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Paleoindian Archaeology in Eastern North America

Current Approaches and Future Directions

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Eastern North American Paleoindian archaeology has attracted the attention of many excellent researchers down through the years, as the chapters in this volume and their associated references testify. A great deal is known about the archaeology of the period, but as the editor points out in his introduction, there is a pressing need for more well-grounded synthetic statements, to provide a baseline about what we know and where we need to go, as well as for more data-rich technical reports on the sites and assemblages found within the region. This volume provides both and as such will prove an indispensable reference for scholars working in the region. As Joe Gingerich points out in his introduction, there are only a very few such volumes from the east, most notably those encompassing lithic resource use and the Southeast (Anderson and Sassaman 1996; Ellis and Lothrop 1989). Indeed, the only synthesis of Paleoindian research encompassing the entire region at present, other than the current volume, is the all-too-infrequently acknowledged *Earliest Americans (Paleoindian) Theme Study for the Eastern United States* (Martin-Siebert 2004), which includes regional overviews encompassing the Northeast by Dena Dincauze, the Midwest by Michael Shott, and the Southeast by me. More focused studies synthesize data from particular sites and states in the east, but the number of volumes taking a larger perspective remains far less than the archaeological record from the region warrants. Hopefully more will be produced in the not too distant future.

Variability in Eastern Paleoindian Assemblages

Miller and Gingerich's chapter demonstrates what can be learned by looking at large samples of artifacts and radiocarbon dates. Appreciable morphological variability has long been recognized within the fluted points found in eastern North America, as evidenced by the many types reported in the literature. The age and spatial occurrence of many of these forms remain somewhat obscure, however, although as more primary site and artifactual data are collected and analyzed, that is changing (e.g., Anderson et al. 2010; Bradley et al. 2008; Ellis 2004; Haynes, this volume). Miller and Gingerich's examination of fluted points from the Carolinas, for example, indicates that depth of basal concavity appears to increase following Clovis, at least in some areas. This attribute may prove a useful dating tool, and the study serves as an example of how morphological variability within fluted points found and reported in eastern North America can be examined. Morrow and Morrow (1999), examining a geographically far more extensive sample, have previously argued for a latitudinal trend from south to north in basal concavity depth in North America, with the points with the greatest basal depth occurring in the Northeast. Work by Goodyear

(2006, 2010) and Miller and Gingerich herein have shown that points with deep basal concavities also occur in the South Atlantic region, and recent work with information in the Paleoindian Database of the Americas (PIDBA) has shown that deeply indented forms, most typically classified as either Clovis or Redstone in local surveys, occur widely throughout the eastern and central part of the Southeast (Anderson et al. 2010). I suggest that the morphological trend in basal concavity depth observed by Morrow and Morrow is shaped, in part, by the age and incidence of these forms. That is, there are likely proportionally far greater numbers of points with deeply indented bases in the northern part of the region than to the south, where many more weakly indented, presumed Clovis forms are present in survey collections. Examination of the variability within eastern fluted points has much left to tell us, and they should be explored with the rich data sets that have been compiled by researchers in many states and provinces in recent decades. Any such analyses should probably proceed independently from existing typological biases and preconceptions, at least as much as possible. That is, rather than using named types as the beginning point in research, or seeking to understand the variability in points already classified, we should be looking to see if there is, as I strongly suspect, currently unrecognized morphological variability in the vast numbers of fluted points (>10,000) currently reported from eastern North America. To do this, as discussed in more detail below, researchers will have to make their own data available and their studies replicable.

The dating of archaeological assemblages must be far better controlled than it is at present if we are to improve our understanding of the Paleoindian occupation of the east and particularly the morphological variability in fluted point assemblages. Miller and Gingerich demonstrate that there is a north-to-south trend in the incidence of Paleoindian radiocarbon dates, with more dates from the early and middle centuries of the Younger Dryas in the Northeast than in areas to the south. Given that human occupation across much of the Upper Northeast and Midwest apparently postdates Clovis, this trend is not particularly surprising. What is interesting, however, is the comparatively low number of post-Clovis dates in the Southeast and Mid-Atlantic/Midwest, at least until the latter part of the Younger Dryas, in Dalton times. Miller and Gingerich's study also highlights the fact that there are comparatively few Clovis-age dates from the Southeast and Mid-Atlantic/Midwest. Given the large number of fluted points found in these regions, and the considerable morphological variability among them, they are correct that more stratified sites with datable deposits need to be found, excavated, and reported (see also Miller et al. 2012).

Miller and Gingerich's chapter notes that changes in the archaeological record occurring with the onset of the Younger Dryas are assumed by some researchers to be the result of an extraterrestrial impact. The Younger Dryas climate episode, whatever may have caused it—and most researchers, including me, believe that it was likely the result of a massive outflow of glacial meltwater into the Gulf of Mexico, the North Atlantic, or the Arctic—had major effects on eastern biota, as the paleoenvironmental discussions by McWeeney and Halligan herein document. Likewise, major changes in the eastern archaeological record occur at this time, something long recognized, since it is also when the Clovis horizon comes to an end (e.g., Anderson 1990, 2001). Settlement reorganization and more recently demographic collapse have been inferred (e.g., Anderson et al. 2010; Anderson et al. 2011; Goodyear 2006, 2010), the former a likely consequence of environmental conditions in the early centuries of the Younger Dryas, when an essentially modern fauna appeared, replacing the earlier Pleistocene megafauna. It is also at this time that the Clovis horizon was succeeded by a number of different and more spatially restricted point forms, with evidence for a restriction in the distances groups moved, as indicated by the occurrence of raw materials away from known sources (Anderson et al. 2010). The apparent changes in settlement that characterize the early centuries of the Younger Dryas were most likely tied to the changes in sea level, floral and faunal communities, and climate that were occurring, whatever brought these about, and need not be attributed solely to an extraterrestrial impact, even if one occurred.

Whether a demographic collapse also occurred in the east at the onset of the Younger Dryas is much more debatable (cf. Anderson et al. 2011;

Goodyear 2006; Meltzer 2009; Meltzer and Holliday 2010), as Miller and Gingerich also argue herein. They demonstrate convincingly that comparatively few radiocarbon dates from the first half of the Younger Dryas have been reported in the Southeast and Mid-Atlantic and far more in the Northeast. If a comet or other extraterrestrial object hit the planet in the northern hemisphere and started the Younger Dryas, a hypothesis currently the subject of much debate, the pattern among the dates, Miller and Gingerich argue, is exactly the opposite of what one would expect. I agree but suggest that the observed patterning has little to do with the effects of the Younger Dryas onset on human populations in the east. Instead, what their analysis actually indicates is where more or less research and dating on sites of this period has been conducted and where it is needed. Given that the upper parts of the Northeast and Midwest are currently presumed to have been uninhabitable or only minimally visited or inhabited until the Younger Dryas, furthermore, a greater number of dates from that period compared with what came before is a logical expectation in these regions, if what came before was nothing.

What is really needed from eastern North America, and particularly from the Southeast, as Miller and Gingerich and several other authors argue herein, is more fieldwork directed to finding, excavating, analyzing, and dating sites and even more importantly, given how uncommon it seems to be (see Fiedel's commentary, this volume), the production of final reports on this work. Stratified sites are critical to establishing the age, behavioral meaning, and relationships among the varied Paleoindian assemblages found in the east. Too many fluted points are simply typed as Clovis, or Gainey/Clovis, or any of a host of other type names or even given a generic "fluted point" designation, when we should probably be actively using the attribute and, where available, radiometric data we have on hand to sort them out. Exemplary work along these lines has been occurring in the Northeast and Upper Midwest for many years, making the Paleoindian sequence in these regions the most solidly grounded in the east (Bradley et al. 2008; Ellis 2004; Ellis and Deller 1997). Examinations of technological and morphological variability, and temporal and spatial occurrence, are needed for many eastern point forms in the Lower Midwest, Southeast, and Mid-Atlantic regions.

Major discoveries appear to be waiting in the collections we already have. To give one example, unfluted lanceolates resembling later Plains Paleoindian plano forms occur widely in the east, such as the Varney type in the Northeast (Bradley et al. 2008; Gingerich, Chapter 6, this volume), or Scottsbluff and Angostura-like forms in the trans-Mississippi south (Anderson and Smith 2003; Johnson 1989). Movement or interaction between peoples in what are now the Plains and Eastern Woodlands as far back as the Paleoindian era has been long inferred (Johnson 1989; Munson 1990) and is most convincingly illustrated by the occurrence of Folsom points in the Midwest, in Illinois and Indiana in particular (Munson 1990). Unfluted lanceolates are frequently assumed to be preforms or knives, however, and may receive less research attention than more recognizable and easily typed fluted, notched, or stemmed forms. Just as there was apparently a full fluted horizon immediately following Clovis that spanned much of the continent (e.g., Anderson et al. 2010; Goodyear 2006, 2010), there may have been a partially coeval or succeeding unfluted lanceolate horizon whose age, spatial extent, and behavioral implications we are only just beginning to grasp (see also Bradley et al. 2008; Justice 1987).

Paleoenvironmental Reconstructions

McWeeney's chapter herein, as well as the chapters on work at Flint Run and Shawnee-Minisink (Carr et al., Chapter 8, this volume; Gingerich, Chapter 9, this volume) shows how paleoenvironmental data collected from on or near archaeological sites can be an important source of information about how those sites were used, why their locations were chosen, and what foods played a role in human subsistence (see also Carbone 1974, 1976). Importantly, she demonstrates that late Pleistocene vegetation across the region, particularly in the Mid-Atlantic and Northeast, was characterized by diversified and changing communities. Biota were distributed more in a patchy mosaic of species than in uniform and areally extensive communities. Vegetation in specific localities was determined not only by elevation and latitude, and the greater expanse of the low-lying coastal plain, but also by local topography and drainage. Similar observations apply to the Southeast, as Halligan's chapter herein documents. The seemingly favorable conditions for human settlement during the Allerød or Clovis times inferred by McWeeney for the lower and in some cases more northerly portions of the Northeast, however, must be reconciled with the low incidence of occupations dated to this interval, as reported by Miller and Gingerich herein, and an apparent absence of Clovis points. A thin veneer of Clovis, perhaps reflecting brief visits or limited occupations, however, may exist well into the Northeast, as indicated by the occurrence of Clovis-like points at sites such as Lamb and Hiscock in New York (Gramly 1999; Laub 2003). Fluted point collections from the region should be carefully examined for evidence of classic Clovis forms; some points in existing surveys have weakly indented bases and probably are Clovis in age (e.g., Loring 1980; Ritchie 1957). As the recognition and dating of morphological variants and sites themselves become more refined and precise, we should be able to better document the spread of people into recently deglaciated terrain over time in the late Pleistocene east.

The role of plants in subsistence in eastern North America remains essentially unknown for the Clovis era and is only marginally better documented for post-Clovis times, at least until near the end of the Younger Dryas, when well-preserved paleobotanical remains become more common in some assemblages, particularly at Dalton culture cave and rockshelter settings (e.g., Walthall 1998), such as at Dust Cave in northern Alabama (Hollenbach 2007, 2009). Shawnee-Minisink is sometimes cited as evidence that Clovis populations in the east were generalists, but as Gingerich (2011 and Chapter 9, this volume; contra Dent 2007) demonstrates, the evidence for Clovis subsistence from the site, save for the use of hawthorn fruit, is minimal and equivocal. Equating Clovis populations in the region as either generalists or specialists, at least when evidence from this site is considered, needs to be rethought (see also Haynes, this volume). We must have information from many more sites than we have at present before models of Paleoindian subsistence in the east can be considered more than hypotheses. Clovis populations undoubtedly did eat fruits, as the hawthorn seeds from Shawnee-Minisink indicate, and perhaps used a wide range of other plant species as well, but there is no evidence for the large-scale processing or consumption of these foods during Clovis times anywhere in the region (see Gingerich 2011, and Chapter 9, this volume). Another aspect of plant use, in the manufacture of textiles, receives insufficient consideration from Paleoindian archaeologists (Adovasio et al. 2004). A woven fragment was found with Clovis diagnostics at the Hiscock site in western New York, indicating that eastern Paleoindian peoples were using textiles (Adovasio et al. 2003; Laub 2003).

While our data are currently lacking, Clovis subsistence was probably more diversified and generalized than might be inferred from traditional views of these peoples as specialized big-game hunters, particularly in lower latitudes, as Meltzer (1988, 2009) and others have long argued (e.g., Walker and Driskell 2007). McWeeney, herein, for example, offers further support for the suggestion that migratory waterfowl likely played an important role in Paleoindian subsistence, based on the presence of aquatic plants as well as plants that grow near wetlands in the vicinity of the Bull Brook site in Massachusetts. This meant that an attractive habitat for waterfowl was nearby, which may have been one reason for the site's placement. Migratory waterfowl are common in the well-preserved later Younger Dryas–age deposits at Dust Cave, and a number of scholars in recent years have argued that birds were quite important in earlier Paleoindian subsistence in the east as well (Dincauze and Jacobson 2001; Fiedel 2007; Walker 2007).

McWeeney (this volume; see also Newby et al. 2005) also argues that the earlier part of the Younger Dryas witnessed major changes in vegetation across much of the Northeast, specifically a resurgence of cold-climate vegetation and a possible southward relocation of some animal populations. Such changes undoubtedly affected Paleoindian subsistence and settlement, although to what extent remains unknown. Carr et al. herein, in a discussion of the Flint Run Complex sequence, note that technological changes in the stone tool kit used by Clovis and immediate post-Clovis groups in the Middle Atlantic were comparatively minor. Over projectile points, changes in basal concavity depth and fluting procedures occurred, although these may have been more a matter of style than reflecting a change in lifeways (see also Meltzer and Holliday 2010 for a similar argument on the Great Plains). Large caribou-hunting and -processing locales are assumed to have been present during the Younger Dryas in the Northeast, at sites such as Bull Brook, Debert, and Vail. Faunal remains support caribou procurement at some northeastern sites, although the evidence is scanty (Spiess et al. 1998). Evidence for more diversified economies appears only somewhat later in time in the northern part of the continent, during the terminal Pleistocene or ensuing Archaic period (e.g., Dincauze 2000, 2004; Kuehn 2007). The divide between northern specialized caribou hunters and more generalized southern foragers, Meltzer's (1988) classic geographic division of eastern Paleoindian adaptations, following McWeeney's argument, may have been in southern New England during the later Younger Dryas, although her claim that there is "no evidence of caribou being hunted in southern New England or the Middle Atlantic," while correct, should be considered a hypothesis in need of further testing, given how little evidence for caribou hunting exists farther to the north, where it is all but certain it occurred extensively.

At present, therefore, primary data on Clovis subsistence in eastern North America are extremely limited, as the chapters in this volume and their associated references demonstrate. Assertions that early populations neatly fit into categories like "generalized foragers" or "specialized big-game hunters" will require far more evidence than we have on hand at present (Haynes, this volume). Later Paleoindian adaptations cannot be assumed to apply to Clovis or earlier pre-Clovis occupations. The plant and particularly the animal communities available to earlier populations, with their many species of soon-to-be-extinct megamammals, were markedly different from those encountered by post-Clovis peoples. How the late Pleistocene extinctions impacted human adaptations remains unknown, but it is difficult to believe that they would have had no effect whatsoever. To give one example, if fluting was associated with hunting magic tied to the procurement of large animals, or in some other way served as a mechanism to bind widely dispersed populations together, as some authors have suggested (Meltzer 2003; Robinson et al. 2009; Speth et al. 2010), when the target animal populations disappeared, so too likely did the rationale for the use of this technology. Indeed, it was an additional millennium following the end of Clovis before fluting disappeared completely from the east, perhaps indicating its importance to these peoples. However, it is also telling that this method of point manufacture, once abandoned, was never adopted again anywhere in the Americas in the subsequent 11,000 years prior to European contact.

In spite of the preceding comments, I do believe the Clovis subsistence in the east and Paleoindian adaptations in general were likely fairly diversified. Caribou and megafauna were apparently consumed, at least at some times and in some places, but it is unlikely in the extreme that they were the sole source of subsistence, given arguments based on optimal foraging theory and the varied species large and small exploited on known Clovis sites from across the continent (e.g., Waguespack and Surovell 2003). In brief, following the diet-breadth model, if higher-ranked, typically larger animals are not available, smaller and lower-ranked animals will be exploited. The exploitation of smaller game was likely a response forced upon human populations when megafauna became extinct, assuming that these animals had been taken and eaten previously. The hawthorn seeds and possible fish bones from Shawnee-Minisink (cf. Dent 2007 and McWeeney 2007, and this volume, with Gingerich 2007a, 2007b, 2011), although controversial, indicate the kinds of plant and animal species being used, although under what circumstances and to what extent are unclear. Evidence for the exploitation of some categories of plant foodstuffs such as roots and tubers remains unknown and may be very difficult to document—it would require looking for kinds of evidence that have not been traditionally considered, such as starch grains and phytoliths in site deposits and on tool margins (e.g., Piperno and Pearsall 1998).

Evidence and arguments for generalized foraging in the Southeast during the Paleoindian period are based almost entirely on post-Clovis-age assemblages (e.g., Hollenbach 2007, 2009; Walker 2007; Walker and Driskell 2007). To date perhaps the best evidence for an early and possible Clovis-age exploitation of a diversified array of fauna comes from Florida, from four Suwannee sites yielding well-preserved modern and extinct faunal remains: Ryan-Harley, Norden, Dunnigans Old Mill, and Lewis McQuinn (Dunbar and Vojnovski 2007). Suwannee projectile points have been suggested as a possible Clovis contemporary or as even pre-Clovis in age (e.g., Stanford 1991), although Dunbar and Vojnovski suggest that the point form probably occurs somewhat later in time, coeval with other unfluted Paleoindian types such as Quad, Beaver Lake, and Dalton, which are common elsewhere in the region but rare in Florida. All four sites include extinct Pleistocene species that are thought to have died out elsewhere in the region soon after the onset of the Younger Dryas, suggesting either an early Younger Dryas age (or earlier) for the assemblages or a late survival of megafauna in north Florida. Dunbar and Vojnovski (2007:199–202) suggest, in fact, that given the high incidence of extinct fauna, two of the sites, Ryan-Harley and Norden, might well date to Clovis times.

In her chapter, Halligan briefly reviews evidence for late Pleistocene extinctions in eastern North America, observing that both large and small probable nongame animal species disappeared, rendering questionable the idea that humans were directly responsible for their extinction. Evaluating the role humans may have played in late Pleistocene extinctions in the east is difficult at present. Megafaunal kill sites that are well accepted are rare, with major exceptions being the Kimmswick site in Missouri, where mastodon were killed and butchered (Graham et al. 1981), and a *Bison antiquus* skull from the Wacissa River in Florida with a projectile point fragment embedded in an unhealed wound (Webb et al. 1984). Human–proboscidean associations have, however, also been reported at a number of other locations in the east, including at the Coats-Hines site in Tennessee, the Hebior and Schaefer sites in Wisconsin, Martins Creek in Ohio, and Page-Ladson and Silver Springs Run in Florida, to mention perhaps the best-known examples (Broster et al., this volume; Deter-Wolf et al. 2011; Lepper and Funk 2006; Overstreet 2005; Webb 2006). Tools worked from green bone and ivory from a number of extinct Pleistocene species have also been found in a number of Florida rivers and sinks, suggesting direct exploitation, although scavenging of recently deceased animals also remains an option (Dunbar and Webb 1996; Hemmings 2004; Hemmings et al. 2004). As Fiedel argues herein, extinctions may have been under way 1,000 to 2,000 years before Clovis became widespread, given a decline of spores associated with large herbivore dung and a spike in charcoal particulates indicative of increased burning in the Northeast, secondary evidence for possible earlier human settlement and perhaps predation (Gill et al. 2009). As more archaeological sites are found in the east demonstrating unequivocal associations between Paleoindian populations and megafauna, I expect that the debate about the role humans played in their demise will grow more intense locally.

Halligan's overview herein of environmental conditions in the Southeast during the late Pleistocene is an excellent descriptive synthesis of recent paleoenvironmental research, highlighting the diverse and mosaic nature of the environment, which was completely unlike modern floral and faunal distributions in the kind and range of species present. Halligan points out the limitations of existing data, notably that they are patchy and incomplete in many areas, and makes the case that more multidisciplinary reconstructions of late Pleistocene environments are needed. Importantly, Halligan provides reconstructions of major biomes in the region, mapped at thousand-year intervals. It would be interesting to see whether specific archaeological assemblages or cultures can be linked to these biomes. Models of Paleoindian and Early Archaic aggregation and settlement within the region favoring the use of macro-ecotones, additionally, should pay close attention to the time-transgressive nature of these boundaries (e.g., Anderson and Hanson 1988; Miller and Smallwood 2009).

The impact of sea level rise on late Pleistocene human populations, Halligan argues, was marked and something groups located anywhere near the coast would have had to deal with. Approximately one-quarter of the land area of the Southeast was lost following the Last Glacial Maximum due to sea level rise, much of it prior to the onset of the Holocene. Direct exploitation of coastal environments would have been shaped by these transgressions and, in colder periods, regressions since—given the low relief in many coastal areas, especially along the Gulf and South Atlantic coasts—minor changes in sea level would result in the submergence or exposure of large areas, often within a few years and certainly within a human lifetime. Even minor fluctuations in sea level, on the order of 1 to 2 m, had profound effects on Late Archaic and initial Woodland settlement in the Southeast (e.g., Anderson 2010; Thomas 2008; Thomas and Sanger 2010), and the fluctuations occurring in the late Pleistocene were far greater in magnitude. Coastal margins were dozens to hundreds of kilometers seaward of their present positions, and the existence of this vast but now submerged coastal plain as well as environmental conditions on it for Paleoindian settlement remain largely unexplored, even at the theoretical level. Existing settlement models rarely do little more than mention that a vastly greater coastal plain existed, if they even do that, something that needs to be taken into consideration in future efforts (e.g., Anderson 1990, 1996; Anderson and Hanson 1988; Sassaman 2010; see Gingerich, Chapter 6, Carr et al., Chapter 8, and Smallwood et al., this volume). Our understanding of early settlement will never be complete until we can document what was occurring in those vast areas of the continental shelf lost to sea level rise.

Perhaps the best example we have about how the seacoast may have been used in Paleoindian times comes from the Northeast, where sites have been reported along the margins of the Champlain Sea (Loring 1980). We need to remember that there were potentially rich marine environments that Paleoindian foragers could have been exploiting all along the East and Gulf coasts, where the Clovis shoreline is in ca. 50 to 75 m of water (Balsillie and Donoghue 2004; Halligan, this volume). Archaeological research has been increasingly directed to the submerged portions of the continental shelf in recent years (e.g., Faught 1996, 2004a, 2004b; Illingworth et al. 2010), and the discovery of sites in primary context along now submerged shorelines, and particularly beyond the Clovis shoreline, would provide compelling evidence for use of the littoral by Paleoindian peoples and for pre-Clovis occupations. I suspect that such work will provide important insights about Paleoindian settlement systems in the years to come. The work will also help us understand whether and how archaeological sites survive sea level rise, something that will likely be of critical importance if climate change forces us to salvage the archaeological record from our current coastlines in the decades to come.

Sea level fluctuations and their relationship with the extent of ice volume on the continents also had dramatic effects well into the interior of eastern North America, affecting the extent and even the existence of massive water bodies such as the Champlain Sea and the periglacial Great Lakes. It also affected stream gradients and water tables throughout the region. The swamps and submerged sinks now in the Gulf and Atlantic coastal plains and in the Florida peninsula, for example, were for the most part high and dry during the late Pleistocene, highlighting the fact that the environment was quite different. Areas now rich in exploitable resources, such as modern estuaries or swamp margins, likely did not even exist in the late Pleistocene, at least not in their present locations. As Halligan argues, Paleoindian peoples would need to be sensitive to environmental changes, probably on an annual to at most decadal or generational scale, if coastal areas were to be effectively used. Given this, greater research should be directed to reconstructing sea level fluctuations and how they affected the landscape at and away from the coast during the period of early human settlement, to determine periods of greater or lesser landscape stability and see if any associations can be made with local archaeological cultures. The Younger Dryas, for example, was an extended cold period that may have resulted in a drop or relative stabilization of sea levels, at least when compared with the ca. 25-m rise Halligan argues occurred during the 450 years prior to its onset. Human (i.e., Clovis) populations present on the regional landscape during this interval may well have avoided coastal margins, although the record from the margins of the Champlain Sea noted previously suggests otherwise. Younger Dryas populations, in contrast, may have found coastal margins more attractive during this period, something that may explain an apparent decline in immediate post-Clovis occupations in portions of the interior (Anderson et al. 2011; Goodyear 2006). While sea level fluctuations may have been global in extent, how they played out from locality to locality, as recent work in the later Archaic Southeast has demonstrated, can only be determined through careful research (see Thomas and Sanger 2010).

Clovis adaptations were probably every bit as varied and diversified in different parts of the east as adaptations were in all later periods in prehistory, with a possible exception during the initial expansion of the technology if it was carried by rapidly moving or colonizing groups, as suggested by Kelly and Todd (1988). But once human populations using the technology were in an area, they probably figured out fairly quickly, in a few generations at most, what was good to eat, the best places to live, and where knappable stone was located, if they did not know already from prior (i.e., pre-Clovis) residence (Anderson 1990, 1995; Gardner 1983, 1989; Meltzer 2003, 2009; Miller and Smallwood 2009). While specialized adaptations likely did continue in some places, such as the seasonal to year-round use of caribou in the Northeast and Upper Midwest during the Younger Dryas, such adaptations must be demonstrated with artifactual data—as Loebel herein has innovatively done using wear patterns on endscrapers—and not merely asserted.

Site-Specific Reporting Efforts

A particular high point of the current volume involves the detailed syntheses of work at specific sites, notably Bull Brook, Higgins, Plenge, Shawnee-Minisink, Shoop, Topper, Wells Creek Crater, and the sites within the Flint Run Paleoindian Complex of northern Virginia. The best Paleoindian research has always been well-grounded research, with close linkages between site or artifact assemblages and models and interpretations. Exploring variability in eastern site assemblages as well as over individual categories of artifacts like projectile points, blades, or scrapers, or site settings and size, is of critical importance to breaking out of the somewhat rigid or simplistic settlement models that have tended to predominate.

Kurt Carr, with his colleagues, does double duty providing excellent and highly detailed overviews on the original and more recent work at Shoop and at the Flint Run complex of sites, two localities that perhaps more than any other have shaped thinking about Paleoindian occupations in eastern North America. It is gratifying to see that so many classic sites are the subject of renewed research, interpretation, and publication. The fact that this is even possible where the sites no longer exist, as in the cases of Bull Brook and Higgins reported herein, demonstrates the critical importance of properly curating assemblages and field records. Indeed, Robinson and Ort's chapter on the investigations at Bull Brook shows how early records may be revisited, reorganized, and checked for internal consistency, as well as the vast amount of work this may entail. Many of the essays in this volume, in fact, report on fieldwork conducted upward of a quarter to half a century ago, demonstrating that even though it might be many years before someone gets around to working with the materials again, if collections and records are curated properly, reanalyses will almost certainly happen and, as the chapters herein demonstrate, result in an even better understanding of what was going on in the past.

Carr, Adovasio, and Vento recount Witthoft's (1952) pioneering work with the Shoop assemblage, noting that it was one of only a few Paleoindian sites known and reported from the east in any significant detail by the early 1950s. Their account superbly documents how influential the work was and why it has remained so in the intervening years: because Witthoft focused on the site's lithic technology, including an early discussion of fluting technology and point manufacture, and because Shoop's setting has helped shaped thinking on where to find Paleoindian sites in North America to this day. Shoop was a dense scatter of artifacts located on a ridge crest and slope high above much of the surrounding terrain, with a cul-de-sac nearby where animals could be cornered. The setting was thus likely ideal for spotting, driving, and dispatching large game. Such overlook/cul-de-sac settings are known to have been important in other parts of the continent in Paleoindian times, as Judge's (1973) work demonstrated with Folsom occupations in the Rio Grande Valley of New Mexico, which were often found overlooking playas. Similar patterns have been inferred in the east, where fluted points have been found surrounding sinks at several locations in northern Alabama and near promontories such as Eagle Hill in western Louisiana (Gunn and Brown 1982; Hubbert 1989). The "Shoop" locational model arguably remained dominant until Coe's (1964) reporting of the excavations at Hardaway in North Carolina and, most critically, Gardner's (1974) work at the Flint Run sites, both of which showed the importance of quarry areas as central places in Paleoindian settlement systems in the east. It was Gardner's (e.g., 1977, 1981 1983, 1989) repeated exposition and refinement of his model, and the modifications to it by colleagues working in the same area—such as Custer et al.'s (1983; Custer 1984) arguments that a number of quarries could be used successively, in a serial or cyclical pattern—that led to its acceptance and the recognition that it had widespread applicability. Overlooks and quarries, in fact, remain prominent to this day in guiding where archaeological survey and excavation occur, in the east and beyond, and in the development of settlement models (e.g., Anderson 1996; Goodyear 1999, 2005; Meltzer 1988, 2009).

Witthoft also pioneered lithic technological analyses on eastern Paleoindian assemblages at Shoop, with his description of the Enterline Chert Industry, characterized by the use of guide flakes to facilitate fluting, as well as his consideration of the blades and cores on the site and their relationship to similar industries elsewhere in the Americas and beyond. While many of his interpretations have since been revised (e.g., Carr et al., Chapter 4, this volume; Cox 1986), Witthoft's interest in how eastern fluted points and other tools were manufactured provided a research pathway many subsequent researchers would follow. The Shoop site also provided a model for the existence and extent of Paleoindian activity areas on eastern sites, with artifact concentrations ca. 10 to 20 m in diameter assumed to represent locations where a family or small band camped for a single visit lasting a few days to weeks or perhaps a season or so. Such concentrations have been recognized at several eastern Paleoindian sites since, most famously at Bull Brook (Robinson and Ort 2011 and this volume; Robinson et al. 2009) and also at Debert in Nova Scotia (MacDonald 1968) and Nobles Pond in Ohio (Seeman 1994). Recognition of these clusters in the field, Carr et al.'s work shows, can be difficult, particularly if much of the site record is in overgrown terrain, requiring extensive excavation, or is in the plow zone and has been intensively collected (as Gingerich's work at Plenge, herein, also shows). Relocating concentrations found during earlier fieldwork can be equally challenging if precise provenience data are not available, although Robinson and Ort, herein, demonstrate how this can be done through painstaking examination of photographs, notes, and informant interviews, where the records and ideally the original excavators are still with us. The areal extent and spacing of these artifact clusters at Shoop, Bull Brook, and other eastern sites, as well as Carr et al.'s discussion, herein, of the size of the probable Paleoindian house area found at Thunderbird, at ca. 2,600 ft^2 or 330 m^2, serve as powerful reminders that small test pits and excavation blocks are incapable of telling us very much about how these sites were used.

Flat blades and debitage from blade production were found at Shoop in large numbers, derived primarily from angular and not the more typical bifacial or large polyhedral cores and resulting in curved blades characteristic of some western Clovis assemblages (Carr et al. 2010, and Chapter 4, this volume; Collins 1999). Similar flat blades were found in the Clovis levels at Topper, created from conical and wedge-shaped cores (Sain 2011; Smallwood et al., this volume) and Clovis blade shape, and that of the cores the blades were struck from at these sites appears related to raw material package size and knapping characteristics (i.e., small or lacking in homogeneity). While blades and blade cores are more commonly reported in the Southeast, Lower Midwest, and Mid-Atlantic and southwestern parts of the continent (Tankersley 2004)—where perhaps not coincidentally the greatest numbers of true Clovis points occur—why they are not ubiquitous in Clovis assemblages remains unknown. It has been suggested that there may be temporal or functional reasons for their uneven distribution, that perhaps they may have been made during only part of the interval Clovis points were being made in North America, or that they were used in tasks that were spatially or geographically restricted (Collins 1999; Tankersley 2004). The occurrence of Clovis tool forms such as blades and blade cores, formal endscrapers, and fluted points bears careful examination and explanation at regional or continental scales, since they appear to have somewhat different distributions (e.g., Tankersley 2004). A final tool form noted at Shoop, the blocky adze, has also been observed on other

Clovis sites in the east, such as at Topper (Smallwood et al., this volume). If used for heavy woodworking, as subsequent Dalton adzes are thought to have been employed (Morse 1997; Morse and Goodyear 1973), they may have been used to chop and shape posts for large and presumably fairly durable structures like that found at Thunderbird (Carr et al., Chapter 8, this volume) or alternatively to produce watercraft (e.g., Jodry 2005). Given the much greater distance the coast was from sites like Topper in Clovis times (>200 km), watercraft would have probably been a necessity if there was any interest in the regular exploitation of the littoral by these inland populations.

A final characteristic of the Shoop assemblage subsequently noted in many parts of the east is that the lithic raw materials on the site came from a great distance, in this case from the Divers Lake Onondaga quarries in western New York some 350 km away (Carr et al., Chapter 4, this volume). While other lithic materials were present in the assemblage in low numbers, the use of a high-quality nonlocal raw material was interpreted as related to extensive range mobility, possibly by a colonizing group. The use of high-quality lithic raw materials has subsequently become enshrined as something of a hallmark of Clovis Paleoindian assemblages in North America (Goodyear 1979), although as several of the authors herein demonstrate, locally available materials, often of quite varying quality, were also frequently employed. An examination of raw materials selected for use on typed Clovis and presumed Clovis fluted points from eight states in the southeastern United States, based on information recorded in state surveys in PIDBA as of January 2012, reveals that within this comparatively small area a great many raw material types were employed (Table 16.1). These same data show that while raw material identifications could stand to be far more specific and standardized than they are at present in and between the various state surveys, important generalizations can still be gleaned. While many Clovis points were made on high-quality cherts like Dover and Fort Payne, for example, a number of lesser-quality materials were also used. Many points are recorded as on unidentified cherts or other materials, furthermore, which suggests either that better local identifications need to be developed or that these raw materials may have come from beyond the state where the classification was made, since the material was unfamiliar. This would be in keeping with our understanding of the great distances these materials moved in Clovis times, although these inferences may change following new sourcing studies. Finally, it should be noted that many more points have been reported from the region and from many of these states than the numbers reported here, which encompass only artifacts where raw material was recorded for specific items. In some states, such as Florida, while the vast majority of fluted points are made on chert, unless this was specifically noted, the artifacts were not included in the table.

In another important observation with regard to raw material occurrence, Carr et al. herein note that while some 6,745 artifacts were present in the Shoop collections they analyzed, the total weight of this material was not very great, indicating

TABLE 16.1. Raw Material Types and Incidence on Typed Clovis and Presumed Clovis Fluted Points from Eight States in the Southeastern United States.

Raw Material	Tennessee	Mississippi	Alabama	Georgia	Florida	South Carolina	North Carolina	Virginia	Total
Fort Payne chert	245	18	343	3					609
Unidentified chert	54	56	16	31	8	31	26	364	586
Coastal Plain chert			3	266	2	143			414
Dover chert	360	4	1						365
Waverly chert	287								287
Bullseye chert	135								135
Chalcedony	5	2	3	1		1	7	107	126
Bangor chert	1		105						106
Quartz, unspecified			2	29		11	6	53	101

TABLE 16.1. Raw Material Types and Incidence on Typed Clovis and Presumed Clovis Fluted Points from Eight States in the Southeastern United States (continued).

Raw Material	Tennessee	Mississippi	Alabama	Georgia	Florida	South Carolina	North Carolina	Virginia	Total
Metavolcanic				6		17	72		95
Agate/jasper	5	2	3	7			7	60	84
Quartzite			1		1		4	74	80
Quartz, crystal				5		20	8	47	80
St. Louis chert	78								78
Buffalo River chert	42		5						47
Rhyolite						24		21	45
Silicified slate								29	29
Tuffs						28			28
Ridge and Valley chert				18		7			25
Orthoquartzite				6		9			15
Unidentified chert exotic		11							11
Camden chert	9								9
Unidentified chert local		8							8
Silicified shale								8	8
Greenstone								7	7
Petrified wood								6	6
Metasiltstone							6		6
Ocala chert			5						5
Silicified coral				3	1				4
Tallahatta sandstone		2	2						4
Burlington chert	3								3
Piedmont chert				3					3
Quartz, smoky			2			1			3
Black Mingo chert						2			2
Gravel chert	2								2
Ste. Genevieve chert	2								2
Tuscaloosa gravel chert			2						2
Shale								2	2
Banded slate								2	2
Novaculite		2							2
Gneiss/schist								2	2
Bolster Store chert								1	1
Carter Cove chert	1								1
Chicamauga chert	1								1
Knife River chert?			1						1
Knox chert	1								1
Pickwick chert		1							1
Ridley chert	1								1
Argillite								1	1
Basalt						1			1
Felsite								1	1
Kosciusko quartzite		1							1
Total	1,232	107	494	378	12	295	136	785	3,439

Source: Based on information in state artifact-recording projects submitted to the Paleoindian Database of the Americas as of January 2012.

that the site could have been provisioned by a few people (see Speth et al. 2010). Unless both count and weight data are provided, Speth et al. further argue, we must be careful to avoid equating large numbers of artifacts with either massive procurement efforts or extensive trade networks.

The size and complexity of the Bull Brook site, as painstakingly deciphered by Robinson and his colleagues in recent years, indicate that Paleoindian interaction and presumably ceremonial behavior took place at scales and with an intensity only hinted at prior to this, at sites such as Shoop, Debert, or Caradoc in the east or Lindenmeier in the west (Deller and Ellis 2001; MacDonald 1968; Wilmsen and Roberts 1978). Some 36 artifact loci or concentrations are documented at Bull Brook, 30 dominated by endscrapers and located mostly in an outer ring. Another eight concentrations are located inside this ring, most characterized by assemblages dominated by bifaces (channel flakes, limaces, and drills). Only a minor overlap in tool forms is evident between the two concentration types, which are thought to reflect locations where hide working and biface manufacture and fluting took place, respectively. This activity separation may have been simply for convenience, or as the authors suggest, fluted point manufacture may have been a more centralized public or ceremonial activity, and the distributions reflect a separation of sacred from profane or special from mundane behavior. Fluted point manufacture certainly took place in fewer locations, suggesting that it occurred less frequently than hide working or that finished points may have lasted longer than scraping tools. The distributions might reflect the proportion of individuals making or using each tool category, with fluting accomplished by a much smaller group, perhaps one--quarter to one-fifth of the total population based on the numbers of concentrations.

Resolving the contemporaneity or lack thereof of the artifact clusters at Bull Brook is critical to understanding how the site functioned. Described as an aggregation, marshaling, or staging area early on (e.g., Anderson 1990:196; Dincauze 1993), where multiple groups rendezvoused, Bull Brook may alternatively have been a locale repeatedly visited by a few groups. That the site hosted larger assemblies is supported by the ringlike arrangement of the clusters, the spatial separation of major activities, and the great distances and multiple sources most of the lithics found at the site came from (Robinson and Ort 2011 and this volume; Robinson et al. 2009). Accepting that Bull Brook minimally represents the contemporaneous interaction of two or more groups, and if multigroup aggregation was an occasional if not important part of Paleoindian life, a fundamental question that must be asked is, Where are the other sites like it? That is, why have no other Paleoindian sites the size and complexity of Bull Brook been found? Was the site an unusual ratcheting of complexity and interaction resulting from the actions of one or a few charismatic leaders, or if it actually does represent a regular aspect of Paleoindian settlement, at least in the Northeast, have most other such sites been lost to rising seas or development? Or have they been missed by the kinds of archaeological survey and excavation procedures used in many areas, with their emphasis on particular landforms and the placement of small, fairly widely dispersed test units in overgrown terrain, or the assumption that even on dense scatters, if obvious features that can be exposed by stripping are lacking, comparatively small block units may constitute effective mitigation? Even if another large site like Bull Brook is found, excavating it to the extent necessary to reveal its internal structure would require an immense amount of labor, probably over many years. Debert, the Flint Run Complex, Plenge, Shoop, Topper, and Williamson all may have been aggregation loci of greater or lesser complexity, and the level of effort it has taken to develop our current understanding of these sites—years to decades of concerted field effort—gives an idea of the magnitude of such a task.

The Sloan site in northeast Arkansas, the only Paleoindian cemetery found to date in the Americas (Morse 1997), offers a cautionary tale in this regard. Unusual sites may indeed be present on the landscape and either are undiscovered or have actually been found but are disappearing because we do not recognize what is present. A Dalton culture site characterized by hypertrophic bifaces known as Sloan points, as well as several hundred other Dalton points, tools, and tiny fragments of human bone in a number of discrete clusters, Sloan was located well away from presumed habitation sites characterized by dense artifact scatters, on a sand dune devoid of occupational debris. The landform type is one that even today would probably be only minimally examined during survey projects, and Sloan was, in fact, only discovered when erosion and plowing brought spectacular artifacts to the surface. Indeed, given the small size of the burial area, ca. 14 × 14 m, Sloan would have likely been completely missed in most cultural resource management projects, given the ca. 20- or 30-m shovel test intervals routinely employed in the region. Fortunately, the landowner reported the discovery to a professional archaeologist, Dan F. Morse, who had both the resources and the determination to immediately excavate it. Otherwise looters would have likely soon ripped the deposits apart with little thought to context (i.e., clusters or grave lots) and dispersed the materials in secret. Alternatively, if unreported and unplundered, the assemblage would have likely been slowly exposed by plowing each year and collected until nothing was left. The number of oversized Sloan points known from the central Mississippi Valley suggests that other cemeteries have indeed been found in the region but have not been recognized as such (e.g., Koldehoff and Walthall 2009; Walthall and Koldehoff 1998).

We are thus very lucky to have such good data from even one probable aggregation site like Bull Brook or one cemetery like Sloan. As Robinson and Ort make clear in their chapter herein, without the commitment of dedicated avocational archaeologists working ahead of gravel mining, we would know next to nothing about what is arguably the most significant Paleoindian site found to date in eastern North America. Avocationals continue to supply the vast majority of the labor at many ongoing Paleoindian excavations and are the source of most of the artifacts recorded in fluted point surveys (Anderson et al. 2010; see also Broster et al., this volume). Some ongoing Paleoindian excavations, like at the Topper site, or recording projects, like PIDBA, have as major components the training of avocationals and instilling in them the kind of ethical behavior and responsibilities that the practice of archaeology requires. Many of the Paleoindian research projects in the east that I am aware of in recent decades, in fact, have benefited from the participation of dedicated volunteers.

Like the work by Robinson and Ort at Bull Brook, Gingerich's chapters and other recent research at the Plenge and Shawnee-Minisink sites, Tune's work with Wells Creek Crater, Blong's work at Higgins, and Smallwood, Miller, and Sain's work at Topper all show how new generations of researchers are bringing new analytical and theoretical perspectives to older sites, generating new insights, and putting them back on the profession's radar screen. The variability in the projectile point assemblage at Plenge is remarkable and something not suspected from Kraft's (1973, 1977) original reporting, and it shows that older and more limited reporting of classic archaeological sites may give us a very unrealistic picture of what was actually present (see also Tune, this volume). The Plenge site exhibits a remarkable diversity of fluted, unfluted, and Plano forms. Hence, Gingerich's claim that the assemblage has examples of "nearly every known fluted point style identified in eastern North America" is only a mild hyperbole; Plenge certainly exhibits just about every known point form found in the Midwest and Northeast. About the only obvious southern type missing is the Cumberland, although some of the site's Barnes points are quite similar in appearance, only lacking the pronounced ears that particularly characterize the Cumberland type. What this diversity means is less clear, although Gingerich (Chapter 6, this volume) convincingly demonstrates that it reflects centuries of reuse, even if much of it was short term, by groups whose ranges included the Mid-Atlantic and the Northeast. Another suggestion he advances is that aggregation events, by groups from one or both of these regions, may have occurred at Plenge at various times during the Paleoindian era. The Feronia locality in central Georgia, located near the divide between the Gulf and Atlantic coastal plain watersheds, and hence in a similar macro-ecotonal setting, is also characterized by appreciable artifactual diversity (Anderson 1990:203; Anderson et al. 1990), suggesting that these kinds of settings are particularly good places to look for aggregation sites. The most recent model directed to finding such staging or aggregation areas, in fact, emphasizes such characteristics, notably locations along major rivers on or near macro-ecotones and ideally also near sources of high-quality knappable stone (Miller 2011; Miller and Smallwood 2009).

Tune's (2010 and this volume) analysis and reinterpretation of early work at Wells Creek Crater by Dragoo (1973) is another important cautionary tale, serving much as Gingerich's work at Plenge described above does to remind us that we need to be very careful about taking our colleagues' published work at face value, especially in the absence of detailed site reports indicating what was found and how it was recovered and analyzed. Tune's work also highlights the need to maintain thorough records from our work and curate them in appropriate curatorial repositories (Sullivan and Childs 2003); the records from the work at Wells Creek Crater have been lost even though the investigations were conducted under the aegis of a major museum, the Carnegie Museum of Natural History. In today's digital age, there is no reason why notes or images should ever be lost, since copies can be scanned and placed in multiple locations. I know from personal experience how difficult it is to write up collections where the original field records are missing (e.g., Anderson 2008) and thus strongly believe that researchers should *never* be allowed to remove primary field records from curatorial facilities, unless copies have been made and left on file beforehand. Finally, while all archaeologists emphasize some parts of their work more than others, they have an obligation to document entire site assemblages, especially where this is critical to their interpretation. The collections from Wells Creek Crater were dominated by Archaic and Woodland diagnostics, something not apparent in the original reporting. Since much of the collection apparently came from surface context, this renders suspect interpretations of all but the most unequivocal diagnostics, at this site or indeed any site, as Paleoindian in age.

Accordingly, my inferences and those by others about the importance of Wells Creek Crater to Clovis populations in the region need to be rethought (Anderson 1990, 1995). While it was clearly a location used by Paleoindian groups, as indicated by the recovery of six Clovis points, nine small flat prismatic blades, and a number of preforms, scraping tools, and debitage of probable Clovis age, as documented by Tune (this volume), the site does not have the numbers of artifacts that would be expected if it served as a location where major aggregation events occurred. Instead, it appears to be merely one of many such small Clovis sites in the Midsouth (Broster et al., this volume). Accordingly, while I still believe that unusual spots on the landscape would likely have been readily recognized and remembered, and hence been good locations for differing groups to meet, barring the recovery of much better evidence than we have at hand, that seems unlikely in the case of Wells Creek Crater. I should also note, however, that the number of Clovis points and sites, and the richness of the habitat, leaves me convinced that the Midsouth was indeed a staging area, a place where extended settlement, albeit still coupled to extensive range mobility, occurred early in Clovis times, "providing a stable social environment within which populations could grow and familiarize themselves with their surroundings" (Anderson 1990:187). As such, the staging area model remains an alternative to views that Clovis populations were highly mobile, dependent on their technology rather than settling into particular places (cf. Anderson 1990:202–205; Kelly and Todd 1988).

The chapter by Carr, Stewart, Stanford, and Frank on the Flint Run Complex herein, besides being an excellent overview of the many years of work at this locality and a good summary of the findings at each site that was examined, shows why William M. "Bill" Gardner's thinking, and that of his many students, profoundly shaped not only our ideas about Paleoindian settlement patterning in the east but also the field and analysis methods used to explore sites and assemblages dating to this period. Gardner made researchers aware of the importance of quarries to eastern Paleoindian Clovis groups and the kinds of sites that occurred on, near, and away from them. Six site types were proposed by Gardner (1977:258–259, 1983; Carr et al., Chapter 8, this volume), three associated with quarries and three located away from them: (1) quarries, where primary extraction took place; (2) quarry/lithic reduction stations, where initial reduction prior to transport occurred; (3) quarry-related base camps, where a wide range of activities including extended habitation occurred; (4) periodically revisited non-quarry-related hunting/gathering camps; (5) sporadically visited non-quarry-related hunting/gathering camps; and (6) isolated point finds.

Gardner envisioned Clovis groups in the

northern Virginia area moving in a cyclical fashion, radiating out from and coming back to quarry areas, in a looping seasonal round whose pattern resembled the petals on a daisy. Custer et al. (1983; Custer 1984) modified this somewhat, arguing that groups not only moved to and from specific quarries but also moved between quarries, in a serial daisy-chain pattern (as described in detail in Blong, this volume; and Carr et al., Chapter 8, this volume), making use of lithic raw materials from a variety of sources. A similar settlement model was proposed by McAvoy (1992) based on work in southeastern Virginia. This pattern of serial movement between quarries, Carr et al. herein argue, appears to be more common than systems based on the exploitation of a single source. Paleoindian settlement systems in the Mid-Atlantic region contrast markedly with those in the Northeast and Upper Midwest, however, which are much larger in extent, probably because they were shaped by the movements of migratory prey, specifically caribou herds. In the woodlands to the south, especially following Clovis, principal game animals such as white-tailed deer are unlikely to have ranged very far, if the behavior of modern animals provides an accurate parallel (e.g., Beier and McCullough 1990; Kolodzinski et al. 2010).

The extent to which Paleoindian groups were tethered to raw material sources in areas where knappable stone was widespread has been questioned, although there is no doubt that high-quality sources were highly favored, and repeatedly visited, in many parts of the east (e.g., Anderson 1990:182; Goodyear et al. 1990). The parallels between the assemblages at Topper and the Flint Run Complex are striking, specifically in the kinds of activities that took place and the locations where they occurred (cf. Carr et al., Chapter 8, this volume; Gingerich, introduction, this volume; Smallwood et al., this volume). Initial reduction took place right by the exposed raw material, with more intensive reduction at nearby level areas, often only a short distance away. Habitation areas were still farther away, but still at nearby locations on the landscape.

Perhaps one of the most striking aspects of the work at the Flint Run Complex by Gardner and his colleagues is how extensive it was, with excavations at the Thunderbird site alone continuing for more than 15 years. Huge areas were surface collected in a controlled fashion, using 10-ft square mapped units, and sites were tested over large areas, with test units (typically 10-×-10-ft squares, although the dimensions varied) sometimes taken to great depths, to 11 ft at the Fifty site and almost 15 ft at the Ramsey site. Interestingly, and indicative of the circumstances that sometimes dictate fieldwork, as discussed by Carr et al. herein, Gardner was forced to relocate his team to another site when Thunderbird was flooded following Hurricane Agnes. Goodyear (1999, 2005) likewise was forced to relocate his excavations during a period of flooding from the Big Pine Tree site to Topper, resulting in the discovery of the remarkable Clovis and more controversial pre-Clovis assemblages at the latter site.

Post molds from a possible house measuring roughly 20 ft north–south by 35 ft east–west were found immediately below the plow zone at the Thunderbird site, on a level area on a small alluvial fan, amid a variety of multipurpose tools, bifaces, and utilized flakes (Carr et al., Chapter 8, this volume; Gardner 1974, 1989). The presence of a Middle Paleoindian point in the plow zone, and the near absence of later materials (Carr et al., Chapter 8, this volume; Gardner 1989), suggests that the structure was likely built during the Younger Dryas. As it is the only reasonably well-defined Paleoindian structure found to date in eastern North America, its importance cannot be underestimated, although the post mold pattern remains frustratingly ambiguous. As depicted in Figure 8.6, herein, the post pattern looks like a small longhouse with internal partitions or perhaps two or more structures, some with post-hole patterns only partially surviving, that were either close to one another or perhaps built at different times. More detail on the individual posts would be nice to have, as part of a comprehensive site report, to assess the likelihood that they are indeed structural elements, especially since other concentrations of multipurpose tools found on small alluvial fans elsewhere on the site did not have post molds present. The Thunderbird example does, however, suggest the kind or size of structures that may have been present at the similarly sized concentrations of lithics documented at sites such as Bull Brook, Plenge, and Shoop. Wood for use in shelter construction may have been more accessible farther to the south, although existing paleoenvironmental evidence (e.g., McWeeney, this volume; Newby et al. 2005) indicates that this was unlikely a problem, even in the Northeast, during the Younger Dryas. Identifying structures used by Paleoindian populations in the east remains a major research challenge.

The Flint Run sites are sometimes considered to have provided a complete projectile point sequence spanning the Paleoindian through the Early Archaic periods, and in fact artifacts from the area have been used to propose such a sequence (Gardner and Verrey 1979). As Carr et al. herein document, however, although a number of Clovis points were found at the base of the cultural deposits in Flint Run Complex sites, few points postdating Clovis and predating the Early Archaic period were actually found in stratified context, only one point, in fact, at Thunderbird and none at Fifty, although a great many presumed Middle and Late Paleoindian points were found in surface and plow-zone contexts. As Carr (1975, 1986, 1992) has superbly documented in a research effort spanning many years, Fifty does have a remarkable stratified sequence of occupations spanning the Early Archaic period. Arguably one of the best such sets of assemblages in eastern North America, the site record complements cultural sequences developed for this period in the Little Tennessee River Valley, at St. Albans in West Virginia, and at Hardaway in North Carolina (Broyles 1971; Chapman 1985; Coe 1964). As several authors in this volume note, we need more such stratified sites, or single-component sites with datable remains, encompassing the Paleoindian era in eastern North America. If in the process remarkable Early Archaic assemblages are found, it should be remembered that in many parts of the region we know even less about occupations during that period than we do about those during the preceding Paleoindian era.

Gingerich's (2007a, 2007b, 2011, and Chapter 9, this volume) work with Shawnee-Minisink is another excellent summary of both older and newer work at a classic site, perhaps best known for preserved botanical remains that have played prominently in debates about Clovis subsistence, as discussed previously. Gingerich's recent excavations were superbly controlled, providing a wealth of additional artifactual and paleosubsistence data, as well as new and highly consistent accelerator mass spectrometer radiocarbon dates from the site that place it securely in the range for the Clovis horizon (Waters and Stafford 2007). There is no question, given the analyses for disturbance processes conducted, that the author is correct in arguing that the site is "one of the most spatially intact Clovis assemblages" ever found. Given the excellent preservation of ethnobotanical remains in the deposits it should be subject to more excavation in the future. Gingerich's approach to radiocarbon dating, using charred seeds (i.e., annuals) and submitting samples to multiple labs, is a model of how the process should occur, and when the new dates are added to those obtained by an earlier excavator (Dent 2002, 2007), his comment that Shawnee-Minisink is "one of the best-dated Clovis sites in North America" is a simple fact, not hyperbole.

Gingerich has done a good job not only recounting his own work but also in incorporating the work of other investigators who have worked or continue to work at or with materials from Shawnee-Minisink. The debate about Clovis paleosubsistence between two of these investigators (cf. Dent 2007 and Dent and Kauffman 1985 with Gingerich 2007b, 2011, and Chapter 9, this volume) serves to illustrate how difficult it can be to demonstrate that the presence of botanical remains on a site equates with their use by the people who occupied it. Gingerich's review of evidence from Paleoindian sites providing support for plant use in the Northeast indicates that fruits, such as blackberry, cloudberry, and hawthorn, were eaten in this region. Similar analyses conducted at later Paleoindian sites in the Southeast, particularly in rockshelters where preservation tends to be better than in open-air sites, as at Dust Cave in northern Alabama, indicate that the use of plants was also an important part of Paleoindian life in this area, at least in the period following Clovis when rockshelters began to be heavily visited (Hollenbach 2007, 2009; Walthall 1998). I particularly like one of Gingerich's concluding thoughts, that placing Shawnee-Minisink into existing Paleoindian site categories is not easily done or probably even necessary at present. The uncritical use of any typologies, be they for points, sites, or subsistence/settlement models, can constrain consideration of variability, and as the essays in this volume demonstrate, eastern

North American fluted point cultures and assemblages appear to have been highly varied, both within and between regions (see also Haynes, this volume).

Blong's chapter on the Higgins site is an excellent example of the kind of detailed reporting we need for even the smallest of Paleoindian assemblages and what we can learn from examination of such sites. Higgins yielded a modest Clovis assemblage, unlike the much larger assemblages found at nearby sites like Plenge, Shawnee-Minisink, and Shoop. Blong's chapter, like several others in the volume, also shows the utility of reexamining assemblages reported earlier, in this case during cultural resource management work in the 1980s. The Higgins site excavations were superbly reported in a massive three-volume study by Ebright (1992), but as in any large reporting effort, specific aspects of the work could always benefit from follow-up investigation, as Blong's reanalysis demonstrates. A wide range of lithic raw materials were used at Higgins, a situation markedly different from that reported by Carr et al., herein at Shoop, where one chert type from far to the north, Onondaga, dominated the assemblage. As a number of researchers have suggested, the use of high- vs. low-quality and nonlocal vs. local lithic raw materials not only appears to be related to the availability of these materials on the landscape but may also be characteristics of early vs. later occupations in the Clovis era, before and after people had figured out where stone sources were located on the landscape. Blong demonstrates that the Higgins assemblage is dominated by lower-quality local lithic materials, mostly from bedrock and gravel sources, and, contra Gardner's model, fits a pattern observed by Lowery (2002) from his work on the nearby Paw Paw Cove Paleoindian Site Complex, where Paleoindian populations made extensive use of a variety of lithic raw materials, essentially whatever was knappable in the area. A small amount of nonlocal materials obtained from a considerable distance was also present at Higgins, however, indicating that mobility or interaction included both local and more distant components. Blong also makes the case that quartz was an important lithic raw material to Paleoindian populations in the Mid-Atlantic, and as argued recently, its use during the Clovis era may be more important and prevalent than previously assumed (Speth et al. 2010; see Table 16.1, showing that roughly 5 percent of the typed Clovis and presumed Clovis fluted points in the eastern part of the Southeast are made of quartz). Finally, as at Shawnee-Minisink, possible bird remains, in this case turkey feather fibers, were reported at Higgins in association with a possible hearth, but the age and identification of this material remain equivocal at present (see also Fiedel, this volume).

Smallwood, Miller, and Sain's chapter on the Topper Clovis assemblage, the first major synthetic statement on those deposits, indicates the level of field and laboratory work that has been needed to bring this remarkable site before the profession. The project archaeologist, Albert C. Goodyear, in an effort comparable in scope to the work William Gardner led at the Flint Run Complex, has been surveying and excavating sites at and near the primary outcrops of Allendale chert in South Carolina and nearby areas of Georgia for over three decades. Like Gardner before him, Goodyear has been equally gracious in facilitating student analysis of and reporting on the excavations. When M.A. or Ph.D. students spend several years of their life working on Paleoindian archaeology, important research results come of it, as many of the chapters in this volume clearly demonstrate. Several M.A. and Ph.D. dissertations, and papers derived from them, have been produced or are under way based on Clovis materials from Topper (e.g., Miller 2007; Sain 2011; Smallwood 2010, 2011, 2012; Smallwood and Goodyear 2009; Smallwood et al. 2008; Smallwood et al. 2010; Smallwood et al., this volume). The first monograph providing detailed attribute and spatial data on about 10 percent of the Clovis assemblage from the site, and directed to evaluating its integrity, in fact, appeared in 2010, based on Miller's 2007 M.A. thesis research (Miller 2010). A second monograph on the Clovis blade assemblage from the site has been just released (Sain 2012a), and several more are currently in preparation. Dozens of researchers have visited the Topper site in recent years and witnessed the densely strewn Clovis activity areas being excavated by a remarkable team of young and not-so-young scholars, students, and volunteers. The fact that Smallwood et al.'s chapter herein summarizes the assemblage from only about a fifth of the area actually excavated, and that area appears to represent less than 10 percent of the Clovis deposits present on the landform, indicates how much work remains to be done at the site. The pre-Clovis-aged materials at Topper (Goodyear 2005), while dismissed by Fiedel and Haynes (this volume) and indeed by many Paleoindian researchers (Wheat 2012), are themselves currently undergoing thorough examination as part of graduate student research, which should help resolve the status of this controversial assemblage (e.g., Hoak 2012; Sain 2012b).

Patterns of quarrying behavior at Topper at and short distances away from the outcrops are very similar to those documented at Thunderbird (Carr et al., Chapter 8, this volume). Indeed, besides providing an excellent initial description of the Topper Clovis assemblage, a major contribution of the research by Smallwood, Miller, and Sain, herein, is demonstrating the significant variation in assemblage composition that can occur within no more than a few meters from the outcrops at quarries. Activities like those subsumed under Gardner's (1977, 1983) first two site types, quarries and quarry/lithic reduction stations, occur within short distances of each other at Topper. The authors demonstrate, for example, that the extent of biface reduction is related to distance from the outcrop, with significant changes occurring over meters and not at different discrete sites (see also Miller and Smallwood 2012). Likewise various tool forms such as blades and bifaces were produced or discarded in slightly different locations, and a wider array of tool forms is also present, suggesting that activities characteristic of Gardner's third site type, the quarry-related base camp, were also occurring at Topper, all within a few tens of meters of the outcrops and well within an area most researchers would consider a single site. Microtopographic variation, their analysis indicates, is critical to understanding Paleoindian quarry use. Given the extensive research and publication activity associated with the Flint Run and Topper projects, they are the best samples we currently have for documenting Clovis quarry use in the east.

Broster et al.'s chapter herein summarizes Paleoindian research in a state, Tennessee, where the most Paleoindian points of all kinds have been recorded in North America, thanks in part to decades of data collection and reporting by two of the authors, assisted down through the years by many avocational and professional colleagues (e.g., Broster and Norton 1996). Similar artifact-recording projects have been under way in many other states, in some cases for decades, but Broster and Norton are exemplary for having collected and made freely available attribute and image data for their entire sample, 5,432 points as of January 2012 (Anderson et al. 2010:66–67). The high numbers, of course, also reflect the fact that the Tennessee and Cumberland river basins in the Midsouth—in northern Alabama, central Tennessee, and southwestern Kentucky—appear to have arguably the richest and most diverse Paleoindian record in North America. While quarry, mastodon kill, and even pre-Clovis sites appear to be present, the record from the region is so great that existing publications have only scratched the surface.

The variability in fluted points in Tennessee is thought by Broster et al., herein, to be greater than in any other part of the Americas (Plenge perhaps excepted, as Gingerich [Chapter 6, this volume], argues the same thing for the points found at that site). This variability may suggest a great antiquity for fluting in the Midsouth, something hinted at by the early dates from the Johnson site (Barker and Broster 1996), or as Gingerich, herein, argues it represents at Plenge, a high degree of interaction with peoples in surrounding areas, something that would be facilitated by movement along the major rivers traversing the region. Finally, the large numbers of Clovis and immediate post-Clovis fluted points in the Tennessee Fluted Point Survey, particularly Cumberlands—if the type is not pre-Clovis in age, something Gramly (2009) has argued—indicate that the decline in the total number of immediate post-Clovis forms observed over the Southeast as a whole (Anderson et al. 2010:77–78; Anderson et al. 2011), and in the Carolinas in particular (Goodyear 2006), was not the same everywhere and may not even have occurred at all in some areas. If the Midsouth was an optimal place for Clovis settlement, and a major staging area (e.g., Anderson 1990:187), it may well have remained so during the early Younger Dryas. The lesson is that we must be careful to avoid assuming that trends in radiocarbon dates or projectile point counts observed at a regional scale played out the same way at smaller scales (e.g., Broster et al., this volume; Miller and Gingerich, this volume).

Tom Loebel's chapter on methods for determining the function of endscrapers reminds us that there are other important Paleoindian artifacts worthy of detailed attention beside projectile points. Loebel's work is redirecting much deserved research attention to some of eastern North America's truly remarkable Paleoindian sites, in this case Hawk's Nest, Gainey, Nobles Pond, and Shawnee-Minisink. At these and indeed at many sites where both artifact categories are found, endscrapers are typically an order of magnitude more common than hafted bifaces. Loebel's work shows that a great deal of information about site function, including evidence for the working of perishable materials since lost, can be determined from the careful inspection of endscrapers. Finally, Loebel's photographs of edge wear types provide excellent guidance for other researchers working with these tools, as do his observations on how to properly conduct the analyses. These include such important steps as removing pseudo-polish, some of which is recent and has humorously been described as "fondling wear," or the need to look at edge margins remaining following "failed resharpening" episodes, which often took place immediately prior to discard. While the tool may have broken during resharpening, the surviving unresharpened margins often, he demonstrates, contain traces of wear from earlier use episodes.

Pre-Clovis Settlement in the East: Argument and Evidence

Stuart Fiedel herein reviews evidence for pre-Clovis occupations from a number of well-known sites in eastern North America, devoting particular attention to Meadowcroft and Cactus Hill, as well as to the "Solutrean colonization hypothesis" offered in recent years (e.g., Bradley and Stanford 2004; Stanford and Bradley 2002, 2012). I am not going to recount the many salient points raised by Fiedel, or take sides regarding his conclusions about specific sites, but, instead, suggest that readers draw their own conclusions after reading the available literature. I will say that science would be rather dull if we all agreed with one another or were afraid to ask hard questions or face criticism. Knowledge certainly would not advance as quickly without the clash of ideas and evidence, however uncomfortable it can be when it is our own work that is subject to challenge. Regardless of whether the reader agrees with anything else Fiedel says, an unassailable truth in his chapter is that the thorough reporting of fieldwork and analyses is essential to the evaluation of archaeological arguments based on them. Unfortunately, as he points out, reporting or even making primary data available, typically through the writing of site reports or monographs, or permitting others to work with collections from sites, does not happen as often as we might like. The reward structure of the profession, at least in academia, in fact, causes many scholars to fragment their research into numerous peer-reviewed journal articles, rather than produce the comprehensive books, monographs, and site reports that have long been considered the primary product of our profession (Anderson 2003:116–117, 2005:36–37). This ethos must change. Providing detailed descriptions of our excavations and attributes and observations from our analyses is a basic professional responsibility for archaeologists and is a part of the codes of ethics of most major professional organizations (Society for American Archaeology 2011).

Fiedel raises uncomfortable points in this regard, notably the failure of some Paleoindian scholars to produce final reports on their excavations. Given the labor that went into the fieldwork at many of the classic sites explored in the east, that so few of them are well reported is, as he says, lamentable. As such, the syntheses of work at the classic sites reported herein are even more of a service than many readers might recognize. Yet even in these chapters, references to materials that remain unanalyzed are more frequent than we would like. Brian Fagan (1995) once prominently called the failure of many archaeologists to devote the same level of effort to analysis and reporting that they did to fieldwork "archaeology's dirty secret" and was excoriated as a result (he survived). Producing lengthy site reports encompassing the description of field methods, analyses, and sometimes thousands or tens of thousands of artifacts and features is hard and sometimes tedious work—and not nearly as likely as shorter articles to generate accolades from department chairs and deans, who may be more inclined to count points toward tenure or promotion than scientific importance. Fortunately, most archaeologists know that monographs and books are commonly used and referenced for decades after publication, while the citation half-life of journal articles is far more ephemeral, with only the most exceptional articles referenced more than a decade or two after publication. Journal articles shape current thinking, and hence are critically important contributions, but once issues are clarified and arguments are resolved, the field moves on, and save for historians of our discipline (e.g., Grayson 1983; Meltzer 1983, 2009), most scholars pay little heed to the debates that got us to where we are today. Articles are important, but site reports and monographs are as well, and if articles are all someone produces, he or she probably has no business excavating sites.

Fiedel devotes particular attention to the Paleoindian deposits at Meadowcroft, arguing that a comprehensive monograph describing the Pleistocene materials from the site and their associations, context, and possible sources of disturbance would resolve many of our questions. Given the duration and intensity of the debate about these remains, and in spite of what was said above, I doubt this would be the case. It would, however, have probably helped frame the debate better, avoided some of the inconstancies that Fiedel documents in the myriad of essays that have appeared about the site, and also likely obviated the need for many of them. McWeeney and Fiedel, herein, disagree about the occurrence of deciduous flora in full-glacial deposits at Meadowcroft and the possibility that the Cross Creek drainage was a refugium for these species in pre-Clovis times. Likewise Fiedel finds it difficult to accept that the Cactus Hill site, contra McWeeney herein, has been unequivocally demonstrated to be pre-Clovis in age, much less upward of 5,000 years older than Clovis. Regardless of what one thinks about the tone of the debate, it is interesting that a majority of those members of the archaeological profession who specialize in Paleoindian archaeology, and who participated in a recent survey on the state of the field, either disagree or are uncertain that Cactus Hill, Meadowcroft, and Topper are pre-Clovis in age (Wheat 2012). A majority of the same sample, however, accept that there were pre-Clovis occupations in the Americas. With this in mind, I am certain, as Fiedel has suggested, that we almost certainly have pre-Clovis artifacts in our collections but have not figured out how to recognize them as such.

Reporting and Sharing Primary Data

Thanks to aggressive interest in Paleoindian sites and artifacts, more primary attribute data on early diagnostics have been systematically collected in the east, and particularly the Southeast, than in any other part of the Americas. Much of the data has been compiled in individual states or provinces by one or a few people, and while much of it remains unpublished, several excellent statewide syntheses have been produced that serve as models of what can be done, even at comparatively local scales (e.g., Anderson et al. 1990; Fogelman and Lantz 2006; Goodyear et al. 1990). Many people have been willing to share their data, and for the past two decades my colleagues and I have been compiling them and making them available on the Web as the Paleoindian Database of the Americas (Anderson and Faught 1998; Anderson et al. 2010; http://pidba.utk.edu/). Vast amounts of primary data remain to be compiled, although any primary data sent to one of the PIDBA site managers in electronic form or hard copy are eventually posted. Some 10,000 projectile point images have been posted to date, and as the chapters in this volume demonstrate, there are many other kinds of Paleoindian artifacts like endscrapers or blades that can be added, as well as references and radiocarbon dates. Primary data are particularly needed from the western United States, Canada, and Latin America. People in many states in the Southeast, Midwest, and Northeast are doing an excellent job documenting what is out there. But it never fails to surprise me how few systematic artifact-recording projects exist in the west, although Haynes, Huckell, and Ballenger's efforts in Arizona, Rondeau's work in California, and the work of Beck and Jones and their student Amanda Taylor in the Great Basin are among the significant exceptions. Ruthann Knudson and I started a fluted point survey in Montana recently (Anderson and Knudson 2009)—she is actually doing all the work—and many more such local efforts are needed.

Evaluating the usefulness of projectile point attribute and distributional data like those in PIDBA for a range of research questions is something of a cottage industry and has led to important results, specifically documenting bias in the data (e.g., Buchanan 2003; LaBelle 2005; Lepper 1983, 1985; Prasciunas 2008, 2011; Seeman and Prufer 1982, 1984; Shott 2002, 2005) or in the development of settlement models (e.g., Miller 2011; Miller and Smallwood 2009). Compiling and sharing primary data, which allows for such studies to occur, is equally important, yet it is not as widespread as it should be. When reading articles on Paleoindian archaeology, I would urge readers to ask themselves where the primary data reside: the collections, the field notes, and above all the measurements and ideally photographs of specific artifacts under discussion. Papers and monographs that provide such data or references to where they may be found, I have discovered in 20+ years of compiling data for PIDBA, are the exception rather than the rule, although good examples include MacDonald's (1968) work at Debert and Miller's (2010) at Topper and many of the essays in this volume. A more common approach, however, is data summaries, giving average measurements for points or tool categories. While such studies may appear to yield impressive results, unless the primary data these analyses were based on can be found, they are essentially unverifiable. Fortunately, professional journals are increasingly mandating that such data be presented, typically as supplementary materials online. I am thus optimistic that Paleoindian research will become an ever more open enterprise.

Concluding Thoughts: How People May Have First Reached the East

Unequivocal pre-Clovis-age sites are present in Alaska, and it is not difficult to imagine how peoples could have reached the interior of North America, and specifically eastern North America, if they entered through the ice-free corridor. Populations moving south from the ice-free corridor, or east from the headwaters of the Columbia given a Pacific coastal entry, would encounter major east- and south-trending drainages like the Missouri, the Platte, the Arkansas, the Canadian, and the Red (Figure 16.1). These would funnel people into the Mississippi-Ohio-Tennessee system leading to much of the east, as well as down to the Gulf Coast, offering a number of movement pathways (e.g., Anderson 1990; Anderson and Gilliam 2000). The ice-free corridor entry route may have been closed or uninhabitable until after ca. 11,000 RCYBP or possibly even somewhat later, although this remains controversial since recent geological mapping shows that the corridor was open much earlier, by ca. 12,000 RCYBP (cf. Arnold 2002; Dyke et al. 2003; Mandryk et al. 2001). When movement using this route first occurred is currently unknown, but it was almost certainly by Clovis times or perhaps slightly earlier, when descendants of Nenana/Tanana Valley populations may have moved south through it (e.g., Haynes, this volume).

But what if, as has been argued, human populations were moving down the West Coast well before the ice-free corridor opened (e.g., Erlandson et al. 2007; Erlandson et al. 2008; Fladmark 1979)? How, then, did people reach eastern North America? It is difficult to reconcile early populations far down the coast in South America after ca. 12,000 ^{14}C BP with the minimal evidence we have at present for contemporaneous occupations in interior North America (Surovell 2003; Waters et al. 2011). But what if coastal environments were the "megapatch" of choice (after Beaton 1991), with people content to stay on or near the coast, perhaps bypassing the Columbia River route into the interior entirely? Assuming rapid movement coupled with minimal infilling of the interior, with populations opting to stay and infill only coastal and near-coastal areas, they could have quickly moved down the coast. Even if they did move away from the coast and into the interior farther to the south, along drainages like the Sacramento or San Joaquin in California, they likely did not get very far to the east, at least easily, given mountain ranges like the Sierra Nevada and beyond them the interior-draining Great Basin. Interesting enough, such a pattern of movement into the interior would encompass roughly the same area occupied by the western stemmed horizon, which some have suggested could be a signature of inland movement by coastal populations (e.g., Beck and Jones 2010).

There is an easier or at least an additional way for coastal peoples to get to the east, assuming they did not travel down the Columbia River or

FIGURE 16.1. Baja/Colorado River Interior North American Colonization Model. Migration along the west coast of North America would proceed in a southerly direction until the southern end of the Baja Peninsula, after which movement would turn northward. At the northern end of the Gulf of California, the Colorado River would offer a major route into the interior. From the Colorado River and its tributaries, many south- and eastward-flowing drainages are a short distance overland from the Colorado. (Image prepared by Stephen Yerka using topographic data from Amante and Eakins 2009 and ice sheet locations from Dyke et al. 2003.)

across the Great Basin, and that is up the Colorado River (Figures 16.1 and 16.2). When peoples moving down the West Coast reached the end of the Baja California peninsula, and land literally ran out, unless they had watercraft capable of making the crossing to Mexico (an open-water gap of about 100 km at 15,000 cal yr BP) and were even aware of the possibility, they would have had to turn back to the north. Moving north along the east side of Baja, unless they island-hopped over to Sonora near the northern end of the Gulf in the vicinity of Isla Ángel de la Guarda and Tiburón Island, they would have encountered the mouth of the Colorado River. Some populations may have chosen to follow this mammoth waterway into the interior, while others, continuing on down the

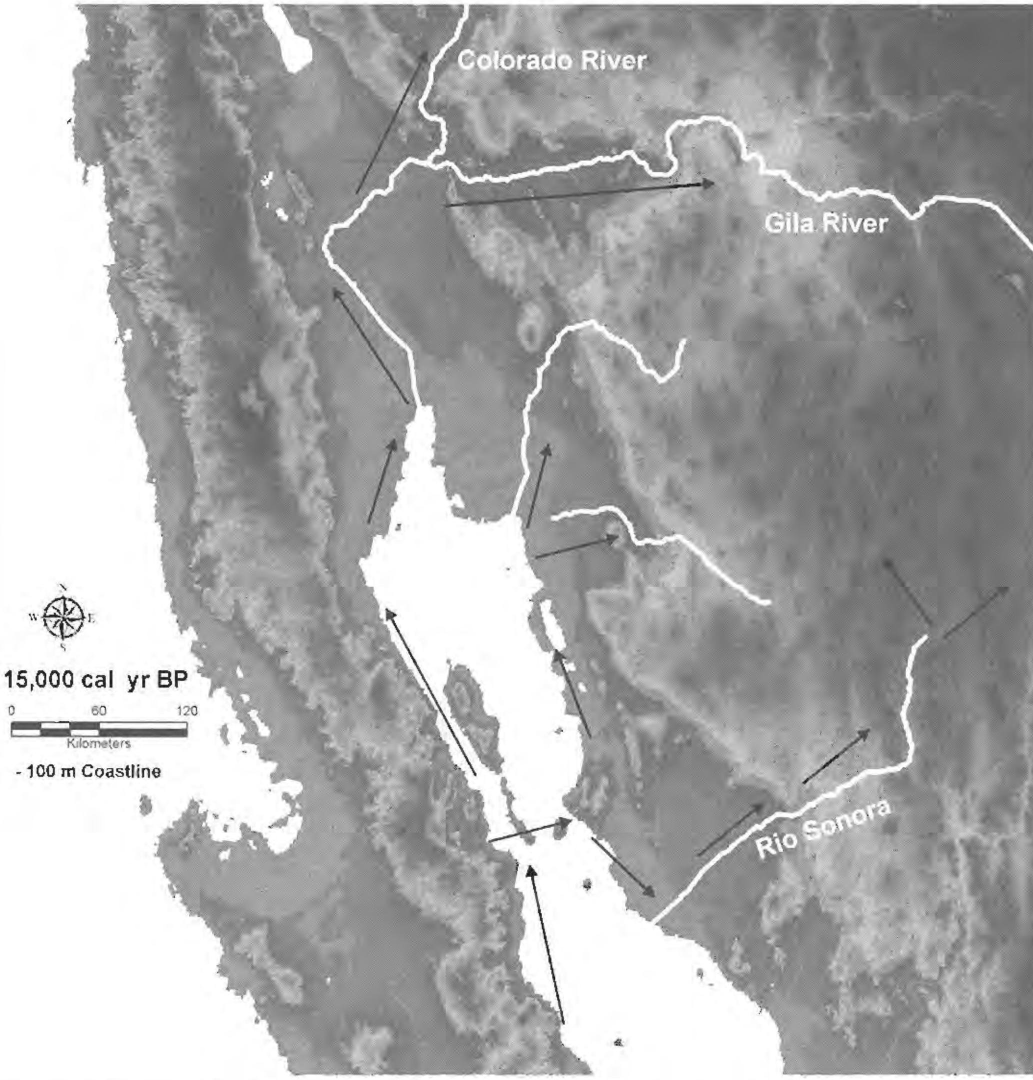

FIGURE 16.2. Close-up of the northern end of the Gulf of California showing possible movement pathways into the interior, which likely took place along the Colorado River or the rivers of Sonora. Rivers in Sonora are easily reached across the narrow open-water gaps formed by lowered sea levels in the vicinity of Isla Ángel de la Guarda and Tiburón Island. The large numbers of Paleoindian points and sites found in Sonora, accordingly, may be a result of early settlement (Gaines et al. 2009; Sánchez 2010). In addition to drainages reached from the northern Colorado, a major tributary near its mouth, the Gila, flows across southern Arizona and New Mexico and may offer a particularly rapid route into the east via the Rio Grande to the Gulf of Mexico. (Image prepared by Stephen Yerka using topographic data from Amante and Eakins 2009 and ice sheet locations from Dyke et al. 2003.)

western coast of Sonora, would have eventually reached Central and South America. Populations moving inland along the Colorado would have been able to travel far into the interior, placing them at or near the headwaters of a number of major drainages leading to the east. With comparatively little overland travel from the Gila, one of the Colorado's major tributaries crosscutting southern Arizona and New Mexico, for example, people would have been able to reach the south- and eastward-following basin of the Rio Grande and from there the Brazos, Pecos, Canadian, and Red rivers, as well as the Gulf of Mexico. From the Upper Colorado it is likewise a comparatively brief distance overland to the Arkansas, Platte, and Missouri systems, which would have also funneled people into eastern North America. Once people reached the Gulf of Mexico, they may have also moved along the coast, another route to various parts of the east. If this "Baja" or "Baja/Colorado River" model is viable, we would expect that some of the earliest populations in the interior of North America—save perhaps for areas in the Pacific Northwest and Alta and Baja California—would have been in places within or easily reached from the Colorado River Basin. The vicinity of the Debra L. Friedkin site in Texas, with its recently announced pre-Clovis horizons, is one such possibility (e.g., Waters et al. 2011). We must thus consider the possibility that eastern North America, with its remarkable fluted point tradition, could have been initially settled from the Southwest.

Acknowledgments

Joe Gingerich deserves the thanks of all Paleoindian researchers, first, for organizing the diverse and information-laden 2010 Society for American Archaeology session that this volume is based upon and for the even more herculean effort of assembling and editing this volume. Producing it is a real service to the Paleoindian research community in eastern North America, whose future, the chapters herein demonstrate, is in excellent hands. Joe also deserves my personal thanks for his patience in waiting for my comments, which were improved through discussions with Joe Gingerich, Al Goodyear, Stuart Fiedel, Shane Miller, Dan F. Morse, Doug Sain, Ashley Smallwood, and Stephen Yerka. Stephen also prepared Figures 16.1 and 16.2, and I am grateful for his expertise with large geospatial databases. My thoughts on Paleoindian archaeology have also been shaped for many years by interaction with the many avocational and professional colleagues who record and share their data in Paleoindian Database of the Americas and their thoughts on Paleoindian archaeology in the Americas. Finally, I knew and had the privilege of spending a lot of time with Bill Gardner and several of his Thunderbird site colleagues when he worked in the Richard B. Russell Reservoir some 35 years ago (e.g., Anderson and Joseph 1988; Gardner 1984), and I appreciated his love of archaeology, his emphasis on multidisciplinary research, especially the need to work with geomorphologists, and, as Kurt Carr noted in his presentation at the symposium that led to this volume, his love of a good party with colleagues.

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