% slope.
2	13462	29.8	Palm bajos bajo margins, toeslopes, and footslopes with slopes of 1-3%.
3	20433	45.2	Uplands with not more than 8% slopes and with well drained soils.
4	3964	8.7	Steeply sloping (8-15%) uplands highly susceptible to erosion.
5	684	1.5	Severely sloping uplands >15%
Total	45238	100	

Table 6: Gross land-use categories from an 1100 sq. km Voroni zone around Tikal (adapted from Lentz et al. 2014: Fig. 52).

Land-use categories	Area	Percentage
Uplands (total forest, cultivated, and residential)	850 sq. km	78%
Bajo (forested and cultivated)	250 sq. km	22%

Lentz *et al.* (2014) later made a similar analysis of land-use categories within a larger and differently configured zone of some 1100 sq. km. Table 6 shows very simplified figures I have boiled down from their study for comparison. Note the close agreement with Table 5 if we use a simple upland vs. bajo contrast. Lentz *et al.* found evidence for cultivation of maize and other plants along the bajo margins and possibly in the seasonal bajos themselves, just as we did.

Concurrently with our project, Robert Griffin (2012) simulated long-term productive capacity around three sites in the nearby San Bartolo region, where soils, topography and vegetation are similar to those at Tikal, and where there are few signs of agricultural terracing or bajo cultivation. Griffin enjoyed the advantage of very sophisticated NASA remote sensing images that, along with ground data, allowed him to model soils, vegetation and agrarian production strategies. He found that various fallow regimes of maize-based swidden agriculture could plausibly support reasonably dense populations for long periods, although locally there must have been some contribution from bajo cultivation and kitchen gardens as

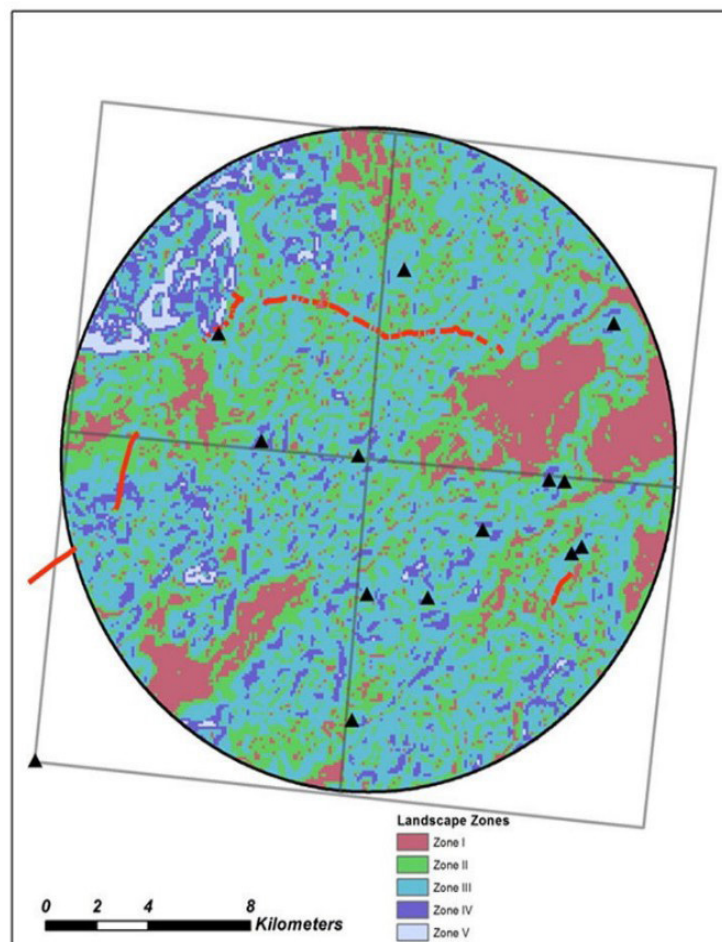


Fig. 10: Landscape zones within a 12 km radius of Tikal.
Image by Tim Murtha.

well. Griffin calculates an *overall* density range of 117 to 150 people per sq. km for AD 800 (Griffin 2012: 235).¹¹⁴ When applied to our Tikal land zones, this range works out to 52,884 to 67,800 people, gratifyingly close to the traditional totals of Haviland and Culbert (*et al.*). The difference is that in our model spreads the population over a region almost four times as large as their earthwork-bounded landscape. Upland productive densities are thus much lower than they calculated, only 137 to 175 people per sq. km.¹¹⁵ I suspect that this range is still too high, but such densities are much more reasonable than Haviland's and Culbert's, especially if they were maintained for a short time at the demographic peak of the Tikal polity. They closely approximate the overall density of 145 people per sq. km by Turner (1990: 303, 312) for three regions of the southern Lowlands totaling some 22,715 sq. km, and the densities calculated by Rice and Rice (1990) for the Sacnab- Yaxha basins.

For purposes of this discussion, I adopt a round figure of 60,000 people for the core Tikal realm (i.e., within the 12 km radius) in AD 750-800, although Haviland's 45,000 estimate is equally acceptable. I put the maximum population somewhat later than the standard date of AD 700 because this interval agrees better with the extraordinary preponderance of Imix ceramics in tested sites as noted by Culbert, assuming that the dating of the ceramic sequence shown in Table 3 is correct. Sixty thousand people within 12 km of Tikal could provide plenty of labor for the constructions made during that time and generate the food surpluses required by the high-density populations in the Carr and Hazard zone.¹¹⁶

Lentz *et al.* (2014) utilized Haviland's Late Classic population estimate of 45,000 people in their recent agrarian analysis of the zones shown in Table 6. They are unclear, however, about how this population was distributed on the landscape (unlike Haviland, who restricted it to the 120 sq. km "earthwork" zone). Their title says their concern is sustainability at the "city" of Tikal, but this entity is not specified in spatial terms. I assume from their presentation that the 45,000 figure refers to the whole 1100 sq. km Voroni region they depict, which retains the confusion of city and hinterland. If so, overall densities are 41 people per sq. km, a "low-density" city indeed. Productive densities in uplands would be about 53 people per sq. km, even lower than our own figure, and the "city" apparently has expanded to incorporate the entire local polity.

We saw above that Puleston partitioned the Tikal landscape in several ways. As a result of our own Penn State project, I prefer a very simple set of spatial and demographic components for Late Classic Tikal that differs somewhat from his. First, there is the *monumental core*, the area shown in red in Fig. 4 corresponding to Puleston's Epicenter. I say monumental because the largest architecture is concentrated there, but it is the political core of the polity where the royal court infrastructure and residences for many other elite people were located. Second, there is the *demographic core*, the blue area shown in Fig. 4 recognized by Tikal archaeologists as a zone of particularly high population density, and that corresponds to Puleston's Central Tikal. Third is the *core agricultural hinterland and kingdom* which is definitely not co-extensive with the area enclosed by the earthwork system (Puleston's Tikal), either as originally conceived or as we remapped it. This Tikal Zone, as Puleston called it, is spatially vague; it approximates our own 12 km radius shown in Fig. 10, or the Voroni diagram illustrated by Lentz *et al.* (2014). Like Sanders and Haviland, I prefer not to call any part of this landscape a "city", although the monumental core conforms to the regal-ritual/court center concept that Sanders and I long ago advocated (Sanders and Webster 1988).

THE "MANAGED FOREST" MODEL FOR THE LOWLAND MAYA: IMPLICATIONS FOR TIKAL

Having championed some variant of upland, rain-fed swidden at Tikal, I feel obliged to review a related model. Ethnographers and agronomists have long recognized that Maya farmers practice various forms of forest management or arboriculture, also sometimes called the *artificial rain forest*, *simulated tropical forest*, or *forest garden* system (Wiseman 1978; Sanders 1979: 494; Nations and Nigh 1980; Ford 2008; Ford

and Nigh 2014; Ford and Nigh 2015; Lentz *et al.* 2014).¹¹⁷ Conditions of low population density allow main fields to be widely scattered and shifted at short intervals, resulting in long fallow periods (about 7 to 20 years). “Fallow” is a misnomer, however, because old fields are still tended and planted with orchard crops, and they remain the sources of many valuable plant and animal products. Puleston’s ramon hypothesis envisioned a variant of this model (see Appendix D for details). Systems of this kind are a combination of resource diversification and concentrated main field production of staple crops, and there is no sharp distinction between cultivated and uncultivated land, as described by Boserup (1965: 13).¹¹⁸ Some archaeologists think that such variegated systems enable us to retain traditional conceptions of rain-fed, “extensive” agriculture with high energetic efficiency, and at the same time explain unusually high population densities. A recent version is the “comprehensive multicomponent land use model” proposed by Lentz *et al.* (2015: 177-178), which posits a complex mosaic of house lot gardens and orchards, some areas of irrigated crops, managed forest plots, and widespread short-fallow fields.¹¹⁹ This is similar to the system called “fixed-plot variable-fallow farming” by McAnany (1995: 78). One expectation of such landscape use is that the wide range of wild species available in the “fallowed” zones should show up in botanical and faunal remains recovered from archaeological deposits. Another is that farmers retain some form of tenure rights to a patchwork of fields in varying degrees of use and fallow. Forest garden systems unfortunately require few landesque improvements, so ancient ones are difficult to recognize in the archaeological record.

Two polar conceptions of agricultural behavior and decision-making relate to the managed forest model. The first is closely associated with the work of ethnographers who emphasize decisions made at the household level by smallholders -- farmers who have reasonably effective control of their agrarian resources.¹²⁰ Agricultural landscapes are often very patchy in terms of soils and other agrarian assets, so decision-making is said to reside most effectively with the farmers who live on or near these microenvironments and have secure access to them. What some archaeologists call the *precision agriculture* model envisions a complex mosaic of cultivation strategies adjusted to local scales of soils, slope, drainage, etc. The idea that households have (and had) effective agency in this regard is very attractive to some Mayanists because it emphasizes intricate, local agrarian adaptations (this was essentially MacKinnon’s perspective in 1981). Efforts at centralized agrarian management by kings or nobles from this point of view might result in disruptive meddling. On the other hand, if the managed forest model operated on a large scale, it might encourage long-term landesque investments (terraces, orchards, drained fields, irrigation, field boundaries, etc.) and consequent claims on land that would benefit from top-down management. The model also presupposes effective management of some zones to prevent deforestation and to preserve timber, and that there are mechanisms for the effective transfers of farm-stuffs from household to household. Finally, the intricate mosaic of local adaptations to patchy environments is prone to social and ecological disruption (e.g. population growth, drought, competition over access), so top-down control of tenure has a selective advantage.¹²¹ In other words, one could link either bottom-up, household agency or top-down elite/royal agency to the managed forest model as applied to the Classic Maya (see Appendix F for discussion). The thoroughness of the recent Lentz (*et al.*) model and its projected Late Classic carrying capacity conclusions are impressive. The authors say little, however, about how this intricate landscape was managed, although they speculate that royal administrators oversaw areas such as forest groves.

However decisions were made, managed forest systems are claimed to be minimally destructive of natural agrarian capital and highly resilient, especially in the face of environmental perturbations such as drought (see for example Ford and Nigh 2014).¹²² I know of no archaeologist who has offered a detailed and plausible model of the caloric inputs of these managed “fallow” resources for ancient populations, or the degree to which such management conflicted, in terms of time and energy, with investments in the main crops.¹²³ Whether the Maya of the central and southern Lowlands could have maintained such

managed systems by Late Classic times is questionable. Here are three little simulations incorporating several simple components:¹²⁴

- 1) A region of 1300 sq. km (130,000 ha), some 50% of which (65,000 ha) consists of well-drained upland soils suitable for Maya agriculture. I use a region three times the size of the 452 sq. km zone shown in Fig. 10 simply for the sake of argument.
- 2) A base farming population with an average household size of five people.
- 3) Each family must cultivate a main field of minimal size to generate its economic requirements.

The three simulations have main field sizes of 2, 1, and 0.5 ha respectively. Only one main crop is grown in each simulation (2-3 ha is about the limit a small production unit could handle with stone tools).

Simulations 1 and 2 assume that main fields produce predictable yields of edible maize in the 800-1500 kg per ha range. Nations and Nigh (1980) have claimed much higher yields based on ethnographic research among the Lacandon Maya.¹²⁵ It is not clear to me how they made their estimate, and I think it is hugely inflated. Nevertheless I use it in Simulation 3 to accommodate a ridiculously optimistic scenario that assumes a family would need only 0.5 ha of main field production.

Simulation 1

Population	Productive density	Area in main fields	Residual managed zone	Ratio of farmed to managed land
5000	7.7/sq. km	2000 ha	48,000 ha	1: 24
30,000	46/sq. km	12,000 ha	53,000 ha	1:4.2
60,000	92/sq. km	24,000 ha	41,000 ha	1:1.7

Simulation 2

Population	Productive density	Area in main fields	Residual managed zone	Ratio of farmed to managed land
5000	7.7/sq. km	1000 ha	64,000 ha	1: 64
30,000	46/sq. km	6,000 ha	59,000 ha	1:9.8
60,000	92/sq. km	12,000 ha	53,000 ha	1:4.4

Simulation 3

Population	Productive density	Area in main fields	Residual managed zone	Ratio of farmed to managed land
5000	7.7/sq. km	500 ha	64,500 ha	1: 129
30,000	46/sq. km	3,000 ha	62,000 ha	1:21
60,000	92/sq. km	6,000 ha	59,000 ha	1:9.8

Under conditions of low population density, there is plenty of land in all the simulations to support a forest management system that supplements main field production. In each, however, the capacity of the managed ("fallow") component of the landscape is quickly reduced in spatial scale, and almost certainly in quality, as population rises. This is true even if the productivity of the main field component does not decline. If one factored in diverse main crops or a shift to root crops as dominant staples, the

system would still involute rapidly with population increases. The situation is actually direr than it appears because no account is taken of environmental degradation and declining yields as the cultivation interval decreases, and because support of non-farmers is not included.¹²⁶ Consider the implications for the Tikal region. If the population at AD 400 were roughly 5000 people (very light density because they inhabited a large zone in the simulation), then the managed fallow system would have been effective, assuming that the preceding 2000 years or so of human use had no deleterious effects on the ecosystem. By the time of peak population of about 60,000 people, it would have been completely unworkable, especially if that population were distributed over an area much smaller than 1300 sq. km, as I have suggested above. Even in the wildly optimistic Simulation 3, a fallow system of 7-20 years is impossible and food inputs from the residual 9.8 ha are proportionately very low. Simulations using the 452 sq. km zone would obviously involute much more rapidly.

The population dynamic in all the simulations represents some 3.25 doublings in about 400 years, which is reasonable given our current understanding of the demographic history of the Tikal kingdom. Once productive densities approach 100 people per sq. km in all the simulations, there is severe stress on agrarian resources and probably internal competition over them.¹²⁷ Permanent cropping, multiple main field crops and intensive mulching, along with some landesque improvements, might push up densities (for a time) to 150 people per sq. km or so. Any perturbation of the final stage system by drought or some other extraneous factor, however, would have much more damaging consequences than a similar perturbation in the initial stage. Resilience through time is in fact low.¹²⁸ This relates back to the issue of human fecundity I raised at the beginning of this paper. If human reproductive capacity and population growth are not consciously controlled, it seems to me that the managed forest system would create the conditions for rapid population increases and an equally rapid and destructive involution of the system as suggested above. Although not often made explicit, it follows that advocates of the Maya as canny tropical forest managers probably envision some kinds of effective and conscious population control.

One final aspect of the managed fallow system deserves mention. If something like the Lacandon strategy were employed by the ancient Tikal Maya, much of the landscape was, and was to be seen to be, a human creation. There is a considerable literature on how modern Maya people perceive their landscapes (see Dunning, Beach, and Luzzadder-Beach 2012: 5, Tokovinine 2013, and McAnany 1995). Yucatec Maya use the term *kol* (the Maya synonym for *milpa*) for an established field, but it has the larger meaning of a part of the landscape that has been ordered and sanctified in ways congenial to humans. *Kax* (or *ka'ax*), on the other hand, means “natural” forest or (better) parts of the landscape that are reverting to some state of primal and dangerous chaos.¹²⁹ Whether the Classic Maya at Tikal or elsewhere shared these perceptions is unknown, but if some effective balance between *kol* and *kax* was essential at Tikal, it is clear that the former exploded at the expense of the latter after Late Preclassic times. By the eighth century, there might have been a patchwork of *kol* and *kax*, but the ordered dimension predominated. Ironically, this ordered dimension was most vulnerable to the kinds of agrarian disorder that undercut the fortunes of the kingdom and probably the physical well-being of its people.¹³⁰ This was William Haviland’s (1967) interpretation based on his study of stature at Tikal, which he thought declined during Late Classic times due to lowered nutrition (see Haviland and Moholy-Nagy 1992 for a re-analysis of these burials).¹³¹

Closely related to the managed forest model is that of the “green“, or “garden” city, long a staple theme in debates about Maya urbanism (e.g. Chase and Chase 2015). Maya urban patterns have recently been linked to the concept of the “low-density” city (see Fletcher 2009 for a comparative discussion). One problem with this perspective (and it clearly bothered Kurjack at Dzibilchaltun) is that without some kinds of obvious limits the “city” expands to take in the whole landscape: civic core, city and polity become confusingly overlapping entities (city-states indeed!). Sometimes it seems we are left with cities without peripheries, or polities without centers. Lentz *et al.* (2014) appear to take this perspective for Tikal. It neatly, if perhaps unconvincingly, avoids the traditional problem of distinguishing between centers and hinterlands.

EARLY TIKAL

Archaeologists knew little about the Preclassic anywhere before 1970, and some, like Sylvanus Morley, seemed actually disinterested. Most Mayanists at the time probably envisioned a stately progression from the small communities that used Mamom and Chicanel pottery to the glories of the Classic eighth century. Tikal was an early exception that produced considerable Preclassic remains. In his recent overview of developments in the early Maya Lowlands, Estrada-Belli (2016: 244) remarks that “The University of Pennsylvania Tikal project from 1957 to 1970 ... produced the greatest amount of data on the Preclassic of any other project”, and for a long time Tikal provided our principal window into early events and processes. Research throughout the Lowlands during the last 40 years has revealed Preclassic and Early Classic scale and complexity that would have astounded Morley and his contemporaries (see Traxler and Sharer [2016] for recent overviews). When Coe, Puleston, Haviland, Culbert and others first made their Late Classic estimates, they knew that there was impressive Preclassic architecture beneath the North Acropolis and that Tikal was one of a cluster of centers with eighth-cycle dates. Only much later did it become clear that the kingdom was a prominent player in a wider Preclassic cultural and political florescence. Evidence for widespread Preclassic vigor comes from core architecture, along with associated burials, inscriptions, and iconography, at sites such as Becan, El Mirador, Nakbe, Tintal, San Bartolo, Cival, Holmul, Cuellar, Ceibal, Cerros, El Palmar and Wakna. All these and other early polities burgeoned for centuries and there was considerable political and demographic pulsing probably related to climatic fluctuations. Many polities failed between about AD 100 and 300, possibly because of water deficiencies associated with destructive niche construction, warfare, disruption of trade, and drought (see various chapters in Lentz *et al.* 2015 for discussion of possible Late Preclassic drought). More recently, Ebert *et al.* (2017: 229) remark that “One of the most prolonged and severe droughts in recorded paleoclimate proxies from throughout the Maya Lowlands occurred at the end of the Late Preclassic period (cal 100-300AC)”.

Estrada-Belli (2011: 61; 2016: 255-256) calls this interval a Lowland “conjuncture”, characterized by political reorganization and new institutions of kingship, but also by dramatic political collapse and population declines or dislocations. Somehow Tikal not only survived, but thrived during this crisis and is now seen as “... the main hub of an immense network of political hegemony and trade connections within the southern Lowlands, for at least the first two centuries of the Classic Period” (Estrada-Belli 2011: 139). Tikal remained a dominant political and cultural force leading up to the “entrada” of AD 378, after which it engineered the establishment of a new Teotihuacan-related political order incorporating many other centers. This expansion, in turn, was prelude to Tikal’s great hegemonic struggles with the Snake kings that began in the sixth century (see Appendix F for details).

All this precociousness creates an archaeological expectation: we should detect early population surges in many parts of the central and southern Lowlands. Oddly, however, as I noted at the beginning of this paper, one labors mightily through the literature to find good extended discussions of the demographic contexts of these developments. There is a reason for this -- only in a few regions such as the Copan Valley have archaeologists acquired data comparable to those we have for Tikal that might provide insights into deep settlement and population history. Despite its venerable age, Tikal settlement research thus remains a benchmark not only for the Late Classic, but for reconstructing earlier demographic processes. Given our recent appreciation of Tikal’s early political dynamism, we might reasonably expect a large population surge at Tikal during the Late Preclassic and the first two centuries of the Early Classic.

Tikal settlement and population before AD 700

Discussions of Tikal’s dynastic history frequently lack considerations of its earlier landscape and how many people occupied it, apparently assuming that we all know what Tikal looks like.¹³² This is

unsurprising because the visible architecture, and the demographic conclusions partly drawn from it, pertain to the latest big occupation. All this predisposes us uncritically to conflate patterns from different periods. We have seen that Late Classic population estimates heavily focus on the zone defined by the earthwork system, long thought to date to the Early Classic. Somehow, this set of earthworks, supposedly erected during the Early Classic, was supposed to constrain the settlement features of a much bigger Late Classic polity. Our Penn State project showed that such conflation of settlement and emic boundary is wrong. Time conflation is admittedly very tempting, as I know from looking at maps of Becan, where I worked long ago. Familiar as I am with Becan, I still must make a mental effort to disassociate the dominant built feature there – the earthworks – from the mapped architecture, which is heavily Terminal Classic and so some 500 years later (Ball 2014).

Haviland (1963) early on noted that Tikal's landscape, along with most others that were well surveyed, are blanketed by Late and Terminal Classic surface remains, often to the point that systematic detection of earlier household occupations, and especially those of the Preclassic, is difficult. Data useful for reconstructing pre-AD 700 Tikal population scales are scant, apart from our impressions about construction efforts at the North Acropolis and the Mundo Perdido zone. Culbert (1977: 31-33) analyzed ceramic samples from 267 excavated locales, most in Central or Peripheral Tikal (unfortunately not shown on a map) in order to make some sort of spatial sense out of the distribution of the Preclassic population. One hundred and twenty-eight locales produced Preclassic sherds, but in only 35 cases were concentrations "abundant." Culbert provided no absolute demographic estimates, but the paucity of Preclassic material, even taking into account the masking effects of later deposits, surprised him. Fry's tests out in Puleston's transects produced even fewer traces: only 14 of 102 test pits yielded Preclassic sherds. Haviland (2008: 273) provides a short overview of Middle and Late Preclassic ceramic distributions.¹³³

Culbert *et al.* (1990: 112) state that reconstructing "... Early Classic population at Tikal is unusually dependent on factors that are poorly controlled.", although there is a significant increase in Manik phase ceramic frequency compared to Preclassic deposits. Nevertheless, they, along with Fry, give the two most informed estimates by calculating relative frequencies of occupations by ceramic or time phase, with the Ik period peak (AD 550-700) assigned 100%. Culbert (*et al.*) think that the Early Classic Manik Phase (AD 250-550) populations of Central Tikal and its sustaining area (i.e., the 120 sq. km zone) were respectively 33% and 50% of their Late Classic (Ik) counterparts. I apply these proportionate reductions to the two principal Late Classic estimates in Table 7, using Puleston's 57% to 43% ratio of uplands to bajo on the enclosed landscape. Resulting overall or productive (upland) population densities are very high from Manik times on, far exceeding those from any of our comparative examples except the Basin of Mexico in AD 1519 and parts of Hawai'i.

Fry (1990: 296, 299) offered his own proportionate reductions for Central and Peripheral Tikal, which I take to mean the zones so designated by Puleston. I have laid these out in Fig. 11 using his ceramic phases (why Fry confined his reconstructions to only part of the 120 sq. km area supposedly enclosed by the earthwork, not Tikal as Puleston defined it, is unclear).¹³⁴ Table 8 shows his reductions from the AD 700 estimates of Haviland and Culbert (*et al.*) for the earthwork zone. Table 9 shows the corresponding productive density estimates (based on 69 sq. km of uplands for this zone). Again, densities are extremely high at all times from Manik times until the abrupt decline in the Caban phase.

Results of our own Penn State household excavations conform well to the interpretations of Culbert, Haviland and Fry (Table 10). Kirk Straight's household test pitting in 2005 and 2006 at 52 (mostly small) rural sites produced overwhelmingly Late Classic material (Classic Ik and Imix phases -- AD 550-825). Late Preclassic and Early Classic ceramics or contexts were extremely sparse. Note that Straight's data suggest that the peak occupation occurred during Imix times, a little later than the

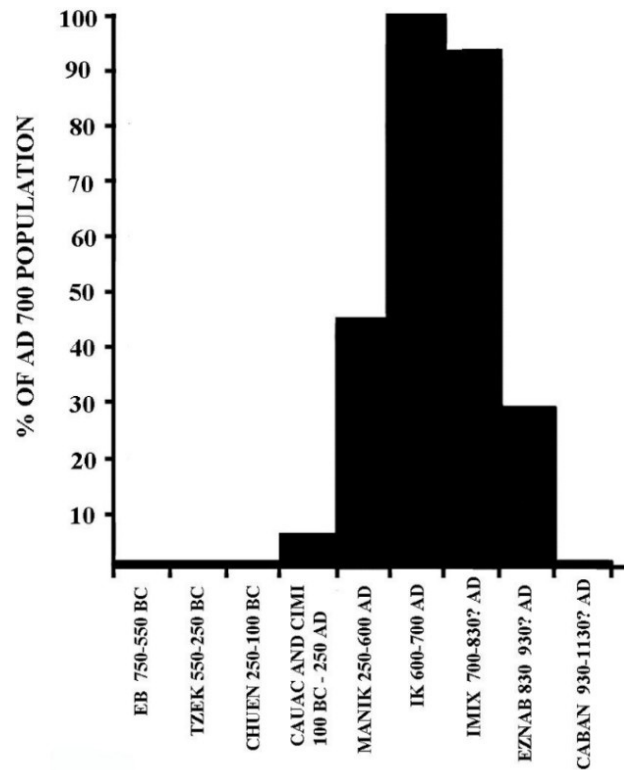


Fig. 11: Fry's conception of the demographic history of Tikal. Adapted from Fry 1990: 297.

Table 7: Early Classic Tikal populations using the estimates of Culbert et al. 1990.

Time	Central Tikal	Sustaining Area	Total Pop.	Overall Density 120 sq. km	Upland Density 120 sq. km
AD 550 (Culbert et al.)	5372	22860	28232	235	392
AD 550 (Haviland)	3861	16500	20361	170	283

Table 8: Proportionate reductions in population following Fry.

Source	Eb	Tzec	Chuen	Cauac-Cimi	Manik	Ik	Imix	Eznab	Caban
Haviland	450	450	450	2700	20250	45000	42300	13230	450
Culbert et al.	620	620	620	3720	27900	62000	58280	18228	620

Table 9: Upland productive density estimates.

Source	Eb	Tzec	Chuen	Cauac-Cimi	Manik	Ik	Imix	Eznab	Caban
Haviland	6.25	6.25	6.25	37.5	281.25	625	587.5	183.75	6.25
Culbert et al.	8.6	8.6	8.6	51.7	387.5	861	809	253	8.6

AD 700 date usually favored by Haviland, Culbert and Fry. During the century or so that relates to the dissolution of the polity, the archaeological (Eznab) ceramic trace simply returns to Early Classic proportions.

Table 10: Kirk Straight's test-pit summary.

Evidence of Early Classic occupation (at least one sherd)	10%
Evidence of Preclassic occupation (at least one sherd)	4%
Distinct Early Classic levels	2%
Distinct Ik levels (AD 550-700)	25%
Distinct Imix levels (AD 700-850)	42%
Distinct Eznab levels (AD 850-950)	2%

I take all this to mean that pre-Late Classic populations were heavily concentrated in and near the Late Classic site core or at outlying smaller centers such as Ramonal. Settlement expansion over the wider landscape seems to be quite late.¹³⁵ When the “strangers” arrived at Tikal in AD 378, they probably found a local polity of about 5000-10,000 people, most of whom lived in or immediately around the Epicenter. Two centuries earlier, near the end of the Preclassic, this whole core population might have been about 2000-3000 people, roughly the number estimated by Fry. Farmers at this time had access to much more vacant upland in the Carr and Hazard zone, something like 6 to 8 ha per house group, assuming a proportionate reduction in small structures from the AD 700 peak. Such numbers simplify carrying capacity debates and make it less likely that Tikal exerted any kind of strong political/economic influence over vast regions and outlying centers (cultural influence is another matter). That the biggest investments in monumental architecture occurred at Tikal after AD 600-700 is no surprise because the landscape was then demographically saturated, and labor most abundant and cheap. Even at the saturation point, though, the construction of Temple I utilized only about two-tenths of one percent of the estimated population of 60,000 people for several months each year during a decade or less.

Three things seem clear if we take a long, diachronic perspective on Tikal's demography. First, the population was small in density and absolute terms before Manik times. Second, the pre-Manik population was heavily concentrated in the zones Puleston called Central and Peripheral Tikal, along with the Epicenter. Third, a major surge occurred about AD 500-600 that resulted in unprecedented size.¹³⁶ Sabloff (1990: 79) put Tikal's A.D. 600 population at approximately 39,000 people, and suggested another 10,000 or so lived in the hinterlands. Here he channels Haviland (1969), but for some reason he applies the latter's estimate to a date a century or more earlier. Harrison (1999: 180) was similarly enthusiastic, saying that “By AD 600 Tikal was a substantial city with a population that could have been anywhere between 25,000-50,000 inhabitants, or more.” What he means by “Tikal” here is unclear too, but probably the uplands within the earthworks. His productive densities for this zone at AD 600 would consequently be 362-725 people per sq. km.

Early and late population surges

Tikal had a small regional population at least until the Late Preclassic, even though farmers had been on the landscape for more than a millennium by that time, probably practicing some kind of rain-fed swidden agriculture supplemented by wetland cultivation to hedge against drought (see Appendix C). The long period of very low overall population up until about 1000 BC is one of the several major demographic problems that needs to be explained, but is beyond the scope of this paper. It remains a quandary, especially given the early appearance of maize as a staple crop and later intervals of rapid population increase. In my 2011 paper on maize domestication I speculated that highly productive forms of maize were not present before about 1000-800 B.C. We now know that there were dry and fluctuating climatic conditions during much of the Early Preclassic (Ebert *et al.* 2017) that might have been made heavy dependence on agriculture risky, whatever the productivity of the staple crop.

Unexpectedly missing is any sign of a major population surge commensurate with current ideas about Late Preclassic and Early Classic political florescence. Is all this evidence of absence, or absence of evidence? Either our current demographic reconstructions are flawed, or the demographic scale of all this early activity at Tikal and elsewhere was much smaller than we imagine (the explanation I favor).

If Maya kingdoms thrived for centuries, their populations must have increased significantly, either through intrinsic fertility or through other kinds of recruitment.¹³⁷ It follows that we have to back down their early populations markedly from their Late Classic peaks. John Wingard (2013: 146-147) recently came to the same conclusion when modeling population trends in the Copan Valley. If we imagine that early Maya populations were almost as large as later ones, we are faced with a paradox – thriving kingdoms that were somehow demographically stable, but whose centers and monuments only achieved maximal size in the Late Classic. If we retain the early Tikal population estimates shown in Table 7, but assume that the landscape utilized by farmers was our 452 sq. km zone, we lower overall densities to 45 to 51 people per sq. km, and upland densities to 53-74, easily within the limits of some variant of rain-fed swidden. Unfortunately, there are no long sediment cores from Tikal from which to assess the environmental effects of such densities (but see below for some short ones). Cores from Copan, however, suggest that even very small Maya populations could alter vegetation in substantial ways (Rue, Webster and Traverse 2002; Webster, Rue and Traverse 2005). My suspicion is that by Manik times the uplands around Tikal were already degrading.

The demographic surge evident at Tikal after about AD 500 is roughly contemporary with others elsewhere in the central and southern Lowlands, where local populations often doubled in a century or so.¹³⁸ Even before settlement data were widely available, this surge, and the resulting Late Classic population peak, were exceptional (see Willey 1977: 397-398). Sanders (1962: 11) marveled that “... the Classic Period witnessed a spectacular population explosion that resulted in a denser population than any part of Mesoamerica except certain Highland regions where intensive agriculture was practiced...” More recently, Lentz *et al.* (2014: 1) characterize this surge at Tikal as “meteoric.” Harvard and Penn State surveys detected similar spurts at Copan, and they also occurred at Piedras Negras and at Dzibilchaltun after AD 700.¹³⁹ Like Estrada-Belli’s “conjuncture” several centuries earlier, this Late Classic surge has political implications. Simon Martin (2016: 529), for example, notes its rough correspondence with changes in emblem glyph forms, and many archaeologists have documented increasing regional ceramic diversity and more intense warfare.

How real is the late surge detected at Tikal? Haviland realized the importance of contemporaneity in making demographic calculations from mapped household remains, and he thought the key was adequate testing of a large sample of them. His own research convinced him that “... it is unlikely that any Early Classic and Pre-Classic structures are to be found on the site map to upset any population estimate for Late Classic times” (Haviland 1965: 19).¹⁴⁰ That is, the Carr and Hazard map presents us with the image of an overwhelmingly Late and Terminal Classic Maya landscape and a population that is greater than at any previous time. The map serves as comparative grist not only for other Late Classic polities, but also for much earlier Preclassic landscapes elsewhere. Antonia Foias (2013: 9) says that El Mirador was the urban capital of a Preclassic state and that “At sixteen sq. km and approximately 50,000 residents, it rivaled Late Classic Tikal in overall settlement and population size.” Her comparison is in fact circumspect. Her figures indicate an El Mirador population five times (or more) bigger than calculated for the 16 sq. km Carr and Hazard map, with a density of 3125 people per sq. km. Needless to say, no one has ever seen an El Mirador settlement map comparable to the Carr and Hazard one, much less one with good sampling of structures of all scales. Extravagant population figures for El Mirador seem based largely on the scale of its monumental architecture, as discussed in Appendix B.

For many years I have identified the late population spurt that occurred at Tikal and elsewhere as one of the most perplexing puzzles of Maya culture history (Webster 2002; see Appendix G for further comments). Anthropologists and demographers have long debated whether large, pre-modern human populations lacking effective contraception naturally tended to increase under resource-rich and low-risk conditions, or whether people controlled their fertility and hence population growth. Most anthropologists I know take the position, as I do, that conscious efforts to stay below carrying capacity were ineffective. George Cowgill (1975, 1976: 60) thought otherwise:

“... if population increase means only that, on average, people have to work harder to meet their food requirements, and if there is nothing one can do with food in excess of one’s own household’s nutritional requirements except store it as long as possible for insurance against possible future shortages, and eventually to let unused food rot, then there is no incentive for population growth. It seems likely that this, indeed, was the situation throughout most of the Pleistocene, and that it has much to do with why both population growth and cultural innovation were so slow then. In the broadest terms, the key seems to be the presence of institutional, ecological and technical factors that enable one to convert some food (or other resources) that one has produced or procured into something else that one desires” (1975: 516).

More specifically, Cowgill rejected the idea that the Late Classic Maya population spurt reflected some natural propensity for human populations to increase, and attributed it instead to intensified political and economic problems that arose in the Lowlands after the decline of Teotihuacan.¹⁴¹ He argued that intensive warfare motivated rulers to encourage population growth for political and military advantage. Population pressure did not cause warfare, but was the result of new forms of militarism (the “incentive”).

Cowgill’s argument aside, four explanations (not mutually exclusive) come to mind for the population spurt at Tikal and elsewhere. First, there might be a sampling problem: we simply have better access to the latest occupations and these spurts are illusions.¹⁴² Second, it might represent population movements that concentrated people around old sites like Tikal in ways not seen before, but that did not reflect major overall increases in larger regions. Third, it might represent higher fecundity and/or decreased mortality of local populations for unknown reasons that increased indigenous population growth rates. Finally, long-term rates of increase might have remained unchanged, but resulted by the sixth century or so in major *aggregate* increases we can easily detect. This last explanation was our conclusion for Copan (Paine 1996; Paine, Freter and Webster 1995). I think we can discount the sampling issue for most well researched sites and regions, including Tikal. Of these several explanations, only the third seems to me to warrant the description “meteoric.” Nevertheless, the spurt appears to be real, and Culbert (1974), like many later Mayanists, saw it as demographic overshoot heavily implicated in the Classic collapse.

As I said above, the most obvious reason to pay attention to demographic history is that it provides the context within which we reconstruct institutions and events. Imagine that the famous “strangers” who arrived in January of AD 378 (Stuart 2000; Guenter 2002: 269-274) encountered a polity of 30,000 people scattered over several hundred sq. km. Imagine alternatively that the whole Tikal kingdom at the time had only 6,000 people clustered much closer to the Epicenter. Our conceptions of the character of the intrusion and its associated political dynamics would be radically different under these two scenarios.¹⁴³ A few centuries earlier the putative “first” Tikal king Yax Eb Xook, if correctly identified from retrospective inscriptions (Estrada-Belli 2011:120-122), ruled an even smaller polity. The purported troubles at Tikal between AD 557 and 692 have often been associated with population decline. Moholy-Nagy (2016) argues instead that during this “big” hiatus Tikal in fact prospered. I agree with her, but curiously she pays little attention to the population increase that in late Manik and Ik times would

bolster her case. Claims of expansive political power provide another example. Adams and Jones (1981: 318-319) stated that early Tikal dominated a political hinterland and many satellite centers over an area of 100,000 sq. km, extending into Belize and the Calakmul and the Chenes/Rio Bec zones to the north. Joyce Marcus (1992: 406-407) thought that early Tikal rulers directly administered a more modest, but still impressive realm that included many other centers scattered over 30,000 sq. km (by AD 700 this great polity was reduced to 10,000 sq. km as its dependencies asserted their independence). How the apparently modest demographic scale of Tikal squares with such expansive models is difficult to imagine.

More important is that some grasp of population dynamics helps us to understand adaptive (or maladaptive) processes that operated on the agrarian landscape (see Webster and Murtha 2015). Tikal's demographic history is complicated because we cannot assume that major population trends represent only the intrinsic fertility and mortality regimes. At various times, Tikal might have received migrants from elsewhere, and at other times lost inhabitants through the reverse process (see Rice and Rice 2004 for discussion of late migrations in the region). Price *et al.* (2014: 44) list Tikal as one of the Mesoamerican sites (along with Copan, Kaminaljuyu and Teotihuacan) that exhibit large strontium bone isotope variation values indicative of high proportions of "foreigners". In-migration at Tikal seems most likely after the collapse of the Mirador Upland populations around AD 100-150, and after resurgent Tikal's conquest of Calakmul in AD 695. Out-migration is most likely in the troubled times of the sixth century, although if so it occurred on a very modest level. Doyle, Garrison and Houston (2012) think that El Zotz, about 20 km west of Tikal (Fig. 11), might have been a donor region at the beginning of the Late Classic. Doyle (2017: 127) notes that abandonment of the big Preclassic El Palmar center near El Zotz sometime around AD 1-250 coincides with increased growth at Tikal. If such movements occurred on any scale, they have left no clear traces in the Tikal ceramic sequence (or anywhere else) so far as I know, so I will proceed as if population dynamics were mainly internal, as do other scholars who address the topic.¹⁴⁴

The overall demographic picture for Tikal is one of very low population and slow growth before 800 BC, a slight uptick during the Late Preclassic and Early Classic, then rapid increases after AD 500 or 550, a pattern widespread in the Lowlands but difficult to explain. Essentially the same pattern was posited by Turner (1990: 321), who thought that at all times before AD 600 overall densities around Tikal were always well within swidden capacity (32 people or fewer per sq. km). The broader implication is that Tikal's socio-political evolution, its attendant institutions and ideologies, and its early regional prominence, wars, alliances, and cultural influence, all occurred under a small absolute population regime and comparatively "un-intensive" agriculture until sometime late in the Early Classic. If Tikal's rulers or other elites ever interrupted an essentially folk agrarian system and assumed some sort of managerial control over it, they probably did so sometime after AD 550.¹⁴⁵ I think the elite, and particularly the royal component of Tikal society, were by nature conservative and tended to perpetuate a status quo that so obviously benefited them.¹⁴⁶ They probably still did so as their agrarian systems become poorer, precisely when they needed to innovate. After a millennium or so of tolerable demographic and political scale, the population surge that occurred over just a few generations overwhelmed royal-elite managerial capacities – a case of adaptive lag.¹⁴⁷ As David Stuart (1993: 336) once remarked, "... the whole Late Classic sees a single, long-term demise of the institution of Lowland Maya rulership." If by this he means that Late Classic kings, given the long tradition of rulership they inherited, could not adequately adjust to new circumstances, I could not agree more.¹⁴⁸

Many years ago, Joseph Tainter (1988: 4) presciently noted that "A society has collapsed when it displays a rapid, significant loss of an established level of political complexity." Mayanists have long agreed that rapid political collapse of dynastic rule was conspicuous, although by no means synchronized, at scores of polities in Terminal Classic times. I think most kings were overwhelmed by challenges that their traditions and experience made them ill equipped to meet. Where I depart from Tainter is in the

presumption that the political collapse resulted from the disintegration of "... the overarching structure that provides support services to the population" (Tainter 1988: 20). If by "support services" he means effective agrarian management, I doubt that Maya kings ever had such roles (see Webster 2005 for a discussion). Durkheim's old conceptions of "organic" vs. "mechanical" solidarity are pertinent here, and I suspect that for the Maya the latter are most applicable. All of this leads me to some conclusions about Maya socio-political organization that I discuss in Appendix F.

BIASES AND LIMITATIONS OF THE TIKAL RESEARCH AND SOME COMPARISONS WITH COPAN

I argue for small populations with comparatively low densities at Tikal and other regions, but I did not always hold this view. Many years ago, influenced by the Dzibilchaltun, Coba, and Tikal settlement research, I accepted overall densities in the 400-500 people per sq. km range (e.g., Sanders and Webster 1978: 295). What changed my mind were the Harvard, Penn State, and other survey projects at Copan between 1977 and 1989, along with my later work at Piedras Negras and at Tikal itself. These projects also convinced me that our window into the Tikal kingdom's demographic history and settlement distribution is more limited and distorted than we usually imagine. Some comparisons with research at Copan (Fash 1983; Fash and Long 1983; Baudez 1983; Freter 1988; Webster, Freter and Gonlin 2000) will make this clear.

The most obvious bias at Tikal is small surface-sample size. Puleston's total survey coverage is considerably less than we later managed at Copan but in a much larger potential survey universe.¹⁴⁹ Our Penn State Tikal project added 7 sq. km to Puleston's total, but coverage (including the 16 sq. km of Central Tikal) still amounts to only about 77% of that available for Copan (48 sq. km vs. 62 sq. km).¹⁵⁰ Put another way, at Tikal, we have a smaller sample of a larger agrarian and political landscape. Of course the problem that Puleston faced, as we did later at Tikal, is just how to define any limits to the "rural" or "intersite" area.¹⁵¹ Puleston thought the earthworks resolved this problem, but our own research shows otherwise, at least for Late Classic populations. The Sustaining Area surveys radiated out from Tikal's monumental core about 10-11 km.¹⁵² 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 (Freter 1988; Webster Freter and Gonlin 2000). The number of mapped Copan structures (4553 when we finished our work) is greater than those mapped at Tikal by the Penn projects (3856), and the pulse of recent Copan research has added many more. By comparison then, we have data applicable only to a small radius around Tikal. Haviland and others noted shortcomings with the Carr and Hazard map. In the roughly comparable zone at Copan, the 24 sq. km Copan pocket, settlement is much more comprehensively mapped (Fash 1983; Fash and Long 1983).¹⁵³ We also have an insufficient grasp from actual mapped settlement components of what the overall population around Tikal might have been in a region of 400-500 sq. km, a reasonable estimate of its whole sustaining area or core realm, although simulation provides plausible insights. Ford's work partly compensated for this limitation on Tikal's eastern periphery, but we need surveys to the south and west. The latter direction is particularly important because of the natural corridor (a link between two drainages) leading in this direction to El Zotz and other centers recently investigated by Steve Houston and his colleagues (see below Fig. 13).

One glaring deficiency of Tikal settlement research is extremely limited test excavations of rural sites. Even when we add our own sample to Fry's, there are only about 159 test excavations beyond Central Tikal, a woefully inadequate number. By contrast, the Baudez PAC I and Penn State PAC II projects completed 870 test excavations (182 and 688 respectively). Another problem is the relative paucity of extensively excavated outlying sites beyond the main civic precincts of Tikal. Although Haviland, Becker and others did sizable exposures at some Tikal household groups they did not completely strip most of them, and few were very far away from Central Tikal. Copan has much more extensive lateral

excavations, and its chronology is more well-controlled by very large ceramic inventories expressed as actual sherd counts (see Freter 2004 for maps of site/household distribution for the periods between AD 700 and 1150) and an abundance of chronometric dates (Freter 1988; Freter and Abrams 2016). Despite Haviland's pioneering biometric efforts, no large, systematic isotopic studies of diet using skeletal material exist for Tikal as they do for Copan (Gerry and Krueger; Reed 1998; Whittington and Reed 1997; Kennett and colleagues in progress). Finally, at Tikal we lack long sediment cores similar to those we have for Copan, Tikal researchers such as Lentz et. al (2014) consequently depend on cores from the Peten lakes to the south (although these authors illustrate two shallow profiles from near Tikal that we shall discuss shortly, both of which are mainly quite late).

Having championed Copan as much better known than Tikal, let's look at some density figures for that polity. Overall Late/Terminal Classic densities were on the order of 55 people per sq. km for the drainage as a whole (roughly 500 sq. km in Honduras), but with striking concentration immediately around the Copan Main Group, where densities might have reached 8000-10,000 over a one sq. km zone. The Honduran portion of the drainage is pretty well covered. We know less about settlement in the downstream Guatemalan section of the river, but it seems to be rather sparsely populated (Murdy 2009). Productive densities on the valley's core cultivated landscape were about 134 people per sq. km and did not endure for more than a century or two. These levels conform comfortably to our several comparative examples and to Griffin's simulations, but are dwarfed by many traditional estimates for Tikal's hinterland.

Our demographic reconstructions for Copan used our sample 4553 mapped structures (Webster and Freter 1990; Webster, Sanders and van Rossum 1992; Wingard 1996, 2013). I think around AD 800 Copan's king probably ruled over about 18,000 to 20,000 people.¹⁵⁵ This is our peak plausible population as figured several ways.¹⁵⁶ The structure/population ratio is thus not quite 1:4 (23:100). Tikal is much more problematical. I personally doubt Puleston's projected total of almost 14,000 structures on his whole earthwork-bounded landscape, but if one accepted his numbers, and then went out on a limb and applied the Copan structure/population ratio, the result (about 56,000 people) falls squarely between Haviland's and Culbert's traditional figures for the polity. Either of their estimates is believable, but only if applied to a much larger region than Tikal's defunct earthwork zone.

Some years ago, Henry Wright (2007) called for more agent-based analyses of archaeological materials and characterized most approaches as overly dependent on old-fashioned culture history and culture process perspectives. Wright acknowledges that very seldom are individual agents and their intentions visible except superficially (e.g., real people obviously decide to bury someone, dig a grave, and place a corpse in it, perhaps along with mortuary offerings, etc.). Only under unusual conditions, and particularly when writing and other kinds of detailed symbolic information is available, do individual actions and intentions emerge in some more specific and personalized sense. Wright remarks about the kind of data reviewed above that "The accumulations of settlement debris found in archaeological surveys and the associated sets of artifacts and ecofacts found in excavated loci are treated in the aggregate, at worst as records of culturally normative behaviors and at best as records of inter-correlated variation in past cultural processes" (Wright 2007: 175). Sympathetic as I am to Wright's concern with agency (and his adherence to current fashion), I plead guilty of presenting and critiquing the Tikal data partly as history and partly as process. For Tikal, Copan, and the Classic Maya generally, we possess a wealth of inscriptions and iconography that identify individual agents (at least elite ones) and some of their actions. None of it tells us how or if they (much less ordinary people) envisioned, controlled, and managed subsistence resources in practical ways (but see Freter 2004 for a perceptive discussion of Copan). Maybe omission of such commentary is itself revealing. In any case, I think the historical-processual record at Tikal, deficient as it is, provides a framework within which we can better appreciate the varied and competitive actions of agents, whether farmers or kings.

HOW MANY MAYA LIVED IN THE CENTRAL AND SOUTHERN LOWLANDS DURING LATE AND TERMINAL CLASSIC TIMES?

I began this paper by characterizing Tikal as a benchmark in our conceptions of Maya demography, both for Late Classic times and for reconstructing earlier populations. While writing it, I came across many casual estimates of other local or regional Maya populations that are unattributed, in the sense that one cannot trace them back to any detailed descriptions of residential samples or methods of generating the numbers. There are actually very few places in the central and southern Lowlands that provide such basic information on an appropriate scale. Tikal is one of them and Copan is another. Many archaeologists have concluded, as I have, that the Haviland/Culbert long-standing conceptions of the size of Tikal's Late Classic population are reasonable (although not its density or distribution). For a very long time, their estimates have thus served as standards that Mayanists often use, consciously or not, to make demographic assessments of other centers or regions where residential surveys and test pitting are much more limited. For example, I have found several estimates that put the population of Calakmul at 50,000 people, often without attribution -- see Delvendahl (2008: 79) and Vance (2016: 82). Needless to say there are no settlement data or analyses for Calakmul comparable to those published by Tikal archaeologists (Delvendahl 2008: 68-81 gives a harsh critique of the Calakmul data in hand). Just where do these and similar numbers come from? I suspect that many are simply projections: Calakmul is roughly comparable to Tikal, so it is plausible that it had a similar population.

I think this kind of projection is perfectly reasonable if done judiciously, and I will now do it myself. My Tikal exercise has made me rethink the scale of the peak population of the central and southern Lowlands between AD 700 and 900.¹⁵⁷ As we saw earlier, Morley blithely imagined that big centers like Tikal might have had 200,000 dependent farmers.¹⁵⁸ At the other extreme, William Bullard thought his own surveys showed that most centers had sustaining populations in the range of 5750 to 8625 people.¹⁵⁹ For years, I favored an estimate of some 3,000,000 people for the population peak of the central and southern Lowlands at AD 800, in line with the 2,663,000 to 3,435,000 calculated by Whitmore *et al.* (1990: 35). Now I think there were far fewer.

Table 11 shows my current order-of-magnitude guesstimates about the populations attached to some well-known Late and Terminal Classic Maya realms and/or regional constellations of them. Some of them (Becan, Copan, Piedras Negras, Tikal) I know pretty well. Totals for others represent my *maximal* informed guesses, sometimes based on discussions with other archaeologists.¹⁶¹ My list shows only the most prominent Late Classic centers and regions, including the big "supersites" that have so long impressed archaeologists. Anyone wishing to enlarge the list can add many more examples -- El Peru, Holmul, La Corona, Motul de San Jose, etc. -- but the aggregate increase in total human numbers would be increasingly small.¹⁶² Looking at my admittedly impressionistic total, I don't see how such additions, if on scales similar to mine, could inflate the total population of the central and southern Lowlands much past a million people, even assuming that all the population peaks were synchronized, which they clearly were not.

Now take my little exercise to an absurd extreme, using as a baseline Culbert's core Tikal supersite population of about 60,000 people that I and most other Mayanists find non-controversial. Suppose that populations in all the realms and regions on the list hit their peaks simultaneously and that each of them had 60,000 people. The result for the central and southern Lowlands works out to a population of about 1,200,000.¹⁶³ If we let those enthusiasts who think that Calakmul, Coba, or Caracol were more populous than Tikal add a few hundred thousand more, it still is difficult to argue that there were more than a million and a half people on the Late/Terminal Classic landscape -- fewer than Turner thought lived in the Tikal region alone.¹⁶⁴ Although northern Yucatan is not included in my analysis, I doubt that its addition would boost the whole Classic Lowland population to more than 2,000,000 people.

Table 11: Populations of major polities.

Realm/Region	Total Peak Population
Copan	20,000
Tikal	60,000
Piedras Negras	10,000
Becan	10,000
Palenque	15,000
Yaxchilan	10,000
Calakmul	50,000
Petexbatun region	30,000
Quirigua	8000
Uaxactun	10,000
La Milpa	20,000
Southern Belize	60,000
Tonina	10,000
Bonampak	6000
Caracol	60,000
Coba160	50,000
Yaxha	40,000
Naranjo	20,000
Altar de Sacrificios	15,000
Ceibal	15,000
Total	519,000

Figure 1 shows an area of roughly 150,000 sq. km. If we apply a population range of 1,000,000 to 1,500,000 to the entire zone, the overall density range is 6.7 to 10 people per sq. km. These of course are very low numbers that do not take into account the many parts of the landscape that by Late Classic times were unused or unusable, for whatever reasons. We (like Puleston before us) can roughly reconstruct the size of the usable Late Classic landscape for Tikal (see above Tables 5 and 6) and I calculate productive densities there at 137-175 people per sq. km at the demographic peak. In restricted locales in the Lowlands with drained fields, terracing or other kinds of intensification, densities might have been even higher, at least for a time. Unfortunately, we have no good estimates of potentially usable land for the whole region of 150,000 sq. km. Table 12 shows the productive densities associated with three population scales and three very general estimates of the proportions of the landscape that were unusable or otherwise unavailable for agriculture by the ninth century. I throw in the 2,000,000 figure not because I think it is reasonable, but simply as a sop to those who might claim that I have vastly under-counted the Late Classic Maya. To get to this 2,000,000 figure I have essentially multiplied the aggregate totals shown in Table 11 by a factor of four (i.e., *we have somehow missed almost three quarters of the polities or regional populations!*). All the iterations nevertheless yield very low overall densities.

Table 12: Productive densities in relation to available land.

Total Population	25% unavailable	33% unavailable	40% unavailable
1,000,000	8.3	9.7	11.1
1,500,000	12.5	14.11	16.6
2,000,000	17.8	19.9	22.2

As I said above, overall densities tell us very little, although if we calculate them for large areas they do enable us to gauge the demographic scale of polities. I think that the central and southern Maya Lowlands always had low overall densities, spatially punctuated by zones of much higher concentrations. Some of these zones were durable for long periods, and the core Tikal kingdom was one of them. Our main demographic enterprise should be to scrutinize the patches that achieved and maintained high densities in order to explain how they emerged, and in some cases abruptly declined, why they existed, and how they affected their landscapes. This is what we, along with Lentz *et al.* (2014, 2015), have done for Tikal. The temptation to extrapolate from known high-density zones (which often turn out to be less dense than imagined) to the larger un-surveyed landscape is one reason we overestimate the Late Classic population.

The figures shown in Table 12 are not new. They agree in order-of-magnitude terms with the sober Maya assessments of Brainerd, Hester, Thompson, Sanders, and others already cited, and they place the Maya much more convincingly in line with the other civilizations considered here. For example, the upper end of my own overall range (10 people per sq. km) puts Maya densities at about 21% of those that characterized the much richer Nile Valley in Old Kingdom times, a proportion that seems reasonable to me. Like my cautious predecessors, I am thus a minimalist (although with the benefit of several generations of archaeological hindsight they did not have). I now suspect that the total population of the central and southern Maya Lowlands as shown in Fig. 1 was 1,000,000 people, give or take a few, around AD 800. This is less than half the Whitmore *et al.* (1990) estimate, and it puts Late Classic Maya populations in alignment with the ethnohistorically known population of northern Yucatan.¹⁶⁵ I also think that the demographic contexts of the Late Preclassic and Early Classic dynamism we have detected since 1970 were either surprisingly modest, or else our methods of population reconstruction are wildly unreliable. Many Mayanists today will find these numbers implausible and even preposterous, but a professional lifetime of thinking about demographic issues forces me to take them seriously.

DISCUSSION AND CONCLUSIONS

Throughout much of my academic career, Tikal was an exemplar of many things Maya, particularly as a model of Late Classic settlement character and demographic scale. We long ago learned that sites such as Palenque, Piedras Negras, Yaxchilan, Copan and many others departed from the Tikal pattern. Estimates of Tikal's population by contrast have weathered very well, as have their implications for Late Classic demographic scale throughout the central and southern Maya Lowlands. Implications for earlier times, and especially the political (and demographic?) collapses at the end of the Preclassic, are more puzzling.

My low Late Classic population estimates represent reversions to early ones commonly, but not universally, thought reasonable by Mayanists prior to the 1970s, and especially before the watershed year of 1973 that marked the publication of Puleston's dissertation. An avalanche of similar surveys shortly followed, accompanied by vigorous claims about alternative Maya agricultural systems that challenged the maize- staple/swidden model. Looking back over the literature, I find few low or moderate estimates after 1973. Large populations and high densities became the conventional wisdom. Oddly, the estimates by Puleston, Haviland, Culbert, Fry, Sanders and others for Tikal, although variable and sometimes confusing, hold up pretty well in the face of this enthusiasm. I think Haviland's and Culbert's estimates for the peak population of Tikal -- 45,000 and 62,000 people respectively -- are perfectly acceptable, although I incline toward Haviland's end of the range, as do Lentz *et al.* (2014, 2015). When this peak was reached is unclear. Probably the best we can do is to put it sometime in the eighth century, although later in that interval seems most sensible to me. This core population, however, was distributed quite differently on the landscape, and was less concentrated than most Tikal scholars have envisioned. Earlier demographic history remains murky, but the kingdom probably had low Late Preclassic and

Early Classic populations that I suspect were heavily concentrated near the site core and around some emergent neighboring centers. The dramatic events detected in inscriptions and art probably played out under contexts of surprisingly modest demographic scale.

With regard to the central and southern Maya more generally:

- 1) There is no reason to postulate large Maya populations on the basis of architectural output.
- 2) What we know about the complexity of Maya sociopolitical organization does not necessarily imply large populations.
- 3) Current evidence for Maya diet and for agricultural intensification does not justify conceptions of large, dense populations.

Population estimates at Tikal and elsewhere have benefitted and been distorted by the settlement data that archaeologists have so diligently collected over the last 50 years. Such data are impressive, especially the scale of visible and accessible household remains, when compared to those available for almost any other ancient civilization. Unfortunately, the algorithms we apply to them to grind out population figures usually result in excessively high numbers. There are many reasons for this, the most important being insufficient sampling, poor chronological control, lack of complete horizontal excavations, unrealistic assignment of people to floor space, to structures, or groups, and incautious extrapolation from known regions to unknown ones. As an example, archaeologists sometimes assume that structures dated to a ceramic phase were simultaneously occupied by resident groups of some standard size.¹⁶⁶ This was essentially Haviland's conclusion for Tikal, and echoed by MacKinnon and others. I find this very unconvincing, especially when the demographic implications vastly exceed expectations from other information. Kurjack rethought his initial Dzibilchaltun calculations many years ago for just this reason.

Ancient Maya landesque transformations, residential, agrarian, or otherwise, are rarely deeply buried, and so (vegetation aside) tend to be quite visible on the surface, especially in the interior lowlands. This situation creates particularly rich palempsests of features that might or might not be contemporary. "Palempsest" usually implies some final, visible layer of contemporaneous information overlying faint traces of earlier material. My point is that Maya landscapes exhibit particularly rich mixtures of non-contemporaneous materials, not faint ones, and if we do not sort these all out properly, our demographic inferences are deeply flawed. The new LiDAR images from around Tikal will probably show just such complicated arrays of landscape features. I predict that long before we understand what we are seeing, there will be new claims for enormous Maya populations, just as happened in the mid-1970s.

I think the best we can do is to generate reasonable estimates using several lines of information, rather like triangulating in on an unknown radio signal:

- 1) Settlement data derived from extensive survey, abundant test pitting, and large horizontal exposures.
- 2) Scale and output of monumental construction or other communal efforts.
- 3) Landscape/soil characteristics and reasonable reconstructions of agrarian systems and outputs.
- 4) Isotopic (or other) reconstructions of dietary intake and staple production from skeletal or soil data.
- 5) Modeling of resources – food, water, wood, etc. -- necessary for populations of various scales.
- 6) Comparisons with other ancient and ethnographically known societies.

Each of these elements has its own problems and there is no way to combine them into some neat algorithm that spews out consistent numbers. Balancing them all and using some modicum of demographic and anthropological common sense is the best we can do in the absence of real census information. I have taken this approach for Tikal and I am encouraged that the results simplify or eliminate several vexing problems, rather than exacerbating ones about which we have long wrangled. On the other hand, a big problem still exists – the lack of any perceptible Late Preclassic or Early Classic pulse of population growth congruent with the dramatic events and architectural vigor now documented for those times. Our methods might be deceiving us, but I suspect that the demographic contexts of all this activity were very small.

We need not delve further into the urban vs. non-urban debates that were stimulated by the Carr and Hazard map, but Haviland, Sanders, Coe and others were probably close to the mark in thinking that roughly 10,000 people lived there in Late Classic times. We could tweak this figure up or down a bit, but it seems very reasonable as an order-of-magnitude estimate. As all these archaeologists knew, the local agrarian resources – uplands that might somehow be productive – were insufficient for such a population unless one championed something like the ramon/garden perspective favored by Puleston, MacKinnon and others. No doubt the density of central Tikal seemed high at the time, but we have since learned that the cores of Copan, Piedras Negras and Palenque held even more concentrated populations (see Nelson 2005: Fig. 4.1; Liendo, López and Campiani 2014: 111). And revealingly, centers like Palenque and Piedras Negras have much less internal space for gardens or orchards than Tikal.

Tikal archaeologists (e.g. Culbert 1973: 72) and others advanced two explanations for the seeming imbalance between population density and food production capacity in mapped zone:

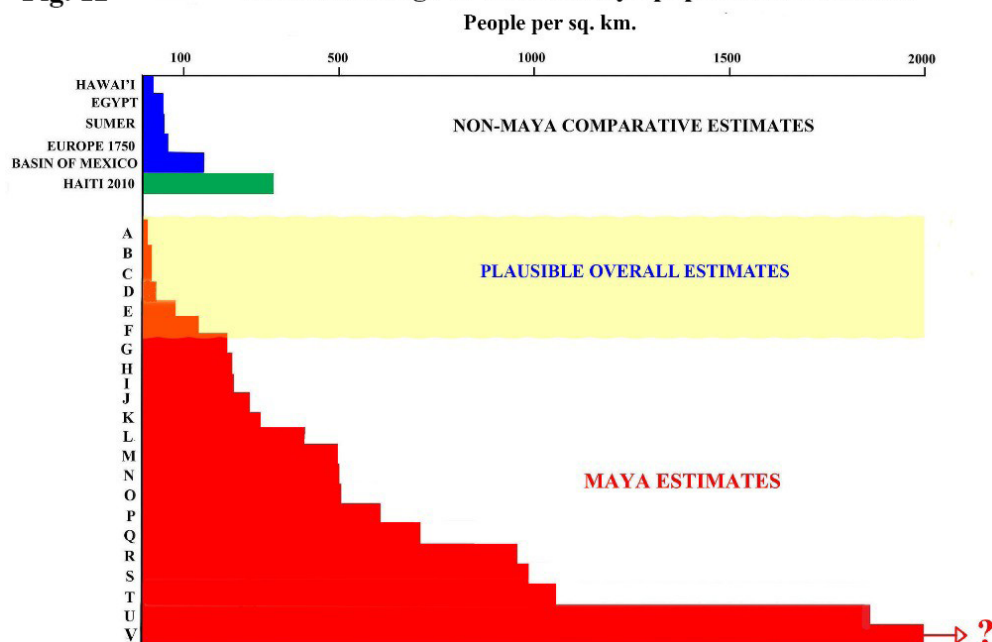
- 1) Reconstructions of the subsistence system were incorrect (Culbert's favorite). Staples were root crops (Bronson 1966) or, as Puleston imagined, ramon arboriculture, not maize.¹⁶⁷
- 2) Food was imported in bulk over considerable distances.

These two assumptions structured much of the subsequent research of the University of Pennsylvania and other institutions, and had the salutary effect of turning attention to ecological-landscape studies and outlying settlement surveys. Instead of confirming one or both of these explanations, however, the new research exacerbated them. Few archaeologists (Sanders was one) entertained a third possibility: the population reconstructions were simply too high.

Our Penn State research, supported by that of Griffin, allows retention of the traditional and non-controversial population figures of Haviland and Culbert (*et al.*), but distributes them over a larger region of 452 sq. km. The effect is to lower overall densities to about 100-175 people per sq. km. Although the high end of this range still strikes me as excessive, it brings Tikal and the Classic Maya generally into reasonable agreement with the Basin of Mexico and Hawai'i, and much reduces the unlikely contrast with the other comparative examples (Fig 12). Such densities are within the range of various systems of swidden agriculture (Dumond 1961) and they compare favorably with the results of Griffin's (2012) recent simulations. One could assert that Tikal's farmers used many kinds of intensification on the Tikal landscape, but that we have just not recognized them. I prefer the Occam's Razor explanation that we haven't found them because they don't exist, at least in large numbers and obtrusive ways (the new Tikal LiDAR images should tell). We do not have to imagine a complex but hidden agro-engineered landscape if the Maya were less numerous than we think. Survey and excavation elsewhere reinforce this conclusion. For example, Piedras Negras dominated a landscape with few obvious signs of landesque agrarian features and a population that even at its Late Classic height was within the capacity of swidden maize production (Nelson 2005: 156-161).

Fig. 12

Plausible range of overall Maya population estimates



Looking back over all the Tikal projects, what forcibly strikes me is the discrepancy between the view from the epicenter and the view from the periphery. Inscriptions and architectural stratigraphy at the site core suggest a much longer history than the outlying settlement remains, and dramatic cultural and political influence far earlier than expected. Judging from the epicenter, Tikal was an active, vital political capital long before people settled its hinterland with any intensity. Peripheral excavations are so minimal at Tikal, though, that the existing sample might fool us. Nevertheless, if by some weird quirk research at Tikal had progressed from the periphery to the center, the much greater time depth, complexity, and political events apparent there would astound us.

Boundaries and limits

These reflections bring me to the topic of political or territorial boundaries, which I think we might be seeking in vain, as suggested by the linguistic analyses of Tokovinine and Quezada (but see Prudence Rice 2004 for a spirited argument for ritual as opposed to territorial boundaries tied into calendrical cycles). I take boundary to mean the intended or desired spatial limits of political jurisdiction (as opposed to political, cultural, or religious identity, which many polities clearly had). Once the Tikal earthworks famously seemed to be just such an emic boundary on the ground.¹⁶⁸ We have seen that Tokovinine (2013: 71) identifies several expressions in the Classic inscriptions, such as *kab*, that might refer both to land in a general sense and to land attached to a community or ruler or dynasty. Others, such as *Huxte' Tuun* seem to have larger spatial connotations that incorporate several centers or dynasties, possibly under the real or claimed hegemony of one ruler. Some rulers or dynasties seem to be associated with poorly understood collectivities numbering seven and thirteen. Other dynastic lines had fluid attachments with great hegemonic centers (Martin 2005: 12; Vance 2016; see also Appendix F).

None of this helps much to determine the political limits or sustaining areas of polities. The most tempting view is from Maya centers outwards, but any conception of territoriality we attribute to kings must be tempered by the realization that many people -- kings, sub-royal elites, and ordinary farmers -- had diverse interests in landscape definition and jurisdiction. Any powerful Maya ruler certainly aspired to a firm grip on some reasonably compact core hinterland within at least a few hours walk of

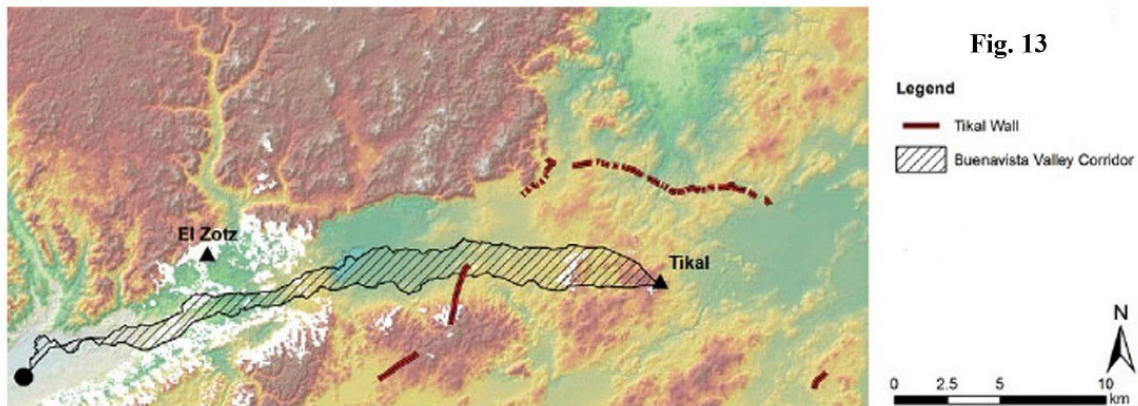


Fig. 13

The Buenavista corridor leading west from Tikal. Image courtesy of Thomas Garrison.

his dynastic seat, whether he envisioned control in spatial terms or as webs of personal relationships or jurisdictions a la Quezada.¹⁶⁹ This is what we have tried to simulate with our 12 km radius around Tikal, and the Voroni diagram of Lentz *et al.* (2014) is a similar attempt. Based on other spatial principles, one real-world boundary might lie somewhere along the natural Buenavista corridor between Tikal and El Zotz (Fig. 13). These polities were sometimes enemies, so there must have been some sort of zone of territorial ambiguity and occasional contention.¹⁷⁰ A similar zone probably lay to the east between Tikal and Naranjo during the early eighth century. Such buffer zones were no doubt common on the Late Classic Maya landscape, and they can inflate carrying capacity calculations and overall Maya population densities if we cannot take them into account. I think territorial conceptions in the strict sense took a back seat to political personal relationships and diverse (and even conflicting) interests. The “kingdom” was wherever kings had personal connections, where they could go with impunity, where they could expect their influence to apply, and where they could predictably obtain resources or allies.¹⁷¹ Examples of such political dynamics are easy to find – this pattern was traditionally true of the “kingdom” of Afghanistan in the nineteenth century and still obtains there today.¹⁷² To be sure, features that reflect political tensions and frontier facilities have been detected, as between Piedras Negras and Yaxchilan (Golden *et al.* 2007), but even these might have fluctuated with the vicissitudes of conflict or alliance. In the core political sense, the viable kingdom was where most of the politically “attached” people were at any particular time. Copan’s core is clear because of its comparative isolation and topographic constraints, but Tikal’s is not.¹⁷³

Attachment

By attachment, I mean how the bulk of the population – farmers – was affiliated with a political center, a dynastic line, a specific territory, or a set of sub-royal nobles. McAnany (1995) argues that Maya people had strong lineage/ancestral attachments to their landscapes, which in turn implies considerable inertia in residential mobility, particularly in movement among polities. To the extent that farmers devoted effort to building landesque capital, such inertia was enhanced. Hirth (2003) and Gutierrez (2003) suggest the widespread existence of a basic Mesoamerican political form – the *altepetl* -- known best from Central Mexico. There commoners and elites were bound together in a kind of ethnic polity within which they shared common political identity, maintained bonds with “legitimate” traditional leaders, and utilized a compact “contiguous” territory (Hirth and Gutierrez recognize regional variants of this model, which can include more complex systems with non-contiguous territorial segments and internal ethnic diversity). Restall (1997) thinks the sixteenth century Maya equivalent was the *cah*, but Postclassic Maya political structures in northern Yucatan

were different from their Classic predecessors in many ways, and there seems to be no Classic Maya term for such a polity except possibly *kab ch'en*. Simon Martin (2005: 12) cautions that we have to question our traditional political models in order to make sense out of much of the epigraphic data: “It may be our notion of the Maya “polity” that is at fault. We need a definition that sits comfortably with dramatic – if rare – shifts in location, and the transfer of identity and affiliation that affects not only places but whole populations.” The rare shifts Martin refers to here are mainly inferred from the epigraphic records of Dzibanche and Calakmul, and relate to hegemonic political patterns we are just beginning to understand (see Appendix F below).¹⁷⁴ Multiple dimensions of royal/elite attachment and identity are increasingly evident, but unfortunately do not tell us how or whether common farmers experienced these shifts as significant.¹⁷⁵

Regardless of their attachments to particular landscapes or kings, how did farming households produce food? This was a main concern of Puleston during the Sustaining Area research. One thing evident from his work, as well as Haviland’s and our own, is that there are few signs of agricultural intensification at Tikal.¹⁷⁶ Particularly unexpected is the almost complete absence of agricultural terracing (but see Lentz *et al.* 2014), which we were careful to look for because the landscape is somewhat hilly.¹⁷⁷ Nor did we find evidence of the field boundary systems that increasingly show up on new LiDAR images of various parts of the Maya Lowlands.¹⁷⁸ Small residential terraces could have supported limited house-lot gardens, but nowhere are there extensive terrace systems or house-lot markers like those documented at Caracol or Becan. I expected our surveys to locate big agricultural terraces at Copan, but we only found them in one limited locale and we did not investigate them.¹⁷⁹ I concluded that patterns of attachment and land tenure might have discouraged farmers at Copan and elsewhere from making such investments (see Webster 2005 for a discussion), but on the other hand that their absence might just reflect comparatively low population densities. Kings might not have been overly concerned with distinct boundaries, but it seems clear that by Late Classic times, if not much earlier, Maya farmers had to define agrarian capital in reasonably predictable ways, whatever their specific agricultural strategies. New LiDAR data from Copan (Von Schwerin *et al.* 2016) reveal previously undetected terracing of unknown date in the Copan pocket, so I might have to rethink all this.

Climate, demography and the Late/Terminal collapse at Tikal

Widespread opinion (e.g. Fauseit 2016a) holds that archaeologists have overly focused on the collapse of complex societies at the expense of processes of resilience, recovery, and transformation. Ford and Nigh (2015) champion high resilience based on the Maya forest garden, and believe the demographic collapse is much exaggerated. Fauseit (2016b: 5) characterizes collapse as “... the fragmentation or disarticulation of a particular political apparatus”. This definition will sit poorly with many Mayanists, who have long grappled with a famous big collapse seen as a heavily demographic phenomenon, contra Ford and Nigh. After all, until the 1950s we knew little about Maya political organization except that leaders of some kind could mobilize people for ritual and construction.¹⁸⁰ Why such leadership failed was difficult to understand without the detailed political insights we have today. What did seem apparent at the time was the rapid and catastrophic loss of whole populations, with few signs of any effective recovery (Turner and Sabloff 2012). Lentz *et al.* (2014) think that the *coup de grâce* of major drought, coupled with high population densities, triggered the Tikal collapse.¹⁸¹ Unfortunately, no long, detailed, sediment cores from the immediate vicinity provide proxies for drought (but see below for some short sequences).¹⁸² Evidence from farther afield in southern Belize derives from the Yok-I speleothem (Kennett *et al.* 2012). Fig. 14 shows the relative wet-dry fluctuations derived from its analysis.¹⁸³

If we assume that this paleoclimatic sequence is accurate and we extend it to Tikal, what are its implications? Unfortunately, Kennett’s fine-grained proxy record only provides climatic context for

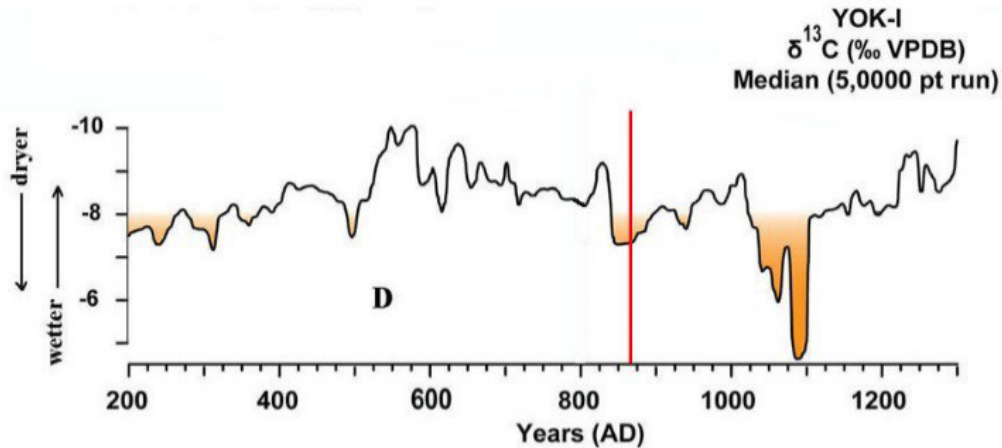


Fig. 14: Adapted by Webster from Kennett et al. 2012 and used with Kennett's permission.

what happened after AD 200. The speleothem record shows a pulse of low moisture between AD 200 and 325. If this extended back in time for another century or so, it overlaps with the troubles experienced by many Late Preclassic centers. A moistening trend after about AD 500 might have stimulated the spurt of population that begins about that time, assuming that moisture was a limiting agrarian factor. Somewhat drier conditions between about AD 790 and AD 896 might have exacerbated agrarian shortfalls and status rivalry during the reigns of the last kings and the population peak, but moisture conditions during that interval appear roughly similar to those during the Early Classic (300–500 AD).¹⁸⁴ If the dynastic collapse were coeval with the last inscription in AD 869 (the red line in Fig. 14), it occurred shortly after the onset of a pronounced dryer period, which agrees with the Lentz (*et al.*) position.¹⁸⁵ There is a comparatively dry interval between about AD 850 and 1020, roughly in line with reconstructions of wider Maya mega-droughts (see Iannone 2014).

The overall trend during these times, however, is toward greater moisture, and for much of the interval moisture levels are again reasonably comparable to those of the Early Classic (pre-AD 500).¹⁸⁶ This post-850 dry spell hardly qualifies as mega-drought if we take the long view of paleoclimate. The big downturn in moisture happens after about AD 1020, much too late to explain the political collapse of the dynasty or any associated population decline. It seems unlikely that Tikal's lack of population recovery after AD 1100 is due to deficient rainfall, because a substantial moisture rebound begins then.

The Yok-I climatic patterns, accurate to decadal or smaller intervals, have recently been extended to northern Yucatan (Hogarth *et al.* 2017) as part of a Malthusian model of population history using Colonial period documents. The authors provide highly detailed lists of dry intervals after AD 600, and present the only long-term Maya demographic history that couples drought with census information. They properly question whether conditions in Colonial Yucatan after AD 1500 provide good insights about what happened to the Classic Maya, given different environmental and technological conditions, the impact of introduced diseases, community concentration, and Spanish food importation. Nevertheless, some patterns they detect bear comment. They put the pre-Conquest population at something over a million people (which I suspect is too high). Their main concern, summarized in their Table 1, is the interval between AD 1528, by which time war and disease had whittled down the population to some 800,000 people, and AD 1781. Population dynamics and famines show a high correlation between population decline and episodes of instability caused mainly by drought. Populations also rebounded quickly after dry conditions eased. What I find most interesting about their reconstruction is the amplitude of the demographic swings. At no time between AD 1528 and 1781 did population dip much

below 100,000 people (99,942 in AD 1688). Even during the interval of greatest decline, in other words, some 12.5% of the population was still present, and had rebounded to 156,250 within a dozen years. If we apply their model to the Classic Maya, using the figures in Table 12 above, then the central and southern Lowlands should have supported a minimum of 125,000-250,000 people even during the worst times, and usually considerably more. Such a population should have had the capacity to rapidly recover from shocks such as drought like the Colonial one did.

The big unknown in all of this, as in almost all current paleoclimate proxies, is what *relative* fluctuations in the speleothem record mean in terms of *absolute* amounts of rainfall and their seasonal spacing.¹⁸⁷ These after all are the two biggest variables, along with good soil, essential to ancient rain-fed agricultural systems. Most rainfall proxies are far too coarse to detect the magnitude of the short-term fluctuations so critical to farmers. The Yok-I speleothem is an exception, but even the sub-decadal intervals it detects still leave the absolute amounts of rainfall unspecified. A dry year (departure from average) in northern Yucatan and a dry year in the northeastern Peten might be two very different things. The real breakthrough in this method will occur when it is possible to specify the magnitude of very short-term fluctuations in absolute amounts of rainfall for a region such as Tikal.¹⁸⁸ So far as I know, no one has raised the question of whether some drying (as opposed to outright drought) might have been a good thing there. I begin to suspect that drying both amplified the extent of Tikal's zone 2 lands, which by Late Classic times were prime agricultural resources, and rendered their soils better drained. From the perspective of individual farmers, short-term, seasonal fluctuations in rainfall amounts were probably as important as simply getting enough rain annually. In any case, if trends in population size and density reflect the biophysical well-being of Tikal's farmers, the data suggest that for several generations they were better off after AD 500-600 than they were before, or that their landscape could support large additional aggregates of people.

Unfortunately, palynology at Tikal does not help much as an alternative line of evidence (see French and Duffy 2016: 56-57 for a brief overview) because of the lack of long, "wet" sediment cores from the immediate vicinity.¹⁸⁹ Lentz *et al.* (2014: 215: 163) illustrate two short pollen-bearing sediment profiles from Tikal – one from Vaca de Monte and the other from Aguada de Terminos. The Aguada de Terminos profile (Dunning *et al.* 2015: 109-110) has five dates, but the bulk of the sediment appears to be very late. A very narrow zone somewhere in the 80-90 cm levels represents the Classic interval, so one cannot detect much of a pattern. There is a reversal among the three Postclassic dates, but if the latest date is correct, then maize pollen continues until (plausibly) the thirteenth or fourteenth centuries, and the biggest percentage surge in charcoal is definitely Postclassic. The Vaca de Monte core, surprisingly, shows no indications of maize pollen unless it is lumped into the larger category of Poaceae.¹⁹⁰ There are only two dates for this profile so it is difficult to relate the pollen to a detailed chronology. The earliest date at the base is roughly AD 600, so most of the deposit accumulated well after Classic times and in fact after about AD 1500. Charcoal concentrations remain high until well after AD 1500, and the only surge in arboreal pollen is very late. A recent core from the El Zotz *aguada*, about 20 km west of Tikal (Beach *et al.* 2015: 271) is truncated above the 50-60 cm level and does not tell us much about late ecological conditions there.

Artifacts provide other insights into the collapse. Culbert identified the Terminal Classic Eznab ceramic complex (AD 850-950) largely from deposits of pottery left on the floors of palaces, and especially the Central Acropolis excavated by Harrison. Eznab collections show decline in quality and a great reduction of polychromes, but according to Culbert "There is absolutely no indication that the Eznab inhabitants of the site were anything other than descendants of earlier Tikaleños" (Culbert 2003: 62). He remarks that the succeeding Early Postclassic Caban complex is sparse and difficult to define, but that it seems to be very different from the earlier ceramic tradition, and more strongly related to pottery used by people living around the lakes to the south as identified by the Rices. Puleston (1973: 225-227) summarized the

Caban phase data from several of his surveyed zones and argued strongly for a very drastic demographic as well as political collapse, the latter shown by Fry's figures in Table 9. This, he thought, undercut Thompson's old idea of peasant rebellions, which envisioned substantial numbers of farmers still living outside of abandoned elite centers.

One could read all this as continued presence of a small post-collapse farming population that predated the migration of the Itza and other groups into the region, or of a local landscape that was almost entirely depopulated sometime between AD 850 and 950. Political and demographic decline were clearly linked in the latter case. Unfortunately, the ceramic samples that provide information about Tikal's late inhabitants come most abundantly and securely from excavations in big architecture in or near the site core. Our own household excavations, limited though they were, show only scant traces of pottery that post-dates Imix times (see Table 10). Imagine, though, that the population of the whole Tikal kingdom declined from 60,000 to 5000 people by AD 950, most of whom lived scattered over the whole 452 sq. km zone we have identified. I suspect we would have a very hard time finding them without a lot more work, although the short Tikal cores hint at their presence.¹⁹¹ Eric Taladoire came to the same conclusion as a result of his surveys and excavations of small postclassic "hamlets" at Balamku to the north (personal communication, 2014). Prudence Rice (2004) and Rice and Rice (2010) provide good discussions of Tikal's possible Terminal and Postclassic successors and survivors near the old polity core and in the Peten lake region to the south.

Whatever causes we champion, the quandary of the Classic collapse, along with its earlier counterpart at the end of the Preclassic, is much more comprehensible if the scale of population decline was less than we imagine.¹⁹² There long have been two contrasting views of Maya demography and the Classic collapse. The first posits a sudden and dramatic population crash sometime during the late eighth and early ninth centuries. Destructive human niche alteration is seen as a contributing factor because this crash is associated with Late Classic population peaks.¹⁹³ According to the second view, the Maya not only achieved high population densities, but also maintained them without deleterious effects on their agrarian environments. This position envisions much more resilience and persistence of people on the landscape, as Ford and Nigh (2015) have argued. Since the 1980s, I have inclined toward the first explanation with the proviso that the demographic "crash" was slower than we think. If Maya agrarian practices were as benign and effective as the second view implies, then why the loss of population, however long it took? And why was there no robust recovery even after the postulated severe droughts ended at some point? (see Webster 2014 for some ideas).¹⁹⁴ If, on the other hand, the Maya were basically swidden maize farmers, then the great pulse of population growth around 500 to 600 AD probably created many problems, and not just food shortfalls. Many recent archaeological projects such as those of the Rices in the Peten Lakes region and Houston and his colleagues at El Zotz have documented substantial populations after AD 800-850, and the same appears to be the case for the dryer Rio Bec region (Ball 2014).¹⁹⁵ We should seriously consider that regional demographic declines (as opposed to the political ones at individual centers) represent a slower "crumble" that reduced a peak Lowland population from a bit more than a million people to several hundred thousand over a very long time.¹⁹⁶ This is a far cry from the demographic catastrophe imagined by Morley and several other generations of Maya archaeologists, and a confirmation of Maya resilience, at least up to a point (see Ford 2008 for a similar argument). If the resulting densities of dispersal that followed political collapse were as low as those shown in Table 12, we face a very serious challenge in detecting Postclassic populations through settlement data alone. Nevertheless, the Classic Maya of the central and southern Maya lowlands do not seem to have experienced the kind of widespread recovery that repeatedly occurred during Colonial times in Yucatan. As for the earlier Late Preclassic "collapse", we understand its political effects better than its demographic effects. It remains to be seen whether there was at that time a significant loss of population and subsequent recovery in the central and southern lowlands as a whole.

I conclude that the Maya themselves were not particularly exceptional, but that the demographic reconstructions made by Mayanists often are. Most exceptionalist claims about the Maya, such as their lack of serious warfare or their invention of the Long Count, were susceptible to falsification. High Maya population estimates for Tikal or elsewhere are not. Fortunately, broad comparative perspectives serve as convincing checks on excessive numbers. Many of my colleagues will dismiss my conclusions, but they do possess obvious advantages. They render the Maya much more congruent with what we know about other ancient civilizations. They allow us to assess social and political organization, as well as actual events, in the context of more realistic demographic scales. Most importantly, they provide a simple solution to the knotty problem of how long-used Late Classic landscapes supported so many people: there were far fewer we assume. Lack of big landesque investments such as terracing or drained fields in most places is less puzzling than it appears.¹⁹⁷

I think our own overestimates have created three problems that that might not really exist:

- 1) Why are many Maya population estimates so excessive compared to those for other ancient civilizations?
- 2) How did the Maya support such large numbers on their agrarian landscapes?
- 3) What accounts for the abrupt disappearance of millions of people in the eighth and ninth centuries?

My demographic heresy effectively eliminates the first and greatly diminishes the quandry of the others. At all times maize was the main staple crop.¹⁹⁸ While there are impressive signs of intensive agriculture, much of it might represent adaptation to risk. Much of the landscape remained under rain-fed maize cultivation (albeit usually short fallow or permanent) and lacked major landesque investments even in the eighth century.¹⁹⁹ Many such “improvements”, where they existed, represent coping behavior, not Bosrupian innovations that raised overall carrying capacity (see Murtha 2002, and Appendices B and F).²⁰⁰ 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 investments in art and architecture. Surely this makes them more, rather than less, impressive. Low populations also help to explain what many archaeologists call Maya resilience. Small numbers of people on the Maya landscape from very early times make our impressions of social durability more comprehensible. Even few people, however, could account for the deforestation and erosion so evident at Tikal and elsewhere if they used and abused their agrarian landscapes long enough, particularly if there were unprecedented episodes of high density on parts of the Late Classic landscape where drought exacerbated anthropogenic effects.

APPENDIX A

Population Density Calculations

Many years ago, William Sanders and I (1978: 252) wrote that the concept of population density was so “commonly understood” that it needed no discussion. Now I think we were overly optimistic, hence this appendix. The several meanings of population density are often unclear in the Maya literature, creating confusion and inappropriate comparisons. This is true of many of the Maya estimates discussed above. To make things clearer, below are some definitions from most general to most specific (I’m making up the labels -- the sense is what counts). My main concern is pre-industrial agrarian populations, although I realize that almost all such people, the Maya included, supplemented their agricultural diets with various forms of wild foods and other resources. The following definitions would have to be heavily qualified for hunter- gatherers or foragers or for societies practicing mixed economies, such as many of the prehistoric “farmers” of the US Southwest.²⁰¹

Overall densities

This measure simply indicates density for some particular region, irrespective of differences in population distribution or variation of land quality within it. These are essentially minimal densities because if uninhabited or unused land were excluded they would be higher. Overall density estimates are commonly given for the Maya, as seen in most of those listed in Table 1 at the beginning of this paper. They are conceptually easy to understand, are useful for very broad comparisons, and they provide base measures that can be appropriately scaled up or down to accommodate more specific density figures as listed below. On the other hand, they tell us little about how populations were distributed, details of agrarian adaptation, or how land was accessed or managed. Overall densities are useful for other purposes, such as calculating overall carrying capacity or how many people kings or other elites might mobilize for construction or warfare. Plausibility of overall density calculation is in part dependent upon the size of the region considered. For example, Morley applied his 1946 Uaxactun density range to the whole Maya Lowlands, and at this scale it is highly implausible. Parts of his range are acceptable for smaller areas.

Spot densities

Spot densities are calculated for small and well-defined areas of particular interest such as Epicentral Tikal or the walled area of Mayapan. An early example is Richard Adams’s (1974) reconstructions of palace populations at Uaxactun, and a more recent one is the village of Ceren (Sheets 2006). Hammond’s estimates of 200-400 people per sq. km for Lubaantun (Table 1) refer to a zone of some 3.1 sq. km immediately around the monumental core, a spot density even smaller than Mayapan’s. Spot estimates are not very useful because it is difficult to extrapolate from them to the wider landscape. They do have implications for supporting agricultural densities, especially where information about structure function and configuration is very detailed, as at Ceren.²⁰² Unfortunately, spot densities given in the Maya literature tend to blur into those for larger zones. For example, Haviland, Sanders and Coe all give density estimates for the whole Carr and Hazard mapped zone of 16 sq. km. These begin to take on the character of overall densities if extended to larger chunks of the landscape.

Productive densities

This is the density of population in relation to the potentially useful agricultural landscape available to the inhabitants of a region at any given time, given their technology and systems of cultivation. Don Rice (1978: 46), borrowing a term from animal ecology, calls this the *economic density*, and William Allan, in his 1965 book *The African Husbandman*, calls it the *cultivable land factor*. For example, about 85% of the

landscape around Tikal (Table 5) consists of potentially usable uplands lying outside the permanently humid, low-slope bajos. This zone is the basis for productive density calculation. Productive densities (and the others listed below) are obviously elastic, depending upon the specific subsistence system(s), strategies, technologies, crops, competitive exclusion, erosion, loss of fertility, etc.

Optimal productive densities

A considerable part of a landscape might be productive (as defined above), given a particular set of subsistence strategies, but landscapes are usually “patchy”, in the sense that some portions of them have better soils, are less risky, are more stable, less degraded, etc. Farmers will prefer these zones, and so they define the highest quality land. At Tikal, about 11% of uplands are steep and especially susceptible to erosion, and so are excluded as sub-optimal from any long-term characterization of the productive landscape.

Cultivation densities

These are densities associated with the amount of land actually planted and harvested in a yearly cycle (one or more crops), or that is under permanent cultivation as orchards or house gardens at any given time. Allan (1965) calls the amount of land that must be cultivated in any given year to sustain an individual (or some other social unit of known size) the *cultivation factor*. This is the most important density measure, but it is very difficult to calculate for ancient agrarian systems. The several world regions compared above obviously differed greatly in terms of what proportion was planted and harvested, and how often. Much of the usable landscape of ancient Egypt was under permanent or annual cultivation during any given year, and so was much of the Basin of Mexico. By contrast, a large proportion of the ancient Maya landscape was in fallow or otherwise uncultivated. Such variation is one reason why overall density estimates are deceiving. We usually have no direct evidence for Maya cultivation densities unless there are features like terraces, field boundaries or drained fields. Nevertheless, they are often calculated for simulation purposes. The Maya case is complicated by the fact that “fallow” fields were probably still managed to provide resources long after the main cultivation effort shifted.

Agricultural densities

This means density in relation to land that is in some stage of cultivation (e.g., it includes land lying fallow). Allan (1965) calls this the *land-use factor* and defines it as how many units of cultivable land are required to maintain the cultivation factor. Example: 1000 people at Tikal practice a 6:1 forest fallow system, and require 0.5 ha per capita for their energy/nutritional requirements. 500 ha are in cultivation, and 3000 are in fallow, for a total of 3500 ha or 35 sq. km. Agricultural density = 29 people per sq. km. The land-use ratio number can vary considerably. A long-fallow swidden system might require 10-20 units, whereas an irrigated zone of deep alluvial soil might need only one.

Roy Rappaport (1971) gives an example of proper use of density measures in his *The Flow of Energy in an Agricultural Society*.²⁰³ In his study of swidden production by the Tsembaga people of highland New Guinea, Rappaport reports the following:

- 1) Population over the whole territory of the Tsembaga = 24.7 per sq. km. This is the *overall density*.
- 2) Population per area of potentially arable land given the agricultural systems in use = 37.5 per sq. km. This is the *productive density*.
- 3) Population in relation to land that is, or has been, cultivated = 124 people per sq. km (49 people per sq. km). This is the *agricultural density*.

Rappaport further notes that the Tsembaga population was well below carrying capacity, which he suggests might be 200 people per sq. mile (77 per sq. km). Even this density is surprisingly low because, unlike the Maya, the Tsembaga depend on root staples (taro and sweet potatoes) that typically have higher yields than maize, and had semi-domesticated pigs.

Archaeologists, unfortunately, are seldom as informed as ethnographers. Overall density is easiest to compare from one archaeologically known region to another, but for obvious reasons is the least revealing calculation. We can make a stab at productive densities in many regions when we know enough about landscape features such as slope, soils, bedrock, and vegetation. Spot densities, while sometimes easy to compile from field data, are unhelpful with regard to the larger landscape. Our focus of comparison should be agricultural and cultivation densities, but very seldom do we know enough to reconstruct them. About all we can do is to create ranges using plausible simulations. Finally, population density calculated for *any given time* is really just one measure of a dynamic, diachronic process. For most pre-modern societies, and especially agrarian ones, both the absolute numbers of people on a landscape and their densities tended to fluctuate as affected (obviously) by population growth or decline, by uncontrollable risks to production, by anthropogenic or other alterations of the landscape (niche construction), by new technologies or crops, or by managerial fiat. For example, early farmers at Tikal probably cultivated the steep upland zones that are highly prone to erosion, but at some point they became very marginal or perhaps were even abandoned. Under conditions of drought, the moister soils along the margins of bajos might have been preferred over steeper upland soils. Similarly, if elites carved out estates or did away with traditional ancestral claims by arrogating all land possession to themselves (as Hawai'ian kings and elites did), they might have redistributed farmers on the landscape by fiat.²⁰⁴

Calculating some sort of mean population size and density in any of these categories is usually very deceptive, especially if simulations assume some optimal or average yearly yield. Ancient populations were poorly buffered against production shortfalls, so the number of people consistently on the landscape is usually far lower than our abstract "carrying capacity" calculations suggest, and more closely related to limits set by the bad years than the good ones as noted above (Leibig's Law).²⁰⁵ This is one reason for the remarkably low and stable overall Egyptian population until the nineteenth century. Warfare also favors the creation of buffer zones that lower carrying capacity to the extent that they remove productive landscape from use. However difficult it might be to quantify carrying capacity from an etic perspective, there seems little doubt that ancient people perceived limitations on basic resources and acted accordingly. Whether their perceptions were accurate or not is in some respects irrelevant.

APPENDIX B

The Big Stuff

Some years ago, Richard Hansen guided me around Nakbe, El Mirador and Tintal. The imposing Preclassic buildings he has investigated in the Mirador Uplands are commonly said to be the largest ever built by the Maya, and are favorably compared in scale with monumental architecture elsewhere in Mesoamerica and beyond. Simon Martin (2016: 540) characterizes their significance, especially with regard to political evolution, this way:

“If we are in the business of drawing inferences from disparities in the archaeological record (e.g., volumetric assessment, settlement density, and population assessments, with their implications for energetics and economic resources), then El Mirador’s physical remains alone are testimony to its importance in the region ... Those directing its fortunes succeeded in constructing a city larger than those built by the Maya in subsequent centuries (larger, even, it is said, than the total building mass at Teotihuacan).”

By disparity, Martin means something that is exceptional in the archaeological record and so strikes a discordant note in our expectations and our conventional wisdom. In the case of the El Mirador sites, the disparity is the sheer scale of architecture and the population presumably responsible for it, which seems to dwarf those of later Classic times. In short, why are the Mirador region structures so big, what does this imply about their demographic, political, and economic characteristics, and how do we square all this precociousness with later developments?

Claims of enormous Preclassic scale, Martin’s included, are often unaccompanied by critical volumetric estimates.²⁰⁶ A recent example is James Doyle’s (2017) otherwise admirable overview of Preclassic architecture, which is much concerned with monumentality. I found no volumetric measures of the buildings at El Mirador, Nakbe or any other Preclassic sites in his book, nor any estimates of the populations from which labor was mobilized.²⁰⁷ Even when published sources do give numerical estimates, they are often hyperbolic, confusing or unconvincing. Estrada-Belli (20016: 249), not content with Mesoamerican comparisons, recently claimed that the Danta complex is the largest construction as measured by volume in the *whole ancient world*, without offering any numbers. I don’t wish to belabor this issue so I will confine my brief comments to the Tigre and Danta complexes at El Mirador.

Both the main Tigre and Danta pyramids are parts of larger complexes, and it is often unclear from the literature to what the published volumetric estimates refer. For example, Michael Coe’s previously cited figure of 200,000 m³ for Danta might refer just to the pyramid, but I can’t be sure (and in any case is clearly wrong, wherever he got it). Chip Brown (2011), on his own tour of El Mirador, instead quotes Hansen as saying that the mass of the Danta pyramid *alone* amounts to some 99 million ft³, or 2,803,368 m³. This would indeed make it a bit bigger than the Great Pyramid complex of Khufu. On the other hand, Vernon Scarborough (2007: 170) says that the Danta and Tigre pyramids together “... have a volume greater than 1,000,000 m³, or the equivalent of the Pyramid of the Sun at Teotihuacan ...” Ray and Deanne Matheny (2011: 66, 110), following Hansen (1990) say that the Tigre complex has a volume of 428,000 m³, or about 36% of the bulk of the Sun Pyramid at Teotihuacan, which amounts to 1,175,000 m³ (Cowgill 2015: 62). They caution, however, that some of this Tigre bulk is a natural outcrop. I could give many other examples of this kind of confusion. Instead, in Fig. 15, I show Cartesian images of the Tigre and Danta Complexes to the same scale as the Sun Pyramid Complex, and in Table 13, the heights of the buildings. I adapted the Sun Pyramid image from Rene Millon’s map, and the El Mirador images are from Matheny and Matheny (2011; Figs. 5.1 and 5.32). To my eye, the biggest pyramids in the two El Mirador complexes are roughly 30-40% the mass of the Sun Pyramid proper. Readers can judge for themselves if “... the Danta complex is the largest Mesoamerican pyramid” (Estrada-Belli 2011: 49).

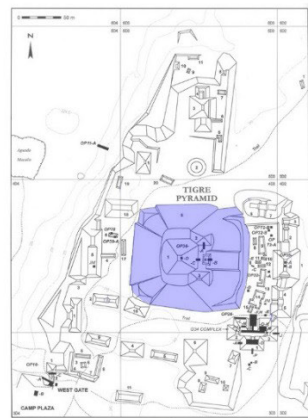
Big Stuff enthusiasts could object that there is a lot more to El Mirador than these two big complexes, but one could make the same point about Teotihuacan; inclusion of the Moon Pyramid, the Ciudadela structures, the Great Compound and the many temples along the Street of the Dead would dwarf the scale of El Mirador.

Hansen (2016: 397), to his credit, gives labor estimates. He calculates that the Tigre Complex absorbed five million man-days of labor and the Danta Complex some ten-to twelve million days. Supposing that these figures are roughly correct, a labor force of 2500 people could built the Tigre complex in less than eighteen years, even if they worked only for 120 days during the non-agricultural seasons. The lesson is that once we dig a bit into seemingly gigantic numbers, the results look much less impressive.

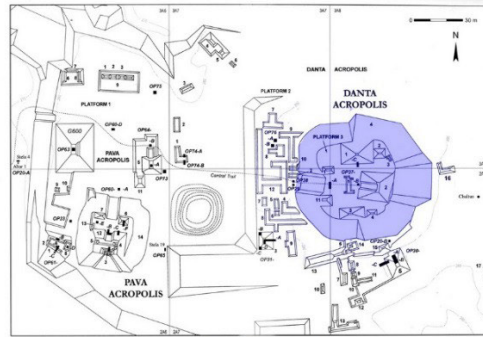
What else can we make of all of this in terms of construction effort? First, a big job finished in a short time is more costly, politically and otherwise, than a job of the same scale done over a much longer interval. The great El Mirador buildings took many generations or several centuries to complete, although no one knows just how long. A much smaller El Tigre labor force would have sufficed if the construction interval were much longer than eighteen years. In sharp contrast, Khufu built Egypt's Great Pyramid and its associated structures during his reign of some 23 years, and all three of the big Giza pyramids together were finished in three generations, or about 90 years.²⁰⁸ Teotihuacan's Sun Pyramid was erected in two main stages during an interval difficult to pin down exactly (Sugiyama 2012: 220-221), but construction probably took about 150 years, from roughly AD 200 to AD 350. If we amortize the Egyptian effort in relation to time, the El Mirador complexes were even less costly than the volumetric comparisons indicate. Construction was more protracted at the Sun Pyramid, but still probably shorter than that of the Danta and Tigre complexes.

Mayanists commonly infer that the sheer mass of structures reflects the scale of the populations that built them, bringing us back to demographic issues. Some claims for Late Preclassic populations in the Mirador Uplands, especially in the popular literature, are simply preposterous. Chip Brown (2011: 41) enthuses that El Mirador itself had 200,000 people and that there were 800,000 more in neighboring "cities." If true, the overall population density of the region was at least 435 people per sq. km and something like 12,500 per sq. km for the "city" itself. Brown's figures imply that the Mirador Uplands had a total population verging on that of Old Kingdom Egypt, but in a territory only about one-seventh the area of the Nile Valley. If one took these figures seriously, and if Hansen's labor requirements are correct, then labor was hugely underutilized.

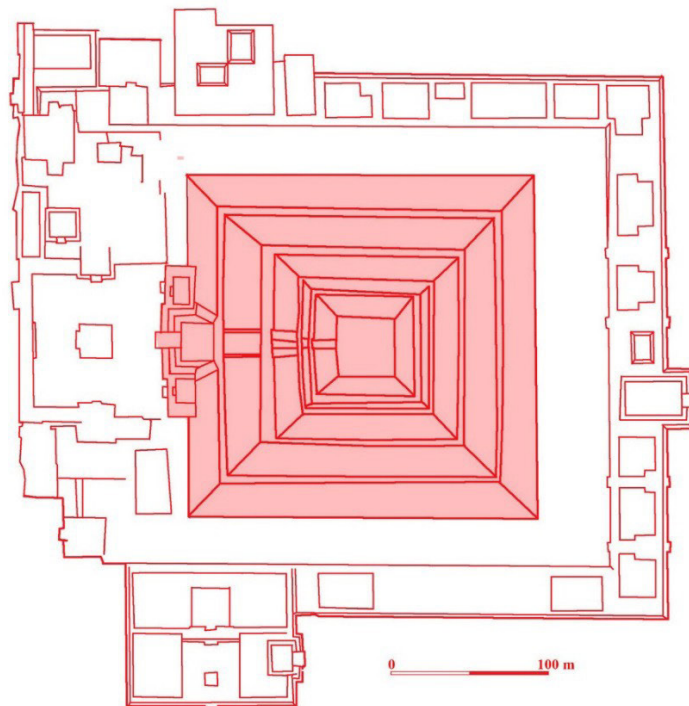
Lehner (1997: 224) calculates that only about 20,000 to 30,000 people, or roughly 2% of the whole Nile valley population, were involved in hands-on pyramid construction at any given time. Teotihuacan's big civic buildings along the Street of the Dead were probably in place by AD 350, a time when most of the inhabitants of the Basin of Mexico were still concentrated in the city itself, although population growth had slowed or plateaued. Estimates for the city's peak size range widely, up to a high of 200,000 people. George Cowgill (2015: 143) thinks this last is an overly generous figure originally derived from Rene Millon's mapping project, but it is often repeated. Michael Smith is a bit more conservative (2012: 33) with a figure of 150,000. Cowgill (2015: 144) sensibly takes a middle ground: "Teotihuacan's population during the 'plateau' may have been somewhere around 85,000. A maximum of 125,000 people seems possible, but so are estimates much lower than 80,000."²⁰⁹ When Teotihuacan's major civic buildings were constructed, the whole Basin of Mexico probably had far fewer inhabitants than Brown estimates for just the "city" of El Mirador. The point of all this is that no matter which figures we choose, a small Teotihuacan population, say 100,000 people give or take, was responsible for the biggest collective construction enterprise in Mesoamerica, one that greatly exceeded those at El Mirador, although it falls short of Old Kingdom Egyptian efforts.²¹⁰ The lesson once again is that Big Stuff does not necessarily indicate lots of people, and certainly not the very high population estimates for El Mirador that some advocate.



Tigre Complex



Danta Complex



Sun Pyramid Complex

Fig. 15. Top: the Tigre and Danta Complexes at El Mirador with the principal buildings shown in blue. Bottom: Teotihuacan's Pyramid of the Sun Complex. All shown to the same scale.

El Mirador images courtesy of the New World Archaeological Foundation.

Table 13 Comparative scales of big complexes

Structure	Height in meters
Danta basal structure	15-18
Danta pyramid only	21-22
Tigre basal platform	24
Tigre pyramid only	29
Pyramid of the Sun	69*
Great Pyramid of Khufu	147

*Adds assumed height of summit temple to current top of pyramid (65 m).

Comparative heights of the Danta and Tigre architecture from El Mirador (from Matheny and Matheny 2011), the Sun Pyramid at Teotihuacan (from Cowgill 2015: 62; Evans 2013: 351), and Khufu's pyramid at Giza (from Lehner 1997: 108).²¹¹

APPENDIX C

Agricultural Intensification

Mesoamerican and Maya archaeologists traditionally view intensification as a complex set of processes influenced by environmental, demographic, socio-political, and cultural conditions. Ester Boserup (1965: 117), the most influential proponent of intensification as a major factor in agrarian change and development, argued that it is “unrealistic” to regard any cultivation systems, intensive or otherwise, as adaptations to the natural environment. She thought intensification, or investment, as she also called it, results instead from differences in population density, which I regard as a serious overstatement of her case.

Archaeologists face special problems in detecting and interpreting signs of intensification. I am mainly concerned with intensification in societies like those of ancient Mesoamerica, where much of what was produced was consumed by the producing units, where there were no metal tools, no complex machines or non-animate energy sources, no large domestic animals, no hybrid or genetically-engineered crops, and no systems of bulk marketing and exchange such as those characteristic of the last several centuries in many parts of the world.²¹² The literature on intensification is much too large to cite here. The definitions and discussions below are simply my own restatements of what many others have said long before and are certainly not original to me. Apologies to anyone who feels slighted by non-citation. For useful overviews, see Turner and Doolittle (1978) and Boserup (1965). Seefeld (2017) provides a recent comprehensive summary of what we know about intensification and landesque features in the Maya Lowlands, and Richard Hansen (2016: 360-362) gives a recent overview of the Preclassic features he has detected in the Mirador Uplands.

Whatever its causes, intensification occurs when inputs of labor, materials, technology, energy, time, information skill and other factors transform some default (and usually simpler or more restricted) set of agricultural strategies, the object usually being to increase or stabilize productivity, to avoid risk, or to make commercial profits.²¹³ Default strategies typically (but not always) utilize large amounts of land and are labor-efficient in terms of energy input-output ratios compared to the alterations that follow. Interestingly, we have no general term for such default strategies (the clumsy “extensive” will not do), although some terms exist for specific agrarian traditions. For Prehispanic Mesoamerica, a default system might be rain-fed, shifting milpa (from the Nahuatl words for “place of cultivation”) agriculture using fire, human muscles and non-metal tools. This is one variant of the “slash-and-burn” agricultural practices that V. Gordon Childe (1950: 6) and Boserup (1965: 16-18) envisioned as typical of simple “Neolithic” societies.²¹⁴ Childe and others of his time linked most intensive forms of food production to the emergence of early states, so they had “progressive” evolutionary implications.

Intensification is generally a response to stress, necessity, or opportunity, and can involve many species of plants and animals. Here I am concerned only with food crops. Intensification might also involve cotton, tobacco or any other non-food crops, especially if there are commercial incentives. Among the most important dimensions of intensification are additional units of labor, which in turn alter productivity. Below are some examples largely drawn from Elliot Abrams’s (1995) analysis of the relationship between labor inputs and productivity as applied to the ancient Maya.

Total agrarian productivity is the energetic (or food) output of producer(s) on a given piece of land. If one farmer in a family produces 1000 kg of edible maize per ha by his own efforts, his total productivity is 1000 kg. If a second farmer added to the family’s work force results in a yield of 2000 kg on the same piece of land, total productivity doubles but their *average productivity* remains 1000 kg. If addition of the second farmer results only in 500 kg more food, then total productivity increases to 1500 kg but

average productivity falls to 750 kg per worker. The *marginal productivity* of the second farmer is only 500 kg. If additional workers are added, both average and marginal productivity tend to decrease in accordance with the “logic of diminishing returns” (Livi-Bacchi 1997: 91), a discouraging perspective often characterized as Malthusian. Boserup (1965: 14) insisted to the contrary that many cases the output from a given area of land responds far more generously to an additional input of labor than assumed by neo-Malthusian authors”. Sometimes, that is, additional labor increases both average and marginal productivity, especially if new technology, crops, or energy sources are involved.

There are many classifications of intensive agriculture. Frederick Wiseman (1978:84) differentiated between *biointensive* systems, which increase desirable energy flows by modifying the vegetation regimes (e.g., mulching or arboriculture), and *geointensive* systems, which alter soils, hydrology, or topography (e.g., terracing or irrigation). The Classic Maya clearly used both. Intensification is often assumed to be driven by some imbalance between population and agrarian resources, and especially density-driven as Boserup maintains. Such imbalances are usually associated with population growth, but many other factors such as environmental change can cause imbalance. Risk avoidance, profit pursuit, or decisions made by agents other than cultivators sometimes motivate intensification, which is thus not density-dependent.

Intensification is strongly linked to demographic and societal scale. Labor from local households or small networks of households suffices for the construction, maintenance, and use of many kinds of intensive features. Others require large numbers of people, many with special skills, mobilized and directed by some central polity. Few intensification investments made by the Classic Maya appear to exceed the capacity of the household or local community.²¹⁵ Das Gupta (1997) summarizes the intricate relationships among kinship and household structure, inheritance, reproductive incentives and behavior, and agrarian labor.

Intensification from the perspective of a particular area of cultivated land

Boserup (1965: 41-43) disagreed with the “usual definition” of intensification -- the application of more labor to a specific area of cropped land. She maintained instead that intensification is the adoption of patterns of land use that enable farmers to crop specific areas of land more frequently, as in fallow reduction or two or more crops grown in a single year. Such patterns might or might not require larger inputs of labor or any kind of permanent landscape modifications. For example, simple fallow reduction involves no conscious landscape alterations, but simply increases the frequency of cropping (of course it might involve niche construction, for better or worse). The frequency definition has no necessary implications for outputs for any given crop. Outputs might decline, as in yields from individual harvests as fallow reduction occurs, but if cultivation strategies change, and particularly if there is accompanying technological innovation, more frequent cropping might result in increased yields.²¹⁶ In the latter case, as Boserup observes, intensification can occur under any population regime, not just one under pressure.

If we shift our focus to yields of successive crops over more than one cropping interval, then increased frequency of cropping might result in increased outputs per unit of land. For example, suppose that yield under a forest fallow system (1:12 rotation) is 2000 kg per ha, but under conditions of early reduction to bush fallow (1:6 rotation) yield drops to 1200 kg. In one 12-year period the latter system results in a 2400 kg yield increase on the same fields (because they are cultivated more often), than the more extensive strategy. Thus *regional* yields increase (for a time) with increased frequency of cultivation, even though yields per unit of cultivated land decrease, as does the average or marginal productivity of labor. Increased frequency of cultivation might also be associated with use of fertilizers, mulches, etc. An important point is that any cropping system might involve several components. For example, farming households often utilize infields and outfields. The latter generally produce the bulk of the

staple crops. Infields (also called house-lot gardens, home-fields, etc.) are close to the residence and typically are small – less than 1000 sq. m. Despite their size, permanently and intensively tended fields might produce as much as 15% of household foodstuffs. Infields typically have multiple species, ranging from low herbaceous plants, larger leafy plants, shrubs, and tall orchard trees, creating a multiple-canopy effect. Crops grown include staple foodstuffs, but also condiments or fruits, as well as plants useful for medicinal, recreational, or industrial purposes. Even when people shift their outfields, there thus remain small, permanently cultivated core fields, orchards, or gardens. We have seen above that when farmers shift their outfields, particularly in tropical forest environments, the old fields remain productive resources.

Intensification from the perspective of a region with many parcels of cultivated land

A *region* is used more intensively if a cultivation strategy formerly restricted to one portion of it is extended to lands of similar quality elsewhere. Boserup (1965: 12) calls such lands the “extensive margin”. Norman Hammond (1978: 25) and others have called such expansion among the Maya “*extensification*” or “*lateral expansion*.” If additional farmers join the workforce and cultivate new pieces of land, total regional productivity will increase while average and marginal productivity might remain the same. Such expansion is usually possible only early stages of agricultural utilization of a region. The managed forest model reviewed above represents one dimension of intensification because there is an additive process in which more parts of the landscape maintain some sort of production as main fields shift location.

A region is also used more intensively if more marginal portions of it are put into cultivation (by whatever means) as the highest-quality landscape suited to a given form of cultivation becomes filled up. In other words, regional intensification does not necessarily mean more intense use of individual plots of land within it. Marginal cultivation might or might not be markedly different in character from that used on the original high-quality landscape. An example is the expansion of maguey cultivation to dryer parts of the Teotihuacan Valley in Mexico that are unsuitable for maize and other staples (Evans 1992). Such expansion usually allows increased overall population density for the region. Production on marginal lands might also involve specialized crops that have market value (to an extent this is true of maguey). If cultivation extends to land marginal for subsistence or commercial purposes, variation in well-being among cultivators in a region tends to increase, as does vulnerability to drought or other risks. Such expansion is often dependent upon technological innovation. A field yielding only 250 kg of grain under conditions of rainfall agriculture would probably not be worth cultivating if a farmer used his own muscles and hand tools. With a plow and animal power, as in the Old World, it might yield decent returns (although Boserup properly notes the considerable expense of using and maintaining draft animals). Similarly, in many parts of highland Mesoamerica steep hillsides are cultivated using contour hoeing (see below), which inhibits erosion.

Input definition

A plot of land is used more intensively if the amount of labor input (in terms of energy and/or time) per crop is increased, irrespective of resulting changes, if any, in energy outputs. For example, weeding might be unnecessary under forest fallow and bush fallow systems, but essential under annual cultivation. Input can be measured in terms of energy, but if so a distinction should be made not only on the basis of some abstract energy measure (e.g. kcal per cultivation interval), but also the nature of the energy source (e.g. human muscle, animal power, fossil fuel, etc.). Animal inputs were very important in the Old World. For much of the New World, including ancient Mesoamerica, human muscle input was the crucial factor, thus simplifying calculations. Because it is very difficult to measure energy inputs directly, the usual proxy is time. Increased inputs of time usually (but not always) indicate increased inputs of energy, but do not directly relate to how big the increases are.

Energetic inputs might involve repeated efforts that leave no obvious landscape traces, such as mulching or fertilizing soils for successive crops. This sort of input can be calculated as part of each successive cropping episode, just like hoeing or weeding. On the other hand, inputs might involve initial capital improvements (e.g., building of small terrace or dam walls), with only minor later inputs (e.g., terrace or dam maintenance).

In this case the initial investment must be amortized over the subsequent use-life of the field. Character of inputs may be more decisive than changes in input amounts, as measured in terms of energy. For example, a shift in crops (e.g., from seed crop to root crop) might entail the same (or even fewer) energetic inputs, but result in greater crop yields per unit of land. Other examples are intercropping or new strategies of crop rotation. One might consider the switch to a new crop, or to several species grown in the same field, as the application of a new genetic or bio-intensive technology.

Output definition

A plot of land is used more intensively if the amount of output per unit of land for a particular cropping interval is increased (in terms of caloric yield), irrespective of whether energetic inputs are increased. This is probably our most intuitive definition, and we generally measure intensification by increased total productivity of a plot of land if calculated over some suitably long interval. Output, and hence intensity of cultivation, might involve strategies that do not increase input and might even decrease it (see above).

Input/output ratio definition

Some anthropologists define intensification as a process whereby labor inputs increase while outputs (yields) decrease -- a diminishing return model as measured by marginal productivity.²¹⁷ Sometimes, however, increased inputs result in even greater payoffs in output, particularly if new technology is applied. Many archaeologists appear to envision intensification as necessarily the *improvement* of input/output ratios -- i.e., as cultivation strategies change, increased caloric input must result in higher ratios of caloric output if we are to speak of true intensification. This definition is attractive because it has direct implications for motivation: producers get an immediate energetic payoff for additional increments of input, and carrying capacity rises. It cannot, however, incorporate other kinds of processes we might include as intensification, such as when reduction of fallow in swidden systems entails more labor input per cropping episode, but with diminishing yields. Putting in more time and effort might involve either increasing or diminishing outputs depending upon circumstances. In any case, quantification of input/output ratios is essential to the evaluation of any process of intensification.

Modification definitions

Intensification can involve new cultivation strategies that entail landscape (geo-intensive) modifications, such as terraces, canals, chinampas, drained fields, etc. -- these are sometimes called *landesque* features by archaeologists and geographers because they are comparatively permanent and obtrusive parts of the landscape. Ancient features of this kind are also easier to detect than more subtle kinds of intensification. I use the term *landesque* throughout this paper in its original sense of durable agricultural features, but Maya archaeologists increasingly refer to many other kinds of *landesque* capital -- sacbeob, fortifications, etc.²¹⁸

Some kinds of *landesque* agrarian modifications are very simple and not at all durable. An example is contour hoeing on steep hillsides in the Copan Valley (Fig. 16). This practice, widely used in highland Mesoamerica where soils are light and friable, helps retain soil moisture and inhibits erosion (although if farmers had access to good low-slope land they probably would not cultivate hillsides like

the one pictured at all). Contour hoeing requires only energy inputs, but it would probably be prohibitively costly without the light, wide, cheap steel hoes that farmers use today. Whether ancient Mesoamerican technology would be effective is doubtful. It is, in other words, heavily technology dependent. Abandonment of fields would quickly result in obliteration all traces of such ancient hoeing.²¹⁹

The mere presence of landesque features might suggest to archaeologists that intensification occurred, and in a sense this impression is correct. Permanent alterations require capital investments of energy and materials. But modified landscape features in themselves tell us nothing directly about input, output, or the ratios between them. Landesque features are almost reflexively seen by some archaeologists as indicative of large inputs of organized labor, and hence of political complexity and elite mobilization and management. The slightest familiarity with the ethnographic record shows, to the contrary, that impressively large drained fields, terrace systems, dams, etc. are often built by very small social units, including households, even under highly egalitarian social conditions (the massive rice terraces of the Ifugao in the Philippines are examples). Often landesque capital improvements start out on a very modest local scale (see illustrations below), and are co-opted and enlarged by elites. For example, small chinampa systems existed in the southern Basin of Mexico lakes before the fifteenth century. Later construction of massive canals, dykes, and other hydraulic features in the second half of the fifteenth century was only possible because of highly centralized intervention by the Aztec state. Interestingly, the initial motivation for major water-control infrastructure appears to have been flood control to protect Tenochtitlan. The chinampa expansion in Lakes Xochimilco and Chalco was a byproduct of this effort (Luna 2014).

In some parts of Mesoamerica, farmers built terrace-like structures across small local drainages like the one in the Teotihuacan Valley shown in Fig. 17. The walls functioned as dams or silt-traps, and the soil level behind them gradually built up by deposits from seasonal flooding. Such constructions created new fields with deep, humid soils. The “terrace” wall shown is several meters high and such walls can extend laterally for 100 m or more. Although they create new micro-niches for cultivation, the area of trapped soil is usually much smaller than the area of higher slopes that erodes to provide the soil, so there is a net loss in planting surface in the locale. Despite their imposing size, household labor applied over many years built such landesque features. Their scale, much larger than most Maya terraces, is a sobering reminder of the potential of small, steady increments of labor.

The modern Maya terraces illustrated below also caught water and silt running down small hillside swales in the Copan Valley, but they are tiny compared to the Teotihuacan one. Erosion has blown out the terrace walls of the system shown in Fig. 18, but new ones can be built quickly and cheaply (Fig. 19).

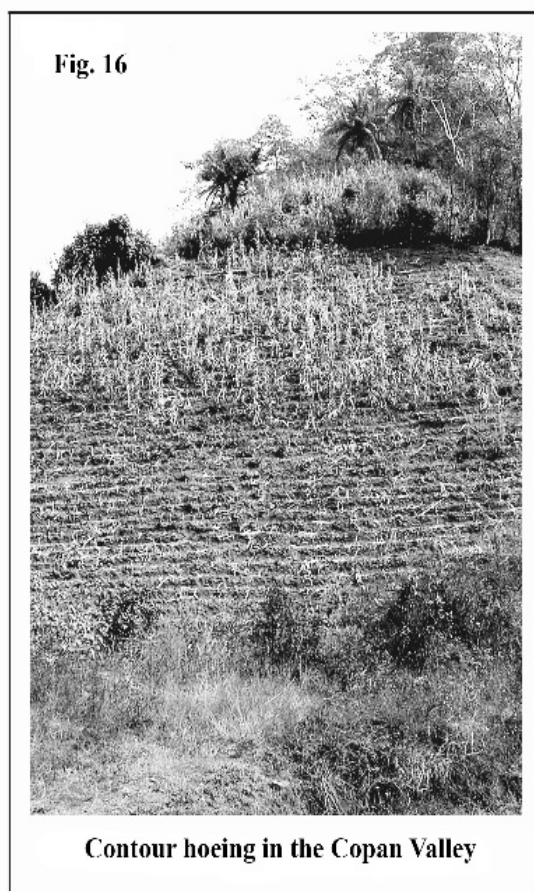




Fig. 17 A large terrace or silt-dam built across a small drainage in the Teotihuacan Valley, Mexico. Photo courtesy of William Sanders.



Fig. 18. Small terraces or silt-traps across a little swale at Copan. The walls have been blown out by erosion.

The little Caracol terrace shown in Fig. 20 is different from the swale terraces because is not positioned across a drainage, but instead along the contours of a hillside. Land tenure determines willingness of farmers to construct such features (see Appendix E). Check dams or soil traps are conspicuous in recent LiDAR images from Copan but their dates are unknown.



Newly-constructed silt-trap terrace on a Copan hillside. **Fig. 19**



Fig. 20: Tim Murtha beside one of the many terraces he mapped at Caracol, Belize. Photo courtesy of Tim Murtha.

Complicating factors

Intensification does not necessarily increase productivity either from the perspective of a region or a plot of land, but might be a response to degradation. For example, terracing in upland areas of the Maya Lowlands might reflect attempts to stabilize eroding soils and maintain or increase soil depth and moisture retention. Tim Murtha (2002) found that terracing around Caracol did not improve input-output ratios or raise annual yields on fields, but instead made these yields more sustainable over time. In this case, productivity was not raised over the short run, but the productive potential of the landscape was increased in the long run. Terrace building was essentially a coping strategy, and we should not reflexively take it as evidence of increased well-being, efficiency, or productive capacity of farmers. Fertilization can counteract loss of soil or soil fertility and is a practice that is simultaneously bio-intensive and geo-intensive, although most people would associate it with the former. The amount of land so “improved” might actually yield less than earlier extensive (but unstable) cultivation of hillsides -- i.e., energy input and landscape modifications might increase while output decreases. In this case, the more “intense” strategies do not increase food supply for a producer unit. Nor do they necessarily imply the capacity to support denser regional populations, or particularly sophisticated management of agricultural resources. Nevertheless, we would probably say that a landscape exhibiting such features was more intensively used, and that intensification was density-dependent insofar as it was exacerbated by increasing numbers of cultivators and consumers through time who damaged hillsides in their pre-terrace states.

Another factor is the ability of farmers to move efficiently to and from their fields for agricultural tasks and to bring harvests to their residences. Insofar as farmers live in dispersed households near fields, mobility problems are few so long as land is a relatively free good. Reina's (1967) celebrated

study of Peten milperos presents another picture. Farmers in nucleated villages such as San Jose on the north shore of Lake Peten Itza, cultivate nearby milpas but also fields at considerable distances. This is possible because they use small watercraft to travel to and transport loads from various points along the lakeshore. Without this option, the landscape near San Jose would no doubt be used more intensively.

Inputs of energy, time, materials, or technology might be adaptations to risk rather than the necessity to increase average yearly yields, whether from the perspective of whole regions or particular plots of land. In good years, such systems might not appreciably add to production, but they might prevent crop loss or unacceptable production shortfalls in bad years. Their long-term effect is to raise productivity by eliminating short-term risk. The intent is not to produce more on a given part of the landscape over the long run, but simply to guarantee certain required levels of production in the short run. For many risk factors, such as low or erratic rainfall, some forms of what we take to be intensification thus are not density dependent. Nor do agricultural strategies fall nicely into some sort of “evolutionary” sequence in which there is a general Boserupian progression from extensive to intensive.

Example: The construction of drained fields in the Maya Lowlands began in Preclassic times long before the upland agricultural landscape was saturated with farmers. There was no shortage of land for swidden agriculture, but drained fields helped people cope with the problem years with insufficient rainfall for upland crops. Intensification is often thought to raise or stabilize yields over some expanse of *space*. Insofar as risk-reduction motivates intensification, its effect is to raise or stabilize yields over some expanse of *time*. The latter strategy helps insulate populations against periodic mortality crises caused by insufficient harvests.

As an aside, there are important differences between drained fields and raised fields despite their superficial similarity. Both kinds of landscape features create planting surfaces in moist or wetland soils. Drained fields channel away water and so elevate planting surfaces while retaining moisture in soils that tend to be clayey and thus difficult to cultivate with hand tools. If water levels are not well-controlled, crops (especially maize) are vulnerable to conditions such as root-rot. Raised fields such as Aztec chinampas are built up much higher above the water level and repeatedly fertilized with organic material from canals. Soil nutrients and texture are thus frequently renewed and moisture problems minimized except in times of heavy flooding. Drained fields appear to be more common in the Maya Lowlands.

In some cases, and especially where market exchange is prevalent, commercial exchange or profit motives might stimulate intensification. This occurs even in many pre-modern and non-industrialized societies. Farmers switch production to specialized crops that might include non-edibles such as cotton, tobacco, flowers, etc. Such crops might themselves be completely unsuited to directly meeting the household food-budget or other requirements of the producers. Instead, they must be sold or otherwise exchanged for subsistence resources. I call this intensification because the exchange value of the specialized crop might in fact result in a higher return of subsistence commodities (say for example maize) than bulk staples: a kilogram of flowers grown in a field might be worth more than a kilogram of grain grown in the same field. Such specialized production is most useful where there is a market system, where values of the specialized crops are high and reasonably predictable, where transport is efficient, and where large numbers of consumers are nearby, as in a city. It also presumes there is surplus production of subsistence crops elsewhere in the system so that the “market” (or some other mechanism) can redirect the food surplus to the specialized growers.

Example 1: Many geographers have observed that farmers with fields located near preindustrial cities use them to grow herbs or fruits or flowers that cannot serve to directly sustain their households (this is a well-known pattern in China) or feed the urban population. Such crops can be profitably sold in nearby urban markets, where the household can purchase its dietary staples and other necessary foods.

Example 2: Pineapple can grow on steep hillsides and on very thin, depleted soils. While doing survey in the Copan Valley, we saw many damaged maize fields converted to pineapple production. Assuming that pineapple prices are high, more maize could be purchased with the profits from selling them than could be grown on the field.

Many forms of agricultural intensification result in the capacity to support increased overall population densities in any large region, or at least to sustain existing numbers longer. These outcomes might be positive ones from the perspective of elites and managers. Because intensification is typically associated with increased per capita energy inputs and decreased outputs (lowered average or marginal productivity), as well as degrading landscapes, it is not necessarily beneficial from the perspective of farmer well-being or long-term ecosystem stability. Various ecosystems exhibit wide ranges of resiliency with respect to such pressure.

Example: The ruler of an agrarian state taxes peasant households at the rate of 10% of their crops. He gets more tax from three farmers each working hard to produce 1000 kg on three one hectare fields than he will from a single farmer working less hard to produce 2000 kg per hectare using a less intensive strategy. This is one reason why, from the perspective of elites and rulers, agricultural landscapes might seem under-populated. Up to a point, adding farmers to a landscape results in more revenue for the political economy, and until some sort of crisis occurs the well-being of the individual producers is not a major concern.

One form of bio-intensification available to the Maya and much debated in the literature is adoption of staple crops other than maize that produced higher yields and had fewer deleterious effects on the environment (see Appendix C). Alternative staples included tree crops like ramon, but more importantly root crops such as sweet potatoes and manioc. Both of the latter were certainly available because they have great time depth in the Maya Lowlands. Ancient Maya gardens at Ceren have yielded manioc, along with another potential staple, maguey.

Decision-making

Intensification strongly relates to cultural factors, most importantly the nature of tenure to land, as Boserup insisted. Some models of intensification presume that cultivators (often called smallholders or shareholders) control access to their own lands and other agrarian resources and are the prime decision-making agents about agricultural strategies. This is not true in many cases. Sometimes top-down control and decision-making are wielded by landlords, nobles, kings, or other kinds of proprietors. Under these circumstances, farmers might not have much, or any, decision-making input or motivation, including decisions about what to grow or whether to intensify. They have little or no agency. The question here is the age-old one of “who benefits?”

Examples: Much hillside land in the Central Andes, especially at intermediate elevations, was converted into complex systems of irrigated terraces by the Inka state. This form of intensification had little to do with population pressure or risk in the density-dependent sense, but was part of a command economy. A major purpose of such intensification was to grow maize to make *chicha*, or maize beer, essential to the Inka political economy. Systems of terraces linked to royal estates provided many resources for their elite proprietors. Similarly, large-scale flood control was first effectively used in the Nile Valley in the nineteenth century when huge hydro-engineering projects became feasible. Much of the land thus improved by intensification did not grow the traditional crops central to the diets of Egyptian farmers, but instead was devoted to cash crops such as cotton or sugar cane that for exchange on international markets.

Archaeological interpretation of intensification

Ethnographers, economists, agronomists, botanists, geographers and ethno-historians face different (and usually fewer) challenges in evaluating processes of intensification than archaeologists. Their advantage is much better information about the systems they study. One big problem for archaeologists is simply detecting various kinds of intensification and its scale, by which I mean both the scale of the features themselves and their potential economic significance. Even if features like terraces are visible in remote sensing images, they can be confused with field boundaries, animal tracks, etc. Many kinds of *landesque* features such as irrigation canals, terracing, or drained fields are also very difficult to date. It is easy to assume that a system of drained fields appeared late in a local agrarian system and was a density dependent phenomenon, but it might be a much earlier adaptation to risk. Given the general correlation between intensification and altered patterns (usually more restrictive) of land owning or proprietorship (see Appendix E), it is very difficult to determine in archaeological contexts who the decision-making agents were and who benefitted most. Sometimes the apparent absence of intensification is itself an issue. I worked for many years in the Copan Valley in Honduras, a region of generally high relief and spatially restricted prime bottom lands. I saw many scattered examples of minor swale terracing as illustrated above. I also saw many steep hillsides that had lost much of their productive potential because of erosion that might have easily been controlled by large, durable contour terraces. Nevertheless, people seldom if ever build such features today. I concluded that many small farmers were not in fact smallholders, but instead cultivated land at the sufferance of big landholders (see Appendix C). Under these circumstances, farmers lacked the motivation to improve land that was not predictably under their control.²²⁰

Four things to remember when discussing intensification in archaeological contexts are:

- 1) The term does not necessarily imply density dependent processes.
- 2) Intensification is usually an additive process, occurring in a mosaic pattern on landscapes where less intensive forms of cultivation are still practiced.
- 3) Intensification does not necessarily increase productive potential per unit of land, overall carrying capacity or population, either in absolute or density terms.
- 4) Intensification does not necessarily require hierarchical levels of decision-making or management.
- 5) It is critical to understand who makes agrarian decisions and who benefits from them.

Models of intensification such as that of Abrams owe much to the classic 1965 formulation of Ester Boserup (see Appendix G). William Sanders (1979: 497) sums her hypothesis up this way: "... population growth forces farmers into investing increasingly higher amounts of labor as the land per capita decreases in quantity." He thought that the Boserup model, based mainly on recent cultures in tropical parts of Africa and Asia, required significant modification when applied to the Classic Maya because of the limitations of their stone tool technology, energy sources, the nature of their soils, and the seasonal rainfall regime (Sanders 1973: 333-334). He also thought that Boserup underestimated soil variability in the tropics and the restricted capacity of many soils to support reductions in fallow or other intensification.²²¹ He concluded that the optimal form of Maya swiddening was bush fallow, which could maintain densities of 100 people or so per sq. km under favorable soil and rainfall circumstances.

One final point relates to surplus food production, often conceptually linked with intensification. For many reasons pre-modern peasant farmers required surpluses in excess of the anticipated dietary needs of their households (see Eric Wolf's classic 1959 work *Peasants*). Quite apart from next year's

seed, ceremonial events, taxes, payments to landlords, etc., peasants produced surpluses to hedge against unpredictable demands on their resources or uncontrollable future production shortfalls. Surpluses might be stored against a future bad year, although in many environments long-term storage of staple plant foods was impossible. In some cases, surpluses might be opportunistically diverted to market transactions or other exchanges that advantaged the household. What matters, however, is adaptation to the bad years, not the good ones. Consider, for example, the staple grain economies of Classic Maya kingdoms, which were highly localized and which operated on highly redundant sections of the landscape. An unexpectedly optimal year might result in a maize harvest that was 50% or more in excess of household requirements (including those of non-farming specialists or elites). But what could farmers do with this surplus? They could not “bank” surpluses by feeding them to domestic animals as Old World people might do.²²² They could theoretically transfer them to other nearby households, but remember that such people very likely had surpluses of their own. The Maya could not transport bulk staples very far, and they seem to have lacked well-developed market economies that would provide outlets. Even had local markets existed, though, grain would be in such large supply that its exchange value would be very low. Surpluses under these conditions are not very useful.

The anthropologist Robert Hunt (2000) argues that agricultural intensification (which, unlike Ester Boserup, he thinks is very hard to measure even in traditional agrarian societies) is not the same thing as agricultural development. Over the long run of human history, ever-smaller proportions of human populations have supported ever-larger numbers of non-food producers. That they have done so is mainly due to increased productivity as intensification occurs. Hunt thinks that increased productivity is almost always results from new technology, which he conceives in its broadest sense (new knowledge, new tools, new crops, new energy sources including animals, etc.). All this he calls *agricultural development* because productivity per worker increases. It is very hard to apply this kind of model to the ancient Maya because effective new “technological” inputs are difficult to identify.²²³ I suspect that by Late/Terminal Classic times (and earlier in some regions) per-capita worker agrarian productivity was stationary or declining, even as their numbers grew. Output of buildings or other non-agrarian landesque features is another matter.

Intensification processes have social and political consequences too numerous to discuss here. For example, if some parts of the agrarian landscape are more productive and stable than others, they promote differentiation in wealth and rank and are prone to eventual expropriation and domination by elites, as happened in much of Hawai’i and the Basin of Mexico. Landesque capital investments also serve to anchor people strongly to segments of the landscape, thus reducing flexibility of choices and mobility during times of stress.

APPENDIX D

Maya Food Shortfalls and their Consequences

Puleston (1968: 3-4) remarked on the tendency for Maya population estimates to rise as new data accumulated, and the incompatibility of high-density estimates with maize cultivation. At that time Maya dependence on maize as a staple was, as he put it, "... still only an assumption, based on very little proof." His solution to the food shortfalls implied by the high Tikal densities was bio-intensification based on ramon (*Brosimum alicastrum*, or ox in Maya, Fig. 21), although he acknowledged that other kinds of food production supplemented it.²²⁴ In this, he followed a much earlier suggestion by Cyrus Lundell (1937: 10), who identified an arboreal association called the *ramonal* that was dominated by ramon trees and spatially associated with Maya ruins.

Puleston was impressed by ramon yields, asserting that "The trees produce up to, and probably more than, 3000 lbs of edible seed per acre. Thus ramons are more than 10 times as productive as maize as it is cultivated in roughly the same area" (Puleston 1968: 118). This claim influenced colleagues like MacKinnon (1981) and others, but it is a puzzling statement. Puleston's figure works out to edible ramon seed yields of 3368 kilos per hectare, an estimate far exceeding the most extravagant maize production figures we have for anywhere in Mesoamerica, including the Aztec chinampa zone. We cannot be sure what average maize yields were in Late Classic Maya times, but the figures of Ursula Cowgill cited by Puleston (1968: 99) appear much too low. Late Classic Maya yields might have been somewhere in the neighborhood of 1000 kg/ha so long as reasonable fallow intervals on good soils were possible and rainfall was sufficient (see Griffin 2012 for production data, including from the Peten).²²⁵ If the farmers Puleston observed around Tikal in the mid-1960s were indeed harvesting only 300-400 kg per ha (single crop or main crop) in good years, as implied by his statement, they were either notably unsuccessful or else they did not depend on maize as a staple to the extent that the ancient Maya did. Others attracted by the ramon hypothesis also sometimes indulged in bad math (see Webster 1981 for ramon miscalculations, and Sanders 1979 for a harsh critique of the math behind the artificial forest ecosystem model). Nevertheless, considerable (if short-lived) enthusiasm for the ramon hypothesis took hold. Although he changed his mind later, Sanders initially concluded that "Under the extreme population pressure of the Late Classic period, the Maya gradually shifted away from a long-cycle, maize-based swidden system to a short-cycle, root crop one, combined with permanent ramon orchards" (1972:12). Don Rice (1975: 55) echoed this view: "By the end of the Late Classic period no conditions existed under which maize could have been the staple crop." Puleston (1968: 114) even speculated that the extremely high population densities of the Late Classic at Tikal relegated maize to the role of luxury food for elites, grown mainly on special estates or reserves.



Ramon fruits -- probably not a Maya staple as Puleston thought.

Fig. 21

Enthusiasm for ramon (or root crops) as staples waned swiftly after the early 1980s because of our better understanding of the paleobotanical record, and because isotopic studies eventually showed considerable, and sometimes very high, dietary reliance on maize (Gerry and Krueger 1997; Reed 1998; Somerville, Fauvelle and Froehle 2013). Commoner Maya diets varied little over the 600 years prior to the collapse, and maize was always the main staple. Sanders reversed himself, declaring that, "... maize was the major crop of the Maya from the beginning to the end of the sequence..." (1979: 504), although he thought fallow reduction increasingly intensified maize cultivation. Direct archaeological evidence for ramon use is sparse. Ramon seed macrofossils (as opposed to ramon wood) are not identified in the most recent list of tree crops found at Tikal (Lentz *et al.* 2014: 10; Lentz *et al.* 2015: 175). Whatever the stresses and shortfalls of the Maya agrarian economy, farmers do not seem to have adapted by wholesale switching to tree or root crops as bulk staples.²²⁶ The Maya ideological and symbolic fixation on maize from very early times is also ever more apparent.²²⁷ See Tokovinine (2013: 155-123) for the high incidence of maize (-nal) suffixes in Classic Maya titles and place names.

Apart from various forms of local intensification, well-populated regions like the Tikal kingdom might have counteracted shortfalls by limited importation of food. Archaeologists debate about how self-contained Maya polities were in terms of their subsistence economies, and whether such transfers of bulk food were feasible on a large scale. My opinion is that staple economies were usually extremely localized, and other archaeologists working at Tikal agree (Lentz *et al.* 2014). This is an important issue because it relates to the capacity of a polity like Tikal to acquire cheap bulk food by various means such as warfare, redistribution, or trade, from distant places. The following discussion refers to the Maya landscape in Late Classic times when it was demographically saturated, and specifically to Tikal and its neighbors at about AD 700-830. By this time, defection or emigration to lower-density parts of the landscape or to other kingdoms was probably not a viable solution to local food resource shortfalls, at least for large numbers of people.²²⁸ I also assume that the capacity for rapid agricultural intensification resulting in improved or less risky harvests was not feasible by AD 700-830, or at any rate was rarely undertaken. This appears to be the case for Tikal.

Food shortfalls in Maya polities had numerous scales, causes and patterns, particularly with regard to risk and productivity (see Sanders and Webster 1978 for the interplay of these two factors). In all cases, the issue is whether some mechanisms of exchange, redistribution, storage, or appropriation (e.g., through warfare) might have been effective short-term buffers against such shortfalls. Several constraints affected any such solutions, whether the shortfalls were local (intra-polity), regional or pan-Lowlands:

1) Basic bulk foods could not be efficiently moved very far in large quantities (see Sanders 1978: 503- 504). This is especially true if transport occurred within the context of market exchanges because the losses in transit would undercut profits.²²⁹ Limited medium-distance transport of small amounts motivated by political demand is feasible, as we shall see below.

2) Even if large quantities of cheap, bulk foodstuffs could be moved long distances efficiently, a major problem was procuring them.²³⁰ Bottlenecks related to Maya household labor, time and technology mean that Maya farming units under the best conditions could produce (or would be motivated to produce) only small surpluses above household dietary needs. I put such surpluses at about 50% of their domestic food requirements even under the best conditions of land availability, good soil, and rainfall. Moreover, much of this "surplus" was essential to households as seed, ceremonial fund, or as a hedge against short-term uncontrollable risks, and thus could not easily be alienated (see Webster 1985).

3) Any uneaten or otherwise unused surpluses from good annual harvests could probably be stored only for about a year, so there was no ability to "pile up" surpluses over a series of good years. Nor could annual surplus staples be diverted to large domestic animal food reservoirs as occurs in the Old World.

4) Maya elites probably lacked the political ability (or will) to stimulate excess food production on a large scale, especially if the landscape was demographically saturated. Even if they could somehow try this, they probably had insufficient managerial or extractive clout to do it successfully, especially under conditions of stress. Complicating this issue from the royal perspective is the presence of sub-royal lords who had their own ties to farmers and who might interfere with, resist, or ignore the decisions of rulers.

5) Maya agricultural landscapes had considerable local variability. Many events that caused shortfalls were not evenly distributed, but there was nothing like the ecological variation characteristic, say, of Central Mexico or highland Guatemala.

Scenario 1

By the Late Classic, farmers in polities like Tikal lived on landscapes that were more variegated than in earlier times, partly because of human niche construction. Stresses related to shortfalls (for whatever reasons) were accordingly distributed unevenly within kingdoms. One solution was to take “surplus” food from local farmers who were comparatively well off and redistribute it to those in trouble nearby. Although energetically feasible, such a solution would probably be very politically divisive unless it was carefully negotiated, and operated only for a short time and on a modest scale. Farmers might resist and even more importantly, sub-royal lords might defend the client-farmers to whom they were linked by kinship or clientage. If the Maya had a monetized/market economy, foodstuffs might have flowed from outside the local polity to where they were in short supply and prices were correspondingly high.²³¹ The Maya apparently had no mature economic institutions of these kinds.²³² Even if they had, what resources would a commoner family possess to trade for food to make up their shortfalls? And of course when staples were scarce, their “prices”, whatever that might have meant to Maya farming families, were at their highest.²³³ Late/Terminal Classic demographic saturation meant that the commoner resource in greatest supply was labor, but how could it be applied in ways that paid off energetically?

Scenario 2

Some sort of large regional event (say a major hurricane) wiped or severely damaged local food crops throughout an entire polity. Even if the money/market model operated, how would the system in deficit “pay” for imports from neighboring polities? Supposing that nearby independent polities were unaffected by the event, it is conceivable that appropriation of their resources through warfare or threat might occur, but of course this just creates the same shortfall problem for a different region. If neighbors were allies in the traditional web of Maya sociopolitical relations, one polity could not pursue the appropriation option, which might only work if neighbors were enemies.²³⁴ Localized raiding for foodstuffs from enemies might be a short-term solution, but not if there were recurrent crises because the productive capacities of the donor (or victim) polities would degrade over several years. Actually conquering neighboring polities probably would not be very effective in the long run because overall populations (food demand) would not thereby be reduced very much, and in any case the Classic Maya seem not to have been effective at creating and administering sociopolitical systems larger than the local kingdom.²³⁵

Scenario 3

There were widespread shortfalls in food production over very large areas, or perhaps over the whole Lowlands, because of a mega-event such as a major drought. Under these circumstances, there were no reservoirs of food anywhere. Warfare or other processes of redistribution could do no more than situationally address shortfalls in a feeble way.

Look at this in a couple of other ways. Imagine that a producer family had a serious food deficit for whatever reason. It had no market option (even if markets existed) because it had few or no resources beyond its labor and its annual produce, or perhaps some limited craft products.²³⁶ Nor could the affected family easily decamp for some better part of the landscape.²³⁷ Assuming that the shortfall was local and episodic, families might hope relatives or local lords would tide them over for a very short time, assuming that such people still had resources. These options were cut off if the whole polity or region experienced a shortfall. Neither could the family farming sector collectively expect a bailout through imports from neighboring polities for reasons already discussed, even if kings or other leaders tried to negotiate or enforce such redistribution. Sufficient surpluses simply would not exist. My conclusion is that under circumstances of a regional or larger shortfall farmers are pretty much screwed.

Kings and elites are another matter.²³⁸ We have long known that kings had far-flung relationships with one another, and evidence is accumulating that sub-royal nobles had them too. For example Ajpach' Waal, a second-tier noble at El Palmar in southeastern Campeche, had some sort of relationship with the 13th king of Copan and possibly even visited him (Tsukamoto 2014: 62-63). Insofar as such grandees through marriage, conquest, or other factors had access to the products of estates in distant polities (perhaps the *kab ch'en* noted above) or tribute from them, they could arrange for imports of food on scales that would buffer their own households (see Appendix E below for an extended discussion of the issue of Maya estates). That is, one could not bail out an entire local population by moving large amounts of food long distances – say 100 km or more – but lesser amounts could be imported over such distances to sustain royal and elite households, assuming that there were distant reservoirs of good food production. Transport losses during such movements could be largely ignored. Movement of food on this scale would have been particularly effective between regions that differed in their local ecologies and hence their risk factors -- obvious examples are Copan and Quirigua. Prior to the political schism of AD 738, Copan kings could easily have imported food from the Motagua Valley in amounts needed for their own households. Similarly, Tikal elites could have supplied their households with food from Uaxactun, Yaxha, or even as far away as Caracol. On the other hand, Ajpach' Waal could not have been much help to his Copan noble compatriots, who lived almost 400 km away as the crow flies. Alternatively, elite people (though probably not kings) could actually go and live with other elites – particularly relatives – at distant courts until the emergency at home abated (we have no evidence that anything like this ever happened). If kings and lords were buffered in some of these ways while their subjects starved, there is obviously great potential for internal discontent.

One very abstract way to think about all this is to use a weather analogy. Imagine regions with sufficient staple foodstuffs at any given time as high pressure zones, and regions with shortfalls as low pressure ones. What would make (or enable) pressures to “equalize” in some sense proportionate to the scale of the shortfalls?

Ideological and moral ecology

All available evidence points to the Maya as “maize people”, locked heavily into maize not only as a staple food, but as an essential cosmological and cosmogenic element.²³⁹ The terms ideological and moral ecology might seem odd, but I think the Maya fixation on maize at all levels helps us understand their Late Classic predicament and their failure to adopt better agrarian strategies. The huge ideological significance of maize is apparent at least as early as the first kings, most blatantly in the murals of San Bartolo, and was no doubt present long before. Estrada-Belli (2011: 43) extends it back to pre-Mamon times. In my opinion, this early ideological vigor reflects the enormously successful adoption and spread of maize as a staple during the Early and Middle Preclassic. Maya rulership evolved partly through the pretensions of kings to keep the world ordered, and the central outcome of that order was reliable maize

production. Maize early on became central to world creation and to the cultural identity of kings, both individually and as members of larger dynasties or collectivities. In a universe that required balance and order for agrarian success, kings came to exemplify and embody a moral imperative closely linked to agricultural productivity, which was generally successful during the Preclassic. Maybe it is not too fanciful to borrow a term used for Chinese rulers and call them *cosmocrats* (Clunas 2007: 26).

For a long time, rulers made political and ideological hay (no pun intended) by adopting maize names and titles. Credit accrued to kings in good times, but bad times signaled their moral failings, and here I use the word moral in the broadest sense – personal failings of rulers to deliver the ritual guarantees of order and prosperity.²⁴⁰ Maize-centric ideology and agrarian well-being usually meshed effectively before AD 500. The demographic crunch over the next two or three centuries made it virtually impossible to jettison maize in favor of some widespread non-maize staple (no kings, revealingly, made the jump to manioc or sweet potato titles). In currently fashionable terms, their predicament was a classic rigidity trap (see various chapters in Fauseit 2016 for applications of this concept). I hasten to add that this is not an ideological explanation for the Maya “collapse”, but rather a materialist one. Early agrarian well-being gave rise to a maize-centric world and political view whose later inertia precluded adoption of other staples on a large scale. Agrarian failures implicated kings during the Late and Terminal Classic precisely because of maize-centric ideological pretensions were successful for so long. And of course many other enhanced dimensions of instability that also afflicted the Late Classic landscape, most notably warfare, are directly or indirectly related to demographic history and the fortunes of rulers.²⁴¹

APPENDIX E

Agrarian Capital, Land Tenure, Inheritance, Entitlements, and Agency

Much of the previous discussion focuses on human-environmental relationships and processes. Equally important are the human-to-human relationships that bear on agrarian adaptations. A short anecdote introduces this topic.

On May 5, 2011, the Mississippi River was in full spate while an NPR reporter interviewed a farmer near Vicksburg, Tennessee. As they looked out over 500 acres of the farmer's flooded bottom lands, the reporter asked him why he didn't farm elsewhere. The farmer answered, "It's all spoke for." This little vignette nicely sets up three observations:

- 1) Competitive exclusion (impeded access) causes people to use productively marginal or high-risk segments of agrarian landscapes, in this case alluvial soils vulnerable to flooding.
- 2) Humans adapt through time to niches that they themselves construct and inherit. The Mississippi floodplain is reasonably secure for modern farming only because of the efforts (not entirely successful) of the U.S. Corps of Engineers to tame the natural river and create artificial ecosystems.
- 3) People relegated to marginal or high-risk locales are highly vulnerable to reduced well-being, with consequent impacts on their larger sociopolitical systems.

Our farmer's complaint was actually a bit disingenuous about his competitive exclusion. He cultivated his 500 acres of floodplain not just because other parts of the agrarian landscape were already "spoke for", but because the rich alluvial soils were prime resources. No doubt he made a fine living during good years, and would not in fact have wished to cultivate inferior upland soils. For him risk was a relative concept. With a little luck, he could resume farming when the floods receded. Even if this were impossible, he and his family were buffered from the worst consequences of risk by many well-established institutions of the modern American economy and state, and if he chose to abandon farming altogether, he could reasonably expect to find other employment. Maya farmers faced similar problems, but they were largely un-buffered on increasingly competitive and risky agrarian landscapes.

Cultural anthropologists have long been aware of the complexities of agrarian niche construction, land tenure or ownership, and inheritance. I recently re-read an old classic ethnography called *Struggle for Land* (Brookfield and Brown 1963) that is broadly concerned with ownership of and access to land as a capital resource in a traditional horticultural society, the Chimbu of the New Guinea highlands. At the time of European contact, the Chimbu had high population densities and intensive agricultural systems, which they maintained under conditions of strikingly egalitarian political organization. Almost all arable land was claimed or "owned" by individuals -- it was "All spoke for" as our Mississippi farmer said. Claims to land were manifested and legitimated in various ways, most clearly by original acts of clearing and cultivation, but sometimes by conquest. Capital improvements to land, such as planting trees, also validated ownership. Despite socially recognized individual custodianship, the Chimbu readjusted access to land in many ways apart from war. It was inherited, but was also given away as gifts or dowries, and loaned to affines and cognatic kin, usually within clans and sub-clans. Ownership and other rights of access were carefully remembered through time. Disputes nevertheless occurred, as when someone who borrowed land asserted enhanced claims to it because of improvements such as tree planting. Chimbu land ownership was strongly linked to unusually high population densities and a long contingent history of land utilization, which obfuscated rights through time. Two things struck me forcibly about the Chimbu. First, how intricate and complicated all these various forms of ownership, access, and inheritance were, and how we could not understand Chimbu society without detailed

knowledge of them. Second, and more depressing, how utterly impossible it would be to reconstruct these economic conventions from archaeological data alone.

Ester Boserup (1965: 14) asserted that “... the degree of security of tenure for the cultivator is one of the important determinants of investment”, and she devoted much of her book to this topic. Boserup thought that specific sequences of economic and agricultural change usually resulted in agrarian systems with mixed tenure. Nevertheless, she devised a long-term evolutionary process that began with a stage of collective community tenure and ended with private property, thus echoing Lewis Henry Morgan (1963: Part 1V).²⁴² What does all this have to do with the Classic Maya? Archaeologists pay much attention to the social histories of objects, but little to the social history of land, mainly because it is so difficult to detect or reconstruct. In Appendix C, I treated landesque features mainly as infrastructure important for production, which downplays their role as property and their inevitable linkage to community and social networks. The long-dominant model of Maya shifting cultivation also made conventions of land entitlement or ownership easy to ignore. For many years I have been convinced that the most essential thing to know about the Maya is their practices of agrarian land-holding and inheritance (Webster 2002; 2005; Webster and Murtha 2015).²⁴³ This is an old issue well addressed by Patricia McAnany (1995), who argued strongly for lineage entitlement and inheritance of individual fields based on ethnographic and ethnohistoric data.²⁴⁴ It matters because demographers have long known that how kinship groups are structured and how they relate to land as agrarian capital affect reproductive choices and fertility regimes, as well as disease and health patterns (Das Gupta 1997 gives a good overview).

Unfortunately, we cannot uncritically use as models the broadly shared patterns of access and inheritance that operated in Yucatan during the Contact period. Quezada insists that elite authority during the highly fragmented political conditions at that time was jurisdictional and not territorial.²⁴⁵ With regard to land, he says

“It would have been incomprehensible and alien to Yucatec reality to speak of the lands of the halach uinic, of the batab, of the ah kin (sun priest), of the almehenhob, or of the lineages themselves. Neither the Spanish nor the indigenous documents of the Colonial period indicate that the pre-Hispanic nobility had direct dominion over the land... Simply put, the contact-era Maya lacked the concept of private land ownership” (Quezada 2014: 21).

Quezada obfuscates the issue here, because private land ownership in the western sense is far from the only convention by which elites or farming families might claim some sort of predictable access or custodianship to segments of the agrarian landscape. In Quezada’s view, the relations among elites (jurisdictions) are presumably what entitled nobles to products and labor of affiliated farmers, who must certainly have had interests in stable access to land. Kepecs and Masson [2003: 42], who are partisans of Quezada, emphasize contested boundaries for batabil lands and territorial disputes. But even if we accept Quezada’s contention, it has few clear implications for the much more durable, centralized and densely-populated Classic polities to the south.

Ruben Reina’s ethnographic study *Milpas and Milperos* (1967) briefly addresses the issue of land tenure in swidden-farming communities south of Tikal around Lake Peten Itza. At the time of his research, land was not privately owned and was “conceptually a free good” (p. 3). It was nevertheless subject to certain restrictions including usufruct claims and the requirement that land be registered with municipal authorities, an act that conferred contingent legal title of a kind, but not ownership. Land could not be sold, loaned or rented to others, but individuals could “inherit” rights to land with the permission of the town authorities. Although Reina does not elaborate on exactly what managerial functions the municipal authorities served, the implication is that contingent legal access was necessary to dampen competing claims. Even though the region had (and has) low population densities, farmers possessed

very elaborate knowledge of, and preference for, specific micro-niches within the broader categories of mature and regenerating forest, and these greatly reduced their optimal choices.²⁴⁶

Similar local micro-niche factors conditioned how access to land was conceived and transmitted during Classic times as well. Such conventions at Tikal, for example, probably differed markedly from those at Copan, where pockets of good alluvial soils, some irrigated, formed the primary agrarian capital. Rich families probably dominated these zones just as they do today. Nor should we expect royal and elite inheritance to mirror that of commoners. Large and powerful Late Classic kingdoms like Tikal developed distinctive patterns of inheritance that reflect centuries of elite marriage, warfare, status rivalry, and other historical contingencies.²⁴⁷

Trying to understand patterns of access and inheritance is especially difficult if the Classic Maya in most regions depended on major food inputs from some sort of shifting, swidden agriculture. As Sanders (1962: 85) noted long ago, decisions to shift fields depend not only on input vs. yield ratios, but also on the areas available to cultivators as constrained by biotic and human factors. Both of these variables were very dynamic, and by Late Classic times at Tikal and elsewhere, Maya agrarian landscapes must have been quite literally constructed and inherited niches, both in the ecological and the social senses.²⁴⁸ Those who lived on them consequently experienced increasing variance in well-being (see Wood 1998) and attendant competition of many kinds. If large parts of the landscape were an intricate mosaic of intensification and/or a complex managed forest system, they would be vulnerable to many kinds of social and ecological disruption. I suspect that during Late Classic times each regional landscape consisted of a patchwork of tenure and inheritance practices, as Boserup predicted.

Unfortunately, we have no good insights into how all this might have worked, especially with regard to the social groups and networks (shareholders) that had somehow to map onto agrarian resources. I generally presume, following McAnany and Vogt, that the ancient Maya had patrilineages, as do various authors in Lentz *et al.* (2015). We typically find Classic Maya burials in household precincts, and there seems to be the broad presumption that some sociologically distinguished burials functioned in part as ancestral claims to land.²⁴⁹ Such ritualized and symbolic claims might work well with respect to the hard structures of the household and its immediately associated gardens or orchards, but their implications for more detached parts of the agrarian landscape are problematical. And if something like this did occur, it might have engendered bottom-up status rivalry as populations grew and land became less available and more variegated over time. Ancestors, whether royal, noble, or otherwise, begot many lines of descent and many dimensions of potential lineage dispute as well as cooperation.

Complicating such suppositions is the possibility that powerful Classic Period rulers diminished or suppressed the lineage organization and prerogatives of low-ranked people, just as Hawai'ian kings and chiefs did. Contact period and ethnohistoric accounts from northern Yucatan provide some detailed information, as summarized by Matthew Restall (1997), McAnany (1995) and Quezada (2014). Some nobles had privileged access to various parts of the agrarian landscape, including cacao plantations. Ordinary households broadly shared swidden-cultivated outfields via usufruct within the lands of the *cah* (the local polity), and the *chibal* (the local patrilineage). Men and women within the family variously inherited house infrastructure, kitchen gardens, and orchards. I agree with McAnany that commoner lineage organization was well established among the Preclassic Maya just as it was among their sixteenth century counterparts. My opinion, however, is that these conventions of early and late, low density Maya populations lacking powerful kings provide us with few dependable insights into the southern Classic Maya during their demographic apogee. Lineage organization is really only well documented for Classic elites, and especially for royalty. Haviland (2008: 269) argues that at Tikal "... those households most likely to be occupied by farmers do not appear to be organized according to lineage."

Also inhibiting our understanding is that specific conventions of inheritance (e.g., partible, impartible, communal, usufruct) must have shifted markedly through time at Tikal and elsewhere. Inheritance resulted not only in redistribution of land, but also in transformations of kin/affiliation relations and factions, and the relationships among them.²⁵⁰ If, as I have argued, population was generally low before AD 500-600, then something like the Contact-period patterns might have worked reasonably well, with general usufruct access to outfields and perhaps family ownership of orchards and gardens. Abrams (1995) suggested that authorities such as lineage heads might have been mediated all this, assuming they existed. Peak densities attained in the eighth and ninth centuries, along with an attendant burgeoning of lordly people, might have made such management no longer been feasible. At some centers, there are natural and cultural features that provide intimations of how ownership might have worked. It seems likely that at Copan elite groups dominated the deep alluvial soils on the valley floor, just as they do today. Resident commoners at Caracol probably had privileged claims on terraced hillsides. Unfortunately, despite the importance of various forms of agrarian capital and numerous agrarian metaphors associated with kings, surviving Classic inscriptions "... simply do not relate to agriculture or landholding" (Tokovinine 2013: 55).

The problem of Classic Maya elite estates

Contact period Mesoamerican kings, nobles, and institutions such as temples held rights to land, and to the products of land and labor, through outright ownership, prebends, usufruct, or endowments. Dependent farmers and sometimes slaves cultivated such "estates".²⁵¹ Puleston (1968: 114; 1973: 229) surmised that similar estates might have existed on Tikal's Late Classic landscape, even though his understanding of the nature of Maya elites was then comparatively vague. He suggested that Classic Maya estates, given limitations on land, were still cultivated by slash-and-burn methods to provide maize for "... the gastronomic inclinations of an increasingly imperious elite" who apparently disliked ramon and other staples grown more intensively by commoners. Maize, he surmised, had become a culinary status symbol as well as a core ideological value. Puleston's speculation now seems quaint and doubtful, but I think the more general estate idea is a provocative one, as do several authors in Lentz *et al.* (2015). It deserves more consideration, especially given the expansive and hegemonic dimensions of Maya politics discussed below in Appendix F.

One kind of estate found in parts of the Old World was created by top-down capital investments that improved waste or marginal land for cultivation and settlement. Such projects were typically "grub-staked" in their early stages by powerful rulers, nobles or institutions, and then became the property, or endowments, of the elite individuals or institutions and managed on their behalf. Such royal foundations or domains were common in Egypt during Early Dynastic times (Wilkinson 2001: 116-127). The Egyptian king Sneferu funded his great pyramid-building efforts partly through the foundation of many estates along the Nile that eventually also paid revenues to support temples, pyramid-towns, and the royal court (Lehner 1997: 228- 229). Elites underwrote similar transformations at various times during the history of Japan. In many such cases, the new agricultural infrastructure (literally newly created wealth) required major landesque additions, such as local drainage and irrigation systems in Egypt, hence the necessity of elite patronage. Whether Maya kings or lords ever did anything like this is unknown, but the investment made by the Triple Alliance in the Basin of Mexico chinampa system seems similar to what Sneferu did.

I suggested many years ago that warfare enabled victorious Maya kings to reassign land in return for military support and thus subvert traditional social, economic, and political relationships (Webster 1975, 1977). Ethnohistoric documents make it clear that rulers rewarded successful warriors in Central Mexico and Oaxaca with rights to tribute and personal service during Postclassic times, and that tribute included land and/or its products and associated labor. Oudijk and Restall (2007: 55) say of Mesoamerica

generally that, “A typical aspect of conquest practice prior to the Spanish invasion was the division of land by a warlord, a religious leader, or a supreme ruler among his captains.” A hint of such practices among the Contact period Lowland Maya of northern Yucatan is that some chiefs and lords enjoyed privileged access to cacao plantations at some distance from their residences and had lands cultivated for them by commoners or slaves.²⁵² An estate system certainly operated briefly in northern Yucatan just after the conquest. Roys (1940: 10-13) cites the case of Juan de Contreras, who with his father was a distinguished supporter of the Montejos. In return for his services, he became the *encomendero* of several estates in various parts of Yucatan. Although he resided in Merida and later Valladolid, Juan de Contreras derived income from many towns and villages, some of which had as many as 250 inhabitants.²⁵³

Prior to the arrival of the Spaniards, a somewhat different (but often parallel) Mesoamerican tradition seems to have been more common than estates *per se*. Exactions were made from subordinated rulers, towns, neighborhoods, or other social groups. Flows of all kinds of things (including crops, prestige goods, household products, labor, and military service) linked hierarchical sets of givers and receivers. We usually call such exactions tribute or tax, but I prefer the more neutral term “entitlements.” Specific ties of givers (mainly farmers) to landscape segments in some ordered, cadastral sense were probably of secondary concern to elite proprietors, and entitlements could simply be reassigned as conditions dictated (see Oudijk and Restall 2007: 48 for a good Precolumbian example). Quezada (2014: 45) argues that the first generation of Spanish conquerors in Yucatan simply carried on this very old Precolumbian tradition, hence the importance of elite networks and jurisdictions. But, as we saw, he strongly inveighs against the idea that notions of property existed among the Contact period Maya lords and farmers or that elites divided land as described by Oudijk and Restall.

If elite estates of some kind existed among the Classic Maya, what might their economic and political roles have been? To be clear, I use the word “estate” here very flexibly. Estates might have been specific territories -- that is, well-defined tracts of land as we think of them from an Old World perspective and that are compatible with our concept of “landlords.” In such situations, landlords often had the right to alienate their property by sale, gift, or other means. Various highland Mexican codices provide hints of such formalized landholding in their preoccupation with detailed property descriptions (e.g., Williams and Harvey 1988). Unfortunately, they were painted well after the conquest and might not reliably reflect Precolumbian conventions (but see Lockhart [1992], who argues that they were used before the Spaniards arrived). Although kings and nobles in Central Mexico could alienate and re-assign land, it is not clear under what circumstances this was routinely done by non-elite individuals, groups, or institutions.²⁵⁴ Many Aztec or other Mesoamerican elite proprietors probably never visited their “estates” and only knew vaguely where they were. We would call such people today absentee landlords. “Ownership” aside, what mattered was predictable flow of whatever was demanded by or “owed” to receivers.²⁵⁵ I referred to this kind of buffering that might have provisioned Classic Maya elite households during times of local agrarian shortfalls in Appendix D.

Despite my early enthusiasm for warfare as a direct vehicle for land redistribution (it obviously worked well for the Contreras family), there were other means by which estates could have been founded and transferred in pre-Conquest times. Entitlements to agrarian resources might have been gifted away to attract allies and cement alliances, another practice known from highland Mexico. A second possibility is the use of dowries, as occurred among the New Guinea Chimbu and as commonplace in Europe. Dowry exchange is poorly documented for Mesoamerica, but we know it sometimes occurred. Oudijk (2002: 88) cites the case of the rich town of Cuilapan, just to the west of Monte Alban, which shortly before the Conquest was gifted away as a dowry by the Zapotec king. Lockhart (1992: 159-160 suggests that the presentation by a Texcocan ruler of *cihuatlalli* (“woman land”) to his daughter upon her marriage might be just such a dowry gift. Given the political significance of royal and elite bride exchange, such dowries (or their correlate bride-price) were probably more common than such scattered ethnohistoric

examples suggest. If so, they would have immensely complicated Late Classic Maya status rivalry and elite entitlements, especially if kings and lords were polygynous.

If the Classic Maya developed estates, by whatever means, they would have operated most effectively over short distances, consistent with what we know about many incidents of Maya warfare and most elite intermarriages. Presentation themes on polychrome vessels, commonly interpreted as commerce or tribute, might sometimes instead represent scheduled deliveries of cacao, cloth, and other items that were routine elite entitlements. Important as elite estates might have been, we will probably never have direct evidence of them barring some spectacular and unanticipated epigraphic revelations. I suspect that this is one of those features of ancient Maya culture, like the presence of slaves, which it is essential to know about but that will always remain unclear.

Agrarian agency

I have always distrusted the application of the agency concept by archaeologists, not because I discount the divergent interests and decision-making capacities of human individuals, including farmers, but for four other reasons. First, significant changes often occur that have nothing to do with the perceptions, intentions, or capabilities of agents. Second, in the absence of historical records, ancient agents are anonymous. We can discern only the most limited and superficial effects of individual actions, although in the case of the Classic Maya some royal and elite agency is evident. Third, we tend to associate agency with individual decisions, but humans usually make decisions as members of groups (and multiple ones at that), and both individuals and groups are heavily in thrall to larger customs, traditions, institutions, and historical contingencies.²⁵⁶ Fourth, we tend to invest ancient actors with motivations that reflect our own assumptions and sensibilities. I have read many accounts about how agents in various ancient hierarchical societies, including the Classic Maya, supposedly negotiated power, resisted oppression, or asserted identity. In fact, we have absolutely no idea whether Classic Maya commoners generally felt oppressed by Maya kings and nobles, whether they negotiated much of anything, including power relations and access to resources, or whether they unduly fretted about identity.²⁵⁷ It is currently fashionable (and academically politic) to imagine they did, but such presumptions reveal more about us than about the ancient Maya. Quite possibly, like ordinary folk in many complex societies, ancient Maya people habitually acquiesced (albeit not without anxiety and griping) to seemingly timeless, customary sociopolitical structures that were quite stable, or that appeared so in the short-term human perspective.²⁵⁸ Custom, as ethnographers know well, is a powerful force in traditional societies, and acceptance of the status quo is probably at least as commonplace as dissent, resistance, and negotiation. Nevertheless, people collectively face new conditions and must do new things. Archaeologists are reasonably well equipped to identify the large-scale processes of change and the collective actions and transformations that for good or bad result from them, but seldom the details.

Regardless of the posturing and rhetoric of kings, I think that Classic Maya agrarian agency was usually vested in individuals, nuclear and extended family households, lineages, or neighborhood communities as suggested by Watanabe (2004), except where elite estates were present.²⁵⁹ Such local control and decision making seems consistent with a long history of low-density development (see Appendix F). Kings and lords, in other words, traditionally indulged in very little hands-on agrarian management, and their concerns were extractive rather than directive.²⁶⁰ The important exception is that by the eighth and ninth centuries there was so much squabbling among farmers themselves over shrinking agrarian capital that kings and lords had to step in, probably ineffectively.²⁶¹ Abrams (1995) made this suggestion long ago. Whether management was a top-down process initiated by elites, or a bottom-up one initiated by clients seeking elite patronage and protection is unknown, but such stresses contributed to the political collapse of the Tikal and other kingdoms. See Webster (2005) and Webster and Murtha (2015) for more detailed discussion of these complex issues.

I have emphasized entitlements as they relate to individuals or groups such as extended families or lineages. Some pre-modern complex societies had various kinds of institutional ownership or entitlements. In many parts of the Old World temples, monasteries, guilds, or other non-kinship institutions held rights to land and other resources. We know nothing about such arrangements, if any, for the Late Classic Maya, although if noble “houses” had estates as I have suggested above, these might fit the pattern.

Before leaving the topics of ownership, tenure and entitlements it is worth noting the corpus of ancient Mesopotamian documents that provides insights not only about various forms of land-holding, but also legal transfers of land (Renger 2009). While there are many difficulties in interpreting just what these documents mean, Renger’s basic position echoes my own. Strip out animal husbandry from the following quote and it applies equally well to the Maya:

“Mesopotamian society and its economy are based on agriculture with integrated animal husbandry. The manufacture of crafted goods and their distribution and allocation play only a subsidiary role. This is the prevailing paradigm for the entire history of ancient Mesopotamia. Since arable land is the most decisive productive factor in a society based on agriculture, the form of control of the arable land, i.e., the system or regime of land tenure, is of utmost importance with regard to the social fabric of such society. Specific land tenure regimes not only are determined by ecological or natural factors, but also are profoundly shaped by social forces. Social and ecological factors are interdependent and interacting” (Renger 2009:1).²⁶²

APPENDIX F

Classic Maya Political Organization and Institutions

Eric Wolf, Milton Altschuler, and other anthropologists of the 1950s thought that low-density populations supported by shifting cultivation were resistant to political centralization and centralized agrarian management.²⁶³ In currently fashionable terms, agrarian agency resided heavily at the household level (usually conceived as nuclear or extended families, or small local lineages), as it does in many ethnographically known cases. On the one hand, this view presented an evolutionary paradox because even though we could not then understand Classic inscriptions, the Maya at Tikal and elsewhere clearly had major central places, some sort of central leadership, mobilized lots of labor, and erected impressive buildings. On the other hand, it provided support for the widely accepted “priest-peasant” conception of Maya society. Assumptions of Classic Maya exceptionalism associated with the then prevalent “Maya Mystique” negated the necessity to see broad, cross-cultural regularities in Maya cultural evolution or to explain their absence (see Becker 1979 and Webster 2006 for discussions). All this began to break down with Proskouriakoff’s (1960) epigraphic research.²⁶⁴ By the mid-1960’s archaeologists were beginning to internalize “... accumulating evidence for royal lineages ... that hardly squares with our image of the rustic priest-leader” (Willey *et al.* 1965: 580).

V. Gordon Childe’s (1950) comparative analysis of urban civilizations famously conflated Great Tradition elements with organizational and settlement features. He used the term “state” only three times to point out that considerable complexity and cooperation could be maintained *without* state-type organization, and as a gloss for vaguely-defined institutions centered on temples or palaces that bolstered organic solidarity. Just how to characterize Classic Maya urbanism and organization clearly puzzled Childe. He included the Maya among his four comparative cases because of their monumental architecture, art, writing and calendars, and because his analysis of the independent evolution of cities required a New World example. One gets the impression, though, that he would have preferred to exclude the Maya altogether from his discussion (why he ignored Teotihuacan and Tenochtitlan is puzzling). Only during the following burst of neo-evolutionism led by Julian Steward, Elman Service, Morton Fried, Marshall Sahlins and others did the linkage of well-defined state institutions and civilization become conventional wisdom.²⁶⁵ Most conceptions of Maya political institutions focus on Late Classic developments although there is a recent burst of enthusiasm for Preclassic state origins. Mayanists agree that Late Classic kingdoms varied considerably in organization depending on scale, location, landscape features, and historical contingencies. They pay less attention to the obvious dynamism of sociopolitical institutions and behaviors as they changed through time, as I have suggested above with regard to niche inheritance. Particularly important is the interval of peak population, which in most southern kingdoms is the eighth through the early ninth centuries when Classic civilization was most mature.²⁶⁶ If ever we could expect selective pressures for new mechanisms of administration and management, it is then. That the biggest architectural efforts at Tikal, including the “Great Temples”, occurred after about AD 740, when labor was in greatest abundance and elite management, such as it was, became most necessary, is probably not coincidental.

Monumental architecture, sophisticated art, architecture, calendars and writing left no doubt for Childe or anyone else that the Classic Maya deserved the label “civilization.” Much debated, in contrast, were (and are) the socio-political underpinnings of Late Classic Maya society.²⁶⁷ By the late 1960s and early 1970s, our still-feeble understanding of inscriptions, along with early settlement research, convinced many archaeologists that the Maya had state-like institutions. William Sanders, for example, thought that by the Late Classic “... a truly bureaucratic, state-like structure evolved...” albeit one that still accommodated powerful unilineal descent groups.²⁶⁸ We saw above one prop to this presumption of state-type organization (although Sanders did not share it) - the indirect proxy

of high population densities. Another was the increasing evidence that the Maya practiced various forms of intensive agriculture, including landesque investments in terracing and drained fields. Such innovations provided a way around Wolf's and Altschuler's objections because such agrarian features plausibly required some forms of royal/elite management and its associated institutions of decision-making – call this bureaucracy. About this time Kent Flannery (1972: 400), dissatisfied with the vagueness of the term civilization, shifted attention to core organizational principles. He defined civilizations as “... that complex of cultural phenomena which tends to occur with the particular form of socio-political organization known as the state.” Notice that Flannery reserved himself a bit of wiggle-room here (“tends to”), but he went on to emphasize decision-making and information flow. Far from being epiphenomena, art, religion, ideology, style, and other basic accouterments of civilizations in Flannery's scheme all have causal significance in the realm of ecosystem information flow.²⁶⁹ Nevertheless, the concept of “state” supposedly lends itself better to cross-cultural comparison and regularities than the qualitative elements of “civilization.” John Baines and Norman Yoffee (1998: 199) later made a similar distinction: states are “... the specialized political system of the larger cultural entities that we denominate “civilizations.” Mayanists often waffle on the issue of Maya statehood, while retaining the concept of civilization. Estrada-Belli (2011: 45-55) seems to think that Maya states emerged as early as Middle Preclassic times, but later (p. 140) says that states are “idealized social types” that are not only difficult to detect, but which we can variably apply to the Maya as we choose. He seems to use the state label more confidently in his 2016 chapter on early Maya political evolution.

Like Childe before him, Flannery (1972: 412) identified a related problem: “In the case of some ancient civilizations, such as the Classic Maya, such (state) superstructure was supported in spite of agricultural practices believed to be no more sophisticated (except in rare cases) than those of the most egalitarian tribes...” (my interpolation of “state”).²⁷⁰ Flannery knew that people like the New Guinea Chimbu had high population densities, some kinds of intensive agriculture, and complex forms of land holding. He championed the emergence of complexity among the Maya and other ancient complex societies as being “... in the realm of decision-making and its hierarchical organization, rather than in matter and energy exchanges”, a stance he took in opposition to the “cultural ecologists” of his day. According to Flannery, complex Maya state-type political superstructure was only loosely, or not at all, linked to its agrarian underpinnings. This left unanswered the questions of what such a superstructure administered or managed, or what processes had selected for its emergence in the first place.

Most discussions of Maya political organization focus on the period after about AD 650 when inscriptions and dates become abundant. Easy to forget is that Maya political organization, institutions, and ideologies at Tikal and elsewhere evolved under regimes of low population density and generally “un-intensive” agriculture for more than a millennium *before* the Late Classic (always allowing for regional exceptions where high densities did occur). This insight, based on much better settlement data than available to Flannery in 1972, seems to reinforce his perceived disconnect. I draw a different and more radical conclusion. My suspicion is that even the Late Classic Maya were not very state-like as envisioned by Flannery and most other scholars, but rather highly traditional and court-centered without well-established bureaucracies or managerial hierarchies.²⁷¹ Put another way, the disconnect lies in Flannery's assertion that that the admittedly vague attributes of “civilization” must signal the existence of the state (itself a rather loose and variable concept). Flannery (1972: 403-404) provides a standard list of the attributes of states – one that has been pretty much preserved in the literature down to the present (e.g., see Fukuyama 2011: 80-81) that I won't repeat here. I've long suspected that Classic Maya kingdoms – even big ones like Tikal – conformed poorly with this political model, or that they were really urban.²⁷² I think it entirely possible that the Maya fully lived up to the comparative term civilization, in the sense of “Great Tradition” or “high culture” as associated with elites, without

achieving “statehood” or urbanism as we normally understand these labels (see Redfield 1960; Baines and Yoffee 1998: 235).²⁷³ This is not a new proposition. It is essentially the one, minus the theocratic trappings, held by Morley, Sanders and most other Mayanists before 1960.²⁷⁴ Willey, remember, speculated that the Maya collapse might have been caused in part by the failure to achieve true urbanism. I would rephrase this to say that the Maya did not achieve the institutional complexity usually associated with ancient, urban-centered societies. And of course rulership and management are closely associated with demographic scale. Ruling or managing a population of 100,000 people is very different from ruling or managing one of 10,000. Even so, Baines and Yoffee (1998: 232) estimate that only about 500 high officials administered all of Old Kingdom Egypt.²⁷⁵

My doubts about Classic “statehood” derive partly from my understanding of political arrangements among the Contact period Maya in the early sixteenth century (Roys 1965, Farriss 1984, Restall 1997, Quezada 2014). Sometimes labels like “class”, or even in Roys’s case “caste”, are used to describe hierarchy in these generally small polities ruled by *batabob* or sometimes by *halach winikob*. I prefer to call them minimally stratified polities in the sense that Morton Fried (1967) used the term: some people had privileged access to essential resources through various kinds of payments in the form of taxes, clientage, rents, etc., while others had impeded access.²⁷⁶ Although there seem to have been well-established categories of nobles and commoners, there were gradients of achievement, wealth, prestige, kin affiliation and personal influence that crosscut such boundaries. Lineage membership was important on all levels of society, and people of various statuses made political decisions through intra-polity negotiations. An important point is that stratification and lineage structure co-existed. Fried envisioned stratified societies as fragile because they lack mature state-type political institutions that reinforce stratification, and Maya political volatility is notorious, especially in northern Yucatan (Marcus 1993, Quezada 2014).²⁷⁷

Contact period Maya polities, unfortunately, provide questionable direct analogies for understanding Classic Maya ones, partly because of their generally small demographic scale, but also because they lacked powerful and deeply entrenched royal dynasties (but see Prudence Rice 2004 for a spirited advocacy of strong political continuities with the earlier Classic and Preclassic Maya). Nevertheless, no Mayanist today doubts that Classic polities were stratified, although there is much disagreement about dimensions of centralization as opposed to decentralization or heterarchy (see various chapters in Marken and Fitzsimmons 2015). Tikal lacks the necessary wealth of archaeological and epigraphic data to analyze this issue. Copan provides much better prospects. Long ago, Abrams (1994) showed through analysis of architectural energetics that Copan residences exhibit a continuum of construction effort and quality. Some 15% of all residences - those of royalty and sub-royal elites - were impressively “improved.” He concluded that such architectural variation was consistent with a maximal lineage system that supported considerable stratification and multiple foci of power and authority. On the other hand, Katherine Miller’s (2015) biogenetic study of several hundred Copan burials leads her to reject both lineage and house models for Late Classic Copan.

David Reed and Scott Zeleznik analyzed 736 Copan burials (royal ones excluded) using 51 variables. They found that “The most striking feature of the mortuary data analysis is the continuous nature of the distribution and the great range of mortuary programs across space and site type in the valley” (Reed and Zeleznik 2016: 194). Such a pattern, they think, offers little support for a class model.²⁷⁸ Instead the data “... suggest to us a society moving out of a ranked form of organization into a stratified society with upper- level elites attempting to gain control of economic capital and develop a class-based hierarchy through the accrual of symbolic capital” (Reed and Zeleznik 2106: 200). All this is consistent with Abrams’s (1994) construction data, with Freter’s (2004) analysis of the adaptive flexibility and local economic specialization reflected in our Copan settlement data, and with Miller’s (2015) detection of multiple origins and forms of household filiation. Counter-intuitively, isotopic studies show that

the considerable variability in artifact and mortuary behavior among elite residences is not strongly reflected in diet.

Revealing as all this might be for Copan, located on the southeastern margin of the Lowlands, we cannot safely extrapolate from it to much larger and older kingdoms such as Tikal, where political evolution might have gone farther and where populations might not have had similarly diverse origins. Rulers at Tikal, Calakmul and other polities might, for example, might have effectively eliminated commoner lineage organization, as occurred in Hawai'i, and thus edged closer to class structure. Haviland thinks that lineage organization had largely disappeared at Tikal by Late Classic times (personal communication 2016), and along with Moholy-Nagy (1992) argued strongly that well-developed class structure was present at least by Late Classic times. If so, Tikal probably was more centralized and less heterarchical than Copan, and was a central player in a great dynastic struggle with Calakmul.²⁷⁹

Hegemonies

Ever since Martin and Grube (1995) identified Maya "superstates", we have known that networks of interaction dominated by overkings operated beyond the level of local politics.²⁸⁰ Whether we call them superstates, superpowers, or hegemonies (the best term), these networks add considerable complexity to analyses of Maya political organization. We now know a lot more about them, though much is still murky, most importantly their demographic contexts.²⁸¹ Current hegemonic reconstructions feature Tikal as the central player in the early expansion of a Tikal/Teotihuacan political order after the dramatic events of AD 378.²⁸² Beginning in the mid-sixth century the Snake or Kaanul dynasty kings at Dzibanche (perhaps originally at El Mirador – see Hansen 2016: 406) and after AD 635 at Calakmul, oversaw a similar process of expansion through war, diplomacy and marriage alliance, probably in reaction to Tikal's efforts. Calakmul and its allies such as Waka (El Peru), Caracol, Naranjo, El Zotz, and possibly Holmul, tightened their hold on the landscape around Tikal, eventually defeating their great enemy in AD 562, although they had to subdue Tikal again on several occasions over the next century. These military reverses, as we have seen, appear to have had little effect on Tikal's population, which began its surge about this time. The Kaanul kings, along with their proxies and allies, remained dominant during much of the sixth and seventh centuries until a resurgent Tikal in turn defeated Calakmul in AD 695, and again in AD 731. Once- subordinated kingdoms such as Naranjo, which retained vestigial loyalty to its old Snake ally until at least AD 711, thereafter asserted their own regional independence and power. All this means we can expect new kinds of political roles and offices, as hinted at by impressive titles that nevertheless lack the "divine king" appellation used by most local rulers (see for example the case of the Holmul kings [Estrada-Belli and Tokovinine 2016: 164]). Some of my colleagues now even refer to a Kaanul empire, which almost by definition has the implication of contending, dominated, and subordinated states, rather than other kinds of political entities. Archaeologists debate what caused such episodes of expansion. Martin and Grube (2008: 20-21) think motivations were non-territorial, aimed instead at creating new relationships among powerful elites:

"Political expansion, where it occurred, was not an acquisition of territory per se, but rather an extension of these elite networks. The most powerful dynasties brought rival 'holy lords' under their domination, with ties often reaching far beyond their immediate region."

This sounds like Quezada's reconstruction of Contact-period elite jurisdictional maneuvering cited above. But unlike Quezada, Martin and Grube insist that expansion had fundamental economic dimensions, namely the flow of goods and services in the form of tribute along these elite networks. It seems to me that the estates and entitlements discussed above are easily accommodated to this system, thus adding a territorial or 'property' dimension to it.

Some of the evolutionary labels we commonly use, including stratification (but not “state”), derive heavily from Polynesian ethnography. Many years ago I noted the confusion, or at least variety, of ways in which these labels were applied to Polynesian societies themselves. I concluded that “Such confusion of terminology itself is revealing, since it signals the existence of complex sociopolitical forms for which our comparative or evolutionary terminology is inadequate” when dealing with the Classic Maya (Webster 1998: 312; see also Webster 1992: 154-156). Simon Martin (2005; 2016), among others, has similarly speculated that Classic Maya notions of royal identity and territory might be very different from those derived from our standard Old World models, especially because of the kind of hegemonies described above.²⁸³ Reed’s and Zeleznik’s conclusion that the Late Classic Copan kingdom was in considerable social and political flux even during its heyday reinforces Martin’s perspective. John Watanabe (2004) discusses our tendency to reify, simplify and overextend elements of social organization such as “lineage” and “house” and conflate them with societies as a whole (see also Miller 2015). While we conventionally refer to class societies, rank societies, stratified societies, etc., we should rather say societies with classes, with ranking or with stratification, thus recognizing the other institutions, associations, values, and behaviors that make up whole social fabrics. Especially important is that all such hierarchies are potentially very unstable (see Clunas 2013 for pertinent examples from China).

Bureaucracies

Questioning Classic Maya “stateness” will raise the hackles of many colleagues, especially those who envision Maya states as early as Middle Preclassic times (see Traxler and Sharer 2016; Estrada-Belli 2011, 2016). I could defend my skepticism by more discussion of population scale, class structure, territoriality, monopolization of force, codified law, etc. Here I will only comment on bureaucracy, a set of institutions closely related to Flannery’s concerns of information management, flow, and decision-making. Much sociological interest has focused on bureaucracies in recent nation-states, but ancient bureaucracies also existed, as in late third millennium Mesopotamia (Gibson and Biggs 1987; Hunt 1987) and Archaic and Old Kingdom Egypt (Lephron 1995). Some of the earliest bureaucracies we know about developed to service the needs of the first cities (Hunt 1987).²⁸⁴ Early documents such as the Sumerian “Titles and Professions List” provide insights into how they were structured, although little about what institutions they served (Algaze 2013: 80). Regrettably, we lack comparable documents for the Maya.²⁸⁵ Winterhalder and Puleston (nd) nevertheless suggest a model of how bureaucratic decision-making might relate to revenue generation in ancient complex societies, including the Classic Maya.²⁸⁶ Anthropologists almost reflexively associate bureaucracy with concepts of urbanism and statehood (although Childe did not use the term in his 1950 paper). Willey and Hammond seemed to take this position (see Endnote 75), as did Sanders. Most recently Moholy-Nagy (2016: 264) speculates that at Tikal there was “... an elite class of knowledgeable bureaucrats who managed the essential functions of the state...”²⁸⁷

I understand well developed bureaucracies to be ranked hierarchies of trained, specialized officials who are often literate, recruited by the polity (not necessarily state-like), ruler, or other authorities on the basis of aptitude rather than, or at least in addition to, birth, and who discharge specific duties according to well-established procedures and rules. Just such administration operated in China since late Tang and Sung times, but there are other kinds of ancient bureaucracies as well.²⁸⁸ Early Dynastic Egypt had elaborate hierarchies of offices and title-holders, but tended to parcel out major administrators situationally among them (Wilkinson 2001: 145). Thus the same official might oversee a royal construction project, then lead a mining or military expedition, and later serve at the king’s court, wearing a different administrative hat and title each time. Sometimes what appear to be administrative hierarchies are corrupted by other means of recruitment. During the seventh century, the Japanese imported wholesale a set of bureaucratic offices typical of the much more mature Tang Chinese state, but this complex system never managed to suppress the power of the great families, who leveraged candidates into offices through marriage, influence, and greed. Much of the central administration thus consisted of sinecures and titles with no real functions,

held by people because of pedigree, marriage alliance, and political clout (a good discussion is found in *A History of Japan until 1334* by Sir George Sansom).

Whatever the specific bureaucratic syndrome, officials implement and sometimes formulate decisions and policies and also gather, assess, and manipulate the information necessary for these purposes. In return for their services they are rewarded in accordance with their rank, skills, and performance. In short, mature bureaucracies are institutions that, at least ideally, impersonally serve the interests of the central government (which in some cases is the ruler or the dynasty) -- interests that might or might not coincide with their own or those of the wider society. Wherever polities are strongly segmented and court-centered, there might be multiple bureaucracies, some focused on religion or the military as well as civil government, or on the establishments of great lords. This rose-tinted view of system-serving administrators ignores the tendency for bureaucrats and their institutions to become highly self-serving and even sometimes dominate their royal masters, but let that pass. William Sanders and I (1988) proposed that Maya polities were essentially court-centered and administered as extensions of royal households. Today I would slightly amend that statement to say royal *and* elite households, given the evidence for various powerful noble lords at centers such as Copan and Palenque. The crucial questions are what was administered, by whom, and by what means? Only seven things seem obvious to me: flow of food revenue for provisioning and maintenance of royal/elite households and enterprises (as opposed to general food production or risk management), construction of buildings and monuments, inter-polity relations, rituals, acquisition and distribution of precious things, warfare, and (possibly) management of elite estates, which might include non-agricultural resources such as forests, as Lentz *et al.* (2014, 2015) argue. Markets are another possibility. Haviland believes that there was a large Tikal central market (at locale TR 16) and that its proximity to the Central Acropolis indicates royal management (personal communication 2016). Turner and Sabloff (2012: 4) speculate that Classic elites managed commerce, and that depletion of their “financial coffers” might have been one factor in the collapse. This seems as unlikely to me as the anachronistic phrasing.

Some archaeologists think kings managed water in ways that provided political leverage, and hydraulic features are well-documented in Central Tikal (e.g., Scarborough *et al.* 2012; Lentz *et al.* 2014; Scarborough and Grazioso 2015). In other regions such as northern Belize, the northeastern Peten, the Pasion/Usumacinta drainage and the Copan valley, there were large, permanent lakes and rivers, so availability of water was widespread. I think that control of central water resources even at Tikal provided kings with minimal socio-political leverage, although it had limited irrigation potential near the site core. Long ago, Edward Higbee noted that “The landscape of southern Yucatan, particularly the area about Tikal, has a feature that made it especially attractive to primitive man. A profusion of little ponds, or *aguadas*, dot the countryside. They are seldom more than a few kilometers apart, and most of the soils between them are well-drained. This splendid mosaic of *aguadas* and arable land made the region suitable for concentrated settlement... To recognize the significance of *aguada* distribution around Tikal is to appreciate why it was so desirable a site for the development of the first great city of the Old Mayan Empire. A broad hinterland could be occupied!” (Higbee 1948: 459).

Puleston (1973a) also observed this profusion of water-sources (Fig. 22) during his surveys. Their abundance is one reason why I doubt that management of dry-season drinking water gave rulers much political leverage. Water control would be a puny political lever elsewhere too, as at Palenque, Copan, anywhere along the Usumacinta, or at Tikal’s lakeside neighbor Yaxha. Higbee of course has causation a bit backwards here. Most of the *aguadas* are probably not natural features that attracted settlers as he implies, but rather artificial constructions, or at least remodeled natural basins. Nevertheless, they demonstrate that small outlying social groups or households could create reservoirs that served them during the dry season.²⁸⁹ Recently Adrian Chase (2016) reported some 1590 small household reservoirs at Caracol, Belize, and concluded that they provided little scope for elite managerial leverage.



Fig. 22. One of the many small aguadas outside the central precincts of Tikal. Photo by Tim Murtha.

Above, I raised the issue of elite estates, especially in the context of spatially distant entitlements. These of course had somehow to be managed. Another pattern might have existed around Copan, which is more like the riverine zones in Belize than the interior Peten. Central to the agrarian ecology of the valley are large tracts of active or old alluvium, along with adjacent low-slope foothills. These highly productive (often irrigated) and circumscribed agrarian resources are today heavily in the hands of a few rich families. If a similar pattern existed at Late Classic Copan then there were *local* elite estates. Whether estates were distant or local ones, they and the labor attached to them would have required elite oversight in the form of some kinds of managers (this was the pattern in Hawai'i). At Tikal, there is less stark differentiation between the highest-quality lands and those of lesser quality.

Whatever the range of elite managerial roles, I think local Maya polities generated very little administrative demand, and consequently had very few hands-on administrators, resulting in "flat" hierarchies focused on the ruler (see Hodge 1996: 20 for discussion). Inscriptions reveal a substantial number of Maya titles apart from royal ones, many of which might be just honorifics. Sarah Jackson (2015) has singled out five titles that appear to identify offices attached to royal courts. These titles seem to be administratively formal, in the sense that individuals adopted and displayed them during rites of passage parallel to those of rulers. Almost all of these titles emerged after AD 600 during the interval of struggling hegemonies, and their distribution is spotty in the inscriptions, with only Palenque manifesting all five (and Jackson notes that many sites list no titles at all). Although we don't know all the roles that went with these offices, Jackson sees in their widespread occurrence a shared tradition of governance that nevertheless could be adapted to specific circumstances. All this she calls "mild bureaucracy", a cautious term that seems suited to the small demographic scale I champion in this paper. Many years ago, Tainter (1988) linked collapses with "diminishing marginal returns" of increasing complexity. I think that over the long run of Maya political evolution growth in complexity (and hence diminishing returns) was much less of a problem than growth in demographic scale.

Scale apart, our conceptions of administration and management depend on whether we envision the royal/elite component of Maya polities as composed of large, corporate kin-based segments with their own internal ranking, as Sanders believed, or as more centralized and stratified. I strongly incline toward the former view, which seems better to explain the elite Copan settlement, mortuary, and epigraphic data.²⁹⁰ At Copan, there would have been multiple administrative nodes, each similar to the others, with that of the ruler pre-eminent but not necessarily much more complicated. Grandees who dominated

great families were probably both courtiers and potential competitors of the king. I admit, though, to being spoiled by the abundance of evidence available for Copan, which in many respects might have been less centralized than older and larger kingdoms such as Tikal.

Wolf's and Altschuler's assertion that low-density swidden populations 'resisted' political centralization can be restated to say that agrarian agency in such populations was primarily vested in individuals, households, neighborhood groups, or perhaps patrilineages that needed little or no top-down management of decision-making, labor organization, or labor expenditure (Childe and Michael Coe would say that they had mainly mechanical solidarity). By the 1970s, evidence for more intensive forms of agriculture stimulated the corollary belief that the Classic Maya stewardship of resources enabled them to sustain very large populations and to respond to risks with great resiliency. Such a view suggested more collective and intricate processes of decision-making that operated on the group level at the same time as the linkage of civilization and state became evolutionary wisdom. It was easy to suppose that these processes selected for institutions of elite management and for something like bureaucracy, although often conceived as more formal than implied by Jackson's analysis.²⁹¹ The dramatic political and demographic crumble that began in the eighth century belies assumptions of Late Classic resiliency and some modicum of effective agrarian management. For some archaeologists drought explanations are attractive because they allow retention of Maya exceptionalist perspectives. The crumble occurred *despite* the agrarian cleverness of the Maya, and was caused by extraneous forces beyond their control.²⁹²

I personally cannot imagine that well-developed bureaucracies operated among the Classic Maya, partly because of the small scale of local polities and because I think that Maya cultural evolution up until the seventh century in most places was not subject to strong selection pressures for specialized decision-making or for state-type institutions.²⁹³ Suspiciously lacking, so far as we can tell, are the record-keeping features (writing, *kipus*, etc.) associated with other ancient bureaucracies. I suspect that the royal courts of the Classic Maya were similar in character, and usually in scale, to those described for Hawai'i (Moore 2015: 19-21). The small demographic size of most Maya polities even in Late Classic times, and certainly earlier, was consistent with localized kin or client-based principles of internal organization rather than well-developed class structure.²⁹⁴ When the crunch came, between roughly AD 650 and 850, the old hidebound Maya systems of governance, heavily court-centered and with a millennium of political inertia, lacked the flexibility to adapt to and manage a host of unprecedented problems, especially those associated with increasingly dense populations on anthropogenically-altered landscapes (see Abrams 1995, Webster 2005, Webster and Murtha 2015). Where we have good settlement data, as at Copan, many local households and communities seem to have had the flexibility to survive this political collapse for some time (Freter 2004).

APPENDIX G

Malthus, Boserup, and the Maya

“The question of the effect of demographic growth on the economic development of agricultural societies remains open and unresolved. It is a question over which two hardened points of view oppose one another. The first sees demographic growth as an essentially negative force which strains the relationship between fixed or limited resources ... According to the second, demographic growth instead stimulates human ingenuity so as to cancel and reverse the disadvantages imposed by limited resources” (Livi-Bacchi 1997: 80).²⁹⁵

A year before Puleston completed his dissertation on Tikal settlement, there appeared a major overview volume that addressed Livi-Bacchi's dichotomy (Spooner 1972). It reflected increased interest in both demography and agricultural intensification, heavily stimulated by Ester Boserup's (1965) *Conditions of Agricultural Growth*.²⁹⁶ Among the many distinguished contributors to this volume was William Sanders, whose summary statement on Mesoamerican and Maya demographic history was just a bit too early to take full account of the Tikal Sustaining Area data, although he still center-pieced Tikal in important ways. Several chapters contrasted Boserup's ideas with the much older ones of Thomas Malthus. Boserup's claim that she had inverted Malthus by making population growth the independent, rather than dependent variable in processes of agricultural intensification was widely accepted. All this conveniently coincided with documentation of various kinds of intensification on ancient Maya landscapes that helped explain how dense Late Classic Maya populations were supported and that called into question the standard maize-swidden model of Maya subsistence. Turner's (1974) influential *Science* article on intensive Maya agriculture is a good example of Boserup's influence. Correlates of intensification, such as competition over land and new patterns of land ownership, also promised answers to the question of what had selected for centralized Maya political systems, and (maybe) state-type organization (see Dumond 1972). Somewhat later overviews such as Harrison's and Turner's *Pre-Hispanic Maya Agriculture* (1978) summarized the new data and pronounced the death of the old milpa hypothesis.²⁹⁷ Applications of the Boserup model have mainly focused on the rise of Maya civilization and the innovations that characterize its agrarian success, its resilience, and its purported capacity to support very high population densities.²⁹⁸ The Malthus model (or neo-Malthusian model as Boserup calls it) is mainly linked with the pressures and stresses associated with environmental degradation, decline, and collapse.

Like Sanders, Boserup published her ideas before the results of the Sustaining Area Project were known, but even in 1965 archaeologists working at Uaxactun, Dzibilchaltun, Belize and elsewhere had identified the Late Classic as an interval of unprecedented population size, posing a problem for the old milpa perspective. Her assertion that population growth might have positive agrarian outcomes attracted those who saw the Maya as resilient and benign managers of their agrarian resources. Still, the embarrassing specter of the great collapse, the most dramatic episode in Maya demographic history, was at odds with this view.²⁹⁹ Long before Boserup, many Mayanists, especially when analyzing the collapse, aligned themselves with general Malthusian perspectives even though they might not explicitly identify with Malthus (see for example Morley 1946: 71-72). By general Malthusian perspectives, I mean the idea that rapid Late Classic demographic increases and consequent imbalances between population and resources contributed heavily to the decline that began in the eighth century.³⁰⁰ I am something of an exception because I have explicitly associated my interpretations for a long time with Malthus's ideas (Webster 1985, 1998b, 2005, 2014), and another recent example is Hogarth *et al.* (2017).³⁰¹ I have tried to do this in a nuanced manner that does not lapse into a vulgar determinism that simply equates the fortunes of the Maya with overpopulation, subsistence failures, and their consequences. My core arguments in this paper are, after all, that there were not nearly so many Maya as many scholars imagine and that

the demographic consequences of the collapse were not as abrupt and dramatic as often thought. My own position (see Webster 2002) is that many internal problems overtaxed the capacities of Late Classic kingdoms, and that these included subsistence shortfalls and ingrained political incapacities. A complex mix of stresses set in long before Maya populations approached strict carrying capacity limits, and undermined the general capacity of their fragile sociopolitical systems.

Both Malthus and Boserup present population pressure models in which food supply is the limiting resource.³⁰² In the Malthusian model, the innovations or conditions that permit growth in food supply are often said to be extraneous and not directly linked to population pressure. Boserup instead asserted that population growth and pressure (or at least the perception of limits) might produce innovations that stimulate intensification, food production, employment, and economic development, and so raise carrying capacity and living standards.³⁰³ Boserup's basic idea is actually an old one known to Malthus (see Malthus 1992: 199). Livi-Bacchi (1997: 91) points out that economists of the eighteenth century fretted that low or declining populations inhibited development. Malthus certainly recognized that population pressure and the consequent availability of labor (among other things) encouraged farmers to carry out various kinds of intensification, thus leveraging themselves, for a time, into conditions of greater well-being (Malthus 1992: 25-26). Examples were plentiful in his day, notably in the Low Countries of Europe and in parts of England itself, where rich agricultural landscapes were created through human ingenuity and effort.³⁰⁴ He thought, however, that population growth would soon undermine such improvements, resulting in short-term oscillations of hardship and well-being.³⁰⁵ Because application of Boserup's ideas to the Maya is well known and abundantly referenced above, I will mainly comment on Malthus. Both, as we shall see, would be puzzled by the historical demographic patterns we detect in the central and southern Lowlands.

Malthus made a basic assumption that is widely echoed in the modern literature on ancient complex societies:

"It must ever be true that the surplus produce of the cultivators, taken in its most enlarged sense, measures and limits the growth of that part of the society that is not employed upon the land. Throughout the whole world the number of manufacturers, of merchants, of proprietors and of persons engaged in the various civil and military professions, must be exactly proportioned to this surplus produce, and cannot in the nature of things increase beyond it" (Malthus 1992:131).

When he wrote this passage, most people everywhere still labored on the land. In Malthus's time, and long before, densely settled agrarian landscapes were seen, especially from the perspective of rulers and elites, to indicate prosperity in many parts of the world. So too did population growth. In Europe, "A numerous and increasing population was the most important *symptom* of wealth; it was wealth itself – the greatest asset for any nation to have" (quoted in Livi-Bacchi 1997: 92). The contemporary Ming Chinese, at the other extreme of the Eurasian continent, shared this view: "Increase meant prosperity, prosperity pointed to good government, and this is how a dynasty liked to be known." As one Ming administrative axiom said, "... an increase or a decrease in population attests to the strengths and weaknesses of conditions in the realm" (Brook: 2010: 44). In one sense this is obviously correct – in a traditional agrarian state productivity must increase to feed additional mouths, and to rulers more people meant more wealth in the form of taxpayers, laborers, and military resources. Whether the actual well-being of farmers also increased is another matter, and was one of the things that Malthus questioned.³⁰⁶

Malthus's "dismal" theory has been widely excoriated, misrepresented and refuted over the last 200 years, often by those who have a cartoon-like perception of what he said. I have met archaeologists, anthropologists and not a few economists who disparage his ideas despite the fact that they have never

read Malthus, and who paradoxically sometimes make arguments with which he would have agreed.³⁰⁷ Malthus, like Darwin a bit later, had a big idea. He called his principle of population a *natural law* (though unlike some Enlightenment thinkers he thought such laws were God-given). And with regard to “dismal”, it is true that much of what Malthus said was distinctly shocking to many of his contemporaries and still shocks us today.³⁰⁸ The same, of course, is true of Darwin’s big idea and Galileo’s. All these men followed their ideas and supporting evidence to their logical conclusions, however unpalatable.³⁰⁹

Central to many criticisms is that Malthus’s dire predictions have not been borne out in the intervening two centuries, and that he was an apologist for and defender of the inequities that attended the first burst of robust industrial capitalism in Western Europe. My intention here is not to comment on these critiques, except to say that if Malthus could be magically resurrected, he would not be surprised by what he saw of the basic human condition (his main preoccupation was, after all, economic inequality), nor convinced that he got everything wrong. Imagine his reaction to this recent lament by a Greek father:³¹⁰

*“What concerns me most as I have five children is their future
and the high levels of youth unemployment we are facing. Three of
them are studying, and I fear they will have no prospects.”*

I imagine instead how Malthus would react if he were exposed to what we know about Maya cultural and demographic history. Although Malthus promulgated his principle of population as a natural law that operated at all times for human societies, he was quintessentially a thinker of the late eighteenth and early nineteenth centuries. We must remember the specific societal contexts to which he mainly applied his theory, many of which differ radically from what we know about the Maya.³¹¹

Malthus’s worldview was blinkered by a kind of symmetrical ignorance. He could not foresee the future, with all of its geographical expansion, urbanization, complex machinery, chemical fertilizers, new crops, cheap energy sources, efficient transport and communication, advances in health care and sanitation, and especially effective contraception (the latter, not available in his day, would have dismayed him as vice, as would abortion and infanticide). His famous assertion that rates of human reproduction had the capacity to outpace rates of food production was contradicted by the agrarian revolutions of the next two centuries. That only a tiny fraction of the total population of developed countries today works to produce food on the land would have been an inversion of the agrarian equation he took for granted. He certainly would have been astounded that reproductive restraint today operates most powerfully in wealthy, not poor nations, and that completed fertility rates are below replacement levels in some developed parts of the world. The other dimension of his ignorance was the past.³¹² Malthus knew a little about “primitive” people such as eastern North American Indians, but he (like Marx a bit later) was no comparative ethnographer.³¹³ He referenced other civilizations such as Egypt, Rome, China, and Japan (and very occasionally Native Americans, Pacific islanders, etc.), but his main concerns and expertise were focused on European societies of his time and their offshoots such as the nascent United States. The major entities were nation-states (or in some instances smaller principalities or colonies) that had long traditions of centralized governance and political institutions, along with well-developed bureaucracies. As a mathematician turned political economist (or, as some have called him, a political moralist), he asserted that economic equality and well-being, in the absence of widespread moral restraint, were impossible because social and economic engineering, no matter how well intentioned, would always be subverted by the capacity of human fecundity to outpace increases in food production. Malthus’s conclusions were a direct challenge to the burgeoning philosophical progressivism of his day (Tainter 2016: 34-35) and anathema to many contemporary *philosophes* and the somewhat later Marxists.³¹⁴ They also challenge our own widespread neo-Bosrupian presumption that technological

innovation will provide sustainability and resilience, especially with regard to cheap energy and food supply. Specific features of the European societies, especially Britain, that were central to Malthus's thinking are the following:

1) Pre-industrial forms of agriculture were the economic foundation, although rendered quite efficient by the availability of large domestic animals, animal fertilizer, wheeled vehicles, sailing vessels, metal tools, complex machines (mills, etc.) and an expanding variety of subsistence and other crops, including non- native staples. All these existed side by side with emergent industrial capitalism, and there was an uneasy tension between tradition and change on many levels. Movement of rural people into cities sparked an urban revolution that was one of the defining characteristics of the nineteenth century. All this, and especially the impact of fossil fuels, happened just too late for Malthus (or his near contemporary Adam Smith) to take into full account.

2) The agricultural landscape in countries like Malthus's native British Isles had long been settled, and only marginal lands remained uncultivated. Although capital investments in landscapes sometimes increased production, the motivation was often the pursuit of profit, not increase in food supply.

3) For British social elites and middle classes, the right to private property was a sanctified constitutional principle that prevented its seizure by kings (or tyrants, as they are called in the Fifth Amendment of the U.S. Constitution).³¹⁵ Insulation of private property and economic initiative from state interference was at the core of European state-formation and capitalism. Malthus viewed "security of property" as essential to social and economic well-being, and most of his contemporaries thought it was fundamental to progressive civilization. Property referred to any form of capital, but particularly land. Land as property was held in a variety of legally specified and protected ways, most importantly in Britain as vast estates owned by wealthy landlords, who also held huge tracts of urban property in rapidly-growing cities such as London. Large private rural holdings constrained the availability of land to ordinary farmers, and Malthus can be accused as not giving proper weight to this aspect of political economy.³¹⁶

4) Many people who worked on the land were neither freeholders nor smallholders, but salaried workers or tenants. Workers were paid wages and tenants were obliged to pay rents. Most agricultural workers were low-ranking commoners whose labor and products supported a complex and sizable class of non-farmers. Many non-farmers belonged to privileged classes, but others were disenfranchised workers who had migrated off the land to reside in cities, where their cheap labor was the capital for incipient industrial production, and where they were often required to pay rents for housing to great landlords.³¹⁷ Raw industrial material as stone, metals, coal and timber were also products of lands frequently owned by wealthy landlords.

5) Farmers rarely lived only off the products of their own land and labor but instead sold cash crops, animal products, and craft goods (directly or indirectly) on the market. Both agricultural labor and its products were commodities whose prices were subject to supply and demand. Prices were assessed in terms of money or all-purpose currency as we know it, and were heavily set by market forces.

6) Market forces were no longer primarily local. Efficient transport, especially by sea, river and canal facilitated the trade of cheap, bulk commodities on international markets. Prices and demand for what farmers produced were thus heavily influenced by mercantile decisions and transactions operating on an increasingly global scale, often in association with stock exchanges and other kinds of financial speculation.

7) Income of many farmers (and other low-ranked producers) was heavily derived from wage-labor, which itself was a commodity priced according to its supply and was vulnerable to the vagaries of weather, war, and other factors beyond the control of workers. Wages fluctuated according to market forces, such as increased or decreased demand for commodities, and to demographic change, such as loss of people during the Napoleonic wars that made labor more scarce and valuable. Malthus recognized that if the only resource you have is your own unskilled labor, then oversupply of people like you renders your labor even cheaper.

It is useful to consider some other blind spots in Malthus's writings, especially in light of his central enterprise, understanding the origins of human misery and devising possible remedies.³¹⁸ Malthus thought that overpopulation in relation to food resources led to misery. Our own modern anxieties about population growth have heavily to do not with the capacity of food-production systems to feed ever-more people, but rather with ancillary effects such as deforestation, soil erosion, fresh water deficits, pollution, climate change (especially sea-level rise), loss of "natural" ecosystems, diminished bio-diversity, and outright species extinction.³¹⁹ In a word, we are impressed by the capacity of humans to alter their agrarian or other niches in deleterious ways. Mayanists have long associated high populations with destructive niche construction, and it is this perspective that has Malthusian overtones.

Broadly speaking, four sets of conditions (not mutually exclusive) caused deleterious population/food imbalances in pre-modern complex societies:

- 1) Agrarian landscapes had and retained the capacity to produce food efficiently, and could be used more intensively with little or no degradation. Nevertheless, population growth outstripped food supply and always will.
- 2) Agrarian landscapes lost capacity to produce food at required levels through various kinds of degradation, much of it human-induced. Even if population did not increase, human well-being suffered.
- 3) No matter how productive the agrarian landscape was, and how innovative farmers were at manipulating it, uncontrollable "natural" shocks (e.g., drought, excessive cold, floods) occurred with sufficient frequency and scale to diminish human well-being.³²⁰
- 4) Unequal distribution of land, agrarian mismanagement, excessive taxation, shift from food to other crops, and war all undercut the food supply available to large proportions of the population. Where bulk staples were transported long distances, as from North Africa, Sicily, or Egypt in later Roman times, disruption of transport was also a serious problem.

Malthus particularly championed the first scenario and in this he turned out to be wrong with regard to the two centuries that followed.³²¹ Boserup's position attracts us because it seems much more hopeful than Malthus's dismal one, both as process and as borne out by recent history. Seen in the long view, however, one could derive exactly the opposite conclusion. A neo-Malthusian perspective is that the very capacity for agrarian innovation (and change more generally, especially in technology) championed by Boserup, and the availability of astoundingly cheap forms of energy, have resulted in considerable misery over the last two centuries, precisely *because* of the human population growth they unleashed. Were Malthus here today he might say that in a strangely inverted sense he was correct. He might also be surprised that, in the face of all the problems we currently face, including collateral damage to the global ecosystem, we are reluctant to admit the role of overpopulation or somehow try to deal with it. One could credit Malthus with being a pioneer of the idea of coupled biological and cultural systems. He seems, though, to have had little appreciation for the human capacity to induce environmental degradation despite a long history

of deleterious changes that had affected many of the landscapes he knew about (see Pye 2014: Chapter 7). His concern was instead the relationship among food supply and economic and social inequality and its implication for how individuals and institutions should act. Most of the Maya literature pays scant attention to inequalities in well-being among social components, or the stresses generated by population growth.³²² Tim Murtha and I (Webster and Murtha 2015) recently raised just these issues but we know little about them. Like others of his time, Malthus took technological progress for granted, and he touchingly (given his later influence on Darwin) hoped that humans might exercise rational restraint when making reproductive decisions. It is this latter idea that in one sense Darwin stood on its head with regard to organisms in general.³²³ Malthus also seems to have thought of wealth, and particularly the money supply, as being comparatively inelastic, or at least subject to slow increase. He probably would have been appalled at the quantitative easing (printing money) that occurs today.

Malthus comes down on both sides of the issue of whether humans could effectively control their fertility, and hence population growth rate, in relation to food resources. He knew that reproductive capacity enabled populations rapidly to increase up to the limits of the food supply. On the other hand he thought, although without much conviction, that through conscious preventive checks (late marriage, abstinence, etc.) an enlightened populace might restrain such increase. If the first perspective is correct, the long interval of low overall population in the Maya Lowlands is as difficult to explain as the big surge that occurred after AD 500-600. For 1500 years or more prior to the Late Preclassic the inhabitants of the central and southern Lowlands had at least one important staple crop – maize – and lived on a vast, lightly-settled landscape rich in agrarian and other resources.³²⁴ They seem to have lacked the epidemic and pandemic diseases that resulted from the close proximity of humans and domestic animals in the Old World. Comparatively dispersed Maya populations were also insulated from the worst effects of intestinal and other diseases typical of high-density settlements. Also apparently absent were endemic diseases such as malaria and yellow fever that caused much mortality in post-Conquest times. All these favorable conditions should have been conducive to comparatively high fertility and low mortality, but this was not the case. Malthus grappled to understand what restraints or checks kept such populations lower than expected in many parts of the world. Boserup (1965: 14) struggled with this same issue and decided simply to ignore it.

But from the perspective of his times, how would Malthus regard the ancient Maya? That the bulk of the Maya population consisted of farmers would make sense to him. He would be puzzled by the primitive nature of Maya agricultural implements, the absence of large domestic animals as energy sources, and the failure of these to evolve much over several millennia. Even more puzzling would be systems of shifting, swidden agriculture, so different from the agrarian strategies and property conventions that he knew about. That a political economy could function without (maybe) private property, formal rents, efficient transport, general-purpose currency, well-developed market exchange, and monetary pricing for labor, food and other commodities would seem incredible to him.

More specifically, how would Malthus assess what we know, or think we know, about the population history of Tikal and the central and southern Lowlands? First, as I already noted, he would be bemused at the very low overall populations we detect in most regions for centuries after maize first became available as an efficient staple around 1000 BC (see Webster 2011 for a discussion of the demographic effects of maize in Mesoamerica). No one knows much about short-term population dynamics on Maya landscapes prior to the Late Preclassic. Malthus probably would have envisioned episodic fluctuations of want and plenty that kept populations, or at least rates of increase, very modest, but he would have been hard-pressed to relate them to food supply. Rapid growth and dense populations in regions such as the Mirador Uplands during the mid-to late Preclassic would not have surprised him, but he would have been perplexed that these developments were so circumscribed, and that the Mirador population deflated and failed to recover its original numbers.

What about the impressive pulse of population growth in the sixth to the ninth centuries documented by archaeologists working at Tikal, Copan, Dzibilchaltun, and many other sites? This pattern, which seems to hold for much of the southern Lowlands, is one of the biggest demographic puzzles of Maya population history. If it is real rather than some artifact of sampling, then how does it relate to the ideas of Malthus and Boserup? One could argue from a Malthusian perspective that extraneous conditions such as the sudden introduction of more productive new cultigens or the onset of a much more favorable climatic interval permitted all this growth. A Boserupian argument would instead be that human agrarian investments somehow greatly improved long-term agrarian conditions beginning around that time. Unfortunately, we currently have too little evidence to argue the case convincingly either way. That this great pulse happened after such a long period of generally low population levels would be a mystery to Malthus just as it is for us, especially in the absence of major technological changes or the wholesale adoption of new staple crops. He would be astounded, though, by the extravagant population density estimates shown in Table 1 that are so at variance with anything he knew from Europe. Malthus would probably take a kind of grim satisfaction at the political disruption and demographic declines that plagued the Maya beginning in the eighth and ninth centuries. The collapse would appear to him as one of those frequent oscillations (although an unusually large one) that he anticipated in any population history, and he might well identify the basic cause as imbalances between humans and food supply. Finally, Malthus, like the rest of us, and certainly Boserup, would be perplexed by the lack of recovery over the following millennium of anything like the population size and density characteristic of the Late Classic landscape.

The dichotomy expressed by Livi-Bacchi at the beginning of this section is too stark. We cannot know what Malthus might think about Boserup's ideas. We do know what she said about Malthus, namely that he had things backwards. Equally obvious is that Boserup overstated her case, especially in the introduction to her book.³²⁵ Boserup herself noted many qualifications and exceptions to her model. On p. 22, she remarks that in many parts of the world agrarian landscapes were permanently "spoiled" by growing populations that did not effectively intensify land use. She goes on to say that "Growing populations may in the past have destroyed more land than they improved, but it makes little sense to project past trends into the future..." The latter part of this statement reveals Boserup as primarily an economic geographer concerned with agrarian development under modern conditions of technological innovation, all-purpose currency, markets, urbanism, and capitalism. Rather than a strict anti-Malthusian, she believed that the positive effects of population growth are dominant over Malthusian ones, at least in recent times. William Sanders recognized that wider application of her ideas, or those of Malthus, must be heavily contextualized with regard to technology, energy sources, soils, and other fundamental variables. As my colleague Jim Wood (1998) points out, in some times and places Malthusian conditions apply, and sometimes Boserupian ones. From the archaeological perspective the trick is to determine which apply when, where, why, and for how long. Malthus had the misfortune to write at a time when Europe and other parts of the world were just entering what is probably the longest non-Malthusian interval in world history, especially characterized by enormous supplies of cheap energy. Boserup wrote as the great post-war economic boom was at full throttle. Malthus knew nothing of the ancient Maya, and Boserup knew very little. Had they been better archaeologists or prehistorians, they might have agreed that the ancient Maya world was especially vulnerable to interludes of Malthusian misery.

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ENDNOTES

¹ See Tokovinine (2013: 1-2) and Ashmore (2015) for derivations and connotations of the word “landscape.” Here I use it in the etic sense to mean how physical things and people were distributed over a specific locale or region, as non-Maya observers would perceive them. The Classic Maya had their own emic perceptions, sometimes called *cosmovisions*, of locales, features, and places (see Ashmore 2015: 310).

² Coba was clearly involved in the great hegemonic struggles to the south between Tikal and the Snake Kingdom – see Guenter (2002: 225-226).

³ *Land, Water, and Territory: A 3000-year Study of Niche Construction and Cultural Evolution in the Tikal National Park, Guatemala*. Timothy Murtha PI, Chris Duffy, Kirk French, Richard Terry and David Webster, co-PIs. National Science Foundation: Dynamic Coupled Natural-Human Systems.

⁴ Human populations are limited in size, distribution and density by many factors, most importantly food, water, climate, shelter, social circumscription, and sometimes by patterns of endemic disease, as in many parts of Africa and New Guinea. For the purpose of this paper, I assume the most significant limiting factor is the food supply. Isotopic studies of diet at Copan show heavy maize intake amounting to some 59% of bulk diet according to Gerry and Krueger (1997: 204). Recent studies of skeletal material from sites nearer Tikal (Somerville, Fauville and Froehle 2013) show that maize was the basic staple for elites and commoners alike, although elites had more varied diets that diverged from those of commoners toward the end of the Classic period. Assays for inland regions are no doubt different from those closer to marine ecosystems. I also understand that food availability only relates to population dynamics in a crude fashion. Short-term fluctuations in availability, nutrient balance in the diet, activity levels, and a host of other factors, including cultural as well as biological ones, complicate demographic effects of food supply on fertility and mortality.

⁵ A recent overview of Tikal agrarian ecology (Lentz *et al.* 2014) somewhat qualifies this perspective. It sees the Tikal (and other) Maya as clever ecosystem managers who nonetheless built up Late Classic population densities that rendered their political and agrarian systems highly vulnerable to uncontrollable ecosystem perturbations of unusual scale – namely severe droughts beginning in the ninth century. They seem uncertain how much weight to put on population pressure as a cause for the political collapse of the ninth century.

⁶ Many Mayanists seem to me to be reflexive Boasians, in the sense that they employ inductive, particularistic, and often mentalist, analyses and reconstructions, and hold strongly exceptionalist conceptions (see Lyman and O'Brien 2004 for discussion).

⁷ At the time of the Tikal settlement research most scholars still thought the southern Classic Maya were comparatively peaceful, or at least that their wars were heavily ritualized, small in scale, or otherwise of not much consequence. When Haviland and Puleston finished their dissertations (1963 and 1973 respectively), the flood of epigraphic and archaeological data on Maya war was still on the horizon. The Tikal archaeologists, especially Puleston and Callender, deserve high marks for their willingness to envision Maya war as sometimes large in scale and motivated by materialist processes and political rivalry.

⁸ Perennial archaeological preoccupation with the collapse of complex societies has recently stimulated a more nuanced literature on transformation, recovery and resilience (see for example Faulseit 2016).

⁹ Christine Carrelli's (2004) later calculations of Early Classic royal constructions beneath the Copan Acropolis seem plausible to me in the context of the small regional population we estimated.

¹⁰ It is unclear whether Coe refers here to the whole Danta complex or just its large pyramid. More about this in Appendix B.

¹¹ More precisely, the Great Pyramid and its ancillary buildings have a mass of about 2,700,000 cubic meters, the vast bulk of which, some 2,580,000 m³, is in the pyramid itself (Lehner 1997: 17, 108). Another pertinent comparison is Monks Mound at Cahokia, which is some 621,921 m³ in volume (Milner 1998: 145), more than three times the figure estimated by Coe for the Danta pyramid.

¹² Some Egyptologists (Lehner 1997: 17) think Sneferu built only built three pyramids totaling 1,875,773 m³. If so, the comparative contrast shrinks to a mere 102 Temple 1 equivalents. In Egyptian political lore, Sneferu is remembered as the archetypal good king, much less oppressive than some of his successors who built on a smaller scale.

¹³ The only Maya construction effort I know that might match those of Central Mexico is the Late Preclassic complex of canals and embankments mapped at Edzna, which Matheny (1978: 209) puts at 1,107,000 m³. But of course this is not a single structure

and we do not know how long it took to complete. Sanders (1979: 498) insisted that Matheny's comparisons of buildings and agrarian features were irrelevant. Some of the major causeways in northern Yucatan are also very impressive in bulk, albeit simple in construction.

¹⁴ Some of my colleagues say this argument ignores the cost of maintenance after structures were initially built. I think this is a false issue for most big buildings. For example, Tikal Temple 1 was in pretty good shape after centuries of abandonment (non-maintenance) when eventually cleared by archaeologists. The principal routine upkeep would have been re-plastering, and I don't think the inputs at yearly or other intervals would be very great. Someone should do the numbers. A good grasp of the chronology of construction is essential to understanding the effort involved. Thus a structure with twice the mass of Temple 1 built over 30 years would be much less impressive in terms of pro-rated costs than the real temple that was built in about seven years. Xie *et al.* (2015) have recently argued that calculations of labor input into earthwork construction fail to include the cost of stone implements that were used in Chinese agrarian and civic projects. Large Maya constructions differ strikingly from the Chinese ones in the materials used – mainly loose soil and rubble – and required only simple tools such as digging sticks and baskets that were easily made by householders. Large chert bifaces were arguably used for digging and shaping stone, but these have spotty distributions as major artifact forms and seem never to appear in discard quantities similar to those of the Chinese tools.

¹⁵ See for example Sahlin's (1963) famous expression of such relationships in Pacific societies.

¹⁶ It seems to have its origins in the writings of Alfred Kroeber on ancient civilizations.

¹⁷ Altschuler is largely forgotten today, but I first learned about the Maya collapse in a course I took from him.

¹⁸ Sanders thought that the 600,000 Conquest-era Yucatan figure might reflect the low end of a range that fluctuated during Pre-Columbian times, with the peak possibly as high as 1,250,000.

¹⁹ Roughly twice the number of people recorded by the 1930 census of the state of Yucatan, some 384,790 (Shattuck 1933: 386).

²⁰ Revealingly, William Bullard (1960), one of the early practitioners of such surveys, was very skeptical of estimates derived from mound counts.

²¹ In 1960, Robert McC. Adams, in an influential comparison of the rise of four ancient civilizations, lamented that "... the lack of data on population densities and land use underlines the purely speculative character of all those heuristic hypotheses which regard cultural change as an adaptive response to direct environmental factors" (Adams 1960: 290- 291). Despite current bickering, we have come a long way since that time.

²² Unusually, these surveys were followed up by Robert Wauchope's excavations of five house mounds (Wauchope 1934), and the Uaxactun work has long been championed as methodologically innovative. Wauchope told Haviland (personal communication to Webster) that the Ricketsons' survey was carried out in a much more slapdash manner than usually believed, which is probably why Puleston later had to modify its results during his re-survey. Nevertheless, it provided structure counts that were the initial stimulus for large population estimates, reinforced by the later mapping at Mayapan.

²³ Such a radius is too large, placing the southern boundary much too close to Tikal. Morley excluded the wetlands as uncultivable, and so his estimates might be considered productive densities for a region of about 800 sq. km.

²⁴ I personally doubt whether the whole population of Mesoamerica before the arrival of the Spaniards reached even the lower end of this range. If these figures are correct, the southern Maya Lowlands was indeed one of the most densely settled regions in the ancient world. Morley's high estimate is roughly equivalent to the entire population of Western Han China during the great initial phase of dynastic centralization in the first century BC when census figures become available (Chang 2007: 67-85; Kidder *et al.* 2016: 80-81). Overall densities in the empire were 43.2 people per sq. km, rising to 76.4 per sq. km in the heavily populated north China plain. Even in the floodplain of the Yellow river itself, the most productive (if flood-prone) part of the landscape, overall mean densities in four well-recorded regions ranged between 42 and 104 people per sq. km. The Roman Empire in the second century AD had roughly the same population as the Han, 50 to 60 million people.

²⁵ I know Copan very well. Such a figure overestimates the eighth and ninth century population there by a multiple of ten (see Webster 2005) and would put overall peak densities at something like 400 people per sq. km.

²⁶ Some influential Mayanists like Herbert Spinden (1928) were mainly interested in Maya art and tended to ignore demographic issues. Spinden (1929) did opine that there were two peak periods of Maya population. He thought the first one, with some

8,000,000 people, was associated with the southern Classic Maya in the mid-sixth century. The second, perhaps involving more than 8,000,000, occurred during “Toltec”-influenced times in the twelfth century. His estimate puts overall Classic densities at about 53 people per sq. km if by the southern Maya he meant roughly the area shown in Fig. 1. This number is well within the capacity of swidden agriculture, but of course implies much higher productive densities.

²⁷ Brainerd seems a bit cautious about his own figure, noting that the overall density implied was twice that of Mexico as a whole at the time he wrote (1954:78-79). Five million was, he says, a maximum estimate. Twenty years later, William T. Sanders (1973: 331-332) postulated the same total on the basis of a detailed examination of Maya ecology and agriculture.

²⁸ He guessed that 2,000,000 might be a better figure than 3,000,000, which would put the entire Lowland population at about 1,000,000 or perhaps a bit more. Although Thompson was not clear about how many people lived in the Lowlands alone, I assume for the purposes of this paper that 2,000,000 would be his maximal number. This works out to an overall population density of about 10 people per sq. km.

²⁹ Although she was not concerned with detailed population issues, Betty Meggers (1954) can certainly be counted as a skeptic. She thought the Maya Lowlands were inhospitable to large, dense human populations over long intervals and thus to the development of durable civilizations. Edwin Feldon (1959) later challenged Megger’s environmental classifications, and his own comparative analysis put the Peten in the same rank as the Basin of Mexico with respect to agricultural potential. This conclusion seems to me not to take into account the population histories of the two regions, and especially the population of the Basin in the early sixteenth century.

³⁰ The Mayapan mapping (Pollock *et al.* 1963) was especially influential, but essentially provided just a big spot density. When Ursula Cowgill (1962) wrote her article about Maya subsistence near Tikal, she insisted that only good surveys could assess the validity of her arguments about population density, and bemoaned the fact that most such data (the Uaxactun and Bullard surveys excepted) came from northern Yucatan, not the southern Lowlands.

³¹ Reading their provisional reconstructions, one sees how tied they were to the milpa subsistence model. Nevertheless, their conclusions about population scale seem much more sensible to me than many that emerged shortly thereafter from other projects.

³² Surveys at Ceibal were of about the same vintage, but they extended over a comparatively small area and were not published in detail by Gair Tourtellot until 1982.

³³ The Mayapan research was probably the original flashpoint for 50 years of debate about Maya urbanism. By the early 1960s, its high residential densities stimulated many archaeologists to call it an urban center (see Haviland 1969 for a discussion).

³⁴ Although low, thorny scrub and henequen plantations are no fun to survey in. On the other hand, there is much less topographic and hydrological variation than compared to the Tikal landscape, which greatly facilitates density estimates. Surveys in such conditions also provide much more detail because it is possible, even without excavations, to identify house plans, doors, internal spaces and other architectural features, as exemplified in surveys at Xculoc (Michelet *et al.* 2000).

³⁵ I suspect he was also sobered by the much more conservative estimates made a decade earlier by Haviland (1963) for the slightly smaller area of the Carr and Hazard Tikal map.

³⁶ Comparatively low topographic, soil, and hydrological variation makes calculations of overall densities converge more closely with productive densities in northern Yucatan. It also makes surface remains a more complicated palimpsest of different time periods.

³⁷ Haviland (1963: 516-517) hints at just such a perspective for Tikal settlement features. I would rephrase Kurjack’s comment to say that architecture is a better reflection of status than wealth.

³⁸ Density figures are usually given in square kilometers. When given in square miles I convert them to metric values in subsequent charts and figures.

³⁹ Ursula Cowgill (1962) claimed that Peten swidden systems could support densities of 39-78 people per sq. km., a very reasonable range, and thought that modern swidden provided a good analogue for ancient farming. She noted but downplayed the obviously greater efficiency of steel tools, and seems not to have appreciated how such tools helped avoid some of the bottlenecks in timing that limit milpa production.

⁴⁰ I am aware that most readers devoted to Maya exceptionalism, or what might be called “the general rules don’t apply” school of thought, will not be swayed by these comparative examples. Trigger (2003: Chapter 14) provides a good overview for

those who think such comparisons are meaningful. Colleagues who read early versions of this paper suggested that I include density estimates for the Harappan polities and for China. I know of no reliable Harappan estimates, nor any for pre-Han China, but I briefly discuss some Han dynasty estimates in Endnote 24. An obvious comparison would be the tropical forest polities of Southeast Asia, but there is considerable disagreement about population scales and densities for this important part of the world. Iannone (2016:195-196) gives a good recent overview. His figures for Angkor at its peak are 750,000 inhabitants in a total territory of 70,000 to 100,000 sq. km. These numbers yield an overall density range of 7.5 to 11 people per sq. km, surprisingly modest for a polity heavily supported by rice cultivation. Local productive densities in areas of canal or flood-recession agriculture would have been much higher.

⁴¹ The Valley of Mexico is technically in the tropics, but because of its high elevation its environment does not accord with our stereotypical notions of low-altitude, humid, tropical ecosystems. Hawai'i is also semitropical with great variation in elevation.

⁴² The Maya engaged in various forms of water control, and these are most clearly documented in northern Yucatan where rainfall is lower than in the south. Irrigation proper (as opposed to wetland drainage or water storage) is known for some areas. Most recently, Lentz *et al.* (2014) summarize evidence for runoff channeling and impoundment at Central Tikal apparently used to irrigate low-lying parts of the landscape. What remains unclear is both the extent of the landscape watered by such irrigation and its productive potential as a proportion of total agrarian requirements. Most food production in the Maya Lowlands always depended directly on rainfall (as these authors acknowledge). Irrigation, where it could be deployed, probably had three main uses: 1) to grow specialized crops such as cacao, 2) to supply water during years when the main crop is threatened by drought, and 3) to provide for one or more dry season crops in addition to the main rainy season one.

⁴³ Hassan (1993: 165) gives a slightly lower estimate of 1.42 million; I use Butzer's here.

⁴⁴ According to Goldstein (2014: 151), the polis of Athens at its political and creative height in the mid-fifth century BC was just about the same size as the Umma province – some 100,000 people, about one third of whom were citizens. This works out to an overall density for Attica as a whole (2400 sq. km) of 42 people per sq. km. Connelly (2014: 8) instead puts the population at 300,000 to 400,000 (125-166 people per sq. km). Whichever figure we prefer, big and glorious things come from small packages (see Connelly for the extraordinary scope of building projects leading up to the completion of the Parthenon). Of course, the Athenians had the advantages of slave labor and a money economy, with enormous income from war booty and appropriations from the treasury of the league of Greek states that they dominated.

⁴⁵ The Sumerians constructed very large buildings of mud brick, timber and stone at Uruk as early as the mid-fourth millennium, even though populations were still comparatively small. Algaze (2013: 78-79) provides some calculations. The labor estimates he cites seem high to me compared to the Maya ones. On the other hand, the Maya piled up mostly quarried earth and rubble, while the Sumerians had to make huge numbers of mud bricks even before they began to assemble buildings. The labor investment per m³ is thus high by comparison, but even so, single substructures and temples had masses of tens of thousands of m³. The local Uruk polity probably incorporated about 80,000-90,000 people at the time, so a lot was built by a modest population, including an Early Dynastic defensive wall some 9.5 km long.

⁴⁶ A 40-50,000 sq. km region of Central Mexico, with the Basin as its demographic core, would be a fairer comparison with Egypt and Mesopotamia, but good settlement data are lacking for the landscape on this scale.

⁴⁷ Just to carry on with the construction estimate comparisons, the Pyramid of the Sun at Teotihuacan has a mass of about 1,175,000 m³, almost six times that of El Mirador's Danta complex using Coe's figure, and some 55 times the mass of Tikal Temple 1. See Appendix B for more reliable figures and comparisons.

⁴⁸ I got the 7700 sq. km figure from recent GIS analysis done by Greg Luna, who recently completed his Penn State dissertation on the pre-Conquest Aztec chinampa system, and who has generated an impressive series of associated maps (Luna 2014). Eliminating the area of the lake (about 1000 sq. km) results in higher population densities, but it is unclear how to do this. Much of the western and southern lake system was, by AD 1519, itself part of the great chinampa breadbasket of the Basin, and many kinds of resources, including food, were obtained from other sections of it. Sanders himself sometimes favored an area this large. For example, in 1978 he used a figure of 8000 sq. km (Sanders and Webster 1978: 256).

⁴⁹ Sanders thought the Basin proper had a 1519 population of about 1,160,000 people, to which he added another 400,000 along its northern fringe, bringing the total to roughly 1,600,000. This is close to Thomas Whitmore's (1991: 477) estimate (1,590,000) based on epidemiological/agricultural simulations, for a 7000 sq. km basin. Whitmore's overall densities are higher than Sanders's, some 227 per sq. km using the 7000 sq. km area figure, and 206 per sq. km using Luna's 7700 sq. km.

⁵⁰ See Evans (2013: 449). Trigger (2003: 306) gives an overall density of 200 people per sq. km for the Basin.

⁵¹ Most recently, Greg Luna (2014) reconstructed population density on a 1010 ha sample chinampa zone at about 250 people per sq. km. Such numbers of course do not represent any kind of carrying capacity because a large proportion of their crops – probably about two-thirds – supported urban (or other) populations.

⁵² The Valley of Oaxaca and its environs were second only to the Basin of Mexico as a major nuclear region of Mesoamerica. Unfortunately, the population of its 11 to 13 city-states at Spanish contact is very poorly known. Oudijk (2002: 80-83) gives rough estimates that are well below 100 people per sq. km. Helen Pollard (1993) estimates that the well-documented Tarascan empire in western Mexico covered an area of 75,000 sq. km in AD 1519, and had a population of some 1,500,000 people. Overall densities work out to 20 people per sq. km.

⁵³ This comparison is a bit deceptive because it compares the peak period in the Basin of Mexico with an early period in Egypt. If the Roman population of Egypt peaked at about 4,500,000 people, then overall densities were about 132 people per sq. km, squarely within the Basin of Mexico range.

⁵⁴ Mayanists often bristle at the notion that the Classic Maya were less than state-like, and many analyses are concocted to counter such criticisms; see for example Adams and Jones (1981). High densities and supposed settlement/administrative hierarchies bolster arguments for complexity as already noted – see Appendix F.

⁵⁵ Four major polities contended for dominance in the islands in 1778, particularly for the irrigated taro and sweet potato zones.

⁵⁶ I suspect this number is a bit too high, but I retain it here for the purposes of comparison.

⁵⁷ Most Polynesian domesticates come from eastern and southeastern Asia. Sweet potatoes were a late introduction from South America, although how they got to the Pacific is hotly debated.

⁵⁸ This period not coincidentally is that which most concerned Thomas Malthus and provided core data for his *principle of population* – see Appendix G.

⁵⁹ As late as 1801, when census data were reasonably good, most parts of France had densities of 80 people or fewer per sq. km (see Robb 2007: 16) – this in one of the richest agricultural countries of western Europe.

⁶⁰ Hawai'i is something of an exception because Europeans saw it at its peak, and in fact the modeled population closely accords with some early estimates. Europeans also observed the Basin of Mexico first-hand, but ethnohistoric population estimates are quite variable.

⁶¹ When an author has given a density range, I use the high end in Fig. 1; it is these densities, after all, which are most at issue.

⁶² It is not clear to me if Roscoe's figures refer to overall or productive densities; I suspect the former. Some New Guinea swidden horticulturalists such as the Tsembaga (Rappaport 1971) had population densities under 50 people per sq. km even though their root-crop staples, taro and sweet potatoes, have higher yields than maize (see Appendices A and C). And, of course, absolute population size becomes critical here. A local population of 500 Chimbu on the New Guinea landscape with the density of 100 people per sq. km has very different evolutionary implications than a polity of 30,000 people with a much lower overall density.

⁶³ Sanders is widely thought to equate population density with social complexity. To be clear on this point, he thought that populations with overall low densities, as in the ethnographic African cases, could support state-type organization. He did believe that high population density in combination with large absolute numbers of people were critical factors in the evolution of urban centers.

⁶⁴ This is mainly but not entirely true for the Basin of Mexico. In some places, such as the Teotihuacan Valley, residential features and terraces were often visible on the surface. Some parts of Hawai'i have similar remains.

⁶⁵ Estimates prior to the Tikal surveys were highly off-the-cuff; for example Eric Wolf (1959: 98) gives an uncited figure of 100,000 people at AD 600.

⁶⁶ Haviland (1963) notes some of the deficiencies of this map. Not all of the 16 sq. km were mapped by plane table, and some features were incorrectly mapped as structures. Others were invisible on the surface, especially those predating the Late Classic, and some structures had been leveled to make way for later ones. Nevertheless, the map had immediate impact. William Sanders (1962, 1963) completed his influential overviews of Lowland Maya ecology, in which Tikal figures prominently, just as the map was published, but before he had access to the later survey and household data of Haviland, Puleston, Fry, and others.

⁶⁷ In this they were partly reacting to V. Gordon Childe's (1950) discussion of urbanization in the Old and New Worlds published a decade earlier. We saw above that Childe only grudgingly included the Maya as one of his four urban examples for two reasons. First, Maya "cities" clearly had monumental architecture and evidence of impressive attainments in art, calendrics and writing, and so exhibited several of his criteria for urban civilization. Second, he argued that cultural evolution everywhere passed through certain stages, the latest of which – civilization -- was defined largely by the emergence of cities. His three Old World examples might have had contacts through diffusion, so he had to include New World cities that were clearly the products of independent evolution. Either Tenochtitlan or Teotihuacan would have been better choices than the problematical Maya, and I have always wondered why Childe ignored them. He might have shied away from Tenochtitlan because in 1950 we mainly knew about the Mexica capital from ethnohistoric accounts, not archaeology. Teotihuacan was monumentally visible, but right through the 1950s many scholars maintained it was a ceremonial center, not a city, and there were few overt signs of the intellectual and artistic attainments so characteristic of the Classic Maya.

⁶⁸ Not only did Haviland and his colleagues excavate many sites on the Carr and Hazard map, but they also published their results in very detailed (although not always user-friendly) fashion. See *Tikal Reports* 19, 20A, 20B, and 22.

⁶⁹ He called his estimate minimal in part because he thought that ongoing palace excavations by Peter Harrison would add some information about a significant component of royal-elite people. This is an early intimation in the Classic Maya literature that there were palatial residences in site cores (although archaeologists at Uaxactun had earlier traced the evolution of such a residence at Group A-V). Unfortunately, Harrison wound up devoting most of his time to the royal palace, the Central Acropolis. Haviland (2008: 268-269) provides an overview of palace locations.

⁷⁰ This pernicious figure comes from the Chan Kom studies of Redfield and Villa-Rojas (1934), and its application usually results in overestimation of population. The basic problem is that Chan Kom was a pioneering *ejido* community founded in the early 1920s. Its inhabitants had an extremely skewed demographic profile because of their migrant origins. Of the town's population in 1929 (197 people), fully 100 were under the age of 15 and 168 were less than 35 (Shattuck 1933: 142). No established Late Classic population would have had this kind of profile or its potential fecundity. For another criticism of the Chan Kom evaluation, see Michelet and Becquelin (1994). A better analogue is probably the Peten population in 1845, before the recent period of rapid growth. At that time, mean household size was four persons (Schwartz 1990:105). William Haviland (personal communication 2016) cautioned me that objections to five or more people per house reflect our own ethnocentric experience of large amounts of per-capita space. Just to be clear, I know perfectly well that large numbers of people often live in small, traditional houses today in regions like the Copan Valley, and that some very small domiciles might have five or even more residents. My objection is rather that such numbers cannot be extended over all contemporaneous domiciles in a settlement because they do not take into account the short-term cycling of household members and the long term effects of high or low fertility on mean household size (for discussion see Webster, Freter and Gonlin 2000: 158-160). What to make of the multi-family groups living in single houses in the oft-cited 1570 Cozumel census (Roys, Scholes and Adams 1940) is unclear to me, but it has no obvious relevance for the pre-Conquest Maya residential remains I know about.

⁷¹ Haviland (1969: 429) puts the excavated "small structure" sample at 117 (associated features such as chultunes were also tested). These structures were all in the central 9 sq. km of dense settlement on the Carr and Hazard map and many of them were not very small, given what we now know about the range of Maya residential architecture at Copan and other sites. Haviland (1965: 17) realized that big lateral exposures yielded much better results than simple test-pitting. He provisionally estimated that all but 25 of the 117 structures he excavated (84%) were "houses" because they supported pole-and-thatch superstructures like those in modern Maya communities. Others appeared to him to be kitchens or shrines. Puleston (1973a: 128) adopted the 84% residence ratio in his own demographic calculations. House groups in three locales clustered around more elaborate buildings that Haviland called 'palaces', most notably in Group 7F-1. For a summary of Tikal's non-royal palaces see Haviland and Moholy-Nagy (1992: 51-52). Elaborate façade sculpture, carved thrones and inscriptions are not among the sub-royal palace identifiers they list, in striking contrast to elite residences at Copan, Palenque and some other centers.

⁷² This total specifically refers to the 9 sq. km of the map that were intensively mapped, and where Carr and Hazard counted 2120 structures, including temples and other big buildings (Haviland 1963: 522).

⁷³ Sanders and Haviland envisioned similar population scales, but squabbled about how these should be calculated from settlement data, particularly how contemporaneity among residential sites should be assessed and what size social groups should be assigned to household remains (see Haviland 1969). Haviland also thought that the residential structures he excavated showed signs of constant use, and so discounted episodes of abandonment. He believed forest would quickly regrow on abandoned buildings, damaging them so badly that they could not be re-inhabited. I find this reasoning curious, having personally dug many small platforms that might easily be refurbished even though they have been abandoned for a millennium or more. Moreover, Haviland's buildings were in the heavily occupied 9 sq. km zone of the Carr and Hazard map, where high forest would probably not have been allowed to regrow at all during the eighth and ninth centuries. Sanders (1962: 98-99) remarked that mid-twentieth century census data from the Valladolid district of northern Yucatan, which retained

an unusually dispersed pattern of settlement, showed surprising patterns of abandonment and re-occupation of small house clusters over periods of 20-30 years, and that old house platforms were re-used. Sanders and Haviland did agree that Tikal was much less “urban” than Tenochtitlan or Teotihuacan, and neither thought that density alone was a good criterion for urbanism. Although many Mayanists at the time characterized Mayapan as an urban center, Haviland (1969: 431) argued instead that it had the same organizational character as the more dispersed Tikal, but that settlement had been enormously compressed within its defensive wall by the threat of warfare. Haviland apparently derived his Tikal population estimate by multiplying structure numbers by the factor 5.6, as Mayanists commonly do. Although I think this greatly overestimates population and I accept the round figure here of 10,000 for the sake of discussion, I would not jibe at one lower in Sanders’s range. Sanders (1963: 207-208) thought a figure of four people per platform was better, but seems not to have considered that many platforms, especially within mound groups, were not habitations.

⁷⁴ Haviland argues that Tikal was an “accidental city” in that its growth and layout was much less planned than Teotihuacan’s. More specifically, he thinks that it was a “power place” that attracted people because of its auspicious connections to “supernatural beings, forces and ancestors” (personal communication 2016). As to the “accidental” dimension, remember that Teotihuacan itself had much more unplanned settlement during the first two centuries or so of its urban existence. And the “power place” conception seems completely consistent with the notion of Tikal as a regal-ritual center focused on the royal court, as Sanders and I argued years ago (Sanders and Webster 1988).

⁷⁵ In this, he echoed Hester (1954: 132), who long before had asserted that Maya “cities” were primarily political and administrative centers, but not urban in the demographic sense. A little later, Willey changed his mind and adopted a ‘true city’ model that “... brings ancient Maya society into the mainstream of cultural evolution, entailing as it does the institutions of state-level governance, an urban-type economy, and the correlates of a complex class system” (Willey and Hammond 1979: xii). This comment betrays the ancient and powerful conviction, derived principally from Plato and Aristotle and channeled through Childe, that the city was a metonym for the most perfect community form evolved by humans and hence a diagnostic of evolutionary attainment (see Pagden 1982: 68-69).

⁷⁶ Note here and elsewhere the very flexible and inconsistent notion of what parts of the Tikal landscape are the “city.”

⁷⁶ It remains to be seen if this conclusion would hold up if settlement in a much larger region such as the whole 120 sq. km “earthwork” zone were similarly measured. I also think that Landa’s description of Maya settlement quoted by Arnold and Ford (1980: 714) is misunderstood. In my opinion the landscape described by Landa was not a “town”, but instead the settlement landscape of an entire local political unit, or *cah/batabil* (Restall 1997; Quezada 2014). There are also distinct problems in making volumetric/labor calculations on the basis of the kind of map produced by Carr and Hazard.

⁷⁸ MacKinnon (1981) was puzzled by the lack of phosphorous concentrations expected in soils near residences if intensive household gardening was practiced. He explained this away by surmising that heavy rainfall redistributed soil nutrients.

⁷⁹ Like many archaeologists at the time, MacKinnon (1981) thought that “... the hierarchical nature of Tikal’s social and political system...” remained unclear (p. 246). He confidently identified a palace population component, but his settlement distributions led him to still entertain the kind of “cargo” model advocated by Vogt, Culbert and others.

⁸⁰ See Chapters 2, 6, and 8 in Lentz, Dunning and Scarborough (2015). Lentz *et al.* (2014: 8) report very small concentrations of terraces, and also document the degree to which the built landscape of central Tikal channeled runoff into drainage systems and reservoirs, where it seems to have been used for limited irrigation. The area irrigated and its productive potential are not given. More extensive terraced areas might lurk elsewhere on the Tikal landscape and might be seen on the new LiDAR images. Surface surveys at Copan documented a few areas of contour terracing, but recent LiDAR imagery of the core of the Copan kingdom reveals many more, although they are of uncertain date (Von Schwerin *et al.* 2016; see comments below in Appendix C).

⁸¹ Puleston critiqued Haviland’s earlier estimates rather crankily in his dissertation (Puleston 1973: 201-207). One gets the impression that Haviland and Puleston were not of one mind on many things. If I could talk about all these issues with either one of them over a beer today it would be Haviland, who seems to me to have the more refined anthropological sensibility. See, for example, his attempt to trace descent within lineages using burials, architectural stratigraphy, and monuments within Group 7F-1 (Haviland 2015: Fig. 38).

⁸² Haviland and Puleston benefited, of course, from the pioneering Uaxactun surveys of the Ricketsons, from those of Willey in the Belize Valley, and from the Carr and Hazard (1961) map.

⁸³ Puleston’s 1973 dissertation appeared about the same time as that of Edward Kurjack’s (1974) on Dzibilchaltun. The latter was equally innovative, but was based on very different survey methods and coverage on the comparatively open landscape of northern Yucatan.

⁸⁴ A great deficiency in the Tikal research is lack of data on diet from isotopic traces in skeletal samples. Obviously we would have to adjust our models of subsistence behavior considerably if maize intake proved to be generally low. Given recent isotopic dietary studies elsewhere (Somerville, Fauvelle and Froehle 2013), the maize-staple model seems entirely justified. We found apparent isotopic traces of maize production in our Tikal soil samples.

⁸⁵ Don and Prudence Rice did similar surveys in the nearby central Peten lakes region (see Rice and Rice 2016). Vilma Fialko did surveys in the late 1990s between in the Rio Holmul region, but I know of only a short report of her findings (Fialko 2005).

⁸⁶ Puleston (1974) also resurveyed the old Uaxactun transects and found many structures missed by the Ricketsons. Jeffrey Parsons (2010), one survivor of the brecha surveys, provides a personal account of conditions and difficulties.

⁸⁷ In evaluating subsequent calculations of population density by Tikal project archaeologists, I use Puleston's 57% to 43% ratio that has been widely applied to the landscape of the northeastern Peten. Puleston regarded the uplands as the main agricultural landscape, although he probably recognized that by Late Classic times much of the uplands within his survey zone were covered with settlement, or had already been degraded by centuries of use. If Puleston's proportion were extended over thousands of sq. km, one would also have to subtract those zones that were political buffers during Late Classic conflicts, or otherwise not used.

⁸⁸ The resulting structure densities are far below those recorded for Dzibilchaltun by Kurjack. Any estimates based simply on such densities would thus make Dzibilchaltun's population much larger than Tikal's.

⁸⁹ The zone delimited by bajos and earthworks is variously given in the literature as 120, 120.5, 122, or 123 sq. km. I use the round figure of 120. The total number of structures in an area of similar size around Dzibilchaltun, using Kurjack's extrapolation logic, works out to 50,400.

⁹⁰ Traces of such features have been identified since Puleston's time – see Lentz *et al.* (2014) for a recent summary. Even where they exist they might represent coping strategies, not innovations that much (or at all) raise productive potential or efficiency (see Murtha 2002 and Appendix C for discussion). Various forms of what we take to be intensification, such as drained fields, can also be adaptations to risk and are not therefore density-dependent. We should not be surprised if they appear very early in the Maya Lowlands.

⁹¹ Presentations of Maya glyph meanings are sometimes confusing. For example, Velasquez (2010: 266-277) first notes the absence of any glyph for "city", but then goes on to attribute just this meaning to the glyph *ch'en*, which literally means cave. Sometimes it was used as a toponym, but certainly not in any sense that relates to urban places as distinct forms of settlement in a broader comparative perspective (see Endnote 93).

⁹² An abundant literature, ably summarized by Prudence Rice (2004: 78-83), documents the early Colonial period ritual circuits, feasts and other ceremonies that defined territorial units in northern Yucatan. As early as 1545, such rituals were sometimes used to sort out contested boundaries of "provinces" dominated by great *halach winikob* such as the Kokom and the Xiu. Rice thinks these practices represent pre-Spanish traditions, but the ones recorded in ethnohistoric accounts took place after Spanish influence took firm hold on native traditions. With regard to larger economic patterns, there have long been two competing views of economic behavior and institutions just before the Spaniards arrived (see Kepecs 2003: 259-261). The older view emphasizes a high degree of agrarian/household economic self-sufficiency. The more recent one sees northern Yucatan as a hotbed of specialization, markets and trade, and asserts that elites had strong interests in entitlements with respect to certain kinds of "real estate", such as salt beds and cacao-producing lands. This highly commercialized world-system model is contrasted with the more archaic prestige-goods system of the Classic Maya (Kepecs 2003: 266). I agree with Kepecs about the early prestige-good economic focus, but I think the commercialized model is overstated. If one accepts it, the intense materialist bent of the Late Postclassic system seems to me to sit uneasily with Quezada's view of the lack of territorial concern on the part of elites.

⁹³ *Ch'en* literally means "cave", but Tokovinine and others point out that the real sense is some kind of sacred locale or feature at Maya royal courts central to the local patron deities and to the spiritual identity and well-being of the dynasty, along with its kingdom and population. Because of their close identification with rulers, *ch'en* features were often despoiled during warfare. One must be careful not to stretch the altepetl comparison too far. It is unclear if the Classic Maya commonly had the local markets so characteristic of Central Mexican polities, and there is no clear sign of the heterarchical or multiple sets of rulers that occurred in some "complex" altepeme, although polities like Pomona do show suspicious segmentation. There are many signs of Maya co-kingship or joint rulership of various sorts, including age-based ones (see Martin 2017 for a recent summary).

⁹⁴ Worth mentioning here is the small scale of the core Tikal polity in terms of human movement. For example, if there were well-maintained trails, one could walk to the Central Acropolis from virtually anywhere on the Carr and Hazard map in an hour or two. A three or four hour walk would take one to Uaxactun or more than halfway to El Zotz to the west. We used to walk, sometimes heavily loaded, along forest paths from Piedras Negras to the Mexican border, some 18 km away, in about 3.5 to 4 hours.

⁹⁵ Their 45,000 figure is also derived from their simulations of forest cover and wood use, which suggests that larger populations could not have been supported (Lentz *et al.* 2015: 183).

⁹⁶ Puleston (1973a: 262-263) says analysis of old topsoils from below the earthwork embankment showed high fertility, one probable reason why he favored an early date for the system.

⁹⁷ But see Rice and Puleston (1981: 147) for indications that just before his death Puleston and his students were taking a more statistical approach to structure groupings.

⁹⁸ Sanders (1963: 216-223) had earlier carried out ambitious household test-pitting around Terra Nueva in the Chontalpa, and at Tancah in Quintana Roo.

⁹⁹ The high concentration of population in the Copan urban core made me suspect that many residential sites there did double duty. Copan farmers with principal residences near the Main Group might have maintained secondary seasonal residences in outlying parts of the valley. None of these would have been more than a two-day walk from the urban core. Such dual residential arrangements would go a long way to explain why the rural population outside the Copan pocket was so small (Webster, Sanders and van Rossum 1992). The same possibility occurred to me for Piedras Negras during our later settlement work there. If such dual residential patterns existed they would be extremely difficult to detect archaeologically. They might explain some differences in artifact assemblages between core residences and outlying rural sites at Tikal and at Piedras Negras (Nelson 2016).

¹⁰⁰ Bronson (1968) found 26 “hidden structures”, all of which were abandoned by AD 770. Many were Early Classic (Puleston 1972a: 1965-66). This means that the survey data probably under-represent early occupations, but that there is no serious deranging effect for the Late Classic. Fourteen of eighteen “vacant ground” tests yielded at least traces of Preclassic pottery (Culbert 1977: 32).

¹⁰¹ One obvious problem is that unless all structures are well-tested, there can be considerable distortion in assigning population to occupations. For example, Haviland (2014b: 147) was able to chart the rough evolution of some 20 small structure groups at Tikal. These collectively had about 21 “houses” when the groups were founded, and eventually incorporated some 51 houses just before abandonment. If a single test-pit in a group revealed, say, the presence of artifacts from all ceramic phases, one might fall into the trap of imagining that early occupations had the same number of structures as the later ones, with consequent inflation of population estimates. In other words, “occupation” might be clear, but its scale remains unknown, complicating assignment of people to residences. Extensive lateral excavation helps avoid this problem.

¹⁰² This is especially true if one wishes to incorporate per-capita floor space calculations such as advocated by Narrol (1962), a complicated methodological approach that I will say no more about here. The other big advantage is that it helps to distinguish domiciles *per se* from other kinds of ancillary structures and so allows better assignment of people to structures. Imagine the demographic distortion at Ceren (Sheets 2006) if we counted all structures as domiciles.

¹⁰³ Haviland waffles a bit. He usually sticks to his 45,000 figure, but in 1969 said instead a minimum of 49,000 people for a somewhat larger area (1969: 430). Later he confusingly says that there were “... 50,000 people at Tikal alone, with at least that many more in the surrounding countryside” (Haviland 1992: 937). Nevertheless, the scale of his estimates has remained pretty constant. His 45,000 figure is found in his latest overview (Haviland 2008: 259) and I use it here, as do Lentz *et al.* (2014).

¹⁰⁴ One might argue that if population were equally dense in Ix and Imix times, ceramics of the former complex would be under-represented because they were cleared away by later Imix people. Very large proportions of artifacts from household excavations, however, came from fill, where recycled early ceramics would probably wind up, so there might not be a real problem.

¹⁰⁵ He probably means the upland part of the landscape within the earthwork limits, which using Puleston’s figures actually works out to about 69 sq. km.

¹⁰⁶ This figure includes bajo zones with little or no habitation. If bajos are excluded, the total upland zone is only about 11.52 sq. km.

¹⁰⁷ This study enlisted Prof. Richard Terry of Brigham Young University as sub-contractor to the main project. Tim Murtha, Jay Silverstein, Kirk Straight and Horacio Martinez, along with Terry and his students, did the fieldwork.

¹⁰⁸ Various Tikal archaeologists, including Puleston’s widow Olga Stavrakis Puleston, report that one of their Guatemalan fieldworkers, Gil Martinez, found an east-west segment of the earthwork during the 1967-68 season near the southern end of the south transect and the Santa Fe bajo (see Fry 1969: 48). Martinez is lauded as a careful observer, but unfortunately, no one knows exactly where he was or what he saw. We were aware of this reference when we did our own project, but we found no traces of this feature. To our knowledge, no one else has seen it either, unless it is the little southeastern segment mapped by Murtha.

¹⁰⁹ Guenter made this estimate before our own fieldwork revealed that the earthworks were much larger than previously thought (Webster *et al.* 2004). He is correct about the scale of construction if one assumes that the known sections of the earthwork were envisioned and built as a single construction effort. Unfortunately, we don't know that this is true.

¹¹⁰ This is only my surmise. One of the attractions of this date in the 1960s was the inception of the earthwork with the so-called hiatus (Guenter 2002; Moholy-Nagy 2016). Our conceptions of this purported time of stress at Tikal, and more generally in Classic Maya society, have radically changed. If the earthworks were begun around AD 550, however, it does place them only a few years after the famous "mystery cloud" event that occurred in AD 535 or 536, as detected in dendrochronology, varve, and ice core data, as well as in historical records from as far away as China. Dull (2014) and Payson Sheets (personal communication 2012) believe that this event was caused by the Ilopango volcanic eruption, formerly dated sometime between the second and early fifth centuries.

¹¹¹ Why archaeologists beginning with Haviland (1969) so confidently thought that the earthwork demarcated Late Classic Tikal settlement remains unclear to me, given the uncertainty in its dating and their own surmise at the time that it might be Early Classic. It is this Late Classic boundary function that was endlessly accepted and repeated in the literature.

¹¹² One wonders what the ancient Maya would have made of the layout of the Sustaining Area Project's surveys as shown in Fig. 6, which accords weirdly with Maya and Mesoamerican spatial conceptions and cosmograms.

¹¹³ Steggerda noted many years ago that soil at the base of hillslopes in the Puuc region was enriched by soil creep, and that such zones could sustain cropping for considerable periods (see Sanders 1962: 86-87).

¹¹⁴ This range is almost exactly the same as that calculated a generation earlier for the southern Maya Lowlands by Whitmore *et al.* (1990), but was generated by an independent set of algorithms.

¹¹⁵ It is only fair to remember Don Dumond's (1961) analysis of the potential of various forms of swidden, and his conclusion that Lowland Maya swidden might have supported 150 people per sq. km or so. Sabloff (1990: 79) was even more optimistic about swidden capacity, saying that it could support densities as high as 160 to 320 inhabitants per square kilometer. He does not cite the source for his estimate, but the high end of the range is certainly excessive. Perhaps these numbers assume root crops rather than maize. Our own overall population estimate for Tikal is roughly the same as that calculated for the Contact period Mani "province" in northern Yucatan, ruled by a *halach winik* of Xiu descent. At 8000 sq. km, the Mani province was spatially larger, however, so overall population densities were much lower (see Sanders 1962: 91).

¹¹⁶ Using this model, roughly 50,000 people outside the Carr and Hazard zone had to contribute about a 20% surplus to support the 10,000 people or so inside it *if the latter produced no food for themselves*. This scenario seems extremely unlikely. Households located within the zone (those of royalty and nobles apart) probably had house-lot gardens and orchards, and of course might have seasonally exported labor to cultivate outfields at some distance.

¹¹⁷ Boserup (1965: 20) calls this a "dynamic theory of land use" that emphasizes a continuum of use involving natural forest and fallow systems.

¹¹⁸ Maize in the main fields is usually inter-planted with many other species (see Nations and Nigh 1980:10-11 for lists from ethnographic observation). Energetic yields of all these ancillary plants are usually not given.

¹¹⁹ Some forms of irrigation and wetland cultivation aside, the various managed forest models are all based on rainfall and so are vulnerable to drought.

¹²⁰ See for example Ruben Reina's (1967) analysis of the complex decisions of modern milpa farmers in Peten even where population densities are low and forest land seemingly still abundant.

¹²¹ In current resilience theory parlance, complex top-down agrarian management is one form of interconnectedness that might sometimes be beneficial, and at other times constitute a rigidity trap.

¹²² These authors claim that I "... blame environmental changes exclusively on human activity (Webster 2000: 348)." If you read that page in my book, you will find that I said no such thing. More oddly, my own (uncited) chapter (Webster 2014) in the same volume where their comment appears makes clear my recognition that there were synergistic relationships among demographic variables and environmental changes, including drought, hurricanes, and soils. Nor do I think that damage was so extensive as to eliminate productive potential. At Copan, which I know well, soil simulations by John Wingard show that however destructive human activity was, plenty of land remained to support a sizable population long after the dynastic collapse.

¹²³ Puleston attempted to assess the productivity of ramon, but with suspect results (see Appendix D). Benedict and Steggerda (1936) were surprised that their study of foodways of traditional Maya people showed low consumption of fruits, and ramon is not among those they list as food sources. Both Schwartz (1990) and Reina (1967) characterize ramon as a starvation food.

¹²⁴ They apply to interior regions such as Tikal and the northeastern Peten in general, where upland mollisols predominate; they are only partly applicable to riverine zones associated with Palenque, Copan, or many sites in Belize.

¹²⁵ They report (1980: 8-13) that traditional (pre-1970) Lacandon swidden produced 2800 kilos of shelled maize per ha (after pest losses of 500 kilos) during the first (spring) cropping of a newly cleared milpa, usually cut from second growth. They themselves remark that these yields are much higher than reported by other observers for Maya swidden (see Table 3) and of course required metal tools. The reported totals do not include energy yields of ancillary crops planted in the main milpa or the “fallowed” *acahual* fields. New milpas are generally cut from second growth because primary forest takes about 4-5 times as much labor to clear using steel tools. Milpa cycling is 5-7 years or 20 years. New fields are generally cut adjacent to old ones so that the *acahual* fields can be managed to produce plant and animal foods and provide other resources. Needless to say, the Maya Lowlands were drastically underpopulated if yields approaching those championed by Nations and Nigh (1980) were reasonably widespread and common.

¹²⁶ Lentz *et al.* (2014: 4) suggest that “... soils may have been naturally renewed via the capture of windblown soot, dust, and volcanic ash.” No doubt such deposition occurs, as I know from personal experience. In 1982, we sat in our project house at Copan and watched as ash from the El Chichon eruption near Palenque dusted our courtyard garden. No one so far as I know has made the case that such importation occurs sufficiently regularly and on the necessary scale to counteract human-induced degradation of soils or soil chemicals such as phosphorous. On the other hand, Tankersley *et al.* (2015) document that the Quaternary sediments from wet locales tested around Tikal derive heavily from degraded volcanic materials. They argue that “Without ash fall contributions soils on limestone surfaces would have likely remained skeletal and posed significant problems for the long-term subsistence by ancient Maya populations”(p. 211). Of course heavy ash falls would also have played havoc with agrarian production over the short term.

¹²⁷ This is the same density threshold advocated by Sanders and the Cowgills in the early 1960s (see Sanders 1962: 96).

¹²⁸ Managed forest models are usually applied to the humid ecosystems of the southern Maya Lowlands. Revealingly, the northern Maya over the long run showed more resilience on a dryer, less well-vegetated and well-watered landscape than their southern neighbors where the model should work best – this despite the major droughts postulated for the ninth century and later throughout the Lowlands.

¹²⁹ During our film project, William Sanders and Don Rice interviewed farmers from the village of San Jose on the north shore of Lake Peten Itza, where Reina had earlier conducted his influential fieldwork. *Kol* was their term for milpa and they used *kax* to refer to high forest. These farmers knew that yields were higher in fields cut from high forest, so the term *kax* to them signified both a mature forest community and elevated soil potential. But we must be careful here to distinguish between high forest and original (never-cut) forest as I have argued elsewhere (Webster 2014). Both are *kax*, but not much forest in the central Peten or around the lakes was still original forest by Late Classic times. By then it had been cut over many times, with consequent effects on soils. The Preclassic Maya had lots of *kax* in the sense of original forest, but their descendants had much less.

¹³⁰ One could of course imagine that by Late Classic times Maya rulers and elites had become risk-managers who functioned to ameliorate the worst stresses of agrarian shocks and the divergent well-being of their subjects. I see no signs of this, and I don't know how it could have worked effectively (see Appendices C, D, and E).

¹³¹ The 1967 article was very influential at the time, although based on a small and possibly biased sample.

¹³² Just when kings first appeared is unclear. Martin and Grube (2008) and Martin (2014) give the most influential overviews of Tikal's dynastic history. They put the first known rulers at around AD 50-100, but think there might have been earlier ones poorly documented in the inscriptions, as is possibly the case at Copan. According to Estrada-Belli (2011: 120-122), the dynastic founder at Tikal at around 100 AD was Yax Eb Xook. Haviland (2008: 277) says that the founder of the ruling dynasty died about in about 75 BC, and that “... evidence for kingship precedes this date by about 100 years...”

¹³³ Haviland (1970: 193) claimed that most structures and groups were occupied by AD 550, so population was already substantial by that time. Puleston (1973a: 147) simply opined: 1) that Middle Preclassic populations was “rare”, widely scattered, and located on well-drained ground in “peripheral areas”; 2) that Late Preclassic occupation sites might have been seven times more numerous, and are within 5 km of Epicentral Tikal; and 3) Early Classic sites might be three times as numerous as Late Preclassic ones.

¹³⁴ Puleston (1973: 217) thought that Fry's decision to make plaza *groups* the focus of testing, rather than individual structures, created an over-representation of Early Classic sherds.

¹³⁵ Haviland (1963: 403) detected very little pre-Late Classic material when he excavated eight groups to the northeast of the Epicenter. He drew a different conclusion – that early settlement was more spatially dispersed. I suspect it was more concentrated, but also very small.

¹³⁶ This surge is a relative one, and not to be confused with the absolute numbers of people on a landscape. It is unclear how it relates to the “hiatus” of the latter part of the 6th century, if that term any longer has meaning.

¹³⁷ Some Maya polities have multiple population peaks. One might expect a big Late Preclassic surge at Tikal, followed by an Early Classic decline associated with the “hiatus”, and then another peak, but our data do not support this demographic history.

¹³⁸ If one wished to cast this in cybernetic terms (Maruyama 1963), the demographic increase might be seen as the initial deviation-amplifying input that “kicked” the Classic Maya kingdoms into sufficient disequilibrium to precipitate the collapse. Interestingly, most of the deviation-amplifying examples offered by Maruyama were constructive or positive. He downplayed kicks that sparked downward or destructive developments. He also nicely anticipated niche construction and inheritance, although he didn't know our currently fashionable labels. For similar anticipation of niche construction see Robert McC. Adams (1960: 288-289).

¹³⁹ Kurjack (1974: 95) says that the Late Classic population was “overwhelmingly” larger than earlier ones.

¹⁴⁰ Haviland informs me that he excavated two structures (SE 159 and 322) located some 7.5 km out on the south survey strip that appeared to have brief Early Classic occupations. His general impression is that Manik phase occupations closer to the site core were rather impermanent compared to Late Classic ones. Haviland also recognized that not all Late Classic houses were contemporary, but as he made successive population estimates, he imagined that there was some point when, for all practical purposes, they could be considered so.

¹⁴¹ Cowgill then thought that Teotihuacan declined rapidly during the eighth century, aligning it well with the Maya collapse. We now know the decline occurred much earlier, probably beginning about AD 550. The old idea that the decline of Teotihuacan precipitated the Maya collapse now seems to have had it backwards. The Teotihuacan decline is coeval with a big demographic surge in the lowlands.

¹⁴² Zaro and Houk (2012), for example, enlarged the architectural sample at La Milpa and concluded that there was more pre-Late Classic activity than previously detected, thus diminishing the apparent spurt of population at that time.

¹⁴³ This event is much discussed in the literature and archaeologists still disagree about just what it represents (Tokovinine 2013 gives a good overview, as does Guenter 2002).

¹⁴⁴ Some regional ceramic sequences show signs of sizable intrusions, such as that during the Terminal Classic Xcocom phase at Becan (Ball 1977; 2014). I know of no similar evidence from Tikal. If migrants trickled in from somewhere as close as El Zotz, we might never detect any clear site-unit intrusion. With regard to out-migration from Tikal, I suppose it is just possible that some inhabitants of the Tikal polity relocated to Caracol after that center, in alliance with Calakmul, defeated Tikal in AD 652. In any event, Caracol seems to have a spurt of growth about that time, although terracing began on the landscape centuries before this event (Murtha 2015: 88).

¹⁴⁵ Just such intervention occurred around the sixteenth century in Hawai'i. Kings and nobles by fiat reorganized traditional patterns of land tenure, eliminated the old lineage/land-owning corporate system, and assumed royal/elite ownership and management (Kirch 2012: 221-225).

¹⁴⁶ In Giuseppe Tomasi di Lampedusa's novel *The Leopard*, a great Italian noble says: “If we want things to stay as they are, things will have to change.” I suspect Maya rulers saw it the other way around – the more things change, the more they must seem to stay the same.

¹⁴⁷ The institution of southern Maya kingship certainly changed from its late Preclassic origins up through the eighth century, but not, I suspect, in ways that made it more effective in dealing with ever-growing populations on deteriorating landscapes. One dimension of change, as many archaeologists have imagined, was probably from highly charismatic/shamanistic forms of leadership to more formal offices associated with distinct rules of succession. After AD 650, we also see much more direct evidence of other powerful nobles, many of whom were titled. Some scholars (e.g. Prudence Rice 2004) think another step might have been the emergence of collective rulership.

¹⁴⁸ To put this another way, the farmers who made up the bulk of the early population were richer in terms of productive potential and agrarian choices than their descendants during the apparent blossoming of Late Classic civilization. This is Hunt's point (see Appendix C) about the difficulty of linking agricultural intensification with agricultural development.

¹⁴⁹ The literature often overstates the area surveyed by the Sustaining Area project. Sabloff, for example, says that some 63 sq. km were sampled outside the Carr and Hazard map, but it was actually much less. See also Glassman and Anaya quoted above.

¹⁵⁰ The proportionate coverage at Tikal is much smaller if we factor in the 36% of the landscape covered by bajos within the various survey zones (including the Carr and Hazard map).

¹⁵¹ The topography of the Copan valley, with its well-defined alluvial pockets, foothills and tributary streams, makes the problem of spatial and territorial demarcation much simpler.

¹⁵² Except on the north, where it linked up with the old Uaxactun survey.

¹⁵³ To be fair, of course, largely because it was not obscured by heavy tropical forest.

¹⁵⁴ Many samples have been taken from short terrestrial cores and excavation profiles in various reservoirs at Tikal – see Lane, Scarborough, Dunning (2015) and Dunning *et al.* (2015). Some or all of the reservoirs were probably periodically cleaned, so the deposits are disturbed.

¹⁵⁵ This estimate is for the core population in the main drainage at AD 800. It does not include outlying centers such as Los Higos, El Paraiso and others (but certainly not by this time Quirigua) that might have fallen into Copan's larger political orbit (see Canuto and Bell 2013). Unfortunately, we have no reliable population estimates for the regions surrounding these sites. My current hunch is that they would add no more than a few thousand people to the eighth century kingdom, assuming that these centers were integral parts of it. My estimate of roughly 18,000 people for the Copan core kingdom of some 500 sq. km seems reasonably in line with 45,000-62,000 for the Tikal kingdom in a region of approximately the same size.

¹⁵⁶ Sanders and I (1988) estimated Copan's population at 18,000-20,000 people. In a later publication (Webster, Sanders, and Van Rossum 1992), we calculated the Late Classic *maximum* population as almost 28,000 people. Wingard's subsequent agrarian simulations (1993; 1996; 2013) suggested a lower number, and I agree with him. My more recent estimates revert to the original 18,000 to 20,000 persons range. A simulation by Wingard (2016) puts the peak the population at around 23,000 in the early ninth century, a close approximation of our original 1992 estimates.

¹⁵⁷ Populations peaked at different times in different regions, but the *overall* peak fell sometime in this interval. My map purposely excludes the extensive Tabasco plain, which Sanders (1962, 1963) included in his own demographic and agrarian overviews.

¹⁵⁸ And of course if one accepts high numbers such as suggested by Morley, Spinden and other Mayanists, the amount of construction at major centers becomes even more insignificant given the huge labor capacity.

¹⁵⁹ Cited by Haviland (1963: 510). Bullard presented these estimates in an unpublished 1962 paper.

¹⁶⁰ Although it is technically not in the central or southern Lowlands, I throw Coba into the mix because of its proposed scale as discussed above.

¹⁶¹ For example, a reasonable population estimate for the Palenque kingdom at its height is about 15,000 to 20,000 people (Liendo 2013). Some 28,000 lived in the recently surveyed region of 570 sq. km along the escarpment, which had eight sizable centers apart from Palenque, most notably Chinikiha, and probably multiple, autonomous political units (Liendo, López and Campiani 2014: 114-115). The overall density for the surveyed region works out to about 49 people per sq. km.

¹⁶² I bear in mind that we cannot easily assign many small centers in southern Campeche and in some parts of Belize and the northeast Peten to one or another major kingdom. Centers of considerable scale are still being located and mapped, especially in the Rio Bec/Chenes regions. Examples are Chactun, north of Becan (Šprajc 2013) and El Palmar in southeastern Campeche (Tsukamoto 2014).

¹⁶³ Haviland's low-end Tikal estimate of 45,000 people would reduce the total to 900,000. If most of these kingdoms had populations in the 45,000-60,000 range, they are even less impressive for their monumental constructions.

¹⁶⁴ In one of my early papers (Webster 1977: 342), I made various carrying capacity calculations for the Preclassic Maya and estimated an order of magnitude population for the Lowlands as a whole of 1,500,000 people. I now think this is a great overestimation, although the competition and warfare I predicted is increasingly documented and might have been exacerbated by the climatic fluctuations now evident.

¹⁶⁵ Although calculated in a different manner, my figures are remarkably close to those generated by Hester (1954) in his studies of Maya subsistence agriculture. He thought that the whole Lowlands, including Yucatan, probably had no more than 1,500,000 people in AD 800 (6 per sq. km overall density) who practiced short fallow cultivation supplemented by household gardens and orchards. Hester guessed that intensive agriculture of unspecified kinds might increase this number to 4,500,000, but his overall population densities (18 per sq. km) were still very low. Even more presciently, he said that a population of 1,500,000 people "... would more than suffice to account for all known and inferred construction of monumental architecture" (Hester 1954: 130).

¹⁶⁶ I do not mean to imply that Haviland failed to adjust for structures that were not domiciles *per se* – kitchens, shrines, etc. Quite the contrary, he was unusually sensitive to this issue.

¹⁶⁷ Conceptions of intensification during the 1960s and early 1970s did not yet envision the widespread use of landesque investments such as terracing or drained fields. Prudence Rice read an early draft of this paper and suggested that I mention food intake from hunting and gathering of wild resources. She tells me that remains of turtles, fish and snails are evident in excavated contexts in the Peten Lakes region, particularly in Colonial-era sites. Net-weights from pre-colonial sites might indicate that fish and turtle flesh were dried and moved inland; interestingly these weights are more infrequent in Classic and Postclassic times, a possible indication of overexploitation. Whether such resources played much of a dietary role at landlocked Late Classic Tikal is unknown. Terrestrial protein resources are another matter. Reina noted the importance of hunting associated with milpa cultivation, and the interdependence of the two in local perceptions: "Lack of corn means lack of meat" (1967: 17).

¹⁶⁸ This political conception of boundaries is heavily conditioned by natural factors. The core territory of the kings of Copan was in a sense naturally bounded by the rugged territory delimiting the alluvial bottomlands and associated foothills of the larger region, and also to a degree by the distribution of their monuments. The original attraction of the Tikal earthwork was that it, along with bajos, seemed to delimit territory where there were fewer natural constraints.

¹⁶⁹ And just maybe some kings at Tikal tried, however ineffectively, to delineate such a core by building earthworks at some unknown time, but probably prior to the Late Classic. Chronological insights from our project about when segments of the ditch were constructed are very feeble, but they suggest an episodic pattern. I am being generous with my "few hours" statement. Distances among some of the Petexbatun centers, which at times were autonomous, are much shorter.

¹⁷⁰ Admittedly, the western earthwork arm identified by our recent Tikal project could be considered a formal boundary, but it is far closer to Tikal than to El Zotz, the much smaller site of the two, and so probably is not a good marker for the kingdom's hinterland by itself.

¹⁷¹ 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. I already noted that Mutal lords of Tikal in the late seventh century seem to have envisioned themselves as central to a wider geopolitical order that included other prominent centers associated with various directions. What this meant in terms of actual political relations or the layout of the earthworks is unclear.

¹⁷² There is also the macro-level intimation of this pattern in Classic Maya geopolitics, in which dominant kings "possessed" lesser kings who sometimes lived considerable distances away.

¹⁷³ And, of course, there are the celebrated monuments of the 12th ruler that seem to demarcate the core of the kingdom.

¹⁷⁴ Martin (2016: 540) emphasizes disparities in the archaeological record and singles out the developments at El Mirador as one of the most puzzling and significant of these. He finds it paradoxical that scholars find the Middle and Late Preclassic Mirador region florescence as somehow "developmentally inferior" to the achievements of the Classic.

¹⁷⁵ In his classic ethnographic overview of Pacific cultures, Marshall Sahlins (1963) was careful to make the evolutionary distinction between the dependent followers of leaders, on the one hand, and their opportunistic political adherents on the other. How adherence versus dependence played out on Maya landscapes remains unclear.

¹⁷⁶ Neither the Copan nor the Piedras Negras landscapes, where I have also surveyed, have much potential for the intensive bajo cultivation envisioned by some archaeologists for the northeast Peten, and that is apparently impressive around Dzibanche.

¹⁷⁷ Tikal has low topographic relief, but Turner (1983) documented terrace-like constructions around Becan, where the topography is even flatter. Murtha is not likely to overlook such features, having written his dissertation the Caracol terraces. Eric Taladoire informs me (personal communication 2018) that surveys and excavations at Rio Bec documented many terrace-like constructions whose functions remain unclear. I have always thought that the terraces identified by Turner (1983) were indeed features that trapped eroding soil on hillsides, but that the soil surfaces behind them were much smaller in area than the original soil cover on the un-eroded hillsides. In this case, the terraces might not have raised productivity over original conditions, but stabilized it at some level (probably lower) over the long run.

¹⁷⁸ Golden *et al.* (2016) summarize initial archaeological implications of LiDAR aerial surveys of parts of southeastern Mexico conducted in 2013 by the NSA-Goddard Space Flight Center. These surveys consisted of some 610 spatial samples but were not done with archaeological targets in mind. Nevertheless, images of the central Yucatan uplands show high densities and distributions of terracing and field boundaries. Imagery that I have seen from transects flown over northeastern Yucatan shows extraordinary densities of small mounds surrounded by low walls. LiDAR surveys over Guatemala are few, and some not widely available, but Stephen Houston tells me (personal communication March 2017) that new imagery from around El Zotz reveals many landesque features, and Tikal is included in this coverage. Whether these are of Preclassic or Classic date is currently unknown. Copan has limited LiDAR coverage centered on the urban core and the surrounding Copan pocket (Von Schwerin *et al.* 2016).

¹⁷⁹ Stone terracing is common in several Maya regions, so what to make of its near-absence at Tikal is unclear. It is worth remembering that some farmers, such as the Chimbu of highland New Guinea, construct small but effective terraces entirely of perishable materials (Brookfield and Brown 1963: 45). We would have great difficulty in detecting such features on Maya landscapes.

¹⁸⁰ Although Alberto Ruz unearthed the first royal tomb between 1949 and 1952, non-calendrical inscriptions remained un-deciphered, and it was only well into the 1960s or even later that archaeologists broadly accepted the idea of kings and royal dynasties.

¹⁸¹ See Ford and Nigh (2015: 35–38) for a very broad overview of climatic and environmental history in the Maya Lowlands.

¹⁸² Lentz *et al.* (2014) use as proxies sediment core data from the Peten lakes some 20 km to the south of Tikal. To my knowledge, these data show no clear signs of major drought.

¹⁸³ One possible confounding factor in speleothem formation is that the fluctuations in water they measure might not only reflect climatic change, but other processes that affect percolation such as deforestation. See Ebert *et al.* (2017) for inferences from a wide range of proxies that might apply to Tikal, although the authors are careful to note that responses to drought are quite variable and local.

¹⁸⁴ What was different, clearly, was the denser Late Classic population and the intervening wear and tear on the landscape.

¹⁸⁵ This downturn in moisture, interestingly, seems to be similar to the levels recorded in the speleothem for the Late Preclassic between about AD 200–320. One could argue that this interval of early dry conditions contributed to the demise of El Mirador. On the other hand, Tikal seems to be doing just fine at this time, so obviously conditions for Maya agriculture were reasonably favorable.

¹⁸⁶ The Early Classic (prior to AD 500) seems to be particularly non-dynamic in terms of population growth at Tikal and elsewhere. In some places, such as the Petexbatun region, there appear to be declines during this time (O'Mansky and Dunning 2004). At others, such as Naachtun, there is a strong pulse of Early Classic population growth probably related to the decline of the Mirador upland polities (Nondédéo, Arnould and Michelet 2013).

¹⁸⁷ My colleagues Kirk French and Chris Duffy (2016: 62) applied sophisticated hydrological modeling to Palenque and are now similarly analyzing the Tikal landscape. They inform me that the biggest driver of surface water availability is deforestation. If this conclusion holds up, a counterintuitive inference is that runoff into streams and bajos, as well as percolation into the caves where speleothems form, might reflect human action as much as climate shifts. If so, isotopic or speleothem data might be giving us false signals.

¹⁸⁸ Modern traditional Maya farmers experience frequent episodes of crop failure due to insufficient rainfall and other threats. Karen Kramer (2005: 69) reports that the isolated northern Maya village of Xculoc suffers significant crop failure four to six years out of every ten. Yucatecan informants told Hester (1954:107) that they lost entire crops every twelve years or so to locusts or other pests, and that about once every seven years droughts destroyed half their annual crops. Admittedly, such information comes from a region that only gets about two-thirds of the annual rainfall found at Tikal. On the other hand, northern Yucatan's Classic and Postclassic populations seem to have weathered the purported mega-droughts better than their southern counterparts. And of course the modern people of Xculoc or Telchaquillo are buffered against the worst effects of crop failure in many ways.

¹⁸⁹ See Ford (2008) and Ford and Nigh (2014) for spirited critiques of pollen samples from sediment cores and the ways in which they might distort reconstructions of ancient forests.

¹⁹⁰ The most interesting lesson from this sequence is that we cannot conclude much from the absence of an anticipated pollen signal. This is particularly true of maize, which tends to drop its pollen very close to the parent plant.

¹⁹¹ There remains another intriguing possibility. An AMS radiocarbon sample (BETA 4135470) submitted by our current Tikal project yielded the following results:

Calibrated Result (95% Probability) **Cal AD 1050 to 1080 (Cal BP 900 to 870)**
 Cal AD 1150 to 1250 (Cal BP 800 to 700)

The sample was a piece of charcoal deep in a midden at Group 45, a large outlying site we tested at the western end of the northern earthwork (see Webster *et al.* 2004: 72 for a map). There is nothing surprising about the late date by itself, given that the Tikal ceramic sequence runs to at least AD 1200. More importantly, Kirk Straight analyzed the overlying ceramics in the midden and identified none later than AD 870. Three possibilities present themselves: 1) the date is incorrect; 2) the stratigraphy is disturbed; 3) some Imix ceramic complex types survived much longer than we imagine were missed in characterization of the later complexes.

¹⁹² I thus agree with Anabel Ford's (2008: 234) assertion that "... the impression of severe depopulation of the lowland Maya region after the Classic is likely an artifact of ... unrealistically high population estimates." We both agree that there were far more Maya after AD 800-900 than is commonly imagined. I disagree, however, with her rejection of anthropogenic, deleterious landscape changes caused by overpopulation as a major factor in the collapse.

¹⁹³ This is a very old idea, although our appreciation of the nature of the alterations has shifted. For example, Morley (1925: 64) opined that swidden burning created thick sod layers in which cereals could not grow. No one now sees this as a major form of niche construction. Another venerable idea (Shattuck 1933: 424) is that soil exhaustion was partly due to actual burning and deflation of humus in the soil. This happens when milpas are burned and when there are natural or accidental fires. At Piedras Negras, I saw escaped fires creep through standing forest and consume the soil in just this way (Webster 2014). I think this idea has real legs.

¹⁹⁴ As I noted at the beginning of this paper, there has been a recent vogue for resilience, revitalization and transformation as alternatives to simple models of collapse, and Mayanists now use such perspectives (Faulseit 2016). Why the central and southern Maya Lowlands failed to experience any kind of vigorous revival over the long run, either demographically or organizationally, has been a big issue for decades (see for example Ursula Cowgill 1962) and remains so. Archaeologists at one time envisioned the flowering of the "New Empire" of northern Yucatan as the major dimension of revitalization and transformation after the collapsed in the south: mobility promoted resilience. We now know that the northern part of the peninsula had its own very ancient population and local processes of indigenous social evolution.

¹⁹⁵ Sediments from the El Zotz Aguada (Beach *et al.* 2015: 270-274) indicate continued, if reduced, presence of farmers on the landscape during the Early Postclassic.

¹⁹⁶ Turner (1990: 312) envisioned a drop-off from the overall AD 800 peak of 145 people per sq. km to only 40-45 people by AD 1000, with continuing declines thereafter. I think his rate of decline is plausible but that the absolute numbers are too high.

¹⁹⁷ Pertinent to bajo cultivation is Greg Luna's PhD (2014) on the Aztec chinampas of the southern lakes in the Basin of Mexico. While chinampas for many reasons are not close analogues of Maya drained fields, there is one point that seems important to me. Although there were earlier chinampas, the growth of the big systems came after the mid- fifteenth century when Aztec imperial labor built flood-control infrastructure. The major purpose of the construction was to protect Tenochtitlan from flooding, but an ancillary effect was the capacity to regulate the water levels in the southern non-saline lakes. Chinampas for the first time were extended over much of these lakes because the sensitive relationship between planting surfaces and water level could be maintained. State chinampas were abandoned once the old Aztec dike system fell into disrepair after the Conquest because of uncontrolled flooding and the lakes reverted to swamp. The biggest problem for Tikal is that we have little or no evidence for drained fields in the local bajos. It strikes me, however, that if they were locally present, in the absence of large-scale top-down management they might have been very unpredictable in their agricultural potential given annual fluctuations of the bajo levels.

¹⁹⁸ See Somerville, Fauvelle and Froehle (2013). Interestingly, Maya commoner skeletons reveal little variation in diet over a 600 years period as reflected in isotopic values. Elites, while still heavily dependent on maize as the staple crop, had wider diet breadth, and the non-maize portion of their diet increased with time.

¹⁹⁹ There are important exceptions. One is Ceren (Sheets 2006), where household gardens and cultivation of maguey and manioc appear to be significant. Unfortunately, we have no isotopic insights into the diets of Ceren's inhabitants, so the roles of various staples are poorly understood. I think we have to be very careful in using Ceren as a model for Tikal as Lentz *et al.* (2014) do. Although there is no space to develop the topic here, retention of some forms of shifting cultivation over much of the interior southern Lowland landscape also helps to explain the wide range of terrestrial wild plant and animal resources that paleo-ecologists often identify in the Maya diet in the Petexbatun region and elsewhere (although the scale and proportional contributions of most of these, as opposed to maize, are difficult to determine). If much of the landscape were in some sort of managed fallow, as is the case among low-density traditional Maya farmers today, then reservoirs of such resources might have been available even in the eighth century, although quantitatively much diminished compared to their availability prior to the Late Classic. Documentation of diversity of resources tells us little about their respective caloric inputs. One reservoir of timber and other forest products for Tikal might have been the Mirador Uplands immediately to the northwest, just outside the northern and western earthwork and west of Uaxactun. This very rugged country with few water sources is poorly known archaeologically, and might not have been attractive to swidden farmers, at least in large numbers.

²⁰⁰ Many years ago, William Sanders and I (1978: 263) noted several logical outcomes of increased energy inputs in agrarian systems (and these might include landesque features like terracing). One outcome results in higher production per unit of land, and hence higher demographic potential, but at the cost of less favorable input/output labor ratios. More ominously, increased inputs might do no more than stabilize production or even be associated with declining yields (although yields would decline faster without the inputs). Sanders identified such processes as particularly characteristic of humid tropical regions where soil fertility is a problem. Whether agricultural intensification results in decreased or increased returns to inputs is a complex issue (Hunt 2000).

²⁰¹ One might add to the following list of conventional density concepts another one called *survey density*. This refers to some density measure – structures, population, etc. – derived from a segment of the landscape actually surveyed, such as the 16 sq. km Carr and Hazard Tikal map combined with Puleston's transects. Mayanists often extrapolate such survey densities, themselves sometimes suspect, to much larger regions, as Puleston did for Tikal and Kurjack did for Dzibilchaltun.

²⁰² Lentz *et al.* (2014) make heavy use of Ceren as a comparative agrarian model. The range of plants used at Ceren is probably a good guide to those available at Tikal, and there must have been similar dooryard gardens and orchards around Tikal's residential sites. By contrast, there is a major difference in soils. Ceren's rich, deep, volcanic-derived soils on a flat valley floor with permanent streams would probably have seemed remarkably productive and stable to a Tikal farmer.

²⁰³ Whether Rappaport's fieldwork accurately determined the actual numbers he reported is a separate question.

²⁰⁴ A common feature of many ancient civilizations (Sumerian, Egyptian, Aztec, Japanese, Khmer and many others) was the *temple-corporation*; that is, temples through royal fiat, pious donations, or other mechanisms became the custodians of landed estates. Mesopotamian scholars refer to such temples and other land-owning corporations as "institutional households". The Maya, despite the dominance of temple architecture, seem to have lacked any such institutions and their organizational correlates, professional priesthoods.

²⁰⁵ Carrying capacity (Brush 1975) is a suspect concept for many anthropologists and archaeologists, especially because humans through innovation readjust their relationships to resources. Consider, though, the null hypothesis: human population size is unrelated to essential resources – food being one of them. No one would take this seriously. My own opinion is that carrying capacity is useful precisely because it allows us to understand the stimuli and motivations for such adjustments. This appears to be Boserup's (1965) understanding of the concept. As I argued many years ago (Webster 1977: 338), carrying capacity should not be directly correlated with food supply because it is conditioned by many other factors. There is also a strong cultural component to human perceptions of carrying capacity. Probably few Classic Maya people found substituting root crops for maize a desirable choice. Conditions not controlled by humans, such as rainfall patterns, can lower or increase the capacity of landscapes to support agrarian populations.

²⁰⁶ An exception is Estrada-Belli (2011: 77) who gives volumetric measures for Middle and Late Preclassic structures at Cival, and who recognizes that small populations can create big buildings.

²⁰⁷ Part of the problem here is a preoccupation with the *meanings* of early E-groups, triadic groups or other architectural forms. But as Barbara Price (1980) insisted long ago, construction mass by itself conveys powerful meanings.

²⁰⁸ Mayanists might object that my comparisons don't take into account the greater efficiency of metal tools available to the Egyptians. The ones we know about are copper, and without bronze or iron, I don't really know how effective (and numerous) they were as opposed to stone. Every museum I've been to in Egypt and elsewhere exhibits lots of stone tools. Of course, details of construction raise all kinds of considerations. For example, most of the people who worked on big structures at Tikal

or Copan probably lived within a very short distance of the work site, so household economies subsidized their labor. Kings conscripted Egyptian workers from all over the Nile Valley and brought them to Giza, so the logistical demands were more like supporting an army in the field. Much of the cost involved supply of food and other necessities, and I think this probably more than offset the efficiency of limited amounts of copper tools.

²⁰⁹ More modest Teotihuacan estimates are the current tendency; see for example Evans (2016: 78) for one that agrees with Cowgill's low range.

²¹⁰ Conceivably Aztec imperial constructions exceeded those at Teotihuacan if we include the giant dyke, causeway, canal and raised fields they built throughout the 15th century.

²¹¹ Estrada-Belli (2016: 249) claims that the Danta Complex is 73 m high, and the Tigre Complex is 53 m. Why these figures differ from those of the Matheny's is unknown, but is characteristic of the varied estimates in the literature.

²¹² Foraging people practice various forms of intensification. Here I am only concerned with agriculturalists.

²¹³ Farming households, the basic social and economic components of traditional agrarian societies, typically (but not always) show a strong propensity to be located in close proximity to those parts of the landscape that are of optimal value for cultivation. This tendency is strengthened to the extent that permanent capital investments require increased labor inputs. Caracol's settlement conforms well to this principle, but Copan's does not. See Murtha (2015) for discussion.

²¹⁴ That small, low-density populations practicing long forest-fallow agriculture necessarily had only rudimentary occupational specialization or division of labor is a common assumption. See for example Boserup (1965: 70).

²¹⁵ One example might be the huge hydraulic system at Edzna, although we still don't understand well how it was used.

²¹⁶ Ruben Reina (1967: 6-7) notes that farmers around Lake Peten Itza sometimes plant one or two subsidiary or "emergency" crops each year. They do this reluctantly because multiple cropping is hard on the soil and is only feasible if there is sufficient labor (e.g., unmarried sons in the household).

²¹⁷ Boserup (1965: 62) correctly argues that long-fallow forest systems often have very high rates of return. She notes that technological regression is common in regions like Latin America, where people shift to more extensive strategies when environmental and political conditions allow.

²¹⁸ In its usual sense, a *landesque* feature is created for some sort of agrarian benefit, such as to increase, stabilize or diversify production, or perhaps to demarcate resources, as with field walls. Such investments must be maintained by frequent human effort, but they have distinct energetic payoffs. *Landesque* features such as temples, tombs, stelae, ball courts, roads, fortifications are by contrast called "sunk-cost investments". They have no corresponding immediate energetic returns, but rather payoffs such as social cohesion, connectedness, communication, protection, or shared identity.

²¹⁹ Modern contour hoeing can easily be mistaken on remote sensing images for more durable and ancient terracing.

²²⁰ This situation represents an inversion of the "tragedy of the commons", according to which individuals pursuing their own rational self-interests use shared resources in ways that degrade the well-being of the whole community. In the Maya case, individuals rationally (and possibly unwittingly) degraded their own long-term well-being by using resources held by others in destructive ways.

²²¹ Xie *et al.* (2015) argue that an important consideration in agricultural intensification and expansion is the capacity of tools – and particularly hand tools – to deal efficiently with particular soil textures. Their work in south-central China links the economic costs and energetic capacities of specialized farm implements made of cattle scapulae and polished stone to patterns of the spread of early agriculture. Ancient Maya farmers seem everywhere to have been able to supply their households with the necessary agricultural tools with minimum of effort. In fact, there is no obtrusive and widespread occurrence of essential agrarian tool-types across broad regions. Certainly there were soil types, such as the heavy clays of *bajos* that would have been very challenging for Maya farmers.

²²² The historical record is full of references about how surpluses harmed basic producers. Farmers who situationally produced large surpluses in societies with money economies and markets often suffered heavily when prices fell.

²²³ And where identified, the effects are poorly documented. For example, I have never seen experimental agrarian data showing how feasible drained field cultivation was, or whether it was unusually productive.

²²⁴ He adopted this perspective from earlier Mayanists. Sanders (1962: 88) remarked that tropical orchard crops are “... most in harmony with the ecology, since it simply means the replacement of a natural forest of limited food productivity with an artificial forest of great productivity.” An attraction of such tree cropping is the comparatively small amount of annual labor required, its impressive productivity (maybe), and the stability and resilience implied by cultivating a long-lived constituent of the “natural” forest ecosystem. Of course, there are also many risks, as early critics identified. One is the fluctuation in annual yields typical of many tree species, and a second is the competition with wild herbivores for which ramon was always an important food source. To these I might add the risk of destruction by human enemies. Mature perennial orchard species represent long-term capital investments – long-term both in reaching maturity and in annual yields. Maize crops are quickly reconstituted but orchards are not. They are also immovable, and dependence on orchards implies a high degree of residential stability. Cultivated arboreal species in many parts of the world have been tactical targets during warfare. This was the case in the Mediterranean and Near East with respect to olives and dates. Not for nothing did the old Greeks say “Better to lose your son than your olive trees; you can always get another son.”

²²⁵ Although I begin to suspect that by Late Classic times, long-term averages were more in the range of 600-700 kg per hectare. Haviland (1965: 23) asserted that in the mid-twentieth century “... one man, with some help from his family, can supply corn for about 12 people, freeing up have the population for other kinds of activity.” He apparently got this figure from Ursula Cowgill (1967: 277). Haviland uncritically projected this productive capacity back to the Classic Maya, allowing him to envision a very high proportion of non-farming economic specialists at Tikal. Several Maya ethnographic studies have found that per-capita consumption of maize in traditional communities was about 160 to 224 kg annually (see Ursula Cowgill [1967: 277] and Hester [1954: 105-106] for a review). Given losses in storage or to pests and extra-household obligations, per capita production must obviously have been higher. Haviland’s enthusiastic figures imply that a single producer, with a little help, predictably produced some 2700 kg of maize each year by his own efforts.

²²⁶ Beans, squash, tomatoes, and other crops are routinely intermixed mixed with maize, but none of these ranks with maize as a staple. Interestingly, however, traditional Lacandon farmers sometimes inter-mix both manioc and sweet potatoes with maize (Shattuck 1933: 197). Recent paleoethnobotanical studies show surprising discontinuities in plant utilization, including staples, even over short distances. For example, there is no evidence for manioc and sweet potatoes at Copan, although both crops were use a bit further to the east in the Ulua Valley (Shanti Morell-Hart 2016).

²²⁷ Maize has fallen in and out of fashion as a Maya staple. Contributions to the 1978 Maya subsistence volume edited by Harrison and Turner championed alternative staples, as did MacKinnon (1981) in his residential spatial analysis of Tikal. One possibility, the sweet potato, has been dated to the seventh century BC at Tikal (Lentz *et al.* 2014: 3), but does not seem to have gained much traction there or elsewhere.

²²⁸ This assumption is not universally relevant. There were sufficient outlying, low-density regions on the margins of polities like Copan so that people did in fact have this option.

²²⁹ I don’t think the Classic Maya had well-developed markets, or that polities were enmeshed in large market systems comparable to those found in Central Mexico. Many Mayanists disagree. The issue with regard to trade in bulk staples is *incentive*, a word beloved by economists. If the Maya had all-purpose currency of the kind found in the Old World, it is easy to discern the incentive. If prices of maize varied significantly over reasonable distances, farmers (or middlemen) had the incentive to move maize to take advantage of the differential prices, even assuming some transit losses. They could return home with either light, valuable loads or with standard currency that could buy other products. This still supposes that people in the zones where maize was in short supply had the wherewithal to pay for the staples they required. Incentives are quite different in political transfers, as in gifts between lords or movement of goods from elite estates.

²³⁰ For example, Turner (1978: 181) argued that water transport on canals and bajos might supply the landlocked Tikal region with basic staples from the Rio Hondo Valley some 60-70 km distant. This still doesn’t resolve the very high political and labor costs at the producing end of the supply chain.

²³¹ Some ethnohistorians argue that the Late Postclassic Maya of Yucatan had a highly commercialized economy, and at least a partially monetized one. Landa, for example, says that maize farmers produced household surpluses and stored them in order to take advantage of shifting market prices (see Kepecs 2004: 260 for a discussion). I suspect this idea is a projection by Landa based on European agrarian practices. Of course the Maya might have swiftly adopted such behavior after the conquest and Landa might have seen this new pattern.

²³² Most pre-modern complex societies had small markets where people could regularly exchange foodstuffs or other local products, particularly those used by ordinary householders. This is a far cry from the much larger, formal market systems that operated, say, in Central Mexico. I have never seen a reference to markets on any scale for pre-European Hawai’i, and they seem to have been small-scale affairs in Old Kingdom Egypt and among the Inka. Even such small local markets seem to be absent in some parts of the world, such as pre-contact New Guinea.

²³³ Some of my colleagues refer to the “incomes” of ancient Maya farming households. If income simply means resources that accrued to households as a result of their economic activities, then the usage makes a certain sense. Unfortunately, some archaeologists seem to view “income” almost as we do today in terms of monetized resources. A modern household has wide choice in how to deploy assets in the form of all-purpose currency. An ancient Maya household possessed of a large surplus of maize or other basic staples did not.

²³⁴ Uncoordinated defection of individuals or household groups from a grain-depleted region to adjacent ones with more food probably occurred under Scenario 2, but given small surpluses, migrants would have caused severe problems.

²³⁵ Prudence Rice (2004: 259-265) argues that a major motivation for Late Classic Maya warfare was not political supremacy or material gain in the normal sense, but rather acquisition of dominant “seats” in a complex calendrical (may) order of great cosmic significance that functioned on a supra-kingdom basis.

²³⁶ As so often happens in traditional agrarian societies, people could sell their labor or even themselves in times of trouble, but the larger agrarian system still somehow had to support them.

²³⁷ Shifting residence and allegiance from one chief to another was about the only option available to Hawai’ian farmers, who otherwise lived on landscapes totally “owned” by great nobles and kings.

²³⁸ Curiously, the Tikal epigraphic record is practically devoid of the sub-royal elite titles (*ajk’uhuun*, *sajal*, etc.) so common at Copan and in the Usumacinta-Pasion region (see Jackson 2013).

²³⁹ The lid of the Palenque sarcophagus shows Pakal, personified as the Maize God, entering the Underworld as a prelude to his solar apotheosis (Chinchilla 2006: 54).

²⁴⁰ See Brook (2010: 73) for a related concept: “moral meteorology” as it played out in traditional China. According to Confucian political theory, the emperor was morally responsible for all “natural” phenomena.

²⁴¹ See Golden and Scherer (2013) for a similar take on the rise and fall of Maya polities.

²⁴² Morgan (1963: 547-548) thought Spanish claims of private ownership of land and estates by “Mexican” elites were wrong. Like others of his time, he asserted that ownership was instead communal. I have often thought that in many parts of the Old World an early and very important stimulus to concepts of ownership was the presence of domestic animals. Pastoral or herding people I know about, including the Kurdish villagers I worked with who had a large component of domestic animals in their household economies, usually know who owns what right down to the individual sheep or goat.

²⁴³ I say “practices” here instead of “rules”, mindful of Watanabe’s (2004: 162) discussion of the interplay between both.

²⁴⁴ Here she echoes the much earlier suggestion by Evon Vogt (1969: 592) that the basic unit of ancient Maya social organization was the localized patrilineage, an analogy drawn from his ethnographic studies of the highland Maya.

²⁴⁵ Quezada’s analysis of the extremely fragmented political landscape of northern Yucatan following earlier cycles of semi-centralization is similar to the earlier reconstructions of Joyce Marcus (1993).

²⁴⁶ Reina’s description of modern milpero decision making thus suggests that even under the low population densities of most regions in Preclassic times, there might have been competition for preferred parts of the landscape.

²⁴⁷ Many archaeologists believe that the early fifth century “founder” of the Copan royal dynasty and his entourage were ethnic Maya who came from somewhere in the northeastern Peten and established dominance over an indigenous non-Maya (probably Lenca-speaking) local population. If this is so, Copan from early times might have developed very different practices of land holding than the much older Tikal.

²⁴⁸ In both medieval Europe and traditional China, one big problem for peasant farmers was that inheritance over time broke up big landholdings into small, scattered parcels. A farmer might have enough land in aggregate to support his household, but travel costs between parcels were very high.

²⁴⁹ I have dealt with burial samples from many Maya households, and my back-of-the-envelope calculations indicate that only small percentages of household members who died wound up buried in and around their domestic precincts. Where are the rest, and what conclusions do we draw from this pattern of partial mortuary inclusion?

²⁵⁰ For example, many anthropologists have noted that impartible inheritance of land (and wealth more generally) favors lineage organization, while partible inheritance favors nuclear family organization. Some ancient elites dictated inheritance patterns for larger political purposes. Fairbank and Goldman (2006: 21) note the long tradition of state-enforced partible inheritance that helped to maintain centralized elite dominance in China. Different kinds of lands “possessed” by Aztec kings and nobles fell under various rules of inheritance, and free Aztec commoners generally held usufruct rights to communal lands.

²⁵¹ I use this term advisedly, mindful of the many kinds of statuses that our concept of “slave” tends to obscure (see Finley 1973: 62-94 for commentary).

²⁵² Boserup (1965: 73) remarked that “It is a fair generalization to say that all the ancient communities which applied intensive systems of land utilization used servile labor, usually captives of war and descendants of such captives. These were the men and women who performed the toil of investment as well as much of the current work in agriculture”. She thought that one of the principal causes of warfare in such societies was capture of labor. Following this logic, if the Classic Maya had intensive agriculture there must have been a large component of slaves (although there are other conditions of relative servitude). Boserup was a good economic geographer, but had a real penchant for making suspect generalizations like this one about ancient societies.

²⁵³ We don’t know how much time, if any, Contreras spent supervising his estates, nor even if he routinely visited them at all. Interestingly, the proprietorship of *encomiendas* in Yucatan at the time seems to have been flexible and shifting (Roys 1940: 11). The first generation of *encomenderos*, including Montejo himself, enjoyed their privileges only briefly. The Spanish crown intervened and abolished the requirement of service from the Indians in 1549, making the new arrangement much less like the old Pre-Columbian one (Quezada 2014: 45).

²⁵⁴ James Lockhart (1992:141-202) argued strongly for various patterns of private landholdings in Central Mexico and thought that the Nahuas in this respect converged with European practices (see his Table 5.3 for indigenous land tenure categories). In his otherwise detailed analyses, he only briefly mentioned dowries or bride price. Dramatic descriptions of Aztec gambling practices by Sahagun (1979: 29) and Duran (1971: 301-311) intimate individual ownership. Inveterate gamblers would wager cloaks, feathers, jewelry and many other kinds of personal property on the outcome of games, including ball games. More importantly, wagers might extend to capital resources such as slaves, and even to houses, fields and whole estates. The speed with which native elites learned to use the Spanish courts for land titles also suggests a strong Precolumbian tradition of land ownership.

²⁵⁵ An extreme pattern of such “archipelago” estates was highly developed in Japan during the Heian period (AD 794- 1183). Absentee landlords were court nobles who hardly ever left the capital of Kyoto, but who depended upon widely scattered estates for goods and services. Ottoman elites had similar entitlements; while living in Istanbul or other cities, they drew rents from distant villages. Such privileges proved very tenacious, and sometimes survived the land reforms of Atatürk. When I worked in Turkey in the 1960s and 70s, Prof. Halet Çambel told me the remarkable story of a wealthy city-dweller who visited his family’s old attached village many years after such payments had been abolished, only to find that the villagers had each year put away their rents just in case one of their old landlords showed up.

²⁵⁶ As I re-read Ester Boserup’s book, it struck me how central to it were all kinds of agency, although in 1965 she did not use that word. She mainly analyzed agency (as we would call it) in the collective rather than the individual sense, although she tried to determine the conditions under which people might make individual choices.

²⁵⁷ One exception, as I have argued above and elsewhere (Webster 2005), is that there must have been negotiations over entitlements to land. I also think that there were critical junctures in Maya culture history when various kinds of identity had to be re-configured, such as during the Late Preclassic breakup of the Mirador Upland polities and the more general decline of the eighth and ninth centuries.

²⁵⁸ In modern parlance, they were not very “politically aware.” Trigger (1993: 53) characterized early civilizations as “... societies in which inequality was accepted as a normal condition and injustice viewed as a personal rather than systemic evil.”

²⁵⁹ If some sort of shifting cultivation still operated during the Late Classic, the Maya, at least on the peasant farming level, stand out markedly from farmers in the other civilizations considered here. Where the state dominated intensive agrarian systems, such as in Hawai’i, Sumer, and Egypt, some sorts of top-down field demarcation, allocation and transmission are easy to envision. If Late Classic Maya kings and lords, like those in Hawai’i, asserted direct ownership of productive resources, then ordinary people had very little managerial say. Of course, all this would have been very different under the much smaller demographic regimes of Middle and Late Preclassic times, when commoner lineage organization might have been very important.

²⁶⁰ Ideological management is another matter. Tokovinine (2013: 122) says that the striking “... connection of royal families to various maize gods effectively identifies these families as the sources of group sustenance.”

²⁶¹ This gets us to the larger issue of Classic Maya politics and competition. An ecological definition says that competition is the active demand of two or more organisms for scarce but essential resources. If conflicting interests stimulate political action, then there were many disparate interest groups in Late Classic Maya society. I have long thought that what caused some of the most volatile competition and its attendant political consequences were commoner concerns with secure access to land.

²⁶² I especially like Renger’s insistence on the economic primacy of agriculture. Mayanists are often so fascinated by archaeological evidence for specialization, exchange and trade (to all appearances much more feeble than in Mesopotamia) that they downplay the underlying, all-important, agrarian underpinnings.

²⁶³ This view continued into the 1960s. For example, Sanders (1963: 236) echoes Wolf in saying that “... there were few integrative factors operating in ancient Maya society toward the formation of large socio-political groupings, and many disruptive ones.” See also Michael Coe (1961).

²⁶⁴ My late colleague William Sanders, who is sometimes accused of being ignorant or dismissive of inscriptions and iconography, said that if he could be remembered for having written a single article on Maya archaeology, he would like it to be Proskouriakoff’s famous 1960 one.

²⁶⁵ Many historians think the concept of state is too Eurocentric in its origins for broad comparative application (see, for example, the excellent analysis of this contested issue by Timothy Brook [2005: 183-190]).

²⁶⁶ Although one might regard the great (but inconclusive) struggles of the sixth and seventh centuries, when the lords of Calakmul tried to assert some kind of hegemonic dominance, as politically more dynamic and interesting.

²⁶⁷ And by saying it this way I obscure the obvious – that even contemporary Late Classic Maya polities had their own distinctive internal arrangements.

²⁶⁸ He had in mind here various African kingdoms, and probably other societies such as traditional Hawai’i and Japan. Sanders (1963: 276-277) early on espoused a definition of civilization that included 1) “successful” adaptations to environments that support relatively dense populations; 2) people organized into a single political/social system over a large region; and 3) the presence of at least two classes, an “inferior” one that produces food surpluses and an “directive” one that channels their use in culturally specific ways. Sanders departed from his earlier skepticism about the integrative, centralizing processes at work in the Maya Lowlands by stressing bureaucratic, state-like Late Classic systems.

²⁶⁹ Here Flannery harks back to earlier characterizations by Alfred Kroeber (1953: 275): “To the historian, civilizations are large, somewhat vague segments of the totality of historical events which it may sometimes be convenient or useful to segregate off from the remainder of history, and which tend to evince certain dubiously definable qualities when so segregated. To the student of culture, civilizations are segments of the totality of human history when this is viewed less for its events, and less as behavior and acts, than as the more enduring products, forms, and influences of the actions of human societies. To the student of culture, civilizations are segregated or delimited from one another by no single criterion: partly by geography, partly by period; partly by speech, religion, government, less by technology; most of all, by those activities of civilization that are especially concerned with values and the manifest qualities of style.” I would amend Kroeber’s comment by upgrading the importance of technological and energy source differences if our comparisons are Old World and New World civilizations.

²⁷⁰ Flannery presumably means here some variety of shifting cultivation of the kind that Childe (1950: 6) singled out as the default agrarian strategy of “Neolithic” societies. Flannery was well aware, however, of the evidence for more intensive/landesque agricultural features and practices that were becoming apparent on Maya landscapes. Perhaps he had in mind that essentially tribal peoples in various parts of the world built and maintained impressive systems of drained and ditched fields, terraces, irrigation systems etc., as we know from ethnographic sources.

²⁷¹ Several organizational syndromes (and a weak evolutionary progression) are found among court-centered societies: 1) people around the ruler are relatives, favorites, attendants, retainers and dependents (e.g., Hawai’i); 2) all these are present, plus great semi-autonomous lords and titled officials who form the kernel of a professional bureaucracy (e.g., various medieval European societies and Japan after about AD 650); and 3) the personal entourage at the ruler’s court proper (but perhaps not great lords) as well as a professional bureaucracy (e.g., China from Tang times on). In the latter case, bureaucrats were servants of the ruler and thus an extension of the dynastic household, but they also served the long-term interests of the state that

superseded individual reigns or even dynasties. Demographic and spatial scale obviously condition these syndromes. In a Maya kingdom, almost everyone of importance could potentially be in attendance on the king and be personally known to him. This was impossible in traditional China, where even many high officials never saw the emperor.

²⁷² Haviland (personal communication Oct. 2014) tells me that “... regardless of the actual number of people living there, I think by Late Classic times Tikal came closer to being a true state than most other places in the Maya lowlands.” How close is the issue. Most recently, he thinks that the Tikal polity became a “genuine state” at least by AD 692 under the strong monarch Hasaw Chan K’awill, who presided over a central marketplace located at TR 16. For Haviland, the market’s close proximity to the royal court implies royal oversight (personal communication 2016). He believes that some kind of market system was well-established at Tikal as early as 350 BC, and that it was one of the “pulls” that account for the growth of the center (Haviland 2008: 279).

²⁷³ At one time, I expressed myself differently about Maya statehood. Both Sanders and I argued strongly for Late Classic Maya state organization in 1978 (Sanders and Webster 1978: 291). A decade later, we championed the regal- ritual model of Richard Fox (Sanders and Webster 1988). We followed Fox in calling such centers “cities” and by linking them to weak or segmentary states, using Copan as an example. Our eventual ambivalence about the character of the Copan kingdom comes through if you watch the *Out of the Past* telecourse program *Power, Prestige, and Wealth*. Both Sanders and I, academic directors of the project, send mixed signals in the narration about the centralization of wealth and power in Late Classic Copan. I would write the script very differently today. More specifically, I would not use the terms “state” or “city” with regard to Copan or other Maya polities. I find Redfield’s distinction between the “Great” and “Little” Traditions in complex societies very elegant. He probably got this idea from ethnographers in India. For a lengthy application of these concepts in a book recently banned throughout that entire subcontinent, see Doniger (2009).

²⁷⁴ Given the obvious variability in Classic Maya political forms and my own doubts about Maya statehood, I have long used the neutral label “polity” to refer to their political jurisdictions. I also use the term “kingdom” on commonsense grounds. We routinely apply the title “king” to Maya rulers, and where there are kings there must be kingdoms. In Old English, however, the word king did not connote rulership of a state, but rather had the Anglo-Saxon sense of the leader of a local kin group or clan, so there is no necessary state or strict territorial implication.

²⁷⁵ Part of the problem is semantic. Regardless of when they ruled or the size of their polities, we tend to call Maya rulers “kings”. Using such a common label obscures the fact that an early ruler of, say, Tikal, who had 5000 local subjects, played a very different kind of leadership role than one of his successors in the 8th century. To give another comparison, Mary Beard (2015: 490) thinks that about 200 elite decision-makers administered the mature Roman empire of some 54 million people. Even in the second century AD, the Romans had a surprisingly ad-hoc approach to civil administration that varied greatly from region to region. Romans preferred to rule through the grass-roots administrative apparatus of the communities they dominated, and by co-opting local elites. Roman governors seldom served for long and relied on cadres of assistants that included many household slaves. Even functions as essential as tax collection were sold off to private contractors during early stages of expansion.

²⁷⁶ Although it is unclear whether even the most powerful local rulers could effectively interfere with usufruct rights of commoners to farmland, especially under conditions of very low population densities as Quezada (2014) has argued. I call them minimally stratified for this reason. Conditions under the earlier hegemonies of Uxmal, Chichen Itza and Mayapan might have been very different. And of course, the Postclassic practice of slavery added an element of stratification that might not have existed during Classic times, although I suspect that slaves were present then.

²⁷⁷ I have long thought that Fried overstated his point here, and that history provides us with many such examples. Having worked in the Near East and taught Near Eastern archaeology for many years, I suspect that many polities or communities there were stratified, but not very state-like, between about 6000-4000 BC. Medieval Iceland is another example, as was Viking society in general before about AD 1000.

²⁷⁸ Unless one views the royal dynasty itself as a separate class. I personally doubt that any Maya polity ever had a well-developed class system. Like most anthropologists, I employ a more expansive definition of class than used by Marx and his followers, for whom classes were determined by “... the division of labor and the accumulation of capital...” (Berlin 1909: 249).

²⁷⁹ Many years ago, Michael Coe (1965: 98) pointed out that early anthropologists such as Morgan and Maine envisioned mature states as polities in which territorial loyalties and identities had replaced kinship identities. This was a bad idea then and still is. Coe thought that Mayanists had overlooked “... the primitive state in which political power has been constructed out of already-existing and far older social units based on kinship”. Most Mayanists would probably agree with one or another kind of organizational structure such as Coe suggests, whether or not we wish to use the state label.

²⁸⁰ Before inscriptions could be effectively read, George Cowgill (1976: 61) argued that new economic stresses and opportunities led increasingly centralized big Maya polities to attempt incorporation of many lesser ones into small imperial systems. Tatiana Proskouriakoff, Joyce Marcus, Clemency Coggins and others also discerned signs of such political expansion. Martin's and Grube's classic 1995 article originally called such hegemonies "superstates", but the authors quickly decided that "superpower" was a better label because it does not imply widespread central administration that cross-cuts the boundaries of local kingdoms.

²⁸¹ Much of this section derives from the symposium *Understanding Classic Maya Hegemonic Networks through Textual-Material Synergies: The Case of the Snake Kingdom*, organized by Simon Martin and Marcello Canuto for the 2016 Society for American Archaeology Meetings, Orlando. Guenter (2002) reviews Tikal's involvement in these hegemonic struggles, and Vance (2016) also provides a useful if very general summary. Martin (2017) gives a new interpretation of the Snake Dynasty.

²⁸² Some archaeologists think that there were Late Preclassic hegemonies centered on El Mirador and Cival. Others doubt this interpretation, particularly in the absence of supporting texts. Much of the discussion of hegemonies focuses on Teotihuacan's interactions with the Maya, which seems to have had two distinct pulses between AD 300 and AD 450.

²⁸³ Mayanists have not been strongly shackled by standard, generalized political models. For a long time, they championed models that were highly unique and that emphasized Maya exceptionalism. A recent example is that of Prudence Rice (2004), whose work provides much more detail and political scale to suggestions made long ago by Michael Coe (1965). During the 1960s and early 1970s, there was considerable enthusiasm for an indigenous "phylogenetic" model of Classic Maya political organization stimulated by the ethnographic research of Evon Vogt (1963, 1969) and his colleagues among the highland Tzotzil Maya. This modified cargo model faded quickly as the dynastic import of inscriptions became ever more apparent. Some archaeologists tried to accommodate the Vogt model with the obvious stratification present in ancient Maya polities, but without much success. Oddly, the modified cargo model seems consistent with the continuous markers of status as seen in burials at sites like Copan.

²⁸⁴ Robert McC. Adams, in an influential and ambitious comparison of four early civilizations, envisioned "bureaucratization" as one of the shared evolutionary developments (Adams 1960: 271).

²⁸⁵ Many years ago, William Sanders and I (1988: 524) observed that "... since so much of the anthropological literature on early state development has stressed the emergence of formal bureaucracies, it is worth remembering that the less formal court dimensions of centralization may also be extremely important."

²⁸⁶ Cedric Puleston is Dennis Puleston's son.

²⁸⁷ Moholy-Nagy (2016: 260) sees no evidence that Maya kings were interested in managing "quotidian" matters. She envisions theater-states in which kings were primarily preoccupied with rituals and displays that reinforced their ideological significance while sub-royal bureaucrats managed state affairs. Unfortunately these sub-royal notables and their offices are virtually invisible in the Tikal epigraphic record.

²⁸⁸ China is famous for an examination system that matured in late Tang and Song times and funneled scholars into state offices. Although people of very low rank sometimes were successful, it was not a real meritocracy because the exams heavily advantaged individuals with high social prestige and those from families of rank (see Reischauer and Fairbank 1960: 164-167). Wealthy individuals could also buy degrees of low rank that conferred titles and prestige even though they had not passed the exams at suitable levels. This prestige spilled over to their relatives, helping to produce the landlord-gentry families who were so essential in local decision-making and social order. In China, as elsewhere, bureaucracies often consisted of internal sub-institutions that competed vigorously with one another. Thus eunuchs of the Chinese court were often in conflict with the civil bureaucrats selected by the examination system.

²⁸⁹ Carnegie Institution medical surveys of Maya communities in northern Yucatan between 1929 and 1931 showed that the single biggest causes of illness and mortality were intestinal afflictions (Shattuck 1933). The communities sampled exhibited high fertility, high mortality, and comparatively short life expectancy. No one as far as I know has speculated about the deleterious health effects on Classic populations that relied on concentrated communal water reservoirs for several months of the year.

²⁹⁰ And of course this leaves aside how people of lesser social rank were organized. "Centralized" here does not imply highly autocratic or even individualized modes of governance.

²⁹¹ Remember, though, that scholars like MacKinnon (1981) reserved considerable independent agency to farmers and discounted heavy-handed elite management.

²⁹² Haviland (2014b: 146-147) envisioned a plateau of high population at Tikal beginning at about AD 550. He believed his skeletal studies showed “undernutrition” caused by problems in food production during the following 300 years. While he did not ascribe the Maya collapse to these agricultural conditions, he thought that “... should food production have been adversely affected by something else, a crisis may have resulted that was far more serious had there not already been a problem with undernutrition.” Today he might see drought as a possible contributing cause. Drought has been linked to the collapse for a very long time – see Michael Coe (1963: 39) for an early example.

²⁹³ An exception, as I argued elsewhere (Webster 2005), is that kings and other elites probably tried to manage increasing disputes over access to agrarian resources by the eighth century. I understand that my interpretation of the Maya does not easily fit into the evolutionary categories we commonly use and so opens me to the same charge of Maya exceptionalism that I decried at the beginning of the paper.

²⁹⁴ Although I agree with Moholy-Nagy that Maya kings were largely “hands-off” in day-to-day management of their polities, it is only fair to mention one dimension of demographic scale that favors centralized monitoring and management. Many Maya kingdoms had such small and concentrated core populations that it is easy to envision effective “panopticon”-like political scrutiny. At Copan, Piedras Negras and Palenque, the vast bulk of the population, including most nobles, resided almost literally in the view-field of the royal palace.

²⁹⁵ Here Livi-Bacchi compares two perspectives that seem at odds, but both that agree population pressure and growth, for better or worse, are important drivers of human behavior, culture change and cultural evolution. Some archaeologists take a third position, especially those with Marxist proclivities, and dismiss either argument. A good example is Bruce Smith, whose vast analysis of early civilizations I have long admired. In summarizing various theories of cultural evolution he says there is not much support for the “... suggestion that population pressure was the main factor in promoting the development of early civilizations... population growth is not an independent variable, but is influenced by numerous economic, social, and cultural factors.” (Smith 1997: 676). His comment echoes the position of George Cowgill quoted earlier. Smith mentions Boserup in his book only once and in a rather dismissive manner, but never mentions Malthus at all, which is like referring to Laurel but not Hardy.

²⁹⁶ Here I restrict my remarks to Boserup’s original 1965 formulation, not her later versions.

²⁹⁷ See Sanders (1979) for a highly critical review of this volume.

²⁹⁸ Boserup repeatedly uses the concepts of “traditional” or “tribal societies”, and is most concerned with the potential for agricultural intensification among such people under modern conditions. Her examples derive heavily from the Old World tropics, particularly Asia and Africa, as well as from temperate Europe. The New World tropical societies she occasionally mentions are usually no longer traditional in the strict sense of the word. Read Boserup’s little story of agrarian evolution on pp. 58-59 of her book and try to accommodate it to the Classic Maya.

²⁹⁹ The much later claims of mega-drought were welcomed by some devoted to the tropical ecologist perspective because drought absolved the Maya of anthropogenic complicity in the collapse (e.g., Ford 2008). Clever and accomplished as the Maya were, so this line of thinking goes, the big drought was a catastrophe beyond their control. Lentz *et al.* (2014) take a somewhat ambivalent middle ground. They see the Maya as generally canny ecosystem managers until the ninth century, when high population densities rendered them vulnerable to droughts of unusual scale. My own opinion is that, regardless of their skills as agrarian managers, generally low population densities for centuries prior to the Late Classic enabled the Maya to rebound from droughts and other uncontrollable risks (Webster 2014: 338-339). The conjunction of long-term environment degradation, high populations, and drought finally disrupted their political systems. Both my formulation and that of Lentz *et al.* incorporate population pressure as a major explanatory component.

³⁰⁰ Sometimes the imbalance is attributed to population growth, density and their anthropogenic effects, and sometimes to other factors such as major drought, competition and warfare, etc.

³⁰¹ Winterhalder and Puleston (n.d.) recently proposed another Malthusian perspective involving ancient complex societies, including the Classic Maya, as have Hogarth *et al.* (2017).

³⁰² Malthus and his contemporaries lived in societies that were still heavily agrarian and powered by human and animal labor. Today we might say more generally that the limiting factor for Malthus was energy. He could not foresee the impending shift to fossil fuels that created enormous and very cheap energy surpluses.

³⁰³ See Abrams (1995) for a very thoughtful application of Boserup’s ideas centered on the interplay among intensification, population growth, and labor value throughout Maya history.

³⁰⁴ See Pye (2014: Chapter 7) for a good account.

³⁰⁵ One issue that Boserup does not handle very well is the *rate* of population growth. For example, while giving no numbers, she says that it took a whole century for Europe's population to recover from the Thirty Year's War (Boserup 1965: 62). She seems to think that such growth is characteristically slow. I think it sounds pretty fast. Malthus paid much attention to population growth rates. The obvious point is that growth might be so rapid as to overwhelm the processes of innovation that Boserup champions.

³⁰⁶ During later Ming times there was a population explosion brought on partly by the introduction of New World crops such as maize and potatoes. The official Hong Liangji noted that while there would always be a surplus of people, land would always be in short supply. The resulting imbalance was controlled as floods, drought, and plagues took their toll. His perspective eerily anticipated that of Malthus by five years (Mann 2011: 177-178). Shades of Wallace and Darwin.

³⁰⁷ Part of the problem is that even people who read Malthus tend to stick to his First Edition (1798) presentation of the "principal of population". Malthus himself recognized many deficiencies in his original argument, which was a rather raw polemic – what one might call enthusiastic political philosophy. He followed up in 1803 with a well-researched Second Edition much more in the vein of analytical political economy. Right up until 1826, however, he made numerous alterations to his work in response to critics and commentators. The edition I cite in the bibliography is the Second Edition, to which the compiler has attached most of the later emendations made by Malthus up through the end of his life. Another problem, admittedly, is plowing through Malthus's cumbersome Regency-era prose. Boserup is much shorter and more accessible.

³⁰⁸ And his idea was later appropriated and used in destructive ways, much as natural selection was by the Social Darwinists.

³⁰⁹ At Cambridge University, Darwin aspired to become a priest in the footsteps of Rev. William Paley, the famous champion of Biblical creationism.

³¹⁰ From *Portraits from Greece as it Endures a Crisis*. *New York Times*, p. B4, June 24, 2015.

³¹¹ We must do this when reading Boserup too, who after all was a twentieth-century developmental economist and geographer. Her book is filled with concepts such as financing, income, price, wage labor, savings, money, employment, unemployment, export crops, etc., that make little sense when deployed to analyze most premodern societies and economies, particularly the Maya. Boserup frequently generalized about ancient societies, but her application of anthropological perspectives, whether applied to the past or present, is often fuzzy. I would like, for example, to see her unpack her oft-used concept of "tribal chief".

³¹² And filling in this deficiency if we want to apply big ideas about the human condition is one of the best arguments I know for practicing archaeology and other "historical" disciplines.

³¹³ Marx was famously comparative in his analysis of modes of production and property, as he had to be as an apostle of a universal historical process. He made many references to ancient Rome and Greece, to the "Germanic" peoples of northern Europe, and to Asian societies such as Egypt, India, and Japan. He devoted much thought to China, and popularized the concept of "Oriental despotism" that Wittfogel later found so alluring. What I mean here is that Marx lacked the detailed knowledge built up after his time by ethnographers, historians, ethnohistorians, and archaeologists about ancient and non-Western societal forms, particularly those on pre-state levels. Marvin Harris (1968: 227-229) provides pertinent commentary. Nor did Malthus or Marx have the benefit of modern comparative demographic research that links reproductive decisions and dynamics with family and household composition (see Das Gupta 1997).

³¹⁴ Marxists and many of the great Enlightenment thinkers had in common a faith in progress and the improvement of the human condition. Most western political thinkers before the late 18th century regarded inequality and poverty as natural conditions until Condorcet, Paine and others asserted that these evils could be eradicated. Adam Smith, along with Paine and Ricardo, thought that individuals exercising rational self-interest in economic decisions led to general well-being (a good discussion is in found in Isaiah Berlin's *The Crooked Timber of Humanity*, which mentions practically every culture historian you've heard of and many you haven't, but conspicuously omits Malthus). Overviews of Marxism and its historical context (see for example Menard 2016) also frequently ignore Malthus entirely, as if current problems of inequality have nothing to do with the six-fold increase in world population since 1850. I suspect that this reaction had much to do with the fact that Malthus did not so much challenge ideas with other ideas, as was common in the great debates of the late 18th and early 19th centuries, but instead identified an intuitively clear and embarrassingly powerful mathematical flaw in progressive human engineering. This flaw was especially upsetting to those, especially early economists, who saw human progress as tied directly to scientific advances with their roots in quantification.

³¹⁵ Some of his countrymen strongly disagreed. Malthus's contemporary Thomas Paine, whose writings so enflamed American sentiments during the Revolution, was one of them. In his pamphlet *Agrarian Justice*, he railed against private property, especially

in the form of land, as a pernicious and destructive correlate of the emergence of civilization. Although Paine despaired of ever eliminating landed property, he thought redistribution of wealth might ameliorate its worst effects. Lewis Henry Morgan (1963: 559-563) later agreed that individual ownership of private property in the form of land was an unfortunate feature of cultural evolution, but opined that the institution would gradually fade away.

³¹⁶ Malthus is vulnerable to the accusation of being a reactionary because he did not sufficiently link poverty to the vast inequities of wealth typical of his time, especially in the form of landed estates and impoverished urban workers, or rail against them as did Paine. Malthus would still have insisted that population growth would outpace food production even if all lands were redistributed more fairly. Much criticism directed at Malthus reflects the tenor of his times – the intense political and philosophical antagonisms resulting from the American and French revolutions and Napoleon’s depredations in Europe. For many people then and now, the difficult message to digest from Malthus is that ordinary individuals are complicit in the creation of human misery, including their own, and that we cannot simply blame such misery on unfair institutions and social traditions or predatory elites. We are, in a real sense, victims of our own impulses.

³¹⁷ Malthus also knew about other forms of agricultural labor. Many Western societies up into the nineteenth century had the institution of serfdom, in which laborers were tied to particular lands and landlords (although they might have various legal protections). Serfdom in Prussia was only abolished in 1807, when it became necessary to raise large conscript armies to fight the French. There were some 2,000,000 “state serfs” in Russia in the early 1850s – i.e., serfs apart from those attached to private nobles or landlords. The Aztecs and other pre-modern people had similar institutions. Whether the Classic Maya had a component of unfree people is unknown, although their sixteenth century counterparts certainly did.

³¹⁸ Malthus recognized that cycles of well-being and misery differentially affected human populations in a mosaic fashion throughout history. His dismal view of Britain and other European cultures was counterbalanced by contemporary examples of rapidly expanding populations in zones of plenty, notably the nascent United States.

³¹⁹ Boserup (1965: 21) claimed that destruction of agrarian landscapes was underplayed by Malthus, and instead is a position characteristic of more recent “neo-Malthusians”.

³²⁰ Drought was seldom a major problem in Europe as it was for the Maya. European historians in Malthus’s time were well aware, though, of other deleterious climatic conditions, such as the unusually cold and wet years that caused widespread famine, misery and death in AD 1315 and 1316.

³²¹ He would probably not be surprised that the same innovations that have led to extraordinary population growth over the last 200 years have failed to create economic equality or to eliminate poverty.

³²² Concern with detecting very basic forms of inequality is reflected by isotopic analysis of Maya diets, and early on scholars like J. E. S. Thompson envisioned oppressive elites and suffering commoners.

³²³ When reading Malthus, I wondered what would have happened had he extended his natural law to non-human organisms, as Darwin so famously did a generation later. Darwin’s intellectual enterprise, the origin of species, was of course more universal than that of Malthus, whose concern was economic inequality among humans and its social and economic consequences. Darwin linked Malthus’s big idea to a much bigger one. Alfred Russel Wallace also came to his conclusions about evolution partly after reading Malthus.

³²⁴ Sediment cores from Copan reveal the presence of maize and signs of probable human-induced burning at least as early as 2300 BC (Rue, Webster and Traverse 2002). Population remained very low, however, until AD 400 or later. Unfortunately, we have no comparable data from Tikal, but there seems little doubt that people there practiced a mixed economy of hunting, gathering, and gardening by 2000 B.C. or earlier.

³²⁵ In this respect, she reminds me of Wittfogel (1957), who tried to extend his hydraulic hypotheses far beyond its effective limits.