

12-15-2013

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Recommended Citation

Carneiro, Robert L. (2013) "Reminiscences of a Stalwart Adversary," *Andean Past*: Vol. 11 , Article 5.

Available at: https://digitalcommons.library.umaine.edu/andean_past/vol11/iss1/5

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REMINISCENCES OF A STALWART ADVERSARY

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Betty Meggers and Clifford Evans. Photo courtesy of Barbara Watanabe, Smithsonian Institution

Betty Meggers was, without a doubt, the most commanding figure in the history of Amazonian archaeology. Her ideas about the limita-

tions on Amazonian cultural development imposed by a tropical rain forest environment, so often and so vigorously asserted, became her

cause and she pursued it throughout her career (Meggers 2001). For decades it remained, if not the dominant view of Amazonian prehistory, at least a view to be contested. Even today her thesis is sometimes referred to as the “standard model” when Amazonian prehistory is discussed (Heckenberger *et al.* 1999:353-354). Gradually, though, this view became more and more a minority opinion, and by the time of her death the minority consisted of just one person, Betty herself.

As an Amazonian ethnologist, but one very interested in the prehistory of this region, I crossed swords with Betty Meggers more than once. My first encounter with her occurred while I was still a graduate student at the University of Michigan, where she herself had studied. Indeed, at Michigan we both had the same mentor, Leslie White, a fact that became relevant to our relationship a few years later.

In 1953, while carrying out field-work among the Kuikuru of the Upper Xingú region of central Brazil, I came upon a trench which ran for about a mile and a half, enclosing and defending a settlement site which had probably once been a sizable village. Excavating this trench must have required a larger labor force than the Kuikuru of the mid-twentieth century could have mustered. It also must have demanded stronger political leadership than that of the Kuikuru. In short, it seemed to me that the trench was mute evidence that a more complex culture than that enjoyed by the Kuikuru had once existed in the Upper Xingú.

This surmise about the higher culture of the “Trench Diggers” was being incorporated into my dissertation (Carneiro 1957). Somehow, and I never learned how, Betty Meggers got wind of what I was saying, and complained about it to my dissertation committee. In her opinion, I was engaging in unwarranted speculation. She made no bones about making her objections known.

Because Meggers had not communicated with me personally, I saw no need to respond to her directly. That would come later. But from that point on, and for half a century thereafter, it was clear that Meggers and I were adversaries. However, unlike her relations with several other critics, our dispute never rose to the level of personal animus.

My next encounter with Meggers occurred a few years later, in 1958. Gertrude Dole and I were putting together a *festschrift* for Leslie White (Dole and Carneiro 1960), whose student, as I’ve already indicated, Meggers had also been. Trudie and I felt that the papers being submitted for that volume would benefit from being read and commented on by an editorial board, and we invited Meggers to serve on that board. However, she declined, stating that she was sure that we wanted a bunch of “yes men”, and insisted that she had no interest in playing that role. We assured her that that was not the case, that we wanted individuals who would give their frank and unvarnished opinions about the papers. In fact, we added, that was precisely why we wanted her as an advisor. Convinced, apparently, that we meant what we said, Meggers answered by postcard with a single word: “Okay.”

In 1954 Meggers’ landmark article “Environmental Limitation on the Development of Culture” appeared in the *American Anthropologist* (Meggers 1954). In it she gave full-throated voice to her conviction that the tropical rain forest was unsuited for the emergence of higher culture. In support of this view she claimed that, with one exception, nowhere in Amazonia had native peoples gone beyond the level of simple, autonomous villages. For the one exception she was willing to countenance, Marajoara, she had a ready explanation.

Marajoara culture, known only archaeologically, is characterized by elaborate ceramics, suggesting a degree of cultural complexity distinctly above that of the rest of Amazonia. Meggers declared, however, that Marajoara was intrusive, having originated outside of Amazonia. Its place of origin she located somewhere in the Andes where conditions for its rise were, according to Meggers, more propitious. After having arisen in the Andes, it had quickly been carried down the Amazon by peoples unknown, coming to rest on the island of Marajó at the mouth of that river. Here, for a time, Marajoara continued to produce the ornate pottery for which it became famous.

However, Marajoara's florescence was to be short lived. Unable to cope with a rain forest environment, the culture was soon watered down to a "Tropical Forest" level of small, simple villages. Gone were the fancy ceramics, to be replaced by an undecorated ware. Gone, too, was its original political structure, surmounted by a paramount chief who had presumably fostered the great ceramicists, replaced by simple village headmen.

It was not long, however, before Meggers' vision of the Amazon's limitations was challenged. In a letter published in the *American Anthropologist* in 1957, Richard I. Hirshberg and Joan F. Hirshberg applied the rules of logic in questioning Megger's law-like statement of "Environmental Limitation on the Development of Culture" (Hirshberg and Hirshberg 1957). Next was Edwin Ferdon, who argued that tropical forest soils were not as impoverished as Meggers had claimed (Ferdon 1959).¹ I was also in line to challenge Meggers, doing so in a paper delivered at a symposium on Amazonia that took place at a meeting of the American Anthropological Association in Mexico City in

1960. Citing archaeological, ethnohistorical, and ethnological evidence that Meggers had either overlooked or glossed over, I argued that chiefdom-level cultures had not only originated in Amazonia but had lasted there from their inception until the arrival of Europeans. Only after the latter had decimated native societies did these higher cultures disappear, leaving in their place the now-familiar simple Tropical Forest cultures. Meggers was present at that symposium, serving as a discussant, but today, half a century later, I have no recollection of what she might have said in defense of her thesis.

The papers from that symposium were later published. My paper was among those included, being entitled "Slash-and-Burn Cultivation among the Kuikuru Indians and Its Implications for Cultural Development in the Amazon Basin" (Carneiro 1961; see also Dole and Carneiro 1960). Meggers' comments, however, did not appear in the volume, most likely because she chose not to submit them. Whatever remarks she had made as a rejoinder to my arguments were no doubt later repeated many times over in Meggers' subsequent publications.

Over the years, I have had occasion to express and elaborate my objections to Meggers' ideas about Amazonia. On my side of the argument I was later joined by several other Amazonianists, most notably the archaeologists Donald Lathrap (Meggers 1971a) and Anna Roosevelt (Meggers 1992). Meggers' polemics with these two reached a level of incandescence that hers and mine, I am happy to report, never did.

My next face-to-face encounter with Meggers came late in the 1980s during a *fest-schrift* conference in honor of Elman Service held at the University of Michigan (Rambo and Gillogly 1991). Participating in that conference were persons who had been associated with

¹ For an obituary of Edwin Nelson Ferdon, Jr. by Earl H. Lubensky see *Andean Past* 8 (2007), pp. 9-32.

Service or been influenced by him, including Meggers and myself. My paper dealt with the now-familiar question of what level of culture had native Amazonian societies been able to attain on their own. But before recounting what I said on that occasion a certain amount of background is required.

Several years earlier, Meggers had published a slim volume entitled *Amazonia: Man and Culture in a Counterfeit Paradise* (Meggers 1971b). The “counterfeit paradise” was, of course, Amazonia. Throughout most of the book Meggers restated her accustomed position regarding the limitations imposed on native cultures by the rain forest. Near the end of the book, however, she introduced a chapter whose tone was strikingly different. This chapter was entitled “Aboriginal Adaptations to the Várzea,” *várzea* being the banks of the larger rivers which were enriched every year by fresh layers of silt deposited by the overflowing rivers.

Citing ethnohistorical evidence, Meggers showed in that chapter that at the time of Orellana’s voyage down the Amazon in 1542 the Omagua, who lived on the upper reaches of that river, were a flourishing chiefdom. Located squarely on rich *várzea* land, the Omagua enjoyed good harvests of manioc, supplemented by ample catches of fish from the bountiful waters of the Amazon. Nowhere in the rest of the book, however, did Meggers come out and declare, “I hereby recant the view I have previously held that higher cultures could not arise in Amazonia.” To me, this admission, even if only implied rather than openly stated, was clearly the most memorable feature of the book. Yet, the reader was left to ponder what lay behind Meggers’ unexpected and unexplained *démarche*.

In the paper I was to present at that conference, I intended to begin by discussing Meggers’ oft-repeated views on Amazonian prehistory and then turn the spotlight on her unacknowledged

change of course. However, I knew Meggers would be in the audience, perhaps even sitting in the front row, and I did not wish to appear unduly harsh in my remarks. Thus, I was not going to say, for instance, that in the intervening years between her first pronouncements on the subject and the *várzea* chapter in her latest book, *she had come to see the error of her ways*. No, I intended to choose my words very carefully, but again, before proceeding to tell just what I did say, I need to provide some additional background.

After getting her masters degree at Michigan, Meggers went on to Columbia University for her Ph.D. There she was a student of Julian Steward, who had recently moved to Columbia from the Smithsonian. At some point. I don’t recall just when, I began to suspect that the argument about the limitations of a rain forest environment, an argument Meggers’ advocacy had made world famous, did not originate with her. It was, I decided, a view she had *absorbed* from Julian Steward! Despite having edited the *Handbook of South American Indians*, Steward was not, in my opinion, a bred-in-the-bone, dyed-in-the-wool South Americanist. Certainly he was not to the extent that Alfred Métraux, who made extensive contributions to the *Handbook*, was. Somehow Steward had come to believe that Amazonia could not give rise to, or support, a relatively high level of culture.

Steward’s explanation of the emergence of high culture in South America was that it had originated in the Andes, where conditions for its rise were more favorable. Then, some bearers of this culture had broken away from their Andean homeland, swept across the northern coast of South America, circled around the Guianas down to the mouth of the Amazon, and finally penetrating up that river into the heart of Amazonia. However, the environment encountered along the way proved too difficult and the higher culture was forced to decline to the

Tropical Forest level (Steward 1949:752). This theory, incidentally, was later put to the test by Irving Rouse and refuted on the basis of archaeological excavations carried out by Rouse himself in northern South America (Rouse 1953).

Returning at last to Betty Meggers, it had long seemed to me that in her early reading on Amazonia she had somehow failed to familiarize herself with de Carvajal's account of Orellana's voyage down the Amazon (Carvajal 1955 [c. 1542]). Had she done so, had she thus been made aware of the chiefdoms Orellana had encountered on his journey, including the very same Omagua whose complexity Meggers was now trumpeting, she would never have espoused the theory with which her name will forevermore be linked.

So again, here was my dilemma: I felt the need to point out the discrepancy between Meggers' long-standing pronouncements, the result of her early failure to read de Carvajal, and her 1971 portrayal of the Omagua as a true chiefdom (Meggers 1971b). However, I wanted to do so delicately. After much thought, I finally told my audience that between 1954 and 1971 Betty Meggers "*had broadened and deepened her knowledge of Amazonian prehistory.*"

It was, evidently, a happy choice of words. At least I can report that later that evening, as I walked Betty back to her campus lodgings, while we didn't exactly hold hands, the conversation between us was decidedly civil.

That is as much as there is to relate about my personal dealings with Betty Meggers. I long since ceased to be her principal *bête noire*, my place having been taken by Donald Lathrap. With Lathrap's death, the mantle passed to Anna Roosevelt. More recently, though, it seems that Michael Heckenberger came to play that role. I say this because Heckenberger's archaeological work in the Upper Xingú

(Heckenberger 2005; Heckenberger *et al.* 1999, 2003) had more than confirmed my earlier surmise, the surmise that had ruffled Meggers' feathers years before, that a significantly higher culture had once flourished in the Upper Xingú than any observable there during the twentieth century.

Finally, I would like to say a few more words about Meggers' familiar argument and where it stands today. Recent archaeological work has gone far to invalidate her contention that chiefdoms could not arise, nor long endure, in the Amazon basin. For one thing, the duration of Marajoara, which Meggers had originally put at less than a century, turned out to be several centuries (Schaan 2001). The new dates, of course, proved fatal to her belief that nowhere in Amazonia could such a culture be sustained for very long.

Moreover, recent excavations, especially those at the confluence of the Rio Negro and the Solimões, have disclosed the existence of a large settlement site (Heckenberger *et al.* 1999), quite possibly the seat of a chiefdom once seen by Orellana as he passed by this spot five centuries earlier.

Around the modern city of Santarém at the mouth of the Tapajós, continuing archaeological surveys have revealed an extensive area of Indian occupation, suggesting that a very large village, perhaps deserving to be called at least a town, once existed there (Gomes 2001). But Meggers chose to interpret these and other extensive prehistoric sites, against the opinion of most Amazonian archaeologists, as being the result of successive but discontinuous occupations, with small settlement sites having overlapped, creating the false impression of a single large site. Her hope was that this interpretation would negate the prior existence of a complex culture at such sites, something which a single

extensive settlement site would have strongly suggested.

Over the last several decades in fact, the evidence, both ethnohistorical and archaeological, has continued to mount that the thesis Meggers so ardently and tenaciously embraced has long since lost acceptance among archaeologists (Smith 1980; Woods and McCann 1999). Soon, I predict, no one discussing Amazonian prehistory will again refer to it as the “standard model”.

Death has at last silenced the proud voice of Betty Meggers, which for more than fifty years staunchly defended a view that other Amazonianists grew increasingly to abandon. In broad perspective, I see Meggers as having had the misfortune, at the very start of her career, of mounting the wrong horse and never being able to dismount. Of course, to the last, she failed to see, or at least was never ready to concede, that it *was* the wrong horse. She rode it valiantly to the very end. To those of us who disagreed with her, and took up arms against her, she proved to be a redoubtable and indomitable opponent. Doing battle with her forced us all to sharpen our weapons and to wield them more effectively. And that, I submit, was no small contribution.

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