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THE SEQUENCE OF AMAZON PREHISTORY: A METHODOLOGY FOR ETHICAL SCIENCE

A SEQUÊNCIA DA PRÉ-HISTÓRIA AMAZÔNICA: METODOLOGIA PARA UMA CIÊNCIA ÉTICA

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ABSTRACT

This article is about my research on the prehistory of the mainstream lower Amazon in Brazil and problems in research ethics there. The "New Archaeology", my dissertation in the Orinoco, Venezuela, and my museum job led me rethink the human occupation of the tropical forest. I was influenced by 19th century naturalists' research on Amazon archaeology. They recognized an occupation that began thousands of years ago with hunters who made projectile points and monumental rock art and early fishing villagers with pottery and culminated by the time of the European conquest in populous civilizations with mounds and elaborately decorated pottery vessels and figures. But in the mid-twentieth-century, "scientific" archaeologists decided that the sequence had been short and derivative, impeded by the hot, humid tropical forest. They ignored the early cultures and attributed Amazonian cultures to intrusions from the Andes. When radiometric dating contradicted their chronology, they suppressed the problematic dates and when other North Americans came to do research they tried to prevent their excavation permits and criticized them with false statements about their own teams' results. Thus, they involved Brazilian proteges in unethical practices and prevented their rediscovery of the sequence of the 19th century naturalists.

KEYWORDS: Tropical Forest - Methodology - Archaeology - Ethics.

RESUMO

Este artigo é sobre minha pesquisa sobre a pré-história do baixo Amazonas e problemas de ética com a pesquisa nessa região. Os preceitos da "Nova Arqueologia", a minha tese no Orinoco, Venezuela, e meu trabalho museológico me levaram a repensar a ocupação humana da floresta tropical. Fui influenciada pelas pesquisas dos naturalistas do século XIX sobre a arqueologia da Amazônia. Eles reconheceram uma ocupação que começou há milhares de anos com caçadores que faziam pontas de projéteis e arte rupestre monumental, depois com os primeiros pescadores que produziram cerâmica e o auge na época da conquista europeia com civilizações populosas que construíram montículos e vasos e figuras de cerâmica decoradas de forma elaborada. Mas em meados do século XX, os arqueólogos "científicos" decidiram que a sequência havia sido curta e que foi impedida pela floresta tropical quente e úmida. Eles ignoraram as culturas mais antigas e atribuíram às culturas amazônicas como provenientes de intrusões dos Andes. Quando as datações radiométricas contradiziam sua cronologia, suprimiam as datas problemáticas e quando outros norte-americanos vinham fazer pesquisas tentavam impedir suas permissões de escavação e os criticavam com falsas declarações sobre os resultados de suas próprias equipes. Assim, envolveram seus protegidos brasileiros em práticas antiéticas e impediram a redescoberta da sequência dos naturalistas do século XIX.

PALAVRAS-CHAVE: Floresta Tropical - Metodologia - Arqueologia- Ética.

INTRODUCTION

This article is about how and why I established a new prehistoric sequence for the lower Amazon mainstream and what I learned in the process about both archaeological methodology and ethics. One thing I discovered in the course of the work was that accepted anthropological theory about the ecology of human evolution did not predict what we found. It was faulty, therefore, as all theories always are at least in part. Moreover, I found that adherents of the theory who came to do research in Amazonia did not understand that general theory should not be a guide to help you shape prehistory but rather a tool to reveal prehistory, and they could not rise enough above its predictions to let a different sequence of occupation from what they expected to emerge from their research. The methodological problem was that they never had the intent to test the theory but only to apply and confirm it. To test the theory they would have had to do very different research than what they did. In its application to Amazonia, as I'll try to show further on in this paper, the main flaw of their theory of environmental limitation was that it erred in assessing the nature of humid tropical resources for human use and development in prehistory.

Perhaps this first generation of anthropological archaeologists in Amazonia who claimed an identity as scientists were still too new to the practice of science to understand that the scientific method involves more steps than they were taking. It first involves inferring general theory from existing evidence, deducing hypotheses from general theory, then "operationalizing" the hypotheses for the particular phenomena you are trying to elucidate at the site chosen for your research, collecting and analyzing the resulting data for comparison with the hypotheses, then going back to the general theory and adjusting it or augmenting it, based on the your results. In the later 1960s archaeologists interested in developing the method and theory of research in archeology, led by Louis Binford and colleagues, challenged archaeologists to use the scientific method more fully, and they wrote up a series of studies they had carried out trying to do that (BINFORD and BINFORD, 1968). Archaeology has not been the same since then. New generations of archaeologists have moved beyond those pioneers to refine the application of theory and method in their fields and expand its reach beyond what was thought possible by the Binfordians, whose studies admittedly were rather limited. But the original lesson of those archaeological theorists about basic method and theory still stands and without it it's hard to get anywhere in archaeological practice. Without the scientific method, archaeologists are condemned to merely describe speculative scenarios of their own making, devising them based on feelings of identity toward mentors in different university departments or in training from different dig projects.

Another other problem with midcentury archaeologists' work in Amazonia was ethical, involving a lack of transparency and truthfulness. In the face of finds contrary to their theory, they could not accept the need to alter the theory

but instead altered the appearance of the evidence. That is, they manipulated how they reported their results or limited their reporting to try to avoid contradicting the theory. In one example, they misstated the original cultural context of samples for dating in order to change their significance for culture history; in another, they held off publishing dates that did not fit their hypotheses. The personal reasons for such ethical lapses? Difficult to say in every case, but perhaps some were authoritarian personalities loath to admit to having wrong expectations because they thought they might appear weak to their followers or institutions? Or perhaps they had such a personal commitment to the theory that they felt they could not go so far as to admit that they had found contrary evidence? Or, in some cases, did a feeling of solidarity with colleagues prevent them from being frank about the problems and be willing to publish alternative interpretations? Or a competitive feeling with another researcher that made them reluctant to admit to the need to change their interpretation to fit the other's theories or results?

Perhaps, in the end, these and some other archaeologists committed to the general theory were stymied in facing the data because they could not grasp that, realistically, *a priori* theory could never be expected to provide a correct description of exactly what happened in human evolution in a particular place and time because mostly white European-origin theorists' ideas about indigenous human prehistory are of course quite uninformed and their theories not very well-developed. Ultimately, also, there are limitations on any human's abilities to perceive the strands of multifarious evidence of human evolution and imagine the complex causality of past human-environment interaction on earth in the long term. The need to recognize such limitations of our intellect and ability to observe can motivate certain humility in theoreticians, without, however, damping enthusiasm for the endeavor of explanation. But over and above the need to try to be critical minded, without transparency and truthfulness the archaeological record cannot exist in a useful form for it becomes unnecessarily muddied by dodges, tergiversations, and even falsities. The sequence that came out of Lower Amazon research had lessons not only for Amazonian culture history but also for some larger stories: the peopling of the Americas, the evolution of prehistoric technology and art, the ecology of the early human evolution, and the organization of societies.

THE THEORY OF ENVIRONMENTAL LIMITATION IN AMAZONIA

In the theory of human evolution as it is applied to human ecology and adaptation, the tropical rainforest had been assumed to have been occupied only in late prehistory because of the assumed hostility of this habitat to human occupation (e.g., NEVES, 1991; ROOSEVELT, 1991a). The presumed limitation on human occupation is that it is too hot, too wet, too dark, too poor in soils, plant foods, and game, and too disease-infested for humans to penetrate until com-

plex societies outside the rainforest had developed sophisticated agriculture to replace the forest, augment its soils, and create enough subsistence and surplus for permanent human settlements, specialized occupations, and hierarchies. For early human evolution in Africa, paleoanthropologists have long assumed that the tropical apes of the evolutionary lineage that led to sapient humans only started in that direction when Ice Ages chilled and dried the tropical belt, effacing the rainforests with vast steppes and savannas (e.g., PRANCE 1982). That this idea of the environmental context of early human evolution in Africa was itself very flawed has not had much effect on the history of method and theory of research there, but today it seems very clear from the point of view of method, theory, and evidence (ROOSEVELT 2005, ROOSEVELT 2022).

The earliest scientists interested in the prehistory of the Amazon were 19th century natural scientists - geologists and biologists - trained before the tropical forest limitation theory had been applied there (e.g., BARBOSA RODRIQUES, 1886, 1888; FERREIRA PENNA, 1876, 1877, 1885; HARTT, 1971, 1985; NETTO, 1885). Despite being natural scientists, they were very interested in Amazonian archaeology and researched and wrote about the magnificence and monumentality of the ancient artworks, the magnitude of the ancient sites and habitation mounds, and diversity of the ancient cultures. For example, Ferreira Penna, who founded the Goeldi Museum, exavated in coastal Amazon shellmounds and persuaded Hartt to excavate the riverine one at Taperinha, which he inferred was an early fishing village. And he, Hartt, and Netto all considered the Marajoara polychrome culture to be a civilization. In the absence of a theory that the tropical forest should have been a barrier to indigenous human cultural development there, they had no inhibitions about seeing Amazonian cultures as very early or as civilizations.

Furthermore, natural science was already working with the scientific method: inferring theories by observation, then refining theories by comparing them to the emerging evidence, without being hindered by what prominent scientists had claimed. For example, Charles Hartt, a geologist who worked extensively in Brazil and served for a time as the Commissioner of Geology of Brazil, eventually critiqued the pre-conceived creationist theories of Louis Agassiz, his famous Harvard mentor, about the role of the Ice Age in Amazonian geology, because of what he observed in the stratigraphy and sedimentology in his repeated visits there in the second half of the 19th century (BRICE and FIGUEIROA, 2001). Interestingly, for Agassiz the theory of worldwide glaciations was crucial for proving his racist notion of polygenesis: the separate and unequal divine creation of the human races that contradicted Darwin's ideas of evolution by natural selection on a sucession of related species. [In European polygenesis of the time, the White race assumed to be the one that developed first, and other races were seen as degererate, primitive, and separately evolved (LURIE,1954).

But by the time scientific archaeology came to the lower Amazon in the middle of the 20th century, the two most influential researchers who came from the US - Betty Meggers and Clifford Evans - were indoctrinated in the evolution-

ary theory of environmental limitation. According to their application of the theory (EVANS and MEGGERS, 1968; MEGGERS, 1954; 1971), human occupation of the Amazon should be late and a product of an invasion from civilizations in the Andes, where advanced agriculture had been developed. This influential theory seems to have led them to avoid dealing with the kinds of contrary archaeological finds described by the early natural scientists: the rock art at Monte Alegre, the finely flaked projectile points that had reached museums, the sherds in the shellmounds, and the habitation features in mounds on Marajo Island. For their dissertations and later research, they focused on ancient pottery styles that could be relatively safe theoretical territory, for the time being (EVANS, 1950; MEGGERS, 1952).

Thus, we can see that the theory focused their interest away from sites that could falsify the theory and toward ones that could confirm it. To fit the theory, they made assumptions about the chronology of ceramic styles, such as the Polychrome Horizon, which they claimed had come from the Andes and established the Amazonian styles of the Horizon. For a while, their assumption was safe, because radiocarbon dating, which would falsify their chronology, had not yet been applied to the lower Amazon. Furthermore, despite the environment being a key factor in their model of cultural causality, they did not make a concerted effort to retrieve biological remains from archaeological sites. These archaeologists seemed to feel that it was unlikely that research would turn up patterns at odds with their theoretical assumptions and finds at the time they began to work in the Amazon.

The method of absolute dating by radiocarbon analysis was developed by the midcentury, so the archaeologists who had trained the first Amazonian archaeologists to identify as scientists had not included absolute dating in their graduate education. Thus, it was at first not an important tool in their archaeological kit in Amazonia, and when they eventually embraced radiocarbon dating, they got unexpected results, pushing the dates for Amazonian culture history much deeper into the past, something that also happened in other regions of the Americas. When they found that the radiocarbon dates for certain of their pottery phases, such as the Polychrome styles and the shellmound pottery, did not fit the the guess dates of their hypotheses deduced from the environmental limitation theory, they tucked the problem dates away unpublished for a time and in the case of early Holocene shellmound pottery, they changed how they characterized the cultural context of the dated material, to make the published dates more acceptable for the theory, as we shall see, below.

A rival view to the explanatory approach by the Smithsonian archaeologists came with the entry of Donald Lathrap to the field (1970), occasioning academic strife. Although Lathrap also was prone to diffusionary theories, his had a different geographical slant, and he definitely considered Amazonia as a major area of development on its own and an important influence on the Andes, expecially in the Formative period. For the shellmound pottery, though, Lathrap

believed its origin was in Africa, whereas Meggers and Evans said it originated in Japan. Both theories were diffusionary, but in different directions. Later on, Lathrap's students worked on the Polychrome Horizon, both at the Andean foothills and at the mouth of the Amazon (BROCHADO, 1980; MAGALIS HARRIS, 1978; WEBER, 1975). Lathrap's idea about that Horizon was that it originated and spread from the central Amazon, not from the Andes. In addition to these diffusionary ideas, Lathrap and his students also focused very much on identifying the languages for each group of horizon styles, despite evidence from ethnohistory that the horizons were multilingual (ROOSEVELT 1991).

Lathrap's and Meggers' and Evans' teams felt in competition against one another, and Lathrap told me that Meggers lobbied against his students getting excavation permits in Brazil, with the result that one of them could only study museum collections for her dissertation (MAGALIS HARRIS, 1975; Simões confirmed to me that Magalis was not allowed a permit because of Meggers' influence). When I published that Meggers and Evans was preventing archaeologists from getting excavation permits (ROOSEVELT 1992, 1995), it was angrily denied by their proteges (Baffi 1996), but their denial was untruthful and unethical (HURT, 1996). Similarly, Lathrap felt free to express explosive anger at proteges of Meggers and Evans, and even interrupted Mario Sanoja's formal presentation at SAA in 1977. For my independence from both warring sides, he shouted at me in the crowd at the same SAA and later wrote me and my publisher that my dissertation "cannot be published", though it was published (ROOSEVELT, 1980). A North American full professor publicly bellowing in anger at grad students and younger South American archaeologists hardly seems ethical, given the power differentials.

Because of the when I came into archaeology, just after the first publications of the "New Archaeology" (BINFORD and BINFORD 1968), I was exposed to ideas that gave me a more critical theoretical approach and a commitment to the scientific method, as well as some useful field methods to apply in Amazonia, such as soil flotation, stratigraphic analysis, and archaeobotany. I had worked with Junius Bird as a undergrad intern at the American Museum of Natural History and became influenced by his meticulous excavation and object analysis methods, including ethnographic actualization. Because of my different methodological training and lack of assumptions about tropical habitats, the Brazilian projectile points and painted rockshelters that midcentury American archaeologists in Amazonia had avoided, strongly drew my interest. I had no reason from personal history or opinions to agree with the environmental limitation theory. In fact, from the experience of the dissertation research I did in the Orinoco on the Caribbean mainland of South America in Venezuela, I was inclined to think that the tropical riverine environment would have been a welcoming and favorable environment for ancient people. I liked living in the reed, thatch, and mud hut at Ronquin, cooled by the river breezes, had bathing in the mild, muddy Orinoco waters and enjoying the abundant fish, fruits, nuts, and starchy crops we ate there. Furthermore, when preparing for that research in Venezuela, I read up on the na-

ture of soil resources in lowland South America. In doing that, I realized that the alluvial river bottom soils there were the same classes as in the bottomlands of the Nile but a hundred times more extensive than in Egypt. If those soils were not a problem for the development of ancient Egypt, I could not believe they were a problem for ancient Amazonians (ROOSEVELT, 1980, 1997; VAN DER MERWE et al. 1981). As I learned more about Amazonian rocks and soils as I went along, I saw that upland - terra firme - soils also were better than the limitationists assumed, because of the bedrock in some places.

THE STRATEGY FOR ESTABLISHING THE SEQUENCE

Together, the literature on Amazonia, my experience in the Orinoco for my dissertation, and the museum collections I saw as a curator at the Museum of the American Indian suggested that the archaeological sequence in the Amazon might be different from the notions of the mid-century archaeologists. I chose the lower Amazon in Brazil for the investigation of the evolutionary sequence because the literature showed a wide range of different cultures: Preceramic, early Archaic ceramic, Formative, and complex cultures in the common era. Most of these cultures had been recognized by 19th century natural researchers and their research had been summarized in several publications I had read, especially two books by an independent archaeologist (e.g., PALMATARY, 1950, 1960) whose research affiliation at Penn I later learned had been ended due to a letter from Evans to Kidder I found in Penn's archives).

In thinking what would be a good excavation strategy for investigating the sequence, I decided to choose several multicomponent sites with well preserved stratigraphy to excavate using natural layers and features. The overlap in cultural phases among such sites could help verify the sequence, and the cultural "sandwich" of their stratigraphy could be a check on the dating and cultural identifications. I also decided I would sift all the soil I excavated with fine screens and process large samples with soil flotation to find identifiable plant and animal remains and small artifacts, such as sherds and lithic flakes. At the time, many archaeologists in the tropics were either not screening the soil or were using too-large mesh. I was influenced by an archaeologist of the New Archaeology, Stuart Struever, who applied the flotation technique to the processing of soil from excavations in his work in Illinois (STRUEVER 1968: 353). (He was the series editor at Academic Press, where I published.) I knew from the research in the Orinoco that sites in the humid tropics were full of carbonized plant remains and bones but that you needed to fine-screen exhaustively to retrieve them and also pack them well to preserve them. I saw that having just one date per culture on unidentified material was not good because objects could slip down from another layer or come from someone's backfill. Because I was working up a new sequence, I needed to pin down the chronology with many dates.

I was able to get major funding for the field research on the Amazon sequence from National Science Foundation and National Endowment for the Humanities. NSF chose to support the work on Marajo, a prehistoric ceramic complex of the common era, but the research on the earlier part of the sequence and its culmination in the late prehistoric complex cultures was a road too far for the theoretically conventional archaeologists at NSF those days, so NEH funded those other components, to its credit.

THE PALEOINDIAN CULTURE AT MONTE ALEGRE

Having been a museum anthropologist in my first job, I had seen finely flaked triangular projectile points from eastern South America on the storeroom shelves at the Indian Museum, and wondered about the preceramic cultures that they came from. Another curator, Clara Lipson, told me about a National Endowment for the Arts grant, which I got in 1980 and used to travel to collections in South America, Europe, and the US. On that trip I found the triangular points also in Brazilian museums, especially at Santarem at the mouth of the Tapajos river.

The Brazilian points were finely pressure-flaked in the Upper Paleolithic tradition began in Central Africa and was brought to western Europe by African migrants, who created the fine portable art and monumental rock art there. Since evolutionary anthropologists were claiming that the tropical forests of both Africa and Amazonia were off limits for early hunter-gatherers because of the lack of large game and starchy food sources (BAILEY et al. 1998), the presence of the points was provocative, if Amazonia had been forested then, which I conclude it was (ROOSEVELT, 2021).

The Paleoindians were a branch the Upper Paleolithic culture that emerged from incubation in east Asia after the diaspora from Africa. But the Brazilian points, often stemmed and sometimes very large, were completely different from Clovis Paleoindian points of North America. But, around the time of my initial research in Brazil, archaeologists researching early cultures along the Peruvian coast and California coast and islands also were finding triangular, stemmed points in late Pleistocene sites. To me, the stemmed triangular points looked like fish-arrow points or harpoon points, which need tangs and barbs to stick in prey so that it can be pursued and caught. But the Clovis hegemony then insisted that such point shapes had to be later than Clovis fluted points. For them, triangular stemmed points had to be Archaic of the early Holocene after 10,000 BP. But by then I knew from my research on the Clovis culture that archaeologists' claims for its chronological priority were exaggerated and that the link of Clovis to South American preceramic stone-tool cultures was weaker than claimed, as well, considerations I and colleagues later published (ROOSEVELT, 1998; ROOSEVELT et al. 2002).

THE RESEARCH AT MONTE ALEGRE

To resolve the issues on the age and function of the Brazilian points and see if the Lower Amazon sequence might have begun in the late Pleistocene, I needed to try to find them in stratigraphic context, get material to date, and see what subsistence remains were. I had read 19th-century descriptions of the rock paintings, rockshelters, and caves at Monte Alegre, the region opposite the mouth of the Tapajos, where some of the points had been found. They described an artistic culture focused on the sun and animals (HARTT, 1971; WALLACE, 1889). In the later 20th century, Brazilian speleologists and archaeologists had surveyed and published on sites in that rock art area (SILVEIRA et al. 1984; PEREIRA, 2003), but none had yet excavated beneath the rock paintings to sample and date the stratigraphy.

A Brazilian archaeologist from the Museu Goeldi, Mario Simões, had published two large, very finely flaked stemmed, triangular projectile points from the Tapajos river mouth are facing Monte Alegre across the Amazon (ROOSEVELT et al. 1996; SIMÕES, 1976). These were like the ones I had seen my museum research. Like me, this archaeologist thought the points were preceramic, but early Holocene Archaic rather than Paleoindian.

From my research on the Taperinha shellmound and the Smithsonian radiocarbon lab archives, which I report below, I knew that the early Archaic culture along the lower Amazon was not preceramic but an initial ceramic culture. Its dates from our excavations, going back to almost 9,000 BP cal, pushed preceramic back to the terminal Pleistocene, the age of Clovis (ROOSEVELT, 1995; ROOSEVELT et al. 1991). I thought that it was likely that stratified deposits could be found at the Monte Alegre caves and rockshelters, where the paintings and projectile point finds indicated that early people had been there. So, I included Monte Alegre in my grant application to investigate the Amazon sequence and visited the area on a break from our excavations at Santarem and Taperinha on the other side of the river in the late. On that visit, I reviewed many of the known archaeological sites in Monte Alegre and at Cavern of the Painted Rock I found the deeply stratified multicomponent deposit I sought for my sequence. From the remains eroding from the profile of the deposit, where a bulldozer had cleared the entrance "to clean it for tourists", I observed the stratification and good preservation of plant remains, shells, bones, and artifacts.

In 1991 and 1992 I brought a group of American and Brazilian archaeologists and students back with me to survey and excavate at Monte Alegre under my direction. After surveying many sites and sampling 21 with auger, we determined that Cavern of the Painted Rock had a 2-m deep stratified occupation from the preceramic to c. 1500 AD. In 11 square meters of excavations, I and Maura Imazio da Silveira from the Museu Goeldi, working with hand tools, found 17 strata of very different color, texture, and contents. The team used fine screening and flotation to process the soil and piece-plotted formal artifacts and biological

remains. For the chronology, we used many different kinds of materials for dating by different radiometric methods and got taxonomic identifications of many of the plants and animals. There were only about 25 formal stone tools in the Paleoindian levels of the excavations but more than 30,000 lithic artifacts from flaking.

The upshot of our analysis was that the Monte Alegre Paleoindians had used bifacial projectile points of triangular, stemmed shapes and unifacial gravers and cutting tools in a subsistence economy of broad-spectrum forest and river foraging, not of big game hunting. All of the 25 plant and animal species our colleagues were able to identify are still to be found in the humid tropical Amazon today. These showed that the Paleoindians' were in forest, not savanna, and that conclusion and other research helped lead to the death of the Refugium Theory, a beloved 20th century theory of Pleistocene environmental change (ROOSEVELT, 2021). Key initial evidence for late Pleistocene humid tropical forest were the stable isotope ratios of the biota from the cavern, which fell at the negative range of the C-3 plants characteristic of dense tropical rainforests in the interior of the Amazon (ROOSEVELT, 2000b; ROOSEVELT at al. 2002). Had the Amazon been an arid savanna or steppe at the time of the Paleoindian occupation, the ratios would have been significantly less negative - 10 per mil or more - than they were.

To determine the age-range of the Paleoindian strata in the cavern, we ran 56 radiocarbon dates on carbonized wood and fruits, 10 TL dates on burned lithics, and 3 Optically Stimulated Luminescence dates on sediment (MICHAB et al 1998; ROOSEVELT, 1999; ROOSEVELT et al. 2002). The radiometric dates ran from c. 11,000 to 10,000 bp radiocarbon. The abundant pigment chunks and paint drops we found were in the lower levels of the Paleoindian strata, and by electron microscopy by William Barnett at the American Museum of Natural history where I worked after the Indian Museum, and the thin sections by M. Lima da Costa at the University of Para, Belem, we ascertained that of the 20 samples analyzed, most ones from the layers were from the same sources as most of the ones from the rock paintings above our excavations. We published a long research article on our results in *Science* (ROOSEVELT et al. 1996).

Soon after our research article on the cavern in *Science*, the tree ring calibration for radiocarbon dating (STUIVER et al. 1998) was extended back to the late Pleistocene, making our radiocarbon dates into significantly earlier calendar dates: c. 13,000 cal for the Initial Paleoindian levels and c. 11,400 BP cal for the end of the Paleoindian occupation, according to the radiocarbon scientists' calculations. By then we had carried out Thermoluminescence dating on burned lithic tools and Optically-stimulated Luminescence dating on sediment from the initial Paleoindian levels. The c. 13,000 calibration of the weight-averaged Initial Paleoindian radiocarbon dates fit the weight-averaged Luminescence results on the accompanying sediment (ROOSEVELT, 1998a; ROOSEVELT et al. 2002). With this culture, therefore, we were at or near the beginning of the human occupation in the Amazon. After such tree-ring corrections were available for Paleoindian

era dates, archaeologists have had to be careful to compare radiocarbon dates to radiocarbon dates and calibrated dates to calibrated dates. If one mistakenly or purposely compared one's own *calibrated* dates to another's *radiocarbon* dates, it would create a false chronological priority for one's own dates unbeknownst to the unwary reader, as we shall see did happen in regard to Monte Alegre's dates.

Following our 1990s research at Monte Alegre, my University of Illinois graduate student Christoper Davis (2009, 2011, 2014) surveyed and excavated sites there for his dissertation project on the archaeoastronomy of the rock art in the open sites, caves, and rockelters. His excavations at Painel do Pilao, a rock art site immediately above the Cavern uncovered about 2 meters of stratified sediment with a layer of lithic debitage, painted rocks, pigment chunks, and charcoal. His four wood charcoal accelerator radiocarbon dates were exactly the same age as our four radiocarbon and three Luminescence dates of the earliest Paleoindian levels at the cavern (DAVIS, 2016; DAVIS et al. 2017). His dates, which were from the University of Arizona Radiocarbon Laboaratory, were accelerator dates and had errors less than plus or minus 71 years, significantly smaller than most Clovis dates from the same lab. Moreover, Davis' four dated charcoal samples turned out to be tropical gymnosperm taxa, a corroboration of other research suggesting widepread presence of such trees in the tropical lowlands at this time (COLINVAUX et al. 1996, 2000). In his archaeoastronomical research using the GPS and theodolite mapping and the NASA software Starry Night, Davis showed that a series of large red and yellow concentric circle images high on the rock walls along the adjacent sites of Serra da Lua and Serra do Sol bracketed the reach of the sun's rays at sunset from across the valley on the two solstices. Davis, Barnett, and Lima da Costa's electron microscope and thin-section analysis of the iron titanium ratios of the iron-oxide pigment from the strata and walls at Painel identified sources that not only matched each other, but matched some Davis had sampled at Serra da Lua. Thus, the research Davis led confirmed the age of the initial Paleoindian occupation and its contemporanity with rock paintings and solar observatory.

The findings at these Paleoindian sites in the lower Amazon showed that Paleoindians had not avoided the tropical rainforest, as archaeologists had assumed. Furthermore, the Paleos were not Clovis big-game hunters, a lifeway not ideal for the tropical forest, which gives prey cover from predators and limits the biomass of large animals while privileging biomass of small animals and plant foods. The foodways of the Amazonian Paleoindian culture were focused on a wide range of plants and small, mainly aquatic fauna, instead. These findings suggested that the earliest migrants to the New World had developed adaptations to a range of different habitats. Furthermore, the results showed that the early foragers were not primitive, since the results of the astronomical research showed that the Amazon Paleoindians had created monumental rock art and a solar observatory at 13,000 BP cal., the earliest one yet known, worldwide.

PROBLEMS IN THE REPRESENTATION OF THE RESEARCH AT MONTE ALEGRE

Soon after we published in *Science*, there was an immediate reaction by a leading Clovis archaeologist, C. Vance Haynes, who claimed that because our early Paleoindian dates were conventional radiocarbon dates with the usual standard error of several hundred years made them unreliable, so not the same age as Clovis (ROOSEVELT et al. 1997**)**. But, as I pointed out, most Clovis dates were the same kind of dates with the same size errors, so no difference in reliability between Clovis and Monte Alegre (ROOSEVELT et al. 1997). I had detected inaccuracies in literature on the age of Clovis (ROOSEVELT 1998**)**, inspiring me to make a comprehensive review of Clovis dating (ROOSEVELT et al. 2002), showing that archaeologists were claiming an earlier age for the culture not justified by the statistics of the dates or their archaeological associations. So, rather than Clovis being 12,000 radiocarbon years old it was no earlier than c. 11,000 bp, the same age as Monte Alegre Paleoindians, which therefore could not be an offshoot of Clovis. Haynes certainly knew that the standard errors of Clovis radiocarbon dates were the same size or larger than ours - they were run at his university and he had published them - so not acknowledging that fact in his statement about the Monte Alegre dates constituted a lack of honesty and transparency.

When I went to do research at Monte Alegre, I had invited Imazio da Silveira and Pereira from the Museu Goeldi and other Brazilian archaeologists to join our excavations there, which they did, but Pereira refused and petitioned IPHAN (Institute of Historical and Archaeological National Patrimony) to deny our research permit. The agency, which is concerned with the study and preservation of the cultural heritage of Brazil, granted our permit anyway.

However, due to publications by Pereira at the Museu Geoldi, the cultural and historical significance of Monte Alegre may not be fully absorbed in Brazil, although both I and Davis have given multiple lectures on the work there in Portuguese and published in Brazilian publications. Our long-time Museu Goeldi research colleague, Imazio da Silveira, excavated alongside us in the excavations at Monte Alegre, and our publications are in the MPEG library as well as online. But Pereira, who excavated several decades later on left of the cavern entrance, recently published misinformation about both her and our results in a guidebook and research report published at the museum (PEREIRA and BARRETO, 2017; PEREIRA and MORAES, 2019). Her publications down-dated our initial radiocarbon ages by misrepresenting them as c. 11,000 BP calendar years, instead of 13,000 BP calendar years, making them appear two thousand years less than they are. Pereira's dates were c. 10,000-10400 bp radiocarbon years bp, for calendar ages of c. 12,000 to 11,400 BP. Thus, by misquoting our radiocarbon ages as if they were calibrated ages, she made her calibrated ages appear a thousand years earlier than ours, though they are a thousand years later. Could this MPEG archaeologist not know the calibrations of our dates or the TL and Luminescence calendar dates we had

published? She neglected to cite any of those publications. She also neglected to mention Davis's archaeoastronomical discoveries at Monte Alegre, a point of major interest for both tourist and academics. Such material inaccuracies are a serious lapse whether from poor methods - not checking the literature - or poor ethics - misrepresenting the facts of other's research in relation to one's own -, but Pereira's errors remain uncorrected, despite our requests for a correction.

THE POTTERY ARCHAIC SHELL MOUNDS OF THE LOWER AMAZON

For a long time, archaeologists in the Americas had assumed that the development of pottery had coincided with the transition to agriculture because that economy allowed sedentary settlement. However, it also had been recognized that in riverine and coastal areas of the humid tropical lowlands of South America, early pottery had occurred in a context of shellfishing and fishing. The geographer Carl Sauer had proposed that those areas had such abundant aquatic resources that they could support permanent settlements without farming. If pottery was a craft encouraged by permanent settlement and a need for cooking containers, then such areas might be places for pre-agricultural pottery. The sites would also be a favorable context for the slow change of collected and camp-follower plants toward a domesticated state through the effects of plant use and the nutrient-rich refuse at the settlements. The history of thinking about such early waterside sedentary sites and the rise of early pottery worldwide were summarized in a Smithsonian book edited by Barnett and Hoopes (1995) in which I wrote about the Amazonian Archaic pottery shellmounds (ROOSEVELT, 1995).

But the Smithsonian archaeologists had hypothesized that the early shellmound pottery they had excavated in tropical forest Ecuador had been introduced by fishing people from Japan, rather than being developed in situ (MEGGERS, EVANS and ESTRADA, 1965). Finds of earlier shellmound pottery in the far eastern part of the tropical lowlands east of the Andes would tend to counter the idea of a foreign introduction from the Pacific or at least have shown that in some areas pottery could have been a native development. Or, it could support the speculation by Lathrap (1977) about an introduction of the first pottery to the Amazon. His take on Sauer's theories about shellmounds was that perhaps pottery had been introduced from North Africa, where early Holocene shellmound sites with pottery had developed around lakes during period of increased humidity. Thus, both the rival archaeologists who came before me didn't trust Amazonians to have developed pottery on their own.

EARLY POTTERY AT TAPERINHA SHELLMOUND

The possibility that early fishing people in the early modern climate period in the Lower Amazon might have developed pottery was recognized in the 19th century. Charles Hartt, one of the Victorian natural scientists who had written about Monte Alegre had also visited and excavated at a pottery-bearing shell mound at Taperinha, across the Amazon from Monte Alegre (HARTT, 1885). His research was inspired by his search for index fossils for the dating of the sequence of the geological stratigraphy of Brazil, but Domingues Soares Ferreira Penna, founder of the Museu Paraense Emilio Goeldi, had urged him to consider that the molluscs there were a human site because others Ferreira Penna had investigated had proved to be (1876). Taperinha was a huge pile of densely packed and stratified lenses of shells, fishbone, and pottery more than 6 meters high.

I was intrigued to read about Hartt's Taperinha work in excerpts quoted in a book on the Tapajos (PALMATARY, 1960), so I followed up and visited his collections and notes at Harvard and Cornell (ROOSEVELT, 2000a). The archaeologist in charge of Hartt's collection at Cornell demanded that I include him as co-author of any publications on the material as a condition for my taking samples, but I could not agree to that, as he was not involved in any work on Taperinha or its material. (Most peer-reviewed journals require co-authors to have contributed directly to the research.) But Harvard's Peabody Museum, where part of Hartt's collections and archival papers were, had no objection to my sampling the shells he had excavated with pottery from the top the shellmound. Though shells can be problematic for dating, Hartt's pottery sherds from the site were so rare and precious I did not want to destroy them for dating. The shell radiocarbon date came out at 5,705 +/- 80, or c. 6,500 BP cal. (ROOSEVELT 1995; ROOSEVELT et al. 1991). Thus, before I worked at that site or at the shell deposit in the Cavern across the river, I had preliminary evidence of an early Holocene date for the pottery from the area.

Taperinha shell mound where Hartt excavated had been in the ownership of the family of a Goeldi Museum Swiss zoologist names Hagman, since the turn of the century. But his grandchildren had allowed the mound to be bulldozed by the Ludwig Plantation, whose owner needed shell to fertilize plantations of foreign trees. So, I wondered whether the mound was still there and if so, if it was in a condition to be excavated. Through a Santarem historian, Helcio Amaral, I met Wilton Hagman, a great grandchild, who lived in Santarem and went to Taperinha for family vacations. He assured me that Taperinha was still a huge mound of shells. Bulldozing had removed part of the mound, not all. Not only did he agree for us to excavate there, but he welcomed us to stay with them in the mid-19th-century mansion his ancestor had restored and he served the needs of the project with enthusiasm and a useful handiness at generator and well-pump repairs.

My strategy at Taperinha in 1987, 1988, and 1993 employed the use of

geophysical and topographic surveying with several techniques to assess stratification in advance of excavation (BEVAN, 1989; ROOSEVELT, 2007). The survey revealed the chemical signature of the original size of the mound before bulldozing and showed that the mound lay on a sand beach over a lagoon of an Amazon tributary. I was able to cut back from Ludwig's cut to excavate the base of the strata column and clear off and sample the upper levels as well. Hartt's excavations had shown that the upper part had both dense shell and early pottery, but I needed to see if the pottery also was there at the start of the occupation, six meters down. I tested the dating of the pottery by collecting stratigraphically associated and piece-plotted material from a succession of living features and by dating samples of different materials, to see if the materials in the features were an intact, original association, and they were. The shellmound strata consisted of intact strata made up of numerous small lenses of shell and fisbones discarded after consumption. (Most of the habitation features we excavated at the site were located on the beach around the mound.)

The most important feature for our purposes was a hearth on the beach sand near the base of the mound. It had numerous pieces of thick, coarse, grit tempered semi-oxidized pottery bowls with soot on the outside or inside. Most of the pottery was plain but a few sherds had incision and/or punctation on the rims and sides. Along with several hundred pottery sherds were numerous freshwater pearly mussels, fishbones, a few turtle shells and amphibian bones, and some wood charcoal. Unlike the Paleoindian levels at Monte Alegre, there were few identifiable carbonized fruits or seeds in the shellmound and few stone pieces except for burnt lateritic rocks and small, unshaped stone slabs. There also were a few turtle shell and shell scrapers and a bone toggle in the layers. Although shells greatly predominated thoughout the extant 6 meters of the shellmound, the fishbone would been most of the meat consumed.

We ran individual radiocarbon dates on the charcoal, the shells, the charcoal on pottery sherds, the charcoal within sherds, and a TL date on the same sherd whose carbon was dated. The 11 radiocarbon dates from our excavations at the shellmound ran between c. 7000 and 6000 bc, or 8,000- 6500 BP cal, and the TL date was c. 7000 BP cal. The dates showed that Taperinha shellmound had pottery from the beginning of its occupation quite soon after the end of the Holocene. The stable carbon isotopes of the dates on the charcoal and carbon in the pottery were c. -28 per mil, a result indicating nearby closed-canopy forest.

EARLY POTTERY AT CAVERN OF THE PAINTED ROCK, MONTE ALEGRE

When we excavated at Monte Alegre in 1991 and 1992, there was a shell-rich layer above the sterile layer that sealed the Paleoindian deposit. It included fishbone and remains of other fauna and coarse grit-tempered pottery with occasional incised and/or punctate designs (ROOSEVELT, 1995; ROOSEVELT et al.

1996). From this shell layer, we dated six early Archaic radiocarbon samples: a turtle shell fragment, several shells, and the shell temper in a pottery sherd, and one TL on a late Archaic sherd. The span of these dates extended slightly earlier and later than the ones at Taperinha: from c. 7,600 to 6,600 bp radiocarbon years, or 8,500-7500 BC cal for the early Archaic, and a calendar date of c. 5000-4400 BP from the TL on the late Archaic sherd. These dates corroborated the results from Taperinha, showing that soon after the end of the Pleistocene there had been a period of intensive aquatic harvesting when people made pottery, crude but occasionally decorated. This pottery was the oldest yet known in the Americas and continues to be so. Its type of designs continue and become more elaborate in the Formative period which is considered the time of the adoption of farming subsistence in many areas of the tropical lowlands.

EARLY POTTERY ALONG THE ATLANTIC COASTS

Shellmound pottery also had noted along the coast north and south of the mouth of the the Amazon and the Orinoco, according to Ferreira Penna's works cited above. Further excavations were carried out by the Smithsonian archaeologists in the mid-20th century and by Brazilian and Guyanese researchers affiliated with the Smithsonian archaeologists in the later 20th century. When the Smithsonian archaeologists excavated pottery shellmounds along the Guyana coast, they did not radiocarbon date them, claiming the sites lacked suitable material to date, though they report finding all the usual things that are radiocarbon dated: charcoal, pottery, pottery temper, bones, and shells. When their collaborators excavated later on and published dates for pottery from the shellmounds in these areas, the pottery was usually presented as intrusive Formative pottery, not Archaic, and the shell mounds as preceramic (IMAZIO DA SILVEIRA and SCHAAN, 2005; SIMÕES, 1981; WILLIAMS, 1981, 1992). When we published our dates in *Science*, Meggers who had propounded the theory of the Japanese origin of early pottery in the Americas, claimed that no-one had gotten early dates as I did, implying that the dates must be suspect. She stated to the media that all shellmound pottery dates in the Amazon other than ours were no earlier than 3000 BP (Washington Post December 16, 1991, A 15).

However, her statement was not true, as I learned from the records of the Smithsonian Radiocarbon laboratory, which ran those other dates. Jose Brochado, a Ph.D. student of Lathrap's, had found in the Museu Goeldi and circulated a copy of the list of the radiocarbon dates the lab had processed from pottery shellmounds on the coasts north and south of the Amazon mouth and other sites. When I checked records in the Smithsonian Radiocarbon Lab Archives, I found that those sites' earliest dates overlapped with our dates at c. 6000-4000 bc radiocarbon or c. 7000-4500 BC cal, but many of the dates had not been published or were published as preceramic (ROOSEVELT, 1995, 1998b, 1997a). However, ac-

cording to the sample labels, registration forms, correspondence, in the archives, and in the published excavations, all the shellmounds had significant amounts of pottery throughout; all the shellmound material dated by the Smithsonian lab was associated with pottery that differed from pottery in higher levels; and all the assays were documented in writing as intended to date the early pottery culture. Later on, the MPEG curators who published the finds from south of the Amazon mouth told me that the Smithsonian archaeologists had urged them not to publish the earliest dates or their association with pottery (M. Simoes and C. Correa, personal communication). Had they done so, the discovery of the earliest ceramics in the Americas would have been by Brazilians, not an American.

So, this American archaeologist repeatedly and aggressively countered the results of archaeologists who came after her in an untruthful manner. She knew the lab at her institution had gotten comparably early dates with shellmound pottery but did not acknowledge that and instead tried to impugn our results, on the grounds that new results that overturned earlier understandings should not be accepted (Meggers quoted from the Washington Post, in Roosevelt 1995: 130, Endnote 3). And their acolytes simply repeated this mantra without reference to the facts of the actual dates and stratigraphy (e.g., NEVES, 2007: 118; WILLIAMS, 1997). Even after I published all these early Holocene dates from all three areas, many of them directly on pottery sherds, Meggers, in a further example of defensive illogic not dealing with specifics, continued to repeat the claim that all the early shell mounds must be pre-ceramic (MEGGERS, 1997).

SIGNIFICANCE OF THE EARLY POTTERY

How is the chronology of pottery-making important in culture history of the Amazon and in regions beyond? Because it added to the existing evidence against the traditional assumption that early pottery was necessarily produced by early farmers (BARNETT and HOOPES, 1995; HOOPES and BARNETT, 1995). It helps us understand that soon after the close of the terminal Pleistocene era, people in nutrient-rich riverine and estuarine areas of the tropical lowlands settled down in the first villages there on the basis of intensive fishing and shellfishing. And it shows that ceramics were independently developed in several such areas, rather than diffused from one geographic center of invention. This conclusion is a change in understanding of the course of human cultural development and the role of environment and subsistence economy in the rise of the important technology of container ceramics.

THE FORMATIVE ERA

The Formative stage of occupation in the Americas has long ago been defined as the beginning of the period of developed village farming, sedentary settlement, ceremonial centers, and elaborate crafts such as pottery and textiles between c. 3000 and 1000 bp in North America (WILLEY and PHILLIPS, 1958). As one moves further south, the dating of Formative cultures get earlier, and there's a lot of variation in the degree to which farming versus gathering supported Formative communities. The original concept of the Formative became problematic in the 1990s partly because it had not allowed for initial pottery cultures based on fishing that have been discovered in Archaic shellmound sites.

In general in lowland South America, Formative pottery styles tend to be elaborate and have decoration of incised designs, modeled decoration, and/or bichrome painting, but cultures developed regional formations. For the tropical lowlands in the Orinoco basin, Caribbean coasts, and the Guianas, some Formative cultures of the Salaroid-Barranocoid Horizon had elaborately decorated pottery, earth mounds, and cultivated plants (OLIVER, 2014; ROSTAIN, 2008; VERSTEEG, 2008). My middle Orinoco dissertation in the 1970s established that the Saladoid Formative culture there was quite early, beginning at least 4500 years ago bp (ROOSEVELT, 1980), and the other researchers have documented the great continuity and innovative development of these pottery styles through time and space in the northern lowlands during the entire era from c. 5000 bp to ad 500 (ROOSEVELT, 1997b).

In my research there I was interested in changes in Formative occupation. I hypothesized that the introduction of maize from the highlands could have moved economies away from reliance on cultiated manioc as a staple and allowed the expansion of population on floodplains suitable for maize cultivation. The results on human bone chemistry showed that the Formative cultures had a C-3 crop, such as manioc. The archaeobotany showed that a highland race of corn came in during the late Formative, about 2000 bp years ago, but bone chemistry showed it had only became the staple food when a tropical lowland race of corn was subsequently available (ROOSEVELT, 2016).

The Formative sites in the Orinoco were substantial, long-term archaeological deposits containing decorated pottery bowls and bottles, thick and thin griddles, grater chips, abundant tree and palm fruits, and fish remains. The grooved and sculptured pottery was elaborate and emphasized animal rim adornos. There also were rare bottle effigies of humanoid personages with animal features. The art and life-style seem consistent with cultures that recognized animistic religion and had sedentary settlement. The Formative sequence in the Guianas seems similar to the Orinoco one begins with Saladoid cultures at at least 4200 bp. In the subsequent Barrancoid sites, evidence of mounds was found.

In Amazonia proper, the history and character of the Formative is much less well known, and the environmental determinists have even argued that the

patchy evidence for the era in Amazonia means that a period of drought made greater Amazonia inhabitable much of the Formative period (MEGGERS, 1977, 1997; NEVES 2007). However, a lack of concerted research on a particular cultural development is not the same thing as its non-existence. Most archaeological sites that have been excavated in the Lower Amazon have produced some Formative pottery, but those components have usually not been the focus of researchers' interests, with important exceptions (see below, GOMES, 2008). Furthermore, the idea that lower rainfall would force people out of Amazonia contradicts those very researchers' idea that Amazonia was inhospitable to humans because of too much rain. But most prehistoric human habitation sites are situated for convenient daily access to a water source, so the documented period of lower sea level worldwide during the middle Formative period would have diminished Lower-Amazonian river levels and groundwater levels several meters and therefore changed locations of sites in the landscape during that time. Future surveys for Formative sites will need to take those kinds of changes into consideration.

The original concept of the Formative for Amazonia was first developed by Meggers and Evans and then elaborated by Lathrap. Meggers and Evans recognized an Incised Rim Horizon in the Amazon Formative and discovered the Formative Zoned Hachure Ananatuba phase during their early work on Marajo Island (MEGGERS and EVANS, 1961). Although they had assumed that the Formative would date no earlier than the common era, the radiocarbon dating put Ananatuba at 3400 BP cal, and a Smithsonian associate at the Museu Geoldi showed that an Ananatuba site was an artificial mound and had large baked clay stoves (SIMÕES, 1969). In his upper Amazon research, Lathrap also discovered a Zoned Hachure culture, the Tutishcainyo phase. He speculated that it was 4000 bc years old but never got radiocarbon dates for it (LATHRAP, 1970).

In the Lower Amazon, I found *in situ* Formative components in three of the multicomponent sites I excavated there: Cavern of the Painted Rock, Taperinha shell mound, and Santarem city. At the Cavern, the Formative layer above the pottery Archaic shell layer had oxidized sand-tempered pottery with occasional grooved lines, along with carbonized plant remains, faunal bone, and poorly preserved human burials. From this component, we dated 8 samples: 4 charcoal pieces, a carbonized *Astrocaryum* seed, a human tooth, and a human cranial fragment, with resulting ages between c. 4000 and 3300 bp or c. 4600-3500 BP cal (ROOSEVELT, 2000B; ROOSEVELT et al. 1996). As in my results on Orinoco Formative people, the human remains had stable carbon isotope ratios typical of C-3 forest plants. At Taperinha shellmound, there was a Formative deposit of sand-tempered pottery above the pottery Archaic strata (ROOSEVELT, 1995; ROOSEVELT et al. 1991). At the multicomponent site of Santarem, below the black soil Santarem component, we found a "terra mulata", or mottled-soil, occupation with Zoned Hachure decoration with two Formative dates between c. 3500-2000 bp radiocarbon or c. 4100-2100 BP cal on wood charcoal. The pottery was similar to pottery later found by Denise Gomes at comparable ages in the lower Tapajos

(2008). The Santarem Formative charcoal had significantly negative stable carbon isotope ratios compatible with closed canopy hardwood evergreen forest, unlike that from the late prehistoric Santarem urban settlement, whose tree taxa and carbon isotope ratios indicated more open, fast-growing forest (botanical identifications by L. Newsome). We hypothesized that the mottled-soil site represents occupations by part-time swidden farmers. Food remains of the Formative components included cultivated palms, fish, tree fruits, and small terrestrial animals. The sandy sediments at Santarem did not preserve phytoliths, but an exhaustive review of the carbonized remains from the screens could identify other cultigens in the future.

Thus, in our Lower Amazon sequence, there was no lack of Formative occupations, contrary to the absent-Fomative idea of the environmental determinists, but like most other archaeologists we focused on unitary, multicomponent sites, which cannot tell about settlement patterns throughout the larger area. However, Brazilian archaeologist Denise Gomes (2008) carried out systematic river-bank transect walk-over survey for more than 30 km, near Santarem in the lower Tapajos, and uncovered 8 Formative sites of the mottled-soil type. The radiocarbon dates between c. 4300 and 2000 BP cal overlap with the ages of our Lower Amazon Formative components and those in the Orinoco and Guianas, cited above. Furthermore, her sites had substantial deposits and communal-household organization that she deemed inconsistent with environmental determinists' expectations for a slight occupation of occasional camping. Recently, some new late Formative sites have emerged at the mouth of the Baixada de Maranhao estuary in the Lower Amazon as well (GUIDA NAVARRO and ROOSEVELT, 2021).

THE MARAJOARA AND THE POLYCHROME HORIZON

For the period following the Formative, we were invited to carry out research on Marajo Island by the then Director of the Goeldi Museum, Jose Seixas Lourenco. The earliest published field researchers in the later 19th centuries had noted that there were numerous artificial mounds of varied shape and size on the eastern half of the Island (e.g., DERBY. 1879, 1897; FERREIRA PENNA, 1877, 1885, 1897; HARTT, 1971b). Their excavations had revealed both domestic refuse and numerous clusters of polychrome painted and incised anthropic burial urns, feast dishes, female figurines, and round ceremonial stools. A Brazilian botanist interested in the art wrote that the culture seemed to be highly developed and focused on women (NETTO 1885).

However, the midcentury North American researchers from the Smithsonian Institution argued that the sites were merely ceremonial mounds lacking residential occupations and therefore were not evidence of ancient population concentrations in the tropical forest (MEGGERS, 1952; MEGGERS and EVANS, 1957). They argued that the Marajoara culture had arisen from an invasion of agricultur-

alists from the Andes and had soon collapsed because of the overly hot and rainy tropical forest conditions. They pointed to polychrome sites in the Andes foothills as the source of the invasion (EVANS and MEGGERS, 1968). However, between 1965 and 1967, Brazilian collaborators of the Smithonian archaeologists got radiocarbon dates from a Marajoara mound that showed that the Marajoara culture began c.1000 years earlier than they expected and more than seven hundred years earlier than the Ecuadorian sites they had radiocarbon dated to 1200 ad, as a Brazilian archaeologists later affiliated with Lathrap pointed out (BROCHADO, 1980; ROOSEVELT, 1991 313-314). The dates made unlikely an Andean origin of the Polychrome Horizon, which dissertations by other Lathrap students at the University of Illinois at Urbana showed was a very wide-ranging pottery complex (e.g., MAGALIS HARRIS, 1975; WEBER, 1975). Thus, the Smithsonian scholars left out of their 1968 book the very radiocarbon dates dates that would have contradicted their hypotheses about the Andean origin of the culture.

Their assumption that the mounds were mere ceremonial facilities also soon was falsified in the 1970s by a Brazilian team led by geophysicst Jose Seixas Lourenco from the MPEG. They investigated the strata of a mound using magnetometers to map large concentrations of burnt clay, which they identified as large bowl- or trough-shaped domestic hearths, or stoves (ALVES and LOURENÇO, 1981). The Goeldi archaeologists associated with the Smithsonian denigrated the use of geophysical methods, according to Lourenco, so when I came to the museum to study its archaeological collections in preparation for research on the sequence in the Santarem area, he invited me to work with him to assess his hearth idea and collect samples for dating. (He told me that Meggers asked him not to give me a permit to excavate, but as he saw advantages in my coming to excavate, he did not take her suggestion.) At his suggestion, we applied for an NSF Cooperative Science grant, and Wesley Hurt of Indiana University and Ben Rouse of Yale were among the peer-reviewers who supported the project's funding.

As Lourenco's earlier work had suggested, we found there an array of large domestic hearths clusters set into the ground within clay structure floors of wide extent (BEVAN and ROOSEVELT, 1985; ROOSEVELT, 1991a, 2007; ROOSEVELT et al. 2012; ROOSEVELT and GUIDA NAVARRO, 2021). Our magnetometer map suggested that there had been a group of about 20 communal houses in the uppermost Marajoara occupation. The food and pottery of these domestic areas was very different from what was in ceremonial open areas, in size and decoration of vessels and in types of food. Thus, we found at this mound both domestic and ceremonial remains, making the mound sites definite population concentrations along with their ceremonial spaces and urn cemeteries, not the empty ceremonial centers as the midcentury archaeologists had thought. Also, with our 19 additional radiocarbon dates, we were able to confirm the dates that the Smithsonian archaeologists had delayed publishing, which gave the Polychrome mound culture a time span from about AD 300 to 1200 cal. With this dating, the polychrome culture was hardly a flash-in the-pan Andean invasion that succumbed in the terrible tropics

but rather a substantial long-lived culture that arose at the mouth of the Amazon and spread all the way to the foothills of the Andes by c. AD 1100-1400 cal.

Later research on Marajo, like Meggers and Evans' earlier research, characterized the culture as a complex chiefdom (Schaan 2001, 2004), but our own analyses did not find clear evidence of paramount rulership or social stratification but found probable evidence of graduated ranking (ROOSEVELT, 1991a). So far, all known Marajoara sites have both household occupations and ceremonial deposits, such as feasting areas and discrete urn cemeteries. In our research on human remains in museum collections, female burials are rarer than male but richer in artifacts, but no full cemeteries has been systematically excavated, so there are too few burials from each site for useful conclusions. In order to better evaluate evidence on socioeconomic variation, we plan to excavate fully two of the cemeteries in Guajara mound of the Monte Carmelo mound group, where our team topographically and geophysically mapped and then test-excavated both domestic and ceremonial areas at Guajara mound.

Some geologists have claimed that all Marajoara mounds originated as natural eminences, based on a lack of cultural material in their samples from a site. But their research on mounds was limited to very cursory surface survey, and they made only two very slender (5-cm diameter) core samples from a single mound (ROSSETTI et al. 2009). Thus, they had a completely inadequate sample to support their suppositions. Our resistivity and conductivity maps and our and others' excavations have documented at many mounds deep stacks of superimposed house floors and thick moundbuilding layers, which contain indubitable *in situ* stratified cultural features. But both house floors and mound-building layers mostly lack artifacts, so if researchers rely on only a couple of slim core samples, they are likely to miss the cultural features.

All the related polychrome Horizon phases in the Amazon are distinct regional manifestations that developed from offshoots of the original Marajoara culture of the mouth of the Amazon Islands and mainland at Belem. Although all emphasize the skin patterns of the anaconda, they often differ from Marajoara styles in how they represent gender. Female images predominate in Marajoara styles, but male images, almost non-existent on Marajo, come into the iconography more as the Horizon spreads upriver. However, in all the cultures of the Horizon, females predominate in the human figurines. Recent research by a Brazilian team led by Guida Navarro at stilt villages just south of the mouth of the Amazon in the estuarine lowlands of Maranhao extends the geographic reach of the horizon, showing that people there developed a related style of polychrome pottery and female figurines contemporary with the later Marajoara phases, between about AD 800-1100 cal (GUIDA NAVARRO, 2018, 2019).

SANTAREM, A LATE PREHISTORIC CENTER DAMAGED AND DIMINISHED BY BULLDOZER

The latest culture in our Lower Amazon sequence was Santarem, an expansive complex culture of the terminal prehistoric period. This archaeological culture centered at Santarem city is known for its elaborately modeled, incised, and often painted ritual ceramic vessels, large human figures of men and women, female figurines, musical instruments, spindle whorls, tools, and carved jade ornaments. The city site is a huge continous black soil deposit with numerous diverse constructions and features; detailed surface observations and reports on the archaeological site over more than 100 years show that it extends over at least 4 square kilometers (Roosevelt 2002; Roosevelt et al. 2010; Roosevelt et al. nd). The archaeological culture's extensive zone of influence is marked by numerous, deep, often mounded black soil sites along both the banks of the Amazon (and some interior sites on the right bank) for about 400 km upstream and downstream of Santarem city (NIMUENDAJU, 2004: pl. 200).

From its regional settlement pattern, the organization and types of the structures and features at its sites, and it nature of its varied artifacts, the culture seems to have been a chiefdom culture with shamanic animistic religion, co-rulership of men and women, and religious participation of both men and women. Habitation mounds are placed in long lines at the site and include bell-shaped pits with rich remains from feasting and fine-artifact manufacture and cremation burials. Wide but shallow clay-soil platforms embedded with many ritual offerings of special objects and sometimes burials were erected at least at one location beside a row of the house mounds. But how the overall center site was organized we do not know because of the intrusion of the historic and modern city of Santarem in the "Aldeia" part of the town and the destruction of the "Port" site of the town by bulldozer and deposition of caustic chemicals from the port operations.

It was always assumed to be a contact period-culture and accordingly was given the name of an ethnohistoric group, the Tapajo (GOMES, 2001, 2002, 2013; NIMUENDAJU, 1949, 1952, 2004; PALMATARY, 1960; BEZERRA DE MENESES, 1972). However, the historically recorded zone of influence of the historic Tapajo was much smaller than that of the archaeological culture (NIMUENDAJU 2004), and our surveys and excavations in the Port area of Santarem city indicated that the archaeological culture there was not historic in age but late prehistoric (QUINN, 2004; ROOSEVELT, 1994, 1999, 2002, 2007, 2010; ROOSEVELT et al. 2012). Deposits of the characteristic culture that we observed on the surface and excavated from bell-shaped pits for cremation burials, from black soil garbage, and from house-mound floors held no identifiable historic materials at all. Furthermore, our radiocarbon dates associated with the typical so-called "Tapajonic" pottery (QUINN, 2004) were all pre-contact, starting at c. AD 1200 cal and ending by c. AD cal 1500. These radiocarbon dates clearly pushed the culture back to prehistoric times, contrary to earlier assumptions of its historicity. Thus, the complex culture that had existed at Santarem could not have been a reaction to the period of contact with

the Portuguese but was an indigenous prehistoric development that ended either just before or after the first contact with Europeans. The impact of this change in dating is that it changes the history of indigenous complex culture in the Amazon.

In terms of methodology, we used geophysical survey to detect and map archaeological deposits over wide areas of the site. Its purpose was to locate archaeological features and structures and allow excavations of a sample of them to inform on the unexcavated deposits in the geophysical maps. But first we made a detailed topographic map of the Port site and walkover survey of the area to observe and record surface remains. The combined topo and walkover allowed us to create a series of maps of black Indian soils and vegetation.

Our stratigraphic excavations focused on major features mapped in the geophysical, topographic, and our walkover surveys produced information about what features were archaeological. The excavations showed that the black-soil mounds were residences and the adjacent bell-shaped pits were ritual termination disposals of ceremonial craft materials and food from feasts associated with cremations. Subsequent research by others at Santarem town center has shown similar results from excavations after radar survey (GOMES, 2013). As at Marajo, we found that the daily food (very small fishes and common palm fruits) was different from the ceremonial food (larger fishes, Amazon water turtles, and more varied and special fruits). Our excavations also showed that the archaeological site was a multicomponent site that had both Formative occupations and a Polychrome Horizon occupation, as could be expected based on the multiple styles of the artifacts preserved in the Santarem Museum, which I first saw in my 1980/1981 visit, include all those cultures in addition to the Santarem culture.

Unfortunately, the continued bulldozing of the site in a collaboration of the dictatorship-era Brazilian parastatal company CDP with Cargill, a very large American food corporation, made it impossible to do more than these geophysical and topographic maps and test excavations to properly investigate the deposits indicated in those maps. Each field season we would do elaborate surveys and test excavations in preparation for wider excavations in subsequent seasons, but the features we mapped and tested would be gone when we returned, removed by bulldozer. By the time we gave up, most of the Port site has been mechanically scraped down to the remnants of features and strata that extended below the main archaeological deposit. Excavations by a Brazilian team in an area adjacent to where we worked in the Port site reflects that fact in its peculiar excavation profile drawings, which record only the base of a grossly truncated archaeological deposit (SCHAAN, 2010).

It's not clear what was the overall motive of the continued bulldozing but our research over the years revealed several different purposes at different times. Managers at CDP referred to the bulldozing as "cleaning" the (dirty?) vegetation and black archaeological deposit on the site. They also mentioned their desire to create flat surfaces for the soccer games of several private sports clubs. In addition, CDP bulldozed one area for the construction of the Cargill facility.

Although both CDP and Cargill claimed there was absolutely no archaeological deposit there, our team members examined and recorded in drawings and photos the surfaces of the bulldozed excavation just before the Cargill facility was put in there. The drawings and photos documented that it had in-situ artifacts in a multicomponent deposit dating all the way from early Archaic pottery and shells, Formative pottery, Polychrome horizon pottery, and material of the Santarem culture. In its profile exposure, the bulldozer excavation even revealed a linear series of black-soil house mounds, some of which we later excavated (ROOSEVELT and GUIDA NAVARRO, 2021: 108-113).

We complained repeatedly about the continuing destruction of the Santarem site to CDP, to town officials, and to prosecutors, but it never stopped. At one point, a negotiation with Cargill led to the company providing funding for one season of our research. Administrators of CDP thereupon urged us to stop mentioning the damage to the site by them and Cargill, because of the grant, but I could not stop the criticism because it was factual and the bulldozing continued. In fact, CDP used sediment it had bulldozed from other areas of the site to cover up the archaeological deposits cut through for the Cargill facility. We found this new layer over the archaeological structures when we excavated beside the facility. So much sediment had been piled by CDP onto the archaeological deposit that we had to use a back-hoe to get down to the cultural layers.

CDP's claim for the absence of deposit by the Cargill facility was based on a report from the MPEG. The museum had a convenio with CDP to investigate the extent of archaeological deposit there but one of the archaeologists involved had family links to CDP, which is a conflict of interest. Whether because of those links, poor methodology, or just bad luck, the report from that work stated that their testing showed no archaeological deposit by the Cargill facility (GUAPINDAIA, 2001). But over that entire area certified as lacking archaeological remains, we subsequently found deep, intact archaeological deposits including domestic and ceremonial structures and features: a wide artificial earth platform with multiple Santarem offerings and a burial and row of black-soil house mounds. The house mounds had actually been cut in half in the cross-section of the bulldozer cut for the Cargill facility and can be clearly seen in our photos (ROOSEVELT and GUIDA NAVARRO, 2021: 108-109; ROOSEVELT et al. 2010). These rows of house mounds were the same as those we mapped and sampled in the site across the Avenida Cuiaba, and their presence showed that this housing pattern was repeated at the site. Thus, the neglect of MPEG's archaeologist to detect these major archaeological constructions, whether because of poor method, family links, or the institution's need for funding, would have limited understanding of the social organization of a site but for our persistence in looking under the surface.

The debacle of the Santarem site means that the center of one of the most influential, culturally and conceptually rich complex cultures of the Amazon will never be properly mapped, excavated, and analyzed because most of it has been willfully destroyed, and no-one powerful enough to stop the destruction did so.

CONCLUSION

The important thing to recognize about methodological and ethical problems in archaeology is that unless dealt with frankly in print they can end up deforming the picture of culture history and cultural development of an area by obscuring the empirical patterns on which inference and interpretation must be based. The problems I've mentioned in this article have been committed both by individuals and by institutions. The Smithsonian archaeologists who several times tried to limit others Americans' excavation permits, suppressed information about their own results and misstated others' information, were working for the main US government scientific museum and should have known and done better. In some cases, the limitations they placed on their Brazilian collaborators' publishing meant that important discoveries the collaborators could have announced were made by foreigners, such as myself. In archaeological dating, all dates must be published and accounted for because, regardless of how the submitting archaeologists feel about the dates, they are relevant to the interpretation of sites, their contents, and their stratigraphy. The recent role of the Goeldi and some of its researchers in obscuring archaeological information both at Monte Alegre and Santarem contrasts with its very beneficial role when it began under Ferreira Penna, a great and influential pioneer and proponent for Amazonian archaeology and its long sequence of human occupation. Some archaeologists hesitate to speak out about such individual and institutional ethical problems out of a misplaced sense of decorum or for fear of giving offense; others don't recognize the problems because of the others' silence. But if archaeologists don't speak and write about how archaeological research should be conducted for particular research problems, our discipline will never improve. Though criticism stings, all of us learn from critiques of our work, which we regularly encounter in anonymous peer reviews of proposals and manuscripts and in published reviews of our works. Throughout our careers we develop our methods by reacting both to how things worked in our past research and to suggestions arising from criticism. And if archaeologists do not call out ethical offenses in access to excavation permits and in the presentation of archaeological results when they learn of them, then factual patterns are obscured that would have resulted in very changed conclusions if revealed. Therefore, the clearing up of both methodological and ethical problems through transparency, critique, and open debate is a healthy process with positive effects on our discipline.

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