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Early Paleoindian big-game hunting in North America: Provisioning or Politics?

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ABSTRACT

We question several common elements of conventional descriptions of Early Paleoindian adaptations. Specifically, we examine the presumed scales of residential mobility, the role of high-quality lithics in these movements, and the extent to which First Americans hunted large game as a fundamental part of their food-getting activities. We compare the Early Paleoindian data to relevant information on hunting, mobility, and weaponry documented ethnohistorically and ethnographically. We then construct an alternative explanation for the Early Paleoindian record based on the premise that the hunting of large mammals, presumably by men, may have been motivated more by social and political factors than by the need to regularly and reliably provision a family or band with food. By proposing a plausible alternative explanation for the available data, we suggest that there is good reason to think critically about several of the basic components of the conventional view of Early Paleoindian adaptations.

1. Introduction

> …*in the literature as a whole, successful farmers have social relations with one another, while hunter-gatherers have ecological relations with hazelnuts.*
> Bradley (1984: 11)

Nowadays, when one thinks of the Paleoindian period in North America, what comes to mind is an image of small bands of intrepid hunters, traveling over immense expanses of tundra, forest, and plains in pursuit of (mostly) big game—mammoth and maybe mastodon in the "pioneer" days of Clovis and its eastern fluted point contemporaries and, during the succeeding Folsom and Plano periods, a jumbo-sized cousin of the modern bison. Paleoindian hunters in the northeast may at times have focused their efforts on the next best thing—caribou (e.g., Seeman et al., 1994: 77; Robinson et al., 2009: 442). While this depiction of Paleoindian lifeways is obviously an oversimplified caricature of the diversity of lifeways that surely must have existed during the millennia that encompass the Paleoindian period, it nonetheless captures the essence of how many of us perceive the early stages of human habitation on the North American continent.

There are of course some who do not subscribe to this view, and their number is growing steadily (Grayson and Meltzer, 2002; Cannon and Meltzer, 2004, 2008; Hemmings, 2004; LaBelle, 2005; Bamforth, 2007; Hill, 2007; Kornfeld, 2007; Walker and Driskell, 2007; Andrews et al., 2008; Kornfeld and Larson, 2008; Sandweiss, 2008: 153–154; Borrero, 2009: 160; Roosevelt et al., 2009: 446–447). Nonetheless, it is not too much of a stretch to suggest that this view of the "First Americans," in North America at least, is widespread in the professional literature, in undergraduate textbooks, and in the popular media—and it is one that many accept with little hesitation. Never mind the fact that the territories over which Paleoindians are thought to have wandered during their annual peregrinations—distances for the most part based on the movement of exotic flints—are often larger than the largest hunter–gatherer ranges we can find in the ethnographic literature (Smith, 2010; Sulgostowska, 2006; Whallon, 2006; Wilkins, 2010), even for highly mobile caribou-hunting arctic and boreal forest groups, or the peripatetic "desert people" of interior Australia (e.g., Amick, 1996; Burke, 2006; see also papers in Ellis and Lothrop, 1989; Tankersley and Isaac, 1990). This seeming discrepancy is easily dismissed by reference to Wobst's (1978) oft-cited classic "The Tyranny of the Ethnographic Record". After all, Paleoindians were living on a continent emerging from the last Ice Age, with unstable landscapes, environments, and climates having no known modern analogs (Kelly and Todd, 1988; Wright, 1989: 346; Seeman, 1994: 284; Tankersley, 1998: 8; Burke, 2006; White, 2006; Meltzer, 2009: 41–43). So why shouldn't we expect hunter–gatherers at that time to have lived and behaved in ways that likewise have no modern counterparts? True enough. But when a perspective such as the one which currently dominates our view of Paleoindian lifeways becomes so well

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established that there seems to be no need to question it, maybe it is time to put that view under the microscope for a closer look. Maybe what seems so intuitively obvious really is not.

So this paper is a deliberate attempt to rock the boat, to question something that seems so compelling and well established that there is no need to examine it. We don't hesitate to admit that, for us, writing a paper such as this is both presumptuous and risky, because the Paleoindian literature is not only vast, it is complex and often quite contentious. Nevertheless, setting caution aside, let us launch this trial balloon and see whether it contains enough helium to float; or, as is quite likely, it ends up joining its many lead-filled cousins.

2. Basic assumptions, questions, and issues

Essentially, all models are wrong, but some are useful.
Box and Draper (1987: 424)

As the reader will quickly discover, in what follows we make many generalizations about Paleoindian lifeways that gloss over or homogenize what must have been a great deal of both spatial and temporal heterogeneity. Thus, while we talk about "Paleoindian", our focus here is really almost solely on North America, as the Central and South American records differ in many very striking and important ways. However, were we to broaden our scope to look at the entire New World, we would face an undertaking far beyond the scope of what we have attempted here. Similarly, we recognize that the Paleoindian record, just within North America, is far from homogeneous. Paleoindian adaptations along the Pacific coast or in the forests of the Southeast are likely to have been quite different from those that developed on the northern Plains or in the tundra landscapes of the northern Great Lakes area, New England, and the Canadian Maritime provinces. The resources these ancient foragers used, their annual ranges, their patterns of aggregation and dispersal, even their reliance on non-local flints, must have varied from region to region. These same factors are likely to have varied over time as well, so the way Clovis and other Early Paleoindian peoples lived and responded to their surroundings was probably quite different from the way Late Paleoindian foragers went about their daily lives. In light of this variability, many of the general arguments we offer here will very likely only apply to some spatial or temporal segment of the total Paleoindian record, as for example the issue of exotic flint use, a widely noted hallmark of Paleoindian economies in parts of the Southwest, Great Plains and the Northeast, but not so in the Southeast; and likewise a characteristic more pertinent to the early stages of the Paleoindian period than to later ones. We therefore ask the readers' indulgence as we put forward ideas under the overarching rubric of "Paleoindian" when in fact they may only be applicable to certain parts of the North American continent or at certain points in time. Without this "shorthand", an already lengthy paper would take on unmanageable proportions. With these caveats in mind, let us now turn to the issues at hand.

At the risk of (gross) oversimplification, here are several of the basic assumptions and tenets that underlie what we perceive to be the "conventional" view in Paleoindian studies.

Paleoindians, particularly Early Paleoindians, were, first and foremost, hunters of big game—in fact, according to many, they were *bona fide* big-game-hunting "specialists". Since hunting very likely required foragers to exploit larger ranges than if they relied solely or primarily on plant foods, Paleoindians, who presumably relied mostly on the biggest herbivores the land could support, of necessity must have used even larger tracts of land. So, high mobility and huge territories are precisely what one would expect.

How far did they actually move in their annual rounds? Of course, no one really knows, but the answer to this question is usually based on the straight-line distance to the sources of "exotic" (i.e., non-local) flints that archaeologists often find in Paleoindian sites, sources not uncommonly located hundreds of kilometers away from where archaeologists dig them up (e.g., Meltzer, 2002: 37; Loebel, 2005; Randall and Hollenbach, 2007: 217; see Hofman, 1992, for interesting modifications to this baseline assumption, taking into account the frequency with which projectile points and other tools had to be replaced over the course of the annual round).

Underlying this assumption is another even more fundamental one—procurement of these exotic raw materials is assumed to have been *embedded* in the annual round of the entire social group. In other words, the non-local flint wasn't acquired by exchange or by individuals or small task groups who periodically visited the source; it was acquired at some point during the annual cycle when the entire residential group moved for a time to the vicinity of the quarry or outcrop (e.g., LeTourneau, 2000). There are of course a number of Paleoindian scholars who don't subscribe to this view, or who do so with varying degrees of hesitation (e.g., Curran and Grimes, 1989: 72; Deller, 1989: 219; Tankersley, 1989: 270; see also Bamforth's, 2003, interesting critique of the presumed role of bifacial technology in Paleoindian mobility strategies), but we think it is fair to say that the idea of high mobility involving entire residential groups, with acquisition of exotic high-quality flint embedded within the annual round of subsistence activities, is still the predominant view.

But why not exchange? Australia provides a continent's worth of examples of hunters and gatherers, many living at very low densities, who frequently engaged in long-distance exchange over hundreds, sometimes even thousands, of kilometers; and projectile points made on non-local, and often ritually significant, materials featured prominently among the items that were traded. The usual answers that are offered in the Paleoindian community are either that there is "too much" of the exotic materials in some of our Paleoindian sites to have been obtained that way; or that population densities were so low, and people were moving around the landscape so often, that it would have been too risky to pin one's critical lithic needs on the whereabouts and good graces of other equally peripatetic bands (Goodyear, 1989; Meltzer, 1989: 17; Amick, 1996; Burke, 2006).

Why were they so dependent on these particular types of flint? This brings us to the assumption that forms the bedrock underlying this whole chain of reasoning, in fact one of Paleoindian archaeology's most cherished, yet seldom seriously questioned, assumptions: Paleoindians needed not just cherts and flints, but cherts and flints of "high-quality", in order to be able to fashion their elegantly flaked projectile points, the most critical component of their weaponry, the part that assured them success in their forays after megafauna or at least "big" fauna. We assume this means "no high-quality flint, no dinner".

What we have here is an edifice built up of several layers of assumptions, some quite tenuous, a few quite circular, all of which need to be looked at a lot more closely. In this brief paper, we will barely be able to scratch the surface of what really needs to be a complete and thorough reexamination of them all, from the top of the skyscraper all the way down to bedrock. Tossing caution to the wind, let's take a first stab at it, beginning by enumerating several of the more obvious questions and issues that need to be addressed.

1) Were big game, whether megafauna like mammoths or mastodons, or just the biggest critters that happened to be available on the landscape like caribou, really the mainstay of Paleoindian diet? In other words, were Paleoindian foragers truly big-game-hunting "specialists"?
2) Do the distances from kills and campsites to sources of exotic flint reflect the actual geographic space over which Paleoindian groups ranged on an annual basis?
3) Why did flint procurement have to be embedded in the annual round of a social group? Why couldn't they get it by exchange,

or via the activities of individuals or groups of individuals who made periodic treks to the sources for the express purpose of getting flint?

4) Why all the hoopla about "high-quality" flint in the first place, and why was it so important to Paleoindian survival?
5) Does one really need a stone point on the end of a projectile weapon to kill a big animal?
6) Why are Paleoindian projectile points so remarkably pretty? Why are they so delicate, fragile, and hard to make? And why did Paleoindians often make them on a limited range of "exotic" flints when equally functional points could be made on materials that were closer at hand?
7) Finally, even if we grant that big-game hunting was a major and recurrent focus of activity during the annual rounds of our Paleoindian forebears, was this hunting really motivated by the food (and non-food) needs of the hunters and their families, with prestige a distant second; or was it perhaps the other way around—was big-game hunting done for its prestige value or in pursuit of other social and political goals (e.g., "costly signaling"), while putting food on the family table was at best a distant second?

3. Big-game hunting

…the faunal record provides little support for the idea that all, or even any, Early Paleoindian foragers were megafaunal specialists. Cannon and Meltzer (2004: 1955)

Let us take a closer look, albeit a brief one, at each of these questions, beginning not surprisingly with the first one—the extent to which Paleoindians actually engaged in big-game hunting. Although few positions ever gain unanimous acceptance, we think it is fair to say that during much of the 20th century many, perhaps most, archaeologists endorsed the view that Paleoindians didn't just hunt big game, they were "specialists" in big-game hunting, meaning, we suppose, that when they hunted they directed most of their efforts at the large end of the body-size spectrum (Wormington, 1957; Martin, 1967; Mosimann and Martin, 1975). This of course doesn't tell us how many kills they were able to pull off each year, or whether these kills occurred throughout the year, or just during certain seasons, but it at least indicates where the lion's share of their efforts was directed.

Of the questions laid out above, this is the one that has come under the most scrutiny, particularly over the last decade or so (Collins, 1999: 30–31; Grayson and Meltzer, 2002, 2003; Lepper, 2002: 85; Bousman et al., 2004: 75, 79, 84, 87; Cannon and Meltzer, 2004, 2009; Chilton, 2004; Hemmings, 2004; Byers and Ugan, 2005; LaBelle, 2005; Bamforth, 2007; Hill, 2007; Kornfeld, 2007; Walker and Driskell, 2007; Andrews et al., 2008; Kornfeld and Larson, 2008; Lemke and Timperley, 2008; Surovell and Waguespack, 2009). More and more archaeologists are beginning to question the big-game "specialist" view, pointing out that many well-excavated Paleoindian sites produced more than just mammoth or bison bones. In those cases where the excavators screened the deposits, and where the recovered bones of the smaller animals were identified and published, there are remains of smaller taxa (sometimes lots of them), including waterfowl, turtles and tortoises, and a variety of smaller mammals. This gradual shift in perspective is a healthy one, because it has brought one of the fundamental "truisms" of Paleoindian archaeology under the lens. Maybe the First Americans ate more than filet of mammoth and bison.

Surovell and Waguespack (2009, see also Waguespack and Surovell, 2003) offer an interesting counterpoint to the increasingly popular view that Paleoindians were dietary generalists, not specialists. They point out that this debate to some extent stems from an unrealistic view of what "specialization" means or implies. Many archaeologists see big-game "specialists" as foragers who eat virtually nothing but prey from the upper end of the size spectrum, ignoring the smaller taxa altogether. Since smaller taxa are now being found with increasing frequency in Paleoindian sites, the inevitable conclusion is that Paleoindians were generalists. Surovell and Waguespack point out that this view of "specialization" is inappropriate; no known foragers base their subsistence system exclusively on the biggest animals available on the landscape. They point out that the critical issue is not whether Paleoindian foragers ate small animals, but whether they *underutilized* smaller prey relative to what was naturally available on the landscape. Using this relative relationship as their yardstick, they conclude that Clovis hunters actually did underutilize the available smaller taxa and hence were *bona fide* big-game "specialists".

This debate is far from resolved. We clearly need to know much more about the smaller animal taxa (and of course about the plant foods as well). But in order to accomplish this, we will have to invest considerably more effort in basic taphonomy (Haynes, 2007: 86–87). Consider the taphonomic studies that have focused just on the handful of early Pleistocene localities in Bed I and early Bed II at Olduvai Gorge (Tanzania), much of it in fact aimed at just one—the justly famous FLK-*Zinjanthropus* site. Over the past three decades, literally thousands of pages have been written on the taphonomic history of these few faunal assemblages: but, despite this torrent of highly focused, sophisticated, and often pioneering archaeozoological research, we still aren't sure how much of the fauna owes its presence, composition, and condition to human activities (a few examples include Binford, 1981; Potts, 1983; Bunn, 1986; Blumenschine, 1991; Oliver, 1994; Fernández-Jalvo et al., 1998; Madrigal and Blumenschine, 2000; O'Connell et al., 2002; Pobiner and Blumenschine, 2003; Njau and Blumenschine, 2006; Domínguez-Rodrigo et al., 2007; Faith et al., 2009). By contrast, most discussions to date about the animal bones in Paleoindian sites that are from taxa other than the jumbo-sized ones have not moved much beyond the level of presence–absence or rudimentary counts of the number of identifiable bones or the minimum number of individual animals the bones represent.

Of course, a fair amount of the pioneering work in Olduvai, such as using SEM to determine how to recognize cutmarks (Potts and Shipman, 1981), or documenting what happens to bones when they lie exposed on the surface and begin to weather (Behrensmeyer, 1978), need not be repeated at every site on the globe. Nonetheless, until we dive into Paleoindian faunas with both feet and devote the time and energy needed to unravel the complex taphonomic history of the smaller remains, we will never be able to attribute them with any degree of confidence to human activities. This is especially important in the many Paleoindian sites that were located close to springs, bogs, or ponds, where other taxa are very likely to have been part of the paleontological background "noise". We cannot simply assume that these smaller species were brought to the site and consumed there by people. Even the presence of burned bones of the smaller taxa is not sufficient to demonstrate their use as food, since bones can become inadvertently burned as fireplaces are relocated in a site and rekindled (see Stiner et al., 1995; Speth and Clark, 2006). Nonetheless, these smaller faunal remains may prove to be very important, and more detailed taphonomic studies may ultimately reduce the supposed dietary prominence of the big ones.

What about the recurrence interval of successful big-game kills? This is a tough question to answer, given the current state of the discipline, and frankly remains virtually unknown. For mammoths, we are still unsure whether kills were fairly frequent, that is, often enough to feed people on a regular basis; or, at the other extreme, a once-in-a-lifetime event, nicely captured by MacNeish's (1964: 14) oft-repeated quip that "the hunter killed one elephant and spent the rest of his life talking about it". For mastodons, with a very small number of exceptions like Kimmswick (Missouri), Manis (Washington), probably Monte Verde (Chile), and possibly Big Bone Lick (Kentucky) and Hiscock (New York), we still aren't sure if they were even utilized by humans, let alone hunted or scavenged (Gustafson et al., 1979; Graham et al., 1981; Dillehay, 1992; see also Fisher, 1987, 1996 and Tankersley et al., 2009).

We are in somewhat better shape with bison, because our sample of kill sites is much larger and we can more easily determine the season of kill events (e.g., Reher, 1973; Frison et al., 1976; Todd et al., 1990; Todd et al., 1996), but even in this case we still can't be sure whether such kills were annual events or much less frequent, as has been suggested for example for the late prehistoric Vore site in Wyoming (Reher and Frison, 1980; see also Fawcett, 1987; Cooper, 2008). If bison were at times taken individually, as seems very likely (Bamforth, 2007: 247), we are in much worse shape, because such sites are harder to find and seasonality is not as easily determined.

Even in the northern High Plains, where impressive communal bison kills seem to dominate the Paleoindian landscape, our view of Paleoindian foodways may be a poor reflection of reality:

> Archaeologists may have tended to place Paleoindian bison hunting groups into lifeways with subsistence strategies that are too narrowly focused toward bison procurement alone. The communal type of bison kill strategies indicated by such sites as Mill Iron, Agate Basin…, Casper…, Hudson Meng…, and Horner…, to mention a few, may represent only a small increment of the total range of subsistence activities and only those that are the most visible archaeologically. Stratified sites such as Agate Basin, Carter/Kerr-McGee, and Hell Gap indicate use of the locations throughout Paleoindian times but not occupations of a magnitude that suggest long-term, repeated annual returns to the same locations for large-scale bison hunting episodes. Bison bonebeds in the sites such as those mentioned above appear to have been the result of either discrete events or events repeated only a time or two. None contains the accumulations of bone, tools, and weaponry that would be expected if these sites were used for the same kind of communal bison procurement repeatedly, year after year, for long aggregates of years. (Frison et al., 1996: 209–210; see also Bamforth et al., 2005: 576, and Kornfeld, 2007: 36)

So we think the best one can conclude at this point is that, yes, Paleoindian hunters did kill big animals, including megafauna, and sometimes in large numbers. But we don't yet know how often they did it over the course of the year, and we know even less about the recurrence of successful kills from year to year. While a great deal of progress has been made on the taphonomic front, we still have a long way to go. It would seem, therefore, that taking a firm position at the present time—either Paleoindians relied heavily on big-game as their dietary mainstay, or Paleoindians relied on a much broader diversity of resources, with big-game an occasional contributor to the diet—is still premature, although the balance of evidence is clearly shifting toward the more "generalist" side of the equation.

4. Lithic procurement

> *…if we cannot usefully employ the stone tools, we cut out a very large part of our direct data from the past, data which provide many of the foundations of our more theoretically oriented upper stories.*
> White (1977: 13)

> *…the majority of stone tool forms were not necessary, in a utilitarian sense, at all.*
> White (1977: 26)

Now let us turn to the second and third questions enumerated earlier. We treat these together because they entail a number of closely related issues: Do the distances from kills and campsites to sources of exotic flint reflect the actual geographic range over which Paleoindian groups foraged on an annual basis? Why did flint procurement have to be embedded in the annual round of an entire social group? Assuming they didn't live in a vacuum (a point that becomes increasingly persuasive if we accept the existence of occupations pre-dating Clovis—e.g., Roosevelt, 2002; Roosevelt et al., 2002; Prasciunas, 2008—and likewise if we assume that these Pre-Clovis peoples didn't remain tethered to the Pacific coast until the Clovis phenomenon exploded on the scene), why couldn't they get their non-local flint by exchange with other groups, or via task groups, presumably composed of males, who made long-distance treks to the sources for the express purpose of getting flint?

As far as we can tell, the assumptions addressed by these intertwined questions ultimately rest on arguments published more than 30 years ago by Binford (1979) in a seminal paper entitled "Organization and Formation Processes: Looking at Curated Technologies". Subsequent papers—especially one by Goodyear (1979, reprinted in 1989), another by Kelly and Todd (1988), and another by Meltzer (1989), have reinforced this view and helped, *through no fault of their own*, to turn an interesting idea into Paleoindian gospel. A key passage in Binford's paper is worth quoting:

> Raw materials used in the manufacture of implements are normally obtained incidentally to the execution of basic subsistence tasks. Put another way, procurement of raw materials is embedded in basic subsistence schedules. Very rarely, and then only when things have gone wrong, does one go out into the environment for the express and exclusive purpose of obtaining raw material for tools. (Binford, 1979: 259)

While Binford's assertion may well have been true in the particular cases and contexts that he was talking about, and very likely in other specific cases as well, such as the interesting late prehistoric/early historic Pawnee example documented by Holen (1991), why should we turn a handful of examples into a universal generalization that we then apply to all times and all places, including Paleoindian (for other reactions to Binford's overgeneralizing, see Hayden, 1982; Ellis, 1984: 359–360; Wilke and Schroth, 1989: 147; Reher, 1991: 278–279; Bamforth, 2006: 521–522; and Whallon, 2006)? The moment you add a social, political, religious, or other symbolic dimension to the raw material in question, or to its source, or to the item made from that material, or to the context in which that material or item is used—be it flint (e.g., perhaps symbolizing lightning and rain-making), obsidian (e.g., perhaps symbolizing warfare and death), or other material—the argument collapses (see, for example, the interesting ethnographic cases discussed by Hughes, 1978 and Brumm, 2004, 2010; and the equally compelling archaeological example presented by Molyneaux, 2002: 137, 139). In the words of Helms (1988: 119), just the "…sheer distance and the magical or symbolic potency associated with distance or with distant places and polities can be important factors in the value assigned to some resource". But as Bradley (1984: 11) so aptly put it many years ago, archaeological studies of hunters and gatherers are decidedly biased toward utilitarian or economic explanations, with the social, political, and ideological realms inadequately considered or ignored altogether: "…in the literature as a whole, successful farmers have social relations with one another, while hunter–gatherers have ecological relations with hazelnuts". One could substitute "Paleoindian" for "hunter–gatherers" and "high-quality flint" or "megafauna" for "hazelnuts", and it would be a pretty good caricature of a fair amount of thinking in Paleoindian studies nowadays. While some scholars whose work focuses on the stone tools made and used by foragers and small-scale farming societies do recognize the non-utilitarian side of the equation, as the following comments nicely illustrate, such perspectives remain marginal in most lithic studies:

> With the amount and quality of stone flaking materials available, all Paleoindian groups should have been able to obtain what they needed without exploiting subsurface deposits. On the other hand, the effort expended in obtaining subsurface materials by some prehistoric groups raises the possibility that there was always the hope of acquiring something special in the way of stone-flaking materials and that these efforts were driven by more than purely economic needs. (Frison et al., 1996: 213)

> The author recently analyzed the use of Flattop chalcedony from three Folsom sites in Colorado—Lindenmeier, Powars, and Hahn. Flattop chalcedony occurs in two basic colors, white and lavender. Although the white variety can be worked as easily as the lavender and was employed by all other Paleoindian groups who used the quarry, only the lavender variety occurs in the studied Folsom assemblages. This seems to indicate that the white variety was rejected in favor of the lavender. (Stanford, 2005: 303)

> …it is not simply a matter of distance and labor that causes one set of goods to gain currency as desirable objects. The symbolic character of objects had important significance for both prehistoric peoples and those in historic cultural contexts. (Dillian and White, 2010: 11; see also Whitaker et al., 2008)

> There was a pattern of selection of Medicine Lake Highland obsidian for a multitude of uses, yet Glass Mountain obsidian, arguably the largest and most spectacular obsidian flow in the Medicine Lake Highland, was neglected for utilitarian purposes. Instead, Glass Mountain obsidian was used for large, ceremonial bifaces. As such, the archaeological record strongly supports the hypothesis that Glass Mountain was a special obsidian source, reserved for the production of ceremonial and high value objects, and deemed inappropriate for the manufacture of utilitarian tools. (Dillian, 2002: 266)

> Most intriguing is that the area around Nieto [Panama] contains many sources of high-quality cherts and jaspers where bifacial material is absent. It appears that the beauty of translucent crystals over less-attractive stones may have overridden more practical factors in the decision-making process leading to the manufacture of some points. Similar observations were made at the La Mula-West workshop, where Paleoindians manufactured many Clovis-like points from brittle, translucent agate cobbles…found among better-quality cryptocrystalline jaspers. (Pearson, 2003: 317)

> At the quarry, the men spoke of the stone growing up in the ground. Only here at Ngilipitji did true 'killing stone' grow. The cross-sections of weathered rinds were compared to that of a kidney, with the best interior stone of pinky-grey silcrete referred to as *djukurr* or 'fat'. An esoteric oblique meaning of this word is power. It is this mystical power derived from supernatural sources integral to the site that gives the Ngilipitji stone blades their stupendous killing force. Once struck, man or beast is doomed. (Hiatt and Jones, 1988: 10)

Curr (1886: 70–72, cited in Mulvaney, 1976: 79–80, emphasis added; see also Horne and Aiston, 1924: 130, Peterson and Lampert, 1985: 6) provides an eye-opening example of an *annual* 300-mile-long trek, totally unrelated to subsistence, in order to procure a material of great symbolic importance to Australian Aborigines—red ocher. It also shows how far off the mark we can be in assuming that on such extended treks pedestrian foragers would be limited in the loads they could, or would, carry to just a few kilograms.

> *Every winter*, in July or August, a council of all the old men is held, relative to the starting of an expedition for red ochre, to a place called Burratchunna Creek…where there is a large mine of it.
> On the day the party must start, the old men rise with the sun, and grasping their weapons and singing promptly depart without leave taking or farewell to their wives and children.
> The women, …conscious of the men's intentions, commence screaming, screeching, yelling, hooting, hissing, and making all kinds of hideous and uncouth sounds, calling on their husbands, sons, brothers, and friends to remain, and not to be led into a strange and hostile country; they, unheeding, proceeding on their way for about five hundred yards, for the purpose of arranging with the old men who are left behind to build wurleys [Aboriginal huts]…for the reception of the party when it returns. The site being selected, and instructions given to build substantial huts, farewell is taken, the expedition singing a rather mournful ditty, encouraging the young lads to keep up their spirits; and indeed some of them require encouragement, knowing that besides having to *travel over three hundred miles through strange country*, many a hungry belly they will have before reaching their destination, independent of the load of ochre they will have to carry back.

> The party travels about twenty miles a day, and on arrival at the mine each member of it digs out his own ochre, mixes it with water, making it into loaves of about 20 lbs. weight, which are dried.

> *Each man carries an average weight of 70 lbs. of ochre*, invariably on the head, and has to procure his own food; the party seldom resting a day while on the journey, which lasts usually from six or eight weeks….

Howitt (1904: 710–712, emphasis added) provides a very similar account of these annual expeditions for the sole purpose of acquiring ocher, and others for gathering a highly prized native tobacco-like narcotic known as *pitcheri* or *pituri* (*Duboisia hopwoodii*—McBryde, 1987: 258–261; see also Thomas, 1886: 342 and Howitt, 1891: 77–78). Again there is no hint that such treks involved the entire band, nor were they embedded within the group's annual subsistence rounds.

> In July or August *in each year* the Dieri sent out an expedition southwards to procure red ochre. This was always regarded as being a perilous journey, with many dangers and privations. It seems to have been one of the most important duties of the Blanch-water division of the tribe to see to this matter. Some seventy to eighty of the picked fighting-men of the tribe went on this mission, under some great leader.

> They were well armed, and, if necessary, fought their way against all opposition. The distance to be travelled depended upon where the party started from, *and might be as much as three hundred miles*.

> The red ochre, when dug from some aboriginal mine, for instance near Beltana, was kneaded into large cakes, *weighing when dry from seventy to eighty pounds*. The red ochre is used for paint, for magical charms and such purposes, and also for barter with other tribes for spears, shields, and other weapons.

> All the tribes about Lake Eyre, and indeed far beyond it, use as a narcotic the dried leaves and twigs of the Pitcheri bush. The Dieri, at the time when I was in their country, sent a party of able-bodied men *annually* to the Pitcheri country, on the Herbert River in Northern Queensland, *a distance of some two hundred and fifty miles* from their boundaries. This party had to pass through the country of several hostile tribes, and if necessary to fight their

> way. On arriving at the Pitcheri country, the leaves and small twigs of this bush were picked off. Small holes, two feet deep, were dug in the sand and heated with live coals. When the holes were sufficiently heated they were cleaned out, the Pitcheri placed in them, covered up with hot sand, and then baked. When the sap had been evaporated, the Pitcheri was taken out and packed in netted bags or small wallaby skins, *each man on the return of the party carrying a load of about seventy pounds*.

One final example from Australia will suffice, in this case a 300 mile trek for the express purpose of obtaining suitable rock for making groundstone axes or hatchets (Morgan, 1852; see also Smyth, 1878: 359).

> …before I go any further I must say something about their tomahawks…. The heads of these instruments are made from a hard black stone, split into a convenient thickness, without much regard to shape. This they rub with a very rough granite stone, until it is brought to a fine thin edge, and so hard and sharp as to enable them to fall a very large tree with it. There is only one place that I ever heard of in that country, where this hard and splitting stone is to be had. The natives call it karkeen; and say, that it is at a *distance of three hundred miles* from the coast, inland. The journey to fetch them is, therefore, one of great danger and difficulty; the tribes who inhabit the immediate localities being very savage, and hostile to all others. I was told that it required an armed party of resolute fighting men to obtain supplies of this very necessary article…. (Morgan, 1852: 73–74, emphasis added)

The ethnographic literature is replete with examples of individuals and small groups traveling vast distances to fulfill their sacred charge to procure raw materials that were imbued with the requisite symbolic properties, such as obsidian, turquoise, mica, copper, silver, galena, freshwater pearls, quartz crystals, greenstone for making stone axes, salt, marine shells, feathers, shark's teeth and other fossils, furs, hair (both human and animal), red ocher, herbal medicines, catlinite, special construction timbers and wood for making bows and arrows, and many other materials and substances (e.g., McBryde, 1984a: 148, 1987: 262, 265, 2000; Ross et al., 2003: 79). So, why not "high-quality" flint? We suspect that many of the gorgeous, delicate, difficult-to-make, and extremely fragile points for which the Paleoindian period is so justly famous likewise fell in to precisely such a category (e.g., Bradley et al., 2010: 106, 137). Evidence of symbolism, though seldom abundant (or at least in a form that we as archaeologists recognize), is surprisingly widespread in Paleoindian times. Particularly interesting in this regard is Bradley's (1993: 255–256) suggestion that the fluting process itself may have had a symbolic role, at least in Northern Plains Folsom contexts. Equally interesting are the oversized and clearly non-utilitarian fluted points that have been found in a number of Clovis and later caches (often called "hypertrophic" by archaeologists who work in the Midwest and Southeast; e.g., Sassaman, 2005: 83; Emerson and McElrath, 2009: 33). Among these are points that were fashioned from unusually large quartz crystals, a material almost universally thought to have supernatural qualities because of its clarity, brilliance, and especially its *triboluminescent* properties (e.g., Howitt, 1887: 26, 53; Eliade, 1967: 177; Levi, 1978; Powers, 1982: 70; Chandra, 1985; Powers, 1986: 200; Reher and Frison, 1991; Olsen, 1996: 65; Rice, 1998: 63; Brady and Prufer, 1999; Whitley et al., 1999; Miller, 2000: 133–134; Toyoda et al., 2000; Malotki and Gary, 2001: xxvii–xxviii; Koerper et al., 2002: 61–64; Lewis-Williams and Pearce, 2004: 13–19; Saunders, 2004: 136; Warren and Neighbour, 2004; Karr, 2005: 7; Reynolds, 2009; VanPool, 2009: 183). Ellis (1994) attributes a symbolic role to the miniature projectile points found at Parkhill and other eastern Paleoindian sites, as does Stanford (1978) when describing a tiny Hell Gap point associated with a bone flute or whistle and a possible "medicine" post at the Jones–Miller bison kill in Colorado. Finally, Roper (1989) documents the widespread presence of red ocher in Plains Paleoindian caches, and Stafford et al. (2003) present compelling evidence that Paleoindians actively mined red ocher in Wyoming (see also Tankersley et al., 1995; Zedeño, 2009: 412).

To be fair here, we should point out that there are a number of Paleoindian scholars who do not see the caches, the large points, or the red ocher as necessarily symbolic in nature, and their skepticism is not without justification. Demonstrating that a particular artifact or feature conveyed something beyond simple utility can be very difficult. For example, the caches can be seen, not as ritual deposits that were never intended to be retrieved, but as a strategically placed stockpile of raw material, preforms, or finished artifacts that Paleoindian foragers could tap, either *at some point in the future* should the need arise (e.g., Meltzer, 2004a: 127–128; Kilby, 2008), or as a "rainy day" surplus designed to head off potential shortfalls in needed toolstone *during the course of an extended (winter) occupation* (Surovell, 2009: 138–139). Likewise, the large size of some of the "projectile points" could simply mean that they were hafted knives or butchery tools rather than oversized armatures for spears or darts (e.g., Lyman et al., 1998: 897), or perhaps points that had not been repeatedly resharpened and subsequently discarded like those found in kills and campsites (e.g., Kilby, 2008). The red ocher on the proximal end of some of the points or knives could have been added to a mastic used in hafting that would make it more durable and less brittle (see Wadley et al., 2004; see also Allain and Rigaud, 1989—cited in Collins, 1999: 28–29).

But the problem works the other way as well. Just because something appears to be utterly mundane and utilitarian in no way rules out the possibility that in the context of a cache it was laden with symbolic significance. A cracker is ordinary snack food, but in Holy Communion it takes on a totally different meaning. Similarly, lamb can be eaten as one's daily fare, but it can also be offered up to the Divine in sacrifice. John Robb elegantly sums up the dilemma that confronts us as archaeologists in our attempts to artificially dichotomize the material world into those things that are "symbolic" versus those things that are "utilitarian".

> In many ways, the question is not whether we can find symbols archaeologically, but whether we can find anything cultural that is not symbolic. Many powerful symbols in any culture are the commonest things: bread, water, houses, the river, and the hills beyond. (Robb, 1998: 331)

Even if objects have already been used, perhaps heavily and extensively, and perhaps even damaged or broken, in contexts that might reasonably be called "secular" and "utilitarian", this in no way precludes the possibility that at some point in their life history, these same objects were employed—and subsequently disposed of—in contexts that were charged with symbolic meaning.

> Ironically, one such regularity or correlate…is that many artifacts used in rituals would be classified on the basis of their forms as "utilitarian" objects. Sometimes even identical looking objects (e.g., cooking pots, houses, hoes, corn, water) function as either ritual or nonritual artifacts, or both. (Walker, 1998: 247)

> Although the technological functions of artifacts are often readily inferred, identifying their social and ideological functions has been more difficult…; as a result, many archaeologists have fallen back on a more fundamental dichotomy, that between utilitarian and nonutilitarian artifacts. This dichotomy, however, places the study of ritual objects at a disadvantage. (Walker, 1998: 249)

> …it is the act of discarding, rather than the nature of the discarded object alone, that constitutes ritual behavior. The underlying logic holds that the intentional disposal of certain objects helps to harness the power residing in the place of disposal; at the same time, the human act of disposing powerful objects in a certain place lends power to that place…. (Carroll et al., 2004: 131)

While all of this may seem pretty obvious in the abstract, it underscores the difficulty we face in trying to determine whether cached Paleoindian projectile points, preforms, cores, blades, ivory rods, and even unmodified flakes are merely supplies and equipment strategically stashed for a rainy day, or a potent ritual offering whose retrieval was never intended, or both.

In this context, the reader may find it particularly interesting to look at Zedeño's (2009) discussion of objects and substances that most contemporary Native American consultants, when addressing issues related to NAGPRA, consider to either inherently possess "power" or "life force", particularly red pigments and quartz crystals, or to acquire *animacy* through their association with ocher, crystals, or other such objects. The reader may also find it interesting to look at the strikingly similar debate that has been going on for decades among Bronze Age archaeologists in Europe concerning the interpretation of buried "hoards" of metal ornaments, tools, and weapons. Stuart Needham provides a comprehensive and very readable overview of this interchange, and comes to conclusions that aren't all that different from ours:

> …most if not all deliberate deposits were 'ritual' at one level or another, and yet, should circumstances permit and demand, some if not all were also available for recovery. From this viewpoint, it may not be productive to perpetuate the ritual-utilitarian opposition, since ritual and utility are unlikely to have been mutually exclusive categories. (Needham, 2001: 294)

Paul Taçon, in discussing the lithic materials and stone tools made and used by Australian Aborigines, makes some important observations about the symbolic qualities and properties that are often associated with such materials and items, and also notes the particular connection between these objects and symbolic properties on the one hand, and *initiated men* on the other. These kinds of linkages may have been far more common and widespread among foraging societies than most archaeologists would like to acknowledge (see, for example, Ross and Davidson, 2006 for an interesting discussion of comparable sorts of linkages between ritual, "special places", and rock art). We will make reference to this connection more explicitly toward the end of the paper, when we discuss male big-game hunting as a possible form of costly signaling.

> …a growing body of ethnographic evidence supports the contention that many forms of stone tools produced over the past 6000 years in western Arnhem Land have both aesthetic and symbolic value which influenced their manufacture. Much of this is related to ideas about power: the power of Ancestral Beings that created the landscape, including rocky outcrops used as quarries; the power and properties of stone as a substance, and especially quartz and quartzite; *the power of initiated males who made, used and controlled access to certain stone tools*; and so forth. Some of this power was harnessed during the manufacture and later was heightened through ritual, story-telling and other practices. Some of the changes in tool types detected in the archaeological record can be related to these aspects of the belief system. (Taçon, 1991: 194, emphasis added)

In Australia often what is important about a particular type of stone or the quarry from which it derives is not its utilitarian value for producing functional tools, but the connection between the stone or its source and some important mythological event in the distant past or "Dreamtime". These relationships are nicely summarized by Robert Paton and Paul Taçon:

> The nature of restriction of access to resources is thus closely bound up with the idea of responsibility for the Dreamings, or creation myths, associated with the resource and not necessarily with its scarcity or abundance. (Paton, 1994: 178).
> Given this situation, access to any rock outcrop may be restricted by factors such as the importance of the myths directly or indirectly related to the outcrops, the level of knowledge of a person who may want to use or visit the outcrop or the gender of any such individual. These factors have combined to restrict access to some potential rock resources totally while leaving others relatively open. (Paton, 1994: 178)
> This example of exchange clearly shows the embedded social value of the artefacts. Certainly, there is little evidence to support the propositions that either the leilira blades or the boomerangs are intrinsically valuable either as utilitarian items or as a [sic] raw materials which are later used to manufacture more functional tools. There is no substantive evidence that either class of artefact was used, or that either raw material would be difficult to obtain if it were desired for day-to-day tool manufacture. *The real value of these artefacts lies in the socially indispensable messages they help communicate*. (Paton, 1994: 181, emphasis added)

> …there are many levels of sacredness used to define and describe landscapes, with some more sacred than others. Often these are related to issues of restriction and access, powerlessness and authority, initiate and initiated. As people move through different levels of knowledge acquisition, access to more varied sacred sites—hence landscapes—becomes culturally possible. (Taçon, 1999: 41–42)

Many archaeologists seem to assume that in order for a group of hunters and gatherers to have unimpeded access to an exotic flint source, the quarry or outcrop must be located within their annual foraging range; otherwise some other group, simply by dint of proximity, would likely have *de facto* "control" of the source, making access to it unreliable, and possibly even physically dangerous or impossible. In Paleoindian studies this assumption is often *implicit*, and only becomes evident in the way archaeologists delineate the foraging range of individual Paleoindian groups or bands—*these typically are shown on maps as discrete, non-overlapping spatial units*. Whether implicit or explicit, this assumption may not always be warranted. Propinquity to a resource like flint need not imply control, *de facto* or otherwise. Flint sources are absolutely predictable in both space and time. The material doesn't run away, or hide, or learn to avoid human flint "predators", it needs no special preparation to preserve or store it, and, depending on the nature of the outcrop or source, the flint may require relatively little effort to extract. As well, flint is hardly limiting to groups living in the vicinity of the source. As we will suggest below, it is probably seldom limiting to anybody anywhere, except perhaps in the most unusual of circumstances where there simply are no rocks, or none of a type that break with sharp edges. So why control it? Is it worth the risk of possibly having to fight over it? Why not simply step back and let others who want the stuff come in and get it themselves (e.g., Park, 2010: 49–52)?

> Evidence given by Fenton (1940) suggests that chert may not have been a major trade item at contact, at least by the Neutral in whose territory was located the major source. Rather, the data indicate that the source was a neutral area for Huron and Iroquois who needed flint. This is quite comparable to the remarks made by Catlin on the Red Pipestone or Catlinite source in southwestern

> Minnesota. The source was "held and owned in common, as neutral ground, amongst the different tribes who met here to renew their pipes". (Catlin, 1926: 190; Wright, 1967: 187)

Thus, even if a flint source lies within the normal foraging range of some other group, we cannot automatically assume that other groups in the region would have been precluded from recurrently entering the area to acquire flint. Rather than contradicting our previous discussion of exchange, we wish only to suggest that the boundaries delimiting the spatial distribution of a particular flint type need not correspond to the foraging range of any particular Paleoindian group. In fact, the spatial distribution of artifacts made of a given flint type may cross-cut the annual ranges of several bands, perhaps reflecting linguistic ties, intermarriage, shared ceremonies, political alliances, or other such linkages (e.g., McBryde, 1984b: 282, 2000; Whitaker et al., 2008). The bottom line, judging from the ethnographic literature, is that hunters and gatherers gained access to non-local materials, including toolstone, in many different ways, embedded procurement involving an entire social group and some form of down-the-line exchange being but two of these. Reducing our discussions to just these two options, a common practice in Paleoindian studies, is likely to be misleading and ultimately counterproductive.

In developing a theoretical model to account for variability in patterns of intersocietal resource acquisition among small-scale egalitarian societies, Spielmann (1986) understandably focused on foods. However, a similar logic can easily be extended to include intersocietal access to non-edible resources like flint, chert, and obsidian. Thus, in the following quote, we have replaced the words "subsistence" and "food" and inserted in brackets the word "flint". A comment on Spielmann's use of the word "territory" in this quote is also needed. "Territory", as she envisions it, is not a rigidly bounded and invariably closed socio-spatial unit, but rather an area inhabited and regularly used by a group whose boundaries exhibit greater or lesser degrees of *permeability* to other groups depending on the context and the particular resources of concern (see also Kelly, 1992). Thus, a group's "territory" might be closed, at least seasonally, to other groups who might wish to enter the area to hunt large game or collect honey, but open if these same groups wish, instead, to obtain marine shells, obsidian, or flint. Without such mechanisms to monitor and regulate the use of key limiting resources, hunter–gatherers might all have gone extinct a long time ago.

> Two distinct situations which select for intersocietal [flint] resource acquisition have been discussed. The first, which buffers local [flint] resource shortages, involves periodic reliance on [flint] resources outside one's own territory. These resources will generally be acquired by entering that territory and harvesting the resources oneself. Exchange for resources is only expected when resource productivity in the donor population's territory is not sufficiently high to warrant unregulated access to these resources, when the donor population has already invested energy in the procurement/production of these resources, and/or when the obligations exchange implies confer an advantage to the donor population. (modified from Spielmann, 1986: 288)

But models such as these, interesting and useful as they are, often focus largely on a limited range of pragmatic, mostly *economic*, variables that may have been at work in flint procurement, while underplaying those of a more social, political, religious, or symbolic nature (e.g., Duke and Steele, 2010). These non-economic factors may be of considerable importance in traditional societies, and hunter–gatherers are no exception. For example, Chatwin (1987: 57; see also Mulvaney, 2002, Davidson, 2010: 386) eloquently captured the non-economic essence of Aboriginal trade in Australia more than two decades ago in his classic book *The Songlines*:

> The 'goods' did not have to be edible, or useful. People liked nothing better than to barter useless things—or things they could supply for themselves: feathers, sacred objects, belts of human hair.
>
> Trade goods…should be seen…as the bargaining counters of a gigantic game, in which the whole continent was the gaming board and all its inhabitants players. 'Goods' were tokens of intent: to trade again, meet again, fix frontiers, intermarry, sing, dance, share resources and share ideas.
>
> The trade route is the Songline…. Because songs, not things, are the principal medium of exchange. Trading in 'things' is the secondary consequence of trading in song.

Aboriginal quarrying in Australia highlights the potential importance of *non-economic* factors. Specific outcrops where cherts could be obtained were often sacred, powerful, and, not uncommonly, dangerous places on the landscape, and only certain individuals had the religious knowledge and the traditional rights to quarry these materials (Jones and White, 1988; Ross et al., 2003). Others might be able to gain access to these stones, but not directly at the quarry. Instead, the individuals who could safely quarry the rock brought cobbles or blocks of the material to a designated "workshop" area located away from the quarry itself, and at such localities the visitors negotiated for the right to use the material, tested the cobbles that had been brought to them, and participated in ceremonies associated with the place and the event. Thus, in these Australian cases procurement was neither strictly embedded in other subsistence pursuits, nor did its acquisition necessarily involve whole social groups. While the entire process might well be construed as a form of exchange, the material was not passing from group to group over vast distances before it reached its final destination.

So why do archaeologists so often rule out exchange as an important means by which Paleoindians obtained their high-quality flint? Judging by the citations in many of the more recent papers concerning the way in which Paleoindians procured exotic toolstone (see, for example, Skinner et al., 2004: 227–228), it would seem that rejection of this sort of exchange rests heavily on arguments made in a landmark paper by Goodyear (1979, reprinted in 1989) and two similarly influential contributions by Meltzer (1984–1985, 1989). Let us take a brief look at their arguments, beginning with Goodyear's take on mobility and exchange:

> Perhaps the only way to archaeologically monitor mobility patterns among Paleoindian groups is to examine the geographic distributions of the distinctive raw materials they utilized. Here I am referring to tracing exotic raw material distributions from source points to locus of deposition.
>
> Assuming that the presence of exotic lithic raw materials on these sites is truly a by-product of wide ranging movements of human groups, then impressive evidence has been adduced for mobility among Paleoindian groups. While most archeologists would accept that some or all of these distributions are referable to mobility, the question of alternative mechanisms needs to be considered.
>
> The question of procurement by direct expeditions or movement of artifacts by trade or exchange are not processes that can be directly examined using archaeological means. There simply are no archaeological correlates of either…. (Goodyear, 1989: 4–6)

We suspect there are quite a few archaeologists who would take exception to the view that exchange cannot be recognized and studied archaeologically; needless-to-say, there is an impressive literature—for hunter–gatherers, not just complex societies—that would suggest otherwise, far too much to cite here (including a substantial amount that was published during the 1970s, the decade when Goodyear's article originally appeared). Australia alone has a vast literature on hunter–gatherer exchange, and is an obvious place to turn for insights into how such systems functioned and how they might have evolved (e.g., McBryde, 1984a,b, 2000). Closer to home, California and the Great Basin also have a rich ethnographic and ethnohistoric record on exchange, and provide another ideal context in which to examine how such systems operated among widely dispersed and highly mobile foraging groups. True, exchange may be difficult to differentiate from other means of procurement, an issue of *equifinality* that Meltzer (1989) addresses head-on, in the process identifying a number of criteria that might in fact be helpful. Goodyear's argument is also strangely circular—high mobility is used to account for the distribution of the lithics and the distribution of the lithics is used to infer high mobility.

Meltzer (1984–1985: 19) clearly leans toward the view that exotic lithics found in Paleoindian sites, particularly in sites situated "in the low-diversity environments of the northern latitudes", are largely the result of direct procurement by highly mobile residential groups. Nonetheless, in his 1989 paper he takes an admirably cautious position:

> The unfortunate bottom line is that there do not seem to be clear cut rules for sorting direct from indirect acquisition in any deterministic fashion. It is for now impossible to devise bi-conditional statements of the form "certain attributes of an assemblage will appear *if and only if* indirect (or direct) acquisition occurred". From this, it follows that *any* assertion that one or the other of those mechanisms was responsible for bringing stone to a site, particularly assertions unsupported by consideration of alternative possibilities and evidence for [the] same, are empirically unacceptable. (Meltzer, 1989: 30, italics in original)

The usual reasoning that seems to lead archaeologists to reject exchange as a viable means of acquiring non-local flint is tied to the quantities of exotic materials that are found in Paleoindian sites—i.e., if the quantity of non-local flint in an assemblage is above a certain amount, we assume that it would have been impractical to carry such bulk long distances. Curiously, most of these arguments express quantity in terms of relative frequency, not weight. Thus, if, say, 5% or 10% of the assemblage came from some distant source, archaeologists would probably conclude that the quantity of exotics was small enough to have been acquired by exchange, but when bigger figures are involved, say, 30% or 40% or more, many would argue that the amounts were probably too great to have been acquired that way. Instead, as the usual argument goes, the entire band must have been living fairly close to the outcrops during some part of their annual round, near enough to allow them to accumulate large amounts of the material in question.

At least two problems with this sort of reasoning come to mind. First, archaeologists commonly assume that exchange in the Paleoindian context must invariably have been of the *down-the-line* type; hence, the quantity of material would have diminished rapidly as the distance from the source increased. One therefore wouldn't expect non-local lithics to comprise more than a few percent of an assemblage in a site located far away from the source. But why not some form of *directed exchange*, much like the Wyoming-Idaho obsidian, grizzly bear canines, copper, mica, and other "exotic" materials that found their way in considerable quantity into a small number of Hopewell sites in Ohio?

> Crafting aimed at achieving power or influence is epitomized by the power questing evident in the long, challenging journey that relatively few Ohio Hopewell individuals made to Yellowstone…. The quantities of obsidian and grizzly bear canines interred in Ohio Hopewell mounds, and their scarcity in the regions between their sources and southern Ohio, document the journeys that some made from Ohio to the far west. Raw obsidian was brought back to Ohio and then skillfully crafted into large, impressive bifaces, up to 35 cm in length. Vast quantities of debitage under Mound 11 at the Hopewell site attest to the fact that the bifaces were crafted after the trip had been completed. These journeys took people well outside the world of the eastern woodlands, and thus it is not surprising the obsidian bifaces are highly concentrated at the Hopewell site, the site in the Scioto drainage with the most abundant socially valued goods. Hopewell may have been in part a mortuary site for the highest status leaders in the Scioto area….. (Spielmann, 2008: 64–65)

> Thus, many Hopewell Interaction Sphere raw materials would likely have been perceived as powerful. The combination of a long journey and a spiritually extraordinary end point logically suggests the possibility that shaman-like practitioners, initiates to adulthood, or others seeking power went on long-distance power/vision quests or pilgrimages to the potent places in which these materials were found in bulk (e.g., Obsidian Cliff, Wyoming; the Brenham Fall, Kansas; Isle Royale in Lake Superior; the Keweenaw Peninsula of Michigan; Cobalt, Ontario) and that they brought back these materials as evidence of the spirits and/or power they had witnessed and acquired there. Archaeological example tokens of such successful journeys include the books of mica, large raw copper nodules, and large galena cubes found in some Ohio sites; the large silver nuggets and relatively expansive sheets of silver found at the LeVesconte site, Ontario, and the Converse site, Michigan; and the multiple but small silver nuggets and masses from the Hopewell site, Mound 25, Burial 260–261, and from the Snake Den site, Ohio…. (Carr, 2005: 583)

Such directed exchange systems might well have been operating in Paleoindian times as well, as exemplified, for example, by the distribution of Upper Mercer and Flint Ridge flints in Ohio. Mullett (2009) plotted the relative frequency of these two flint types along trajectories radiating out in different directions from the two sources. Along many of these trajectories, the frequency of these materials falls off with distance from source, as most archaeologists would anticipate, and the evidence of resharpening not unexpectedly increases. But several of the patterns don't behave this way, and instead show sharp increases in the frequency of these flint types at considerable distances away from the outcrops. These patterns are precisely what one might find in a directed exchange system. The actual mechanisms by which these flint types moved over the Ohio landscape remain unclear, but it would be premature to rule out systems of accumulation in Paleoindian times not all that different from what has been hypothesized for Hopewell.

Likewise, some, perhaps many, of the Clovis and later Paleoindian/early Archaic caches might also be the end-product of directed exchange systems, in which exotic materials, particularly flint, but perhaps also the red ocher that is found in a number of these caches, were selectively acquired by groups living far from the sources of these materials, and then at some point, possibly the death of an individual as suggested, for example, by Anzick (Montana) and Sloan (Arkansas), these materials were removed from circulation and deposited in the ground (Roper, 1989; Frison, 1991; Reher and Frison, 1991; Jones, 1996: 170; Walthall and Koldehoff, 1998; Stafford et al., 2003; Ellis, 2009). Although, as discussed previously, the context that led to the formation of these caches remains very much in dispute, one has to wonder why Paleoindians would transport exotic flint hundreds of kilometers just to cache it as a reserve for a rainy day when equally suitable flint, at least in terms of its knappability and cutting or piercing properties, would have been available much closer at hand.

The other problem concerns the use of percentages rather than weight when discussing how abundant non-local materials are in archaeological assemblages—30% exotics might sound like a lot of flint to carry over hundreds of kilometers, but if its actual weight tallies up to only a few kilograms or less, this amount could easily have been acquired by exchange, or directly by single-sex task groups for that matter. Expressed in terms of weight, not relative frequency, how much exotic material is actually found in Paleoindian sites? Over what interval of time did it accumulate? In many cases we suspect the actual weight is quite small, a point underscored, for example, by Ellis (1989: 147–148), Bousman et al. (2004: 90), and Stiger (2006: 328–329), in which case exchange or direct procurement by individuals or small task groups making long treks for the express purpose of acquiring flint become quite plausible alternatives to the widespread view of embedded procurement by entire residential groups (Seeman, 1994, provides an interesting possible example from Nobles Pond in Ohio; see also Spiess et al., 1998: 244; and Bamforth, 2009).

Unfortunately, assemblage-wide weight data are surprisingly hard to find in the Paleoindian literature. Virtually everyone reports the frequency of exotic materials just in terms of raw counts and percentages. Fortunately, there are a few cases where information on weight is available, including assemblages from several sites in the Great Lakes area and the West. We will begin with the Fisher Site, which has the most complete data.

The Fisher site is a large Middle Paleoindian (Parkhill phase) encampment in southern Ontario. The weight of the entire lithic assemblage recovered from the studied portion of the site, numbering over 32,000 artifacts and pieces of debitage, has been painstakingly reported by Storck (1997: 278). Exotic flints are not very abundant at Fisher; instead, the site's lithic assemblage is overwhelmingly dominated by quasi-local chert (96% by frequency and 88% by weight), most of which is thought to have come from sources some 20–25 km away (Storck, 1997: 275–276). Although by Paleoindian standards the Fisher site's lithic assemblage is quite large, the weight of material (both local and non-local flints combined) adds up to a paltry 29.5 kg or 65.0 lb (Storck, 1997: 185). The reason the weight of the material at sites such as Fisher, Parkhill, and others is so modest, despite the presence of thousands of flakes, is that most pieces of debitage (>99.0%) weigh less than 1.0 g (Ellis, 1989: 147–148; Ellis and Deller, 2000).

To appreciate just how small this amount really is, let us play a little with the numbers. According to Storck (1997: 266), there very likely were at least two discrete episodes of occupation at the site, and each episode may actually be a palimpsest of repeated seasonal visits to the locality over a period of years (Storck, 1982: 16). This translates into roughly 15 kg (33 lb) of chert per episode and obviously considerably less if each episode is in fact a palimpsest of multiple seasonal visits. While we have no idea how many people came to Fisher during each of these visits, for the sake of argument let us assume that we are dealing with just two visits, one for each episode, involving a few families numbering perhaps 25 people. If we further assume that only adult males transported flint to the site, and there were six men in the party, each man would have transported roughly 2.5 kg (5.5 lb) of material (a figure not all that different from Hayden's, 1989: 8 estimated upper limit of about 2 kg per family). Now, if, say, 60% of the flint had been obtained from distant or "exotic" sources, we are talking about a mere 1.5 kg (3.3 lb)/man/visit. If each episode is actually a palimpsest of multiple seasonal visits to the site, or if others besides adult men (e.g., adolescent males or adult women) brought chert to the site, or if the size of the group coming to Fisher was larger than 25 people, the amount that each individual had to carry would have been less than 1.5 kg (3.3 lb), perhaps substantially so.

Only part of the Fisher Site has actually been studied. Thus, the total amount of flint that was brought there was undoubtedly considerably larger than the figures we have presented here. However, it is also likely that the number of people that visited Fisher and/or the number of seasonal visits that were made to the locality were also larger, presumably keeping the kg of flint per person per visit fairly small.

Loebel (2005: 204, 210) provides assemblage-wide weight data for two Early Paleoindian sites in the western Great Lakes—Hawk's Nest and 11 Ls 981, both in Illinois, and the Withington Site in Wisconsin. Hawk's Nest, the largest of the assemblages, yielded 1750 pieces of debitage with a total weight of 1226.51 g. The tools, totaling 212 items, added an additional 2837.8 g, for a grand total of 4.06 kg (8.95 lb). Site 11 Ls 981 in LaSalle County, Illinois, had 162 pieces of debitage weighing 508.9 g, plus 3 bifaces at 102.7 g, and 17 unifacial tools at 463.1 g, for a grand total of 1.07 kg or 2.36 lb (Loebel, 2005: 252–254, 260). The Withington Site (47 Gt 158) in Grant County, Wisconsin, produced 283 items of debitage weighing 921.5 g, plus 2 channel flakes weighing 1.9 g, and 138 tools weighing 3315.45 g, for an overall total weight of 4.24 kg or 9.35 lb (Loebel, 2005: 326–330).

Let us turn now to western North America. Kilby (2008) provides data on the weight of lithic materials recovered from four mammoth kills at Blackwater Draw (El Llano Dig No. 1, MI-MIV), as well as a mammoth kill (Area 3) and a bison kill (Area 4) at Murray Springs. While these data are only for certain classes of tools (points, bifaces, cores, and blades), and do not include the debitage, the results are interesting nonetheless. For all of these localities *combined*, the total weight is a mere 1.1 kg or 2.4 lb (Kilby, 2008: 234; see also Shackley, 2007: 254).

Kilby (2008: 236) also provides data for the same artifact classes that were recovered from Clovis-period campsite areas at the Sheaman Site (Wyoming) and from Murray Springs (Areas 6 and 7, as well as the tool sharpening and manufacturing clusters from Areas 3 and 4). The total combined weight from the two sites for points, bifaces, cores, and blades (as well as 838 platform-bearing flakes from Sheaman), while substantially greater than the amounts recovered from the kill loci at Murray Springs and Blackwater Draw, still add up to a paltry 28.1 kg or 61.9 lb (Sheaman, 1.9 kg; Murray Springs, 26.2 kg).

As one final example, LeTourneau (2000: 103, 107, 132–134, 140–141, 248–251) provides summary weight data for 2555 Folsom-age artifacts (mostly points, preforms, and channel flakes) from 347 localities in the Southwest and Southern Plains (the majority in northern and eastern New Mexico and western Texas). Most of these artifacts are surface finds, but excavated formal tools, as well as retouched and unretouched flakes, from Blackwater Draw, Lubbock Lake, and John Cotter's 1935 excavations at Lindenmeier, were included. What is most striking about LeTourneau's study is the figure he arrives at for the total weight of the sample—the 2500+ artifacts add up to only 6.7 kg (14.7 lb). Let's put this figure in perspective—there are numerous cases of Australian Aborigines traveling distances of 250–300 miles, every year, solely for the purpose of acquiring such things as red ocher, stone for making axes, sandstone for grinding slabs, and *pituri*, a native tobacco. On these annual expeditions, each man returned home carrying a load weighing as much as 32 kg (about 70 lb), more than 4.5 times the total weight of the Folsom sample analyzed by LeTourneau (e.g., Curr, 1886: 70–72; Howitt, 1904: 710–712). *In other words, just one man, in a single trip, could easily have transported LeTourneau's entire sample from quarries hundreds of miles away and still would have had room to spare*. Admittedly, many of the points and other items studied by LeTourneau were broken (e.g., 84% of the points), so the real weight of these artifacts, had they all been complete, would have been larger. Nonetheless, even adjusted to take fragmentation into account, the total weight would still be very small. Thus, in Folsom times, at least in the Southwest and Southern Plains, it would seem that the absolute amounts of exotic flint moving across the landscape on an annual basis may have been surprisingly modest.

Our figures here for the Fisher Site in the East and the various kill and campsite areas in the West are obviously limited and quite sketchy, but they do suggest that raw counts of exotic flint, even when the tally is in the tens of thousands of items, or double-digit percentage values, do not necessarily translate into massive amounts of flint when expressed in terms of kg per person per year, a conclusion not unlike Luedtke's (1979: 261) interesting observation about Late Woodland flint use in Michigan: "For all estimates it is significant that one canoe load or one very heavy pack load could have satisfied all of a family's lithic needs for a year". *There clearly is a great need for more data on the actual weight of flint, both local and non-local, that is recovered in Paleoindian sites*. Without such data, arguments about the mechanisms by which raw materials moved from quarry to site will remain severely handicapped.

What if our Paleoindian friends, at least those living on the Plains, were using domestic dogs to transport their belongings, each dog either carrying a loaded backpack or pulling a travois (e.g., Fiedel, 2005; Meltzer, 2009: 228)? From data brought together by Wilson (1924: 208, 227–228), Wheat (1972: 119–120), and Henderson (1994: 150), a single dog could have managed a backpack laden with perhaps ~16–23 kg (35–50 lb) or a travois loaded with up to ~30–45 kg (~70–100 lb). In other words, if, as Gilbert Wilson described, each individual, family, or task group had several dogs, quite substantial amounts of flint could have been moving around the Paleoindian landscape independently of the residential mobility of the bands from which these travelers originated.

What if Paleoindians in Eastern North America, and possibly elsewhere, were making use of dugout canoes or other types of watercraft (e.g., Ellis, 1984: 356–356; Engelbrecht and Seyfert, 1994; Gaertner, 1994; Blair, 2010), or even sleds as suggested by Gramly (1993: 8, 59)? Considerable amounts of chert, flint, or obsidian could easily be transported in this manner, whether by direct procurement or by intergroup exchange. This is precisely how some of the masses of Wyoming-Idaho obsidian and Indiana hornstone are thought to have found their way into the caches uncovered in Ohio Hopewell sites (e.g., 8000+ Indiana hornstone disks in the Central Cache in Hopewell Mound 2; 136 kg or 300 lb of worked obsidian in the Crematory Basin in Hopewell Mound 11; and hundreds of obsidian bifaces in Altar 2 in Hopewell Mound 25; see Carr et al., 2005: 488, Table 13.2, 490, Table 13.3; see also DeBoer, 2004; Carr, 2005: 584–585, 593).

For another example of the direct procurement of exotic lithic raw materials in a context that was almost certainly not embedded in more general subsistence pursuits, the reader is encouraged to look at Walter et al.'s (2010) fascinating discussion of the long-distance movement of obsidian in Melanesia and New Zealand. Although in some instances the obsidian was transported, much of it by water, up to 2000 km or more, it nonetheless was used in a surprisingly non-economizing manner, with cores and flakes often discarded well before their potential utility had been exhausted. The authors see the incredible scale of this procurement system as part of a broader pattern of gifting and exchange that helped to maintain viable social networks among related communities that became widely dispersed as a result of colonization:

> Successful social reproduction depends on the ability of communities to participate meaningfully in activities which serve to reproduce social order and institutions, as well as to structure and reinforce social and personal identity. Such acts include gifting and exchange, funerary rituals, rites of passage, marriage negotiations and various acts of competition and rivalry. (Walter et al., 2010: 510–511)

Interestingly, in a recent look at the degree of reduction evident in a relatively large sample of Clovis-Gainey and Barnes projectile points from sites in the Midwest, Shott (2010: 289) found less evidence of economizing than one might anticipate among highly mobile Paleoindian foragers who relied heavily on distant raw material sources for their weapon tips: "One surprising conclusion is that Midwestern Paleoindian bifaces were not heavily curated"—a finding reminiscent of the Melanesian and New Zealand cases just discussed.

Our goal here has not been to argue that Paleoindians never got their exotic flints in the course of their annual subsistence rounds. There are a number of cases, Bull Brook for one (Pelletier and Robinson, 2005), where highly mobile foragers may have acquired quite a bit of their "non-local" toolstone in precisely this manner. Rather, our point is that, solely on the basis of percentages, archaeologists cannot simply dismiss the possibility that Paleoindians obtained their non-local flints through exchange or by long-distance task-specific treks. Despite the high percentages that are commonly reported from Clovis and later Paleoindian sites, the actual *weight* of exotic flints that were moving across the landscape, when expressed in kg per person per year, may often have been quite small (see also Bamforth, 2009: 152 for a similar conclusion).

5. High-quality flint

> *It does not seem that the high-quality lithics were really necessary, from a practical point of view. Serviceable Clovis points could be made out of quartzite or argillite (as they were occasionally in southern Virginia); in Tierra del Fuego, Paleoindians made fishtail points of basalt. Nevertheless, the Clovis culture must have put considerable emphasis on the acquisition of beautiful stones. Fluting, too, was not necessarily the most practical technique for thinning bifaces. Alternative methods might have been less costly in terms of accidental breakage. Importantly, these aspects of Clovis lithic technology may be telling us something about broader cultural values….*
> Fiedel (2000: 83)

Now let us turn to the fourth question enumerated earlier: What is all the excitement about "high-quality" flint in the first place, and why do we perceive it as so important to Paleoindian survival? With some notable exceptions (e.g., Curran and Grimes, 1989: 72; Cochran et al., 1990: 155; Fiedel, 2000: 83), the idea that high-quality flint was essential to the success and viability of Paleoindian lifeways seems firmly embedded in the literature. But, why? As we have already noted, flint and chert sources are the ultimate in predictability and reliability, regardless of the quality. So the real assumption seems to be that *high-quality* raw materials, not just any raw materials, are essential for making the sophisticated projectile points necessary for the very survival of Paleoindian groups. Regular and predictable access to these materials would be required to implement the technological system that made viable a highly mobile, big-game-hunting way of life.

If Paleoindian use of high-quality raw materials was the result of a technological necessity, one would expect that high-quality raw materials would have been used to the near exclusion of lesser grades of raw materials. While it is clear that Early Paleoindian flintknappers often did make significant use of "high-quality" toolstone, it is also clear that fluted points were routinely fashioned from materials of lesser quality. A study of fluted points from the glaciated region of northern Indiana, for example, found that two-thirds (58 out of 85) were made of medium- or low-quality raw materials, many of which were locally available (Cochran et al., 1990). Many of the materials at the lower end of Callahan's (1979: 16) "Ease of Workability" scale were commonly used to make fluted points. While it was presumably easier to make a large, thin, sharp point out of a big block of fine, homogeneous, obsidian than out of a fist-sized cobble of glacial chert, it does not appear that Paleoindian flintknappers were unable to make and use fluted points from lesser grade materials.

If quality were the primary criterion influencing the use of a raw material, one would expect that locally available high-quality toolstone would be utilized when present. Why then did Paleoindians in the American Southwest generally make their projectile points using *exotic* high-quality flints, such as Alibates and Edwards, which they had to procure from sources hundreds of kilometers away in the Southern Plains, but the early Archaic foragers who followed immediately after them in the same areas were content to make their points from basalt, as well as quartzites from the Dakota and Morrison Formations and other less-than-high-quality materials, ignoring cherts almost entirely, *even locally available high-quality cherts* (e.g., Judge, 1973: 144–145; Thoms, 1977: 66; Newman, 1994: 494). In the Late Archaic, obsidian in some of these same areas became a common, if not the preferred, material, and cherts, regardless of quality were still largely ignored (Thoms, 1977: 66; Ellis, 1989: 141 and Gardner, 1989: 14 both note a similar contrast between Paleoindian and Archaic assemblages in eastern North America). The pueblo folk who came after them in the same areas, like their Paleoindian forebears, seemed to prefer high-quality materials, but, unlike Paleoindians, focused on sources that were located much closer to home, such as the translucent multicolored chalcedonies from the Cerro Pedernal in north-central New Mexico and the pink-colored Washington Pass or Narbona Pass chert from the Chuska Mountains along the Arizona-New Mexico border (Thoms, 1977: 66; Newman, 1994: 493–494; Harro, 1997; Cameron, 2001). Then, curiously, some pueblo folk, as in Chaco Canyon (northwestern New Mexico) and Homol'ovi (north-central Arizona), suddenly became very fond of obsidian, often obtained from quite distant sources and which they had previously ignored or used only sparingly (Harry, 1989; Cameron, 2001). The pueblo communities scattered across the Pajarito Plateau in north-central New Mexico are also interesting in this regard. Although they were situated very close to several of the largest and most important obsidian sources in the American Southwest (Jemez), materials that were traded widely across the entire Southwest and deep into the Southern Plains, the Pajaritans nonetheless made surprisingly limited use of this quintessential "high-quality" toolstone until after about AD 1325, when they too quite suddenly developed a fondness for shiny black volcanic glass (Harro, 1997; Brosowske, 2004, 2005). *In other words, good-quality flints and obsidian have always been available in the Southwest, but at times one or the other was mostly ignored, as in the Paleoindian period and during much of the Archaic*. Why did Paleoindian foragers feel obliged to travel vast distances to obtain materials in the Southern Plains that are arguably no more knappable than many of the chalcedonies, jaspers, agates, petrified wood, and obsidian that are widely available in many parts of the Southwest?

Similarly, one might ask why Clovis peoples in the Intermountain West generally made their projectile points out of high-quality exotic cherts, while their quasi-contemporary Paleoarchaic neighbors eschewed cherts for their points and almost invariably zeroed in on fine-grained volcanics such as dacite and andesite, reserving chert for their other, more mundane every day tools (Beck and Jones, 2010: 99)?

Since it seems reasonable to assume that lithic sources don't mysteriously appear and disappear or wander unpredictably around the landscape, some factor or factors other than their cutting and piercing properties must have influenced the raw material choices made by the region's prehistoric inhabitants. And in many places and at many different times, whether in North America, Europe, Asia, Australia, or Africa, foragers often seemed quite content to tip their armaments with fractured pieces of igneous and metamorphic rocks, basalt, limestone, dolomite, argillite, quartz, quartzite, bamboo, mollusk shells, thorns, wood, bone, teeth, bison neck tendons, and a host of other materials (e.g., Weitzner, 1979: 240; McElrath et al., 2009: 10). In fact, just about any stream that has decent-sized cobbles in it can provide sharp flakes for cutting and piercing; and when the cobbles are too small to handle, there's always the bipolar technique, an effective way of producing sharp fragments from undersized pieces of raw material. One doesn't have to transport high-quality flint hundreds of kilometers across the landscape to tip a projectile, or to slice up a tuber or cut off a hunk of bison tenderloin. Certainly, if the high-quality flint is close at hand it might well be the material of preference. But if one is going to use material that has been quarried at some expense of labor and time and then transported by one means or another hundreds of kilometers to get to the place where it was used, something else was going on in addition to, or instead of, its presumed cutting or piercing qualities. As Taçon (1991: 206) put it, in discussing Aboriginal stone tools in Australia: "Indeed, it is sudden changes in tool form, material and manufacture that tell us most definitely that social, symbolic and aesthetic influences have to be considered".

Even if the quarry or outcrop happens to be located within the (presumably) vast annual range of a nomadic band of Paleoindians, so that the foragers don't have to make a special long-distance trek for the sole purpose of acquiring flint, they still have to carry the quarried material—or the cores, bifaces, and finished artifacts made of those materials—hundreds of kilometers as they continue their peregrinations in search of food. This certainly does not conform to what one might expect from least-effort considerations, a point also raised by Ellis (1989: 152) in his discussion of Paleoindian lithic use in eastern North America (for an interesting "neutral model" of the factors influencing the nature and composition of a lithic assemblage across a landscape, see Brantingham, 2003).

6. Paleoindian projectile points

> *If wooden spears and fire could kill off Australia's Pleistocene megafauna, what is the conceivable necessity for stone spear points?*
> White (1977: 26)
> *The real value of these artefacts lies in the socially indispensable messages they help communicate*.
> Paton (1994: 181)

It is also important to note that most of the exotic flints that are found in Paleoindian sites were used primarily to make projectile points (Meltzer, 1989: 25; Amick, 1999; Carr and Adovasio, 2002: 21; Bousman et al., 2004: 95; Bamforth, 2009). While these non-local materials were sometimes used to fashion other tools such as endscrapers (see, for example, Carr and Adovasio, 2002: 28, 30, 35, 40), their primary use seems to have been for making weapon tips (but see Koldehoff and Loebel, 2009: 282 for interesting cases in the Midwest where entire assemblages, not just points, are made on exotic materials). This leads us to the next question: Is a beautifully shaped and finely finished projectile point really necessary in order to make a kill (see the discussion in Odell and Cowan, 1986: 208–209; Odell, 1988; and Sisk and Shea, 2009: 2046)? The answer seems to be "no". For example, Dias-Meirinho (2008) provides two telling cases from the European Neolithic—in one radiograph she illustrates a transverse arrowhead, a small trapezoidal-shaped blade mounted *crosswise*, or perpendicular, to the shaft of the arrow (with the widest edge of the blade toward the front), solidly embedded in a human vertebra. Transverse microlithic arrowheads were commonplace in the European Mesolithic and Neolithic and, judging by their ubiquity, were quite effective as weapon tips (e.g., Friis-Hansen, 1990). In a second radiograph, she shows a small blade whose "tip" was merely the blunt, squared end of an unmodified blade also buried in a human vertebra. Likewise, Wilson (1901), in a broad survey of both prehistoric and historic arrow wounds, describes prehistoric Native Americans who had been shot with hafted perforators or drills, leaving these peculiar "projectile points" firmly embedded in their skulls. In another interesting example, Dinnis et al. (2009) document Upper Paleolithic (late Aurignacian) cases in which carinated burins were apparently hafted and used as weapon tips.

Perhaps the most striking answer to this question is provided by the victims of conflict uncovered by Fred Wendorf and his team in a Final Paleolithic cemetery near Jebel Sahaba in Sudanese Nubia (Wendorf, 1968; Honegger, 2008: 163). Here, the excavators found nearly 60 bodies, many of whom had been shot with arrows or darts. Some of the projectile points were found embedded in bone while others were in contexts that made their function as weapon tips highly likely. Wendorf's comment about these points is an eye-opener:

> The most impressive feature is the high frequency of unretouched flakes and chips. In a normal assemblage all of these would be classified as debitage or debris, and none would be considered tools. Yet many of these pieces were recovered in positions where their use as parts of weapons is irrefutable. They were found imbedded in several bones, inside skulls, and in many positions where any other explanation seems unreasonable. (Wendorf, 1968: 991–992; quoted in Odell and Cowan, 1986: 197)

Let's pursue this line of questioning a little further still. Does one need a stone point of any sort on the end of a projectile weapon to kill a big animal? We suspect most Paleoindian archaeologists (and archaeologists, in general) would respond in the affirmative. This is one of those intuitively obvious things that we seldom question. But recently a few have, and the answer makes what has seemed like a no-brainer much less obvious and clear cut (e.g., Haynes, 2007: 92).

Consider the Schoeningen spears, several beautifully made, shaped and balanced, 300,000-to-400,000-year-old wooden spears found in Germany that were used for killing wild horses and probably other large game as well (Thieme, 1997, 2005: 128; Steguweit, 1999; Rieder, 2000). Aside from their remarkable age and marvelous state of preservation, what is striking about them is that they *were not designed to carry stone tips*. Another, somewhat younger, sharpened wooden spear was found many years ago at the Middle Paleolithic site of Lehringen, also in Germany. Like the Schoeningen examples, the Lehringen spear, which has been dated to the Eemian Interglacial (MIS 5e), was also not designed to be tipped with a stone point (Thieme and Veil, 1985; Veil, 1990–1991; Gaudzinski, 2004). The Schoeningen and Lehringen finds—and the apparent scarcity generally of *bona fide* stone projectile points during the Eurasian Middle Pleistocene and subsequent Middle Paleolithic, despite widespread evidence for prime-adult-oriented big-game hunting by Neanderthals and their predecessors (e.g., Moncel et al., 2009: 1906; compare the conclusions reached by Beyries and Plisson, 1998 concerning the function of Middle Paleolithic "projectile points" with those drawn by Shea, 1988, 1989 using artifacts from the same site)—underscore the possibility that stone tips may not be essential to the successful functioning of thrusting or throwing spears, a point already made more than 30 years ago by White (1977: 26) with regard to traditional Aboriginal spears in Australia: "If wooden spears and fire could kill off Australia's Pleistocene megafauna, what is the conceivable necessity for stone spear points?"

The same conclusion also applies to atlatl darts and arrows, judging from the many specimens that have been recovered from dry caves and rockshelters in western North America. Many lack stone tips and clearly were not designed to have them (Waguespack et al., 2009; see also Ellis, 1997; Odell, 2000: 299–300; Zeanah and Elston, 2001: 100; Bryan and Gruhn, 2003: 175–176; Lyman et al., 2009: 10–11). Instead, their wooden ends were tapered to a point (which was sometimes "fire-hardened"), or the end was socketed to receive a sharpened wood foreshaft.

Ethnographic observations and numerous experimental studies using replicated weapons reinforce the picture provided by the data from the dry caves and rockshelters (Odell and Cowan, 1986; Fischer, 1989; Holmberg, 1994; Dockall, 1997; Ellis, 1997; Smith, 2003; Cheshier and Kelly, 2006). For example, Waguespack et al. (2009), in a global survey of 59 ethnographically documented subsistence hunters, found that nearly 65% of the groups they examined used wooden-tipped projectiles in addition to ones armed with stone or metal, and that many of these hunting peoples employed organic-tipped projectiles for both small and large game.

Joe Medicine Crow, in a classic comment on the types of projectile points used by the Northern Plains Crow for hunting bison and in warfare, underscores the effectiveness of an arrow fired at big game *without a stone point at its tip*:

> The Crow Indians used…two types of arrows for hunting and warfare. Arrows with small points (so-called "bird points") or no points (such as greasewood shafts with heat-hardened tips) were used mainly for the quick and deep penetration of standing animals. Some bowmen were reputed to be able to send such arrows clear through animals, including the big buffalo, with an effect similar to that of steel-jacketed bullets. (Medicine Crow, 1978: 251)

Wooden-tipped arrows were not only used for hunting: they were also used with great effectiveness in warfare. As recently as 1987 in the Southern Highlands of Papua New Guinea, vanGurp et al. (1990) within a 12-month period surgically treated 90 patients who had been shot with arrows. According to these authors, "the arrows used have a total length of about one metre and are composed of two parts. The shaft is a dried length of bamboo to which is lashed a sharpened segment of dense wood, black palm 10–30 cm long, that is further hardened by heating" (vanGurp et al., 1990: 183; Jacob, 1995: 394).

In addition to summarizing a wealth of ethnographic data, Waguespack et al. (2009) conducted a series of controlled archery experiments using a remotely triggered 60 lb bow and a series of arrows of comparable dimensions and weight, some of which were stone-tipped, others having only sharpened wooden ends. While the former penetrated farther into gel targets than those tipped with wood, the differences, though statistically significant, were relatively small (both attained depths greater than 20 cm) and therefore may have been of little consequence to hunters in real-life situations (see also Holmberg, 1994). In concluding, the authors raise the interesting possibility that the incentive to invest time and labor in the manufacture and maintenance of arrows tipped with stone may lay more in the realm of costly signaling (including warfare, not just hunting) than in the more mundane sphere of "lethal efficiency" (Waguespack et al., 2009: 797). Hildebrandt and McGuire (2002: 239–240) come to a very similar conclusion regarding the fluorescence of obsidian biface production during California's Middle Archaic:

> Given the importance of the social context of large-game hunting…, we might reasonably ask how show-off behavior(s) would be expressed along a variety of other cultural dimensions, including technology. In this regard, the relationship between hunter and hunting weaponry is obviously fundamental. The

> most visible aspects of this relationship are projectile points and other formalized piercing, cutting, and scraping tools; however, we would also argue more encompassing flaked stone acquisition, reduction, use, maintenance, and discard systems, to the extent they are geared toward providing hunting-related tools, reflect the evolutionary exigencies of high-prestige hunting. This is particularly the case for high-quality obsidian, which was available from a limited number of geologic sources but widely distributed in archaeological sites across the state.

There is another dimension that needs to be considered—chipped-stone projectile points can be quite fragile, and Folsom and many of the long, thin, beautifully-flaked Plano points are right out there at the extreme. Care would have to be taken so that they wouldn't break during transport. This sometimes involved carrying unfinished points and only completing them on an as-needed basis. At other times and places foragers carried points, singly or in bundles, in bark or leather "wallets" (e.g., Jones and White, 1988: 83), and some Australian Aborigines even carried them wrapped in their own hair (Akerman et al., 2002).

Spear points and dart points often break during use, not uncommonly on the first impact, losing their sharp tip, or fracturing along the blade or at the haft, leaving the hunter in the unenviable position of having to face an angry adversary while armed with a very blunt-tipped weapon (Odell and Cowan, 1986; Churchill, 1993; Ellis, 1997; Cheshier and Kelly, 2006). Hunzicker's (2008: 303) recent experiments with replicated Folsom points show this very clearly. In these experiments he shot Folsom-tipped atlatl darts at bovine rib cages. Over two-thirds (68%) of the delicate points were damaged on impact, 56% of them severely enough to require rejuvenation before they could be used again. Thus, prior to the widespread use of metal points, which are far more resistant to breakage than traditional stone points, it is entirely conceivable that hunters may have used wooden-tipped spears or darts to disable large prey and only once it no longer presented a serious threat would they then have switched to stone-tipped weapons to deliver the *coup de grâce*.

Holmberg (1994) conducted a series of archery experiments that produced results with wooden points quite similar in many respects to those obtained by Waguespack et al. (2009). Holmberg used a recurved bow with draw weights ranging from 20 to 50 lb, shooting into three different types of targets from a fixed distance of 6 ft. Unlike most other experimental studies, Holmberg varied both the materials from which the projectile points were fashioned and their shape, from simple tapered wooden points, some fire-hardened, to large triangular and lanceolate flint points. Not surprisingly, he found that obsidian points were very prone to breakage; but he also found that simple tapered wooden points seldom broke, even when they directly impacted bone, and could be used over and over again with only minor rounding or denting of the tip (fire-hardening seemed to have no bearing on the resilience of the tips). Wooden points also did extremely well in terms of penetration, although the internal damage they produced was nowhere as extensive as that produced with large triangular flint points. He obtained the best overall results with comparatively small triangular points, especially those with serrated edges, while large lanceolate and triangular points did quite poorly.

Smith (2003) conducted an interesting set of experiments using wooden-tipped thrusting and throwing spears, the latter thrown by an experienced javelin thrower, and 15 kg lamb carcasses as targets. Inspired by the spectacular discoveries at Schoeningen, his goal in these experiments was to determine whether *wooden-tipped* spears would leave damage on bone distinctive enough that one could identify their use in prehistoric contexts. While that aspect of the study did not yield positive results, the experiment did lead to conclusions that are directly relevant to the issue at hand here:

> …it is apparent that the rib cage is a prime target area for a spear. The javelin and thrusting spears achieved high degrees of penetration into the lamb rib cages, illustrating the effectiveness and efficiency of these implements as hunting weapons. In addition, both [thrusting and thrown] spears displayed exceptional durability and withstood at least 40 direct hits on bone, with both points displaying only moderate point degradation. (Smith, 2003: 112)

The impressive durability of projectiles tipped only with sharpened wood may at first seem counterintuitive; one would think that stone is stronger than wood and hence points made of flint or chert should break less easily. But there are two different kinds of "strength" that enter into the picture here—compressive strength and tensile strength. Brittle materials like flint and chert are very strong when compressed, but surprisingly weak when subjected to tensile forces; it is this characteristic that allows these materials to be flaked so readily by an experienced flintknapper. However, their low tensile strength also makes stone projectile points more likely to break from the vibrations generated in the shaft when the dart or arrow is launched, and again when the weapon impacts the target. The nature of these forces and the role they play in the design of effective weapon systems are discussed at length in Hughes' (1998) interesting paper "Getting to the point: evolutionary change in prehistoric weaponry".

So why arm a spear or atlatl with a tip that was especially difficult to make and frequently broke in the process (e.g., Hemmings, 2004: 208)? Folsom points provide an excellent example, because there have been numerous attempts to understand and replicate the fluting process. Flenniken (1978) and Sellet (2004), both experienced flintknappers, estimated that nearly 37% of all attempts to manufacture these delicate and extremely thin fluted points ended in some degree of failure. By contrast, Flenniken and Raymond (1986: 604) experienced a failure rate of less than 17% when replicating Elko Corner-notched dart points. Similarly, Woods (1987: 26–29) and Gene Titmus, also qualified flintknappers, in experiments designed to replicate both Elko dart points and Eastgate/Rose Spring Corner-notched arrow points experienced a failure rate of ~21% and ~17%, respectively. These studies, and others (see, for example, Ahler and Geib, 2000: 800 and Bousman et al., 2004: 92–93), corroborate the obvious—Folsom points are difficult to make without breaking or damaging as many as a third or more of them in the process. Does fluting really add so much to the point's effectiveness as a killing device that it warrants such waste of time and effort (see, for example, Amick, 2000: 138)? If so, why is fluting unique to the New World—in fact, to only a small part of the New World? Surely others, somewhere, would have hit on the same idea if it truly offered unique advantages as a haft design. Why did it disappear after a comparatively brief fluorescence in popularity to be replaced for the remainder of the Holocene by more easily fashioned weapon tips with haft designs that one finds, in one form or another, around the globe—side-notches, corner-notches, stems of various shapes and sizes, and simple triangles.

Why use an object of fragile beauty that was made from exotic materials the hunter could not easily replenish? It stretches the imagination to think that Paleoindian hunters did all this simply to enhance their chances of putting meat on the table. As Bamforth and Hicks (2008: 136) put it:

> Paleoindian points are good examples of excellent flintworking, suggesting that they were made by highly skilled individuals who cared about their craft and devoted sufficient time and attention to it to produce objects that were more beautiful and technically sophisticated than was necessary for them to serve their utilitarian purpose effectively.

Some archaeologists might well respond that the fragile nature of these points was intentional; if the point broke inside the animal it would cause substantial bleeding and hence increase the likelihood that the animal would die (Christenson, 1997; Zeanah and Elston, 2001: 99–100). True enough. But it might also increase the chances that the hunter would first have to pursue the animal, possibly for hours, even days, until it bled to death, while a simple, comparatively small, slightly lanceolate- or spindle-shaped (fusiform) wooden projectile that opened a hole in the hide large enough to reduce friction on the shaft as it entered, and that slid between ribs and penetrated the lungs, would immobilize the animal and eliminate the chase that might otherwise be necessary (e.g., Medicine Crow, 1978: 251; Frison, 1986: 118–119; Friis-Hansen, 1990).

Guthrie (1983: 282), based on a series of experiments with different types of projectile points, provides an additional reason why a shot deliberately targeting the lung area is preferable to aiming at other parts of an animal's body:

> The deepest penetration occurred in the thoracic area, between the ribs. Here the intercostal muscles between the ribs form a stretched "drumhead" surface. These provide a stiff backing to the overlying tough skin, whereas, the soft large muscle masses and abdominal structures form soft cushions behind the skin, decelerating the point before penetration.

A projectile point, especially if it is barbed, making the arrow difficult to extract, and that enhances bleeding and internal infections by breaking or shattering as it enters a body, might be far more effective when fighting conspecifics than when trying to feed oneself or one's family (e.g., Bill, 1862; Coues, 1866; Cunnar et al., 2009: 205–206). In warfare you want your enemy to die, but when or where the victim dies is usually of little consequence (Dodge, 1884: 419; Dorsey, 1896: 286). According to Bill (1862), a U.S. army surgeon with wide experience in the treatment of arrow wounds, Native Americans in combat often deliberately targeted the abdominal area of their enemies. By rupturing the intestines, the chances of infection were greatly enhanced. Moreover, the body fluids would soften and lengthen the tendons used to bind the arrowhead to the shaft, increasing the likelihood that the point would detach when any attempt was made to extract the arrow (Humfreville, 1897: 169). This may also help to explain the common preference for obsidian points in warfare, since, in addition to their possible color symbolism, they are especially likely to break as they enter the victim, increasing the chance of infection and a prolonged and agonizing death (e.g., Pétrequin and Pétrequin, 1990: 492; LeBlanc, 1999: 47, 111; Saunders, 2001; Ikäheimo et al., 2004: 15; Hodgson, 2007). Meyer (1971 [1851]: 263) provides a particularly vivid mid-19th-century description of the use of war arrows by the Yurok (California) that were tipped with fragile obsidian points:

> The glass arrows are the most dangerous. Their points are from one to one and a half inches long, three-cornered and jagged. They are fastened to the arrow by means of a firm mass of resin. If they penetrate a human body the glass generally splinters on the bones, the wound promptly festering with fatal results.

But using a weapon system that prolongs the demise of a game animal over days is a risky strategy if the hunter's immediate goal is to minimize pursuit time and guarantee that one's family has food that evening.

7. Why do Bushmen and Hadza hunt big game?

> *If Hadza men were primarily concerned with feeding their wives and children, they would do better by pursuing a broader range of resources, including small game and plant foods, both of which are much more reliably acquired and far more readily defended against the claims of others than are large animal carcasses…. The fact that they rarely adopt this strategy indicates another goal for big game hunting, the most likely candidate being prestige, which affects their status relative to that of other men….*
> O'Connell et al. (2002: 836)

Now let us shift gears and consider one final issue, perhaps the most difficult and controversial one of all, but also in many ways the most interesting: Was Paleoindian big-game hunting, first and foremost, an endeavor to put food on the table, with prestige an important but secondary result of a successful kill, or could big-game hunting instead have been primarily about male prestige and costly signaling, with food an obvious but nonetheless secondary outcome (see also Borrero, 2009: 160)? To address this issue, we begin first with a close look at the hunting strategies and outcomes of the two best known hunting and gathering societies of the ethnographic present—the Bushmen or San of the Kalahari Desert in southern Africa (Botswana and Namibia), and the Hadza in eastern Africa (Tanzania). This may seem to be a detour from our focus on North American Paleoindians, but we think the rather counterintuitive insights that emerge from a close look at these modern hunters may offer a rather different view of why Paleoindians might have engaged in hunting proboscideans, bison, and other mammals at the upper end of the body-size spectrum. As we will attempt to show, the hunting strategies of the Kalahari San or Bushmen, contrary to the standard view presented in most introductory anthropology textbooks, are actually quite bizarre and clearly illustrate the shortcomings of using a weapon system that does not immobilize the prey.

San hunters have very low success rates, particularly for large game (Hitchcock et al., 1996: 175). For example, Richard Lee documented the hunting activities of Ju/'hoansi (!Kung San) over 28 days in July–August, 1964. During that period, seven men put in a total of 78 person-days of hunting, successfully killing animals on only 23% of those days (Lee, 1979: 267; Hitchcock et al., 1996: 182).

A few years later, in 1968, John Yellen observed the Ju/'hoansi for a period of 80 days. During this period, men made no attempt to hunt on 14 days and failed to procure anything on an additional 25 days, indicating that on nearly 50% of the days the hunters made no successful kills. Moreover, most of the catch was small animals, especially porcupines and springhare, as well as a number of birds (Hitchcock et al., 1996: 175). If one considers only the ungulates, their success rate was much lower.

Not only do the San frequently fail in their attempts to kill big game, but the way they go about it is truly perplexing. When one of us (JDS) was a graduate student in the late 1960s, publication of the *Man the Hunter* symposium was like a shot of adrenalin for those of us who were interested in hunters and gatherers (Lee and DeVore, 1968). Almost overnight the Ju/'hoansi San (in those days referred to as the !Kung Bushmen) became the gold standard by which we viewed and interpreted the hunter–gatherer past. Almost everything came to be seen "through Bushman eyes". Thus, the way the Ju/'hoansi hunted was the way all good hunters, past or present, must have hunted—with uncanny stealth (Stander et al., 1996) and amazing skill as trackers (Liebenberg, 1990, 2008).

Yet, at more or less the same time, JDS was influenced by a fellow graduate student, George Frison, an expert on hunting, whose insights stemmed from vast archaeological expertise combined with years of experience as a hunter in his own right. According to Frison (1978: 366), big-game hunters whose livelihood depended on the outcome of the hunt would leave as little to chance as possible: "there was careful consideration…as to where animals were killed; nothing was killed where the effort of recovery exceeded the value of the meat...." A good hunter would also choose a weapon that was appropriate for the behavior and size of the prey, and direct a shot at the animal that would either kill it on the spot or at least immobilize it (Frison, 1998: 14579).

Contrast Frison's observations with Bushman hunting. The San use tiny bows, not shock weapons. Robert Hitchcock, an anthropologist who has worked for many years among the San, together with colleague Peter Bleed, provide an interesting statement regarding the nature of San bows and poisoned arrows:

> Poisoned arrows have neither knock-down power nor the ability to open a serious bleeding wound.... Their intended function is simply to introduce the poison. They are most effective against relatively small game and, in any case, they kill relatively slowly; in addition, animals, once hit, must be tracked by the hunters. Often, tracking goes on for long distances. (Hitchcock and Bleed, 1997: 354)

According to Bartram (1997: 325), Kua San bows are less than a meter in length and have an average draw weight of only 8–10 kg (18–22 lb). Silberbauer (1981: 206) provides a similar estimate for the diminutive bow of the Central Kalahari G/wi San—about 9 kg (20 lb). For San (Ju/wasi or Ju/'hoansi) in Nyae Nyae, Namibia, Thomas (2006: 128) gives a slightly higher pull of about 11 kg (25 lb), but adds that: "to kill a deer-sized animal with an unpoisoned arrow requires a much heavier, longer arrow and a much more potent bow—one with a 50 or 60 lb. pull, minimally."

How do Bushman bows stack up against what modern archers would consider appropriate for a hunting weapon? Sparano (2000: 692) in *The Complete Outdoors Encyclopedia* recommends a draw weight of 9 kg (20 lb), or less, for *children* between the ages of 6 and 12, and at least 23 kg (50 lb) for deer hunting. In other words, the draw weight of Bushman bows falls within the range of weights that modern bowyers recommend for children! This is hardly a shock weapon and clearly not one designed to deliver a lethal or immobilizing shot to a large animal.

Not only are Bushman bows weak and ineffective, they use very slow-acting poisons that, according to Lee (1979: 219), take "an average of 6 to 24 hours or more to work". Thomas (2006: 126) indicates that the poison may take even longer to achieve the desired effect: "the poison is the lethal factor, but the process is slow—one to four days, more or less, largely depending on the size of the victim—during which time the injured party could inflict a tremendous amount of damage on its tormentors". As a result, Bushman hunters have to invest inordinate amounts of time tracking wounded animals across the landscape, animals that they frequently lose to other predators (e.g., Hitchcock and Bleed, 1997: 354; Liebenberg, 2006: 1022; see also Meltzer, 2009: 271 for a discussion of what might be similar unsuccessful pursuits by Paleoindian hunters).

> The recovery rate for the ambush hunting activities was 88% and the mean number of man-days spent tracking wounded animals was 1.5. We suspect that this figure is high. The average recovery rate for animals shot with poisoned arrows by the Ju/'hoansi is closer to 50% due to the frequency with which predators and scavengers reach the animal before those trailing it do and the numbers of times that the animal evades its pursuers. (Hitchcock et al., 1996: 185)

In other words, in stark contrast to Frison's perspective of what an economically-motivated hunter must do in order to make ends meet, the San leave a great deal to chance, fail frequently, and invest a huge amount of time and effort doing so. This is hardly a strategy designed to maximize returns of calories, protein, or fat, nor is it an effective way to minimize time, or opportunity costs, unless the payoff lies in some domain other than food.

The Bushman strategy becomes even more perplexing if one considers the number of years (actually, decades) that it takes a hunter to master the skills needed to successfully locate, pursue, and kill large game. According to Walker et al. (2002: 639), hunters do not attain their peak level of performance until they are well into their thirties or even later, long after they pass their physical prime. If, as Hawkes (2000: 65) so aptly put it, "in the long run, big-game hunting is inferior to available alternative strategies for provisioning families", one has to wonder why hunters invest so much of their life, starting already as children, honing these particular skills?

Hadza hunters in Tanzania, though using a much more powerful bow than the one typically used by the Bushmen, do not fare much better in their hunting endeavors. Even though Hadza bows have draw weights of 45 kg (100 lb) or more (Woodburn, 1970; Bartram, 1997), and arrow points smeared with poison, "individual hunters...fail to kill (or scavenge) large game on 97% of all hunting days" (Hawkes et al., 1997: 573).

Even more eye-opening is Hawkes (2000: 64–65) comparison of Hadza hunting success with the return rates that hunters might expect if they instead devoted their efforts to other subsistence pursuits. Hadza men, on average, devote more than 4 h per day to hunting, and yet take home only about 0.12 kg/h of meat. Hawkes concludes that, in terms of caloric returns, adult Hadza men would enjoy higher return rates by gathering.

The tremendous day-to-day variance in hunting success, in which an "average hunter can expect a full month of failures for every day he scores", would be devastating as a family provisioning strategy, especially for children (Hawkes, 2000: 65). O'Connell et al. (2002: 836) take this line of reasoning a step further, concluding that prestige, rather than nutrition, underlies the Hadza's focus on big-game hunting. Thus, just in terms of success rates, big-game hunting by the Hadza, as was the case for the Bushmen, seems like a very inefficient and unreliable way of putting food on the table.

Given the rather dismal and unreliable returns of Bushman and Hadza big-game hunting, let us take a look at some of the alternative foods that these hunter–gatherers have at their disposal, and the times of year when these alternatives are available. In this discussion we omit the citations documenting the nutritional details of these alternative foods; all of these citations, which if included here would more than double the length of the "references" section, may be found in Speth (2010). Let us begin with the San. Through Lee's (1968, 1979) seminal work among the San in the 1960s, we know that the Ju/'hoansi relied heavily on mongongo nuts (*Schinziophyton rautanenii* or often *Ricinodendron rautanenii*). In the area where Lee did his field work, groves of mongongo trees were extremely productive in most years, and according to his input–output studies provided, on average, about 40% or more of the Ju/'hoansi's daily energy intake. The actual percentage varied seasonally from a low of about 10% in the late summer rainy season to as high as 90% in the fall and early winter (dry season) months when the fruits ripened and dropped to the ground (Lee, 1973: 320).

Processing the mongongo is labor-intensive and takes a fair amount of skill, but return rates are substantial, especially during the major fall/early winter harvest season; at that time families often camp in or close to the groves in order to minimize transport costs. Mongongo nuts are available throughout much of the year, since considerable numbers remain edible long after they have fallen to the ground (Lee, 1968, 1973). However, as groves close at hand are progressively harvested out, families have to visit more distant groves, increasing overall travel time and hence transport costs.

Mongongo nuts contain very high levels of fat (45–58 g/100 g) and are very rich in protein as well, averaging between 26 and 29 g/100 g. Lee (1979: 270) observed that Ju/'hoansi during the harvest season ate about 300 nuts per person per day, which according to Duke (2000: 258) would contain "the caloric equivalent of 1,134 g of cooked rice and the protein equivalent of 396.9 g lean beef".

What is perhaps most intriguing about mongongos in the context of the present discussion is the fact that they are most abundant in the months of April, May, and June, precisely the months when the Ju/'hoansi also bring the most kilograms of meat into camp (Hitchcock et al., 1996: 201). Tsin beans (originally *Bauhinia esculenta*, now *Tylosema esculentum*), another very important source of protein and fat for many San groups, ripen at this same time of year. In other words, Ju/'hoansi hunting activities peak at more or less the same time that the return rates from harvesting and processing mongongo nuts and tsin beans also peak (~1300 kcal/h for the mongongo according to Sih and Milton, 1985: 399). If there were any time of the year when the Ju/'hoansi would *not* need to hunt large game for either fat or protein, this would be it.

The same pattern also holds for G/wi San foragers who occupy Botswana's arid Central Kalahari region (Silberbauer, 1973, 1981). While the G/wi do not have mongongo nuts, the availability of other plant foods reaches its maximum during the rainy season, at more or less the same time that their hunting returns also peak (see Speth and Davis, 1976: 443, Table 2; and Deacon and Deacon, 1999: 142, their Figure 8.11).

Thus, while not all San have access to mongongo nuts, most groups, like the G/wi, have alternative wild plant foods that serve as more or less comparable staples:

> …resources such as the mongongo nut, which in some months provides 70–80 percent of the diet in the /xai/xai area…, are completely absent in other *n!ores* 50 km to the west in the centre of the Nyae Nyae area and are not a staple food for any of the Nyae Nyae !Kung (Marshall, 1976). In this latter area the Tsin bean replaces the mongongo as a staple. In the N/umsi (Dobe)/-xai/xai areas, even adjacent *n!oresi* are known to specialize in resources at given times of the year, with some being known for mongongo nuts (*Ricinodendron rautanenii*), others for tsin beans (*Tylosema esculenta* [sic]), others for baobab fruits (*Adansonia digitata*), and so on, all of which are rich in protein, minerals and vitamins. (Wiessner, 1981: 644)

Although Polly Wiessner only emphasizes the protein content of these foods, as is so often the case in ethnographically-oriented nutritional studies, tsin (marama or morama) beans (*Bauhinia esculenta* or *Tylosema esculentum*) and marula nuts (*Sclerocarya caffra* or *Sclerocarya birrea* or sometimes *Poupartia birrea*), in particular, are not only high in protein (30–38% and 23–36%, respectively), they also are rich in fat (33–42% and 42–63%, respectively), making them extremely important staples.

The Tyua San in northern Botswana provide another interesting case in which the timing of big-game hunting coincides quite closely with the peak production of other more reliable and productive foods. And again these are foods that are rich in both fat and protein. The principal large-game species hunted by Tyua are kudu and wildebeest, with most kills, according to Hitchcock (1988), occurring in the rainy season, particularly in January and February. The wet season is also the time when nutrient-dense marama beans become available.

The Tyua have another extremely important resource as well, in this case an insect, the mopane worm, which becomes available at this same time of year, and often in prodigious quantities (generally classified as *Imbrasia belina* or *Gonimbrasia belina*). The mopane "worm" is actually the instar or larval stage (caterpillar) of the Emperor moth. Normally, there are two "outbreaks" or generations of caterpillars each year, the principal one occurring in the early months of the rainy season between November or December and January, and a second more minor one between March or April and May (Ditlhogo, 1996; Stack et al., 2003; Morris, 2004: 83). Mopane worms are highly prized in southern Africa, and have become the focus of a thriving commercial industry, both as a much sought-after delicacy for human consumption and for use as a feed for livestock (Illgner and Nel, 2000). According to Ekpo et al. (2008: 8), "thousands of tons of *Imbracia* [sic] *belina* larvae are eaten by people in Africa. Almost all known cooking techniques have been tried on the larvae. It can be eaten fried, dried, raw grilled and boiled". Their importance in southern Africa is such that the Republic of Botswana, in 2000, inaugurated a series of five *pula* (meaning "rain") coins with the mopane worm shown on the reverse. Given their economic importance, it is not surprising that mopane worms have been the subject of an array of nutritional studies. While most of these have emphasized the caterpillar's high protein content, which typically falls between about 48% and 62%, mopane worms are also rich sources of fat (average 15–19%). In terms of total energy, mopane worms provide about 450 kcal/100 g.

Thus, it would appear that Bushman groups throughout the Kalahari have access to a number of nutrient-dense and often quite abundant plant, animal, and insect resources that become available at more or less the same time of year that they undertake much of their big-game hunting. This intriguing temporal convergence raises the possibility that the San hunt these animals for reasons other than fat or protein. Perhaps, instead, it is precisely because of the reliability and high fat and protein content of mongongos, baobabs, tsin beans, marula nuts, mopane worms, and others that Bushman hunters are able to afford the "luxury" of engaging in such a time-consuming, failure-prone, and costly activity (Hilton and Greaves, 2008, come to very similar conclusions in their interesting study of Venezuelan foragers). In other words, an explanation for their hunting behavior may well lie beyond the strictly nutritional realm.

Now let us shift our focus to the Hadza. For these East African foragers, baobab fruits and seeds (*Adansonia digitata*) assume much the same role that mongongo nuts do for the Ju/'hoansi. Baobab seeds contain almost 30% fat by dry weight, and a similar or even higher concentration of protein (up to 36%). Baobab seeds contain substantially more protein than agricultural plants like sorghum (11.4%), millet (11.9%), and manioc (0.9%). According to Murray et al. (2001: 9), baobab seeds yield about the same amount of energy per 100 g dry weight as honey.

Baobab fruits ripen during the late dry season and/or early wet season, but remain edible for several months after they form (Marlowe, 2006: 363). According to Murray et al. (2001: 12), Hadza "women consistently returned with dozens of baobab fruits or with significant quantities of seeds over the majority of months of the year either through direct fruit collection or through collection of seeds in baboon dung piles".

How does Hadza hunting covary with baobab availability? The Hadza engage in two principal types of hunting—intercept hunting at night from blinds during the late dry season when animals are concentrated close to major waterholes, and daytime encounter hunting which occurs throughout the year whenever hunters are out of camp. Curiously, despite their much more powerful bows, the Hadza, like the San, rely on slow-acting poison and frequently have to track their prey for several hours or longer after they have been wounded (Hawkes et al., 1991, 2001; O'Connell et al., 1992). In the studies conducted by Kristen Hawkes et al. in the mid- to late 1980s, the Hadza made 52 kills just in the last 3 months of the dry season—August, September, and October—compared to a total of only 19 kills during the remaining seasons (O'Connell et al., 1992: 320–321). The late dry season is precisely when the baobabs come into fruit. It is therefore tempting to conclude that Hadza big-game hunting, like Bushman big-game hunting, was only possible because it was underwritten by the availability of other productive, dependable, and cost-effective food sources. With baobabs, mongongos, tsin beans, marula nuts, and mopane worms as staples, the Hadza and Ju/'hoansi certainly do not need to hunt big game for either protein or fat. In the Hadza case, the late dry season also happens to be the worst possible time of year to pursue African ungulates for fat.

8. Why did Paleoindians hunt big game?

> *Hawkes and colleagues argue that big-game hunting is often a form of costly signaling, a means by which men establish and maintain social position relative to their peers and competitors, not just among the Hadza but among foragers in general…. To the degree the hunter is successful, two ends are achieved. First, because big-game hunting is a risky, skill-intensive undertaking, the good hunter marks himself as a powerful ally and dangerous adversary. His relationships with others are likely to be structured accordingly. Equally important, his successes make available a "public good," one that is of interest to all, unpredictably acquired, readily divisible, and thus likely to be shared widely…, considerations that draw still more favorable attention his way. That attention might include deference to his wishes, support in disputes, positive dealings with his spouse and children, and more frequent mating opportunities….*
> Bird and O'Connell (2006: 164–165)

Thus, for our two quintessential hunters, the Bushmen and the Hadza, big-game hunting seems not to be a reliable form of food provisioning, but instead appears to be an activity performed by males largely for reasons other than nutrition, and which is made possible because it is underwritten by the abundance of other foods that become available at more or less the same time of year (e.g., Bliege Bird and Bird, 2008; Speth, 2010). Could a similar argument be made for Paleoindian big-game hunting? Is it possible the mammoths, jumbo-sized bison, and other large prey killed by Clovis, Folsom, and Plano hunters were likewise undertaken largely for social and political reasons rather than food provisioning? Obviously, the colder and more seasonal environments of late glacial and early Holocene North America were radically different from the African environments occupied by the San and Hadza today, and thus it seems eminently reasonable to assume that big-game hunting would have played a bigger nutritional role in Paleoindian lifeways than it seems to be playing in the lives of sub-Saharan hunters and gatherers. Nonetheless, these African cases are a red flag, a warning that perhaps what seems like an obvious conclusion—"of course Paleoindians hunted big animals for food"—may in fact blind us to a pattern of behavior that was, at least to some as yet undetermined extent, motivated by a very different set of factors, factors that were firmly rooted in the social and political domain.

While we cannot demonstrate this to be the case, there is some tantalizing evidence that has been available since the late 1980s concerning Paleoindian bison hunting that could be pointing in precisely this direction. Todd (1987, 1991) already many years ago (see also Hofman et al., 1991: 184; Kornfeld, 2007: 42) noted that Northern Plains bison kills were strikingly different from their late prehistoric counterparts in the same region. The Paleoindian sites often seemed to lack the intensive processing features—boiling pits, fire-cracked rock, bones broken open for marrow and comminuted for grease-rendering—that seem to be so common in late prehistoric sites in the same region, and which figure so prominently in the voluminous ethnohistoric literature. Fire-cracked rock and associated boiling pits seem to be very rare commodities in Early Paleoindian sites continent-wide, not beginning to become common until late in the Paleoindian period and especially in the beginning stages of the Archaic (e.g., Collins, 2002; Hranicky, 2002: 67; Thoms, 2008: 130).

> Evidence of butchering and processing from late Pleistocene/early Holocene bison bonebeds is often limited, as cutmarks and other unambiguous indications of human modification are rare. The conventional equation of the degree of disarticulation with the amount of butchery, or the assertion that the total amount of bone breakage is directly related [to] the magnitude of processing, can no longer be considered reliable indicators of intensity of human use of products from a kill…. Several alternative lines of evidence suggest that the degree of processing at Paleoindian kills may have been less than at later period kills, however. These include (1) skeletal element frequencies indicating either limited bone removal or non-segmental removal of entire limb units…, (2) a relatively low percentage of definitely cutmarked and humanly fractured bones, and (3) paucity of processing features or those associated with long-term use of stored or cached meat products. (Todd, 1991: 224)

> The evidence from Stewart's Cattle Guard site indicates that the bones were discarded after the meat and marrow were removed, and no additional processing for bone grease occurred. There is no suggestion that the articular ends of the long bones were pulverized or even partially reduced by humans. (Jodry and Stanford, 1992: 153–154)

> There are a number of radiocarbon dates now available on fluted point Paleoindian occupations in the New England-Maritimes region. These dates may help us better understand the regional fluted point Paleoindian chronology, although they cannot be used uncritically (Levine 1990). Moreover, there are a large number of "unacceptable" radiocarbon dates, beginning with early attempts to date the Bull Brook site (Byers 1959). *Part of the problem is the ephemeral nature of Paleoindian features, never lined with or associated with fire-cracked rock*. (Spiess et al., 1998: 236, emphasis added)

Interestingly, the results of David Meltzer's recent excavations at the Folsom type-site in northeastern New Mexico, a bison kill on the margins of the Southern Plains, may be pointing in the same direction. While acknowledging that originally there may have been a processing area associated with the kill that he either was unable to find or that had been eroded away, he nonetheless comes to the conclusion that the behavior in evidence at Folsom is entirely compatible with Todd's suggestions about Paleoindian bison hunters in the Northern Plains:

> In the fall of the year and after a long summer of feeding, bison cows are at their peak of fat stores…. This is, of course, the time of the year when the kill took place at Folsom, and this was dominantly a cow-calf herd, thus presenting a prime opportunity for hunters to target fat reserves, which were a critical resource. Yet, the Folsom hunters did not take full advantage of this opportunity. To be sure, they removed tongues and back hump ribs, but there is no evidence of bone impact fractures created in search of marrow, or of the highly destructive processing of bone for grease. (Meltzer, 2006: 302)

Bamforth (2007) echoes the same general view concerning the apparent less-than-complete utilization of bison by Paleoindian hunters on the Great Plains: "…there is no evidence that Paleoindian hunters processed the bison in these kills for long-term storage (for example, by rendering bone grease or producing pemmican as later groups did)…" (Bamforth, 2007: 247). In the process, he makes an interesting observation concerning pemmican, the staple which, during the historic period, underwrote both Native American and Euro-American mobility on the

Plains and in the subarctic and arctic environments to the north. Pemmican is basically a mixture of rendered fat and pulverized dried lean meat which, in terms of calories, consists of approximately 75% fat and 25% protein (Stefansson, 1956; Speth, 2010). Pemmican is lightweight, easily transported, if kept dry can last for many months, and while it lasts can fully provide a hunter's daily food needs. Without it, the incredibly high mobility commonly postulated for Paleoindian hunters in the Plains would likely *not* have been possible. This apparent contradiction between the bison data and the postulated model of Paleoindian mobility clearly needs to be looked at more closely.

The picture with mammoths is less clear. Given the massive size of these ancient proboscideans (Shipman, 1992), cutmarks and other unambiguous signs of processing intensity are likely to be less in evidence than in bison. Nonetheless, Haynes (1991) discusses the Clovis evidence in some detail and concludes that, like somewhat later bison, the mammoths that display reasonably convincing evidence of human involvement do not appear to have been utilized very heavily.

> Clovis people seem to have left behind whole or nearly whole mammoth carcasses, some of which were unsectioned, an odd pattern of carcass utilization when seen in the light of recent ethnographic descriptions documenting the habitually heavy use of elephant carcasses. (Haynes, 1991: 304)

> In Clovis sites, the degrees of utilization are difficult to decipher clearly, because bone assemblages in some sites will have undergone postdepositional disturbances, in the form of erosion, redeposition, or weathering destruction. In instances where postdepositional disturbances have been minor, and where Clovis mammoths have been examined conscientiously, Clovis mammoths have been found strikingly intact, with few or no clear instances of artifactually damaged bones or widely dispersed body parts. The carcasses must have been lightly utilized, or perhaps they were unutilized, as suggested by Hemmings and Haynes (1969) in the case of the Escapule mammoth. (Haynes, 1991: 306–307)

Interestingly, the apparent underuse of mammoths in the New World seems to mirror the picture that is emerging in Europe, where it seems that these gigantic animals were more important for the raw materials they provided than for food:

> …based on the evidence compiled above, preliminary suggestions can be made about the role of Proboscideans in every day Palaeolithic life. Archaeozoological evidence from the entire Pleistocene period shows that we have little evidence of the regular use of Proboscideans as a source of meat. (Gaudzinski et al., 2005: 191)

> In contrast, Proboscidean remains seem to have been more highly valued as a source of raw materials to satisfy human needs in different aspects of every day life during the entire Pleistocene period. Mammoth remains were still important raw materials in certain regions long after this species became extinct. (Gaudzinski et al., 2005: 191)

Again, however, an interesting question is whether Paleoindian hunters (or scavengers) made a concerted effort to extract the lipids from the carcasses of these behemoths (Speth, 2010). If not, we are left with the same quandary that we just alluded to concerning Paleoindian use of bison. So, we not only need to learn what role, if any, Clovis hunters played in the demise of mammoths and other megafauna, a topic which has attracted the lion's share of the attention thus far, we need to learn a lot more about what they actually did with them (see Borrero, 2009: 160 for an interesting discussion of this issue in the South American context).

So, where does this bring us? We end with a question: Why would Paleoindian bison hunters, and possibly mammoth hunters as well, engage in an activity that is almost certainly highly unpredictable as a means of providing a family's or a band's day-to-day food needs, travel hundreds of kilometers to do it, and then not thoroughly use the most valuable part of the carcass—the fat in the marrow and in the cancellous tissue of the bones (Speth, 2010)? The fat would have been important to people overwintering on the Plains; and, as the principal component of pemmican, the fat would have been absolutely essential to hunters as they undertook their long treks across the grasslands. This simply does not make sense. Either we are misinterpreting the faunal data, or there is something very wrong with our current view of Paleoindians as peripatetic big-game hunters.

9. Conclusions

> *Doubt is not a pleasant condition, but certainty is absurd.*
> Voltaire (Arouet) (1919: 232, in a letter dated 1770 to Frederick William, Prince of Prussia)

We would like to close this rather speculative essay with a few additional speculations. We will not glorify these by calling them hypotheses, because at this stage they are far too tenuous to warrant such an appellation. Moreover, continuing as we have throughout this paper, we deliberately overstate some of our concluding remarks in order to underscore the need for further thinking along these lines.

Perhaps what we consider the most interesting conclusion, but probably also the most controversial, concerns the role of big-game hunting. Like a growing number of others, we doubt that Paleoindians were specialized big-game hunters, at least in the sense that their subsistence was heavily dependent on it. Paleoindians took a lot of big game, but possibly not annually and probably not primarily as a means of providing their day-to-day sustenance. Instead, we suspect that the hunting of proboscideans by Clovis peoples, and the somewhat later mass communal drives of bison, were activities whose primary purpose revolved around the social and political affairs and aspirations of men (see also Borrero, 2009: 160); and, rather than being the center-piece of Paleoindian food provisioning, their indulgence in big-game hunting was heavily underwritten and perhaps even made possible by the food-getting activities of others, quite likely the women. Speculative? Yes. Very much so, in particular because we know so little about the plant foods that undoubtedly contributed to their diet, perhaps in a large way (see, for example, Walker and Driskell, 2007, for recent efforts to begin filling in this critical gap in our knowledge). But given what we are discovering about the role of big-game hunting in the lives of quintessential big-game hunters of the ethnographic realm—the San and the Hadza—we think it is time to step back and reexamine the place of big-game hunting in Paleoindian lifeways. If the speculation we present here can be brought under the lens and shown to be false, great; we will be among the first to wave it goodbye—but we cannot continue business as usual and simply ignore it.

Paleoindian groups were certainly residentially mobile, as are most hunters and gatherers, but we doubt that the distances over which exotic high-quality cherts were moving have much if anything to do with the space over which a residential group foraged on an annual basis (see also Ellis, n.d.). We suspect that the movement of chert may be telling us something about exchange, some of it very likely directed exchange, in which different groups targeted specific cryptocrystalline materials for their particular qualities, very likely aesthetic and symbolic ones. It may also be telling us something about direct procurement, probably by males traveling alone or in small groups to acquire exotic or unusual flints, again for special purposes rather than daily subsistence. As well, it could be telling us about the residential movements of some specific subset of the population, perhaps linked to fluidity of band composition similar to that documented among ethnographic hunters and gatherers.

When Paleoindians undertook long, arduous treks to acquire exotic flints that were largely earmarked for making projectile points, it is likely that many of these trips were *not* embedded in activities focused primarily on subsistence, but instead were folded into trips of a largely social or symbolic nature, such as visiting kinsmen or trading partners, or sacred points on the landscape, or as part of a vision quest, or in the context of some other ritually or symbolically laden purpose. This position was argued many years ago by Gould et al. (1971: 161–162), Gould (1978, 1980), Gould and Saggers (1985) (see Davidson, 1988 for an alternative perspective), but effectively smothered by the overzealous excitement, and often excessively reductionist arguments, of the "New Archaeology" (e.g., Binford, 1979; Binford and Stone, 1985).

Whallon (2006), in a recent paper entitled "Social Networks and Information: Non-'utilitarian' Mobility Among Hunter-gatherers", looks anew at hunter–gatherer movement in contexts other than subsistence, and emphasizes the fundamental importance of long-distance trips undertaken by foragers for purposes of information gathering, as well as for creating and maintaining viable social and demographic networks (see also Anderson, 1995: 12–13; MacDonald and Hewlett, 1999; Meltzer, 2004a: 126–132). Perhaps it is time that Paleoindian archaeologists take another look at the "embeddedness" assumption that underlies so many studies about these first inhabitants of North America. The idea that exotic flint procurement was habitually embedded in the food-getting activities of these ancient foragers is something that needs to be demonstrated, not assumed.

The argument is commonly made that Paleoindians could not have relied on exchange as a way of obtaining exotic flint, because their population densities were too low to maintain active exchange networks, and because exchange was too undependable as a means for assuring adequate reserves of such a critical life-sustaining resource. Both of these arguments are based on questionable assumptions, and run counter to evidence from hunter–gatherers in Australia and elsewhere. First, in Australia, exchange of finished artifacts, such as Kimberley and Pirri points, as well as a variety of lithic and other raw materials, was widespread (e.g., Jones and White, 1988: 54), even in the most arid central and western desert areas, and it is hard to envision population densities that were much lower than these classic Australian cases that would still have supported viable mating networks (Wobst, 1974). However, as Yengoyan (1968) argued years ago, it is precisely because of these low densities that patterns of interaction, intermarriage, and exchange were so prominent and far-reaching in Aboriginal Australia (e.g., McCarthy, 1939a,b,c; see also Sackett, 1976; Gould and Saggers, 1985; Myers, 1986: 78; Peterson, 2004: 224; Whallon, 2006; Davidson, 2010: 390). A similar argument can be made for *hxaro* exchange systems among the Kalahari San (Wiessner, 1982), as well as those documented ethnographically and archaeologically among foraging groups in California and the Great Basin (e.g., Steward, 1938; Sappington, 1984; Couture et al., 1986: 151–153; Connolly, 1999: 7; Dillian, 2002).

Secondly, if Paleoindian projectile points were critical, not for their food-getting properties, but for their symbolic role in male-centered social and political endeavors, how much exotic flint would actually have been needed (not in percentages, but in kg/person/year)? The amounts may have been quite small:

> It is something of a paradox that our major window on Early Paleoindian social systems in eastern North America is lithic analysis. Stone tools, while they were essential to these early foragers, were quantitatively a minor component in technological systems. The average family may have needed as little as 10 kg of flint to make virtually all of the stone tools for a given year…. (Seeman, 1994: 284; see Spiess and Wilson, 1989: 90)

Gould (1978: 822), looking at total annual chert use and discard among Australian Aborigines of the Western Desert, came up with a somewhat larger figure—18.99 kg (41.87 lb), or, for our purposes here, roughly 20 kg (44 lb)/person/year.

Finally, Luedtke (1979: 260–261), in what to our knowledge is the broadest review to date of ethnographic and archaeological information regarding flint use, arrived at a larger and more conservative estimate for the amount of flint that would be needed by a family over the course of a year—between ~20 and 40 or 50 kg—but the quantity nevertheless is still relatively small and, needless-to-say, only a fraction of this would have been needed on an annual basis for the manufacture of projectile points.

Incidentally, none of these arguments necessarily invalidates the many studies of Paleoindian lithic economizing, such as rates of retouching and resharpening with distance from source and duration of occupation, deliberate stockpiling or caching of finished or unfinished points and related paraphernalia, fall-off in quantities of material, size of debitage, and amount of cortex as one moves away from a source, and so forth (see, for example, the discussions and references in Hofman, 1991, 1992; Ataman et al., 1992; Elston, 1992; Wilson, 2007; and Surovell, 2009). What differs are the contexts and underlying motivations for the economizing.

Fluted points and their Plano successors, in our opinion, are too pretty, too hard to make, and too fragile to base the outcome of one's day-to-day subsistence activities on them. Judging by the Schoeningen and Lehringen spears, one very likely does not need any stone tip to successfully kill or immobilize these animals, even the biggest ones. Points so decidedly aesthetic must be there as a reflection of the overall symbolic importance of these hunts (or perhaps of the individuals who are doing the hunting)—that is, their importance lay more in the context of something akin to costly signaling than food-getting. They were likely designed, not for their efficacy as killing devices, but for display and perhaps because they were laden with religious or other symbolic significance.

Here we take a large step toward the proverbial "deep end"—we think archaeologists who specialize in the study of prehistoric hunters and gatherers, and who find themselves with little else to work with but flint, often take it as intuitively obvious that lithics must have been as important to foragers in their day-to-day dealings with life as it is nowadays to archaeologists in their day-to-day activities in the lab. As White (1977: 13) succinctly put it many years ago: "if we cannot usefully employ the stone tools, we cut out a very large part of our direct data from the past, data which provide many of the foundations of our more theoretically oriented upper stories". Looking at the enterprise of prehistory from an East Asian perspective, White goes on to challenge the pervasive and enduring Eurocentric notion that links increasing diversity and complexity of stone tool forms with "progress" toward greater efficiency in harnessing energy, suggesting instead—rather provocatively—that "…the majority of stone tool forms were not necessary, in a utilitarian sense, at all" (White, 1977: 26; see also Pardoe, 1995: 710; Morrison and Junker, 2002: 218; O'Connell and Allen, 2007 provide an interesting discussion of this same issue in the context of modern human entry into Australia). Many years later, in 2003, Paul Sillitoe and Karen Hardy raised a very similar caution about our profession's preoccupation with flint, a concern echoed the next year by Alan Bryan:

> Worked stone is of paramount importance in much prehistoric archaeology as it is frequently the only cultural evidence to survive. For the same reason it often dominates interpretation, with lithics afforded a status that is unlikely to reflect their true

place within the material culture of which they formed a part…. (Sillitoe and Hardy, 2003: 555)

> Unfortunately, the material culture of most southern North American native cultures was only sketchily recorded…before they were significantly altered by European contact. However, material culture studies of northern interior natives give a picture that bone, hide, sinew, wood, and other perishables were more important than stone, although scrapers and sharp flakes were important for working other materials…. Fortunately, the material cultures of several lowland South American groups have been recorded…. These studies show that lithics were much less important than bone, wood, fiber, cordage, basketry, featherwork, etc. In some cases the only lithics used were tiny quartz flakes fixed onto wooden boards to make manioc graters. (Bryan, 2004: 218–219)

Ever since Paleolithic and Paleoindian archaeology first emerged as *bona fide* topics of research, we have worked with the underlying assumption that flint sources were so important to forager lifeways that their settlement systems were heavily influenced, at times even determined, by where high-quality flints could be found on the landscape. A fair number of recent papers dealing with the peopling of the New World have gone so far as to state that one of the first things these pioneering "First Americans" would have done when they entered the unfamiliar landscapes of newly deglaciated North America was to locate the sources of high-quality flint, and thenceforth their settlement systems would have been effectively "tethered" by these critical focal points (e.g., Gardner, 1983; for a critique of this view, see Curran and Grimes, 1989: 72; see also Anderson, 1995: 12–13; Fiedel, 2000: 83; Lepper, 2002: 85; and Bamforth, 2006: 512).

Indulging now in more than a little tongue-in-cheek, we suspect that the location of "high-quality" flint sources had about as much impact on Paleoindian settlement patterns and overall annual mobility decisions as the moon does on the size and location of the earth's oceans. While its gravitational tug causes the oceans' tides to swell and recede ever so slightly, it has no significant bearing on where on our planet the ocean basins are located, how big they are, or how much water they contain. Flint is useful, to be sure; and, at times, good flint may be more useful than poor flint. But when it comes to basic survival, we suspect that food, water, shelter, fuel, mates, flow of information, symbolic considerations, and the overall social and political climate in a region would have weighed far more heavily on the minds and actions of Paleoindian foragers than the location of high-quality flint. The fact that people may have camped at a quarry while they were working the outcrops, as discussed for example by Bamforth (2006: 522), is not surprising if the flint-gathering party had come a long distance to utilize the resource, and especially if procuring and testing the flint entailed the performance of ceremonies, feasting, or other observances involving the visitors and perhaps their local hosts, as in many Australian cases, but this does not necessarily mean that the entire residential camp moved each year to the vicinity of the quarry as part of the band's normal round. The quarry might easily have been dozens to hundreds of kilometers outside of their annual range.

It is often argued that Paleoindians had to have regular and guaranteed access to especially high-quality flint so that they could fashion their delicate, sometimes fluted, and often extremely thin projectile points—ordinary flints were too coarse-grained or riddled with flaws and impurities to be up to the task. But if the function of these points was first and foremost for their symbolic qualities and display value, and not for their effectiveness in the daily grind of food-getting, then many of the assumptions we make about how much flint Paleoindian hunters would have needed per capita per year, or the mechanisms by which they would have assured access to these materials, can be seen in a very different light. Put another way, move projectile points into the sociopolitical and/or ritual domain, and their role in Paleoindian lifeways becomes more like that of marine shells, turquoise, and other symbolically-charged materials. The procurement of the raw materials from which such items were made would certainly have been important to members of a Paleoindian band, but there is no *a priori* reason why their procurement would of necessity have been embedded in the group's annual foraging round. As Ellis (2009: 348) puts it:

> I remain unconvinced that we can understand, or even fully appreciate, Palaeo-Indian stone tool production and use solely or simply in terms of environmentally deterministic, utilitarian and least-effort models. The contexts and manners in which we find stone artifacts used, such as at Crowfield Feature #1 and Caradoc, do nothing but reinforce and strengthen that viewpoint. Indeed, I think they demand a wider interpretive scope to our thinking.

If we cannot be sure that the distinctive lithic signature of the Early Paleoindian period—fluted points made of high-quality raw materials transported long distances—is directly related *only* to getting food, then how do we interpret the fluted point "horizon"? What mechanisms made this distinctive artifact class a continental-scale phenomenon? If fluted points were, in fact, objects of some symbolic importance, then fluting may not be the product of evolving technological sophistication in the design of efficacious big-game-hunting armaments, but a socially, politically, or religiously significant innovation that spread quickly among dispersed foraging populations precisely because of its widely shared symbolic meanings (e.g., Smith, 1992). As such, why should we expect fluting to have an extended developmental sequence which we can use to trace its origins back to Beringia (or to Europe for that matter)? Fluting as a symbolic attribute, together with a preference for aesthetically beautiful "exotic" flints, epitomized for example by the presence of extraordinarily large points in Clovis caches such as Richey-Roberts, Simon, and Fenn, as well as points fashioned out of oversized quartz crystals (e.g., Reher and Frison, 1991; Lyman et al., 1998: 897; Kohntopp, 2001; Amick, 2004), could easily have originated just about anywhere on the continent and spread from group to group across North America at a rate that might be virtually undetectable with current dating methods.

Using the archaeological record to discriminate between rapid population movements and the rapid emergence or spread of a new technology or idea is, of course, no simple task. If fluted points are an item of social, political, or other symbolic significance, their widespread appearance across the North American landscape may ultimately tell us more about social interactions than human migrations. Perhaps we should analyze Clovis through the same set of lenses as the projectile point "horizons" that occur later in the prehistory of North America, such as the "Kirk Horizon" of the eastern Early Archaic (Tuck, 1974), the "Jack's Reef Horizon" associated with the spread of bow and arrow technology from the Northeast (Seeman, 1992), and the late prehistoric explosion in popularity of small, unnotched triangular arrowheads, such as the Fresno found throughout much of the Southwest and Southern Plains, the Cottonwood series in the Great Basin and California, and the very similar Levanna and Madison points that become ubiquitous in the Midwest and Northeast (Blitz, 1988; Ellis et al., 1991, 1998: 154; Koerper et al., 1996; Shott, 2003: 258; Adler and Speth, 2004; Carter and Dunbar, 2006). These changes in projectile point form may or may not be linked to corresponding changes in weapon design (e.g., Ellis, 2004: 75), and may or may not be linked to population movements at some scale. In none of these cases, however, is the rapid appearance and spread of a new technology assumed to be solely associated with both a technological change and a wholesale population movement.

Of course, if one accepts the view that North America, not just the Pacific coast, was already peopled prior to Clovis, the entire issue of migration on the scale commonly envisioned for Clovis evaporates (Prasciunas, 2008). Similarly, the link between fluting and migration dissolves if one sees Clovis arising first in the south or southeast and then spreading northward from there into Canada and eastern Beringia (e.g., Anderson and Gillam, 2000; Dixon, 2001; Meltzer, 2004b; Bever, 2006; Goebel et al., 2008; Dillehay, 2009; Beck and Jones, 2010; see Steele, 2009, 2010 for an interesting discussion of the conflicting and ambiguous nature of the radiocarbon evidence regarding both direction and speed of Clovis dispersal).

> …we have taken a simple aspect of a surmised culture—the fluted projectile point—as the sole basis for broad scenarios of early lifeways. We have made an explicit assertion, not always critically questioned, that the spatial distribution of a particular artifact trait—the flute—is the spatial distribution of an actual culture and society. But as archaeologists, we know that we cannot always make a direct correlation between a particular trait and a particular society, any more than we can say that the distribution of cowboy hats equals the distribution of Texans. If a particular trait does not represent a society, how can it represent a distinct culture? And if it does not necessarily represent a culture, what does it really say about early human migration in the New World? (Dillehay, 2000: 285)

As stated at the outset, this entire essay is speculative. What we hope to have accomplished, if nothing else, is to provoke (in the most positive and constructive sense of the word) archaeologists to reexamine some of the assumptions that form the bedrock of Paleoindian research. We need to step back, way back, and reconsider the theoretical (not just the ecological but the anthropological) grounding of many of the basic assumptions that underlie Paleoindian studies.

Now it is time to release the balloon to see if it plummets to the ground or floats. As an added precaution, we are being careful to keep our feet well out from under the balloon….

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