People & Culture in Ice Age Americas

New Dimensions in Paleoamerican Archaeology

Edited by
Rafael Suárez and
Ciprian F. Ardelean

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PREFACE

An Upside-Down View

Rafael Suárez and Ciprian F. Ardelean

It is hard to identify another topic in world archaeology still as hot, controversial, mysterious, shifting, and continuously conflictive as the Ice Age archaeology of Americas. For decades, passions have surged, egos have clashed, academic politics have boiled, and paradigms have risen and changed. Now, almost a century since the initial discoveries that began to challenge the thick ice of preconceptions, we are living in a new era of exciting finds that show us that archaeological knowledge is never definitive.

America's two hemispheres have lived these experiences in separate manners and from relatively divergent positions. To the north, the more homogenous Anglo world (principally, the United States) was long haunted by the conservative theories of single-route recent human arrival on the continent. Scholars developed a culture of caution and skepticism around the strongholds of tough paradigms such as Clovis-first. To the south, the more rebel and eclectic Latin world traditionally stood apart from the northern postures and felt freer to sustain out-of-the-box ideas, often constructed upon expedient conjectures, and frequently cemented by their own regional paradigms. Between the two, dialogue and constructive communication were not the rule, and the creation of models upon the particular archaeological records of the North and the South manifested as parallel, rarely compatible interpretations of the past. The causes behind such a geocultural dichotomy were diverse and many, including tense historical backgrounds, meta-academic and transcontinental political stress, self-taught para-xenophobic and nationalistic postures, and generational attitudes.

Yet the ambience has changed considerably. International collaborations are more frequent and normal, the interchange of data has become easier and is actively promoted, while the national academic barriers within the Americas have turned less cold and more permeable due to a change in the mentality of the old and the internationalizing postures of the young. Nevertheless, we should ask ourselves at this point: is the scientific environment in today's American Pleistocene archaeology really more collaborative and communicative than before, beyond the mere organization of meetings and symposia and the publication of contributed volumes? Do we display true dialogue or only politically correct alternating monologues?

This book is a cautious attempt to test for potential answers to these questions. It is part of a project meant to generate our own environment of discussion in which researchers from different countries may find a place to present and debate a variety of topics related to the early peopling of America. This process started in 2012 with a symposium on lithic technology, chaired by César Méndez and Kurt Rademaker, at the 77th Annual Meeting of the Society for American Archaeology in Memphis, Tennessee,

which culminated in the publication of volume 47, no. 1, of *Chungara: Revista Chilena de Antropología*. Later, in 2014, we organized the symposium "Early Human Occupation during the Ice Age in Americas: New Directions and Advances" at the SAA's 79th Annual Meeting in Austin, Texas, and invited a diversity of contributors from different countries and academic backgrounds as means to obtain the pulse of modern Pleistocene–Early Holocene archaeology. The book you are holding now is the result of that symposium.

Even more recently, in the year 2016, Rafael Suárez and Cesar Méndez organized another symposium, "Mobility and Use of Space in Late Pleistocene South America: Is It Possible to Discuss Early Human Regionalization Ranking?" at the 81st Annual SAA Meeting in Orlando, Florida, seeking to provide a proper space for looking more deeply into the issues of mobility, use of space, and regional differences during the peopling of South America (since published as a special volume of *Quaternary Journal*, 473 Part B, 2018). We are hopeful that in the future new challenges may lead to more concrete and solid collaborations and spaces for discussion.

The present volume of contributions looks at areas where new knowledge is being produced. Of course, many relevant regions and a bounty of crucial specific topics could not be represented, so the content may not satisfy everybody. Like most books based on previously held symposia, this one cannot offer an absolutely thorough coverage of geographical zones and topics. Rather, it depends on the diversity of subjects presented by the people who actually attended the meeting in Austin a few years ago. In our consideration, that participation was important and the spectrum of themes diverse enough, covering most of the significant geographical areas across the hemisphere. However, a few colleagues preferred not to be part of this volume; others withdrew their papers in the early stages of the project. To compensate, we invited other contributors who had not been present in Austin. The apparent lack of "geographical equilibrium" is simply the natural result of those dynamics.

Our book seeks a balance between papers coming from South and North America, leaving evident gaps for Central American regions where new studies are surely about to present new and interesting results. The cover of this volume itself and the arrangement of chapters "backwards," from south to north, were inspired by the work of Joaquín Torres García, an Uruguayan constructivist painter, who drew South America upside down. In this way, we express the need for rethinking and reevaluating our views and paradigms about the last expansion and colonization process undertaken by ancient *Homo sapiens*.

There are several terminological and conceptual ambiguities that require attention if we want to go ahead with a much better and fruitful transcontinental academic communication. Among such words and syntagms that (in English and Spanish alike) lack a properly established significance while allowing a rather promiscuous employment are: "*peopling* of America," "human *occupation*," "*first*" (people, Americans), and "*early*" (early occupation, early people, etc.). What do they mean in the context of American prehistoric archaeology and under the light of evidently shifting paradigms? Because if used indiscriminately in relationship to the arduous problem of the original human presence on the continents, they may reach very different meanings and communicate unsustained realities.

Let us start with the concept of "peopling." The term is widely used in the kind of studies alluded to in this book, freely employed in reference to any sort of "Paleoindian" archaeological records radiocarbon dated to a chronological interval of pristine human presence. But not every trace of cultural activity indicates a peopling process, properly speaking. The human peopling of a territory means a complexity of sociocultural dynamics, a certain degree of permanence, and a gradual occupation and involvement with the landscape, followed by a successful demography and a legacy passed on to subsequent generations. It is hard to tell whether the discovery of an archaeological context showing early human presence speaks of a real peopling of that terri-

tory, region, or continent. We believe that we have not achieved yet a sufficient degree of refinement in our studies to be able to discern, in every case, between actual colonization of a territory and ephemeral contexts of cultural presence. Peopling a segment of land as pioneers means that one remains there for a considerable amount of time, leaves descendants, and passes culture along.

Of course, for nomadic hunter-gatherers, the absolute values of such variables would necessarily be different from those expected for more sedentary societies. However, the peopling of a geographical extension of any size is a process that must involve the assumption of success and stability, positive demographic statistics, and certain regularity of contact between people and the "peopled" landscapes. Transitory populations, or the ephemeral presence of a small exploratory band at a specific location, does not mean peopling. Thus, we should be careful not to use this term as a convenient and expedient cover. We humans went to the moon several times, but that does not mean we were peopling or colonizing it. Such is the case with the "early peopling of America."

Certainly, many of the archaeological finds we read and write about—especially those accumulating in the last decades after a veritable boom of "older-than-Clovis" discoveries—do not yet reflect an actual peopling or settling process. They may be remnants of failed colonizing episodes that left no long-standing legacy, of populations in transit heading to far more distant places, or just meteoric instants in the life of small exploratory groups that passed by just once, only to disappear into the shadows. But it is clear that when viewed from our present perspective and at a very low resolution, the great puzzle we try to articulate from an assortment of disconnected discoveries across the hemisphere is the pale reflection of a long-lasting and diffuse colonization/peopling process that eventually left descendants and a large diversity of human societies by the beginning of the Holocene.

Clovis and Folsom cultures, spreading across their territories and sharing a complex culture over a vast geography, definitely were signs of a successful peopling. But what about humans before them? When can we actually start talking about a true peopling and not mere isolated incursions? Today, all over the western hemisphere, we are facing an increasing wave of archaeological sites whose scientifically obtained ages challenge the new conservative visions and even the liberal ones. Sites giving strange archaeological signals display "dangerously" old dates. Before they become accepted and are integrated in the paradigms of future decades, we can ask ourselves, somewhat rhetorically: if people arrived in America a really long time before Clovis, were they *peopling* this new world? Did their presence last long enough and over wide enough space to be considered proper peopling? Or did they just step on the continent and vanish without trace after a few years, decades, or generations? How many failed entries were there before the archaeological record began to keep track of them? It thus becomes clear that the semantic relevance and the ontological coverage under the term "peopling" now increase in importance, as the antiquity of "pre-Clovis" and "pre-Fishtail" populations recedes farther and farther in time.

The same discussion is valid for the concept of "occupation." We are all familiar with books and articles whose titles speak of human occupations during the Pleistocene and Early Holocene. Occupation actually means that people *occupied* an area: they moved over specific territories with certain regularity and exploited the natural resources in a deeper way than with mere transit or exploratory visit. So, speaking of an actual human occupation implies that the archaeological record entitles us to confirm a recurrence in the involvement of a determined human group with the landscape on a larger chronological and spatial scale. A hearth and a few tools in a rockshelter, do they tell us about an actual occupation by humans who were peopling a region, or are they only ephemeral signals of a small band that moved through, never looking back? Overcoming the biases in our field explorations, developing systematic and sustained regional explorations, and investing in refined absolute dating programs are a few ways to define the

difference between true human occupations and simple human *presence* at a specific locality.

To summarize the discussion so far, there are three levels of early cultural visibility in the Pleistocene archaeological record:

a) "presence": this is the most acceptable and broadest of the terms we can use, especially when we are unsure of the chronological depth and spatial extension of the indicators;
b) "occupation": when people interacted with a territory more than once; when recurrence and permanence are comparable in more than one site and for longer than a single moment; then we can speak of an actual occupation of an area beyond a mere incidental visit;
c) "peopling": when human groups extended and lasted over wider territories and passed their culture on to subsequent generations that continued to live in the same regions and expanded outward.

We should be able to define which of these categories best suit our archaeological indicators in order to communicate an adequate message to our colleagues and the public.

It is also very common to say that we study the "first" people on the continent, the "first inhabitants," or the "first Americans." Again, what does "first" mean today? Two or three decades ago, the valid paradigm of the moment gave the "first inhabitants" a face and a name. For a long time, the Clovis culture's title was almost unchallenged by emerging "heretical" discoveries within and outside the United States. But today the situation has changed radically, and the 13,500 cal BP barrier has long been broken, both on the field and in the minds of scholars. Time's doors opened on an immensity of possibilities, and America's prehistory will soon be rewritten. So, who are "The First" now? For none of us can deny that The Site bearing the oldest human presence in the hemisphere still represents an absolute Holy Grail in our hearts and dreams. And first means first—not the second, not the third. Clovis people, although forever retaining the special aura of those who thrived over a vast diversity of landscapes, are not the first anymore, and for most archeologists working on this topic in Latin America they never were. Under these circumstances, perhaps we should stop talking about the "first Americans" for a while—at least until the whirling waters of this new epoch of exploration settles down and brings us a new apparent "truth."

But then, if the first are no longer the first, are the early sites early? The archaeological semantics of "early" and "old" vary from continent to continent, from region to region, and from topic to topic. What does "old" mean today in American prehistory? How old should an archaeological find be in order to be "old enough" to be accepted as a valid discovery by the defenders of one or another of the modern paradigms?

"Old" and "early" are related but not entirely coincident conceptualizations, for both bear significant burdens of relativity. In the archaeological vocabulary, the lexical distinction between the two is easier to note in English than in Spanish. "Early" should be applied to those cultural contexts that belong to the Pleistocene and Early Holocene in general. Ergo, "early" means older than the terminus point of the Early Holocene, according to the chronological scheme in use—let us say, older than 8,200 cal BP. This way, we can at least defend our professional ego and distinguish ourselves from those doing a "later archaeology." On the other hand, "old" seems to be more like a Golden Apple of Discord, feeding the disputes within our own elitist guild.

In the newly fashioned behavior inside the "Paleoindian academia"—conveniently resettled on the fundaments of a wide and politically corrected acceptance of "pre-Clovis" or "older-than-Clovis"—the absolute age of the site matters more than an outsider could possibly imagine. If a few decades ago it was inconceivable to speak of pre-Clovis radiocarbon dates and remain accepted by the highly paradigmatic cloud of peers, the situation has not changed much. It has only adapted and moved a little bit back along the continuum of the "acceptable" range of dates. Today, it seems that a pre-Clovis or pre-Fishtail date must fall within a decent range of "pre-Clovisness" in order to be mentioned, let alone accepted. If "too old" (the

absolute values of reference varying themselves according to the personal taste of detractors), it definitely causes problems and faces similar aggressions from the naysayers. The gravitational attraction of the "Clovis milestone" produces the same powerful effect on the new wave of nonconventional discoveries as it did decades ago, just in a different direction. From the point of view of the open and relaxed scientific attitudes we should adopt in the twenty-first century, *that* is a harmful situation.

These first pages are not quite the usual foreword. The reader should not expect a summary of the included papers. The last chapter, written by Tom Dillehay himself—one of the most noted celebrities in archaeology today—fulfills precisely that function, from a wiser and much more professional perspective. But there are some technical aspects that do need attention, especially the abbreviations we employ to refer to absolute dating. As it has become evident in the previous pages, this book uses the form "RCYBP" to refer to "radiocarbon or ^{14}C years before present," the age measurements as received from radiocarbon laboratories. The calibration of the dates into calendar years is expressed as cal BP, per the SAA/LAA. We have tried to maintain continuity across the volume, speaking in calibrated years whenever possible, to facilitate easy comparison between regions. The dates obtained by other methods, such as luminescence of sediment grains (OSL) are given as "ka" (kilo annum, or thousands of years before present).

So, does this book pretend to bring new, revolutionary, and yet acceptable evidence to the eager reader? What is "acceptable evidence," after all? Can we ask such a question today from an objective position? That is very hard to answer and even harder to adapt to our times, if we want to avoid another epoch of academic police and Procrustean beds for other peoples' work. Scientific ethics, objectivity, and good data control during fieldwork and in the lab are what we need in our daily work and when we seek to evaluate the relevance of the discoveries emerging around us. Most authors in this volume address these issues from their own theoretical stances and in relationship to their particular geographies and historiographies. But as simple human beings or ambitious scientists, we cannot do more than our own science empowers us to do.

This volume is not a new gospel for a revolutionary movement. In fact, the reader may find it rather "decent" and "unharmful." What, then, should readers expect? Certain diversity, before all. A diversity of approaches and a fairly large geographical coverage spanning the entire Western Hemisphere. It is not something new in itself but something that should become noted in American Pleistocene archaeology. In fact, this volume pretends to change—at least a little bit and at least for a moment—the predominant north-centric view in Pleistocene–Early Holocene archaeological studies and to invert that polarity for the sake of equilibrium.

Fascinating stuff is being done right now in Latin America, and many maps drawn in the North continue to leave our regions blank. The new chronological revolution in Ice Age archaeology is accompanied by an evident geographical boost of discoveries rising from southern latitudes. And there will always be space for surprises.

CHAPTER 7

Mexican Prehistory and Chiquihuite Cave (Northern Zacatecas)

Studying Pleistocene Human Occupation as an Exercise of Skepticism

Ciprian F. Ardelean, Joaquin Arroyo-Cabrales, Jean-Luc Schwenninger, Juan I. Macías-Quintero, Jennifer Watling, and Mónica G. Ponce-González

Although the current panorama of studies on the earliest human presence in the Americas has shifted, and the existence of older-than-Clovis cultures is better accepted than only a few years ago, the subject must still be considered from a position of carefulness and self-criticism. Within this epistemological order, doing research on early prehistory and the first human occupations in Mexico has become synonymous with a sustained and necessary exercise of skepticism and caution.

Official Mexican paradigms have established, among scientists and the public alike, the habit of accepting fairly easily any early date or arguments in favor of an extremely old human presence in the present country. Tens of thousands of years of cultural manifestations have been traditionally handled with ease in the Mexican specialized literature and displayed in the nation's museums. Hopefully, archaeology in the Americas will one day present undeniable empirical evidence of really old, pristine peopling of the continent, and Mexico certainly will not be the exception. But, until then, scientific argumentation and dogmatic speculation are two different coins altogether.

The charisma of prestigious personalities in Mexican archaeology seems to have always carried more weight than strong argumentation built upon solid scientific discoveries, replicated results, and thorough analyses (see critique in Ardelean 2013; Ardelean and Macías-Quintero 2016). The popular image has generally been the same for the last five decades or so: humans arrived in Mexico more than 30,000 years ago, after a migration from Asia that must have occurred even earlier. Iconic sites, strongly embedded in the local archaeological vocabulary, were meant to offer undeniable evidence for something that, anywhere else in the Americas, continues to be an academic bloodbath: the when, the where, the who, and the how of the earliest human arrivals. In the archaeology of the United States, for example, the topic of the "early peopling of the Americas" has been a primordial theme and discoveries have been continuously subject to criticism, scrutiny, and constant revision. In Mexico the situation has been different. Whereas north of the Rio Grande purported ancient sites struggled for decades for recognition as valid "pre-Clovis" occupations, the Mexican early sites gained acceptance quickly among local archaeologists and the general public (Figure 7.1). Often, a couple of unreplicated radiocarbon dates obtained decades earlier, plus the name of the person in charge of the discovery, used to be the equivalent of compelling evidence for a very old human presence.

FIGURE 7.1. Political map of Mexico showing the location of presumably ancient sites commonly found in the specialized literature, as well as the location of the study area and cave discussed in this chapter. Not every site in the map is mentioned in the text. They are included to demonstrate how prolific the Late Pleistocene–Early Holocene human occupation is thought to be in traditional views in Mexico.

Although today the situation is changing at an accelerated pace and a wave of Mexican prehistoric investigations conducted by a new generation of scholars seeks to join the continental debates (see Chapter 6, this volume), half a century of paradigmatic domination has caused lingering damage. Criticism was virtually absent for a long time, criteria weakened, skepticism was rarely practiced, and even now the extremely old dates still gain easy general acceptance. So, how did we get to this situation, and how is that we still struggle with it?

There are several primary factors. First, early prehistory (meaning here Late Pleistocene human presence) has never been a privileged topic in Mexican archaeology. Study of the earliest human presence during the Ice Age received much less attention than the later, "greater civilizations" with monumental architecture. The topic remains underdeveloped—empirically and epistemologically—and is underrepresented in publications and the number of specialists or institutions committed to the subject. Whether humans reached Mexico 10,000 or 50,000 years ago does not seem to merit general concern nor is it considered worth approaching critically.

Second, the national egos and historic rivalry between Mexico and the United States have long suffocated academic dialogue between the two countries and contributed to the paradigmatic divergences.

Third, the force of influential personalities weighed heavily on academia, scholars accepting their writings without much—if any—criticism and skepticism. Such a tendency is reversing now, but dissolving cemented paradigms is a work in progress.

As a fourth identifiable factor, the field of prehistoric archaeology in Mexico seems much too comfortable with shallow data. If, elsewhere, one observes an exaggerated inclination toward rejecting any archaeological context that looks too old for still-rigid paradigms, in Mexico the case seems to be just the opposite. If one discovers a

new site that yields even a single date, let's say, older than 20,000 radiocarbon years, no matter how suspect the context of provenance, one may well find it greeted enthusiastically by colleagues and the public.

Back in 1967, José Luis Lorenzo (former patriarch of Mexican prehistory) published a small brochure titled *La etapa lítica en México* (*The Lithic Stage in Mexico*), which soon became the keystone of the official paradigm. Lorenzo proposed the first chronological model for the Mexican prehistory. Strangely, this particularistic, anachronistic, and outdated work remains in use among many scholars, despite its evident lack of empirical support—a sort of textbook that was worshiped and rarely—if ever—criticized (Ardelean and Macías-Quintero 2016). Lorenzo established the basic fundamentals of the national dogma: the first people arrived in Mexico many thousands of years ago, a statement to be taken as fact ("Lorenzo said it all" is a popular mantra), no matter the absence of strong, reliable indicators in the archaeological record or whether it noted or ignored global and continental debates.

In the middle of this delicate panorama, what would happen if you discovered a new site that yielded some old-looking artifacts or a couple of really old-looking dates? Three scenarios might apply: (a) many colleagues in Mexico, prehistorians or not, encourage you to present your site as new evidence for really old human occupation, as the discovery would fit gently within the local paradigm; (b) most foreign colleagues (especially from the United States) attack your arguments, question your methodology, criticize your context, and request infinite replications of dates and thorough analyses of every possible detail in your data; (c) you choose the middle path of self-criticism, confronting the discovery yourself, adopting a fallibilistic epistemology, and embracing skepticism (but not a priori denial) before enthusiasm and caution before premature belief. In this third scenario, intriguing finds and controversial radiocarbon or OSL (optically stimulated luminescence) dates should not be dismissed by default, nor should they turn into dogmas at once. They rather serve for proposing working hypotheses, for attracting attention to *potentially ancient* sites, for stimulating further testing, further digging, and more detailed exploration.

This chapter does not have enough space for a comprehensive assessment of the current archaeological data and currents in Mexican prehistory. However, a brief review of Lorenzo's model and the available archaeological evidence is merited. This review, forming the first half of the chapter, expresses the opinion of the first author and is based on critiques developed elsewhere (Ardelean 2013, 2014, 2016; Ardelean and Macías-Quintero 2016). The second half of the chapter brings into discussion the preliminary results of recent archaeological explorations in the central-northern state of Zacatecas, where at least one archaeological site yielded data that makes it a good candidate for an exercise in academic skepticism and precaution.

Lorenzo's Model

Mexican prehistory has long been predicated on the basis of an assumed chronological scheme that has turned indestructible. The traditional cultural-historical model launched by Lorenzo (1967) specifically for Mexico is both simple and surprising. First, it includes a long period known as the *Arqueolítico* (*Archaeolithic*), starting with the earliest inhabitants, perhaps 40,000 years ago, and ending with the appearance of the first projectile points. It is followed by a period characterized by finer flaked-stone technologies, named the *Cenolitico* (*Cenolithic*), divided in two phases, Upper and Lower. It presumably ends at the beginning of the Holocene and is followed by the so-called "Proto-Neolithic," posited at the dawn of the sedentary life. Together, the three periods form the *Lithic Stage* (*La Etapa Lítica*), the Mexican particularistic and isolationist chronology that displays its own terminology and chronological spans without sustainable correspondence elsewhere.

The name of the model was justified by the fact that flaked stone represents the dominant surviving artifact from those periods and the main available data. Lithic technologies and the distinct employment of artifacts over time

form the differential criteria used to separate the horizons that integrate the model (Lorenzo 1967:27). The most striking attribute of this cultural chronology is its extremely ancient starting point and also the oscillating date for its end. With the Archaeolithic in particular, it is intriguing how it addresses astonishing dates ranging far beyond anything accepted scientifically in the Americas without providing reliable archaeological data to support the claims—especially if one remembers that the model was proposed *before* any true older-than-Clovis site was excavated. The supposed date for the earliest occupation was originally set at "only" 25,000 years ago (Lorenzo 1967:28), but it soon reached 30,000–40,000 years in Lorenzo's and his supporters' subsequent publications. The shifts between these multiple options seem random to the reader and are never accompanied by justifications (see Mirambell 2000:224). More recent publications continue to perpetuate Lorenzo's unsustained paradigm, long after his death, without contributing new arguments to the model (Mirambell 2000, 2001; García-Bárcena 2001; Lorenzo and Mirambell 2005; González-González et al. 2006).

Lorenzo wrote that he had taken the term Archaeolithic from Jacques de Morgan (1947:79–80) in reference to the "archaeolithic industries of Europe" and the Upper Paleolithic. The term did not find much acceptance in the Old World but was considered appropriate for a first phase of cultural presence in Mexico (Lorenzo 1967:27). According to the author, the Archaeolithic was characterized by crude and simple stone artifacts, direct percussion flaking, exclusively employing stone hammers. Large items displaying incipient bifacial techniques exist, and large flakes were common together with a variety of scrapers, choppers, chopping tools, and retouched denticulate tools, but specialization was minimal. Lithic typology was extremely reduced, the once-valid "Clactonian" technique could be recognized in various artifacts (*sic*!) and stone projectile points were missing, along with grinding stone implements. The main subsistence practice was hunting, and no direct indicators of gathering can be found in the archaeological record, although these may have perished. The same occurred with spear points and other artifacts of organic materials (Lorenzo 1967:28; Lorenzo and Mirambell 2005:483; Mirambell 1988:315, 2001:47).

How such an attribute list came to life never became clear, as it is clearly unfair to the diverse inventory of Paleolithic life (cf. Adovasio 2015). However, it is understandable that the proposal somehow integrated the tendency of its time when other models insisted on a supposed preprojectile point phase (cf. Rice 2015). Lorena Mirambell, who coauthored Lorenzo's publications for many years, *assumes* that those first colonizers came from Asia and affirms that "it is sure" that their material culture resembled lithic industries from—surprisingly—places such as northeastern Pakistan, Japan (35,000-year-old materials), and Superior Cave at Zhoukoudian (Mirambell 1988:316).

When he first defined the Archaeolithic, Lorenzo based it on a brief list of allegedly ancient sites: the Diablo Complex in the Tamaulipas caves excavated by Richard MacNeish (1958), elongated bifaces on the shores of the Chapala Lake, the Teopisca industry in Chihuahua, and Chimalacatlán Cave in the state of Morelos. These confusing examples did not supply reliable radiocarbon dating or technological studies and cannot (either then or now) be considered viable examples. Nevertheless, Lorenzo showed much more skepticism than his followers, refusing to involve controversial examples such as Tequixquiac and Valsequillo (Lorenzo 1967:30; cf. Ardelean 2013). Later, Mirambell (2000:236) included new finds from other sites, but again the actual evidence or dates were never discussed. García-Bárcena (2001:29) added even more examples to this phase, as the list seemed to be open to anyone willing to contribute new "pre-Clovis" sites—literally out of thin air. Accepting the supposed human-made hearths and other finds at Tlapacoya and Cedral as having Archaeolithic antiquity (Figure 7.1), García-Bárcena included the male skulls from Chimalhuacán and Balderas Subway, giving them ages of 33,000 and 17,000 years, respectively. Bryan and Gruhn (1989:91) agreed with the chronological scheme,

considering that the Archaeolithic "was divisible into a lower substage without bifaces and an upper substage after the innovation of bifacial flaking". Others (Serrano and Nuñez 2011:186) perpetuated the paradigm, without critique, assuming the existence of sufficient "evidence" to support the validity of the Archaeolithic phase: open camps, caves, human remains, lithic materials. The certainty is intriguing, considering that the entire archaeological community in the Americas is continuously searching for a minimum of secure evidence for an older-than-Clovis occupation (Stanford et al. 2015).

After the Archaeolithic comes the Cenolithic. The time range comprised in this chronological period is between 14,000 and 7,000 years ago (Faulhaber 2000; Serrano and Nuñez 2011; Mirambell 2000), although Lorenzo had established its commencement at only 12,000 BP (1967:30–31). As was the custom for decades, the authors never specified whether "BP" referred to calendar (calibrated) or radiocarbon years, nor the criteria for delimiting the period's beginning and end.

For our discussion, only the Lower Cenolithic is relevant. Its chronology is confusing and arbitrary, as expected. This phase is said to last from 14/12,000 BP until 9,000 BP (Mirambell 1988; Lorenzo and Mirambell 2005). The creator of this cultural chronology pushed it up to 7,000 BP, leaving no space for an Upper Cenolithic (Lorenzo 1967:30). No genetic relationship is assumed between the two phases. As a matter of fact, one can take this last assumption into account as a valid working hypothesis that Lorenzo might have anticipated correctly, especially if we look at how different the already known older-than-Clovis industries are from Clovis and later ones.

Supposedly, the characteristic traits of the Lower Cenolithic interval are the following: the appearance of flaked stone projectile points; fluted and lanceolate points; bifaces with pressure retouch; flaking techniques using soft hammers; blade technology and prismatic pressure blades became more common; indirect percussion started to be used; grinding present on bases and corners of bifaces; stemmed points appear, without barbs and the stem may be fluted (such as "Fishtails"); and no clear evidence of grinding stones yet available (Lorenzo 1967; Mirambell 1988:316; Lorenzo and Mirambell 2005:483–484). Clovis and Folsom technologies are implicitly combined in this phase. For subsistence, people relied heavily on hunting, but Lorenzo completely rejected any sort of exclusive preference for big mammals, such as mammoths, which is another aspect that he may have understood earlier than others.

While one might be concerned about a scarcity of well-supported sites showing very early human presence in North America, Lorenzo and his followers did not seem to have that problem. The list of sites used as examples for the Lower Cenolithic is shockingly long. The 14 enumerated sites include San Joaquín (State of Baja California Sur), Guaymas (Sonora), La Mota Samalayucan (Chihuahua), La Chuparrosa (Coahuila), Puntita Negra (Nuevo León), Weicker Ranch (Durango), Cueva del Diablo (Tamaulipas), and Hueyatlaco-Valsequillo (Puebla; some shown in Figure 7.1). Some sites did indeed produce fluted points but only as surface finds. But since the publication of *Etapa Lítica* half a century ago, those localities were not secured as old sites and cannot stand today as arguments for early occupations.

Lorenzo's list could be considered appropriate for his intentions to justify the existence of a phase contemporary with Clovis, Folsom, and Plainview traditions. Nevertheless, three decades later, Mirambell exaggerated the controversy and amplified the list to 33 Lower Cenolithic sites (Mirambell 2000:244). Only 10 coincide with Lorenzo's list, the rest being included unexplainably without a minimal discussion of arguments or dates and without any specification of the evidence available for these newly added sites. Many never appear in the literature again. Why were they assumed to be that old, then? Their deployment as examples to define a chronological period is speculative and unjustified, but their effect on the perpetuation of the paradigm has been immense. As Suárez (2015) has recently written, the premature and speculative claim of ancient human presence in

the Americas is harmful to academic practice in American prehistory.

Given this brief review, there is still no reliable chronological model for the prehistory of Mexico. The traditional scheme created by Lorenzo—blindly and uncritically promoted for decades by generations of archaeologists—can no longer be considered of utility. When *Etapa Lítica* was published in 1967, very little data existed about Pleistocene cultural and environmental realities. Even the most important sites, invoked as prime candidates for very old ages (Tlapacoya and El Cedral), were excavated years later. So, any possible candidates to justify an Archaeolithic stage (with its profound time depth) played no role in the initial definition of the scheme, as they were unknown at that time. When these sites finally entered the corpus of data and more and more information came from other investigations, the chronological scheme never adapted to the development of the archaeological knowledge. It simply turned dogmatic.

The long list of sites used as a backup for the legitimacy of the "Lorenzian" model does not represent a realistic image. Their inclusion is merely speculative, especially in the case of the Archaeolithic phase. The historical "guilt" of a 50-year-old publication would have been expiated had there been any significant revisions of its content in the intervening period. Yet, there are still no specialized studies published on most of the above-mentioned sites. Instead, the majority appear primarily in the general literature, mostly for surface finds with no reliable radiocarbon dates to justify claims of extreme antiquity. The separation between the distinct chronological subdivisions remains arbitrary, more guessed than scientifically founded. There is still not a single word explaining why the Archaeolithic ends at 12,000 or 14,000 BP and what that border means in terms of cultural manifestation, geo-environmental data, or absolute dating. Further, the criterion declared in the mentioned publications (differences in lithic technologies) is unsustained by the existing evidence. First, there is absolutely no scientific proof for the alleged age of the artifacts considered representative of the Archaeolithic. Second, the Lower Cenolithic phase mixes all Paleoamerican lithic traditions in the same box. The transition between the Pleistocene and Holocene is completely effaced, leaving no place for discussions about the relevance of climate changes, subsistence adaptations, and cultural responses for the construction of cultural historical chronologies.

At the moment, there is still no clearly confirmed and solid evidence for a truly older-than-Clovis human presence on Mexican territory. Such a pristine occupation must surely exist. The evidence from the Western Hemisphere entitles us to hope that Mexico is not the exception, but mistakes made by previous archaeological praxis render any evaluation skeptical.

Confronting the Model

The Valsequillo complex in Puebla—once a good candidate for ancient cultural indicators—was the scene of difficult academic and even political confrontation (Armenta 1959, 1978; Irwin-Williams 1967, 1981; Irwin-Williams et al. 1969; Steen-McIntyre 2006; Steen-McIntyre et al. 1981; González et al. 2006a, 2006c; Renne et al. 2005; Morse et al. 2010). Today, with its archaeological localities submerged by the waters behind a dam, the old questions can hardly expect new answers. Tlapacoya, an ancient paleo-beach surrounding a volcanic hill south of Mexico City, was and still is promoted by the official paradigm as a main example of Archaeolithic occupation (Lorenzo and Mirambell 1986, 2005; Mirambell 1973, 1986, 2000, 2001; Caballero 1997). The overview of available data and growing critiques of the 1970s results removed the site from its pole position in the pre-Clovis race (Sánchez 2001; Huddart and González 2006; Acosta 2012). Nor have Tlapacoya finds been critically reassessed since.

Two iconic localities, Santa Isabel Iztapan I and II (Aveleyra and Maldonado-Koerdell 1952, 1953, 1956), now covered by the Mexican capital's urban development, raise doubts on the authenticity of the finds and the cultural and chronological relationship between the flaked-stone artifacts supposedly found with two mammoths (Ardelean 2013; Sánchez 2010).

The artifacts there seem to belong to the Cody complex, but the true cultural affiliation of the finds and the relationship with the proboscidean remains have not been reanalyzed in the past four decades. So, any claims regarding human presence there, or any consideration of its older-than-Clovis age (see Rice 2015), cannot be taken lightly.

El Cedral is a Pleistocene hot spring context in the northwest of the state of San Luis Potosí, close to Zacatecas. It is Lorenzo's and Mirambell's flagship but there has never been a proper presentation of the finds (Lorenzo and Mirambell 1981, 1984, 2005; Mirambell 2001). Nor have the El Cedral stone artifacts and supposed hearths lined with mammoth leg bones ever been reassessed or redated since their alleged discovery in the 1970s. Available radiocarbon dates there have huge standard deviations. Moreover, the finds are not available for independent evaluation. The long-expected publication of the site (Mirambell 2012) left many doubts about the excavated contexts, the actual cultural origin of the hearths, and the relevance of the conventional radiocarbon dates obtained decades ago and unreplicated since.

The few secure Pleistocene occupation sites in Mexico come from Clovis sites with Clovis artifacts, mainly in northwestern Sonora at El Bajío (Robles and Manzano 1972; Montané 1988; Sánchez and Carpenter 2003; Sánchez 2007; Sánchez et al. 2007; Gaines et al. 2009) and El Fin del Mundo sites (Sánchez et al. 2009a, 2009b, 2015; Gaines and Sánchez 2009; Sánchez et al. 2014). There is alleged Clovis presence at Oyapa, in Hidalgo (Cassiano 1992, 1998, 2005; Cassiano and Vázquez 1990), but the surface stone artifacts recovered there await additional evaluations before they can be accepted as conclusive. Acosta and colleagues provide further data on Clovis-like occupations in southern Mexico, as well (see Chapter 6 in this book).

Human skeletal remains in the Basin of Mexico (González et al. 2003, 2006b; Jiménez et al. 2006, 2010), together with recent results from caves in Chiapas (Acosta 2010, 2012; Acosta and Pérez 2012), show a contemporary or slightly younger cultural presence (Figure 7.1). As research on the recent discoveries of human remains on the Caribbean coast of Yucatan advances (González-González et al. 2006, 2008, 2014; Terrazas et al. 2006; Chatters et al. 2014), the archaeological record may well amplify chronological extension beyond the "magical" threshold of 13,500 cal BP. But until the new investigations announce their final conclusions, there is yet little evidence from Mexican prehistory to confirm Lorenzo's model.

Chiquihuite Cave: Old Human Presence or Archaeological Illusion?

Chiquihuite Cave ("Cueva del Chiquihuite", in Spanish), situated on the northeastern border of the state of Zacatecas (Figures 7.1, 7.2), is a newly discovered archaeological site that was explored briefly during two field seasons (2011, 2012), during the doctoral dissertation research of this chapter's first author (Ardelean 2013).[1] Three authors of this chapter (Ardelean, Macías-Quintero, and Ponce-González) participated in the actual exploration of the cave, while the other three (Arroyo, Schwenninger, and Watling) were involved in specialized laboratory analyses. For the two initial campaigns mentioned in this chapter, insufficient funding, time constraints, the small crew, and challenging logistics limited field research in this high-altitude cave to surface exploration, topographic survey, and excavation of only one test pit.

The cave is situated high in the Astillero Mountains, at an altitude of 2740 m a.s.l., near Chiquihuite peak (almost 3,200 m a.s.l. at its highest, one of the highest elevations in the northern half of the country), Concepción del Oro municipality, on the border with the state of Coahuila, along the northeastern edge of an elongated endorheic basin, and in the vicinity of a small village named Guadalupe Garzarón (Figure 7.2). This locality was not considered a promising archaeological site during the initial stages of exploration, and its scientific potential was thought to be primarily for paleoenvironmental reconstruction. Its position at the base of a naked cliff, the steep and unstable rocky slopes, and its altitude at more than one km above the basin's bottom made it seem unsuitable for human habitation. Today, the setting of the cave represents a particular ecological niche repre-

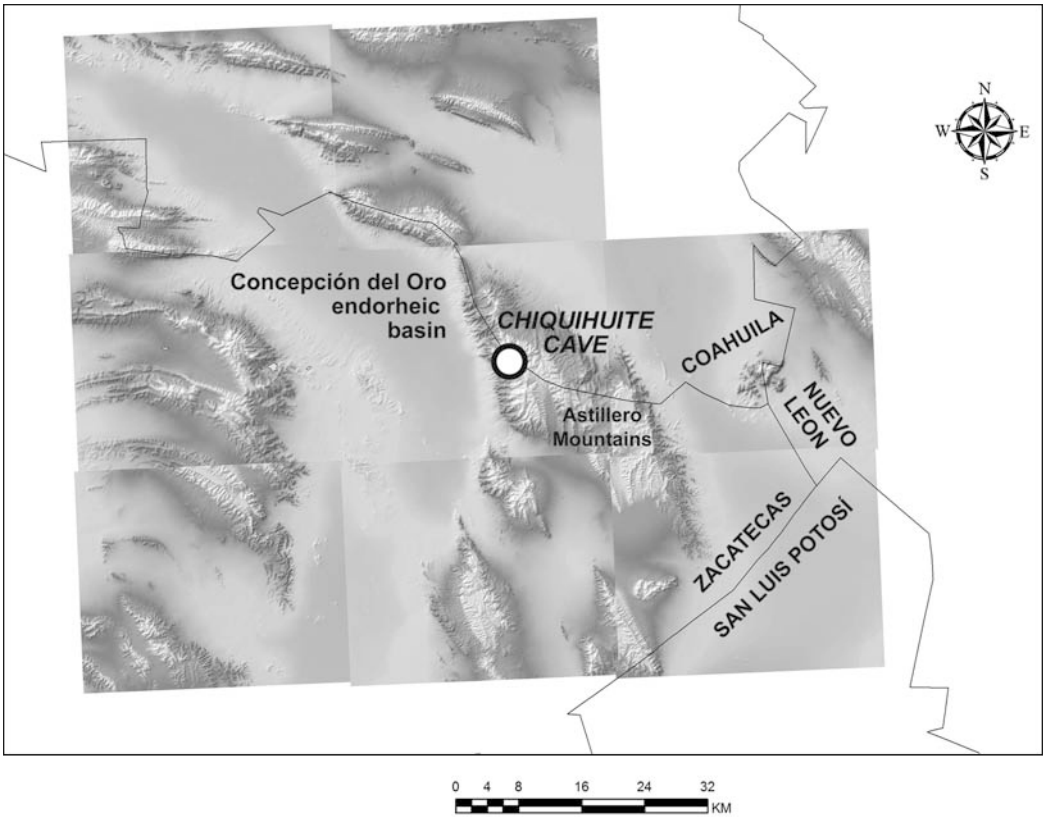

Figure 7.2. Digital elevation model (DEM) of the Concepción del Oro semidesert region in the northeast of the state of Zacatecas, with the Concepción del Oro endorheic basin enclosed between several orographic features. Chiquihuite Cave is on the western escarpments of the Astillero Mountains (DEM elaborated by Juan I. Macías-Quintero; modified from Ardelean 2013:172, Fig. 40).

sented by pine, oak, and juniper, with intrusions of cacti and *Yucca* (Joshua trees), surrounded by a vast semidesert ecosystem (Figure 7.3).

Chiquihuite is a relatively large cave, with at least two ample chambers, heavily modified by continuous natural transformations, including thick deposits of alluvial and colluvial clastic material and massive ceiling collapses (Figures 7.5, 7.6). Being the only cave found in the study area, it was a good candidate for paleoenvironmental studies. However, discovery of an interesting projectile point on the surface near the cave mouth, as well as a small anthropic fireplace exposed underneath large blocks of ceiling debris in the second gallery, suddenly transformed Chiquihuite into a potential archaeological site.

The limestones and intrusions forming Chiquihuite Peak are upright cliffs. They are subject to continuous erosion, showing fractures, brittleness, and cleavages. The cave entrance faces west-southwest, with light entering the front gallery during the afternoon and more abundantly in winter (Figure 7.5). The access path is on a steep and long slope surrounded by vertical stone walls. That slope is completely covered by large, angular, loose boulders and cobbles fallen from the ongoing cliff erosion. Cave access requires at least one to two hours of difficult climbing from the dry creek below.

This continuing transformation of the landscape suggests that at the end of the Pleistocene the area may have looked completely different. The mouth of the cave is now small, reduced to two separate openings (Figures 7.4, 7.5). Originally, the entrance could have been large and wide, possibly about 10 m tall and 25 m wide,

Figure 7.3. The central section of the Astillero Mountains, viewed from the southwest and the basin floor. At 3,200 m a.s.l., Chiquihuite Peak (rocky cliffs in the center of the image) is one of the highest elevations in the Mexican northern highlands. It holds the cave of the same name. To preserve the cave, its exact location is not indicated (photo by C. F. Ardelean, November 2013).

Figure 7.4. The main entrance to Chiquihuite Cave, northeastern Zacatecas. The current access was artificially enlarged in recent, historic times (probably for mining exploration during the Colonial period) piercing cemented sediments that completely obscured the original and much larger entrance (photo by C. F. Ardelean, January 2011; from Ardelean 2013:353, Fig. 212a).

Figure 7.5. View from inside Chiquihuite Cave front gallery toward the current double entry, which faces west. Light enters the dark cave during the afternoon, while Josué Guzmán, Alejandro Arteaga, and Javier Ponce (then undergraduate students at the University of Zacatecas) take a break during excavations. One can appreciate the massive amount of debris filling in the gallery, especially from roof collapses. This picture was taken from the location of the test pit X-11 (photo by C. F. Ardelean, January 2012).

with the cave floor several meters lower than today. Huge amounts of clastic material, originating from the surrounding cliffs, moved into the cave through gravity or were carried in by alluvial events. Thus, the entrance and the front gallery were affected by millennia of debris deposition. In the distant past, the cave may have had a larger, more horizontal platform in front of it, with smoother slopes leading to active creeks and springs below. Their dry remains are visible at the base of the slope.

Current vegetation is mixed. The local, endemic ecosystem is a pine-oak forest. Possibly this was the case by the end of the Pleistocene, as well, although detailed paleoenvironmental data for the site is not yet available. Increasing aridity, episodes of drought in recent centuries, mining activity, deforestation, and goat herding all contributed to the shrinkage of the original biota. Now, desert vegetation is rapidly invading the mountains. Joshua trees, *Larrea* bushes, *agave lechuguilla*, and cacti are found at altitude, even near the entrance of the cave among pinyon pines.

Chiquihuite Cave is situated close to the top of a karst massif of vadose circulation (or of vertical transference). The karst formation was created by differential water erosion at the union between limestones and volcanic intrusive rocks, defining it as an interstratal karst. The cave shows an important speleothem development, with large stalactites and stalagmites, especially in the second, deeper gallery, where exterior elements were less invasive. This large,

FIGURE 7.6. General view of Chiquihuite Cave main gallery, as seen from the entrance. Arrow against the northern wall indicates test pit location (photo by C. F. Ardelean, January 2012).

second gallery—difficult to explore archaeologically because of the total lack of light and abundance of toxic bat guano—presents a truly impressive and complex scene: stalagmites, fistulous stalactites, flag-shaped stalactites, eccentric stalactites, straws, draperies, flowstone, columns, as well as carbonate crystal aggregates known as "moon milk" (Figure 7.13).

The main, frontal gallery contains fewer and smaller speleothems, but the cave is still active and young speleothems are being formed along the northern wall and somewhat to the east and center. There is considerable seeping from the ceiling (mainly during the summer rainy season through capillaries), and the growth of speleothems continues at a relatively accelerated rate. This is definitely not the typical dry cave common to the arid deserts of northern Mexico. Such caves would normally shelter funerary contexts of mummified bodies, as with the more famous Candelaria Cave in Coahuila to the northwest. However, the seeping is localized on specific spots, and speleothem buildup has more recently slowed. Layers of fine dust on the floor and the seasonal presence of a migratory bat colony (likely an insectivorous or pollenivorous species, which inhabits the dark second gallery during spring and summer months) indicate present drier conditions.

The front gallery is a large dome, with roughly circular floor, measuring about 50 × 50 m horizontally and 10–14 m in height between the lowest points and the ceiling (Figures 7.6, 7.7). The eastern part of the ceiling presents a chimney (karst conduit) that seems to connect to upper conducts, as yet unexplored. The thickness of the invasive clastic sediments is suggested by the 14 m of difference in height between the level of the current entrance and the lowest points at the back (east) of the gallery. At several points in the

eastern sector, there are shallow signs of modern anthropic disturbance, such as old looting pits, ephemeral hearths, and garbage. The second, totally dark gallery opens through a vault along the entire southern wall of the first chamber. Our exploration here was cursory. The floor inclines eastward, covered with even larger blocks from roof collapses. Most are very old events, as shown by massive stalagmites formed on top of the debris (Figure 7.13).

Two surface finds provided the first archaeological indicators: a projectile point outside the cave and a human-made combustion feature on the floor of the second gallery.

The elongated biface. While climbing the steep slope in order to explore and map the cave in 2011, we found a biface among the boulders covering the improvised path, about 150 m from the entrance, downslope and westward. The biface is seemingly a projectile point, with its tip missing from an impact fracture (Figure 7.8). A square-angled break on the proximal end suggests bending in the shaft. The color of the piece is opaque white, with dark mineral intrusions. The class of raw material seems to be white-patina silicified limestone or milky chert, possibly the latter. The foliate point was made on a thick flake blank, retaining some high points along the longitudinal ridge on both faces. Consistent, diving flake scars form a central ridge on both sides. Symmetrical and relatively well made, with percussion flaking and pressure retouch (more visible on the distal half), the artifact was probably used, as indicated by the breaks. Additional impact damage, visible on one edge, could have been caused by postdepositional events.

The type and chronology of this artifact are difficult to assess. When compared to point shapes in the Americas, an intriguing possibility appears: its outline is roughly similar to both the "Lermoid" forms of North America and the supposedly older-than-Clovis El Jobo points from South America. Both Lerma and El Jobo are confusing and poorly established types. Rather, they are intuitive taxa based on similarity of shapes, which tend to be related, in some texts, to early occupations of Late Pleistocene–Early Holocene age (Painter and Hranicky 1990; Cruxent and Rouse 1956). Lerma, in fact, is not even a proper type but a fictitious taxon promoted in the literature following MacNeish's work in northeastern Mexico (see discussion about Lerma in Ardelean 2013:100–104, Ardelean and Macías-Quintero 2016:100–101).

Nevertheless, there is another, even more interesting morphological and technological analogy in North American archaeology: the Nebo Hill points from Western Missouri and Kansas (Shippee 1948). Ardelean personally compared the Chiquihuite artifact with such points curated in the Paleoindian collection of the Smithsonian Institution in 2015. The similarities are striking, in both shape and flaking patterns. The chronology of the Nebo Hill points is not clearly established but tends to be placed during the Late Holocene–Late Archaic, toward 3500 RCYBP. Additionally, our surface specimen will always lack a direct dating, so any temporal/cultural implications of the mentioned analogy must remain sterile.

Now, if one were comfortably situated in an uncritical academic environment, one might be tempted to postulate that the Chiquihuite cave biface could be a local variety of Lerma-like or El Jobo-like forms and that it could be a *hypothetical* indicator of early human occupation. But in fact, beyond any cross-cultural comparisons, the only thing the biface shows is that somebody in the past—perhaps an ancient hunter—passed by the cave, possibly entered, and left cultural traces for us to find. That reasoning stimulated efforts to explore the interior.

The fireplace. This feature was crushed under the massive ceiling collapses of the second gallery, exposed on an artificial profile apparently made by people extracting an iron oxide-rich red, silty material, abundant there (Figure 7.9). The position inside the gallery and the well-defined shape of the feature seemingly excluded any possibility that a burning log from an external wildfire could have rolled naturally into the second chamber.

In profile, the possible fireplace has a lenticular, concave shape, suggesting the fire was made within a small, shallow pit. The feature is not

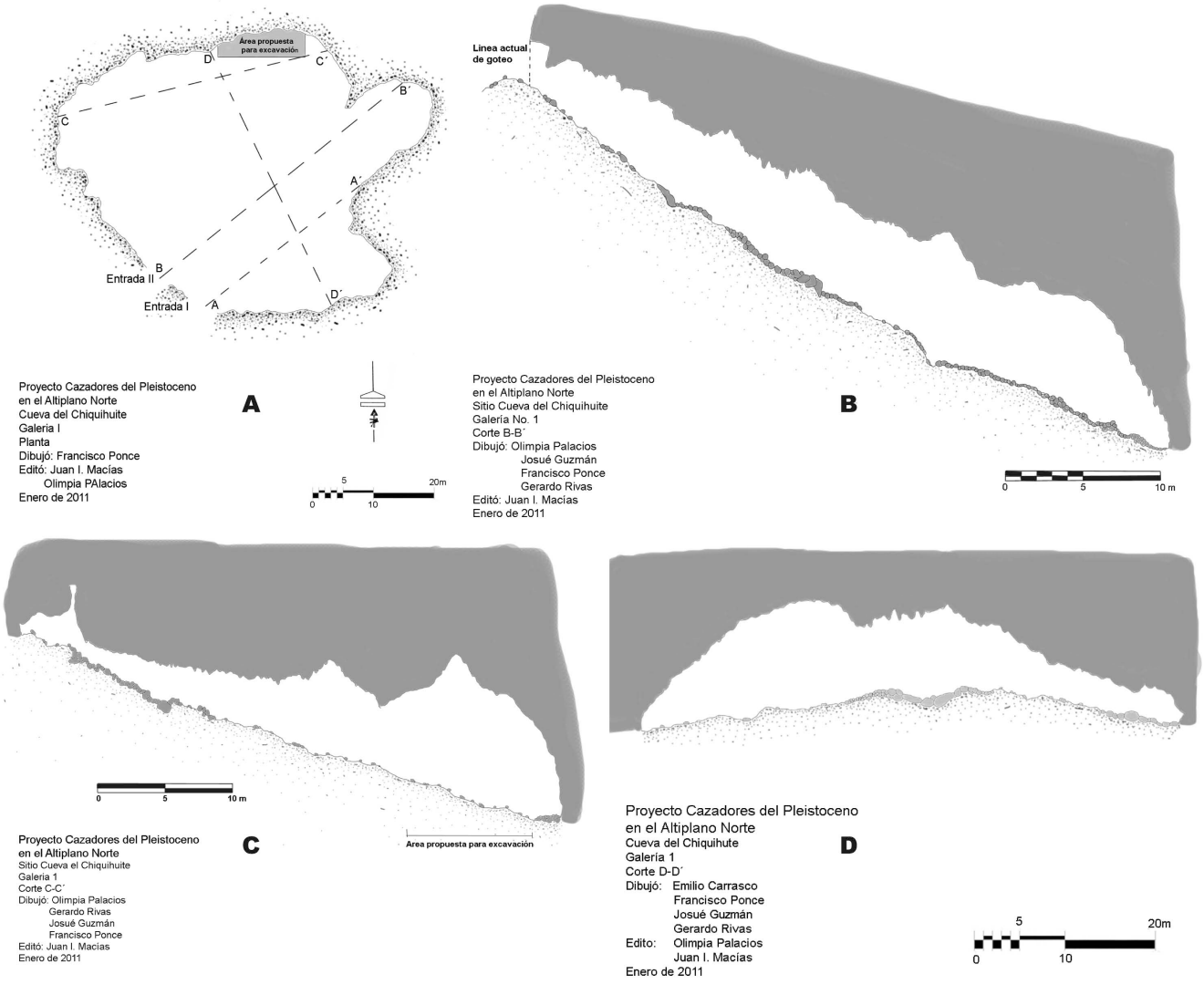

FIGURE 7.7. Chiquihuite Cave floor plan and section drawings of the front, main gallery: (*A*) main chamber floor plan showing location of the excavations next to the northern wall; (*B*) cross-section following the B–B′ trajectory shown on the floor plan; (*C*) cross-section C–C′; (*D*) cross-section D–D′ (modified from Ardelean 2013:357–358, Fig. 215–216).

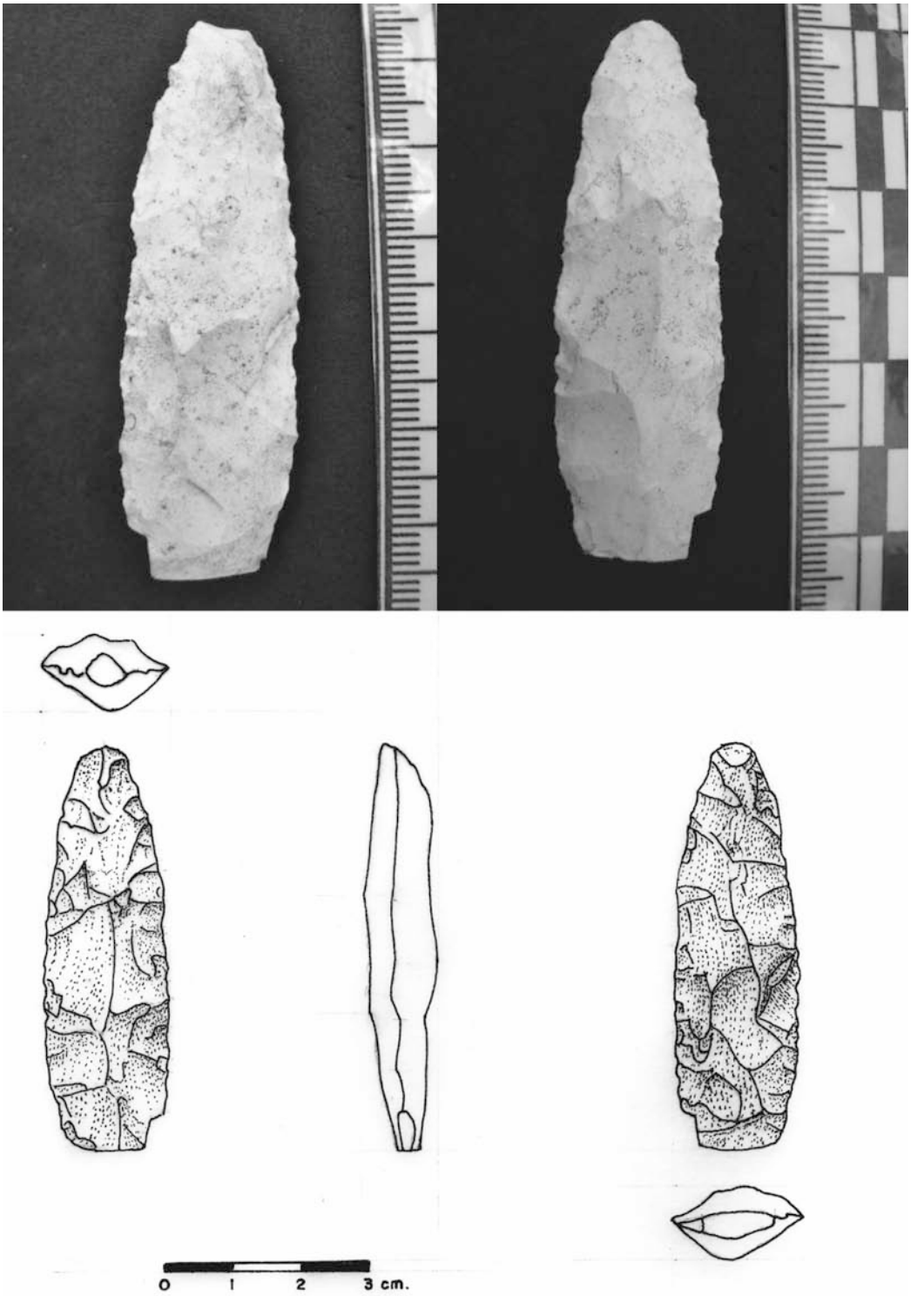

Figure 7.8. Biface (projectile point) found on the surface near the entrance to Chiquihuite Cave in January 2011, prior to the initial site exploration. The artifact is 57.9 mm long, 18.0 mm wide, and 18.2 mm thick, weighing 10.0 g (modified from Ardelean 2013:361, Fig. 219–220; photos by C. F. Ardelean; line drawing by Jaime Castrellón).

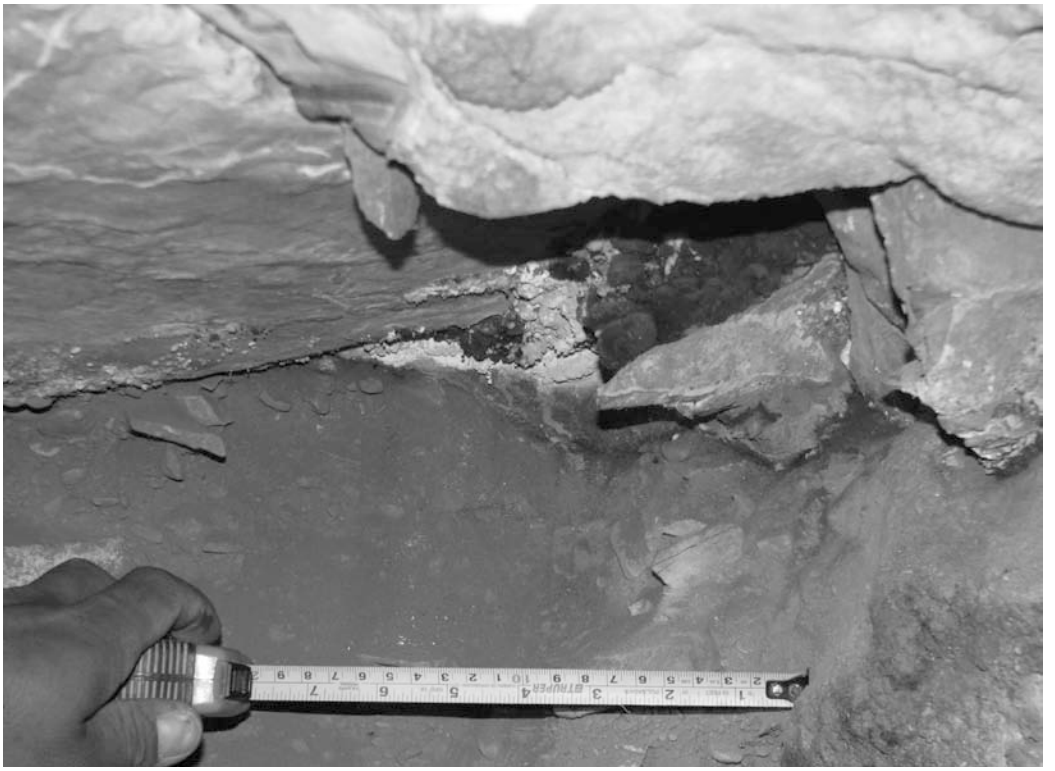

FIGURE 7.9. Close-up of the human-made fireplace observed beneath large blocks fallen from the ceiling, discovered during the surface exploration of the second gallery in September 2011. The position of the feature deep inside the dark chamber, well removed from the entrance, as well as microscopic and radiocarbon analyses, indicated it was not the result of a natural event but most likely an intentional fire made by humans over 6,000 years ago (photo by C. F. Ardelean, 2011; after Ardelean 2013:363, Fig. 221).

wider than 20 cm. Fine ash and charcoal present several hues of grey, brown, and black, indicating distinct materials burned at different temperatures. Samples were extracted for micromorphology and radiocarbon dating. Micromorphology analysis, performed at the National Autonomous University of Mexico (UNAM), evidenced the feature was created by a sustained and intense fire that burned the sediments beneath, confirming it as a combustion feature, presumably a fireplace or the place where a torch burned. Cremated remains of insects were also found inside the samples.

AMS dating was performed on a sample from the "hearth" (Oxford no. OxA-27073). The charcoal yielded an age of 5934 ± 32 RCYBP. When calibrated (OxCal 4.2, curve IntCal13), it gives a date around 6700 cal BP. Surprisingly, the small hearth was of Middle Holocene age, which meant that the thick sediments and debris on the floor perhaps were burying even older occupations. The "hearth" was not excavated and did not show any visible artifactual remains. Regardless of the age of the foliate projectile point from outside the cave, humans seem to have used the cave for at least one brief episode about 7,000 years ago. This was an encouraging argument in favor of even earlier cultural presence in the Astillero mountains.

The test pit. The single trench from season 2012 was placed in the only available, boulder-free sector of the first gallery, attached to the north wall, in order to see the stratigraphic relationships between sediments and rock (Figures 7.6, 7.10). The aim was to evaluate the archaeological potential of the cave, possible human

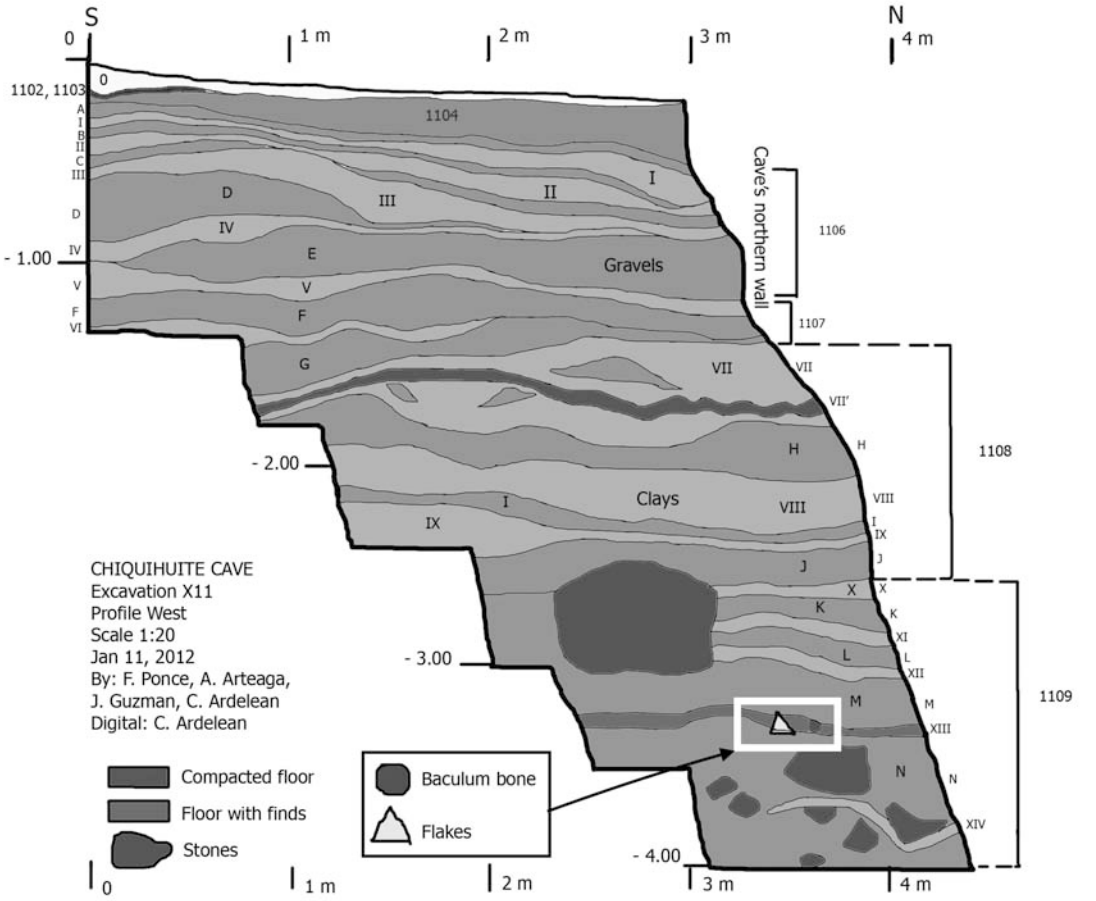

Figure 7.10. Stratigraphic drawing of the western profile of test pit X-11, excavated in the front gallery of Chiquihuite Cave, January 2012. The white rectangle marks the layer where the three flakes, the bear penis bone, and the burned phytoliths were found (modified from Ardelean 2013:369, Fig. 226).

presence, and depth of the deposits. The extent of the excavation was 3 × 2 m, with the longest axis oriented north–south. Deposits proved to be unconsolidated, although well settled, mainly sands and gravels, usually with brittle consistence. This required the walls of the trench to be properly inclined in order to avoid collapse. Steps were kept in the southern half, as the dig went deeper, providing access and additional support to the unstable profiles though it progressively reduced the excavated surface. At the end of two weeks of excavation, the unit reached 4 m in depth, with only 1 m^2 exposed at the very bottom.

The entire stratigraphy, from top to bottom, represents a long sequence of cycles of alluvial deposition (Figure 7.10). Almost all strata were of sands and gravels, suggesting variable levels of energy, probably produced by floods invading through the cave's mouth, alternating with a few more stable periods marked by layers of clay. The clayey floors could have been formed by particle size sorting during some humid events. The environment in the cave was unstable for the time period reflected in the 4 m of depth. Each identifiable layer of sand, gravels, and clay marks one cave floor. Strata were mostly unconsolidated. Only the upper few centimeters were dry and dusty. Moisture increased gradually with depth, with the sandy sediments turning

soggy at the bottom of the trench. Ceiling collapses are almost absent from the excavated surface, and the stratigraphy, in general, seems to be the effect of the sorting of clastic material by colluvial and alluvial processes, with lower-sized clasts moving toward the cave walls. Under these circumstances, any interpretation of paleoenvironmental and archaeological data must be proposed cautiously, at a hypothetical level, and with a certain skepticism.

Nine stratigraphic units (1101–1109) were defined for the test pit X-11 (Figure 7.10). They refer to multilayered strata presenting visible differences in color and composition. The nine major units are subdivided into 29 strata or stratigraphic layers. Fourteen strata of clay and silt (I–XIV) alternate with 15 others of sands and gravels (A–O). Apparently, the excavation exposed 15 successive cave floors.

The lowest stratigraphic block is 1109, starting at 2.4 m of depth. Its interface is clearly marked by an erosion horizon above a gravel layer. The unit is dark colored and rich in tiny black dots adhering to gravels and stones. It was thought to be charcoal first, but the test for radiocarbon dating did not confirm it, indicating it rather was of mineral origin (Oxford no. P-32546). Large boulders and slabs were also present, probably from collapse events. This unit was high in moisture. The controlled micro-excavation was difficult, as the matrix was muddy.

No archaeological object, macroscopic plant, or faunal remains appeared anywhere in the excavation before reaching unit 1109, although the sediments were sieved, and the procedure was careful. The situation changed at about 3.3 m deep, starting with the clay floor no. XIII. Several objects appeared together, concentrated in quadrants A4–B4, next to the cave's wall.

Animal bones. In unit 1109, about 30 small animal bone fragments, from a variety of body parts, were found clustered at 3.60–3.80 m of depth. They were identified as bat and rodent bones by Arroyo's team, and some show indications of having been partially digested, probably as owl pellets or from another predator. This could be indirect evidence that these levels were once exposed as a cave floor. However, rodent burrowing is not excluded as a contributing cause.

Most bone materials identified in the excavation are either complete or fragmented long bones. Several mammal mandibles with teeth were also found, allowing more secure identification. It must be remembered that Mexican biodiversity is enormous, both present and past (Ceballos et al. 2010), making some identifications difficult, but most of the site materials had diagnostic attributes that helped at their taxonomic classification. The animals found at the cave include both birds and mammals. The Class Aves is represented by the acorn woodpecker (*Piciformes*, *Picidae*, *Melanerpes formicivorus*), and bushfinches and sparrows (*Passeriformes*, *Emberizidae*). The Class Mammalia shows specimens from 5 orders and 6 families, with bats (*Chiroptera*), rabbits (*Lagomorpha*), gophers, mice, and rats (*Rodentia*), bear (*Carnivora*), and deer (*Artiodactyla*).

Most of the identified mammals are known from grasslands and xerophytic scrub, which is currently the vegetation around the mountain where the cave is located. However, a few are known from temperate forests, which may have been the dominant landscape during the Late Pleistocene, such as black bear and flat-headed Myotis bat (the latter requiring a mixed vegetation composed of yucca trees and pinyon pines). As for the birds, sparrows are widespread, while the acorn woodpecker is known mostly from oak or pine woodland.

Burned palm phytoliths. Two 100 ml sediment samples extracted from the lowest layers contained 182 phytoliths analyzed by J. Watling in Exeter. The assemblage constitutes 9 percent grasses (rondel and saddle phytoliths), 50 percent wood phytoliths (globular granulates), and 41 percent palms (globular echinates). Grasses and wood are also present in upper unit 1108, but palms are far less abundant there (5 percent). The genus or species of palms could not be specified. Therefore, it is unknown whether the phytoliths originated from foreign taxa. Furthermore, as these morphotypes are produced by all parts of the plant (stem, leaves, and fruits), their anatomical origin cannot be ascertained.

Intriguingly, one quarter of the palm phytoliths from 1109 exhibit discoloration from burning, which implies direct contact with fire, presumably a hearth. It seems likely therefore, as a hypothesis, that palm products were brought directly to the cave by people, perhaps as fruits for consumption, artifacts made of fiber, or leaves or wood for fuel or construction. Without knowledge of the paleoenvironmental setting of the cave, it remains unknown whether this material originated locally or was brought in from a more distant ecosystem.

Bear baculum (penis bone). At 3.30 m deep, in the same reduced space as the rest of the finds, there was a long, needle-like bone with pointed ends and a smooth, longitudinal ridge on one side (Figure 7.11-A). Arroyo's zooarchaeology team from the National Institute of Anthropology and History (INAH) in Mexico City identified it as a bear *baculum* (penis bone). The genus and species are still debatable, but it almost surely belongs to an American black bear (*Ursus americanus*), an animal long extinct in northeastern Zacatecas.[2] However, it could also belong to Ice Age extinct short-faced bears such as *Arctodus simus* (although bacula of this animal are yet unknown; Schubert and Kaufmann 2003) or *Tremarctos* sp. (cf. Mondolfi 1983). The bone was relatively well preserved, with some erosion marks and a broken end, but lacked any visible anthropic intervention.

A mysterious bone. Next to the bear baculum, there was another bone, closely resembling a rib (Figure 7.11-B). Zooarchaeologists do not agree on its identification yet. However, it can be argued that its shape, morphology, and size make it look either like the proximal fragment of a baculum, perhaps a "floating" rib from an unidentified species, or even a fragment of a large hyoid bone. The stratigraphic association of this bone with the bear's *os penis* is interesting and its taxonomic pertinence is crucial if we argued for a potential human agency in their deposition.

Limestone flakes. Three gray-greenish, presumably human-made, silicified limestone flakes were found by sieving, in the same bucket of sediment excavated from the limit between quadrants A4 and B4, at 3.30–3.40 m deep (Figure 7.12). They come from the same stratigraphic unit as the penis bone and the burned phytoliths. Flake *A* seems to be a fragmented thinning flake, with parallel edges, platform missing, and a feather termination. It displays two small narrow scars on its dorsal face, probably from the preparation of the striking platform. Flake *B* has a dihedral platform, an impact bulb, and flake scars on its dorsal side. Flake *C* seems to display a "nippled" ground platform. They are made of a variety of the same raw material as the limestone artifacts found at nearby sites, such as Dunas de Milpa Grande and San José de las Grutas (Ardelean 2013; Ardelean and Macías-Quintero 2016). All three show chemical (postdepositional) erosion. While doubts may persist about the artificial origin of flakes *A* and *C*, specimen *B* is definitely a human-made product. The fact that they were discovered together, and no such

FIGURE 7.11. The two bones recovered together at the bottom of the test pit in the Chiquihuite Cave. The upper one (*A*) was identified as a *baculum* (penis bone) belonging to a bear, possibly *Ursus americanus* (American black bear). It was destroyed during the radiocarbon dating process, yielding a pre-LGM age. The other bone (*B*) may be another penis bone or perhaps a fragment of a large hyoid bone, but its taxonomic identification is still uncertain (photos by C. F. Ardelean).

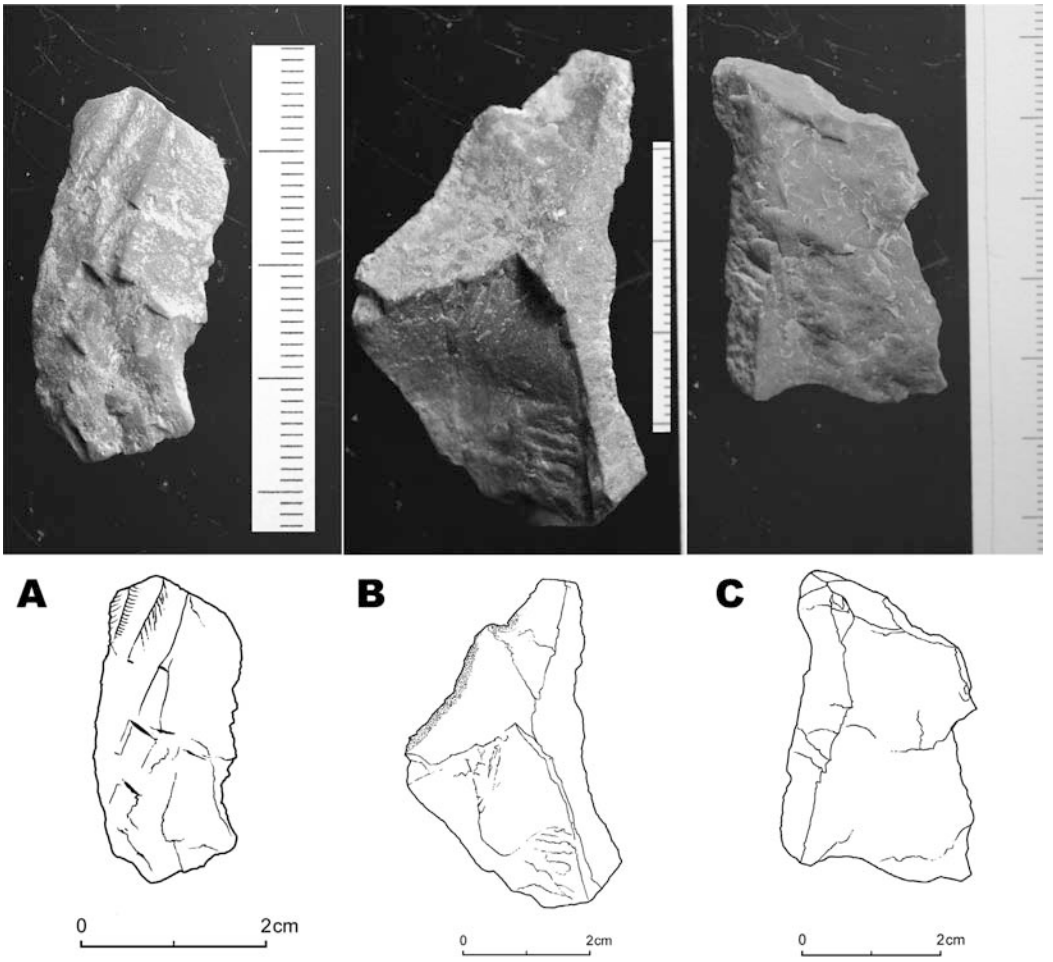

FIGURE 7.12. The three presumably artificially produced limestone flakes recovered in the sieve from the same layer as the bear penis bone in unit 1109. Their original color was grey. Flake A measures 33 × 15.3 × 4 mm; flake B is 49.6 × 31 × 8.2 mm; and flake C is 48 × 28 × 6.3 mm. They show platforms indicating intentional flaking (photos and line drawings by Mike Rouillard, Exeter).

material was found in any other excavated layer in the cave, may argue in favor of these objects as real artifacts.

Two samples were sent for dating, all from unit 1109 where the flakes, the penis bones, and the burned phytoliths were found. One OSL sample was extracted at 3.30 m of depth, from the eastern profile, close to its juncture with the cave wall. The analysis, performed at the University of Oxford's Research Laboratory for Archaeology and History of Art (RLAHA; sample no. X-4135) by Jean-Luc Schwenninger, offered an interesting result. The laboratory announced an initial date of 25,870 ± 2120 BP (or 25.87 ± 2.1 ka). A further calculation was made subsequently, taking into account the hypothetical thickness of the cave's roof set at 40 m, based on exterior measurements, which implies a higher protection from cosmic rays. The new luminescence result (based on a total dose rate of 1.69 ± 0.12 Gy/ka) was 29,180 ± 2,570 years of age.

Facing the challenges and doubts raised by these dating attempts, it was decided to sacrifice the bear *baculum* for AMS dating after making a cast. The collagen from the bone (sample no. Beta-345055) yielded an astonishing age of

FIGURE 7.13. Spectacular speleothems, more than 4 m tall (the "Ancestors"), in the western corner of the second chamber at the Chiquihuite Cave, growing on top of ancient, massive ceiling collapses (photo by C. F. Ardelean, 2014).

27,830 ± 150 RCYBP, calibrating (2σ) at about 32,000 cal BP. The two dating results from different methods seemed to match.

Discussion and Conclusions

The discoveries reported above may provoke tempting assumptions, but any interpretation must be made with extreme caution and only on a hypothetical level at this incipient stage of the research. So, were there people living in a cave in Zacatecas more than 30,000 years ago, which Lorenzo's model and the few findings would entitle one to conclude?

The scientific, self-critical posture requires consideration of a few important aspects. The investigation described in this chapter was at a preliminary level, and only a short amount of time was involved in exploration of the cave. No conclusions at all can be reached after only one test pit. Two dates are insufficient, even if using two different procedures, nor are a few archaeological items. The flakes were recovered in the sieve and their potential relationship to the rest of the context could not be documented within the dig.

Nevertheless, skepticism should not mean outright rejection, and there are facts that cannot be denied. In this case, all the archaeological finds appeared at the same level. No indicators of "intrusions" existed before reaching 3.3 m of depth, when the finds started to appear—all clustered in the northern quadrants. No disturbances are visible anywhere on profiles, the layers being continuous and consistent over the entire pit's surface. Nor is there evidence of burrowing, holes, or cleavages. Even the presence of microfauna bones, probably left by predators, suggest that the clay floor XIII was once exposed and receptive to natural and anthropic depositions. Burned phytoliths appear in that level and are of palms, perhaps from exogenous taxa, possibly brought in by humans, in the same stratum that yielded at least one baculum bone associated with three limestone flakes. In

addition, the radiocarbon dating of the bone and the OSL dating of the stratum match, with sufficient precision.

Anticipating necessary and imminent critiques, a few questions come to the mind: how could a bear's penis bone reach the cave's ancient floors in the apparent absence of any other bear skeletal fragment? If the materials were intrusive, penetrating from above or beneath, how could they all stop *exactly* at the same level and within the same floor unit? If that happened, what made several anthropic indicators converge in the same reduced place?

Based on these facts, one could propose a working hypothesis that there are potential indicators of an older-than-Clovis human occupation at Chiquihuite Cave, dating to about 30,000 years ago. The cave serves as a case study here, meant to support a discussion about how to manage data that can be mistreated. Here, an early occupation is only a hypothesis, not a conclusion, which it would be according to the national paradigm. Still, there is an exciting and striking possibility suggested by a handful of data that must be handled with care and skepticism but not simply denied for being uncomfortable. Ancient human presence in Chiquihuite still remains a possibility that we must not fear simply because academic dogmas cultivated on one side of a river or the other tell us to feel that way. It is a valid epistemological protocol around an incipient corpus of data that must be further tested by extensive excavations in the cave.

This is the middle path that should be followed in the American prehistory: any discovery and dating results should be taken into account as arguments for the construction of working hypotheses meant to be fully tested by subsequent fieldwork. This is the only mechanism we have to separate the objective reality of the human past from prejudice and speculation.

Endnote

At the time this volume went into publication, Chiquihuite Cave had already witnessed two subsequent intensive and fairly long excavation seasons, in 2016 and 2017. Many of the assumptions and hypotheses stated here have been further tested by detailed explorations. The reader is kindly invited to consult the new publications about the staggering results that we hope are already available by the time you read this. Nevertheless, despite the expansion of knowledge at Chiquihuite during this interval, the first author and project director decided not to modify the format and content of this chapter as it was prepared for the 2014 symposium in Austin for one important reason: this text reflects our thinking and inquiries at the time, soon after the initial studies at the cave. They are a crucial part of the development and growth of the knowledge we acquired of the earliest humans in Mexico. Although we know much more today, this chapter needs to conserve the preliminary form our science had a few years ago.

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Notes

1. Chiquihuite Cave has been listed in Mexico's National Register of Sites, G14C6332001, ID 44499.
2. We also performed an extensive comparison of the specimen with bear and carnivore bacula from collections at the Smithsonian Institution in Washington, D.C., thanks to invaluable help provided by our colleagues Dennis Stanford, Stephanie Cannington, and Joseph Villari,

to whom we are deeply grateful. After the derived observations, the American black bear seems to be the most suitable option for this specimen.

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ARCHAEOLOGY/ANTHROPOLOGY

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Rafael Suárez is professor of archaeology at the Universidad de la República in Montevideo, Uruguay, and a researcher within the Sistema Nacional de Investigadores (SNI).

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