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Ambiguity and Debates on the Early Peopling of South America

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An evaluation of recent claims for early human settlement of South America is presented. Some of the problems with these cases are reviewed, particularly the ways in which ambiguity weakens otherwise compelling evidence of early human presence in the continent. The roles of generalized adaptations and cobble industries, the most common explanations of claims of early occupations, are examined, and some new sites that present incomplete evidence but are deserving of further research are mentioned. The incorporation of studies of formation processes in the future may prove helpful in evaluating most of these cases as well as others that emerge in the future.

Keywords South America, ambiguity in archaeological evidence, taphonomy

1. Introduction

The debate about the peopling of America has been significantly transformed during the last few years, mainly due to the increasing use of sophisticated analytical techniques, including the incorporation of molecular information, more focused field work, and the acceptance of ages older than 14,000 calendar years ago (cal yr BP). New classes of sites are being excavated and new research problems are emerging. However, a persistent difficulty is the appearance of ambiguous cases of early archaeological sites and how they are handled. This difficulty is not unknown in North America (e.g., Fiedel 2002; Haynes 2015). Besides the many robust and well-substantiated claims for early human occupations in South America (i.e., Jackson et al. 2013; Politis et al. 2014; Rademaker et al. 2014; Sandweiss and Rademaker 2013; Suárez 2011, 2015), there are also ambiguous claims which sometimes divert attention into useless skirmishes. Needless to say, these ambiguities do not contribute to our understanding of the process of human colonization. Three new sites in particular come to mind in this regard. Arroyo Vizcaíno, in Uruguay, is a bone assemblage of *Lestodon armatus* and many other extinct-animal taxa as well as “a small piece of translucent silcrete” with “macroscopical features compatible with a scraper,” together dated ca. 34,000 cal yr BP (Fariña 2015) (Figure 1). Also, Vale da Pedra Furada and Toca da Tira Peia in Brazil represent a series of sites where long sequences of cobble tools have been found going back in time to 50,000 or more cal yr BP (Boëda et al. 2013). These three sites

have recently taken center stage in the evaluation of the earliest evidence for humans in South America, but for two reasons we have not learned much from their discussion. In the first place, the debate has been narrowly focused on chronology per se; and secondly, most of these studies fail to incorporate formational and taphonomic considerations. A third problem can be added, too — the difficulty of the dialog with researchers convinced that final resolution rests on authority.

2. Age and models

Most people accept that the ages obtained at the most secure South American sites are minimal dates for the colonization of the continent and, if anything, that we have underestimated the antiquity of the process. Since the Paleoamerican Odyssey conference in Santa Fe, New Mexico, in October, 2013 (Graf et al. 2013), the discussion concerning the peopling of the Americas has changed direction. Beyond the classic discussions about how much time before Clovis is acceptable (Haynes 2015), we are more and more discussing the specific adaptations of the early peoples. Hypotheses no longer need be exclusively focused on chronology, which should only be another important variable. Instead, a preoccupation for explaining the processes of exploration and colonization offers more useful challenges (Anderson et al. 2013; Borrero 2015a; Madsen 2015). This is a step in the direction of developing more complex models and theories about the infilling of the continent. In the end, demonstrations of age are relatively easy to sustain on the basis of standard dating techniques and good contextual information. In fact, chronology is not problematic at

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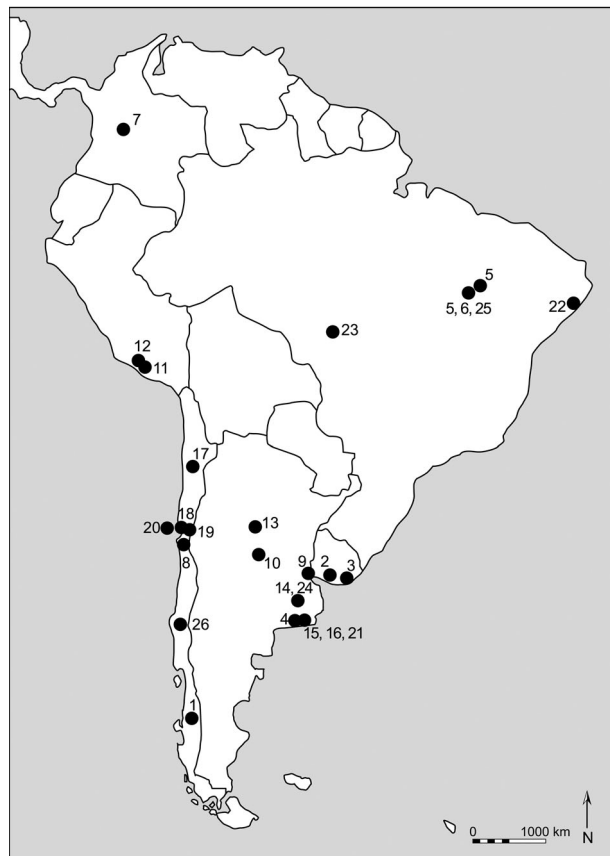


Figure 1 Map of South America showing locations of archaeological sites mentioned in the text (1, Río Ibañez 6 (west and east); 2, Urupez II; 3, Arroyo Vizcaino; 4, Arroyo Seco 2; 5, Vale da Pedra Furada; 6, Boqueirao; 7, Pubenza; 8, Laguna Tagua-Tagua; 9, Arroyo Frias; 10, La Candonga; 11, Cuncacha Cave; 12, Pucuncho; 13, El Alto 3; 14, La Moderna; 15, La Tigra; 16, Arroyo Chocori; 17, Quebrada Mani; 18, Santa Julia; 19, Valiente; 20, Quintero; 21, Pehuen-Co; 22, Sergipe; 23, Santa Elina; 24, Campo Laborde; 25, Piauí sites; 26, Monte Verde).

today's most discussed sites. Arroyo Vizcaino, as well as the Piauí sites, do not present important chronological problems (Fariña et al. 2014; Feathers 2014). What they do have, though, are archaeological insufficiencies. Additionally, a discussion of the process of regional adaptation indicated by tools and other finds, especially in relation to the distribution of raw materials or prey, is usually lacking.

For years it was maintained that some of the more criticized sites were attacked just because they were proposed to be older than acceptable, and it has often been said that sites younger than 14,000 cal yr BP have been accepted without being subjected to the same intense scrutiny (Boëda et al. 2013, 446). I do not believe this to be true. Most of the critiques have not focused on the ages of the sites (Borrero 2015b; Suárez et al. 2014). The recent publication of the Río Ibañez 6-West site in Chile, a late Holocene site in Patagonia, is an example. The existence of “a previously unrecognized lithic industry featuring the

production of a range of tools from coarse volcanic rock actually derived from the wall of the shelter” was presented (Prentiss et al. 2015, 112), as well as the situational conditions under which early humans used this lithic industry. The tools discussed are crude and different from previously known tools prevalent in the region (Mena 1999, 2013). It is too early in the game to make final conclusions about this evidence, but it is clear that this is an ambiguous case that will continue to be discussed in the years to come. The fact that the rocks used for the tools are from the same geological formation as the shelter was recognized as a problem in other cases (Lynch 1974; Meltzer et al. 1994; Yataco 2013), and it clearly will require regional analysis to be corroborated. This is not to say that there is necessarily something wrong with this industry. It only means that ambiguity needs to be removed by continued research. As I said, this is a late Holocene site and the problem is not with the chronology.

Different theories exist about the origin of the first colonizers of North America. The classic Bering route is now considered alongside alternative routes like that of a Solutrean dispersal (Stanford and Bradley 2012) and a Pacific coastal route (Dixon 2001; Erlandson 2013). Also, alternative terrestrial routes are considered (Anderson et al. 2013; Goebel et al. 2008; Ives et al. 2013; Madsen 2015). All are centered on the relationship between the geography, human populations, resources, and specific adaptations indicated by the archaeological record. For example, recent discussions concerning the Solutrean hypothesis focused on the importance of haplogroup X2a in America (Oppenheimer et al. 2014; Raff and Bolnick 2015), or the logistical problems involved in crossing the Atlantic Ocean (Walker and Clineck 2014). Technological discussions concerning the presumed markers of a Clovis–Solutrean affinity (Eren et al. 2013; Lohse et al. 2014), or the credibility of the tool and the mastodon bone recovered at the Cinmar site, at 74 m below sea level in the Chesapeake Bay (O’Brien et al. 2014), are also being discussed. The search for sea-going colonizers following the Pacific coast has been so far tackled from the feasibility side, by considering the existence of ice-free coasts with abundant resources (Dixon 2001; Fedje and Matthews 2005), its coherence with available molecular information, and the existence of coastal occupations with similar ages to those of the interior (Erlandson 2013). Of course, chronology is part of all of these discussions, but it is not necessarily the focus. Nonetheless, it is clear that the available molecular evidence points to Asia as the origin for early American populations (Raff and Bolnick 2015; Raghavan et al. 2014; Rasmussen et al. 2014), a

situation that invites dismissal of the European alternative predicted by the Solutrean model.

The situation about the peopling of South America is different from the northern continent. Several important ecological and geomorphological distinctions between the peopling of North America and the peopling of South America have been advanced in the literature, some of the most prominent differences being the minimal extent of glaciers in South America (Dillehay 2000) and the extensive lowlands of Amazonia (Clapperton 1993). But there are other even deeper contrasts, including that the peopling of South America is a “subcase” of the peopling of the Americas. In other words, we may discuss the Solutrean or the Pacific coast hypothesis for North America or for the Americas in general, but there is no gain in postulating them exclusively in relation to South America. Simply put, the dispersal of humans into the southern continent is secondary to their dispersal in North America. Since the 19th century when early scientists like Ameghino (1915 [1880–1881]; see also Podgorny 2015) suggested a human origin in the Pampas, or since the mid-20th century when Rivet (1943) entertained ideas of an independent colonization of South America from Australia, no serious hypothesis of south to north dispersal has been formally presented. Discussion of the early archaeology of South America focusing on chronology distracts from considerations of process, technology, and settlement.

The differences between North America and South America suggest that we should not rely on North American standards to judge southern evidence (Borrero 2006). One classic archaeological association present in many early sites in North America is between megamammals and specialized tools, which sometimes are seen as mutual requirements for proving pre-Clovis archaeology in the north. This is not necessarily the case in South America, where there are many examples demonstrating that late Pleistocene archaeology may exist without remains of large mammals. In fact, the importance of megafauna at early sites is usually quite minimal, and many tool kits from early southern sites can be considered generalized rather than specialized (Dillehay 2000).

A use of local rather than higher-quality “exotic” lithic sources (Borrero and Franco 1997) also appears to point to an important difference with North America, and this has important implications for differences in land use, too. The role of megamammals appears to have been minimal in South America, and changes in subsistence are evident in each region of the continent. Recent evidence from eastern Brazil (Araujo 2014; Feathers et al. 2010; Hubbe et al. 2013; Schmidt Dias and Bueno 2013), for example,

indicates that it was populated by colonizers which did not interact much with large mammals — which in some places survived into the Holocene — a situation similar to that of the Pampas (Gutiérrez and Martínez 2008) and other environments in South America (Borrero 2009). Under these conditions — the necessity to shift resource exploitation — it is possible that the process of colonization required the definition of new human niches. This situation is completely different from that defined for North America by Kelly and Todd (1998, 235) when they presented their compelling peopling model.

3. Generalized adaptations?

One important issue in South America concerns the degree of sophistication of its early industries. Several authors have insisted for years the existence of widespread unifacial industries (Bryan 1973; Dillehay 2000), a generalization disputed by some, including myself. I must accept, however, that the idea is stronger than I previously thought. Evidence from the Sabana de Bogotá and other places in Colombia were always important in this discussion, but unfortunately both the chronology and descriptions of the early lithic assemblages of northwest South America are still incomplete. Nonetheless, recent syntheses of early lithic assemblages in these regions indeed show an abundance of unifacial tools, even when their distribution (Aceituno and Rojas-Mora 2015; López-Castaño and Cano-Echeverri 2013) does not preclude those assemblages from being components of larger more sophisticated tool kits. Unifacial industries in northeast Brazil during the Pleistocene–Holocene transition are more homogeneous (Lourdeau 2015), but there are also reasons to be cautious about the exclusivity of unifacial industries. Using debitage analysis, Nami (1993–1994) demonstrated the presence of bifacial reduction at early sites for which bifacial forms were not recovered. One particularly interesting case is that of the archaeological locality Laguna Tagua-Tagua in Chile (Jackson et al. 2013). Tagua-Tagua 1 yielded an association of mastodon, horse, and other mammals with lithic tools among which no bifacial points were found; standard radiocarbon assays suggested an age of ca. 13,000 cal yr BP (Montané 1968). Decades later, another site located some 700 m from Tagua-Tagua 1 was excavated. There, at Tagua-Tagua 2, an association between similar fauna and tools was dated to ca. 12,000 cal yr BP, but there the lithic assemblage included two Fell Cave, fishtail projectile points (Núñez et al. 1994). With small samples, as those found at most of the early sites, we must be cautious with our technological characterizations, since our regional pictures are still very incomplete in South America.



Figure 2 General view of 2015 excavations at Arroyo Seco 2. Photograph by Daniel Rafuse.

Another interesting case is that of the Pampas, where the recent publication of the monograph of the archaeological excavation at Arroyo Seco 2 (Politis et al. 2014) has clearly demonstrated the association of megamammals with very generalized tools in the Interserrana region (Figure 2). Together with the evidence from sites like La Moderna and Campo Laborde (Politis and Gutiérrez 1998; Politis and Messineo 2008), a regional pattern of simple lithic technologies with minimal bifacial work has emerged (Politis and Messineo 2008). Is the evidence from Arroyo Seco 2 and other sites an indication of a specific adaptation, or do they represent seasonal or structural components of a larger adaptation? It is well known that bifacial assemblages are important in other areas of the Pampas, as attested by sites located on the sierras of Lobería and Tandilia (Flegenheimer et al. 2003; Mazzanti and Quintana 2001). The idea of a functional connection between the sites discovered in these two areas is plausible (Politis et al. 2009, 117). Also, interactions with Uruguay, on the opposite bank of the La Plata River — a less important water course during the late Pleistocene and early Holocene (Bracco et al. 2011) — have been proposed. Similarly, in many regions of South America we are pondering the point to which different industries are components of a complicated technological mosaic. Only with a systematic regional analysis will we be in a position to evaluate this and other alternatives concerning past technology and settlement (Sandweiss 2015). The existence of generalized industries in South America remains an open case.

4. Comparisons

Extra-American comparisons are sometimes used to substantiate the presence of cobble industries in northeast Brazil, which can be seen as one form of generalized adaptation. Lacking is a discussion of the conditions under which those specific tools were selected and used at each region. In comparison, the existence of winter constraints in the access to raw materials was the basis on which the existence of a crude industry at Río Ibañez 6 (Este) was suggested (Prentiss et al. 2015). Instead of doing this, most of the support for the American cobble industries is founded on comparisons with other continents.

There is nothing wrong in using information from different continents for comparisons, but at some point the local history of adaptation needs to be tackled. We must discuss how people were tuned to the needs of different social and ecological environments, and how they learned to deal with previously unseen situations. In the end, the comparisons per se are not useful as proofs of anything, including geographical or historical contacts. They are just good starters for discussion. One of the most innovative books about the peopling of the Americas — Dillehay and Meltzer (1991) — used all classes of comparisons to suggest ways to redirect the search for early evidence. One example concerns the discussion of the archaeology of Boqueirao, in northeast Brazil, for which the following comment has been offered, “The cobble industries that persisted in east Asia for more than a million years reflect original technological solutions” (Boëda et al. 2013, 446). This comparison between South America and Asia is not useful. A technological solution only reflects the circumstances of a particular

regional problem, in this case the problems posed by social and ecological environments in Pleistocene east Asia. Some effort needs to be put forth to show that the problems there are at least similar to those of late Pleistocene South America. Without such an analysis, there is no relevance for the Brazilian case.

It was recently suggested that *Homo sapiens* probably was not the author of the industries persisting for more than one million years in Asia, as invoked by Boëda and collaborators (Borrero 2015b). Surprisingly, however, Boëda (2015, 27) explicitly sustained that only *H. sapiens* was involved in the creation of that ancient east Asian archaeological record. Probably there is some semantic misunderstanding here, since the hominin record of southeast Asia indicates the presence of *Homo erectus*, archaic humans including *Homo floresiensis*, a still poorly understood hominin found in the Philippines, and of course *H. sapiens* (Bae 2012; Boivin et al. 2013; Mijares et al. 2010). Yi (2015, 259) even refers to marginal areas of Eurasia, specifically Korea, that “provided pockets of sanctuaries for premodern groups during that period of rapid *H. sapiens* radiation.” Beyond the specifics of the record from southeast Asia, consideration of evidence from different continents and probably different hominins certainly invites discussion. Indeed, cobble industries are within the adaptive repertoire of *H. sapiens*, as part of what I call latent or sleeping technologies (Borrero 2011). Its existence as a component of more complex lithic assemblages is also well known and was emphatically observed by Bird (1965) decades ago. However, I see no reason why such cobble technologies should have been the only technological “solution” of early South Americans for many thousands of years.

5. Ambiguity

Ambiguity is usually found by professional archaeologists interrogating an unknown world, where sometimes it is not easy to find and recognize tools and adaptations. It is to be expected that the past is different in many ways from our own experience, and ambiguity is the mark of such differences. The existence of ambiguity invites research destined to clarify the situation. The application of scientific methodologies to such evidence should remove at least part of the ambiguity and point to the limits of what can be accomplished and sustained with the evidence.

One vexing question concerning the long sequences of cobble tools proposed for South America is why a generalized industry was maintained for more than 50,000 years (Guidon et al. 1994). The Boëda team refers to “a continuum of a tool type that transcends time and reappears in successive periods with slightly different characteristics” (Boëda et al. 2013, 456). This is a fair description of the situation at Pedra

Furada and other sites in northeast Brazil. For North America, Haynes already commented, in reference to the Topper site in North America: “the fact that the materials are technologically unchanged for over 35,000 calendar years seems unusual in the late Quaternary prehistory of *Homo sapiens*” (Haynes 2015, 141). This was also my comment for Pedra Furada two decades ago (Borrero 1995). Long-term continuity of a simple technology certainly needs to be explained, especially in a modern human context. The evidence from archaeological cultures in east Asia are not relevant for the eastern South American industries, because they explain nothing about their occurrence and persistence in Brazil. Some mechanism should be provided to understand the reasons behind this technological and typological persistence through the millennia. What are the models within which the evidence from these sites in the Piauí make sense? If we presume to have evidence of techno-typological stasis in America, as claimed by Boëda et al. (2013), all of the problems that are implied by that acceptance should be specified. Instead, what we have is a long list of circular confirmatory cases. The lack of recognition of ambiguity weakens even good evidence, and the Piauí sites are an example of this situation.

Any model of cultural transmission will predict technological and typological changes for a period measured in thousands of years. Technological responses are, of course, facultative, but climatic, ecological, and social conditions affect the ways in which tools are selected and transformed by people. It is difficult to expect that these properties remained unchanged for thousands of years. Climate and environment in eastern Brazil certainly changed sufficiently within the last 40,000 years (Clapperton 1993; Ruiz et al. 2004; Tsoar et al. 2009) to expect changes in the distribution and availability of resources, in turn leading to human adaptation. How are these changes tracked by the archaeological record published at Piauí?

In dealing with ambiguity, the central question becomes how to invest our limited research resources. Which cases are worth us spending our time? It is clear that we do not always need clear geological context, as the work of Anderson and Faught (1998) demonstrated. The distribution across the northern continent of ancient “bona fide” tools is an adequate way to produce archaeological patterns which are relevant for the process of human colonization. The recent publication of artifacts presumed to represent ancient occupations in northeast Argentina, but which lack good stratigraphic context (Castro and Terranova 2015), is a good southern example of this. The fact that there was neither contextual evidence nor chronology in this case should not be an obstacle for us to

consider these as suggestive findings. Our evidence for the early peopling of northeast Argentina is minimal, so that new evidence should be pursued. Archaeology is not in a position to ignore evidence because the context is not good. This is basically the respectable position taken by Bracco Boksar (2015) in response to published critiques of the findings from Arroyo Vizcaíno, including my own (Borrero 2015b; Suárez et al. 2014).

Ambiguity does not automatically translate into “bad evidence,” but it is a condition that requires discussion. When few lithics are recovered, as in Arroyo Vizcaíno, it becomes increasingly important to see if they differ from the local background noise. Are they rocks that are found exclusively at the bone bed or do they characterize the microregion? This is just one of the basic questions that can help dispel ambiguities.

6. Formation processes and taphonomy

Ambiguous or not, most of the sites discussed here were carefully excavated. Anyway, standards of excavation at the Piauí sites, as in other sites excavated by French teams, are very high (Lahaye et al. 2013; Vilhena Vialou 2011). However, this does not translate into “problem-free” sites. The problem is not with the excavation techniques; it is with the idea that careful excavation solves all archaeological problems.

One such problem is the identification of tools. Boëda and collaborators (2013, 459) sustain that “nature produces objects randomly.” However, nature can be systematic in creating pseudo-artifacts, even “taking into account” raw material, size, shape, and location of pseudo-edges as criteria. Andrefsky (2013) presented criteria to help separate tools from pseudo-tools for a specific coarse-grained chert in a given region. Similar studies are required for other regions and rocks (Borrazzo 2011). The recent claim that manuports are also present at the Piauí sites (Boëda et al. 2014, 931) will obviously require additional taphonomic discussion of the lithic background noise (see De la Torre and Mora 2005). The presence of a “mega-structure” or “superstructure” formed by only “two orthogonally oriented blocks” is unconvincing (Boëda et al. 2013, 459). Haynes (2015, 138) referred to a widespread position when he wrote that “Many archeologists may not be convinced that these specimens were actually made by humans.”

The claim made by Lahaye and collaborators that at the Piauí sites “it is impossible to confuse natural breakage and human production” (Lahaye et al. 2013, 2843) has so far not been sustained. If we are not ready to accept that there is potential ambiguity in distinguishing natural from cultural breakage, then there is no room for a useful discussion. Having other sites in mind, Leland W. Patterson was very clear when he wrote that “Fool proof methodology is

probably not possible, but studies of lithic collections supposed to have been created by humans at a very early time can be put on a more rigorous technological basis to improve objectivity” (Patterson 1983, 306). The idea that experts should be believed when distinguishing a geofact from an artifact (Boëda et al. 2013, 446) sounds dangerous. In the first place, lithic technologists do not always agree (Andrefsky 2013; Borrazzo 2011; Driver 2001; Patterson 1983). Such automatic agreement is not in the nature of science. Secondly, there is no better specialist than the one who can sustain his/her claims on the basis of arguments or data. Appeals to authority should be avoided. What is needed is clear evidence that can be shared, analyzed, and interpreted.

Trying to support the case for the Piauí sites, Feathers appealed to another ambiguous case, that of Santa Elina in Mato Grosso (see Vilhena Vialou 1997–1998). He accepted that the lithics recovered at Santa Elina are ambiguous, but emphasized the presence of “two perforated osteoderms” as being undoubtedly of human origin (Feathers 2014, 949). However, only the transformation of the osteoderms at Santa Elina was human; their age is a maximal date for the human manipulation (Martin 2013, 222). In other words, humans could have worked fossil osteoderms.

There are many sites that need systematic attention. Some may contain strong evidence for the early peopling of South America, but at this point it is difficult to tell. It is particularly troublesome that some of these sites have been known for much more than a decade but remain ambiguous and unconfirmed. An example is Pubenza, Colombia, where eight unifacial tools and mastodon remains (*Haplomastodon waringi*) were recovered in a clay matrix and dated between $16,550 \pm 150$ and $13,280 \pm 110$ ^{14}C yr BP (20,300–15,600 cal yr BP) (Correal 1993; Van der Hammen and Correal 2001).

One particularly fascinating site is Urupez II, at the Cerro de los Burros locality, Uruguay (Meneghin 2015; Nami 2001; Suárez 2015). Not only were Fell Cave projectile points, bifacial preforms, and a possible discoidal stone found, but the chronology includes three AMS dates on charcoal from a combustion area, not strictly hearths, between 10,800 and 10,690 ^{14}C yr BP (12,800–12,600 cal yr BP) (Meneghin 2015). The existence of dates of 12,000 and 11,690 ^{14}C yr BP (14,000–13,400 cal yr BP) in another sector of the site should not be a reason to dismiss its record as irrelevant. On the contrary, the dating inconsistencies represent one reason to continue doing intensive research there. Clearly, there is some ambiguity that needs to be clarified for Cerro de los Burros to be fully useful.

In the same vein, the evidence for early settlers from sites in the Sierras Centrales, central Argentina, presently is not overwhelming but suggestive enough to

deserve further work. For example two radiocarbon dates of 11,010 and 9790 ^{14}C yr BP (12,900 and 11,200 cal yr BP) come from the El Alto 3 site (Rivero 2009), not too far from the famous site of La Candonga. At La Candonga, initially researched in the 1930s by Castellanos (1943), an association between extinct megamammals and human remains was recently dated 10,450 \pm 50 ^{14}C yr BP (12,550–12,100 cal yr BP) (Cornero et al. 2014). Another case is the result of work by Politis and collaborators, who evaluated the chronology of some of the human remains that Florentino Ameghino used at the end of the 19th century to sustain their early evolution in the Pampas (Ameghino 1915 [1880–1881]). The results were fascinating, since Arroyo Frías produced ages of 10,300 \pm 60 and 9520 \pm 75 ^{14}C yr BP (12,400–11,800 and 11,100–10,600 cal yr BP). La Tigra was dated to 7270 \pm 60 ^{14}C yr BP (8200–7900 cal yr BP), and Arroyo Chocorí, to 7010 \pm 60 ^{14}C yr BP (7950–7700 cal yr BP) (Politis et al. 2011). The late Pleistocene age for human remains from the Pampas constitutes independent evidence for early human presence already detected at several archaeological sites, and the early Holocene ages of La Tigra and Arroyo Chocorí complement the already dated human remains from Arroyo Seco 2 (Politis et al. 2014).

Many examples of exciting and positive work are abundant. Without being exhaustive, just sampling different regions of the continent, it is possible to mention high-altitude sites like Cuncacha cave, located at 4480 masl, and the Pucuncho lithic workshop at 4355 masl in Peru (Rademaker et al. 2014), which are among the highest sites in the world. There are also examples of new and interesting predictive models focused on the discovery of new sites. Particular among them is a predictive model in northern Chile that used paleoecological information for the availability of water during the late Pleistocene, leading to the discovery of the shallow site Quebrada Maní 12 (Santoro et al. 2013). Further south, the Santa Julia site near the Pacific Ocean in central Chile contains bifacial debitage and a bifacial preform associated with megamammals dated ca. 13,000 cal yr BP, and the Valiente site is a lithic workshop not far from the ocean and dated to about 12,700 and 11,400 cal yr BP (Jackson et al. 2013) (Figure 3). Valiente yielded accumulations of quartz and crystal-quartz debitage, cores, and bifacial fragments in a stratified context and associated with modern fauna and *Xenarthra*, with clear evidence of the local production of Fell Cave projectile points (Méndez and Jackson 2012). The unusual site of Pehuen-có, located on the Atlantic coast of the Pampas (Bayón et al. 2011), must also be mentioned. It is characterized by dozens of faunal footprints and one human trackway associated with a date of about



Figure 3 View of the excavations at the Valiente site, Chile, and layer dating to 12,700–12,550 cal yr BP. Photograph by César Méndez (2015, 105).

13,860 cal yr BP. More studies are needed at this site, which has an important place in the future research agenda (Politis et al. 2015). These are all open-air locations which are contributing to a more complete panorama of early human occupations that previously were for most of the continent mainly based on evidence from caves. It was precisely such an open-air site, Monte Verde II, which provided unusual information about Pleistocene adaptations dated around 14,000 cal yr BP (Dillehay 1997). Clearly made tools have been found accompanying ambiguous wood artifacts, including huts. The reality of the huts has been contested and does not constitute the best evidence of humans at the site. However, the chronology and human character of the occupations is secure. More recently, excavations in the same locality have provided evidence of earlier ephemeral human activity. In this case both clear and ambiguous lithic tools, some of them considered of “probable anthropogenic origins,” are described (Dillehay et al. 2015). The proposed chronology is between about 18,500 and 14,500 cal yr BP (Dillehay et al. 2015). Several issues will need clarification, like the discussion about vertical migration of artifacts or the deposition of so-called exotic manuports — rocks whose geologic sources are outside of the sandur plain, where the occupations and the project area occur (Dillehay et al. 2015). Also intriguing is the proposed notion of the existence of exchange networks before

14,000 cal yr BP. Anyway, the presence of good flakes, burned areas, and paleobotanical remains makes it a promising setting for further research.

Research at the Quintero (GNLQ1) site, found 13 m below sea level in front of the coast of Valparaíso, Chile (Carabias et al. 2015; Cartajena et al. 2013), produced a rich late Pleistocene faunal assemblage including extinct fauna bones radiocarbon dated to between $24,890 \pm 70$ and $21,690 \pm 50$ ^{14}C yr BP (bioapatite fractions) (ca. 29,200–28,800 and 26,100–25,800 cal yr BP, respectively), as well as organic sediments dated to $13,640 \pm 40$ ^{14}C yr BP (16,700–16,200 cal yr BP) (López et al. 2015). What these faunal remains are offering us is a new search model for paleosurfaces which are now under the waters of the Pacific. This particular assemblage, deposited under sub-aerial conditions, may even have an archaeological component. So far, the claim about the possibility of human involvement has been made on the basis of possible cut-marks on some bones, but, as it happens with most of the cases that solely rely on the identification of cut-marks, ambiguity exists so that further research at the site will be required for confirmation of the presence of humans. Quintero 1 is not the only site at which only cut-marks suggest human involvement. For example, very recently an *Eremotherium laurillardii* tooth from Sergipe, Brazil, and dated to $10,740 \pm 30$ ^{14}C yr BP (12,730–12,640 cal yr BP) bears presumed cut-marks (Dantas et al. 2014). However, there are subtle differences in the evaluation of the evidence. It is one thing to claim that the Sergipe evidence “reinforces the hypothesis that humans were present in South America earlier than accepted currently” (Dantas et al. 2014, 197); and quite another to sustain that the Quintero 1 (GNLQ1) site presents two potential cut marks and that “the conclusive evidence to support human activities at GNLQ1 is still scarce and being highly scrutinized” (López et al. 2015, 2). The latter is a useful contribution to the debate, while the former is claiming too much with little basis. It must be noted that this comment is made in spite of the fact that the Sergipe evidence falls well within the widely accepted chronological framework for the human peopling of South America, while the chronology of the Quintero case falls outside that framework. The important distinction is not about chronology; it is about methodology.

7. Conclusions

In reference to the Piauí sites, Haynes (2015, 138) noted that they are not well known and are not widely accepted by North American archaeologists, to which I also have to add most South American archaeologists. Even if we accept that the true debate is between pre or post Last Glacial Maximum occupations (Politis 2015), there is no reason to accept

ambiguous evidence. However, the existence of ambiguity is not a reason to abandon the study of any site, but should be an impulse for further research. In the end, acceptance or rejection of particular evidence should be the result of balanced research, which will be possible only if ambiguity is reduced. On the one hand, sites discovered many decades ago have been successfully restudied using some of the newest techniques (Cornero et al. 2014; Politis et al. 2011; Yataco 2013). Sites like Pubenza in Colombia should be treated in the same way. On the other hand, many sites with good context and chronology exist and are indicating the peculiarities of the process of human peopling. Sites have been found in unexpected places, like high-altitude environments or under the sea, producing new search models that can be applied to other places. High standards of excavation constitute just one of the requisites for the production of reliable information, but such results are useless in the absence of formational studies. Experimental and taphonomic work is required to construct adequate frameworks for separating tools from pseudo-tools and for distinguishing physical from behavioral associations (Borrazzo 2013; Domínguez-Rodrigo 2012). Even more importantly, we must accept that the only way to enter into disputes about the colonization of the continent is by fully participating in the exchange of ideas.

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