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Maybe we do know when people first came to North America; and what does it mean if we do?

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Abstract

The history of research in North America suggests that we already know when people arrived in the continental US: about 11,500 ^{14}C yr BP. Research also suggests that people were in the southern cone of South America by a comparable age, if not earlier. If the New World was colonized by Late Pleistocene migrants from Asia via the Bering Strait, then the earliest sites should be in North not South America. Several possibilities might account for this apparent paradox: (a) the inability to locate pre-11,500 ^{14}C yr BP sites in North America, (b) asynchroneity of late Pleistocene/early Holocene ^{14}C dates between North and South America, (c) inaccurate dating of South American sites, or (d) a coastal migration that by-passed interior North America. All these possibilities currently appear unlikely, and the paradox resists explanation at this date.

1. Introduction

Some years ago David Meltzer asked "Why don't we know when the first people came to North America?" His answer was "it is entirely possible that we already do" (Meltzer, 1989; p. 483). It is from this statement that I take my title.

All archaeologists will agree that people were in North America by at least 11,800 ^{14}C yr BP (uncalibrated ^{14}C dates are used throughout this paper). This is the earliest date on the "Nenana complex" in Alaska (Hamilton and Goebel, 1999; Dixon, 2001). In the continental US, the earliest unquestioned evidence for the presence of humans is the Clovis complex, with dates from 11,500 or 11,300 to about 10,900 ^{14}C yr BP. The standard 'text book' model is that arctic-adapted hunters entered North America by passing through the ice-free corridor that was open by about 12,300 ^{14}C yr BP between the Cordilleran and Laurentian ice sheets that covered northern North America. This population then allegedly grew in size and moved rapidly across the Americas. In some instances these Clovis hunters are seen as exclusive hunters of megafauna (e.g., Martin, 1967), but in other reconstructions they have a more varied diet (Meltzer and Smith, 1986; Kelly and Todd, 1988).

There have always been challenges to this model, North American sites purported to be more ancient than Clovis. Especially compelling evidence has come, however, from South America. Best known to a North American audience is the site of Monte Verde in southern Chile, which appears to date to 12,000–12,500 ^{14}C yr BP (Dillehay, 1989, 1997). This is only one of a number of South American sites that raise the possibility that people were in the western hemisphere before the population who manufactured Clovis spearpoints, and that realization suggests that the 'textbook' Clovis-first model may be incorrect.

It is clear that the ancestors of living Native peoples of the western hemisphere came from Asia, and it is clear that there were several times in the late Pleistocene when they could have crossed over to North America. The question is when did they first do so? An ancillary question is whether some migrants came from other places, such as Europe. In my opinion, the current evidence for a non-Asian origin of Native Americans is weak. But the question here is not so much where they came from but what is the earliest evidence of their appearance. We can never know *with certainty* that we have found the earliest remains in North America because there may always be something that we have not found. But we can generate some idea of the *probability* that we have found the earliest remains. I argue that the probability is very high that we have found the earliest

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occupation of North America. But research also suggests that the earliest occupation of the southern cone of South America is at least nearly as early as that of North America, if not even earlier. If the New World was colonized by Late Pleistocene migrants from Asia via the Bering Strait, then the earliest sites should be in North not South America. Several possibilities might account for this apparent paradox: (a) the inability to locate pre-11,500 ^{14}C yr BP sites in North America, (b) asynchroneity of late Pleistocene/early Holocene ^{14}C dates between North and South America, (c) inaccurate dating of South American sites, or (d) a coastal migration that by-passed interior North America.

2. Dating the late Pleistocene colonization of North America

One way to estimate the probability of whether we have found the earliest occupation of North America is to look at how long it took archaeologists to establish the age of a human presence on the continent. Take the year 1900 as a starting point. A number of tantalizing sites had been discovered in the late 19th and early 20th centuries but it is the Folsom site in new Mexico that is widely regarded as the 'smoking gun' of a Pleistocene presence in North America.

The Folsom site was discovered in 1908 by the African–American cowboy, George McJunkin. He unfortunately did not live to see its excavation in the late 1920s, when an artifact of undoubtedly human manufacture (a Folsom spearpoint) was found in unambiguous association with the remains of extinct fauna (*Bison antiquus*), and its age guessed (not too badly) at about 10,000 yr. During the 1930s other sites of comparable age or slightly older were found (perhaps due to the erosion that accompanied the Dust Bowl years), including the Clovis locality at Blackwater Draw in New Mexico. These sites' ages were later confirmed by radiocarbon dating in the 1950s (see history of research in Meltzer, 1993). We now have quite a few Clovis and Folsom sites, and thousands of fluted points have been found on the surface throughout the US (Anderson and Faught, 2000; Anderson and Gillam, 2000). So, the minimal age of the colonization of North America was established in the 1920s by a site first found in 1908; by the 1930s we had convincing evidence of a Clovis occupation.

In the past 50 yr many sites have been offered as evidence of a pre-Clovis occupation in North America, although most have been discarded by even ardent proponents of pre-Clovis. The question arises: have North American archaeologists looked hard enough? A decade ago, Jelinek (1992) offered a way to answer this question.

Jelinek pointed out that establishing the antiquity of human occupation of a continent has much to do with the intensity of archaeological research on that continent. In the year 1900 American archaeologists did not know how long people had been in North America in large part because there were so few archaeologists and thus so few field investigations. So, to answer the question of whether North American archaeologists have looked hard enough let us compare North America to two other places that also face questions about the timing of initial human colonization: Australia and western Beringia.

In Australia, the earliest sites in 1960 dated to 9000 ^{14}C yr BP; by the 1970s, sites accidentally discovered at Lake Mungo pushed the colonization of Australia to 35,000 ^{14}C yr BP (see history in Mulvaney and Kamminga, 1999). Subsequent finds pushed the continent's prehistory back to at least 38,000 yr. Today we know of over 150 Pleistocene sites in Australia (Mulvaney and Kamminga, 1999). Controversial work in northern and southwestern Australia, as well as new dating of the Lake Mungo skeletal remains may push the continent's prehistory back to 50,000 or 60,000 yr (see Thorne et al., 1999; Turney et al., 2001). In brief, Australia has an established prehistory of at least 40,000 yr.

To the Australian case, we can add that of western Beringia. East of the Lena River, ^{14}C dating of excavated deposits established a progression of early sites: Ushki-1 at 14,000 ^{14}C yr BP in the 1960s, Berelekh at 12,200 ^{14}C yr BP in the 1970s, and Verkhen–Troitskaia, at 18,000 ^{14}C yr BP in the 1970s (Goebel, 1999; Goebel and Slobodin, 1999). There are some claims of still earlier sites but what is important is that western Beringia has an established prehistory of at least 18,000 yr.

In sum, archaeologists in Australia and western Beringia have established prehistories that are older than that of North America. This is interesting because the US has seen far more intensive archaeological research than either of these other places. Cultural heritage laws in the US and Australia have ensured a large amount of archaeological fieldwork in the past 35 yr. But more has occurred in the US than in Australia because there are more people in the US (270 million) compared to Australia (19 million) and thus more construction activities that legally require archaeological research. There are no such heritage laws at work in western Beringia.

There are also more archaeologists in the US than in Australia or western Beringia. The Society for American Archaeology has about 7000 members (and a recent survey suggests that there could be 10,000 practicing archaeologists in the US) while the Australian Archaeological Association has only 430 (and there are probably not more than about 500 practicing archaeologists in Australia; Colin Pardoe, personal communication, 2000). For western Beringia, there are fewer than a dozen active archaeologists (Goebel and Slobodin, 1999, p. 106) working in an area the size of Alaska.

Since the US has seen far more intensive archaeological scrutiny than either Australia or Beringia, it is likely that we have found the earliest remains in North America. Recently, Adams et al. (2001) have made a similar argument by comparing the age distribution of archaeological sites from the US, Europe and Australia. From this comparison they conclude that it is extremely unlikely that humans were in the continental US before the last Glacial Maximum. Obviously, I would take their data further and suggest that there is most likely no occupation preceding Clovis.

Some archaeologists claim there is a bias against pre-Clovis sites such that they go uninvestigated or their claims are shouted down (e.g., Adovasio, 1993). This is simply not true. Possible pre-Clovis sites receive substantial attention in North America and numerous volumes report the results of these projects (e.g., Bonnichson and Turnmire, 1999). For example, more funds have probably been spent, and more attention focused on Meadowcroft Shelter (specifically, its pre-10,000 ^{14}C yr BP deposits) than on any other rockshelter in North America. The National Geographic Society sponsors excavations at several possible pre-Clovis, for example, Cactus Hill and the Burnham Bison site. Private funds—several million dollars—support other projects.

Others say that we have not looked hard enough, that we have not excavated deep enough, or that we have looked in the wrong places (e.g., Butzer, 1988; Adovasio, 1993). Perhaps, but several professional archaeologists have devoted their lives to searching for evidence of pre-Clovis. And the US has seen sufficient earth-moving activities for paleontologists to amass a large database of pre-Clovis-aged Pleistocene faunal locations (see, e.g., FAUNMAP; Graham et al., 1996). If we have located many animal remains that are older than Clovis, then we have obviously dug into plenty of pre-Clovis aged sediments: why have we not located more pre-Clovis archaeological sites? There are, of course, a number of contenders; but virtually all of these (see below) have serious questions about their stratigraphy, their dating, or the evidence for human presence (as at those sites containing no stone tools but only faunal remains with fractures said to be of human origin). So we might rephrase the original question: why cannot North Americans find pre-11,500 ^{14}C yr BP age sites of unquestionable evidence like the Australians and Russians?

Before proceeding, we should note that linguists and geneticists have also registered in on the question of when people first colonized North America. Using existing linguistic and genetic diversity and some assumptions about rates of linguistic and genetic change some geneticists and linguists argue for dates of colonization that range from 11,000 to 70,000 ^{14}C yr BP (Greenberg et al., 1986; Turner, 1986; Greenberg, 1987; Gruhn, 1988; Nichols, 1990; Shields et al., 1993; Szathmáry, 1993; Torroni et al., 1993a, b, 1994; Bonatto and Salzano, 1997; Stone and Stoneking, 1998; Renfrew, 2000). Even spirited proponents of a pre-Clovis archaeology cannot support the older range of these dates. This suggests that some of the assumptions of linguistic and genetic 'clocks', or the relation of linguistic and genetic diversity to dates of colonization, as opposed to genetic or linguistic divergence, are wrong or misunderstood. Nichols (1990), for example, argues that the "unmistakable testimony" of the linguistic evidence is that the New World has been inhabited for some 35,000 yr, but Kaufman and Golla (2000) argue that linguistic comparative methods are not useful beyond about 6000–8000 yr (although they also argue that linguistic isolates in the Americas such as Zuni and Kutenai argue for an occupation in excess of 10,000 yr). Also, genetic analyses reach colonization times that are often widely divergent and that are thus compatible with many colonization scenarios (Szathmáry, 1993) or have such large error ranges that they are essentially useless for dating colonization (Fiedel, 2000). In sum, while linguistic and genetic data can shed light on the process of colonization, the question of when colonization occurred must ultimately be settled by archaeological data.

And in fact, archaeologists have offered many sites as evidence of a pre-Clovis occupation in North America (reviewed in Fiedel, 2000), but there are only two or three serious contenders. Meadowcroft is the best known (see Adovasio et al., 1999 and references cited therein). Although Haynes suggested that groundwater may have contaminated the lowest ^{14}C samples, research into the micromorphology of the stratigraphy indicates this is unlikely (Goldberg and Arpin, 1999). Still, questions about the stratigraphy, the locations of artifacts and the dated material relative to the many alleged features and the layers of rooffall have not been answered (Kelly, 1987). Two other recent candidates are Cactus Hill, in Virginia (McAvoy and McAvoy, 1997; McAvoy, 2000), and Topper, in South Carolina (Goodyear, 2001). Neither site has yet been published in detail. Cactus Hill is a sand dune that contains a fluted point occupation dated to 10,920 $\pm$ 250 ^{14}C yr BP. About 10 cm beneath this lies 1–3 layers of debitage suggestive of a blade industry. The unit bearing the pre-Clovis artifacts is dated to 15,000–17,000 ^{14}C yr BP and in places is found between bands of what are thought to be illuviated clays suggesting rapid burial and no subsequent disturbance. The sedimentation rate between the two units is only 0.002 cm per year (much slower than the apparent rate below and above this layer). Although there is the possibility that artifacts have moved downward in the dune, the Clovis artifacts are of local quartzite and cherts, and largely bifacial, while the pre-Clovis material is made of local quartzite and appears to be primarily blades.

Topper is a sandy deposit that overlies a silty clay terrace. There is a possible Clovis layer, although this is based on the lithic technology suggested by the flakes rather than the presence of fluted points or ^{14}C dates. Beneath this, and separated by a rather thick layer of sterile clay, is found a low density of very small flakes—some argued to be microblades or burin spalls—associated with some larger unworked cobbles. Goodyear (2001) has found it difficult to establish the age of the earliest remains at the site. Radiocarbon dates on the alleged pre-Clovis level are very young, and the optically stimulated luminescence dates, up to 37,000 BP, are difficult to interpret, as they are underlain by ^{14}C dates of 20,000 ^{14}C yr BP. As at Cactus Hill, however, there is a difference in the raw material of the alleged Clovis and pre-Clovis artifacts.

Stanford and Bradley (2000) have resurrected an old idea that these sites are evidence of a Solutrean population that migrated from Europe and that eventually gave rise to Clovis. They make this argument based on what they see as numerous similarities between Solutrean and Clovis stone-working technology. Their argument has not been published in detail, yet already Sellet (1998) and Straus (2000) have both demonstrated that the alleged connections between these two stone-working traditions are more apparent than real, and most likely a product of independent invention rather than migration. Perhaps then, Meadowcroft and Cactus Hill are sites left behind by an unsuccessful colonizing population, one that went extinct long before Clovis hunters roamed the continent (Meltzer, 1995). This might explain the lack of any archaeological sites between Clovis and sites that date to 14,000–16,000 ^{14}C yr BP, and it might also account for why these sites contain an apparently microblade or blade technology, similar to that present in Beringia in the late Pleistocene. But it seems unlikely that a migration from Siberia left remains that have only been found in eastern North America, and not somewhere in the western or central US, since they must have passed through there.

Meltzer (1989) argues that it only takes one site to break the Clovis barrier. I disagree. If the Folsom site had not been followed up by numerous other discoveries, the site today would be an interesting footnote to North American prehistory, nothing more. I would assume that if people were in North America 14,000 or 16,000 yr ago, that we would have, for the reasons given above, found many more sites that date to older than 11,500 BP. So, one or two sites, like Meadowcroft or Cactus Hill, may not be enough to rewrite prehistory.

We should continue to look for and investigate potential pre-Clovis sites with vigor. But compared to other world areas, we have looked very hard. For me this suggests that maybe we do know when people first came to North America—and, for the continental US that appears to be the Clovis complex at 11,500 ^{14}C yr BP.

3. South America

But then there is South America. A reasonable guess is that North America has seen a far greater intensity of field research than South America. And yet a growing list of South American sites seem to be as old, if not older, than Clovis (Fig. 1). Monte Verde is the best known of these sites (Dillehay, 1989, 1997), but there are others (see Dillehay, 1999, 2000; Dillehay et al., 1992; Kipnis, 1998). If the first inhabitants of North America came from Asia, then they had to pass through North America before arriving in South America. How do we explain the presence of people in South America at the same time or prior to their appearance in North America?

There are several possible explanations. There could, of course, be a pre-Clovis occupation in North America that we have not yet found. As should be clear, I think that the probability of this is small. There is also the possibility that South America was initially populated by a migration from somewhere other than northeastern Asia, from across the Pacific, perhaps. This has been suggested by several archaeologists (reviewed in Borrero, 1999b) but there is no good evidence to support it. Three other ideas are perhaps more worthy of attention.

3.1. Radiocarbon date synchroneity

It is worth considering whether late Pleistocene ^{14}C dates from the northern and southern hemispheres are systematically out of sync with each other. For the Holocene, we know that ^{14}C dates are only 30–40 yr out of sync, and no modern reservoir effect seems to account for the early dates at Monte Verde (Taylor et al., 1999). But is it possible that the unique late Pleistocene climate acted in such a way to create a larger difference in the ^{14}C ages of similar calendar-aged material in the northern and southern hemispheres?

It is well known that radiocarbon dates of the late Pleistocene are tricky because they require significant calibration to be converted to calendar years and where a rapid (<100 yr) rise in atmospheric ^{14}C results in a radiocarbon 'plateau' during the Younger Dryas. I do not mean to imply that the "problem" could only lie in the southern hemisphere—it may be that Clovis dates are systematically too young. But radiocarbon

Fig. 1. A selection of pre-10,000 ^{14}C BP South American sites.

calibration curves are established from northern hemisphere data sources. Are they accurate for late-Pleistocene-aged material in the southern hemisphere?

We might look to the timing of the Younger Dryas (YD) as a test of the synchroneity of the hemispheres. The timing of the YD is argued by some to be nearly instantaneous over the globe (e.g., Alley, 2000), but some southern hemisphere data show that the Antarctic Cold Reversal began some 1000 yr before the YD (see also Shen et al., 1998; Bennett et al., 2000; Markgraf et al., 2000; Moreno, 2000; Rodbell and Seltzer, 2000; Shi et al., 2000). However, the proxy data are conflicting, difficult to interpret and afflicted by the imprecision of ^{14}C dating of materials more than 9000 calendar years in age (Osborn et al., 1996; Seltzer and Lachneit, 1998; Borrero, 1999a, b; Geyh et al., 1999; Pendall et al., 2001). Analysis of Antarctica's Taylor Dome ice core suggests that we should expect regional differences in the expression of climate changes due to patterns of oceanic circulation and (unlike other Antarctic ice cores) that the northern and southern hemispheres have been in phase since at least the last glacial maximum (Grootes et al., 2001). This ice-core study, however, is not based on ^{14}C dates. Do studies based on ^{14}C dates reflect regional differences in climate, dates that are out-of-sync, or just inadequate chronological control?

Although several factors may be involved in producing the YD climatic change (Broecker, 1997; Boyle, 2000; Goslar et al., 2000; Renssen et al., 2000) it is fairly clear that it entailed changes in the thermohaline circulation of the oceans. Fluctuations in the ^{14}C content of the atmosphere during the Holocene are linked to fluctuations in solar activity, but the large fluctuations in glacial and immediately post-glacial time may also be linked to changing rates of deep ocean ventilation. If so—and it is not clear if ocean ventilation during the YD was different from today (Goslar et al., 2000)—it could have produced more localized atmospheric conditions that could put ^{14}C calibrations out of sync. At 11,900 ^{14}C yr BP, for example, Antarctic surface waters were twice as old (800 yr) as they are today, suggesting that in the late Pleistocene greater ocean upwelling in the southern hemisphere could have brought old deep water to the surface (see Sikes et al., 2000). Coupled with increased vigor of the southern hemisphere's westerlies relative to the Holocene (but see Hesse and McTainsh, 1999; Boyle, 2000) or perhaps just latitudinal seasonal stabilization (at perhaps 43–45°S;

Markgraf and Kenny, cited in Benn and Clapperton, 2000) changes in ocean upwelling could have created an atmosphere over southern South America that was, relative to the northern hemisphere, depleted of ^{14}C.

Although it seems logical to suppose that atmospheric mixing would have reduced the differences between the hemispheres to the current 30–40 yr discrepancy (a discrepancy brought about by the hemispheres' differences in the ocean-to-land ratio), we do not know if mixing rates have remained constant over time. The factors that produce the north–south change in ^{14}C are complex and not fully understood, but the gradient becomes greater at high latitudes and the ocean poleward of 50°S seems to be a critical area (Braziunas et al., 1995). There may be a need for regional calibration curves. Studies have, for example, produced different estimates of atmospheric ^{14}C during the early YD in the northern hemisphere (Goslar et al., 2000). And a recent European study shows a late Holocene 22 yr offset between areas as close as Germany and Turkey produced by the differential production of ^{14}C in the atmosphere and seasonal differences in carbon uptake by different plant species (Kromer et al., 2001).

For a lack of synchroneity between the two hemispheres to account for the discrepancy between the earliest dates of South America—those dating the 12,500 ^{14}C yr BP occupation at Monte Verde—we need to find a factor that pushes Clovis dates back some 1500 ^{14}C yr or more, or that pushes the Monte Verde dates forward some 1500 or more years, or some combination. Given that the oceanic reservoir effect along the Beagle Channel in Tierra del Fuego is only +620 yr at 6000 ^{14}C yr BP (Albero et al., 1986), it may not be that the correction factor lies solely in the Southern Hemisphere, and it seems unlikely that a late Pleistocene north-to-south ^{14}C gradient could be of sufficient magnitude to produce the necessary northern–southern hemisphere discrepancy to account for 'early' South American dates (Stuiver, pers. comm., 2000). But given the large discrepancy between calendar and radiocarbon ages for materials that are in excess of 9000 calendar years in age, the very rapid rise in atmospheric ^{14}C at the beginning of the YD, the presence of a frustrating plateau in calendar ages relative to ^{14}C ages approximately during the YD (see Fiedel, 1999), and the conflicting climatic evidence from the southern hemisphere, we should perhaps be cautious of other ^{14}C oddities of the late Pleistocene/early Holocene. The most direct way to determine if this is a problem is through construction of (a) ^{14}C calibration curve(s) for South America.

3.2. Are South American dates accurate?

There is the possibility that there are problems with the early sites in southern South America. Certainly some of the pre-11,000 ^{14}C BP candidates suffered from the same stratigraphic, contamination and interpretive difficulties as did North American ones (see Lynch, 1990). But recent excavations have been far more careful and thorough. Space does not permit a thorough review of the radiocarbon evidence from South America. Instead, my purpose here is to suggest that even taking some possible sources of error into account, there still appears to be an occupation in South America that is *at least* contemporaneous with late Clovis.

A number of sites in South America's southern cone other than Monte Verde have produced some very early dates. From its basal layer, Cueva 3 at Los Toldos produced a date of 12,600 ± 500 ^{14}C yr BP (Cardich et al., 1973), but its large error, the fact that its association with artifacts is uncertain and the fact that it is a standard date made from several pieces of carbon cast some doubt on its utility (Borrero and Franco, 1997). Piedra Museo has an even older date of 12,890 ± 90 on its lowest level (Miotti, 1992, 1995; Miotti and Cattáneo, 1997). However, other dates on the site, including one on a cut-marked horse bone, place the earliest level's age closer to 11,000 ^{14}C yr BP (Miotti, pers. comm., 2000). Likewise, at other sites, a range of dates come from the earliest occupational surfaces, including samples from the same hearth (Borrero et al., 1998; Massone, 2000). At Cueva del Medio, for example, one hearth produced dates from 12,390 ± 80 to 10,350 ± 130 ^{14}C yr BP (Nami and Nakamura, 1995). The lowest level at Cueva 1 at Cerro Tres Tetas, which varies in thickness from 6 to 38 cm, contains a date of 10,260 ± 110 ^{14}C yr BP at the top, and dates of 10,850 ± 150 ^{14}C yr BP, 11,100 ± 150 ^{14}C yr BP, and 11,560 ± 140 ^{14}C yr BP for the lower portion (Paunero, 2000). A similar situation exists at Cueva de los Mineros (Rafael Paunero, pers. comm., 2000). This is, incidentally, no different from many North American paleoindian sites; the Paleo Crossing site in Ohio, for example, contains dates from 9230 to 13,100 ^{14}C yr BP (Brose, 1992). Removing the outliers and averaging, Brose (1992) dates the site to 10,000 ^{14}C yr BP. At Brazilian sites the pre-10,000 ^{14}C yr BP dates are afflicted by large standard deviations (Kipnis, 1998) that suggest contamination. At Lapa do Boquete, for example, the pre-10,000 ^{14}C yr BP dates have error ranges from 140 to 500 yr. At Caverna da Pedra Pintada the arrival of humans is said to be marked by "a cluster of four dates between 11,145 ± 145 and 10,875 ± 295" (Roosevelt et al., 1996, p. 380). The sizeable errors associated with these dates places the initial occupation (using 2σ ranges) anywhere from 11,465 to 10,285: an unacceptably large range for the problem at hand. What might explain situations like these?

Recently, a redating of a number of the early sites in Argentina with samples restricted to humanly modified bone and identified plant species (to eliminate the old wood problem), and using two labs has failed to produce dates that are equivalent to the earliest dates already in hand, with the exception of Arroyo Seco 2, where bone dates of 12,070 $\pm$ 140 and 12,240 $\pm$ 110 ^{14}C yr BP confirm previous ~12,000 ^{14}C yr BP bone dates (Steele et al., 2001; Politis et al., 2001). In most cases, however, the sites date to no more than about 11,000 ^{14}C yr BP (Tres Tetas Cueva 1, Piedra Museo), or 10,800 ^{14}C yr BP and younger (Cueva 1 del Lago Sofia, Tres Arroyos; 10,400 ^{14}C yr BP, in the case of Paso Otero 5). Possibly, the first occupants of the region used some wood or bone that had been lying on the surface for a long period of time—since no humans had previously been present to burn the material. Thus, an 'old wood' or 'old bone' problem (especially in dry caves) could appear to increase the age of initial occupation significantly. But multiple dates on strata and hearths can make this problem visible (as at Cueva del Medio, where Nami had wisely rejected the pre-12,000 ^{14}C yr BP date).

Nonetheless, even taking a possible 'old wood' or contamination problem into account the current evidence suggests that people were present in Patagonia and Tierra del Fuego at a very early date, by at least 11,000 ^{14}C yr BP, if not earlier. Other sites produce equally old dates—Quereo I in central Chile, for example, has dates of 11,600 $\pm$ 190 and 11,400 $\pm$ 145 ^{14}C yr BP on wood (Núñez, 2000). And, at present, no good explanation has been offered as to why the dates from Monte Verde, averaging 12,000–12,500 ^{14}C yr BP, should be discounted. Thus, at the moment we appear to be left with an initial occupation of South America that appears to be *at least* contemporaneous with the earliest occupation in North America, if not earlier. How might we account for this?

4. The coastal route

The current suggestion is that a coastal migration route might account for the early dates in South America. It was long assumed that the 1200-km long ice free 'corridor' in central Canada between the Cordilleran and Laurentide ice sheets was the migratory route of the first peoples to the western hemisphere. The corridor was probably closed by ice from 18,000 to at least 12,300 ^{14}C yr BP (Meltzer, 1995; Jackson and Duk-Rodin, 1996; Haynes, 2001), and was probably not really passable until 11,600 ^{14}C yr BP—just before the appearance of Clovis. People would have had to move very quickly through the corridor as well as the Great Plains to be at the Aubrey site in northern Texas at 11,590 ^{14}C yr BP (Ferring, 1990, 1994). This is not impossible, but even when open the corridor was probably biologically dead and difficult to traverse (Fladmark, 1979, 1983; Aoki, 1993; Mandryk, 1993; Anderson and Gillam, 2000; Fiedel, 2000). There is virtually no evidence of late Pleistocene human activity in the corridor except at the southernmost end (Carlson, 1991) but only a small number of small sites (with a low probability of discovery) would be expected if passage through the corridor was quick.

An alternative migratory pathway is along the western coast of North America (Fladmark, 1979). Once thought impassable due to glaciers, new data suggest that it was largely ice-free by at least 16,000 ^{14}C yr BP (Fladmark, 1979; Bednarik, 1989; Dixon, 1993, 1999; Gruhn, 1994), and completely ice-free—and capable of supporting bears—by 13,000 ^{14}C yr BP (see Josenhans et al., 1997; Dixon, 1999; Fedje and Christenson, 1999; Mandryk et al., 2001). Thus, humans could have entered the New World by moving along the western coast, and sometime later turned to the east and moved into the interior of North and South America.

Unfortunately, this is a difficult hypothesis to check for the late Pleistocene rise in sea level covered the coast except in a few areas that saw uplift at the same time. There are some early sites along the western coast—on Prince of Wales island of the Alaskan panhandle, on the Channel Islands off southern California, and along the Peruvian Coast (Sandweiss et al., 1998; Keefer et al., 1998; Josenhans et al., 1997; Dixon et al., 1997; Erlandson, 1994, 1998; Erlandson et al., 1996) but these date from 9000 to at most 11,000 ^{14}C yr BP (and perhaps not older than 10,700 ^{14}C yr BP). In southern California, cation ratio and AMS dates on rock varnish coating artifacts and organics trapped in the varnish have produced dates as old as 16,000 ^{14}C yr BP (Whitley and Dorn, 1993), but these techniques are controversial. In order for them to explain Monte Verde and the other early sites in southern South America sites need to predate 12,500 ^{14}C yr BP. Given that the coastal migration hypothesis will be difficult to verify directly, as an alternative we might ask what are its implications for the archaeology of the interior of North America?

It is possible that if people entered the New World along the coast, that they could have simply kept with an adaptation that they knew until they eventually reached southern South America some 12,500 yr ago. Such a migration could have been driven by local resource depletion by simply lowering the rate of return from hunting and gathering food around the current camp relative to the return rate that could be achieved by moving on to virgin coast. Lawrence Todd and I argued some years ago that Clovis hunters who entered North America with an arctic hunting adaptation would have moved rapidly throughout the continent. Key to this argument was the assumption that naive prey—animals that had never encountered human predators before—could have been easily hunted, but would have rapidly adapted to human predators, thus making movement to unexplored territory—with naive fauna—a more economical strategy than remaining in the current territory (see also Kelly and Todd, 1988; Kelly, 1995, 1996, 1999). Research demonstrates that when bear and wolves are introduced to populations of elk and moose which have not experienced predation for several generations that the predators have far higher success rates than those who have lived with their prey populations for several generations (Berger et al., 2001). These studies also show that prey become wary very quickly, within a generation. Human hunter–gatherer colonists would have been in a situation similar to that of carnivores introduced to a naive prey population. Given the knowledge that uninhabited land lay before them (and the first colonists must have suspected this) they would have quickly learned that moving into new territory was a better option than remaining where they were if they wished to continue to maximize their hunting efforts.

For comparison, consider the case of the colonization of the Canadian Arctic. The high arctic was first occupied by the Arctic Small Tool Tradition about 4500 yr ago. Radiocarbon dates on the Arctic Small Tool Tradition from its far western and eastern edges are statistically indistinguishable (McGhee, 1996), indicating a very rapid occupation. The Arctic Small Tool Tradition people were primarily muskox hunters, relying much less on marine resources (unlike the later Inuit). Muskox's defense strategy—in which males form a circle around the females and young, their horns and bony heads pointing outward—was a successful strategy against wolves, but it was foolish against human hunters. The search for driftwood as a fuel source may also have been partly responsible for the rapid movement (Arctic Small Tool Tradition people apparently did not use oil lamps).

But a late Pleistocene adaptation along the western coast of North America would probably not have focused on mammals, and firewood would not have been so critical as along the Arctic coast. My best guess is that a temperate marine adaptation would not have resulted in rapid migration because marine environments replenish themselves more rapidly than terrestrial environments (this is one reason why hunter–gatherers living along coasts are sedentary) and because the 'naivety' factor would not be relevant for foragers relying on shellfish and fish.

It is possible that the initial colonizers of the New World, occupying an empty niche and being highly residentially mobile, had high rates of population growth, as Surovell (2000a) has argued. If ethnographic data are used as a guide, less mobile coastal peoples would have had high growth rates as well (Kelly, 1995). As a coastal population grew to the point where it had begun to deplete resources, it would eventually send daughter populations to the south (see Surovell, 2000b).

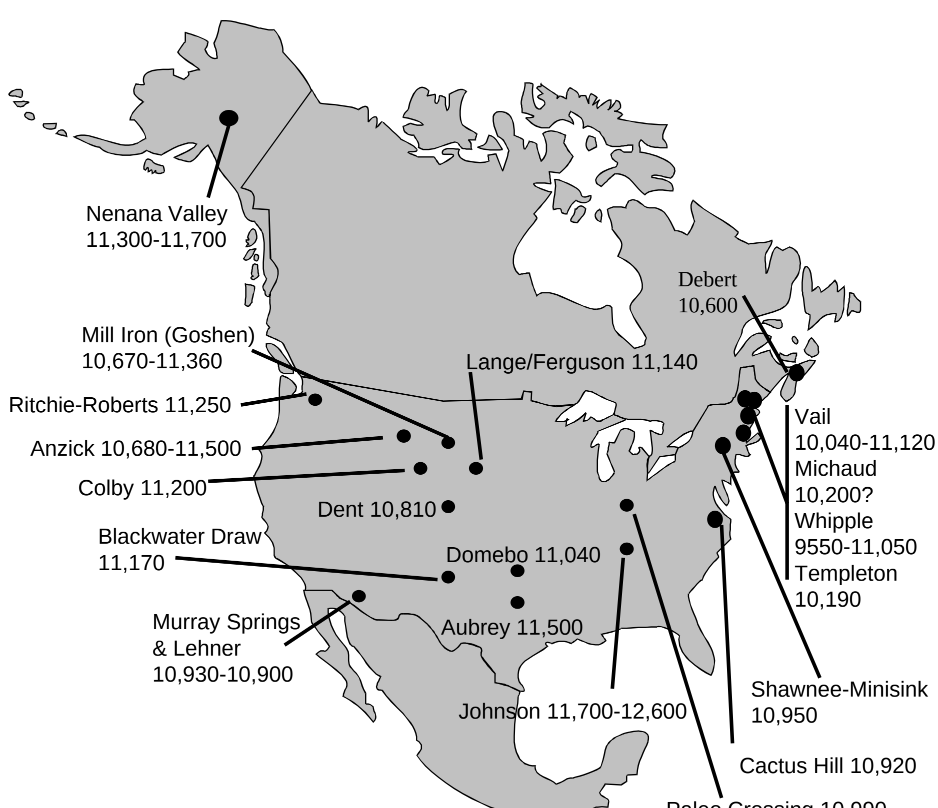

Fig. 2. Dated Clovis (including Goshen) and Nenana localities in North America.

But population-induced depletion takes time. Studies of changes in diet for the Holocene period in California show that dietary changes due to resource depletion took hundreds of years (Broughton, 1994). If the population growth model is correct, for people to have reached southern Chile by 12,500 ^{14}C yr BP they must have been along the coast of California at a considerably earlier date, perhaps 1000 or more years earlier. And if that is true, then as demographic growth along the population front sent daughter populations south, those populations who remained behind would have sent future excess population into the interior of North America (Surovell, 2000b). By the time people reached southern Chile, they should also have been in the interior of North America.

Such a migratory scenario would result in a geographic pattern in which the earliest dates of human occupation would be in the western US, and the later ones in the east (Fig. 2). The latest dates on Clovis are in the east, and the earliest dates tend to be in the interior. But the error inherent in radiocarbon dates, the short time span of Clovis, the small sample of dates and the lack of dates from far western North America make it difficult to use these data at the moment. The Ritchie-Roberts Clovis date is based on the presence of a volcanic tephra of known age on the surface on which the Clovis points were laid. Hence, it is a maximal date. There are some proposed pre-Clovis sites in western North America (reviewed in Bryan and Tuohy, 1999) that may be evidence of an eastward migration from a pre-Clovis occupation along the west coast. But all these sites have serious problems with their stratigraphy or dates (Beck and Jones, 1997; Fiedel, 2000). These include Fort Rock Cave (13,200 $\pm$ 720 ^{14}C yr BP on carbon; but this date's large error and its dubious association with cultural materials renders it suspicious), Wilson Butte Cave (14,500–15,000 ^{14}C yr BP, but these are questionable bone apatite dates), Smith Creek Cave (11,680–12,150 ^{14}C yr BP on wood, charcoal and camelid hair; but the layer also has a 10,420 ^{14}C yr BP date on cordage), Clark's Flat in California (a paleosol with dates of 9170 and 11,720 ^{14}C yr BP), and the Cooper's Ferry site (11,370–12,020 ^{14}C yr BP). Without good dates on western Clovis we are left wondering if the earliest dates for Clovis are truly in the center of the US, or if Clovis is earlier in the western US.

5. Conclusion

The lengthy history of archaeological research in North America when compared to that of Australia and Beringia strongly argues that we have found the earliest evidence of human occupation in North America–Clovis. In light of the argument developed here, it seems likely that South American archaeologists have also found the earliest or nearly the earliest evidence of human occupation there, too, and that evidence points to an occupation that is at least contemporary with that of North America.

What are the implications of these facts? One possibility is that there is some unknown factor that has left late Pleistocene ^{14}C dates significantly unsynchronized between North and South America. At present, however, there is no hard evidence for this.

An alternative is that a coastal migration accounts for the earlier dates in South America. However, this hypothesis is difficult to test since the late Pleistocene coast is nearly completely underwater at present. Still, I would expect a pre-Clovis coastal migration to have been driven largely by population growth, for that growth to have occurred long before ^{14}C dates place people in southern South America, and, consequently, for that population growth to have resulted in a significant population in the interior of North America long before Clovis. The evidence for a pre-Clovis occupation in North America, however, is weak. We lack consensus on the earliest ages of western sites, and, oddly, have virtually no dates on Clovis west of the Rocky Mountains (with the exception of Lehner and Murray Springs in southern Arizona). There is no reason to think that the ca. 11,000 ^{14}C yr BP South American dates, or the early dates from Monte Verde, are wildly erroneous, yet also no direct evidence that a coastal migration can account for them. I think we do know when people arrived in North America, but what this means for understanding the colonization process is not clear.

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