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The Dead Tell Tales

Essays in Honor of Jane E. Buikstra

Edited by
María Cecilia Lozada and
Barra O'Donnabhain

The Dead Tell Tales

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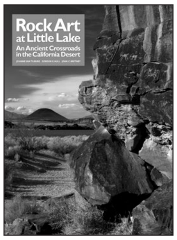

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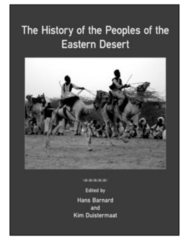

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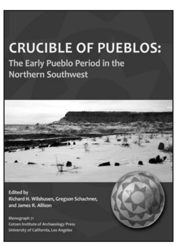

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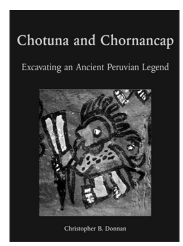

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The Dead Tell Tales:

Essays in Honor of Jane E. Buikstra

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María Cecilia Lozada and Barra O'Donnabhain

Monograph 76

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About the Editors

María Cecilia Lozada is a Peruvian bioarchaeologist who has been conducting archaeological research in the South Central Andes for the last 20 years, and holds a PhD degree in Anthropology from the University of Chicago. She uses a multidisciplinary approach to study the past, combining archaeology, human osteology and ethnohistory, which is exemplified in her book, *El Señorío de Chiribaya en la Costa Sur del Perú* (2002) and multiple publications that showcase her integrated social and biological reconstructions of past Andean societies. For the past 7 years, Lozada has been the main field archaeologist and human osteologist for several multidisciplinary and collaborative projects in northern Chile co-sponsored by the Cotsen Institute of Archaeology at UCLA and La Universidad de Chile. In 2009, she initiated a new archaeological project in the Vitor valley in southern Peru. As the Principal Investigator of the Vitor Archaeological Project, Lozada leads a multinational team that includes Peruvian investigators as well as numerous American students and researchers from institutions such as UCLA, the Field Museum of Natural History in Chicago and The University of Chicago. Her close professional and personal ties to the archaeological community in Peru has helped garner support from the Ministerio de Cultura del Perú which, in turn, has helped the project to flourish. She is currently a Research Associate in the Department of Anthropology at the University of Chicago, and has been actively working to promote collaborations between local universities in Peru and the United States through her projects.

Barra O'Donnabhain, who holds a PhD degree in Anthropology from the University of Chicago, is an Irish bioarchaeologist who has been conducting archaeological research in Ireland and other parts of the world for over 25 years. His publications cover a wide temporal span as well as a broad range of themes but are characterized by an integrative approach in their reconstructions of past lives. This is exemplified by recent papers dealing with the political use of the ritualized violence of executions (2011) and the use of bone chemistry to characterize diet and identity in Viking Age Dublin (2012). He has also challenged in print the traditional narrative of "Celtic Ireland." O'Donnabhain has directed and collaborated in archaeological projects in a number of world areas. His recent focus has been on institutional confinement. From 2009 to 2011, he directed excavations at a putative leper hospital in south west Ireland and in 2012 he began excavations at a 19th century prison. Also in 2012, he directed the mortuary component of excavations at an Inca outpost in southern Peru in collaboration with researchers from UCLA and USC. He is currently on the faculty of the Department of Archaeology at University College Cork and is on the Board of Directors of the Los Angeles-based Institute for Field Research.

About the Contributors

Dave Aftandilian is Assistant Professor of Anthropology at Texas Christian University and co-chair of the Animals and Religion Consultation for the American Academy of Religion. He is the editor of *What Are the Animals to Us? Approaches from Science, Religion, Folklore, Literature, and Art* (Tennessee, 2007), and his research and teaching focus on animals and religion, Native American religion and ecology, food justice, and Illinois archaeology.

Daniel H. Aguilar holds a M.Sc. in material chemistry, and is currently working as a research assistant at Cinvestav-Mérida, Yucatan, Mexico. He possesses experience in the applying the techniques of X-ray diffraction, X-ray fluorescence and small angle X-ray scattering, which he has applied to organic and inorganic materials, such as ceramics, archaeological sustrates, and semiconductors.

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Juan V. Cauich-Rodríguez received his PhD in the IRC in Biomedical Materials at Queen Mary and Westfield College (London, UK) in 1997 and the MSc in Polymer Science and Technology at UMIST (Manchester, UK) in 1994. After obtaining his degree, he has worked as a researcher at the Centro de Investigación Científica de Yucatán A.C. developing biomaterials for orthopedic applications. Currently, he is developing polymers for cardiovascular applications such as biodegradable polyurethanes and CTN composites.

William J. Folan is a Director, professor, emeritus researcher and cofounder of the Centro de Investigaciones Históricas y Sociales of the Universidad Autónomo de Campeche. His interdisciplinary research on Maya archaeology, coupled with paleoclimatology, has centered around the Maya centers of Calakmul, Oxpemul, and Champoton.

George R. Milner's (Professor of Anthropology, The Pennsylvania State University) osteological research currently focuses on age estimation and trauma, and his archaeological work mostly pertains to the prehistory of eastern North America. He has conducted archaeological field work in the United States, Egypt, and Micronesia, and as part of various projects has examined ancient and modern skeletal remains from North America, Europe, and Africa.

Patricia Quintana Owen holds a PhD in chemistry of ceramic materials, and is currently working as a researcher at Cinvestav-Mérida, Yucatan, Mexico. Her research is directed toward studying the chemistry and characterization of materials (ceramics, biomaterials, karstic rocks and archaeological) with electrical and photocatalytic applications; and identification of clay materials present in soils of the Yucatan Peninsula.

Christopher Stojanowski is Associate Professor in the School of Human Evolution and Social Change at Arizona State University. He is a specialist in the bioarchaeology of the Southeast with an emphasis on dental anthropology and biodistance analysis.

Vera Tiesler holds a PhD in anthropology and is employed as a research professor at the Autonomous University of Yucatán, Mérida, Mexico. Her research has centered on Caribbean and Mesoamerican, specifically prehispanic and colonial Maya bioarchaeology and mortuary archaeology emphasizing interdisciplinary approaches.

Lori Wright is Professor of Anthropology at Texas A&M University. Her research examines ancient Maya diet, health, and migration using stable isotope geochemistry of skeletal remains and paleopathology.

Introduction

María Cecilia Lozada
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Barra O'Donnabhain
Department of Archaeology, University College Cork

This volume of essays is offered to Professor Jane E. Buikstra to honor her sixty-fifth birthday, which she celebrated in 2011. The volume began life in a symposium entitled "The Dead Tell Tales: Jane E. Buikstra and Narratives of the Past," held in San Juan, Puerto Rico, in 2006 at the seventy-first annual meeting of the Society for American Archaeology and organized by the editors of this volume. Both of us are active in bioarchaeological research and teaching. As a result, we have always been keenly aware of Buikstra's pioneering work in the development of the discipline, her leadership role in its ongoing development, and her important contributions to other aspects of anthropological research. The SAA symposium marked the first time that multiple generations of Buikstra's former doctoral students and other colleagues gathered to discuss the impact of her mentorship, and the session was remarkable for its breadth, both in terms of the topics discussed and the geographical range they covered. Despite the diversity and scope of the contributions, they were united by the integrative cross-disciplinarity that has always been a hallmark of Jane E. Buikstra's anthropology. The aim of the current volume is to disseminate the contributions to a wider audience and to thereby reinforce this approach. Like the SAA symposium, this volume celebrates more than three decades of scholarship by Jane E. Buikstra and the professional and personal relationships she has built in the process. Buikstra's mentorship has been marked by generosity. She has been a consistent advocate for her students, who have tended to move smoothly from pupil to colleague (see Rakita this volume). She has fostered generations of young scholars, not only from the United States but also from many of the countries in which she has conducted research and beyond. Her foreign students were drawn from countries where formal training in bioarchaeology was not available, and through her personal involvement and mentorship, she has played a direct role in seeding the discipline in a number of different world areas. This pioneering spirit has its roots very early in Buikstra's career.

Jane E. Buikstra's interests in archaeology and human biology can be traced to her childhood. She is from southern Indiana, an area rich in archaeological remains, where she grew up in a medical household. Her undergraduate training in anthropology was at DePauw University. She moved to the University of Chicago for her postgraduate studies. While gaining a strong background in biological anthropology under the tutelage of Dr. Charles Merbs, and others, she had an equally strong background in archaeological fieldwork with Stuart Struever due to her participation in excavations at the Koster Site in southern Illinois. In 1976, four years after she obtained her Ph.D., in a paper delivered to the Southern Anthropological Society, she advocated a new approach that sought to find common ground between these interests and to live up to the aspiration of anthropology to be a holistic endeavor. Bioarchaeology, as defined by Buikstra, sought to integrate the previously distinct areas of study of biological anthropology and archaeology (Buikstra 1977:69). This was in keeping with the ethos of the New Archaeology of the time, stressing as it did the place of humans in broader cultural and ecological systems. Proposing a new modus operandi is easier than giving it a reality. Equally critical to the development of bioarchaeology has been Buikstra's leadership in nurturing the discipline by means of a prodigious research output, much of it collaborative, characterized both by the integration of narratives from a range of discourses and by its dynamism, informed as it is by changing directions in theory, method, and practice (see chapters in this volume by Patterson and Charles).

Jane E. Buikstra's innovative and pioneering approach is also apparent in her career-long involvement with the Center for American Archaeology at Kampsville, Illinois. This involvement grew from the Koster excavations, which played a formative role for many anthropologists of her generation. From its earliest days, one of the distinguishing features of the CAA has been its community involvement. Dialogue with local communities is now commonplace in archaeological research projects, but Buikstra and the CAA anticipated this by decades at Kampsville. The center has played an important role in fostering students of archaeology and bioarchaeology but has always had a broader vision of involving the wider community through its various activities and outreach programs. Under Buikstra's leadership, the CAA has been characterized by its respect for local identities and by the creation of a space for open dialogue between the local and scientific communities.

Buikstra's commitment to sharing knowledge has been a characteristic of her career since her earliest academic appointment at Northwestern University. While also undertaking substantial administrative duties, Buikstra pioneered the integration of osteological data in funerary archaeology, which had lasting implications for both mortuary theory and bioarchaeology. She also collaborated in a number of groundbreaking studies across a range of topics, including bone chemistry and methods of assessing heritability of nonmetric traits in primates. Furthermore, she made important contributions in paleopathology. Her 1981 volume *Prehistoric Tuberculosis in the Americas* set a new standard for paleopathological studies and, true to the roots of bioarchaeology in the New Archaeology, established a new population-based approach that moved beyond the clinically oriented, descriptive case-study methodology of earlier studies. The 1981 volume was also notable for its integration of narratives from different disciplines.

Combining these elements of close collaborations between academic disciplines, and also between anthropologists and local communities, Buikstra was instrumental in establishing centers of bioarchaeological research outside North America. In particular, her work in Peru was critical in the seeding of her vision of bioarchaeology in the Andes. As the discipline developed in Peru, it was again characterized by its interdisciplinarity and its engagement with, and sensitivity to, the wider community. Her significant record of publications is testament to her enduring commitment to the region. She also played a personal role in establishing bioarchaeological research in Spain, where she was drawn by the culture of dialogue and inclusiveness fostered by the leaders of the Gatas Project. Her involvement marked a decisive break from previous approaches in the region and resulted in a broadening of research agendas regarding mortuary contexts. Buikstra has also worked in Argentina, Brazil, Mexico, and Honduras, and through her students she has indirectly influenced the development of the discipline in a number of countries, including Greece, Indonesia, and Ireland.

Buikstra's ability to recruit graduate students from the United States and abroad was enhanced by her move in 1986 to the University of Chicago, where she created a rich learning and research environment characterized by cultural and intellectual diversity. Her term at Chicago was marked by a number of honors. In 1987 Buikstra was elected to the National Academy of Sciences. Two years later she was elected to the presidency of the American Anthropological Association, becoming the sixth woman to hold that post since the organization was founded in 1902. These honors were an affirmation of her standing in the anthropological community but also an acknowledgment of the place of bioarchaeology within the broader discipline. This came at a time of potential crisis for the discipline, as the passing of the Native American Graves and Repatriation Act (NAGPRA) in 1990 allowed for the possible return of cultural items and human remains to their respective peoples and for their reburial. With the prospect of the repatriation of many collections of archaeologically retrieved human skeletons, there was concern about the lack of a standardized approach to analysis and reporting and the need to gain the maximum amount of information from remains that might no longer be available for scientific study. A series of meetings initiated by the Paleopathology Association sought to develop standards for the collection of osteological data. This resulted in a workshop held in 1991 at the Field Museum of

Natural History in Chicago that was supported by the National Science Foundation. The resulting publication (Buikstra and Ubelaker 1994) became a key resource for researchers and students alike.

In 1995 Buikstra moved to the University of New Mexico, where her work was marked by an increase in evaluative and synthetic publications. Her international interests expanded to include studies of the ancient Maya, where she collaborated on population-based studies of hard tissue biochemistry while also generating individual osteobiographies that traced residence changes and the personal, physical cost of wielding political power (see chapters in this volume by Wright and by Tiesler et al.). She also began a productive collaboration with colleagues in Britain, where bioarchaeology, or human osteoarchaeology as it is usually called there, had come of age in the 1980s and 1990s (Roberts this volume). This work was marked by a return to a long-standing interest, the bioarchaeology of tuberculosis. Debates about the coevolution of humans and the pathogens that produce TB are critical to research aimed at mitigating the global threat to modern populations posed by this reemerging disease.

This focus on the relevance of bioarchaeology to contemporary society anticipated Buikstra's move in 2005 to Arizona State University, where she became the first director of the newly established Center for Bioarchaeological Research. The high value placed on bioarchaeology at ASU and the manner in which the center is integrated into the broader research strategies of the School of the Human Environment and Social Change are models of research and higher education that Buikstra has advocated throughout her career. At ASU Buikstra is in an innovative environment that explicitly seeks to move beyond the scope that traditionally defined the study of anthropology and that balances sociocultural and biologically based approaches to humanity. In the years since her move to Arizona, Buikstra has collaborated on three important volumes that, characteristically, span a range of interests—mortuary practices (Rakita et al. 2005), forensic anthropology (Komar and Buikstra 2008), and bioarchaeology (Buikstra and Beck 2006)—with each volume including an assessment of the current status of the topic. In the last few years, Buikstra's accomplishments have been recognized in the form of a number of awards, including the T. Dale Steward Award from the physical anthropology section of the American Academy of Forensic Sciences (2008) and the Charles Darwin Lifetime Achievement Award by the American Association of Physical Anthropology (2008). Her multidisciplinary study of past societies, the hallmark of her research, was further acknowledged by the Fryxell Award by the Society for American Archaeology in 2010. Her service to academia has also been continued through her role in the establishment of the *International Journal of Paleopathology*, providing an international forum for the study of health and disease in the past.

Organization of This Volume

The organization of this volume reflects the extent of Buikstra's influence in the discipline and her advocacy for a bioarchaeology that is embedded in and in constant dialogue with broader anthropological praxis. An opening section devoted to her theoretical impact is followed by papers organized geographically and reflecting the breadth of Buikstra's reach. Part I of this volume specifically addresses Buikstra's contributions to anthropology. Patterson (chapter 1) charts the dynamic interplay between theory and practice that has characterized her career, and he locates her approach within developments in the broader discipline, both in the United States and in other countries. One of the most direct ways to understand an individual's contribution to a discipline is to examine his or her body of published works. Rakita's analysis of Buikstra's bibliography (chapter 2) provides an insight into her signature multidisciplinary approach, her breadth of subject matter spanning the biological and social sciences, and the many geographical areas in which she has worked. Furthermore, the collaborative nature of much of her output reflects the collegiality of her approach and the altruism of her mentorship. In chapter 3, Charles takes up the theme of the dynamic nature of the study of archaeological human remains and notes the development of a diversity of approaches since the 1970s. He eloquently highlights the challenges inherent in attempts to straddle the physical sciences/social sciences divide, and he notes the malleability of Buikstra's bioarchaeology, which has allowed it to mature in the context of shifting theoretical landscapes. Buikstra has been particularly sensitive to these changing paradigms, and this sensitivity is reflected across the spectrum of her engagement with the human body, past and present. Buikstra's concern with advancing methods provided the inspiration for Lewis and Tung's contribution (chapter

4). In the context of new research technologies that feed growing demands for osteological samples, this is a timely reminder of the need to develop and adhere to protocols designed to facilitate research but also the long-term conservation of human remains.

The focus of the papers in Part II is on North America, where Buikstra's academic life has been based and where, through her continuing fieldwork at Kampsville, her anthropological life is anchored. Over the last two decades, NAGPRA has posed serious challenges for the praxis of bioarchaeology in North America. Goldstein (chapter 5) uses reports from the Smithsonian Institution Repatriation Office to examine the nature of relationships between researchers and native peoples in the context of negotiations concerning the repatriation of archaeological artifacts and human remains. Goldstein reflects on the multiscalar nature of identity and the importance of the oral tradition in the formation of collective memory. The emotive connection between living peoples and ancient human remains is also evoked in Watson's detective work concerning the discovery and subsequent histories of mummified remains from Kentucky (chapter 6). She follows the development of popular narratives concerning the biographies of these remains, which are contrasted with results of more recent bioarchaeological analyses. Watson highlights how human remains, both ancient and modern, have an interest beyond the scientific, and she charts the different social lives that dead bodies can have through time. The relevance of archaeological approaches to both ancient and modern contexts is also apparent in the paper by Ubelaker (chapter 7), who notes that in bioarchaeology, contexts with commingled human remains present particular challenges in terms of excavation and analysis. Ubelaker uses the ossuaries of Maryland to address these issues. He presents a rigorous approach to the proper recovery and documentation of secondary deposits of multiple individuals, as well as a systematic methodology that allows for the spatial analysis of remains, their demographic profiles, and the details of burial practices in the past. These approaches are not only relevant to archaeological contexts but could also be adopted for forensic investigations of more recent mass graves. A similarly rigorous methodology is proposed by Aftandilian (chapter 8) with regard to the analysis of animal effigies deposited as grave goods in Mississippian burials from Illinois. This method and analysis provide a good example of the integrative approach espoused in bioarchaeology as it draws together material cultural symbolism within mortuary ritual to document a relationship between specific motifs and particular cohorts of the population, such as infants and children. The integrative approach is also central to the paper by Stojanowski (chapter 9), which examines the fluid relationship between biology and social identity among human groups. In his discussion of the use of morphological distance as a means for drawing inferences about community self-definition, Stojanowski situates biodistance studies within a broad social framework, and he argues for historically contingent reconstructions of the formation and transformation of group identities in the past.

In Part III, the focus shifts to Central and South America, where Buikstra has made a number of important direct contributions since the early 1980s. Tiesler et al. (chapter 10) note that investigations of Maya elites have benefited over the last decade from multidisciplinary studies that seek to integrate bioarchaeological and other data sets with more traditional approaches. These authors present sophisticated analyses of residues found adhering to two royal skeletons from Calakmul, Mexico, the results of which are discussed in the context of other aspects of the burial record to provide new insights into details of the complex funerary treatments of individuals from the Mayan elite. Wright's contribution (chapter 11) also has an individual focus as it takes an osteobiographical approach using stable isotopes to document age-specific changes among elite skeletons from Copan, Honduras. While acknowledging the challenges presented by such chemical analyses, her studies provide an enriched narrative of individual life histories and allow her to chart mobility, rainfall seasonality, weaning, and social differences in diet. Keeping with ancient Maya society but in contrast to the usual situation encountered in bioarchaeology, Ashmore (chapter 12) examines situations where archaeologists have encountered spaces that were prepared as if to receive the dead, but where no bodies were found. The chapter considers inferences about such "absences" to examine not only instances in which bodies may have been moved but also circumstances where interment never took place. These absences are situated within current thinking about past mortuary programs and Maya beliefs relating to the dead and to the deceased's physical remains.

In South America, Andean archaeologists have the benefit of a rich ethnohistorical record of the precolonial period. Lozada and Rakita (chapter 13) use such accounts to interpret social and biological transitions among the Chiribaya of coastal Peru. (The Chiribaya project was directed by Buikstra.) They integrate the analysis of skeletal data, grave goods, ethnohistory, and funerary patterns as a means of drawing inferences about gender construction and attitudes toward key transitions in the life cycle of an individual. Nystrom also seeks to integrate narratives from bioarchaeology, ethnohistory, and material culture studies (chapter 14). In one of the first investigations of the biological impact of the Inca expansion, he documents the effects of the process of imperial domination on local populations that reflect the context-specific strategies used to build Inca hegemony.

The focus of Part IV is on the Old World, where a number of different approaches to archaeologically retrieved human remains developed in parallel to the emergence of bioarchaeology in the Americas. Paleopathology has been central to the emergence of a number of these developments. In chapter 15, Aufderheide considers the long history of the study of pathological lesions in mummified human remains, where the presence of soft tissues has both fascinated and challenged researchers. He documents how the integration of clinical and bioarchaeological approaches in the last few decades has broadened the agenda beyond paleopathology and has led to the emergence of mummy studies as vibrant scientific endeavors that have moved beyond their traditional home in Egypt. In her discussion of leprosy in medieval Europe and particularly Britain (chapter 16), Roberts combines the clinical approach to the malady with historical and bioarchaeological perspectives and concludes that to achieve a more complete understanding of the impact of disease, it is necessary to move beyond biological dimensions and to consider the social context of such afflictions. Perry et al. use bioarchaeological approaches to contest the validity of historical narratives of mine workers in Byzantine-period Jordan (chapter 17). Using bone chemistry analysis and other lines of evidence, they identify a hitherto invisible cohort of the mining population, providing more complete and representative biographies of the laborers behind the production of metal. In chapter 18, Lull et al. follow similar methodologies to those outlined by Ubelaker in Part III and also consider the importance of taphonomic issues in the formation of the funerary record. They argue that categories such as collective burial have a homogenizing effect that masks diachronic variability in mortuary behavior. They advocate the adoption of historically contingent approaches where variability is related to broader socioeconomic factors. Historical contingency is also central to the contribution by O'Donnabhain and Hallgrímsson, who, like Stojanowski in Part II, examine the relationship between biology and social identity. Using morphological distance studies to interrogate the nature of the colonial encounter in Viking Age Ireland, they argue that cultural responses to biological syncretism varied and that material culture and habituated practices were critical to identity construction. In chapter 20, Milner demonstrates that it is unsafe to make broad assumptions about the relationship between health status and social organization by examining the connection between trauma, sex, and longevity in medieval Denmark. He concludes that bioarchaeological research should be historically contingent, and he questions a number of conventional wisdoms regarding temporal and geographical variations in health.

In conclusion, our hopes for this volume can be distilled into two points. First, we hope that the volume documents the degree to which bioarchaeological approaches have become normalized and integrated into anthropological research. It is apparent from this collection of essays that Buikstra's vision of bringing the osteologist out of the lab and into the field, and thereby out of the appendix and into the interpretation of archaeological data, has been achieved. This is testament to the success of Buikstra's pioneering work and her subsequent evangelical approach to the promotion of the discipline both within the United States and internationally. Secondly, we hope this volume serves to highlight the dynamism of bioarchaeology, which owes so much to the strong foundations laid down over the last few decades. New perspectives have emerged, partly in response to broader theoretical changes within anthropology but also as a result of the engagement of the broader discipline with bioarchaeology. Engagement with the broader community has also had a transformative effect on the discipline and has contributed to forging new avenues of research. We anticipate further exciting developments and a long-lasting role for bioarchaeological approaches in furthering our knowledge of the human experience in the past. Such narratives deepen our understanding of the human career and have a significant relevance to our present and future.

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PART I

Buikstra's General Contributions to Anthropology

1

The Anthropological Praxis of Jane Buikstra

Thomas C. Patterson
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Jane E. Buikstra is fascinated with human skeletons. By the time she received her doctorate from the University of Chicago, she already knew at least four things well: (1) human beings live in society; (2) there is a metabolism between human beings in society and the milieus in which they live; (3) how they live and the webs of relations in which they are enmeshed are inscribed both in their bodies and in the demographic structures of those communities; and (4) there are often significant differences within societies as well as between different kinds of society. She soon learned about inequality and the differential treatment of women and minorities in the academy, perhaps as a result of gendered career advice and having to pour tea for male colleagues at faculty meetings (Buikstra 2003). These understandings—based on observation, insight, intuition, and personal experience—are not trivial. Entire careers have been built on a lot less. Buikstra has continued to learn new things and to grow intellectually throughout her career. For example, she is now trying to master saying no when asked to do something; however, it is not clear whether her only modest success so far stems from difficulties with the consonant or the vowel.

My goal in this paper is to explore Jane E. Buikstra's anthropological praxis. By this I mean broadly the creative activity through which she has come to understand the world and to change both her and our understanding of what happened in that world. This exploration involves looking at how she practices anthropology and the theoretical framework that buttresses her practical activity.

Buikstra was a graduate student in the late 1960s when the "new" or processual archaeology was coming of age and being institutionalized. Her early work is recognizably a part of this intellectual tradition, which advocated hypothesis testing, ecological thinking, and a regional approach to the study of societal development, among other things. Lamenting that bioanthropologists were "too often limited to a study of bones for their own sake—far removed from either their burial or their living context," she early argued for an integrating and integrative methodology that would provide those contexts and overcome some of the limitations imposed by hierarchically organized or technical divisions in labor in research projects (Buikstra 1976:3, 1991). This ran counter to long-accepted descriptions of the scientific method and how scientific research should be organized, as well as to then-current practices in multidisciplinary archaeological research projects (Dickson 1979). Her perspective called into question distinctions between those who gathered and assembled information and those who interpreted its significance. Noteworthy in this regard are the collaborative projects in which she has engaged and the large number of papers she has coauthored during her career, often as a junior author, with students and colleagues both inside and outside the field.

A second feature of Buikstra's perspective has been the methodical way in which she and her collaborators approached problems. While methods of multiple working hypotheses and hypothesis testing were longtime features of archaeological research, they received renewed emphasis in the late 1960s. In one sense, Buikstra's work was not exceptional in that she made use of these techniques to address problems defined by archaeologists. What was noteworthy, however, were (1) the clarity with which she dissected and stated hypotheses; (2) the rigor with which she tested them against empirical evidence and theoretically derived models; and (3) her use of diverse lines of evidence to construct "cables" of interpretation composed of a number of independent but mutually supportive strands of argumentation rather than merely additive "links in a chain" logical sequences or forms of reasoning (e.g., Buikstra and Konigsberg 1985; Konigsberg and Buikstra 1995; Konigsberg et al. 1989). Also noteworthy was that the lines of argumentation she used often derived from different fields, that they frequently exposed the limitations of models cherished by the practitioners of one field or another, and that they often argued for the importance of historical processes in ordering particular social forms (e.g., Buikstra and Mielke 1985; Buikstra et al. 1986).

A third feature of Jane E. Buikstra's point of view was that it increasingly threatened to cross not only the subfield boundaries that were being actively promoted in anthropology after World War II but also the chasms that separated anthropology from other disciplines (e.g., Buikstra 1991). It quickly necessitated drawing on steadily widening ranges of evidence, models, and theories to formulate hypotheses. This was not necessarily typical of the interdisciplinary projects of the time. Here, archaeologists tended to define the research problems. The findings of specialists from other fields were typically reported in appendices or data chapters. Their comments were either treated in isolation or used selectively to support the conclusions of the larger project. Often, what else the specialists had to say was not considered at all. From the beginning of her career, Buikstra was reading and incorporating techniques and ideas from fields ranging from physical chemistry and geology to physiology and genetics to history and cultural anthropology. Her appropriations were not relegated to appendices but appeared instead in the body of the text as well as in conclusions. Reading widely and thoughtfully in diverse fields can increase our understanding of a problem by drawing attention to the possible associations of seemingly unrelated data and by forcing us to think seriously about their significance and implications. Buikstra certainly takes into consideration a much wider range of evidence, models, and theories now than she did earlier in her career.

However, reading widely and attentively is also potentially destabilizing. She argued forcefully on more than one occasion that archaeologists should be clear about the assumptions implicit in the methods and theories they use. For example, in their discussion of the interrelations of demography, diet, and health, Buikstra and James Mielke issued a word of caution about the facile transfer of ideas from one field to another and the need to clarify the underlying presuppositions of the fields involved:

> Contemporary demographers use demographic parameters as measures of community health in human groups. This strategy is appropriate for past populations as well. However, the adaptation of demographic methods to paleodemographic study must be accompanied by an explicit appreciation of assumptions made about the uniformity of biological processes in our species—in the past and in the present—as well as the biases introduced by the nature of the archaeological record [Buikstra and Mielke 1985:361].

Making assumptions explicit can be simultaneously revelatory and complicating: revelatory in the sense that the assets and liabilities implicit in particular frameworks are brought into high relief, and complicating in the sense that the problem being investigated suddenly acquires unexpected, unpredicted, and inexplicable texture.

A fourth feature of Buikstra's praxis is that she never really adopted the neoevolutionary arguments of some strands of the New Archaeology. This was partly a consequence of her concern with what happened in a particular region—such as the lower Illinois River valley or the far south coast of Peru (e.g., Buikstra 1976; Lozada and Buikstra 2005). Thus her focus has always been history, which involves a concern with both the succession of historical forms and the historical processes that underwrote their formation rather than an embryonic-like unfolding of society or the emergence of new levels of societal development. She and Douglas Charles have advocated narratives as a means of connecting events in the archaeological record and of entry into the past of particular regions and peoples. In her view, this allows us to see more clearly the intersection of structure and agency in those communities and to understand more fully the transformations of the landscapes inhabited and created by those peoples (Charles and Buikstra 2002:15–17).

Jane E. Buikstra began her career with the tools of the New Archaeology, which informed her practice of archaeology; ultimately, her practice allowed her to hone and refine her theoretical understanding of the peoples with whose remains she worked. Perhaps another way of saying this is that practice destabilized her theoretical base. It forced her to reconceptualize those theoretical foundations and to reconsider what she knew and understood about those societies (e.g., Buikstra and Charles 1999:203–204). In Buikstra's case, this has been an ongoing dialectical process. Her continual reassessment has not resulted in either a theoretical eclecticism or an "add theory X and stir" approach to problems. It has instead led to a much more integrative and integrating view of anthropology and social theory than she held in the mid-1970s. Let us look briefly at some of the ways in which the theoretical foundations have been reworked.

In 1983 Jane E. Buikstra and Douglas Charles noted that the Archaic mortuary sites of the central Mississippi drainage contradicted received wisdom about the occurrence of formal disposal areas of the dead. While sedentary agricultural communities had cemeteries, these were not supposed to exist in foraging societies, which were widely viewed at the time as mobile. From Charles and Buikstra's perspective, the apparent contradiction was a product of bias in the ethnographic sample. They began their chapter on this topic with a lengthy discussion of their basic assumption about ancestor cults: "[T]he occurrence of formal cemetery areas is associated with corporate lineal inheritance of crucial and restricted resources" (Charles and Buikstra 1983:117). This discussion built on notions of property and territoriality (as the practice of defending or marking an area), one derived directly from liberal political thought and the other indirectly from it via ecological theory (Macpherson 1978; Ryan 1984). Both concepts are concerned with relations among peoples (communities) expressed in terms of things (land, resources, places) and behavior (competition). They concluded not only that the Archaic populations of the region were more sedentary than previously assumed but also that "the functioning corporate unit . . . was apparently the village" (Charles and Buikstra 1983:140). This emphasis on community rather than the individual or the family is a theoretically informed statement that builds on diverse currents of communitarian thought that emphasize the social nature of the individual rather than the atomized individual of liberalism.

In 1999 Buikstra and Charles turned their attention once again to the mortuary sites in the mid-continent. This time, their focus encompassed not only the Archaic but also the Woodland and Mississippian phases as well. Here, they distinguished ancestor cults from mortuary rituals. The former referred to "rituals which provide continued access to the deceased in the afterworld," while the latter "separate[d] the living from the deceased" (Buikstra and Charles 1999:204). While ancestor cults reflected defined power relations, mortuary rituals were sites of activity where those relations were contested. They also distinguished between exclusive ancestral cults, often located within or adjacent to villages, and earth/fertility cults, which ethnographically were both more inclusive and located in peripheral positions. In this paper, they argued that ancestor cults had a long history in the lower Illinois Valley. The mortuary rituals associated with them became increasingly elaborate during Middle Woodland times and "served to re-create social and political inequalities among the living" (Buikstra and Charles 1999:221). The transition from Late Woodland to Mississippian times witnessed the resolution of existing contradictions and the emergence of new forms of social inequality. As Buikstra and Charles put it, the "older, more fragile tensions between kin groups . . . [gave way to] institutionalized inequalities" (Buikstra and Charles 1999:221). Their view of social stratification is not based on claims that inequality exists in all societies or that it always develops in the same direction, but rather on the idea that it is socially constructed and historically contingent (e.g., Davis and Moore 1998 [1945]; Gailey 1987:248–266).

In 2002 Buikstra and Charles turned their attention once again to the processes of societal change and history in the lower Illinois Valley (Charles and Buikstra 2002:13, 17). This time they focused on the social and political-economic structures that provided the contexts of funerary practices in the region as well as on the historical transformations that occurred when those structures changed and new conditions developed. Any theory of history must have a theory of structure, a theory of transformation, and a theory of directionality (Callinicos 1995:95–140). The theory of structure they elaborate combines Weberian notions of power and domination with Marxist concerns about property relations. The theory of transformation they develop recognizes the importance of the mechanisms responsible for the changes that occurred in a particular society and for the ways in which one structure was dissolved and replaced by another. They do not attempt to explain all changes by the same mechanism and imply instead that different mechanisms were operative in different historical epochs (for example, Middle Archaic, Early Woodland, Middle Woodland, Late Woodland, and Mississippian), or alternatively that if the same mechanisms were operative, their effects varied from one society or historical epoch to another. This is a neo-Weberian theory of transformation that puts historical contingency at center stage combined with a Marxist theory of transformation that focuses on the development and resolution of contradictions within the domain of social production. Together with Julieann van Nest, they write,

> The end of Hopewell in the lower Illinois River valley corresponds to the slowing down and eventual cessation of this demographic transformation of the landscape. . . . Biological distance measures suggest that kinship networks had stabilized geographically and had begun to expand by this time. Simply put, the previous Middle Woodland level of negotiation among individuals and communities, utilizing the media of Hopewell was no longer necessary. Social, political and economic interaction in a more stable demographic landscape was now being conducted along kinship webs visible only through biological, rather than archaeological, analyses [Charles et al. 2004:49].

Finally, they do not elaborate a clear-cut theory of directionality that accounts for the overall pattern of historical transformation that occurred in the region, since their arguments are not evolutionist in a developmental sense; nor do they seem to rely on notions of progress, decline, or cyclicity. In their words: "The appearance of Hopewell indicates not a rise in social complexity *per se*, but rather the onset of a period of marked social and political activity" (Charles et al. 2004:48).

In conclusion, let us pull together the main strands of Jane E. Buikstra's anthropological praxis. First, her practical activity involves hypothesis testing, both deductive and inductive reasoning, blurring the boundaries between established disciplines, and listening carefully and thoughtfully to the arguments of others before she incorporates their insights into her own work. This has had destabilizing effects on the practical knowledge and methods she brought to the field in the 1970s; it has led her to hone and refine methods, to look at evidence in new ways, and to ask new questions that she probably would have thought unanswerable earlier in her career. Second, the continually evolving nature of her practical activity forced her to clarify the theoretical frameworks she used, to make explicit their presuppositions, and ultimately to explore new theoretical frameworks as she reframed research questions. Third, the destabilizing effects combined with new questions and theoretical insights underwrote the development of new forms of practical activity and fueled further changes in her praxis and understanding of the peoples with whom she worked. Fourth, her praxis has become increasingly historicized in the sense that she argues that a proper understanding of culture is necessarily always historical and involves the concepts of both process and succession. In a real sense, her praxis is a process: theory informs her practical activity; this practical activity allows her to hone and refine theory; and theory permits her to devise new forms of practical activity. Fifth, her concerns are not limited to a past that is hermetically sealed off from the present. Tuberculosis and gender inequities in the sciences, two problems she has addressed in her writings, continue to have impacts on society today. In her view, it is essential to know how they came to be and how they operate today in order to change them (Buikstra 2003; Roberts and Buikstra 2003).

There are two more strands of Jane E. Buikstra's praxis that need to be emphasized. First, while I have focused on her bioarchaeological investigations in the mid-continental United States, she has also carried out research from Honduras to Chile in the Americas, well as in various parts of Europe. In every instance, she has contributed in significant ways to our understanding of the past in all the regions where she has worked and to the intellectual development of students and junior colleagues in all the countries where she has worked. The second and most important aspect of her praxis is that she is a superb mentor to students and junior colleagues. She guided her own students and junior colleagues, as well as those of others, through the often difficult early years of their careers, and she championed their visibility at this stage of their professional development by publishing important papers with them, usually as the junior author. No one can ask for more. Unfortunately, this kind of mentoring, while not unique, is all too rare in the profession today.

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2

Life in Print: The Publication Record of Jane E. Buikstra

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In 1976, at the Southern Anthropological Society meetings in Atlanta, Jane E. Buikstra first used the term *bioarchaeology* to define "a new form of regionally based, interdisciplinary research in mortuary site archaeology and human osteology" (Buikstra 1977a:69). With her paper in 1976 and the publication of an expanded version the following year, Buikstra initiated a novel research perspective in archaeology. The bioarchaeological approach is regional and diachronic in scope, is based in the analysis of populations rather than individuals, is biocultural in outlook, is explanatory rather than simply descriptive, and, above all, emphasizes answering anthropological research questions not simply archaeological or physical anthropological ones. More than anything else, the approach is concerned with understanding humanity through archaeological remains, human skeletal biology, and that uniquely social aspect of humanity, funerary and ritual behavior.

In defining bioarchaeology, Buikstra emphasized that prehistoric mortuary practices and the excavation strategies of archaeologists can have a profound effect on interpretations of osteologically derived data. In a similar fashion, biological anthropology and the analyses of osteologists can provide incredible insights to archaeologists seeking to understand the human condition of prehistoric populations. Thus it is imperative that archaeologists and physical anthropologists work cooperatively in developing research strategies. In this way, Buikstra encouraged a generation of physical anthropologists to learn the methods and theories of archaeology so as to more fully contextualize their results. Furthermore, bioarchaeology had the effect of enriching the research results of both archaeologists and physical anthropologists. In this way, the bioarchaeological approach advocates moving sterile osteological descriptions out of appendices of archaeological site reports and synthetically integrating them with archaeological interpretations. Thus the inspiration of the bioarchaeological perspective was its explicit multidisciplinary viewpoint, which brought together facets of both physical anthropology and archaeology. In this way Buikstra has significantly enhanced multidisciplinary awareness and cooperation within anthropology. Moreover, the role of bioarchaeology and bioarchaeologists in contemporary archaeology (in both academia and the private sector) is testament to the enduring impact her multidisciplinary approach has had and continues to have on the field. More than thirty years later, Buikstra's definition has fundamentally changed how archaeologists view the research potential of human remains.

Far from simply defining the bioarchaeological approach, Buikstra has been instrumental in publishing the foundational works in that approach. Her impressive publication record includes more than 150 individual articles and chapters and more than a dozen edited or authored volumes, spanning more than 35 years and covering a tremendous range of topics, including paleopathology, paleodemography, prehistoric diet, health reconstruction, soil chemistry, bone microstructure, biological distance and ancient DNA, ethnicity, forensic anthropology, mortuary ritual, and general archaeology. She has published on materials from the American midcontinent and Southwest, as well as from the Andes, Spain, and Mesoamerica. Her publications also span a wide range of contributions, including theoretical, methodological, and empirical. Finally, she has coauthored with more than 125 different people in a range of fields. Clearly, part of her influence on the discipline, in terms of publications, is her demonstrated commitment to collaborating with a wide range of scholars on an equally wide range of topics and materials. She leads here by eminent example.

Many of her publications are considered seminal works within the diverse range of her expertise. For example, her publication (with Douglas Ubelaker in 1994) of *Standards for Data Collection from Human Skeletal Remains* presciently saw the impending need for consistent, uniform data collection standards in the era of NAGPRA. However, she has also published fundamental works in paleopathology (with Della Cook in 1980) and has continued to pursue this work both on specific disease conditions (for example, her work with Charlotte Roberts on tuberculosis in 2003) and on general methods in paleopathology such as differential diagnosis (e.g., Buikstra 1977b). Her work in paleodemography (for example, with Lyle Konigsberg in 1985) has been instrumental in moving the approach through a period of stagnation and critique and into a more promising era. Starting with her dissertation and continuing throughout her career, Buikstra has been on the cutting edge of biological distance studies. This focus has continued and in recent years has moved into the complex process of ancient DNA methods. Equally consistent has been her interest in the use of chemical analysis of human bone for reconstructing prehistoric lifeways. Indeed, she has collaborated with Joseph Lambert and others to develop models of dietary reconstruction using both trace elemental and stable light isotopes data. More recently, she has been on the vanguard (Buikstra et al. 1989) in using heavy isotope ratios to identify prehistoric migration patterns. Her work with materials at the origins of agriculture revolutionized our thinking on this important transition for humanity. As noted in the citation for her election to the National Academy of Sciences in 1987,

> Buikstra developed new and more rigorous methods for assessing, from osteological remains, the health and demographic characteristics of prehistoric populations. By the application of these methods to skeletal remains she demonstrated that, contrary to current theory, intensive agriculture in prehistoric American societies often resulted in declining health and longevity.

Buikstra has had a long-standing interest in human ritual behavior, particularly mortuary practices. She has contributed centrally to each of the major mortuary archaeology volumes published in the last thirty years (Beck 1995; Chapman et al. 1981; Rakita et al. 2005). She has also contributed to the emerging interest in ethnicity in archaeology with her recent publication on Andean ethnogenesis. One final example of her influential publication record is that she literally published the book on bioarchaeology (with Lane Beck in 2006): *Bioarchaeology: The Contextual Analysis of Human Remains*. No matter which aspect of bioarchaeology a researcher might be interested in, Buikstra's publications have been foundational.

In this chapter I present my examination of the trends and patterns in Buikstra's publication record. I give special attention to the nature of her publications, be they theoretical, methodological, or empirical, and the geographic and topical coverage of each publication. I drew my inspiration for this study from Buikstra herself. Having been her student and research assistant, and now her colleague, I know that she has made use of the historical examination of publishing trends within the field. Indeed, I remember days spent in library reference sections checking the Social Sciences Citation Index and making hash marks next to lists of particular publications. She has made use of citation and publication data to examine the history and development of biological distance studies (Buikstra et al. 1990), human osteology (Stojanowski and Buikstra 2005), approaches to ritual and religion in American archaeology (Rakita and Buikstra 2008), the careers of colleagues (Buikstra and Maples 1999), and the field of bioarchaeology itself (Buikstra et al. 2003).

Data Set

To examine Buikstra's publishing record, I began with a copy of her curriculum vitae. From her vitae, I coded into a database file characteristics of each of her publications. Specifically, I entered data on all monographs, books, articles, and book chapters published by Buikstra between 1973 and 2005. I did not include book reviews or technical reports in my examination. Nor did I examine the dozen-plus publications listed as "in press," "in preparation," or "accepted for publication" on her vitae as of May 2005. Subsequently, I imported the data into the Statistical Package for the Social Sciences (SPSS). As a further window into Buikstra's publication patterns, I collated a list of all her coauthors for the 1973 to 2005 period and noted how often they had published with her. Finally, I obtained a list of all her doctoral students up to 2005 and calculated how many times each one had published with her, noting each person's sex and from which institution he or she had received a doctorate. The charts and tables presented below are drawn from this data set.

In coding each of Buikstra's publications, I examined a set of characteristics for each work (Table 2.1). To begin, I coded the year, decade, and half decade that each work was published. Second, I identified the type of publication—that is, journal article, book, book chapter, or other type of publication. I coded Buikstra's authorship role, be it editor, single author, first author of a multiple-author work, or secondary author of a multiple-author work. I coded each publication in terms of scope: whether it is primarily an empirical, methodological, theoretical, or historical contribution to the literature. In some cases, works crossed these various scopes, so a "mixed" category was included. Similarly, I made a determination of the primary topical focus of each work. I developed seven topical categories, including biodistance, chemical analysis, demography, forensics, mortuary analysis, nutrition or diet, and pathology and disease. To these were added another "mixed" category (as with the scope variable) and an "other" category. Finally, I coded the regional focus of each publication, be it European, Mesoamerican, South American, or U.S. mid-continental. (Another

Table 2.1. Basic Publishing Patterns of Jane E. Buikstra.

		1970s	1980s	1990s	2000s	Total
Type	Article	10 (66.7%)	29 (56.9%)	15 (39.5%)	15 (37.5%)	69 (47.9%)
	Book	2 (13.3%)	3 (5.9%)	3 (7.9%)	5 (12.5%)	13 (9.0%)
	Chapter	3 (20.0%)	17 (33.3%)	20 (52.6%)	19 (47.5%)	59 (41.0%)
	Other	0 (0.0%)	2 (3.9%)	0 (0.0%)	1 (2.5%)	3 (2.1%)
Authorship	Editor	0 (0.0%)	3 (5.9%)	2 (5.3%)	3 (7.5%)	8 (5.6%)
	Multiple (first author)	2 (13.3%)	12 (23.5%)	11 (28.9%)	7 (17.5%)	32 (22.2%)
	Multiple (secondary author)	6 (40.0%)	28 (54.9%)	17 (44.7%)	26 (65.0%)	77 (53.5%)
	Single	7 (46.7%)	8 (15.7%)	8 (21.1%)	4 (10.0%)	27 (18.8%)
Scope	Empirical	6 (40.0%)	20 (39.2%)	14 (36.8%)	10 (25.0%)	50 (34.7%)
	Historical	1 (6.7%)	2 (3.9%)	3 (7.9%)	4 (10.0%)	10 (6.9%)
	Methodological	3 (20.0%)	17 (33.3%)	8 (21.1%)	8 (20.0%)	36 (25.0%)
	Mixed	3 (20.0%)	12 (23.5%)	12 (31.6%)	14 (35.0%)	41 (28.5%)
	Theoretical	2 (13.3%)	0 (0.0%)	1 (2.6%)	4 (10.0%)	7 (4.9%)
Topic	Biodistance	2 (13.3%)	6 (11.8%)	3 (7.9%)	1 (2.5%)	12 (8.3%)
	Chemical	3 (20.0%)	9 (17.6%)	5 (13.2%)	3 (7.5%)	20 (13.9%)
	Demography	0 (0.0%)	4 (7.8%)	1 (2.6%)	1 (2.5%)	6 (4.2%)
	Forensics	0 (0.0%)	3 (5.9%)	3 (7.9%)	2 (5.0%)	8 (5.6%)
	Mixed topic	3 (20.0%)	17 (33.3%)	13 (34.2%)	14 (35.0%)	47 (32.6%)
	Mortuary analysis	1 (6.7%)	3 (5.9%)	6 (15.8%)	8 (20.0%)	18 (12.5%)
	Nutrition/diet	1 (6.7%)	1 (2.0%)	0 (0.0%)	0 (0.0%)	2 (1.4%)
	Other	0 (0.0%)	5 (9.8%)	1 (2.6%)	1 (2.5%)	7 (4.9%)
	Pathology/disease	5 (33.3%)	3 (5.9%)	6 (15.8%)	10 (25.0%)	24 (16.7%)
Region	Europe (Spain)	0 (0.0%)	1 (2.0%)	6 (15.8%)	1 (2.5%)	8 (5.6%)
	Mesoamerica (Maya)	0 (0.0%)	0 (0.0%)	1 (2.6%)	5 (12.5%)	6 (4.2%)
	Mid-continental (Illinois)	7 (46.7%)	17 (33.3%)	5 (13.2%)	11 (27.5%)	40 (27.8%)
	Other	8 (53.3%)	28 (54.9%)	17 (44.7%)	9 (22.5%)	62 (43.1%)
	South America (Andes)	0 (.0%)	5 (9.8%)	9 (23.7%)	14 (35.0%)	28 (19.4%)
Total		**15 (100.0%)**	**51 (100.0%)**	**38 (100.0%)**	**40 (100.0%)**	**144 (100.0%)**

Note: Percentages represent proportion for the decade.

"other" category was included here.) In all, I coded 144 publications.

For example, the well-known *Standards* volume edited by Buikstra and Douglas Ubelaker (Buikstra and Ubelaker 1994) was coded as Type: *Book*, Authorship: *Editor*, Scope: *Methodological*, Topic: *Mixed*, Region: *Other*. Her 1976 article in the *American Journal of Physical Anthropology* on Caribou Eskimo disease was coded as Type: *Article*, Authorship: *Single*, Scope: *Empirical*, Topic: *Pathology*, Region: *Other*. Finally, her 1979 contribution to the *Hopewell Archaeology* volume was coded as Type: *Chapter*, Authorship: *Single*, Scope: *Historical*, Topic: *Mixed*, Region: *Mid-Continental*.

It is important to recognize that these are my own interpretations of these publications. Other individuals, perhaps more familiar with specific publications, would code them differently. Indeed, Buikstra herself would surely disagree with some of the categories in which I have placed her publications. Moreover, by selecting only one topic, region, or scope for each publication, I am obscuring meaningful variation and nuance in many of her publications. However, I believe the data as gathered are useful for the broad approach I take here.

Basic Publishing Patterns

Table 2.1 presents frequency data for all publications in the data set for the basic categories coded. For example, most of Buikstra's publications have either been journal articles (48 percent) or chapters in books (41 percent). She has been the editor of eight books and a first or only author for 59 publications (41 percent of all her publications). In terms of publication scope, empirical, methodological, and mixed-approach works are most common. Mixed-topic publications dominate, as would be expected of an interdisciplinary bioarchaeologist who regularly integrates various biological and cultural data to understand the human condition. Finally, as she has worked longest in the U.S. mid-continent, this region is the most common geographic focus of her publications, though South America runs a close second.

A series of bivariate comparisons of the five characteristics of Buikstra's works provides further information about her publishing patterns. For example, a comparison of the frequencies of the various types of publication by scope shows that her methodological papers are more often published as journal articles. Alternatively, book chapters are the overwhelming venue for those publications with mixed scope. Cross-tabulating her authorship roles across the various publication scopes demonstrates

Table 2.2. Scholars with Buikstra Numbers of 1.

Allison, M.	Eisenberg, L.	Leigh, S.	Shater, S.
Alon, D.	Ekberg, F.	Levy, T.	Shoreman, E.
Ambrose, S.	Ericson, J.	Lozada C., M. C.	Simpson, S.
Anton, S.	Fornaciari, G.	Lull, V.	Smith, P. Springfield, A.
Arriaza, B.	Frankenberg, S.	Madden, M.	Stevens-Tuttle, D.
Asch, D.	Gale, N.	Magennis, A.	Stojanowski, C.
Aufderheide, A.	Goldberg, P.	Maples, M.	Stos-Gale, S.
Autry, W.	Goldstein, L.	Martinson, E.	Streitz, J.
Baker, B.	Goldstein, P.	McGrath, J.	Swegle, M.
Beck, L.	Gonzalez M., P.	McLaughlin, C.	Szpunar, C.
Ben Itzhak, S.	Gordon, C.	Mico, R.	Thomet, A.
Ben Yosef, A.	Grant, A.	Mielke, J.	Tomczak, P.
Blom, D.	Grigson, C.	Milner, G.	Torres P., E.
Boldsen, J.	Guhl, F.	Nystrom, K.	Twichell, E.
Bradtmiller, D.	Guillen, S.	Picazo, M.	Ubelaker, D
Braunstein, E.	Hallgrimmsson, B.	Price, T. D.	van der Merwe, N.
Breitberg, E.	Hanson, D.	Ragsdale, B.	van der Sman, P.
Bullington, J.	Ho, J.	Rakita, G.	van Vark, G.
Burgi, P.	Holcomb, T.	Rathbun, T.	van Nest, J.
Burton, J.	Holl, A.	Reinhard, K.	Vinh, T.
Cartmell, L.	Hoshower, L.	Renier, C.	Vlasak, S.
Casto M., P.	Indriati, E.	Rice, D.	Webster, A.
Chapman, R.	Janusek, J.	Richtsmeier, J.	Weems, C.
Charles, D.	Johnson, D.	Risch, R.	Weiner, S.
Cheverud, J.	Jones, M.	Roberts, C.	Weydert, J.
Clark, N.	Keng, L.	Rose, J.	Wilbur, A.
Condon, K.	King, J.	Rosen, S.	Williams, S.
Conrad, G.	Knudson, K.	Rowan, Y.	Wittmers, L.
Cook, D.	Komar, D.	Ruiz, M.	Wright, L.
DeRousseau, C. J.	Konigsberg, L.	Sabari, P.	Xue, L.
Dijema, J.	Krueger, H.	Salo, W.	
Dittmar, K.	Lambert, J.	Sanahuja, M.	

that on empirical and methodological papers, she is often a secondary author, while she is a single or first author of theoretical or historical papers. Moving on to type of publication by topic, Buikstra's publications dealing with biodistance, chemical analysis, forensics, and disease are frequently journal articles. In contrast, mixed-topic and mortuary analysis papers are more likely to be chapters in books or books themselves. Only two publications were recorded as dealing with nutrition or diet, and both of these were book chapters. In terms of authorship by topic, she was overwhelmingly a secondary author on publications dealing with biodistance and chemical analysis. As to type of publication by region of focus, there is a wider range of publication types in those regions where Buikstra has been working longest, especially the U.S. mid-continent and South America. Comparing topical focus and region, there is also a great variety of publication topics for these two regions, in contrast to those publications dealing with Mesoamerican and European materials.

Temporal Trends

Table 2.1 also breaks down these various publication characteristics over time, specifically decades. The table provides a temporal perspective on Buikstra's work. For example, book chapters have been a significant portion of her publication types over the last three decades. Indeed, in the last two decades, they represent approximately half of all her publications. Journal articles were the largest proportion of her publications during the 1970s and 1980s. Their frequency within the last two decades has declined, but they still remain a large portion of her publications (more than 35 percent in both decades). Books are becoming an increasing portion of her written works, with the 1990s seeing her publish five, amounting to 12.5 percent of her publications that decade. In terms of authorship, while edited books and single-author works have always been a portion of Buikstra's publications, what stands out most significantly is that she has definitely been a collaborative publisher, whether as a first or secondary author on multiple-author works. Multiple-author works have constituted more than 70 percent of her publications in the last three decades.

In terms of scope, Buikstra has always been a proponent of publishing empirical data; she sees this as one of the most fundamental contributions she can make to the discipline. In each of the four decades, empirical reports represented at least one-quarter of all her publications, and even more during the 1970s and 1980s. Methodological works are also a perennial aspect of her publishing. At least one-fifth of her publications were methodologically focused; in the 1980s this focus jumped to one-third. Buikstra's theoretical publications cluster in the 1970s, at the beginning of her career, and in the current decade. Her historical publications have witnessed a steady increase through time. As expected for an interdisciplinary researcher, her publications of a mixed empirical, methodological, historical, and theoretical scope represent anywhere from one-fifth to one-third of her publications in any given decade.

The most interesting trends, to my view, are those of the topic and regional focuses of Buikstra's publications. Topically, there are numerous trends, and these are shown best in Figure 2.1, which displays counts of her various publications by topic across the four decades. Chemical and biodistance studies saw their peak in the 1980s, yet these topics still represent a significant part of her work. Demographic studies were also most common during the 1980s. Over the past three decades, she has maintained a publishing agenda in the forensic sciences. Publications coded as dealing with nutrition and diet, on the other hand, have been absent for the past twenty years. This may be an artifact of my coding of certain works or it may be that Buikstra's paleodietary work is embedded within those publications coded as having a mixed topic. These mixed-focus publications are by far the most common, especially in the past three decades. Mortuary analyses and publications on diseases or pathologies are on the rise, reaching their highest frequency in the 2000s.

Figure 2.2 displays counts of Buikstra's publications broken down by regional focus and decade. Given her early interest and work in the lower Illinois River valley, it is not surprising to see that she has always been active in publishing on the U.S. mid-continental region. In particular, the 1980s and 2000s show surges of publications related to this region. Clearly, she has not had her final say on prehistoric Hopewell and Mississippian cultures. Since beginning work in southern Peru, she has also had an increasing role in Andean archaeology. Her publication record in this regard shows an ever-increasing publishing agenda. Publications relating to Spanish materials peaked in the 1990s, highlighting her involvement in research related to Bronze Age settlements in southern Spain. Finally, the 2000s show a sharp upswing in publications related to the Mayan

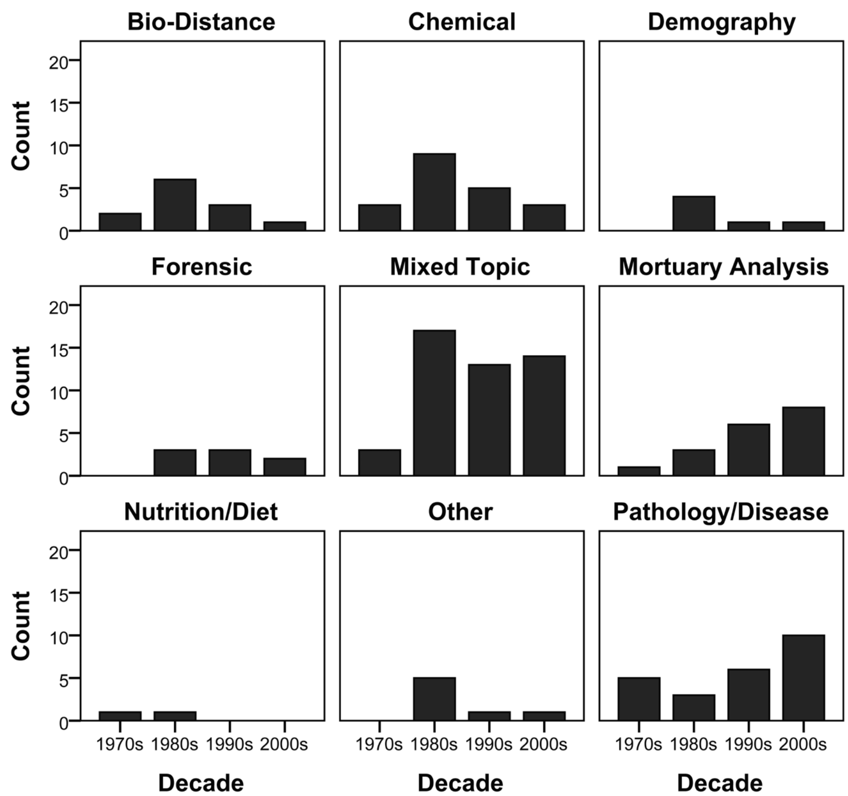

Figure 2.1. Counts of Buikstra's publications by topic and decade.

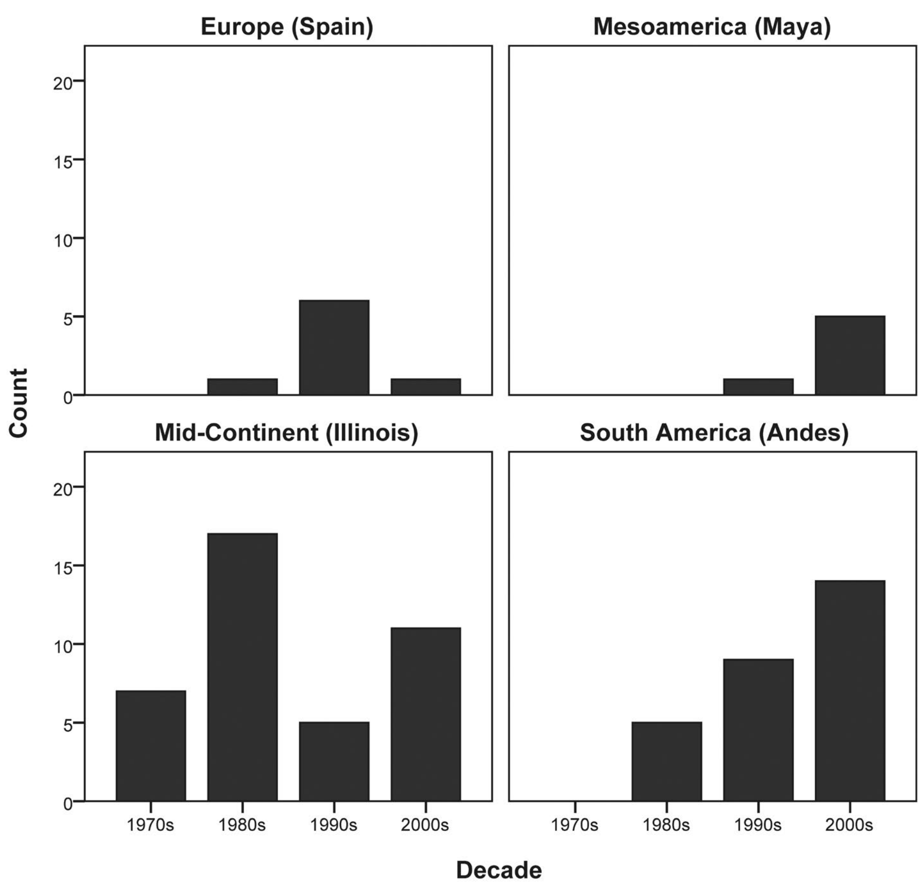

Figure 2.2. Counts of Buikstra's publications by region and decade.

region, illustrating Buikstra's more recent engagement in research in that area.

Collaborating and Coauthoring

More than 75 percent of Buikstra's publications explored in this study were multiple-author works. By May 2005 she had coauthored with 127 different people. Clearly part of her influence on the discipline, in terms of publication, is her willingness to collaborate with a wide range of scholars on an equally wide range of topics and materials. To casually explore this aspect of her publishing, I would like to introduce a numerical publishing statistical method.

Here I suggest the "Buikstra number"—that is, a simple count of how many times an individual has published with Buikstra (Tables 2.2 and 2.3). High Buikstra numbers indicate a stronger publishing connection with her. Table 2.3 provides lists of scholars with Buikstra numbers over 2 as of May 2005. For example, Paula Tomczak has twice coauthored with Buikstra; thus her Buikstra number is 2. Joseph Lambert has the highest Buikstra number at 15, with Doug Charles as the second most frequent coauthor with 14.

These Buikstra numbers also allow us to assess a variety of other aspects of Buikstra's coauthoring. For example, the data show that publishing a book with her is slightly predictive of a higher number of coauthored articles or chapters. Those who have coauthored or coedited two books with Buikstra have an average of 8.5 other publications with her; those who have published one book with her have an average of 3.00 other publications; and those who have not published a book with her have an average of 1.96. Buikstra seems to publish more frequently with her Ph.D. students, although there are exceptions. Those who coauthored with Buikstra and were her doctoral students published with her an average of 3.68 times. Nonstudent coauthors, on the other hand, published with her an average of 1.91 times. These data can also be used to explore how much she has published with her students from the various institutions at which she has taught. The data show that on average, she published slightly more often with Northwestern (2.47) students than with her New Mexico (1.75) or Chicago (1.00) students. However, these averages are being draw upward by outliers such as Doug Charles, Sloan Williams, Lyle Konigsberg, María Cecilia Lozada Cerna, and Deborah Blom because of multiple collaborations. Thus a more accurate estimate would be the median number of coauthorships for students from each school. In this case, New Mexico ranks first (1.50), followed by Northwestern (1.00) and finally Chicago (.00). It should be noted that she also had many more students at the latter two institutions, and many of them did not coauthor with her, thus decreasing both means and medians for these samples. Additionally, I examined whether she was more likely to have published with male or female students. A student's T-test and a nonparametric Mann-Whitney Test do not indicate a statistically significant difference between the number of publications she coauthored with females and males. Indeed, of the twelve former students who have the most coauthored publications with Buikstra (two or more publications), six are females and six are males.

Table 2.3. Scholars with Buikstra Numbers over 1.

Buikstra Number > 3	**Buikstra Number of 3**	**Buikstra Number of 2**
Lambert, 15	Arriaza	Alon
Charles, 14	Bullington	Braunstein
Cheverud, 10	Casto M.	Cartmell
Aufderheide, 8	Chapman	Eisenberg
Konigsberg, 7	Clark	Goldberg
Williams, 7	Condon	Grigson
Cook, 6	Frankenberg	Hanson
Lozada C., 6	Goldstein	Holl
Simpson, 6	Gonzalez M.	Keng
Blom, 5	Lull	Knudson
Nystrom, 5	Picazo	Levy
Szpunar, 5	Price	Roberts
Gordon, 4	Rakita	Rose
Hoshower, 4	Reinhard	Salo
Milner, 4	Risch	Smith
Xue, 4	Sanahuja	Tomczak
	Springfield	Torres P.
	Stojanowski	Van Nest
		Weems

Conclusions

The publications listed on Buikstra's 22-page curriculum vitae demonstrate the incredible quantity, scope, and scale of her scholarship in bioarchaeology. Her wide-ranging list of works spans the full breadth of bioarchaeological topics, including chemical analysis, mortuary practices, paleodemography and paleopathology, forensic anthropology, and biodistance studies. Her work deals with data from three continents. She has been equally comfortable making empirical, methodological, theoretical, and historical contributions. She has more than 140 publications to her credit, with more than 125 coauthors.

However, far from simply publishing in the field, Buikstra has nurtured the bioarchaeological approach. She has consistently sought funding for her research and collaborative efforts. Her curriculum vitae shows that she has served as principal or co-principal investigator on 14 separate National Science Foundation research grants (from both the archaeology and physical anthropology directorates). She has also been a supervisor on more than a dozen NSF dissertation improvement grants. She has received funding from the Wenner-Gren Foundation, the National Geographic Society, and the Federal Emergency Management Agency, to name but a few. As successful grant proposals are reviewed by disciplinary peers, Buikstra's success in this regard is a tangible measure of the approval of her research strategies by fellow anthropologists. Moreover, the financial support these various grants provided allowed her and her colleagues to develop novel methodological approaches and to bridge disciplinary boundaries.

Buikstra's impact upon the field of archaeology and her nurturing of multidisciplinary cooperation perhaps can best be seen in the students she has trained. In her recent remarks upon receiving the American Association of Physical Anthropology's Darwin Award, she noted that it was her students who have most influenced her career. She has advised 42 students through their Ph.D. studies. While each student's specific research may fall within either the archaeological or physical anthropology subfield, each has been trained to be a multidisciplinary researcher. Each is continuing in that same vein to train the next generation of bioarchaeologists. Indeed, it is testament to her influence that she currently works collaboratively with Anne Stone (at Arizona State University), who is herself a student of George Milner (Buikstra's fourth Ph.D. student).

Buikstra has also contributed throughout her career with dedicated and consistent service to both the public and the four subfields of anthropology. Her curriculum vitae shows her service to the American Anthropological Association in a variety of capacities, including as president from 1989 to 1991, shepherding the association through turbulent times. She has likewise served as president of the American Association of Physical Anthropologists (1985 to 1987), the Paleopathology Association (2003 to 2005), and the Illinois Archaeological Survey (1977 to 1978). She has served on the American Association for the Advancement of Science's nominations committee and on the ethics committee and executive board of the American Board of Forensic Anthropology. She served on the board of directors of the Archaeological Conservancy from 1986 to 1991 and on the search committee for the executive director of the Society for American Archaeology in 1992. She has also served as a forensic consultant for numerous local medical examiner offices and has served as an external reviewer for more than 25 academic departments. She has served on the editorial boards of more than seven professional journals, including the *Journal of Anthropological Research*, the *Journal of Forensic Anthropology and Archaeology*, the *International Journal of Osteoarchaeology*, *Evolutionary Anthropology*, and the *Journal of Anthropological Archaeology*. She was a founding member and served on the board of directors of the Society for Professional Archaeologists (now the Register of Professional Archaeologists) and was recently the founding editor of the new *International Journal of Paleopathology*.

Perhaps most illustrative of her nurturing both archaeology and bioarchaeology is her involvement with two important research centers. She is the director of the Center for American Archaeology in Kampsville, Illinois, and the founding director of the Center for Bioarchaeological Research at Arizona State University. Buikstra sees the CBR as an interdisciplinary anthropological endeavor with strong links to the physical and natural sciences, biomedicine, the social sciences, and the humanities. When asked about the CBR she stated, "I am very excited about building a research center focused upon issue-based bioarchaeological investigations. One of our challenges is to bring knowledge of the deep past to problems that face humankind today and tomorrow" (Rakita 2005:10).

Buikstra's career defining, building the foundations of, and nurturing the interdisciplinary field of bioarchaeology exemplifies the important spirit and role of multidisciplinary cooperation in archaeology. Her work has significantly increased the awareness and application of interdisciplinary research in archaeology. Buikstra once was described (in a series of biographies of past AAA presidents) as "remain[ing] committed to the breadth of anthropology as a discipline whose core value is tolerance—both of multiple approaches within the field and of the diversity of humankind" (Darnell 2002:284). Her call for "mutually designed research strategies" in which bioarchaeologists are able to develop and implement research that uses theories and techniques from archaeology, physical anthropology, and numerous other scientific disciplines and that seeks to answer broadly anthropological questions is one of the most significant contributions to interdisciplinary archaeology in America. Her definition of the approach, her fundamental publishing in the field, and her continued meaningful service to the field have extensively increased awareness of interdisciplinary studies in archaeology.

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3

Grave Concerns:
The Intersection of Biological and Social Approaches to the Archaeology of Cemeteries

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When Jane E. Buikstra was conducting her graduate research in the early 1970s, it seemed quite natural to analyze all aspects of cemeteries from the perspective of a comprehensive Darwinian metaphor. One domain was straightforward human biology; the other envisioned culture as part of the human phenotype, an "extrasomatic means of adaptation." Illustrative of this initiatory period of bioarchaeology are the title of the published version of Buikstra's dissertation, *Hopewell in the Lower Illinois Valley: A Regional Study of Human Biological Variability and Prehistoric Mortuary Behavior* (Buikstra 1976), and its chapter headings: "Introduction," "Age and Sex Determination," "Demography," "Status and Social Complexity," "Biological Distance," and "Summary and Conclusions." In the work of one researcher, we have biological analysis of skeletal remains (chapters 2, 3, and 5) and sociocultural analysis of funerary practices (chapter 4).

During the course of the last quarter century, cemeteries have increasingly been approached by one of two metaphors. Darwin is still with us when it comes to the skeletal material, but the sociocultural elements of mortuary behavior—in particular, the *meanings* of facility preparation, grave goods, and the spatial organization of a cemetery—are being explored by many archaeologists through a metaphor of interpretive sociology ultimately derived, directly or indirectly, from Marx. The use of these two metaphors has created a tension in the analysis of cemeteries. Should we seek to redefine a single framework, or is a dual approach preferable, or even possible?

Biology

In the preface to the recent volume *Bioarchaeology: The Contextual Analysis of Human Remains* (Buikstra and Beck 2006), Buikstra (2006a:xvii) asks, "What is 'bioarchaeology?'" The term was first used in Great Britain in the title of Graham Clark's (1972) *Starr Carr: A Case Study in Bioarchaeology*, where it referred to the study of archaeologically recovered biological materials (such as animal bones, mollusk shells, and plant remains). The term independently surfaced in the United States five years later in a volume titled, with a more or less synonymous first word, *Biocultural Adaptation in Prehistoric America* (Blakely 1977), which in turn echoed the title of an earlier paper by Van Gerven and coauthors (1974). In her contribution, Buikstra (1977:69) defined bioarchaeology as a multidisciplinary approach by which to address questions such as social organization, geography of genetic pools, diet, population density, health, and levels of interpersonal violence. She particularly emphasized the joint participation of human osteologists and archaeologists in all phases of research into cemeteries, even, as in her own practice, to the extent of the osteologist being the archaeologist. Thus *bio* meant biological anthropology in either terminology, while *archaeology* (or *cultural*) invoked the study of past cultures. By the early 1980s in Britain, the term *osteoarchaeology* had gained currency, referring to the study of both human and animal remains from archaeological contexts (Roberts 2006). For other perspectives on the development of these terms, see Armelagos (2003, 2008).

More recently, Larsen's (1997, 2002, 2006) conception of bioarchaeology has been widely adopted. For him, bioarchaeology is the study of past human biology, incorporating methods and specialists from other disciplines, such as geology, chemistry, physics, and engineering—although not necessarily archaeology. The involvement of archaeologists tends to end with their excavation of the skeletons (Goldstein 2006). Goldstein (2006:376) further notes that bioarchaeology has returned to a more explicit physical or biological anthropology, becoming "more science and laboratory oriented." Not surprisingly perhaps, Sofaer (2006) wishes to reunite osteology and archaeology, not as a collaborative enterprise in Buikstra's sense but as a synthetic "theoretical osteoarchaeology." While one can sympathize with Sofaer's (see also Goldstein 2006) frustration with the interpretative divide between a biologically based osteology and a humanities-derived interpretative archaeology, there are other possible resolutions.

Running through this brief history of conceptions of bioarchaeology (or biocultural study or osteoarchaeology) are several obvious dualities: American versus British disciplinary histories; scientific versus social or cultural analytical approaches; human versus animal. More broadly conceived, these oppositions reflect the deep structure of Cartesian dualism that purportedly haunts Western thought: science versus humanities; process versus structure; explanation versus understanding. Astuti (2001:430), directly addressing this phenomenon and the attempts to counter it, adds:

> consciousness vs. unconsciousness, thought vs. emotion, object vs. subject. In anthropology, of course, the mind-body dichotomy has been cast in terms of a dichotomy between culture and biology, or the many derivatives, such as the distinction between sex and gender, person and organism, individual and society. There are, therefore, as many anthropological claims for "monism" as there are versions of dualistic reasoning.

ARCHAEOLOGY

The history of funerary archaeology over the past 50 years parallels the more general history of Anglo-American archaeology. During roughly the first half of that period, prior to 1980, one grand theoretical framework sufficed for both biological and sociocultural interpretations of cemeteries. In the 1950s, the neo-Darwinian synthesis drove the transformation of physical anthropology into biological anthropology, instigated in large part by Washburn's (1951, 1953) call for a "new physical anthropology" based on population biology rather than typological approaches. Shortly thereafter, in the 1960s, Lewis Binford and others (Binford 1962; Binford and Binford 1968; Clark 1968) initiated what came to be called the New Archaeology (or processual archaeology), rejecting the classification focus of culture-historical archaeology typified by the work of James B. Griffin and others (e.g., Griffin 1952). Central to New Archaeology was White's (1949) concept of culture as *Homo sapiens*' "extra-somatic means of adaptation." The underlying paradigm or metaphor for both biological anthropology and processual archaeology was derived from Darwin's theory of evolution by natural selection. Integral to this project were concepts such as ecology, systems theory, sampling and statistical distributions, and the hypothetico-deductive model, in addition to adaptation (Johnson 2010; Trigger 2006; Willey and Sabloff 1993).

The archaeology of cemeteries was defined in this period by two works: Arthur Saxe's (1970) unpublished Ph.D. dissertation from the University of Michigan, "Social Dimensions of Mortuary Practices," and Binford's (1971) article "Mortuary Practices: Their Study and Their Potential." In line with the adaptive systems model of culture underwriting the New Archaeology, Binford argued that mortuary customs were not "random" culture-historical phenomena. That is, variation in funerary practices among different societies did not result from historical accidents. Patterns in burial treatments were systematically related to other variables. Based on the analysis of a cross-cultural sample of 40 societies classified in terms of organizational complexity as hunters and gatherers, shifting agriculturalists, settled agriculturalists, and pastoralists (following Murdock 1957), Binford (1971:23) concluded that (1) the number and (2) the specific dimensions of the *social persona* varied with social complexity, and (3) the "forms which differentiations in mortuary ritual take vary significantly with the dimensions of the *social persona* symbolized." Saxe's (1970) theoretical framework was more elaborately developed, but his sample included only three societies. His first seven hypotheses are largely elaborations on Binford's conclusions cited above. Saxe's (1970:119) Hypothesis 8—that the extent to which corporate control of resources is tied to lineal descent is correlated with the probability that formal places for the dead will be established—points in another direction (and has taken on a life of its own). The Saxe-Binford approach dominated mortuary archaeology in the United States for two decades.

Beginning in 1980, the Saxe-Binford model became a focal point for a postprocessual critique of archaeological theory and practice (e.g., Hodder 1980, 1982; Pader 1980, 1982; various authors in Hodder ed. 1982). Parker Pearson (1999) provides perhaps the most comprehensive Saxe-Binford critique and postprocessual reworking of an archaeology of death. The range of postprocessual approaches to funerary analysis does not lend itself to concise characterization, but early illustrations include Shanks and Tilley (1982) and Hodder (1984). Recent examples are seen in Arnold and Wicker (2002) and Gowland and Knüsel (2006b). As Parker Pearson concludes (1999:197):

> Archaeology has grown beyond attempts to explain all human life according to universal laws of behaviour. We now move towards the understanding and explanation of the past through more subtle ideas about human experience and perception. Archaeology can range across space and time to excavate and understand the many paths we make but, immanent in the diversity of life, there is one universal—death. We are all ultimately going in the same direction.

But are we—at least in the shorter haul?

Darwin and Marx

Darwin wrote *The Origin of Species* in England, and its subsequent development was predominantly in the English-speaking world. The Darwinian metaphor has been a strong presence in archaeology, especially during the last half of the twentieth century, notably in the processual concern with adaptation in the 1960s and 1970s (e.g., Binford 1962, 1968) and in the "roots" movement of the more recent selectionist approach (e.g., Barton et al. 1997; Dunnell 1980; O'Brien and Lyman 2000). In his later life, Marx also worked in England, although he wrote in German and was inspired by continental European thought. The Marxian metaphor is the more transformed of the two, having drifted from an early focus on materialism through the ideological bent of the Frankfurt School of critical theory to, in archaeology in particular, a concern with agency, meaning, context, the notion of material culture as text, and, most recently, phenomenology (e.g., Bender 1998; Hodder 1986; Shanks and Tilley 1987). Marxism occasionally inspired Anglo-American archaeology—the historical materialism of V. Gordon Childe in England in the early twentieth century and more subtly the incorporation of Leslie White's thought into the work of Binford and others (Patterson 2003; Trigger 2006). In general, however, the Marxian tradition resided on the continent, and it became a significant component of Anglophone archaeology only after 1980 (e.g., Hodder 1982b).

In practice, the antithetical positions that define much of Western thought have taken the form of the Darwinian and Marxian paradigms, or the biological and interpretative sociology metaphors, as Giddens (1984:1–2) identifies them, as expressed, for example, in the processual/postprocessual debates in Anglo-American archaeology in the final decades of the last century. The Darwinian metaphor is characterized by atomistic, ahistorical, abstract deductive, and prescriptive modes of Enlightenment thought; the Marxian metaphor embodies anti-Enlightenment sentiments, with holistic, historical, particularistic, and descriptive concerns (Charles 1992:909–910; see also Bloor 1976:54–57; Gibbon 1989:129–134). This paradigmatic opposition of Darwin and Marx developed in the twentieth century as others used their writings. Materialism and evolutionary change were central to both Darwin's and Marx's thought, and they did not see their work as antithetical (Foster 2000). Marx's conceptions are as modernist as Darwin's, and much of what now differentiates the two metaphors lies in postmodernist revisions of Marxian thought.

Questions thus arise: Are the Darwinian and Marxian metaphors now mutually exclusive? If this is the case, how does (or can) one researcher effectively shift between these two very distinctive manners of thought? Is the individual successfully compartmentalizing the use of the two metaphors, or are we witnessing cases of dissociative identity disorder, the condition associated with *Sybil* and *The Three Faces of Eve*? Alternatively, if the incompatibility of Darwinian and Marxian approaches has evolved over the course of the twentieth century, does a resolution lie in a new way of viewing Darwin and Marx?

To make what should be a very long discussion very short, we confront the distinction between nature and culture. In particular, we face the question of whether or not human social life can be studied scientifically. A Darwinian would answer yes; a Marxian would reply in the negative.[1] In the most extreme constructivist formulations of the latter, science in fact loses its privileged claim to knowledge. Not only is it inappropriate to apply Darwinian models to social behavior, it is problematic to use what can be argued to be a nineteenth-century economic model to describe nature. It was my attempt to imagine a constructivist of this ilk even engaging in a biological analysis of a skeletal population that led me to consider the relevance of dissociative identity disorder. Conversely, is a postmodern theory of evolution possible?

Biology and Archaeology

Sofaer's (2006) goal is to unite osteoarchaeology and interpretative archaeology as a singular "theoretical osteoarchaeology." Osteoarchaeology is defined as the study of human bones alone and, significantly, "should be taken to include biological and physical anthropology" (Sofaer 2006:xvi). Interpretative archaeology is invoked in the sense of Hodder (e.g., 1991:15–16)—that is, an archaeology of meaning incorporating the material (real) nature of the past; a privileging of "emic" (not the term used by Hodder) perspectives but thereby making meaning local and public; and self-reflexivity and dialogue. Sofaer covers a wide range of topics—too wide to discuss in this chapter. I will focus on the main thrust of her argument: that there should be a theoretical archaeology, a synthesis of osteoarchaeology and interpretative archaeology.

Sofaer (2006:31–61) describes the differences between a science-oriented osteoarchaeology and a humanist-oriented interpretative archaeology (Table 3.1). The culminating nature–culture opposition mirrors the dualisms noted earlier in this chapter. She opposes an atheoretical osteoarchaeology against a theoretical interpretative archaeology. Gowland and Knüsel (2006a:ix) propagate this same formulation where they discuss science and *social* theory, but in a section actually titled "Science/theory divide." From this perspective, osteoarchaeology as science means that it is empirical and technical: "The osteological specialist is seen as being a dry technician . . . on the basis that the study of the physical body is mechanical in nature and executed according to a prescribed 'recipe book' of ideas and methods" (Sofaer 2006:33). This may seem an apt characterization in describing the relations between osteoarchaeologists and archaeologists in the United Kingdom and for that matter in the United States (Goldstein 2006). Larsen's (1997, 2002, 2006) view of bioarchaeology certainly reinforces this notion. To the extent that this is true, Sofaer's unification project is well considered. At the same time, as noted, Sofaer collapses biological/physical anthropology into osteoarchaeology. I (and I suspect most American biological anthropologists) view osteology (which would include its application to mortuary archaeology and its overlap with paleopathology) as one aspect of biological anthropology, alongside studies of human variability and adaptation, human and primate evolution, and primatology. In other words, biological anthropologists are generally and more appropriately perceived as biologists than as anthropologists. (In the United States, anthropology also includes archaeology and, less so now, linguistics.) This is so much the case that the lack of communication Sofaer sees between osteoarchaeologists and archaeologists really extends across anthropology and has led to severe rifts in the discipline in recent years (Holden 1993). This is not due to the fact that biological anthropologists are atheoretical, however. The failure to communicate relates to the fact that biological anthropology is highly theoretized, but it is Darwinian theory, not Marxian.

Table 3.1. Dualities of Osteoarchaeology Versus Interpretative Archaeology

Osteoarchaeology	**Interpretative Archaeology**
Atheoretical	Theoretical
Dead	Living
Inside (unfleshed)	Outside (fleshed)
Nature	Culture

Note: After Sofaer 2006:31.

Some sort of biocultural rapprochement is clearly in order. A dialectical and political economic approach is one means (e.g., Goodman and Leatherman 1998). Sofaer's move is to absorb osteoarchaeology into theoretical archaeology by dissolving the distinction between bodies (people) and objects. This is accomplished through a focus on the materiality of the body, particularly the skeleton, in an archaeological context (Sofaer 2006:64). While I have little argument with archaeologists "borrowing" bodies for their interpretative endeavors, Sofaer's project is part of a broader attempt to transcend a perceived Western tendency toward dualistic constructions. The Cartesian mind/body duality is problematic, particularly as it invariably leads to hierarchy, but attempts to negate it tend to be prescriptive rather than descriptive.

Cast about for attempts at monistic revisions of theoretical approaches to human life, and they are filled with dualistic terminology. Latour (1993) seeks to redefine modernity, which at its base rests on various distinctions: modern versus premodern specifically, but ultimately meaning culture versus nature, as in modern "cultured" Europe of the eighteenth and nineteenth centuries versus those "natural" societies encountered in the course of European expansion. In his conception, modernity rests on *two* dichotomies: nonhumans/nature versus humans/culture and purity versus hybrids. We (as moderns) conceive of things as either natural or cultural while at the same time refusing to acknowledge the cultural construction of "natural" objects, the natural constraints on "cultural" constructions, or, to make it symmetrical, the interpenetration (but with retained identity) of the natural and the cultural. In the latter case, in a chemical analogy, we would see mixtures, not solutions. Latour contrasts this modern outlook with the worldview of premoderns, who are very aware of the "danger" of hybrids and who monitor their creation and existence: magic, trees and rocks with souls, spirits that inhabit everyday objects, dead ancestors present at ceremonies of the living. Likewise, Franklin (2002) sees not a separation between nature and society but a deep interpenetration—but again, mixture not solution. Ingold (1998) is bent on dissolving the boundaries between social anthropology on the one hand and biological anthropology, archaeology, and psychology on the other. In this effort, he systematically discusses each pair, never actually developing an integrative terminology. Each of these endeavors, and the many others one could cite, are laudable. Given their aims, why are they never able to transcend the dualities?

Underlying Latour's, Franklin's, and Ingold's conundrum is the presumption that we are needlessly imposing Western dualisms on what should be unitary objects or processes. It may not be, however, a "needless" imposition. Dualistic thought may not be an exclusively Western phenomenon; it may instead be in the nature of being human, whether Western or non-Western (e.g., Astuti 2001; Bering 2006). Astuti (2001) specifically addresses the issue of whether or not dualistic thinking is confined to Western people, the heirs of Plato and Descartes. Cognitive experiments involving inferential reasoning by the non-Western Vezo of Madagascar, children and adults, indicate that, in contrast to their normative accounts, which appear to be devoid of dualism, the Vezo reason within a dualistic framework. Astuti is well aware of the implication for traditional ethnographic methods that follows from her findings: People's descriptions of what they think may not be accurate renderings of how they think.

Gell (1992), building on McTaggart (1908), distinguishes between A-series and B-series time and thereby lays a foundation for understanding the dualism inherent in biological versus interpretive approaches to cemeteries. Essentially, A-series time is that experienced by the individual, characterized by an anticipated future, a present, and a remembered past. This is a dynamic relationship, however, as anticipated future and remembered past structure the present. Time is experienced as passage, the world and the self as becoming. An event can be in the future; it can be now; it can be in the past. And it will be all those things. B-series time relates to calendrical reckoning: an event has a unique temporal assignment, and it is always before, at the same time as, or after another event with a different date. Time is about being (at a particular point on a scale). "Truth" cannot exist in A-series time. An event may be in the future, but at another time it will be in the past. In the B-series, an event that occurs on May 25, 1762, will always be tied to that date (barring, of course, a revision [translation] of the calendar). For example, for Bourdieu (1977), working in the Marxian tradition, time is A-series:

> It is essential to note the deep affinity between the A-series concept of time, and the concept of time implicit in the Marxist historical dialectic of the production of man by history and the production of history by man. This "history" is not the unchanging, inaccessible, B-series "past," but a past that is dynamically interconnected to the present, and that changes as the present changes. It is not a simple accretion of new events (changes) at the temporal front-line demarcated by the "now"; instead the change initiated in the now-moment occurs in depth. The whole history changes as the present changes, because of the continual interactions of historical residues in the current situation, which is centered in the "now," but which embraces the past and the future as well [Gell 1992:267].

Note, however, that the "accretion of new events (changes) at the temporal front-line demarcated by 'now'" reasonably describes Darwinian evolution. For a Marxian, time is A-series; for a Darwinian, time is B-series. To identify—to discuss an object, a process, a person, an event—we must place it in time (and space, but that is another matter), and to place it in time, we have to choose a time series—A or B. In other words, to study a phenomenon one must first commit to a time perspective, and to do that commits one to one side of a duality.

Things are not that simple, of course. Darwinian models—evolutionary archaeology, evolutionary psychology, sociobiology—readily offer B-series accounts of broad ranges of human behavior. Likewise, an A-series perspective can be brought to bear in a dialectical model of evolution (Levins and Lewontin 1985), and much of the inspiration for the structure of change incorporated in punctuated equilibria models (Eldredge and Gould 1972; Gould and Eldredge 1977) sprung from familiarity with the Hegelian and Marxian dialectic (Gould 2002:1017–1018). Nonetheless, the distinctness of A- and B-series time remains. A-series time is that experienced by the conscious individual or "experienced" by the evolving organism. The latter case refers, for example, to situations where a morphological modification creates a new niche with new selection pressures or where a structure selected for at some time in the past becomes a constraint on subsequent change in the face of altered selection pressures. B-series time is a method of measurement imposed from outside an event—that is, by the conscious person viewing an external process or sequence of events. Thus fossils and potsherds are pegged to particular times. Dinosaurs that lived in the Jurassic will always have lived before there were any *Homo sapiens* on the planet.

Conclusion (in the Mesolithic)

One area of common interest in which the distinction between explanations arising from social theory and those based in biology—that is, between A-series and B-series time frames—becomes paramount is the investigation of hominid evolution and early human history. The study of primate evolution is the same as the study of the evolution of any other mammalian group, or for that matter any animal group (or group of organisms from any of the kingdoms of life on earth). Dialectical biology and punctuated equilibria are revisions of, or additions to, Darwin's theory, not substitutes. Evolutionary psychology can provide some insight into human behavior, but in the end, it primarily produces ideologically embedded just-so stories. Evolution happened, and people are different from other animals. As yet, we do not have a theoretical framework from which to talk about both concepts. To understand Australopithecines we need modern evolutionary theory. To understand the situation in Iraq in the first decade of the twenty-first century, we require an array of postmodern perspectives. Between Olduvai Gorge and Babylon there is a huge gray area not served by either theoretical framework alone.

This tension plays out in the Mesolithic, or in its equivalent in eastern North America, the Archaic and Woodland periods. For Buikstra (1977, 2006a), biological theory is essentially the same.[2] Archaeological theory has changed, however:

> Since it has been repeatedly demonstrated (e.g., Saxe 1970; Binford 1971) that the mortuary activity associated with the individual directly reflects the sum of his socially defined roles (though certain roles may assume primacy), the analysis of Middle Woodland mortuary sites should focus upon (1) a definition of the range of burial behaviors appropriate to the individual at death and (2) the extent to which this variability is readily explicable in terms of age, sex, and other biological attributes. Then, the degree of complexity thus defined may be compared to models derived through the study of extant populations [Buikstra 1976:30].

Compare that to a recent passage (in Buikstra's words, although from a coauthored work) that invokes a very different sense. Note the shift from objective and prescriptive language in the previous quotation to an emphasis on a subjective view, and note the qualifying words like *may* and *interpreted*.

> Later mounds include "empty" tombs and are positioned near open spaces where audiences may have viewed the rituals staged from elevated ramps that encircled tomb structures. . . . These rituals served to anchor the Middle Woodland world, moving the dead across lofty platforms representing the upper world, through the flat disk of this world, into the dark, subsurface underworld. A thin layer of light-colored sediments encircling the tomb at ground level is interpreted to represent this world [Buikstra and Charles 1999:214].

So what is, or should be, bioarchaeology? Good question.

Notes

1. It is worth remembering, however, that Marx himself was attempting to develop a science of historical materialism, a scientific socialism (Tucker 1978:xx).

2. Somewhat ironically for a "postprocessual" theoretical osteoarchaeology that would treat bones as objects, in North America NAGPRA has greatly altered bioarchaeological practice (Buikstra 2006b; Martin 1998). The treatment of skeletons as archaeological objects was seen as desecration and was deeply insulting to Native Americans. Biological anthropologists have had to develop cultural sensitivities not normally associated with "science."

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4

Methodological and Ethical Considerations When Sampling Human Osteological Remains

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We highlight several considerations that must be made when embarking upon destructive analyses of human remains. We view this as a step toward the creation of widely implemented standards within this particular methodological branch of bioarchaeological research. Thus it is fitting that this work is situated in a volume dedicated to Jane E. Buikstra, who, with her colleague Douglas Ubelaker, published the indispensable and oft-cited book *Standards for Data Collection from Human Skeletal Remains* (Buikstra and Ubelaker 1994). *Standards*, as it is known in shorthand throughout the discipline, integrated relevant methodological techniques compiled by a multidisciplinary team of experienced researchers and graduate students. That standardized compilation has contributed to more reliable comparisons of skeletal data sets collected by different researchers.

In its nine printings, *Standards* has found its place in anthropology and criminal justice departments in North, Central, and South America; Europe; and the Middle East (Deborah Sabo, Arkansas Archeological Survey Publications, personal communication 2010). We do not presume that this short paper is within the realm of the comprehensive volume created by Buikstra and Ubelaker. Yet we have taken our cue from those impressive scholars, noting that standardized methodological rigor will aid in improving our discipline. In this chapter we suggest research considerations and procedural practices intended to limit bone and tooth destruction and to maximize research potential and long-term posterity.

Background on Destructive Sampling

Gross observation of diagnostic features on human skeletal and dental remains, including natural, pathological, and anthropogenic traits, provides a means to reconstruct demographic profiles, population morbidity, biological relationships within and between populations, and frequencies of violence and cultural practices such as bodily expressions of identity (for example, cranial modification and tooth ablation) (Buikstra and Beck 2006; Walker 2005). Further details of one's health status and lifeways can be obtained through biochemical and molecular tests, which typically require the extraction and destruction of a small skeletal and/or dental sample. Those samples can provide a wealth of data for dietary reconstruction (Lambert et al. 1984; Schoeninger 1989), radiocarbon dating (Piotrowska and Goslar 2002), assessing migration (Knudson and Price 2007; Price et al. 1994), and documenting physiological adaptations (Bridges 1996), ontogenetic patterns (Dean 2006), demographic details (Wittwer-Backofen et al. 2004), and genetic variation within and among populations (Pääbo et al. 2004).

Because a sample of bone or tooth must be destroyed or altered, a cost-benefit analysis regarding the amount of destruction required for the amount of information to be gained must be evaluated. Sampling procedures should minimize destruction and enhance the quality of data obtained. There is a conspicuous absence of presenting such procedures in research articles. This leaves such vital considerations and procedures to individuals with a broad range of backgrounds, not all of whom will be specialists in bioarchaeology.

Research Considerations

Given the significant ethical concerns involved in the destructive analysis of human skeletal remains (Larsen and Walker 2005), serious consideration should be given to the potential insights to be gained vis-à-vis the material that will be lost and the concerns the process may bring to descendant communities. In the many world regions where indigenous descendant populations reside, there are conflicting views regarding the utility of the scientific study of human skeletons. As Larsen and Walker (2005:111) note:

> The traditional perspective of scientists who study ancient remains has been to consider human remains as valuable objects full of research potential. Many descendants of the people whose remains bioarchaeologists study, in contrast, view ancestral remains as objects of veneration that should be protected from what they see as the indignity of examination by scientists whose motivations they consider suspect at best and immoral at worst.

In light of these potentially disparate perspectives regarding the treatment of human remains, and after consultation with appropriate descendant communities (and see questions 7 and 8, below), the authors suggest that at least the following questions be considered before conducting any kind of destructive analysis:

1. Do I have a clear research question that can be addressed by this analysis?
A research question should always be articulated prior to data collection and analysis, especially if the data collection procedures lead to the destruction of the material under study. If any of the destructive analyses are outside the scope of the research questions to be addressed, they should be avoided. If it is concluded that destructive analysis will be conducted, consider ways to maximize the amount of information to be gained by the sample (see question 2).

2. Have I consulted with other specialists who plan to obtain data from this extracted sample?
Because several kinds of data may be obtained from one specimen, consulting a histologist, an archaeological chemist, and a molecular anthropologist, among others, is advised. These specialists can assess the feasibility of sectioning the sample for different studies. For example, it is typically possible to use a dental sample for multiple studies such that casts are made for microwear analysis, enamel is used for isotopic analyses (of both light and heavy isotopes, which may need to be conducted in different labs, thus requiring detailed planning), and roots are used to obtain ancient DNA. The order in which the specialists handle the samples is an important consideration. For instance, ancient DNA studies are vulnerable to modern contamination from handling, so it may be wise to conduct DNA extraction before other analyses. (In our experiences, removal of the root does not significantly interfere with making casts of the enamel crown.)

3. Is an experienced person or lab providing and analyzing the data?
Many laboratories will conduct analyses of your samples for a fee. Before consulting a lab, consider doing a background check on its reputation and the qualifications of its personnel. Review publications produced from the lab. Be explicit when discussing fees and author order (if applicable) for the resultant publications. It is strongly advised to employ a method of quality control (see below).

4. Is a system in place for quality control?
There are two effective approaches for quality control: the independent lab strategy and the blind sample strategy.

The independent lab strategy is standard for high-profile ancient DNA studies, but the strategy applies equally well to most destructive analyses. Duplicate samples are sent to independent (unaffiliated) laboratories, which report the results directly to the primary investigator, who then assesses whether results from the two labs are consistent.

The blind sample strategy is best used when the laboratory requires no identifying contextual information, only an identification code number. Each sample, regardless of whether it is a duplicate, has a unique identification number that can be linked to the contextual information only by the primary investigator. The laboratory is never given any information regarding which samples are duplicates. In fact, the laboratory may be unaware that duplicate samples are included.

Consequently, the primary investigator assesses whether results from the duplicate samples are consistent.

The independent lab strategy and the blind sample strategy work equally well when the primary investigator of the project and the director of the laboratory conducting the analysis are the same individual. Laboratory directors may wish to include duplicate blind samples to evaluate whether their lab is producing consistent results. Alternatively, laboratory directors may wish to send duplicate samples to a colleague for independent tests.

5. Will I be able to contribute to the conservation of the remaining skeletal material?
Often, monetary support for conservation material is limited. Researchers should consider including a budget for skeletal conservation. At the most basic level, human remains should be stored in an acid-free environment, with padding for the more delicate elements, and all elements should be stored in sturdy, well-labeled containers for long-term preservation.

The environmental conditions of the storage space are a critical aspect of proper conservation. In particular, humidity can lead to decay of skeletal elements. Removing moisture from the air using a dehumidifier will aid in protecting the bones. Pollutants in the air can also contribute to degradation or contamination of the skeletal material. In particular, insecticides, pollution from cars and factories, and even cleaning supplies will hinder conservation efforts. Researchers can protect human remains from contaminants by placing bones in rooms that are adequately sealed.

6. Are there additional samples or contextual information that would complement the skeletal samples?
In addition to the skeletal material, nonhuman samples and contextual information may be required to address questions posed in the study. For instance, samples from local soils, flora, or fauna may be needed for isotope studies on dietary reconstruction or residential mobility (Price 1989; Schoeninger 1989). For molecular studies, the principal investigator may need to obtain permission to acquire biological samples from project members who have handled remains, as they may be sources of modern contamination (Sampietro et al. 2006). Such samples would require informed consent from the participants.

7. Have I obtained the proper permission to conduct a destructive analysis?
National and local guidelines related to repatriation and cultural property rights must be met, and all relevant permissions should be obtained in writing. In addition to getting permission from the person or institution in charge of the material, you may need permission from the original excavator, the repository holding the material, the close relatives or descendants of the individuals studied, and relevant government agencies, such as those involved in cultural resource management. Additional permits from government agencies may be required for shipping samples out of the country from which they originated.

8. Have I considered the impact of this study on related individuals or communities?
While most destructive analyses can be conducted on relatively small bone samples and/or on teeth, any destructive analysis on human bone involves a serious ethical decision. The impact of your study requires careful consideration of the long-term conservation of the skeletal material. Importantly, the impact the study will have on living individuals closely connected with the material, both biologically and culturally, must be carefully considered. There are several well-written reviews on this subject (Larsen and Walker 2005; Walker 2000, 2005).

9. Have I attempted a pilot project to assess the feasibility of the study?
It is possible that postdepositional processes have made certain destructive analyses unfeasible. This is especially a concern for many ancient DNA studies. As a proof of principle, it is wise to begin with a pilot project on a limited number of individuals. The researcher first gauges what level of DNA preservation would be considered acceptable to continue the project. Applying a simple probability calculation can be informative. For example, if the researcher decides that a 50 percent DNA recovery rate is acceptable to continue the project, then, all things being equal, the probability of having a successful DNA extraction within three, four, or five samples is .875, .937, and .968, respectively, calculated as $p = 1 - (1 - r)^n$, where r is the hypothetical success rate and n is the number of samples examined. Thus, if after five samples no DNA preservation is observed, it is likely that the overall recovery rate will be less than 50 percent.

PROCEDURAL PRACTICES

The following procedural standards are intended for someone well trained in identifying a variety of dental and skeletal lesions and standard skeletal and dental traits. Such identifications are best made through hands-on experience rather than photographs.

Handling Bone

Destructive analyses for ancient DNA are particularly sensitive to modern contamination. As a result, minimizing direct contact with the skeleton is ideal. Nevertheless, there are in-house laboratory techniques that reduce modern DNA contamination (Kemp and Smith 2005; Yang and Watt 2005). Consequently, a permanent record of the skeletal material outweighs the potential added complication of having to purge modern contamination from a sample. Still, there are standard procedures that reduce modern contamination during the handling of human remains.

One should wear gloves to avoid passing oils, salts, and other contaminates to the specimen. Disposable sterile nitrile gloves are ideal. These gloves greatly reduce contamination when compared to reused cotton gloves, and they are smoother than disposable latex gloves, so they are less prone to snagging.

If possible, do not wash the skeletal material or treat it with any preservatives before sampling. If a consolidant needs to be applied to a skeleton to excavate it, treat one side only or at least leave a small portion clean for future testing.

In many cases, researchers may want to use museum specimens for isotope studies, and those are sometimes covered in PVAc glue (polyvinyl acetate with acetone solution). A recent study showed that if the PVAc is removed with organic solvents, researchers will still get reliable results for the analysis of carbon ($\delta^{13}C$) and nitrogen ($\delta^{15}N$) isotopes in bone collagen, for carbon isotopes in carbonate from bone hydroxyapatite, and for oxygen ($\delta^{18}O$) isotopes from phosphate in hydroxyapatite (France et al. 2011). However, oxygen isotopes from carbonates exhibit variable results and should not be used in investigations (France et al. 2011).

Data Collection Before Destructive Analyses

Osteological and pathological data should be recorded before samples are cut or extracted. We recommend using the data collection standards as outlined by Buikstra and Ubelaker (1994). This work should include at least the following:

1. Recording of:
- Dental calculus
- Enamel hypoplasias, hypocalcifications
- Dental caries
- Dental wear
- Dental chipping
- Dental and skeletal measurements
- Dental and skeletal nonmetric traits
- Dental and skeletal modifications
- Cut marks or other anthropogenic alterations to the specimen
- Stains from copper or other elements (for example, ochre, cinnabar)

2. Observations of pathological lesions (for example, periostitis, osteoarthritis, bone fractures, porotic hyperostosis)

3. Taphonomic descriptions (for example, color, sun or water damage, burning, rodent or other animal chew marks)

4. Creation of dental casts or a digital rendering of the element

5. Photographs of all views of dental and skeletal specimens (mesial, distal, lingual, buccal, occlusal, and inferior views of teeth; anterior, posterior, medial, and lateral views of bones), with a metric scale in the photos

6. Additional photos after sample extraction (for example, of the alveolar bone where a tooth was removed)

Selecting the Specimen

Selecting a Tooth. Teeth are among the most informative parts of a skeleton. Gross observation alone can provide insights into dental health (caries, abscesses, periodontal disease, antemortem tooth loss), developmental health status (linear enamel hypoplasias and metrics), diet (caries, dental wear, and calculus), genetic relationships (metric and nonmetric traits), and cultural affiliation (dental modifications). As noted above, all teeth should be photographed and an accurate cast or digital rendering should be made prior to sampling. When casting teeth, it is important to use a high-resolution casting material such as vinyl polysiloxane.

There are a number of reasonable tips for selecting a tooth to sample. Choose a tooth that has a lateral counterpart (left and right present) to reduce loss of valuable information in future studies. Lateral counterparts may differ somewhat, particularly in terms of pathological status, and while some studies prefer bilateral comparisons, selecting a tooth with a lateral counterpart is the most sensible.

Teeth with carious lesions or severe dental wear should be avoided as samples because of their potentially useful cultural information and, with respect to molecular studies, the increased risk of modern contamination. Of course, this advice does not apply if the research question itself relates to related studies on caries or dental wear.

Researchers should avoid destroying teeth that exhibit rare nonmetric traits in a particular population (for example, a protostylid), even if the tooth was caste or digitally rendered. Consult an osteologist with in-depth knowledge of the skeletal population to ensure that you are not destroying the one example of a rare trait in that study group.

Teeth with calculus (plaque) should be avoided because they can be used for noninvasive studies such as phytolith analysis (see Fox et al. 1996 for an example). If a tooth with calculus is selected, bag a sample of the calculus, noting its origin and the reason for removing it. Leave the labeled calculus sample with the original skeleton or arrange to have a phytolith specialist analyze it.

Selecting the appropriate tooth is a negotiation between the questions to be addressed, the teeth available, and the information stored in that tooth. Teeth are not equal in terms of the information they yield. For example, canines are much more likely to exhibit enamel hypoplasia and are well suited for histological analysis. In contrast, molars provide many more nonmetric traits than do canines. Nevertheless, it is straightforward to document dental traits before tooth destruction. Consider the pros and cons of each potential tooth sample before the final selection is made.

Selecting a Bone. The type of bone sample necessary for the destructive analysis may vary; consultation with the laboratory conducting the analysis is essential. As with the selection of dental specimens, select a sample that is free of pathological lesions (unless the research question is centered on analysis of such a pathology), and select a bone element that is present on both the left and right sides. As always, photograph and record the presence or absence of nonmetric traits and record all metric data.

There is a better chance of DNA preservation in denser material, such as long bone shafts. Yet long bones provide a wealth of morphological data. If long bones are sampled, every attempt should be made to keep the bone intact while taking the smallest excision required for the analysis. Although, from a conservation standpoint, it might be good to select an already fragmented piece of bone, this might not be ideal for the laboratory because exposed broken edges (especially exposed trabecular bone) often have soil and other contaminants integrated into the trabeculae, requiring additional cleaning. Bone freshly cut with a clean blade (see below) might be better. Ribs and phalanges are common alternatives to long bones in DNA studies and also work very well for isotope studies. For isotope studies, if comparisons are being made within and between skeletal populations, every effort should be made to sample the same element. Standardization is essential for reliable intra- and interpopulation comparisons because isotopic incorporation varies from bone to bone. Thus the sampling of a rib or a phalanx is encouraged because there are 24 ribs and 56 phalanges from which to choose. It is also worth noting that permitting agencies are typically more willing to allow destructive analysis of ribs and phalanges than femora and tibiae.

Sample Extraction

If a sample needs to be cut, do so with a sterilized tool. We suggest manual saws or multiple-speed rotary drills (Dremel, for example) with mini-carbon disks and engraving cutters. Always wear gloves, goggles, and a mask to protect both yourself and the sample. A rotary drill typically has disposable attachments (such as disks and cutters) that must be changed for each sample extraction to avoid cross-contamination. When using a rotary drill, check voltage requirements (110 or 220 volts) in the country where you are conducting the extraction. While manual saws disperse less bone dust, automated saws typically require less direct pressure on fragile materials. For molecular studies, one concern with rotary tools is the heat they generate, which may reduce DNA preservation (Adler et al. 2011); Adler et al. found that using lower speeds (100 rpm) resolves much of this issue.

Obtaining Dental Samples. Teeth are often free or loose in the alveoli (sockets). In these circumstances, extraction requires minimal invasive procedures. Unfortunately, this is not always the case. If a tooth is affixed in the socket, pull it gently to evaluate the degree of resistance. Sometimes a light wiggling of the tooth may free it. If not, determine whether an alternative tooth is available, assuming that your research question is not dependent on extracting that particular tooth type.

Evaluate whether curators or a previous researcher glued the teeth in their sockets. If so, the glue can sometimes be reversed with water or acetone (depending on the type of glue used) or by picking away glue chunks with tweezers and a drill. Obtain permission from the curator to reverse the gluing, and be sure to inform the laboratory if you used any water or acetone (or other reversing agent) to extract the tooth.

If the tooth is tightly affixed in the alveolar bone, additional tools will be required to extract it. Check with the curator, excavation director, or other responsible agent to ensure that you have permission to destroy some of the alveolar bone for extraction. Maxillary molars are much more difficult to extract, owing to their three-pronged root structure, so single- or double-rooted teeth may be better options. Using a drill with a cutting disk or engraving cutters, carefully cut away tiny portions of the alveolar bone surrounding the tooth. (This procedure is not advised if there is an abscess at the socket location.) After removing a small bit of bone, gently wiggle the tooth to extract it. Repeat this process until the tooth comes free. Do not yank the tooth, as excessive force will result in a fractured root, fractured enamel, and/or fractured bone.

Obtaining Skeletal Samples. Ribs and phalanges may be taken whole. For large bones, some cutting will likely be needed. If the bone is already broken, cut off a section from the broken end, ensuring that you have enough intact bone that is free of contamination. If a bone is intact, remove a rectangular piece, as if cutting a window into the skeletal element. This technique is preferred over cutting off the proximal or distal end of the bone, as that destroys the entire integrity of the bone. If you're using a rotary tool, you might need multiple cut disks to complete the removal of the bone sample, and the use of protective eyewear becomes even more essential.

Storing and Transport of Samples

Bone samples that are to be used for radiocarbon dating should not be wrapped in paper; rather, they should be wrapped in aluminum foil. All extracted samples should be individually wrapped in sterile bags. Each sample bag should then be placed inside another bag or container with an accompanying sample tag. The tag should be labeled with a permanent marker. Each tag should be marked with identification information (site name/number, excavation unit, feature, burial number, specimen number, and so on). Excavators on different projects often use different coding systems, so it is the responsibility of the osteologist to learn each project's coding system. Be sure to include all identification information with the extracted sample. Also include details of the sampled element (for example, "shaft fragment from distal end of left femur" or "mandibular right molar 2") and the weight of the specimen in micrograms. Finally, leave a tag in the original bag noting the following information for researchers who may study the skeletal collection 5 or 50 years later: (1) sample taken; (2) tests being performed and at which labs; (3) gross observations made on the element; (4) name; (5) date. Business cards are also good to leave with the original bag.

The loss or mixing of contextual information can have dire consequences for current and future studies of skeletal materials. It is a good practice to complete the sampling process one skeleton at a time. This helps prevent mislabeling samples or mixing skeletal elements from more than one individual. It is important that once someone begins the sampling process, that same person completes the process before moving on to another skeleton.

CONCLUDING REMARKS

The considerations and procedures presented here provide a foundation for studies of human remains that require the destruction of samples. Inevitability, different types of analyses will involve unique challenges for sample extraction, quality control, and skeletal conservation. We ask that papers presenting novel destructive analyses include brief descriptions of how the research assured quality control and conservation efforts. We hope that this brief description helps in establishing and maintaining responsible research agendas and provides guidance to those wishing to add these novel approaches to their own research design.

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PART II

Bioarchaeological Research in North America

5

Negotiating the Gateway:

Working with Multiple Lines of Evidence to Determine Identity

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Until the advent of the Native American Graves Protection and Repatriation Act (NAGPRA) in 1990 and other recent legislation on local, state, and international scales, modern physical anthropologists did not necessarily focus their research on determining ethnic or tribal identities. Indeed, much work over the last 40 years has been devoted to issues of health, disease, and more precise determinations of age and sex. Some work focused on biological distance between groups, not as a way of linking past groups with living tribes or nations but instead as an approach to determining interrelationships between groups living at the same time in the past (cf. Buikstra et al. 1990). Beyond the fact that research took different directions, one reason for this lack of attention to the relationship between the bones of the past and living groups is the difficulty of making such links over what can be hundreds or thousands of years. Another reason is the not unwarranted concern that if one states that there *may* be a relationship, someone could take that statement of possibility for certainty and assume that there is indeed a relationship.

This reluctance to pursue such research has been unfortunate in many ways and was most notable when NAGPRA became U.S. law. NAGPRA requires museums and other institutions holding human remains to make determinations of relationships between those remains and living Native American tribes. The research on how to make these links and on how to determine identity was not well developed, and archaeologists and physical anthropologists did not have sophisticated or mature approaches to address this issue. Because the question had largely been avoided for so many years, researchers approached it with caution, at best. The best current research takes a multidimensional and multivariate approach to answering the question of identity, and indeed NAGPRA requires this. However, how many people or institutions actually go through the time and trouble of taking these steps?

My original intention in writing this paper was to employ case studies to examine what happens when archaeologists and physical anthropologists do and do not work with native peoples in determining identity when conducting repatriation studies. One major line of evidence in identity determination comes from native people, yet this can often be the most difficult kind of data to incorporate and collect. In conceiving this project, I was more than aware that there were biases, set views, and several different sides regarding this particular issue. Some saw archaeologists, physical anthropologists, and some ethnographers as gatekeepers in controlling and legitimating identity; others saw native peoples as having taken total control of the situation in recent years; and still others saw a general leveling of the playing field. To my knowledge, as of this writing, no one has examined real data to see what has actually happened in repatriation cases.

There is little question that anthropologists have traditionally been considered authorities on identity, but over the last 15 years, this view has shifted and changed rather dramatically. The organizers at a Society for American Archaeology symposium in 2007 stated:

> Laws and public opinion have traditionally empowered anthropologists as the authorities on Native American identity. More recently, Native Americans have asserted the right to establish their own identity. They embrace oral history and tradition in opposition to "scientific data" for determining identity. These conflicts have real consequences in debates over repatriation, land claims, and government recognition. This session examines the rights and autonomy of indigenous people in determining their own identity and the role of archaeologists as gatekeepers in legitimating that identity [Atalay and McGuire 2007].

Although a number of people and specific tribes may view the situation this way, one issue is that the law, at least in the United States under NAGPRA, has not changed, and *both* kinds of evidence (as well as other kinds of data) are equally valid. Further, this need not be an either/or debate, since many native people commonly use different kinds of evidence in documenting and determining their identities. Similarly, many anthropologists include and integrate oral histories in determining identity. Nonetheless, my sense in beginning this project was that archaeologists and physical anthropologists, and even cultural anthropologists, were not very consistent or clear about *how* they used oral history and oral traditions in determining identity. In addition, I did not think researchers purposely ignored oral tradition; but I thought they did not necessarily know how to incorporate the data, were uncomfortable doing so, or considered such data too "messy" or problematic. My sense of this situation came from my experience working with many archaeologists and physical anthropologists struggling to determine the potential identities of the archaeological and human remains in their possession. It also comes back to the problem of scholars not having addressed this kind of research for so many years.

Although this particular paper uses examples from the United States, the issue is not limited to the United States; it is a worldwide problem that is increasing as more and more countries wrestle with issues of repatriation and native identities. The focus of the problem is on identity and the kinds of evidence scholars use to make identity determinations from human remains, funerary objects, mortuary contexts, and oral and written histories. These kinds of determinations are made throughout the world, and the best analyses take into consideration every kind of data, as well as the potential biases of every data set. For example, in her case study from the classical Near East, Perry (2007) looks at textual data in addition to bioarchaeological data to answer research questions in Near Eastern archaeology. One of the important points Perry makes—a point also made by Buikstra et al. (2000) for a historic cemetery in Grafton, Illinois—is that it does not matter *whether* the historical and biological data differ but "*why* the texts provide a divergent history from the skeletons" (Perry 2007:490). The associations and interrelationships can be examined to let us better understand larger cultural and political processes.

Outline of the Project: The Smithsonian, Burials, and Repatriation

To examine these issues in a more systematic way, I looked to the Repatriation Office of the Smithsonian Institution National Museum of Natural History (SI). Even though the Smithsonian does not operate under NAGPRA, it is a good choice for this project for several reasons: (1) the SI has chosen to follow NAGPRA policies, and its law was subsequently amended in 1996 to follow NAGPRA more closely; (2) the SI has the financial and personnel resources to attend to repatriation in ways that most other institutions do not; (3) its reports are of high quality, are reasonably consistent in format, and are accessible;[1] (4) the SI has repatriated more human remains and objects from more sites and geographic areas than any other museum (see Table 5.1); and (5) because I served on the SI Repatriation Review Committee for 15 years, I had copies of most of its reports, although these reports are available to anyone who requests them.

For this project, I examined 45 reports—not all the reports produced by the SI Repatriation Office (I eliminated most reports dealing with only one or two items, for example) but a sufficient sample for this purpose. Geographically, the reports cover the United States—from Alaska to the Northwest Coast, from the Southwest to the Plains, from the Midwest to the Southeast to the Northeast (see Table 5.1).

Coincidentally, in 2005 Stephen Ousley, William Billeck, and Eric Hollinger published a long discussion and description of physical anthropology's role in repatriation at the Smithsonian, focusing on consultation between tribes and the SI, the SI's documentation of human remains, and especially the issue of cultural affiliation and the many ways that affiliation can be

Table 5.1. Smithsonian Institution Museum of Natural History Record of Repatriation: 1989–2005.

	Human Remains		Archaeological Items		Ethnographic Materials	
Region	**Returned**	**Not Returned**	**Returned**	**Not Returned**	**Returned**	**Not Returned**
Alaska	3,171	85	751	127	527	10
Northwest Coast	182	68	177	0	10	3
Hawaii	190	0	12	0	624	0
California	2	0	0	0	0	0
Great Basin	50	17	0	0	0	0
Plains	1,695	49	14,961	0	1	1
Plateau	83	8	2,225	15	1	2
Southwest	299	14	0	0	3	0
Northeast	16	37	3	28	2	1
Southeast	0	120	13	0	0	0
Totals	**5,688**	**398**	**18,142**	**170**	**1,168**	**17**

approached from the perspective of physical anthropology in particular. Ousley et al. (2005:9) make the point that the mandate to consider all available evidence and all relevant information is both reasonable and obvious. They go on to say: “The assumption that this process of consideration of evidence somehow pits archaeology against oral tradition, or scientists vs. Native Americans, is generally false.” Because their focus is on physical anthropology, they really do not provide examples of oral histories or traditions that have helped inform cultural affiliation determinations. Indeed, their examples of interactions with tribes have to do with consultations on reburials, traditional care requests, and improved relationships, all of which are important but do not directly speak to the issue of determination of identity per se.

In a close reading of Ousley et al. (2005), it is clear that because of the quantity of human remains in the Smithsonian, much of the physical anthropology team’s time is spent in documentation and sorting accession and other catalog problems. There were originally approximately 18,000 Native American human remains at the National Museum of Natural History; well over 5,000 human remains had been returned as of 2005. Once catalog problems are sorted, physical anthropologists focus their efforts on determining cultural affiliation and identity through various osteological data points and comparisons to known groups. They see their work as being especially helpful when other data sources are absent or ambiguous. However, in their 2005 paper, Ousley et al. do not discuss how they integrate other kinds of data, including oral traditions and oral histories, with biological data.

It is useful at this juncture to say something about the massive amount of work involved in documentation of the materials at the Smithsonian. While it is true that most of the human remains and objects at the Smithsonian have been there for a very long time, and while it is also true that people have been coming to the Smithsonian to study its collections for many years, comparable information is not available for all collections. In other words, beyond some basic catalog data, the Smithsonian does not have the same information for everything in its holdings. No large museum does. This is especially true when a museum and many different individuals have been collecting items over a long period and in many different contexts.

When it became clear that museums across the country would likely have to repatriate most of the human remains in their collections, a group of physical anthropologists met to determine a minimum set of standard measurements and observations that should be recorded for each set of human remains, whenever possible. These sets of observations came to be called the Standards, and ideally they will allow physical anthropologists to conduct comparative research using this information in the future, whether or not the particular remains have been repatriated. Buikstra and Ubelaker

(1994) compiled and edited the volume outlining the Standards. The Smithsonian has used a version of this system for all its human remains and has a digital version of these data available. Other institutions have done the same thing, in the United States and internationally. The comparability is not perfect, but it is far better than anything that would have been possible otherwise. The Standards have also created increased comparability in discussion and in print; they have become a reference tool.

What is most unfortunate is that nothing similar to the physical anthropology Standards was made for archaeology. Granted, physical anthropologists have the luxury of dealing only with human remains, which are certainly complicated, but archaeologists did not even try to decide at a general level what kinds of data and documentation should be recorded and kept. There were no comparable discussions about data.[2] This lack of attention to the problems of data and the overwhelming amounts of data is clear in the inconsistencies one sees in repatriation reports and in the lack of reports in many cases. This case study would not be possible for most institutions because the information does not exist.

Figure 5.1 shows the distribution of all the reports produced by the Smithsonian Repatriation Office from 1989 through 2005 by region of the country (the regions were labeled by the Repatriation Office). A total of 82 reports were produced during this period, and Figure 5.1 illustrates that the geographic distribution is not even. There are several reasons for this biased distribution: (1) Priority was first given to remains of named individuals and those with known descendants; and (2) priority was next given to tribes that had filed for the return of specific remains and/or collections. The distribution reflects both the collecting histories of the institution and the specific requests that tribes made.

Figure 5.1 shows clear bias or preference for Alaska and Northwest Coast regions, as well as for the Plains. The source of some of this bias is relatively easy to explain. First, most of the cases in the sample are historic in nature and are from collections that were originally in the Army Medical Museum; in 23 of the 45 reports examined, one or more individuals had originally been

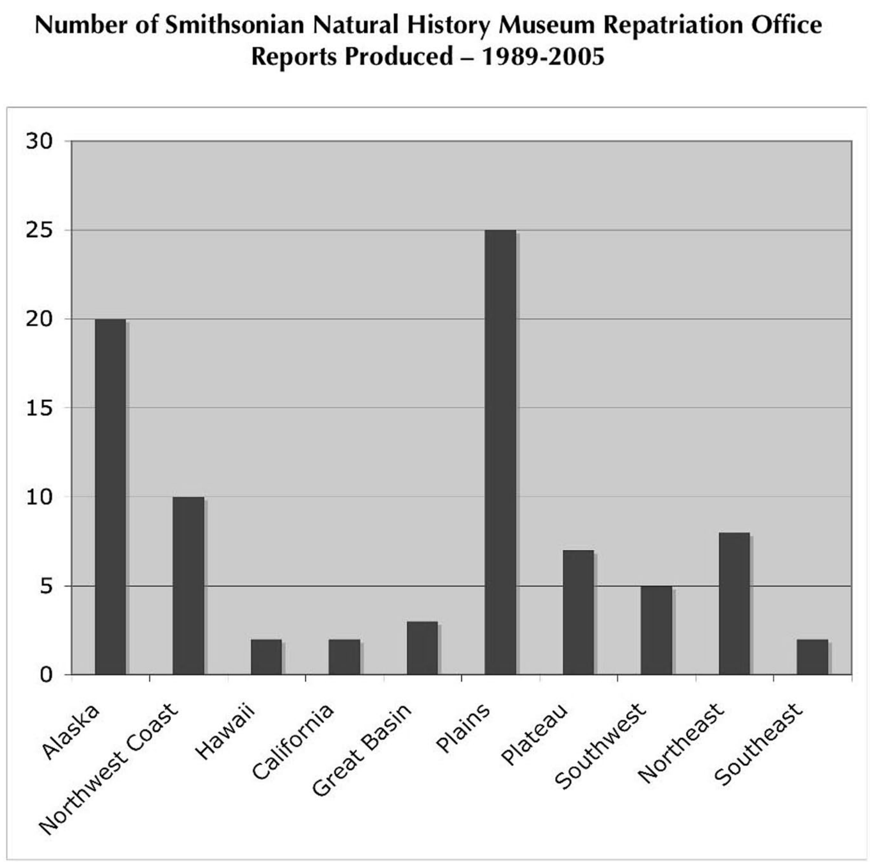

Figure 5.1. Number of Smithsonian Natural History Museum Repatriation Office reports produced from 1989 to 2005.

donated to the Army Medical Museum and were associated with army forts or army operations. Since many army operations in the middle to late nineteenth century occurred on the Plains, the correlation is not surprising.

A majority of the materials in Alaska and the Northwest Coast came to the Smithsonian from the expeditions of Smithsonian researchers, often from the expeditions of Aleš Hrdlička. Hrdlička was an important figure in the history of physical anthropology, but his field techniques were sometimes problematic from a modern perspective, and he documented his own excavations of graves of known individuals. Some sites he excavated were historic sites with clear links to peoples currently living in modern villages.

Hrdlička and other Smithsonian researchers in the first half of the twentieth century were especially interested in issues of migration from Siberia into Alaska. Other Smithsonian researchers during this time included Henry Collins, James Ford, and T. Dale Stewart. Their research included questions about how many waves of migration occurred; how the waves accounted for the present distribution of cultures, languages, and physical types; and whether or not one could identify the areas of origin of these peoples. The major research issues were New World origins and relationships between groups. The researchers excavated major sites and areas throughout Alaska.

Although the Plains have a large representation due to the presence of the army in the nineteenth century, another reason for the region's popularity in the Smithsonian collections is that the Smithsonian houses many materials from the River Basin Surveys (RBS). The importance and extent of the surveys warrant a short, separate discussion.

The RBS came out of the work of the Committee for the Recovery of Archeological Remains, a group of anthropologists sponsored by the American Anthropological Association, the Society for American Archaeology, and the American Council of Learned Societies, with liaisons from the Smithsonian and the National Research Council. These groups wanted to preserve archaeology threatened by post–World War II programs such as dam and reservoir construction. The ultimate result of the committee's work was the Inter-Agency Salvage Program, an arrangement among the Smithsonian, National Park Service, Army Corps of Engineers, Bureau of Reclamation, many universities, and other public and private organizations. It was particularly active in the Missouri River basin, West Coast states, southeastern states, and Texas.

In two articles in *Science* (Johnson 1966; Wedel 1967), the accomplishments of the program are outlined. Wedel, who at the time was the senior archaeologist in anthropology at the Smithsonian and had also been field director of the RBS, focused his discussion on the Smithsonian work in the Missouri River basin. For our purposes, it is significant to note that Wedel outlines the many contributions of the Missouri Basin project, especially since if the work had not been done, all this information would have been lost through flooding and construction. Instead, "[g]reat quantities of artifacts and important collections of skeletal material have been gathered under controlled conditions; and rigidly selected representative samples, with full provenience data, have been added to the national collections" (Wedel 1967:592). Wedel (1967) goes on to note that more than 200 sites were tested or extensively excavated, whereas prior to the program, fewer than a dozen sites in the region had received professional attention. More than 800 historic Arikara burials were excavated, as well as burials from the Central Plains and Woodland traditions.

Through most of its existence, the RBS was a unit of the Bureau of American Ethnology (BAE), with headquarters in Washington, D.C., and with field offices. One major field office was in Lincoln, Nebraska, and that office directed the work in the Missouri Basin. When the BAE was disbanded in 1965, the RBS joined the Smithsonian Office of Anthropology. In 1966 the headquarters moved to Lincoln, and in 1968 the RBS was placed administratively under the director of the National Museum of Natural History. In 1969 the Smithsonian transferred the RBS to the National Park Service, but provision was made for the deposit of records and manuscripts in the Smithsonian. The relationship between the RBS and the Smithsonian is a long and close one.

Analysis of Repatriation Office Reports

The Repatriation Office reports I examined for this exercise include assessments of human remains only, as well as assessments of human remains and funerary objects. The report dates range from 1992 through 2004. Unfortunately, this range excludes one of the major reports and repatriations conducted by the SI—the Arikara collection, consisting of 1,288 individuals and 14,449 funerary objects—but since I did not have immediate access to the completed report, I could not include it in this analysis (although it is included in both Figure 5.1 and Table 5.1).

The reports are biased in terms of when they were prepared; 33 of the 45 were written prior to 1998. This is *not* because the Smithsonian was productive in the earlier years of the Repatriation Office and is not productive now, but the difference likely reflects several factors: (1) a decrease in staff due to an overall decrease in budget; (2) the 1996 change in the Smithsonian law, which changed what the SI had to report and when it had to report it; and (3) a shift in work to some of the larger collections, such as the Arikara collection, which required significantly more time to complete. The time bias is particularly relevant because not only did the reports improve in quality over time, but as case officers gained confidence and experience in working with tribes, their reports became more sophisticated and inclusive.

Reports fall into one of three categories: (1) human remains only; (2) human remains and funerary objects; and (3) objects only (the objects may be cultural patrimony, sacred, or unassociated funerary). Of the reports, 20 reports fall into the first category; 19 fall into the second; and 6 fall into the third. Although it is fairly easy to divide reports into these categories, there is a great deal of diversity within each group.

The first category—assessment of human remains only—is the least diverse group and the one that is least likely to include a discussion of oral histories and oral traditions. Of the 20 reports in this category, 16 did *not* include any mention of oral histories or traditions. Of the 651 individuals considered for repatriation in this group, 454, or 69.7 percent, were offered to tribes for repatriation. I do not know how many reports tribes subsequently challenged, but I am certain that at least a few reports were challenged.[3] Seventeen of the reports examined were completed between 1992 and 1998.

In the second category of human remains and funerary objects, of the 19 reports in this category, 13 did *not* include a specific discussion of oral traditions or oral histories. Of the 2,184 individuals reported here, 2,096 (or 95.9 percent) were offered for repatriation, along with 1,103 of the 1,447 objects considered (76.2 percent). Of the 19 reports, 8 were completed from 1997 through 2004. Several tribes have challenged the decisions of the Smithsonian in this category, and in more than one case these challenges ended in a reversal of the initial SI decision. The tribes brought forward additional evidence, sometimes in the form of oral histories, sometimes other kinds of evidence, but always information that the Smithsonian did not have or did not interpret properly because case officers did not have as extensive or intensive knowledge of the specific area or region.

In the category of objects only, there are six case reports. In three of these reports, the authors did not use oral traditions or histories in their discussions, either because historic and/or archaeological evidence was sufficient to establish the identity and/or because such oral evidence did not exist. The Smithsonian returned objects in four cases; in another case, it returned one object and retained the other. In the sixth case, the Smithsonian used oral tradition and oral histories extensively. The museum established that the tribe requesting the object had a cultural affiliation to the object but had not demonstrated that it was a sacred object or an object of cultural patrimony. Various scholars at the Smithsonian thought the object should be returned, and people worked very hard and deliberately over several years to document that the object had been used historically in ceremonies and/or rituals but found that they could not demonstrate that this had been the case. Indeed, interviews with tribal elders documented that if the object were returned, it would not necessarily be used in a ceremony today. Further, the Smithsonian was able to document that the museum had clear title to the object.

I have deliberately not been specific in outlining these cases because my purpose is to provide only a sense of the kinds of information available and the kinds of cases that have been processed. However, at the end of this paper I have listed a separate and complete bibliography of all the case reports that I used in this analysis.

In terms of the use of oral histories and oral traditions, with a few notable exceptions, the report authors tend to outline what the traditions and histories say, indicate how they agree or disagree with each other and the other data, and then continue. In other words, they either do not appear to know what to do with these data or have decided not to use them. Unlike "messy" archaeological data, which authors might discuss, dissect, or sort, the discrepancies here often seem to be sufficient for authors to set all the data aside. When oral histories and traditions do not agree with archaeological and other data, they are dismissed. Roger Echo-Hawk (2000) and others have discussed specific and systematic ways to approach such problems, but none of the case officers used these methods in the reports I examined. In a few reports, oral histories were used extensively, but these were exceptions; most of the time, the histories were simply outlined.

Indeed, archaeologists and physical anthropologists do not tend to eliminate archaeological or osteological data when they find an errant sherd or an unusual bone, so why eliminate oral history when it doesn't quite fit with other data? Rather than automatically assigning a malevolent cause, I think the reason is likely much simpler: We have not been taught what to do with the data. Few graduate anthropology programs specifically train students in how to incorporate oral traditions and oral histories into archaeological, ethnographic, historic, geographic, and other data. As we desire to be more scientific, we tend to like messy data less and less. This is unfortunate since reality is more and more messy, and more and more interesting. Data that fit a neat pattern are never as interesting as data that have some rough edges. If we really want our students to do better in these circumstances in the future, it would be to our benefit to focus training on such issues.

At the outset, I said I wanted to examine what happens when archaeologists do and do not work with native peoples in determining identity. I am not certain that this examination of using oral traditions in reports gets directly (or even indirectly) to this question, but I can say that since the Repatriation Office at the Smithsonian has been in existence, and as the case officers have gotten more experience, not only has the quality of the reports improved, but the level and degree of interaction with tribes have also improved.

Appropriate to this discussion, we can look at the overall pattern of case reports and record of returns. Table 5.1 provides this information. It is organized by region in the same way as Figure 5.1 and is divided by categories of information—human remains, archaeological items, and ethnographic materials. For each category for each region, I have tallied how many of the items requested and reported were returned (through 2005) and how many were not returned. The human remains represent individuals, but the archaeological numbers are a bit misleading; beads are sometimes counted individually, leading to very large numbers in places like the Plains. The lack of returns of archaeological and ethnographic items in some regions relates to the circumstances of the original report; the return of historic human remains had highest priority, so it was done before everything else.

Table 5.1 demonstrates that the Smithsonian has returned a significant number of human remains, archaeological items, and ethnographic objects and that it has rarely refused to return items. Indeed, in the reports I examined, it returned 93.5 percent of the human remains it considered, 90.7 percent of the artifacts considered, and 98.6 percent of the ethnographic items considered. These numbers may be misleading, however, since the SI has a long way to go in processing all its collections and all the requests it may receive from tribes; the reports I examined represent what can be termed the easiest requests to fulfill.

Buikstra (2006:400–401) discusses the fact that ethnologists, archaeologists, and historians have made strong arguments that Native American oral traditions may reflect ancient historical events. As noted earlier, especially useful is the fact that several scholars have developed methods to validate the substance of these oral traditions (Echo-Hawk 2000; Vansina 1985; Whiteley 2002).

Especially relevant to the subject here, Echo-Hawk (1994, 1997) presented to the Smithsonian Institution a meticulous approach to using information from both oral traditions and archaeology to determine whether or not the Pawnee were culturally affiliated with the prehistoric Central Plains tradition and thus had rights to repatriation. This was the topic of one of the SI reports that tribes challenged. The Repatriation Office did not accept the challenge, so the Pawnee asked that the secretary of the Smithsonian send their appeal to the Repatriation Review Committee for a hearing. It was the first time a hearing was requested by any tribe. The committee accepted Echo-Hawk's approach as well as his interpretations, and human remains from the prehistoric Steed-Kisker site were transferred to the Pawnee (Echo-Hawk 1997).

CONCLUSIONS

To my knowledge, this report data set, as crudely outlined here, is the most complete documentation of how any institution (or even set of institutions) has or has not used evidence to repatriate human remains and funerary objects. But what does it actually tell us? Very little by itself. For the most part, if we look carefully into the reports, we find that the early case reports from the Smithsonian did not use oral traditions and oral histories because the remains and objects being returned were historic and came largely from the Army Medical Museum. Many of these items were from battlefields, forts, or other unsavory collection spots. The Smithsonian case officers rightly focused attention in their reports on specific collectors and their particular histories. Indeed, many of these reports are fascinating accounts of the early development of the Smithsonian and somewhat uncomfortable accounts of how collections got to the museum. If one reads several case reports, one comes to understand that some collectors are more trustworthy than others and that some designations as Kiowa, or Arapaho, or Sioux are more reliable than others.

In other instances, the case officers do not look to oral traditions and histories because the cases are nineteenth century or early twentieth century in nature, and there is ample documentation of exactly what happened, even if it is not a gruesome army situation. This is the case for many groups in Alaska, where Hrdlička and others excavated historic cemeteries, and the documentation of what was done is explicit and specific.

However, it is the third kind of case that should use oral traditions but does not. In these instances, oral traditions are mentioned or discussed, but they are not integrated or analyzed the way that other data are used. It is here that we have to look to improving our relationships with tribes and our knowledge of how to work with these kinds of data. It is here that we must become more comfortable with messy data and try to figure out *why* the oral traditions may not agree with some of the other data, if that is the case. As noted earlier, this kind of analysis may teach us much more about larger cultural and political processes.

Anthropologists may be gatekeepers in that they control access to the collections, but it is relevant to note that, in the Smithsonian case, they also help many native people gain access to a wealth of information stored by the federal government across Washington, D.C. For those tribes who are making or are planning to make a request, the Smithsonian Repatriation Review Committee, on which I sat for 15 years, had (and still has) a travel grant program to bring two tribal representatives per tribe to the Smithsonian to consult with case officers, get oriented to existing documentation, and examine collections containing items that may be subject to repatriation. Further, as a case officer works on a request, it is the policy, at least at the Natural History Museum, to call and consult with relevant tribes. None of the decisions or reports has been made in a vacuum, and there have been significant improvements in these consultations over the last 15 years.

Finally, as I read in more than one report in comments made by tribes, tribes have learned over the years that they get better results if they request specific items or kinds of items rather than asking for "everything." Where does one begin with a request for "everything"? Even if archaeologists are gatekeepers in some ways and in certain senses of the word, it is difficult to respond adequately and promptly to such a request. This increase in sophistication on all sides suggests that perhaps a number of us have learned to work more productively with others.

As long as people see themselves in opposition, I don't think the dialogue or the work will be especially productive or useful. We may not agree on a number of issues, and we may profoundly disagree on others, but there are potentially many ways in which we can move forward. It will, however, require real, open, and honest discussion, as well as new kinds of training for us and for our graduate students.

Notes

1. It is quite startling to discover that most institutions have not documented or prepared written reports on collections subject to repatriation. They have prepared lists for tribes but not necessarily reports on their decisions whether or not to repatriate and on how these decisions were reached. This is a disturbing and troubling discovery that should be addressed by the profession.

2. All comments about data recording assume that the institution has permission from the appropriate group to record, take, and keep such measurements.

3. When a tribe challenges or questions a report, the Repatriation Office may work out the problem or issues with the tribe without the involvement of any other office or individuals. The tribe may present additional evidence, the Repatriation Office may provide a satisfactory explanation, or both. If the tribe is not satisfied, it can request that the secretary of the Smithsonian review the decision, and this review can include bringing in the Repatriation Review Committee for a formal or informal hearing.

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6

An Ancient Cave Mummy from Kentucky

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Unlike the elaborately treated mummies of ancient Egypt or Peru, the human remains addressed here were preserved by natural factors in their burial environments: primarily dryness and lack of weathering. Exsiccation (drying) by minerals in the sediments or on rocky surfaces with which they were in contact also enhanced soft tissue preservation.

The prehistoric individual discussed below was found in Salts Cave, Kentucky, by Euro-Americans in 1875 (Figure 6.1). The remains were known as Little Alice for nearly a century, until their examination by physical anthropologist Louise Robbins (1971). Robbins established that the body was a Native American boy rather than a Euro-American girl or an Indian princess, as some had claimed. Details of Robbins's other findings are summarized, as well as the complex and imperfectly understood history of the discovery and subsequent itinerary of the Salts Cave boy to the present day. In the final section of this paper, I offer comments about the manner in which the boy's body was thought of and treated by the nineteenth- and twentieth-century Euro-Americans who had custody of the remains for various periods of time, concluding with some suggestions about what he might teach us in the future.

The Salts Cave Mummy

During a summer trip to Mammoth Cave National Park in Kentucky, circa 1955–1957, I made a casual visit to a small museum in the old Mammoth Cave post office near the visitor center. Then a graduate student in Near Eastern prehistory at the University of Chicago, I had no thought of undertaking archaeological research in Kentucky. Having recently married a caver (Richard A. Watson), however, a central member of a small group then exploring and mapping big caves in Flint Ridge within the park, I had been drawn into caving there and often joined weekend or summer vacation expeditions.

Included among the arrays of projectile points, ground stone tools, and other artifacts in the old museum was the body of a small person known as Little Alice, who had reputedly been found in one of the nearby caves—Salts Cave in Flint Ridge—in the late nineteenth century. By the time I had committed myself seven or eight years later to an archaeological project in Salts Cave, the mummy was no longer at the park. Eventually I tracked it to the University of Kentucky in Lexington, where the remains had been transferred from Mammoth Cave National Park in 1958. In 1969 the physical anthropologist in the University of Kentucky's Anthropology Department, Louise Robbins, undertook analysis (including X-rays) and dating of the body (Robbins 1971, 1974). As soon as she relocated the remains, one close look was sufficient to overturn nearly a century of folkloric belief that this mummy was female: the external genitalia were still present and unequivocally male.

Other results of Robbins's examination included two radiocarbon determinations on tissue from the lower thoracic and abdominal regions (the uncalibrated dates are 1960 +/- 160 B.P. [M-2258] and 1920 +/- 160 B.P.

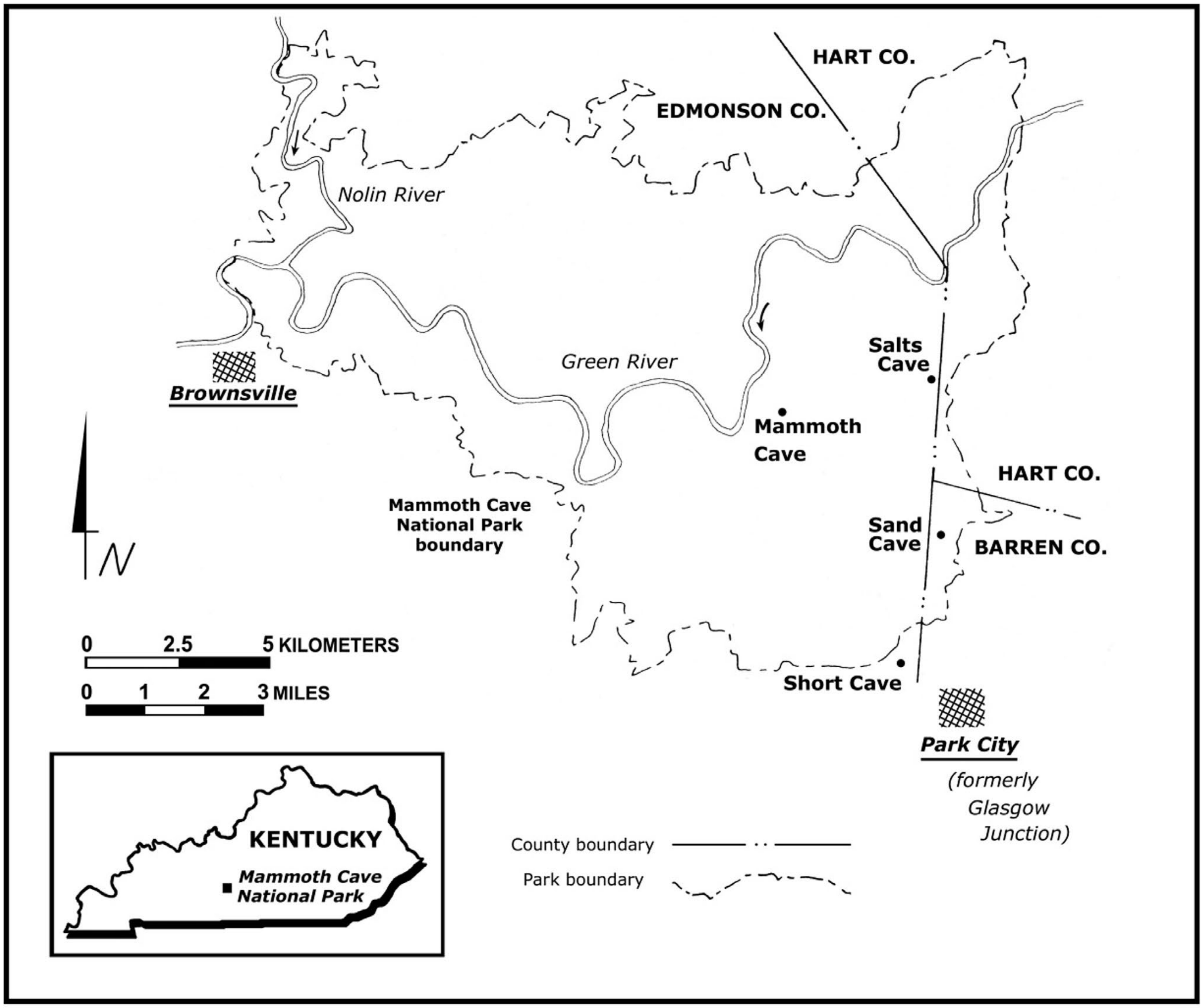

Figure 6.1. Map showing sites mentioned in this paper.

[M-2249]), as well as the little boy's age at death (nine years), the primary components of his last meal or meals (identified by Eric O. Callen and Richard A. Yarnell as hickory nuts, sumpweed, and chenopod; Yarnell 1974, 1978), and an assessment of his general health (good, except for a blood clot in the thoracic region). Robbins suggested that the boy "probably died rather suddenly from an internal hemorrhage resulting from a fall or a blow to the thoracic area" (Robbins 1974:144).

During the initial stages of fieldwork in Salts Cave in the 1960s, Robbins led a crew to the locale where the mummy was supposed to have been found.[1] Soon after the group began careful examination of the passage, one of them noticed a limestone rock bearing a penciled inscription (Watson 1974:25):

> Sir
> I have found one of the Grat wonders of the
> world in this cave, whitch is a muma
> Can all seed hearafter
> found March the 8
> T.E. lee J.L. lee 1875
> an W.d. cutliff
> dicuvers

What has come to be called the discovery rock was found on April 9, 1969, by Kim Dale, a Joint Venturer of the Cave Research Foundation (CRF). Across the passage from the discovery rock, a CRF group recording historic names and dates in the same general part of Salts Cave had previously found another inscribed rock (Stanley Sides, personal communication 2007), which they showed me on May 31, 1969. The message on this rock runs as follows:

> How are you grave robbers. What is it you would
> not do. They is nothing too mean for you to do.
> You low down scoundrls. What is it you wouldn't
> do. Just think for a sec of men to steal the dead
> [unclear; perhaps "Just speculate on it"]
> Sir you are worse than a murderer. Are you not
> afraid it will follow you in your paths of a day
> and your bed at night. You low down dirty
> damed thief of hell yours most [unclear,
> perhaps "respectfully"] J M Smith
> P.S. Call again you honest fellows when you
> get hard up for a few dimes or call at some
> other grave yard.

The second inscription, known as the damnation rock, is undated, but J. M. Smith left his name elsewhere in upper-level passages of Salts Cave with associated dates in the 1870s. Hence, we know he was a contemporary of Cutliff and the Lees.

During the mid-1980s and early 1990s, Tankersley et al. (1994) made further observations at the traditional mummy find-spot in Salts Cave, as well as on the mummy's physical remains at the Museum of Anthropology, University of Kentucky–Lexington. They agreed in general with Robbins's conclusions about cause of death but provided evidence (from SEM examination) for more extensive internal hemorrhaging than she was able to detect, supporting her suggestion that the boy died from internal bleeding, which they thought resulted from a severe fall in the cave. They concluded that he had climbed to a high point in the adjacent breakdown pile, from which he had slipped and fallen to his death. Other members of his group subsequently placed his body on a nearby ledge and left it in the cave.

Postdiscovery History of the Salts Cave Mummy: The Received View (1970s–1980s)

When the CRF Archeological Project was first engaging in research within Salts Cave, a lawyer and avocational historian from Shelbyville, Indiana, Harold Meloy, was pursuing the tangled histories of the various mummies from Mammoth Cave and the surrounding region. What follows here is drawn from Meloy's published accounts (Meloy 1977; Meloy and Watson 1969), with additional reference to an undated newspaper clipping brought to our attention by Cave City resident Ellis Jones.[2]

In 1875 the land including Salts Cave Sink (and the natural entrance to Salts Cave at the bottom of the sink) was widely believed to belong to the Mammoth Cave Estate. Much of Upper Salts Cave, however, underlay the property of Louis Vial. According to his daughter, author of the newspaper story preserved in Ellis Jones's clipping, Vial and his friends explored Salts Cave extensively via a "side entrance known only to themselves." It was during one of these trips that the Salts Cave mummy was discovered. Vial's daughter said that the mummy was found by her father and Bill Cutliff "about 1874." Meloy's information (obtained from archives and local folk traditions) indicated that the mummy was taken from Salts Cave and sold to a local show-cave manager, Larkin J. Proctor, who exhibited it in one of the caves he owned or leased, probably Wright's Cave, also known as Long Cave. At some point during the 1880s or 1890s, show-cave entrepreneur Henry C. Ganter, then managing Mammoth Cave and the Mammoth Cave Hotel, bought the Salts Cave mummy from Proctor and exhibited it as an Indian princess whose mortal remains had been found in Mammoth Cave. According to local oral tradition, the mummy was sent to the Chicago World's Fair in 1893 and was displayed at other exhibitions to promote tourist interest in Mammoth Cave. This may well have been the case, but the 1893 World's Fair mummy is now known to have been "Fawn Hoof," an adult female from Short Cave, not the little boy from Salts Cave.[3]

After Ganter's retirement in 1915, he collaborated briefly with another cave-region promoter, George D. Morrison, who in 1921 opened an artificial entrance to passages of Mammoth Cave that extended beyond the boundaries of the Mammoth Cave Estate. Morrison called his show cave the New Entrance to Mammoth Cave and engaged in fierce rivalry with the post-Ganter Mammoth Cave management for tourist and other public attention. One of the attractions advertised for the New Entrance Cave was the Salts Cave mummy (formerly the Mammoth Cave mummy), now identified as a young white (that is, Euro-American) girl. Meloy succeeded in locating a 1922 brochure that describes New Entrance Cave and refers to

> The Lady of the Cave. The little girl turned to stone; the most interesting and wonderful of all cave phenomena; a little girl, petrified or mummified by the action of the cave air; a mummy that was found in Salts Cave in 1875; that during the 47 years since the discovery, it had been exhibited in the Smithsonian Institution and at various other places by Mr. H.C. Ganter, the former owner and now exhibited at the New Entrance. It is believed that the little girl had been captured by Indians, and rather

> than endure their torture she sacrificed her life [Meloy 1977:12].

During the New Entrance exhibition period, the Salts Cave mummy began to be called Little Alice, a nickname that stuck for many decades. In 1931, when Morrison sold New Entrance Cave to the National Park Commission, which was acquiring property to form Mammoth Cave National Park, the mummy went with the cave, although it was displayed above ground after the government-managed Mammoth Cave was opened to the public. In 1958 the body was transferred to the University of Kentucky, where it remained until Louise Robbins sought it out some 10 years later.

Postdiscovery History of the Salts Cave Mummy: Revised View (1990s–Early 2000s)

After Harold Meloy's death in 1985, several other people developed interest in various aspects of nineteenth-century Mammoth Cave–area history. Three of them—Angelo George of Louisville, Kentucky; Stanley Sides of Cape Girardeau, Missouri; and Norman Warnell of Brownsville, Kentucky—focused to a greater or lesser degree on the discovery and subsequent travels of the Salts Cave mummy. George authored two publications detailing his views (1990, 1994), and Sides and Warnell generously discussed their ideas and interpretations with me.

George suggests that the original discoverers of the mummy may not be the three men whose names appear on the discovery rock. Other claims were made in 1875 by those involved in the cave business (exploration and commercialization) at other locales nearby, especially Grand Avenue Cave (also known as Long Cave or Wright's Cave) near what is now Park City (formerly Glasgow Junction), Kentucky. According to George, Grand Avenue Cave was then owned and managed by George M. Proctor. According to Warnell, who has worked through the actual deeds and titles (archived in the Edmonson County Courthouse, Brownsville, Kentucky), George Proctor's name is indeed on the 1875 Grand Avenue Cave deed, together with the names of his brother and sister-in-law, Larkin J. Proctor and Mary E. Proctor. But George Proctor was actually legally insolvent at the time and in debt to two men who held his promissory note. To settle this debt, the Edmonson County Court eventually intervened and put George Proctor's half interest in Grand Avenue Cave up for sale. In September 1877, Larkin and Mary Proctor paid off George Proctor's note, becoming sole owners of Grand Avenue Cave.[4]

These claims and counterclaims, as well as the entire swirl of events centering upon George Proctor's publicity efforts for the newly commercialized Grand Avenue Cave, and later entrepreneurial efforts by other men for other caves (for example, Mammoth Cave and New Entrance to Mammoth Cave), are quite complex (George 1994:106–121). Because the Salts Cave mummy was a major focus of some of this publicity for more than 50 years (from 1875 to around 1931), I provide a summary of George's interpretation.

George's main point is that "Little Al's 'history' from his 1875 discovery to 1958 is manufactured for publicity purposes to advertise commercial caves. . . . The actual persons who removed Little Al from Salts Cave are not definitely known" (George 1994:120–121). As part of the argument leading to this conclusion, George says, "The inscribed veracity on the 'Discovery Stone' with a date of March 8, 1875 is challengeable. There is no supporting documentation to better frame the event as this point in time" (George 1994:119). George then quotes extensively from an article entitled "That Pickled Squaw," printed in the Louisville *Courier-Journal* on September 27, 1875 (George 1994:112–114, 119–120). The author, unnamed but writing under the byline Fawn, apparently went to Mammoth Cave in September 1875, in a spirit of satirical investigative journalism (as indicated by the title and general tone of the article), to get a firsthand impression of the continuing controversy over the mummy's find-spot: Was it "a cave near Glasgow Junction" (Grand Avenue Cave) or Salts Cave? Fawn seems to have visited Mammoth Cave more than once during that summer of 1875. He says (George 1994:113):

> Early in this summer, during one of my visits to Mammoth Cave, a mummy was offered for sale to the proprietor. The owners of it refused to tell where it had been discovered, but described it as a female lying in a sleeping position with one hand laid across the chin, the other extended, but so nearly decomposed that it would have to be removed with a great deal of care to prevent it falling off. After some deliberation, it was concluded not to make the purchase, and the matter ended. The incident, then, was in a measure forgotten until the recent announcement of the discovery of a mummy in another cave near by revived the subject, while the similarity in every respect to the one which had been found in the summer gave rise to the surmise that the two were one and the same. The intense interest manifested by every

> one caused some of the more curious to make a visit to Salt's cave [*sic*] a few days since.

Fawn then briefly describes the trip made by "some of the more curious" to Salts Cave, culminating in what this group believed to be the place where the mummy had originally lain (George 1994:114):

> Leaving the main avenue you enter a side route, and after climbing over the roughest of roads for a hundred and fifty yards, reach the spot where a mummy was discovered, and from whence it has been recently removed. An indentation in the ground, under a shelving rock, corresponds precisely with that of the disputed mummy, while the mold formed around the body in all these years is still perceptible, with a few locks of its hair, which time has changed to a dark auburn. There can be no doubt that a mummy was here, the dryness of the atmosphere preserving it, as no water is found within the cave excepting at the extreme end of one of the routes.

George (1994:114) says that this is "the first and earliest published description of the discovery site for Little Al." I cannot agree because the description is too vague to establish a specific find-spot anywhere within the main trunk passage of Upper Salts Cave, which is 1.5 to 2 miles long and must be traversed by climbing over, around, and through masses of tumbled breakdown rock (Watson ed. 1969:2–4, 32, Plate 7). There are innumerable niches and rooms of various sizes throughout this jumbled breakdown, as well as so many cut-around and other side passages leading off the main trunk that Fawn's account—like that of Putnam (1875 [1973]), also quoted by George—is useless for pinpointing a specific locale within Upper Salts Cave.

George describes the discovery rock, suggesting that it was "back dated to establish priority" (George 1994:115). He adds (115), "Perhaps both inscriptions are authentic or are forgeries concocted when the cave [Salts] was commercialized in the 1920s."

In the course of his research on Mammoth Cave history, Warnell (2006) has found a number of relevant documents (personal communications 2007). These letters, exchanged among various claimants to Salts Cave and the Salts Cave tract during the 1920s and early 1930s, demonstrate that the Mammoth Cave Estate did not have clear title to Salts Sink or Salts Cave, a fact that emerged in 1896. Further, a local man named Lark (Larkin) Burnett, who made some efforts to commercialize Salts Cave in the mid-1920s (Watson ed. 1969:9; Watson ed. 1974:25), did not have clear title either. Given the long-standing ambiguity about title and ownership, it is not surprising that late-nineteenth- and early-twentieth-century attempts to make the Salts Sink entrance the gateway to a show cave were not very impressive. Whether the rather feeble efforts included forging one or both of the inscribed rocks discussed above is unknown. It is certainly the case that owners of show caves and their employees would literally stop at nothing to catch and retain the attention of tourists (the late nineteenth and early twentieth century in the Mammoth Cave region is still referred to as the "cave wars" period), hence—although I think that Cutliff and the Lees truly were authors of the discovery rock—forging a couple of inscriptions to attract and entertain visitors from near or far is a plausible suggestion.

As noted earlier, the authors of the discovery rock dated their inscription, but the damnation rock is undated. There is some further information relevant to the missing date (assuming the author was indeed J. M. Smith). According to Warnell, archives at Brownsville, Kentucky, and the *Register of Prisoners Confined in the Kentucky Penitentiary* show that on December 23, 1873, J. M. Smith was jailed in Brownsville on charges of murder (Warnell, personal communications 2007; see also Warnell 2006:35–36). On March 20, 1874, he was released on bail. On March 19, 1875 (11 days after Cutliff and the Lees found the Salts Cave mummy, according to the discovery rock), J. M. Smith's case came up for hearing, but it was postponed and he was released on bond. Six months later, on September 21, 1875, J. M. Smith was tried by jury, found guilty of voluntary manslaughter, and sentenced to four years in the Kentucky State Penitentiary. He entered prison on September 27, 1875 (coincidentally, this is the same day Fawn's article appeared in the Louisville *Courier-Journal*), and remained there until September 27, 1879.

Therefore, if the Salts Cave mummy was found on March 8, 1875, by Cutliff and the Lees, and if J. M. Smith did indeed author the undated damnation rock inscription (that is, if the inscription and/or his name was not forged or counterfeited by someone else as a move in the cave wars), then he had to have been in Salts Cave between March 8 and September 20, 1875 (excepting the few days between March 15 and March 19, 1875, when he would have been in custody while his case came up for hearing). Hence, assuming that the mummy was removed from the cave soon after discovery, the inscription date for the damnation rock was most likely mid- to late March 1875. One can readily understand the anger expressed by Smith in his inscription if—in the midst of his legal problems—he found that his sometime caving companions had completely excluded him from a sensational discovery they had made in his absence.

Given the above contextual details, I think it more likely that the contents of the two inscriptions are authentic than that they were both faked decades later to create a tale for such few tourists as may have visited Salts Cave prior to 1931. However, whatever the true status of these two inscriptions vis-à-vis empirical events in and around Salts Cave, Mammoth Cave, and Glasgow Junction in the spring and summer of 1875, the rocks are historic documents vividly conveying the cave wars atmosphere of late-nineteenth-/early-twentieth-century west-central Kentucky.

Discussion

The most striking theme with respect to the Salts Cave boy's nineteenth- and twentieth-century history is clearly the manner in which he was perceived and how the personae attributed to him were created and manipulated by those who made the original discoveries or gained control of the physical remains. Even the much more recent find of an adult male mummy in Mammoth Cave (Neumann 1938; Pond 1935, 1937; Robbins 1974) was viewed by the management more as a publicity gimmick and tourist attraction than as a valuable source of knowledge about an ancient individual and his time and place. Upon discovery in 1875, the Salts Cave mummy was immediately viewed as a commodity to be sold to the highest bidder, the bidders being primarily interested in the body for its cash value as an exhibit attractive to tourists.

Commodification of human remains, especially the nineteenth-century travels of the Salts Cave mummy and of Fawn Hoof from Short Cave, might simply be attributed to ethnocentrism and racial prejudice, but in fact there are striking similarities between what happened to these pre-Columbian bodies and what happened to the historic Euro-American body of Floyd Collins. Collins died in 1925 after having been trapped in Sand Cave, a short distance east of Mammoth Cave (Crothers 1983, Collins and Lehrberger 2001, Halliday 2004, Murray and Brucker 1979). His body was eventually recovered and buried in his family's cemetery, now inside Mammoth Cave National Park but on privately owned land at the time. When the Collins property, including Great Crystal Cave (discovered by Floyd Collins in 1917 and developed into a not-very-successful show cave by the Collins family), was purchased in 1927, the new owner was granted the right to exhume Floyd Collins's coffin and place it within the cave. This was clearly a move calculated to capitalize on the nationwide publicity attending the long-drawn-out and ultimately unsuccessful attempts to rescue Collins, whose reembalmed remains were then shown (through a glass lid with which the coffin was provided by the new cave owners) to those purchasing cave tour tickets between the late 1920s and early 1940s. The story of Floyd Collins's entrapment and death in Sand Cave is still very much alive and still attracts tourists to the area surrounding Mammoth Cave National Park (Benton 2009, Watson 2009:151).

Commodification of human remains from royal or otherwise famous individuals is a venerable theme in European history, perhaps best exemplified by the medieval trade in bones taken from the skeletons of saints and martyrs (Weiss-Krejci 2005). Weiss-Krejci relates, for example, what happened to the osteological remains of King Louis IX, who died in 1297 and was buried in the monastery of St. Denis north of Paris. His grandson, Philippe le Bel, wanted to transfer the body to Paris, but the monks refused to give it up. In 1304, however, Philippe was able to send one of Louis's finger joints to the king of Norway, and in 1306 other portions of the skeleton were taken to Paris. The mandible remained in St. Denis, but the cranium went to the Sainte-Chapelle and one rib to Notre Dame. Although Louis IX was not yet officially a saint (he was canonized in 1324), his mortal remains were highly desirable trophies. Possession and display of such relics not only enhanced the visitation rate and holiness quotient of abbeys, monasteries, and churches but also brought in more and larger donations than would otherwise have been received. Like the cave mummies just discussed, the relics had significant cash value and exemplify one of the many ways in which postfunerary populations profit by interacting with the dead (Rakita et al. 2005; see especially sections 2 and 3).

In spite of all the vicissitudes endured by the Kentucky cave mummies, they have taught us something about themselves and about the communities they represent. Basic information concerning sex, age at death, manner of death, and when death occurred (years before present) is known for each of them. Radiocarbon determinations on tissue indicate that the Short Cave mummy, Fawn Hoof, is approximately a millennium older than the Salts Cave and Mammoth Cave mummies (Horton 2003). Fawn Hoof's tissue date places her at the Archaic/Early Woodland boundary, whereas the other two are in or near the transitional period between Early and Middle Woodland. The Salts Cave mummy was a little boy no more than nine years old at the time of his death. Fawn Hoof, a mature female, was roughly the same age as the Mammoth Cave man, about 45. Fawn Hoof was buried with a considerable quantity and diversity of grave goods (which have recently provided a significant amount of technological and other cultural information), possibly in a cave dark zone but near the entrance, whereas the other two died accidentally far back in the dark zones of their respective caves. There is some indication that both the Salts Cave boy and the Mammoth Cave man may have been engaged in ceremonial activities when they died (Crothers 2012, Crothers et al. 2002, Munson et al. 1989)—the boy from a bad fall; the man crushed by a boulder he had undermined while digging the sediment beneath it. The archaeological context for the Salts Cave and Mammoth Cave mummies is fairly well-known (Crothers 2012, Crothers et al. 2002; Watson ed. 1969, 1974), but saltpeter mining of the early nineteenth century and several activities in Short Cave during the late nineteenth and early to mid-twentieth century (including laying an asphalt road leading to a bandstand built inside the cave, and blasting and bulldozing in an attempt to create a second opening at the far end) have destroyed the original cultural deposits (George 1994:55–56, Horton 2003, O'Malley 1986). Even so, careful attention to artifacts curated for more than a century at the Harvard Peabody and the Smithsonian Institution museums has recently yielded valuable information about the Short Cave mummy and her social role in life. Fawn Hoof's grave goods indicate that she was a woman of accomplishment, possibly a healer, wise woman, or shaman (Horton 2003).

Empirical dietary evidence has been obtained from the intestinal tracts of the Salts Cave and Mammoth Cave individuals, but there is no such comparable evidence for Fawn Hoof and cannot be because only her bones remain. Robbins was able to get X-rays of the Salts Cave mummy, but there are none for the Mammoth Cave and Short Cave bodies. Further radiographic work using modern techniques such as CT scanning would surely enable better age estimates as well as more detailed biohistories for each of these individuals (Komar and Buikstra 2008:258–281; Lynnerup 2007:170–173). If the requisite permissions were forthcoming, it would also be highly desirable to secure more AMS dates from soft or hard tissue samples for all three individuals. Equally important would be comparative isotopic and DNA analyses of the three mummies and of other individuals from the same, earlier, and later time periods in west-central Kentucky. Because artifactual remains in cave dark zones are not often directly comparable to those found above ground in rockshelters or open sites, we do not know whether those who spent long periods in cave interiors were members of local populations or came from other locales. Isotopic, biochemical, trace element, and DNA analyses might provide relevant information, and stable isotope data (e.g., Buikstra et al. 2005; see also Wright and Perry et al. this volume) could furnish additional evidence for health and diet, especially important for the Short Cave mummy. There is no scalp hair available for any of the three mummies, so high-resolution stable light isotopic analyses of their individual life histories (e.g., Wilson et al. 2007) is not possible.

Clearly, none of the Kentucky cave mummies provides an ideal bioarchaeological case study. Two of the three were removed from their primary contexts 201 and 137 years ago, respectively, before archaeology in the United States was professionalized. The other was documented by professionals at the time of discovery (Neumann 1938; Pond 1935, 1937), but that was 40 years prior to Jane E. Buikstra's first discussions of what she came to call bioarchaeology (Buikstra 1977, 2006). Nevertheless, painstaking historic research and physical anthropological and archaeological study during the 1960s, 1970s, and later have furnished significant pre- and postrecovery information for each individual. Thanks to conceptual and technological advances in bioarchaeological inquiry during the past three decades, there is now considerable potential for future interactions with these cave mummies by specialists in the subdiscipline defined by Buikstra and so well exemplified by her own work and that of her students.

Acknowledgments

Thank you, Nene and Barra, for inviting me to contribute a paper to this volume. I am grateful to Patrick Payne for creating the map appearing here as Figure 6.1. I am deeply indebted to Stanley Sides and Norman Warnell, both experts on early Euro-American history in the Mammoth Cave region, above ground and below. Drawing upon their own research, they promptly answered a long series of my questions about nineteenth-century people, land claims, and commercial caves. Norman kindly sent me many details from his encyclopedic knowledge of local genealogies and

countless records pertaining to land patents, deeds, and titles. Stan, a valued consultant at every stage of the CRF Archeological Project's research (he was centrally involved in helping Louise Robbins secure X-rays of the Salts Cave mummy in 1969 and was also a member of the CRF group that rediscovered J. M. Smith's damnation rock inscription), speedily obtained via the Internet a copy of the 1973 reprint publication containing F. W. Putnam's account of his 1874 visit to Salts Cave. Thanks to P. Willey, who sent me a copy of Lynnerup's 2007 synthesis of mummy research, past and present. Finally, I am grateful to Angelo George for his stimulating publications concerning caves, mummies, and early commercialization efforts in the Mammoth Cave region. His suggestions concerning nineteenth- and early-twentieth-century activities in and around Salts and Mammoth caves caused me to revisit and rethink earlier understandings concerning key events and personnel surrounding the cave mummies discussed in this paper.

Notes

1. The Cave Research Foundation Archeological Project (which I had the privilege of directing for 40 years) was initiated in 1963, with some financial support obtained by Joseph Caldwell, then head curator of archaeology at the Illinois State Museum. Caldwell also arranged for his assistant curator, Robert Hall, to participate in that first summer's fieldwork (Salts Cave 1963). See Watson ed. 1969, 1974, 1997 and Watson 2012 for project results.

2. The reference for this newspaper article, "Reminiscences of Mammoth Cave" by M. Carrie Morgan, *Glasgow Times*, is provided by Angelo George (1994:149, Note 9).

3. As indicated here, the Salts Cave mummy was provided with varying proveniences and identities to suit the needs of those controlling the physical remains at any specific time. Meloy (1977:9–10) notes that the Salts Cave mummy was often confused—sometimes deliberately—with another mummy, Fawn Hoof, found around 1811 in nearby Short Cave (Horton 2003). The mummy at the 1893 Chicago World's Fair (and at the 1876 Philadelphia World's Fair, as well as at the Smithsonian) was the Short Cave mummy.

4. Angelo George (1994:119, 121) also thinks it was George M. Proctor rather than his brother Larkin J. Proctor who purchased the Salts mummy. Meloy (1977:7, 9) says it was Larkin, as do Stanley Sides and Norman Warnell (personal communications 2008), although Sides notes that George Proctor might have bought the Salts Cave mummy while acting for his brother Larkin. At any rate, Warnell's data make it quite clear that George Proctor was legally in debt and insolvent in 1875, so he had no disposable income of his own.

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7

Ossuary III from the Juhle Site, Nanjemoy, Maryland: Bioarchaeological Features

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As noted by Buikstra and Beck (2006) and Larsen (2001), the dynamic field of bioarchaeology involves complex interpretations that integrate data from the analysis of human remains with those from history, archaeology, and other related scholarly areas. Such research leads to key information relating to human remains in their archaeological context and the complex social factors that can be associated with them (Chacon and Dye 2007). Although the scope of research within bioarchaeology is extremely broad (Buikstra and Beck 2006), problems relating to the analysis of commingled human remains are among the most challenging (Adams and Byrd 2008). In such cases, standard approaches to analysis (Buikstra and Ubelaker 1994) frequently must be supplemented with problem-specific methodology. Complexity of analysis is enhanced when deposits of commingled remains contain many individuals with extensive skeletal disarticulation. Ossuaries from the mid-Atlantic area of the United States represent such complex deposits.

Mortuary procedures varied extensively among the late prehistoric and early historic American Indians in the mid-Atlantic area of the United States (Boyd and Boyd 1992). These customs included secondary burial, which in some areas resulted in ossuary deposits (Turner 1992). As noted by Ubelaker (1974:8), the term *ossuary* has been applied very generally to burials of multiple individuals. Following Ubelaker 1974, the term is used here to describe largely secondary deposits of multiple individuals.

Ossuary burial is well-known and documented for the mid-Atlantic area of the United States. Although thorough, direct descriptions of the detailed ossuary burial practice are not available in the ethnohistorical record, considerable indirect evidence, summarized in Ubelaker (1974), references the practice of temporary placement of the dead on scaffolds and/or in death houses, systematic cleaning of the bones, and final interment in ossuaries, especially in the coastal plain area of Maryland and Virginia.

Ubelaker's summary of reports of ossuary burial in the mid-Atlantic area lists 34 possible deposits in Virginia, Maryland, and southern Delaware (1974). Although detailed information is lacking for many of these sites, the number of individuals in each deposit varies considerably, with some containing more than 100 individuals. Curry (1999) summarizes that more than 30 ossuaries have been reported from the Maryland area. Ubelaker (1974) and Curry (1999) note that the Maryland ossuaries include evidence of burning on bones, ceremonial fires associated with burials, cut marks on bones likely associated with defleshing, variation in arrangement of bones within pits, the use of crania as containers, and variable artifact presence.

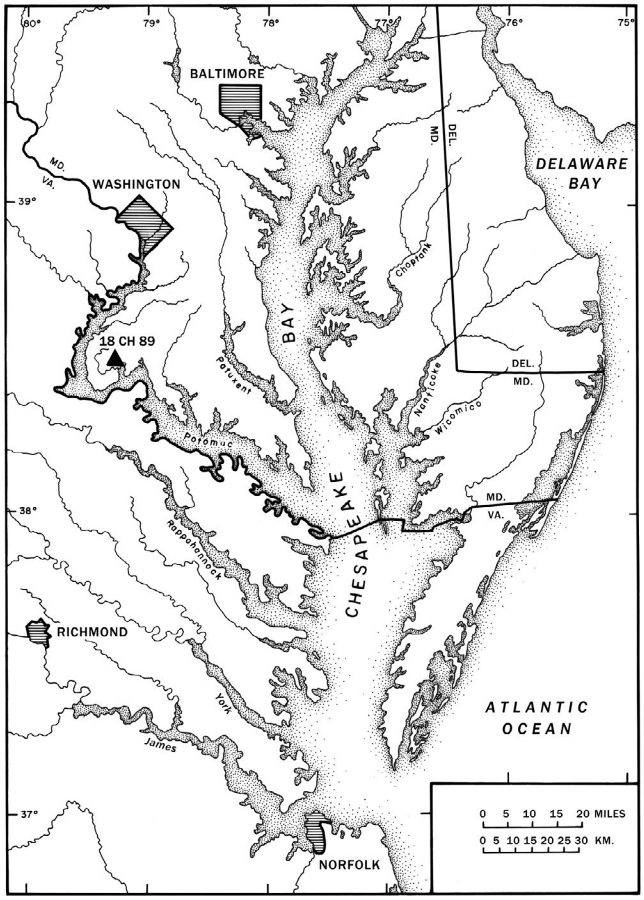

Figure 7.1. Location of Juhle Site, 18CH89.

Ossuaries from Nanjemoy Creek

Two ossuaries have been reported in detail from the Juhle Site (18CH89), dating from the late Woodland period in Charles County, Maryland (Figure 7.1). Both of these ossuaries were located on a bluff overlooking Nanjemoy Creek, a tributary of the Potomac River, and they appeared to be associated with a small habitation area (Ubelaker 1974). The first ossuary was excavated by T. D. Stewart between 1953 and 1955 and contained relatively few artifacts and at least 131 individuals (Ubelaker 1974).

The second ossuary from this site was excavated in 1971–1972 by Stewart and D. H. Ubelaker (Ubelaker 1974). This ossuary was located approximately 30.5 m northwest of Ossuary I and measured about 5.2 m long by about 2.1 m wide. At least 188 individuals were represented in the sample. As with the first ossuary, relatively few artifacts were present. Analysis noted that some remains were articulated, and some evidence of burning was present on the bones.

Ossuary III

In the autumn of 1979, the Juhle family contacted the Smithsonian Institution, reporting that an animal burrow had been found just southeast of the second ossuary, which was excavated in the early 1970s. Bones were detected within the backdirt of the animal burrow.

On November 16, 1979, Stewart and Ubelaker visited the site to recover the disturbed remains associated with the animal burrow and to explore any archaeological features. They noted that the bones recovered included human remains, and they excavated sufficiently to determine that the animal disturbance appeared to be associated with a third ossuary from that site.

The total weight of the disturbed material recovered from the animal burrow was 2,340 g and included four nonhuman animal bones, 15 shells, two wood fragments, 10 stones, and two pottery shards. Human remains in the disturbed sample included fragments representing at least three adults and one immature individual, with the largest fragment (from a left femur) weighing 117 g.

Intensive excavation of Ossuary III began on June 2, 1980, and continued until about September 19, 1980. Initially, excavation concentrated on the removal of topsoil to reveal the entire pit outline, detected at depths ranging from 20 to 22 cm. Once the entire pit outline was exposed, a 1-m grid system with a north-south orientation was organized over it (Figure 7.2). The ossuary intersected 15 of the 1-m squares. Excavation was complicated by the invasion of roots from a tree stump on the southwestern side of the ossuary. The area of disturbance caused by the animal burrow that led to the discovery of the ossuary was located in Square 5 on the east side.

Once the entire pit outline was visible, excavation proceeded downward within the grid system. Soil was carefully removed and screened to reveal bone and related material in situ. All bone concentrations were carefully cleaned for documentary photography and note taking. Any evidence of articulation was noted and given an articulation number. All crania were also noted and given individual numbers. Human remains removed from the ossuary, including the articulated bones and crania, were placed in bags labeled with the squares they were found in, dates of removal, and other relevant information. Excavation began in squares 1 and 2 on about June 12 and proceeded generally sequentially to Square 15.

When all the ossuary contents had been excavated and removed, the remaining pit was photographed

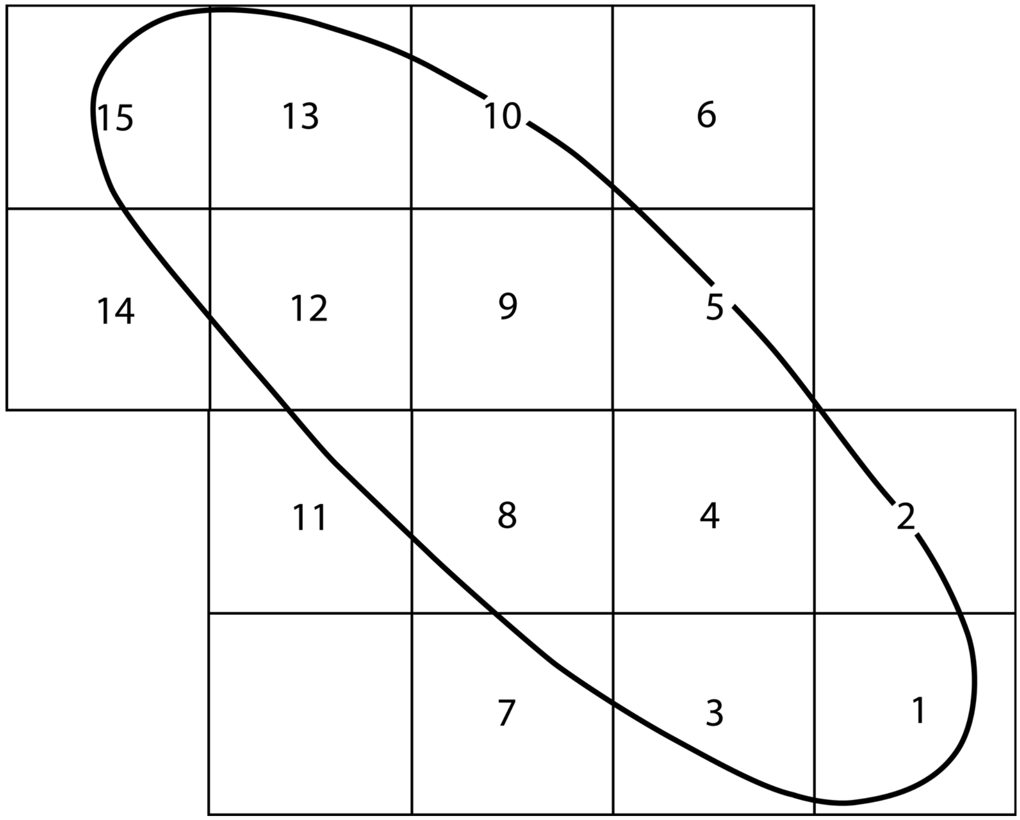

Figure 7.2. The 1-m grid system utilized in the excavation of Ossuary III.

(Figure 7.3) and the entire area was filled in with screened backdirt. Small particles of charcoal and occasionally burned bone were noted on the pit floor.

Location and Orientation of Crania

Seventy-seven crania were detected and given individual numbers during the excavation of Ossuary III. Of these, 14 (18 percent) were found resting on their left sides; 20 (26 percent) were found resting on their bases; 8 (10 percent) on their facial areas; 15 (19 percent) on their vertexes, and 16 (21 percent) on their right sides. Information on position is not available for four of the crania. Thus the crania were found in many different positions, and no evidence of placement in a particular position was detected.

Facial orientations of the crania (the direction each was facing) within the ossuary also were varied. Of the 77 crania, facial orientation was listed as 6 (8 percent) north, 8 (10 percent) northwest, 8 (10 percent) west, 7 (9 percent) southwest, 6 (8 percent) south, 10 (13 percent) southeast, 13 (17 percent) east, 5 (6 percent) northeast, 6 (8 percent) of unknown orientation, 6 (8 percent) face down, and 2 (3 percent) face up.

Articulations

As noted above, all evidence of skeletal articulation was recorded during excavation. Each observed articulation was given a number, documented as to location, and packaged separately for later analysis. In contrast to Ossuary II (Ubelaker 1974), no completely articulated individuals were observed during the excavation. A total of 197 examples of partial articulation were noted, and each was analyzed separately in the laboratory.

Inventory of the individual bones represented by the articulations provides detailed information on the areas of the skeleton and the individuals represented. For immature remains, three individuals were represented by right femora. Two individuals were represented by frontals, right maxillae, sacra, right ilia, vertebrae, ribs, humeri, and the right radius. Single individuals were represented by the mandible, left clavicle, manubrium and gladiolus of the sternum, right scapula, left ilium,

Figure 7.3. Ossuary III pit following excavation and removal of contents.

right pubis, left radius, left ulna, right femur, right tibia, right fibula, and foot bones.

Sorting of the immature bones from the articulations by bone type and age suggests that at least six individuals are present. Two newborns are represented by the left humerus. Two children, likely between the ages of three and four, are represented by left femora. A seven- to eight-year-old child is represented by a cranium and mandible. A 10- to 15-year-old individual is represented by vertebrae and sternum segments.

Immature articulated remains were recovered from eight of the 1-m squares within the ossuary: squares 1, 2, 3, 4, 8, 9, 10, and 13. These squares are distributed throughout the ossuary; thus the distribution of the immature articulations does not present any spatial clustering.

Of the articulated bones of adults, 13 individuals were represented by thoracic vertebrae, 11 by lumbar vertebrae, 6 by crania, mandibles, right radii, right ulnae, left tibiae, and left fibulae, 5 by the third through seventh cervical vertebrae, right tibiae, and foot bones, 4 by left radii, right fibulae, sacra, and hand bones, 3 by left ulnae and first cervical vertebrae, 2 by right femora, second cervical vertebrae, and right innominates, and 1 by a left femur and ribs.

Both sexes are represented by the articulated remains of both young and old adults. Although sex was not apparent for most of the articulated remains, the crania from two articulations were judged to likely represent females. The leg bones as well as the crania of two articulations were thought to represent males.

Articulated adult remains were found in nearly all the squares that had skeletal content. Specifically, articulated adult remains were recovered from squares 1, 2, 3, 4, 5, 7, 8, 9, 10, 11, 12, 13, and 14. As with the immature articulated remains, the adult articulated deposits were distributed throughout the ossuary and showed no clustered pattern.

Bone Weights

As noted above, a 1-m grid system was employed within the ossuary to examine spatial issues. Skeletal remains were found within 15 squares. Depth from the surface to the highest bone ranged from about 26 cm in Square 7 to 35 cm in Square 2. The deepest area of bone concentration (about 71 cm) was in the area of the junction of squares 4, 5, 8, and 9.

Table 7.1 presents the distribution of bone weight in grams from the various 1-m squares within the ossuary. Note that 3,711 g of bone (2.42 percent of the total) could not be associated with any particular square. These remains mostly represent those initially recovered from the disturbed area associated with the animal burrow, and they most likely originate from Square 5.

Table 7.1 reveals that the greatest concentration of remains was found in squares 3, 8, 9, and 12—squares located near the center of the ossuary. The total bone weight of 153,432 g represents approximately 67 percent of the 233,900 g of bone recovered from Ossuary II (Ubelaker 1974:22).

Table 7.1. Distribution of Bone Weight Within Ossuary III.

Square	Weight (g)	% Total Weight
1	3,886	2.53
2	4,242	2.76
3	23,484	15.31
4	14,565	9.49
5	5,742	3.74
6	1,420	.93
7	5,510	3.59
8	21,264	13.86
9	25,387	16.55
10	8,114	5.29
11	3,924	2.56
12	16,279	10.61
13	14,963	9.75
14	769	.50
15	172	.11
Other	3,711	2.42
Total	153,432	100.0

Spatial Distribution of Types of Bones

To investigate if any nonrandom spatial patterning of the remains could be detected, all recovered remains were labeled with the number of the square in which they were found. All remains were then sorted by bone type, with mature remains separated from those showing evidence of immaturity (for example, lack of epiphyseal union), and were weighed.

Tables 7.2 and 7.3 present the percentages of the total weight distribution of each bone category present in each square for adults and subadults. For example, of the total weight of adult femora in the ossuary, 2.5 percent was in Square 1, 1.9 percent in Square 2, and so on. Comparison of these values with the percentage of all bone weight in each square assists in detecting any nonrandom distribution patterns within the ossuary. Square 1 contained 2.5 percent of all bone weight within the ossuary but 6.7 percent of the weight of adult first cervical vertebrae and only .1 percent of the adult innominate weights. Although some variation is apparent, perusal of these data suggests no clear clustering by age or type of bone.

Minimum Number of Individuals

Tables 7.4 and 7.5 present the minimum number of individuals represented by each type of bone for both subadults and adults. The numbers were established by examining all bones of each type and sorting them by size and shape. For example, all mature left femora were compared with each other to establish the minimum number of individuals represented by that bone type. Fragments were assumed to relate to a single bone if no anatomical aspects were duplicated and if the size and shape were consistent. For subadults, this process suggested that at least 37 individuals were represented by the right femur and the right ilium (Table 7.4). In contrast, no immature coccygeal vertebrae were found, and only two individuals were represented by carpal and tarsal bones. Adults were most commonly represented by the right temporal (74), followed by the left temporal (66) and the right femur (65). Only 9 individuals were represented by middle and distal foot phalanges, and 17 individuals were represented by the left triquetral of the hand.

Sex Distribution

Since sex cannot be estimated reliably from immature remains, such estimates in this study were confined to mature individuals. Since the remains were commingled and for the most part not separated into individuals, estimates of sex had to be made from individual bones. Of all the bones of the skeleton, those of the pelvis provide the most reliable indicators of sex. For the ossuary remains, all mature bones of the pelvis were separated into left and right sides and then assessed for sex indicators, considering all available evidence. This procedure suggested that at least 14 males and 27 females were present, represented by mature left pelvic

Table 7.2. Distribution of the Percentage of the Total Square Bone Weight Represented by the Weight of Adult Bone Categories Within Ossuary III.

	Square Number															
Skeletal Element	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13**	**14**	**15**	**Other**
Femur	2.5	1.9	17.5	11.0	4.1	.4	1.6	17.6	9.9	4.5	2.5	12.8	11.0	.4	NDA[1]	2.4
Tibia	1.5	1.7	13.2	8.7	3.2	1.0	3.3	15.4	15.2	6.6	3.7	11.8	11.3	.7	NDA	2.9
Fibula	3.4	2.4	10.2	9.3	3.9	NDA	5.4	15.1	16.1	5.4	2.9	11.2	10.2	.5	NDA	3.9
Humerus	4.4	2.0	13.8	7.8	1.6	1.8	.4	15.1	16.2	4.9	1.3	11.6	14.4	.9	NDA	3.8
Radius	1.5	2.3	14.5	9.9	1.5	NDA	4.9	9.1	21.3	6.1	1.5	9.9	12.9	NDA	NDA	4.6
Ulna	.6	1.7	15.4	10.4	2.8	1.1	4.4	14.3	14.8	9.3	1.1	10.4	10.4	1.1	NDA	2.2
Clavicle	1.9	.9	22.4	11.2	1.9	.9	1.9	11.2	16.8	5.6	.9	11.2	11.2	.9	NDA	.9
Patella	3.3	1.3	15.7	14.5	5.3	3.9	2.3	9.8	15.2	7.8	2.7	5.1	8.0	NDA	NDA	5.2
Scapula	1.3	2.0	12.4	13.1	2.0	.7	7.2	13.7	17.7	5.2	1.3	13.7	7.8	NDA	NDA	2.0
Hand/foot bones	4.3	4.3	19.1	10.1	4.3	1.6	6.1	9.9	14.4	6.5	1.4	7.9	8.1	.5	NDA	2.0
Atlas	6.7	3.8	11.4	7.1	3.4	3.9	4.8	10.9	20.6	5.5	3.6	12.6	2.0	.3	NDA	3.5
Axis	1.9	3.1	16.2	7.8	1.7	2.7	1.8	9.8	31.4	9.6	2.6	7.4	2.1	NDA	NDA	1.9
Cervical	4.0	2.0	18.0	12.0	4.0	2.0	4.0	8.0	12.0	6.0	2.0	12.0	10.0	NDA	NDA	4.0
Thoracic	3.9	2.8	16.9	9.0	2.3	1.1	4.5	12.4	20.8	7.3	1.7	9.0	7.9	.3	NDA	.3
Lumbar	1.7	4.6	23.9	8.6	2.9	.6	4.0	10.3	18.2	5.1	2.9	9.1	7.4	.3	NDA	.6
Other vertebra	4.2	4.2	20.8	8.3	2.1	2.1	6.3	8.3	10.4	8.3	2.1	6.3	6.3	6.3	NDA	4.2
Innominate	.1	4.9	22.8	8.2	.8	.3	4.9	14.6	11.6	7.8	2.4	10.5	10.5	.1	NDA	.5
Cranium	1.9	2.9	14.0	8.5	5.4	.5	4.2	14.2	22.4	3.2	2.4	8.1	8.7	< .1	NDA	3.6
Mandible	1.9	NDA	22.4	4.7	3.7	3.7	1.9	16.8	17.8	5.6	.9	11.2	8.4	NDA	NDA	.9
Total	2.5	2.8	15.3	9.5	3.7	.9	3.6	13.9	16.6	5.3	2.6	10.7	9.8	.5	.1	2.4

1. NDA = no data available.

Table 7.3. Distribution of the Percentage the Total Square Bone Weight Represented by the Weight of Immature Bone Categories Within Ossuary III.

	Square Number															
Skeletal Element	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13**	**14**	**15**	**Other**
Femur	5.1	3.3	4.7	5.4	.7	.6	.5	8.8	36.0	4.2	10.0	16.3	4.0	NDA[1]	NDA	.3
Tibia	4.2	.6	3.5	7.9	6.3	.5	.6	13.4	12.9	.6	8.9	30.2	7.4	.2	NDA	2.8
Fibula	NDA	NDA	3.4	4.9	7.4	NDA	.6	21.2	21.3	11.5	7.0	8.3	7.4	NDA	NDA	6.8
Humerus	.5	.2	1.2	10.9	1.7	.7	.5	9.6	22.5	4.5	8.4	28.7	8.1	NDA	NDA	2.5
Radius	10.1	.2	2.3	13.4	7.0	.1	NDA	22.6	22.5	3.6	3.2	9.2	4.6	NDA	NDA	1.3
Ulna	2.8	.1	3.1	13.3	9.7	1.4	NDA	21.9	20.9	NDA	5.7	9.0	4.1	NDA	NDA	8.1
Clavicle	5.2	.2	5.6	14.6	6.8	.8	1.6	8.6	24.2	2.1	5.4	12.5	12.3	NDA	NDA	.2
Patella	NDA	NDA	NDA	NDA	4.5	NDA	NDA	NDA	17.3	3.8	NDA	60.9	13.5	NDA	NDA	NDA
Scapula	1.1	.2	4.6	9.7	7.4	1.6	.7	18.6	35.1	3.6	8.2	4.9	4.3	NDA	NDA	.1
Hand/foot bones	3.0	.5	2.0	9.0	6.9	6.6	.1	13.4	20.9	6.4	3.3	19.4	7.1	NDA	NDA	1.5
Atlas	NDA	4.8	11.2	16.3	NDA	NDA	NDA	5.2	37.9	NDA	6.8	10.8	4.0	NDA	NDA	3.2
Axis	9.0	NDA	NDA	17.4	NDA	8.0	NDA	30.6	23.2	6.8	NDA	3.6	NDA	NDA	NDA	1.4
Cervical	NDA	NDA	.6	17.2	10.5	.4	NDA	9.1	45.1	1.4	1.3	10.1	4.3	NDA	NDA	NDA
Thoracic	3.5	3.7	12.8	20.3	8.4	.8	.2	7.1	22.8	2.0	.9	9.0	4.9	3.2	NDA	.6
Lumbar	5.7	NDA	5.3	16.7	7.9	.5	1.2	11.7	20.2	.5	.3	12.2	16.4	NDA	NDA	1.2
Other vertebra	5.2	1.4	6.8	8.9	6.4	3.7	2.7	18.9	20.9	3.9	2.6	9.3	6.6	.1	NDA	2.8
Innominate	1.2	8.6	16.6	17.0	8.5	1.7	.3	15.8	16.7	1.7	.5	9.3	2.1	NDA	NDA	.2
Cranium	8.3	6.2	9.1	9.3	2.0	.1	5.2	4.3	20.3	2.8	.1	2.4	13.7	NDA	5.1	11.2
Mandible	2.4	.1	5.7	20.5	7.5	NDA	.3	12.0	20.5	NDA	1.0	19.5	10.5	NDA	NDA	.1
Total	2.5	2.8	15.3	9.5	3.7	.9	3.6	13.9	16.6	5.3	2.6	10.7	9.8	.5	.1	2.4

1. NDA = no data available.

Table 7.4. Minimum Number of Individuals Represented by Immature Bone Types.

Bone	Left		Right
Humerus	36		32
Radius	26		23
Ulna	31		33
Femur	36		37
Tibia	32		29
Fibula		36	
Clavicle	29		31
Scapula	25		32
Temporal	28		32
Maxilla	13		15
Mandible	26		27
Gladiolus		8	
Manubrium		7	
Ilium	34		37
Ischium	23		23
Pubis	18		17
Patella	5		5
Rib		14 (311)[1]	
Vertebrae[2]		17 (249)	
Sacrum		8	
Coccyx		0	
Calcaneus	13		11
Talus		13 (25)	
Other tarsals		2 (29)	
Carpals		2 (21)	
Metatarsals or metacarpals		10 (186)	
Phalanges		5 (225)	

1. Numbers in parentheses indicate actual number of bones.
2. Cervicals, thoracics, lumbars.

bones. Consideration of cranial morphology, coupled with pelvic analysis, suggested the presence of 25 males.

Estimation of Age at Death

For the mature remains, important information on age at death can be derived from the bones of the pelvis and skull. All mature pelvic bones were separated into left and right sides and then arranged in order of increasing age. In this procedure, the age progression was first established by arranging those bones that presented evidence of pubic symphysis. Next, bones that contained an auricular area were added to the sequence. The result was a progressive age sequence of pelvic bones extending from young adult to old adult. This sequence was then divided into five-year age intervals using standard methods of appraising pubic symphysis, auricular area, and related morphological indicators (Buikstra and Ubelaker 1994). The maximum age was judged to be between 65 and 70 years, consistent with the maximum age established from the previous study of ossuaries from this site (Ubelaker 1974). Noting the sex distribution of these bones as well, estimates from the left and right sides were compared.

A similar sorting procedure was employed for bones of the adult skulls. After separation into male, female, and sex-undetermined groups, the remains were assigned the most likely age at death. The resulting information was compared with the sex and age profile produced from the pelvic information, and the nonduplicated information was merged with it to produce a combined comprehensive sex and age-at-death profile. This process produced age-at-death information for 60 individuals between the ages of 15 and 70 years. Note that the minimum number of individuals in this adult age group was 74, established from the total bone inventory.

Although the sex of individuals less than 15 years of age could not be established, the minimum number of individuals in this age group was determined through comparative bone inventory procedures to be 38 (Table 7.6). Of these, 37 individuals were represented by right femora, and an additional individual in the 10-to-14-year category was suggested by cranial and pelvic data. Assessment of long bone lengths and the cranial/pelvic information suggested the following age distribution: newborn to six months: 16; .5 to 1.5 years: four; 1.5 to 2.5 years: eight; 2.5 to 3.5 years: three; 3.5 to 4.5 years: one; 5.5 to 6.5 years: two; 6.5 to 7.5 years: one; 8.5 to 9.5 years: one; and 10 to 14 years: two.

Table 7.5: Minimum Number of Individuals Represented by Adult Bone Types.

Bone	Left		Right
Long bones			
Humerus	58		53
Radius	54		50
Ulna	59		60
Femur	59		65
Tibia	59		63
Fibula	54		48
Irregular bones			
Clavicle	53		47
Scapula	52		55
Temporal	66		74
Maxilla	45		46
Mandible	51		47
Gladiolus		26	
Manubrium		30	
Innominate	43		41
Patella	43		45
Rib	32 (434) [1]		32(434)
Vertebrae			
Cervical 1		53	
Cervical 2		45	
Cervical 3–7		37 (189)	
Thoracic 1–9		53 (473)	
Thoracic 10		19	
Thoracic 11		55	
Thoracic 12		16	
Thoracic 10–11		0	
Thoracic 10–12		10	
Lumbar		61 (301)	
Sacrum		38	
Hand bones			
Carpals			
Navicular	25		32
Lunate	25		24
Triquetral	17		19
Pisiform		25 (49)	
Hand bones			
Carpals			
Greater multangular	25		24
Lesser multangular	19		24
Capitate	29		30
Hamate	25		27
Metacarpal 1	38		41
Metacarpal 2	33		40
Metacarpal 3	33		34
Metacarpal 4	19		37
Metacarpal 5	26		30
Phalanges			
Proximal 1–5		30 (294)	
Middle		24(191)	
Distal 1–5		13 (124)	
Foot bones			
Tarsals			
Calcaneus	44		46
Talus	60		54
Cuboid	41		45
Navicular	40		48
Cuneiform 1	47		43
Cuneiform 2	40		38
Cuneiform 3	40		39
Metatarsal 1	52		49
Metatarsal 2	32		31
Metatarsal 3	49		46
Metatarsal 4	40		44
Metatarsal 5	38		47
Phalanges			
Proximal 1–5		31 (303)	
Middle		9 (67)	
Distal 1–5		9 (81)	

1. Numbers in parentheses indicate actual number of bones.

Table 7.6. Minimum Number of Individuals Under 15 Years of Age.

Age in Years	MNI
.0–.5	16
.5–1.5	4
1.5–2.5	8
2.5–3.5	3
3.5–4.5	1
4.5–5.5	0
5.5–6.5	2
6.5–7.5	1
7.5–8.5	0
8.5–9.5	1
10–14	2
Total	38

Discussion

Although Ossuary III is smaller than the other two ossuaries reported from this site, its bioarchaeological details are similar, suggesting representation of all the deceased in the community for a fixed period of time. The paucity of artifacts and the egalitarian nature of the assemblage are consistent with Jirikowic's suggestion (1990) that the elite controlled wealth and the ossuaries represent the non-elite population. As with Ossuary II, Ossuary III presented a variety of cranial positions and orientations, with all ages, sexes, and skeletal parts distributed throughout the assemblage. As with both previous ossuaries, some articulated remains were noted, in addition to the secondary nature of most. Although no completely articulated individuals were found in Ossuary III, in contrast to the three such individuals in Ossuary II, 197 examples of partial articulation were noted. These represented at least six immature individuals and 13 adults. Ossuary III contained at least 112 individuals, fewer than the 131 found in Ossuary I and the 188 of Ossuary II.

Analysis of the Ossuary II sample noted that about 20 percent of the adults were represented by individuals with articulated lower leg bones and vertebrae. This figure was used to assist in the estimation of the length of time represented by the ossuary deposit (accumulation of the dead over a period of approximately three years). In Ossuary III, about 13 (18 percent) of the 74 adults maintained vertebrae articulation at the time of burial. Thus the Ossuary III data are consistent with those of Ossuary II in suggesting a time interval of about three years (actually 3.9 years for Ossuary III). In both ossuaries I and II, subadults comprised about 47 percent of the total sample. Within Ossuary III, the immature constituted only 34 percent of the sample, suggesting a slightly lower immature death rate and/or birth rate.

The Ossuary III analysis, coupled with those of the previous two ossuaries excavated from this site, reveals a consistent pattern of mortuary behavior, as well as biological profiles of the people represented. The bioarchaeological approach utilized in this analysis reflects a methodological orientation championed by Jane E. Buikstra over her long and accomplished career.

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8

Interpreting Animal Effigies from Precontact Native American Sites:

Applying an Interdisciplinary Method to Illinois Mississippian Artifacts

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In this chapter I use an interdisciplinary method that I modified from existing approaches to help archaeologists move from representations of animals on artifacts to an understanding of the socioreligious meanings of those animals in a given culture. The method has five steps—formal, functional, and material analysis; contextual analysis; natural history; ethnographic and ethnohistoric analogy; and finally interpretation—all of which will be discussed in detail below. I will also illustrate the method's use by applying it to several different kinds of animal artifacts made by Illinois Mississippian peoples. (For another such example, see Aftandilian 2007b.)

The larger study on which this chapter is based (Aftandilian 2007a) focused on interpreting Illinois Mississippian animal effigy artifacts recovered from an area bounded by Fulton and Peoria counties in central Illinois to the north, Madison and St. Clair counties in the American Bottom to the southwest, and Massac and Pope counties in southern Illinois. Key sites in each of these regions are noted in Figure 8.1.

Although comparatively few Mississippian artifacts have been recovered from the lower Illinois Valley (which includes the lower 70 miles of the Illinois River's drainage through Calhoun, Greene, Jersey, and Pike counties), that region is rich in Woodland-period remains and has been the subject of intensive archaeological study since the 1930s. Jane E. Buikstra has made a number of key contributions to the archaeology of this region since she began excavating there in 1966.

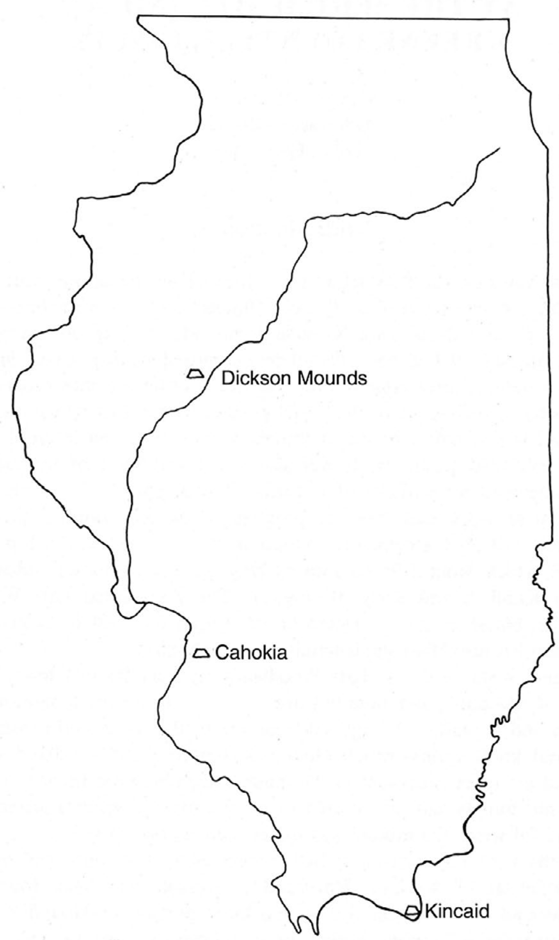

Figure 8.1. Illinois study area and key sites (after Perino 1971:Figure 1).

She has authored or coauthored dozens of articles on the archaeology of the lower Illinois Valley. Through these publications, Buikstra has established the key importance of bioarchaeology, viewed in both diachronic and regional perspectives, to understanding relationships among past cultural groups in this region (Buikstra 1976; Konigsberg and Buikstra 1995). For the purposes of this chapter, Buikstra's most important contributions to Illinois archaeology may have been her long-standing focus on multidisciplinary and interdisciplinary approaches to understanding the past, and on contextual analysis of human skeletal remains and the artifacts associated with them. As we will see, both of these contributions helped inspire my work on the research method presented here.

This approach is based on the research of a number of other scholars, each of whom has employed one or more of the steps discussed below to interpret archaeological materials. For example, it has become fairly common in recent decades for archaeologists to use ethnographic and ethnohistoric analogy. Part of the credit for the rising interest in such approaches is due to Ian Hodder. His ethnoarchaeological work on interpreting decorations on Ilchamus calabashes in Kenya demonstrated the crucial importance of researching the social and contextual contexts within which artifacts are used if we want to understand their functions and meanings to the people who made them (Hodder 1991:107–120). Karl Taube combined artistic, linguistic, and modern ethnographic evidence to interpret the meanings of the maize tamale in Classic Maya culture (Taube 1989). Kent Flannery and Joyce Marcus have also shown how ethnohistoric information, coupled with primary data on archaeological find contexts, can help us better understand ancient Zapotec religious practices in Mesoamerica (Flannery and Marcus 1993; Marcus and Flannery 1994).

The scholars who have employed methods most similar to the one I describe in this chapter are Olga Linares, Scott O'Mack, and those involved in the Mississippian Iconography Workshop at Texas State University–San Marcos. In her interdisciplinary analysis of burial vessels from high-status graves in central Panama (1500–500 B.P.), Linares (1977) used formal analysis of the vessels, natural history of the tropical animal species represented on the vessels, the archaeological contexts within which the vessels were discovered, and ethnohistoric analogy to interpret the meaning of the vessels' iconography to their makers. In his study, O'Mack (1991) drew on natural history, ethnohistoric analogy, and formal analysis to interpret the meanings of Yacateuctli and Ehecatl-Quetzalcoatl among the Aztec of central Mexico. And the participants in the Mississippian Iconography Workshop collectively developed a four-pronged approach that involves "recognition of style regions, visual structural analysis, archaeological content, and ethnographic analogy" (Reilly and Garber 2007:6; see also Reilly et al. 2011:xiii).

In developing my approach to interpreting Mississippian animal effigies, I offer several significant modifications to existing methods. First and foremost, I conduct in-depth studies of the natural history of the animals most frequently represented on the artifacts. While others have worked with biologists to identify the species represented and their habitats (e.g., Jett and Moyle 1986), very few archaeologists have researched the appearance, behavior, and other aspects of the lives of the animals whose representations they are analyzing. This is an unfortunate oversight, because it is precisely these aspects of the lives of real animals that partly inspired the Mississippians and other archaeologically known peoples to use the animals as symbols on their artifacts. I argue, then, that if we truly want to understand what representations of animals meant to the peoples who made them, we must devote serious attention to the natural history of these animals. Second, I apply each of the five steps in this method in a consistent manner to each kind of effigy artifact I analyze. And third, I develop my final interpretations in a manner consistent with Native American ways of knowing. (I will expand on this last point in the conclusion.)

Formal, Functional, and Material Analysis

The first step in this study involves formal, functional, and material analysis of animal images and the objects that bear them. I start the analysis here because these representations are our primary data; they constitute a visible record of symbolism that was important enough to the Illinois Mississippians that they chose to represent it in durable media such as ceramic pots and stone pipes.

The first question I ask of any animal image is: What kind of animal does it represent? And ideally: What species does it represent? Sometimes identifying the kind of animal is not difficult, but sometimes it can be quite challenging. In such cases, when I was unable to identify the animal myself, I sought the assistance of experts in the biological study of that kind of animal in Illinois.

Next I conduct a formal, art historical analysis of the animal image. What aspects of the representation are most detailed and specific? Picking out what seem to be the key features of a representation may help us identify particular aspects of an animal's appearance or behavior that intrigued the object's makers. Especially when the same traits are accentuated in nearly every image of a particular animal, we can assume that there is something about that feature worth looking into further. For instance, while Illinois Mississippian beaver bowls differ in whether they represent just the beaver's forelegs or also its back legs, and whether individual toes are marked off, nearly all the images share a single common feature: a narrow, cylindrical object gripped within the mouth and held by the two forepaws (Figure 8.2). Similarly, owl bottles may or may not represent an owl's face, wings, or feet, but they always include two prominent ear tufts that look like horns atop the head (Figure 8.3).

It is also important to consider the materials of which an object was made and to think about how the object might have been used by its makers. Certain materials have religious significance as materials, regardless of what they are made into. Copper is one example; shell another; and the same was likely true for various types of stone (Boivin and Owoc 2004; Claassen 1998:203–211; Trevelyan 1987:289–358). Several aspects of materials such as these, including their colors, shapes, or sources, might incite special interest.

Finally, we need to think about how a given object might have been used. For example, pipes were obviously smoked, and given their relative rarity in Illinois Mississippian archaeological collections, they were probably smoked only on important ceremonial occasions.[1]

Figure 8.2. Beaver bowl from St. Clair County/Madison County, Illinois (ISM 803/163; courtesy Illinois State Museum).

Figure 8.3. Owl bottle from Jackson County, Illinois (ISM 1997/178.0001; courtesy Illinois State Museum).

Contextual Analysis

The next step involves familiarization with the archaeological contexts within which an animal artifact is found. Most generally: Where in Illinois was the object found? Where have objects with similar animal images been found?

Next, was the object found in a house, a grave, or somewhere else? Were any other items found in association with the animal artifact? If the object was found in a burial, was it found with a man or a woman? An adult or a child? Knowing the answers to these contextual questions can help tremendously in interpreting the meanings of animal artifacts. For instance, nearly all the owl effigy bottles in Illinois come from graves, and in the cases where we know the age of the person they were buried with, they were nearly always buried with children or infants.[2] As we will see, that contextual information provides crucial clues for interpreting the meaning of horned owl bottles to the Illinois Mississippians.

Finally, whenever possible, I draw on the insights of zooarchaeologists to determine whether the type of animal depicted on an artifact lived in Illinois at the time the artifact was made, how common it was, and whether the animal was used for more than symbolism by the Illinois Mississippians. Was it hunted for food or other purposes? Were its bones used for tools or ritual objects? Were the animal's bones spread widely at major sites, or were they found only in certain restricted areas?

NATURAL HISTORY

Once I have found as much contextual information for a given animal artifact as possible, the next step is to delve into the natural history of the animal represented on the artifact. What sort of habitat is the animal usually found in? What does it look like? Also, how does the animal behave? What does it eat? How does it move? And finally, what about the animal's appearance and/or behavior might have made it particularly interesting to the Illinois Mississippians? What about its natural history might have especially fired their cultural imagination?

For instance, one unique aspect of beaver behavior is that they build things, such as lodges, dams, and canals. Animals that display humanlike behaviors often incite human interest, and this has certainly been true for beavers and their building habits, for both Native Americans and Europeans (Aftandilian 2007a:222–226). More importantly, in terms of interpreting the meaning of the imagery on the beaver bowls, beavers in the wild often grasp sticks between their forepaws and gnaw off the bark with their teeth—just as they are depicted doing on Illinois Mississippian bowls (compare Figures 8.2 and 8.4).

Animals that move between different realms—anomalous or liminal animals—also seemed to be of special interest to the Illinois Mississippians, and indeed to many other Native American groups (Hudson 1976:139–148). Here again, the beaver is an excellent example, since it goes on land to cut trees but spends much of its time in the water. Owls are another example, gliding on soundless wings across the crepuscular boundaries between day and night.

ETHNOGRAPHIC AND ETHNOHISTORIC ANALOGY

The next step in this method involves ethnographic and ethnohistoric analogy.[3] The idea here is to use ethnographic information recorded from living or historically known peoples to try to understand what animals might have meant to the Illinois Mississippians. It is important to include collections of stories and folklore in this stage of the analysis, as well as standard ethnographies. Not only do animals often appear more frequently in such stories than in ethnographies, but sacred stories often provide crucial interpretive clues for understanding the history, function, and use of particular ceremonies involving animals (Bowers 2004:340–341). In employing ethnographic analogy to interpret Mississippian symbolism, I follow in the distinguished footsteps of many other scholars (see, e.g., Emerson 1989, 1997, 2003; Hall 1977a, 1977b, 1989, 1997; Howard 1968; Lankford et al. 2011; Reilly and Garber 2007; Townsend 2004; Waring 1968; Willoughby 2000).

Figure 8.4. Beaver carrying stick in mouth (photo by and courtesy of Fran West).

Obviously, the use of ethnographic analogy is somewhat problematic and must be approached carefully.[4] We need to keep in mind such issues as the biases of the observer who recorded the ethnographic data we are using, the specific circumstances under which the data were recorded, and whether the data refer to a mythic or historical past (Barber and Berdan 1998:148–177; Galloway 1986). On the other hand, we have little choice but to use ethnographic analogy if we hope to understand what Illinois Mississippian animal representations may have meant to their makers. As Gordon Willey has pointed out, "there are some aspects of past life, principally those in the ideological realm, that can be satisfactorily explained only with the aid of specific historical analogy" (that is, ethnographic analogy; Willey 1990:303).

For my survey of ethnographic and ethnohistoric data, I focused especially on customs recorded from Dhegiha and Chiwere Siouan–speaking tribes of the central Plains who are likely most closely related to the Illinois Mississippians, including the Osage, Omaha, Kansa, and Ho-chunk, or Winnebago (Dorsey 1885; Howard 1956, 1995:4–5; La Flesche 1995:28–29, 284; Unrau 1971:15–16). Also included were data from tribes likely descended from other (non-Illinois) Mississippian groups, such as the Pawnee, Arikara, and Caddo in the southern Plains and the Creeks, Choctaw, and Seminole in the Southeast. In addition, ethnographies of some central Algonkian peoples whose ancestors might have lived near the Illinois Mississippians, such as the Ojibwe and Fox, were consulted. Finally, animal beliefs of the Pueblo peoples of the Southwest were reviewed, since maize probably came to the Illinois Mississippians from the Southwest (Fritz 1992:28; Smith 1995:184) and related ritual beliefs were likely passed along as well (Taube 2000:297; Young 1994:108). In my research on these tribes, I explored the roles played by each animal portrayed on Illinois Mississippian artifacts, including utilitarian uses, such as for food; social uses, such as for clan names; ritual uses; and uses in sacred stories, folklore, and cosmology.

For example, it is difficult to think of a creature that has had more contradictory beliefs associated with it among Native Americans than the owl (Aftandilian 2007a:433–482). On the one hand, owls are seen as bringers of healing medicine and spiritual power for warriors, hunters, and medicine people. On the other hand, certain kinds of owls, especially great horned and screech owls, are believed to be the familiars of witches and the bringers of ill omens of death or serious illness, or they are thought to be ghosts, transformed human spirits doomed to wander the earth. Perhaps the most consistent spiritual association of owls in many tribes, though, is with death and the afterlife.

Interpretation

The final step in the method involves synthesizing all the data gathered from the other steps into the most likely interpretations of what each animal meant to the Illinois Mississippians. Let's finish our owl example to see how this works. Formal analysis of the owl images revealed that the one feature common to all owl bottles were the "horns" or ear tufts atop the head. From natural history, we learn that three owls found in Illinois have prominent ear tufts—great horned, screech, and long-eared owls (Aftandilian 2007a:418). Since long-eared owls are found in Illinois only in winter, I believe it more likely that great horned or screech owls, or both, were being represented on the owl bottles. From ethnographic analogy, we learn that these two tufted owls, great horned and screech, are also considered the most spiritually charged owls among many Native American tribes. As mentioned earlier, there is a wide range of owl beliefs in any one tribe, let alone among all Native American tribes. We can help narrow down which ethnographic information might be most relevant by relying on the archaeological contexts within which Mississippian owl bottles have been found in Illinois. First, all the owl bottles for which contextual information is available were found in graves. Secondly, most of the owl bottles were found buried with infants or children (see note 2 for details).

So what do we make of all this? We have already said that the most consistent spiritual association of owls in most Native American tribes is with death and the afterlife. Since all the owl bottles were found in mortuary contexts, this suggests that Illinois Mississippians associated owls with death. To be more specific than that, we need to rely on ethnographic analogy. It turns out that an owl or owl-human supernatural being serves as gatekeeper along the path to the afterlife among the Oglala Lakota, the Cheyenne, and the Ojibwe (Baraga 1966:198; Barnouw 1977:18–19; Cowdrey 2003; Jones 1919:311–313, 531–545; Lame Deer and Erdoes 1972:101, 198; Powers 1982:53, 1986:101; Sparks and Soper 1989:199). This owl being stands at the fork in the Milky Way, the road in the sky that leads to the land of the dead, letting some souls pass but condemning others to roam the earth as ghosts forever. I suggest that the owl or owl-human creature depicted on the Illinois Mississippian bottles represents such a being.

One aspect of the archaeological context just mentioned remains unexplained: Why were these owl bottles found most often with children and infants? Two potential interpretations can be offered. First, among the Lakota, a tattoo on the wrist signified to the Owl Woman, who guarded the path to the afterlife, that the spirit who wore it was a Lakota and thus should be allowed to pass to the land of the dead (Lame Deer and Erdoes 1972:101, 198). In many Native American tribes, tattoos are not given to children but only to adults who have earned the honor of a tattoo. Children and infants, then, would probably not have been tattooed yet. Therefore, when they died, the Owl Woman would not have recognized their spirits as qualified to proceed on to the afterlife. Perhaps in this case, the owl bottles were buried with the children to serve as passports to allow the children access to the land of the dead, just as tattoos did for adults.

A second possibility rests on the analogy of these owl bottles with gourd vessels, which were also frequently used to hold liquids among Native American tribes and likely were used for this purpose among the Illinois Mississippians.[5] The shapes of certain varieties of bottle gourds and of Illinois Mississippian owl bottles are nearly identical. This is likely not a coincidence. Among the Yuchi of Georgia and South Carolina, for instance, the shapes of gourds and squashes served as models for the shapes of pottery vessels (Speck 2004:27–28). Among the Yuchi, Pawnee, and Iroquois, gourd bottles and rattles sometimes also functioned in rituals and sacred stories as containers for souls (Hewitt 1895:109, Speck 2004:144–146, Weltfish 1977:466). Perhaps, then, the owl bottles buried with Illinois Mississippian children served as containers for their souls as well. How this might have worked in detail is difficult to know, but we might speculate that since owls are often considered the spirits of humans by Native American groups, perhaps the children's spirits became owls once they flowed into the owl bottles and the children then made their way to the land of the dead in the form of owls.[6]

Conclusion

This study has demonstrated how to apply an interdisciplinary analytical method to move from archaeologically recovered animal artifacts to the religious and social meanings the animals represented on them might have had for the peoples who made them. While this is not the first study to apply any of the various steps described above to interpreting Mississippian artifacts, truly *interdisciplinary* studies of Mississippian worldviews and symbolism, conducted in a systematic fashion, remain rather rare.[7] Guy Prentice's analysis of the Birger Figurine (Prentice 1986) is one key exception, as is the work conducted by participants in the Mississippian Iconography Workshop (Lankford et al. 2011; Reilly and Garber 2007). Natural histories of animals, in particular, have seldom been investigated by archaeologists and others attempting to interpret precontact representations of animals (but see Morphy 1989 for several welcome exceptions). This relative lack of interdisciplinary approaches to understanding precontact animal symbolism is unfortunate, since it is only through such approaches that we can hope to make the conceptual leap from representations of animals on Mississippian artifacts to the meanings of those animals in Mississippian worldviews.

Furthermore, if we truly want to understand Mississippian views of animals, we would do well to employ Native American ways of knowing in our analyses of animal effigies, since the Mississippians were precontact Native American peoples. In other words, I suggest that archaeologists use Native Americans not just as objects of study but as sources of the theoretical and analytical approaches we use in our studies of artifacts made by Native Americans and other peoples, as Christopher Ronwanien:te Jocks has argued scholars of Native American religions should do (Jocks 2000:72). One key aspect of Native American ways of knowing includes a holistic approach to understanding (Cajete 2000; Kawagley 2006); this is another reason to emphasize the importance of a wide-ranging interdisciplinary approach to interpreting precontact Native American artifacts.

There is another important lesson to be learned from Native American ways of knowing in relation to archaeological interpretations of animal effigy artifacts: a given symbol does not mean just one thing to members of a specific tribe, but rather it evokes a range of meanings depending on the circumstances within which the symbol is encountered, as well as one's clan, age, gender, individual experience, level of spiritual knowledge, and so on (in other words, symbols are polysemous and multivocal for Native Americans; cf. Turner 1967:50). Therefore, in our interpretations of animal effigies, we would do well to seek not just one reductive meaning of each effigy as a symbol but rather a range of potential meanings. By doing so, we are more likely to capture some of the wide array of meanings each animal effigy artifact would have had among the people who made and used it. This is why several possible interpretations have been offered for the Mississippian owl effigy bottles discussed earlier.

Finally, although the examples presented in this study were all drawn from Illinois Mississippian artifacts and peoples, such a method could certainly be applied to work on other peoples from other times and places. Depending on the specific context and the data available, various steps in the method would need to be reworked or perhaps dropped entirely, but the overall process would remain the same. Key to this process is the interdisciplinary approach, involving in this case art history, archaeology, natural history, folklore studies, history of religion, and ethnohistory. I hope that my demonstration of how such a method can be used will encourage other archaeologists to apply this or similar interdisciplinary approaches more frequently in the future.

Notes

1. However, pipes were not necessarily *just* smoked. A number of Illinois Mississippian sandstone frog pipes, for instance, have deep grooves on various surfaces (not just in their bowls or stem holes), which suggests that they were used as both pipes and abraders (Aftandilian 2007a:187).

2. Of the 14 Illinois Mississippian owl bottles I studied from definite mortuary contexts, 6 were found associated with infants or children, 1 was associated with a young adult female, and 1 was found near the surface in a possible crematory or charnel structure. The age and sex of the burials is unknown for the other 6 bottles. See Aftandilian 2007a:369–377 for detailed provenience information and references.

3. Technically speaking, ethnographic analogy refers to analogies with living peoples, while ethnohistoric analogy refers to analogies with historically known peoples who may or may not still be living today. In the New World, these techniques are also often referred to as the direct historical approach (see, e.g., Galloway 1986; Marcus and Flannery 1994; Wonderley 2005).

4. This difficulty probably explains at least some of the extreme discomfort with ethnographic analogy expressed by Lewis Binford and other processualist New Archaeologists during the 1960s and 1970s (Binford 1967:1, 1968:13; Willey and Sabloff 1980:205–207; Wylie 1985:84–87).

5. Linguistic data from Central Algonkian peoples suggest that the original Native American use of both gourds and squashes was as containers rather than as foods. The same word stem used for *squash* and *gourd* was also used for *container* and *receptacle* (as well as *spoon* and *dipper;* Munson 1973:119–120).

6. This would help explain a puzzling element in the stories that many tribes tell about the trip to the land of the dead—that the road is fraught with dangers and that the very old and the very young often cannot make it to the journey's end. Perhaps, by transforming into owls, the spirits of Illinois Mississippian children could make that journey unscathed.

7. In addition to the studies that have employed the ethnographic analogy cited above, a number of others in recent years have focused on formal analysis of effigy artifacts or have constructed folk taxonomies based on faunal remains and artistic representations. For selected references, see Aftandilian 2007a:60–61.

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9

Ethnogenetic Theory and New Directions in Biodistance Research

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Biodistance analysis uses phenotypic skeletal and dental variation to infer evolutionary processes in past populations. The approach is heavily quantitative and based on the assumption that cranial, dental, and (less commonly) postcranial size, shape, and morphology are genetically conditioned (not narrow-sense heritable per se; Vitzthum 2003). Thus the mathematical properties of these traits measured in populations (central tendency and variance) are subject to rules of intergenerational inheritance, quantitative genetics, and evolutionary mechanisms such as gene flow, genetic drift, and natural selection. Focus on "distance" a priori emphasizes relationships between objects (either individuals or samples) in multidimensional space. However, increasingly biodistance analysis refers to any investigation of evolutionary process or genetic inheritance in past populations, whether based on means and frequencies, explicit inferences of patterns of variances, or more cladistic approaches utilizing rare anomalies and their expression within biological lineages.

Defined so broadly, biodistance research can be dated to the nineteenth century, when issues of racial taxonomy considered populational differences in cranial and brain size volume (see a readable account in Marks 1995). *Distance* referred to "degeneration" along the *scala naturae*. During the early half of the twentieth century, taxonomic issues continued to dominate biodistance research through the work of noted scholars such as W. W. Howells (Howells 1989). Global surveys of craniometric variation sought answers to questions of modern human emergence and subsequent expansion throughout the Old and New Worlds. Turner's groundbreaking work with dental morphology was within the same vein, with an emphasis on Pacific Rim and New World population relationships and migrations (see Scott and Turner 1997). As recent scholarship indicates (e.g., Manica et al. 2007), interest in broad-scale questions of migration and virgin soil population expansion has not abated, a fact that has not escaped some critics (Armelagos and Van Gerven 2003).

Nonetheless, the advent of the New Archaeology, with its emphasis on social systems, initiated a regional focus in biodistance research, as exemplified by the work of Buikstra and colleagues on Middle and Late Woodland–period populations from the Illinois Valley (Buikstra 1976, 1977, 1980). Buikstra's research established the groundwork for most subsequent regional biodistance analyses, mine included (Stojanowski 2005a). Contributions were both methodological and theoretical. For example, Buikstra (1976, 1980) presented an explicit analytical framework for minimizing the effects of nongenetic variation on resulting biodistances, now familiarly known as preanalysis data treatments (sex effects, age-related variation, intertrait correlation). These tests are performed *de rigueur*. In addition, Buikstra highlighted the importance of a sample's archaeological context for interpreting the meaning of the resulting microevolutionary analyses. This approach was in direct contrast to global biodistance surveys, which often reified archaeological assemblages as biological populations and used phenetic similarity as the global proxy of population relatedness. Thus a more nuanced view of prehistory emerged from Buikstra's research program through goals that were multiscalar and multifaceted—understanding site formation processes, reconstructing community interrelationships, documenting interaction scales and boundaries, and providing a demographic basis for health inferences. However, the most important development to come out of the Illinois Valley research program was the emphasis on contextualization. Microevolutionary research was linked to the social world of past peoples, allowing modern scholars to "'flesh out' the image of our mound builder as a cultural individual: one who inherited or acquired wealth and status, who perhaps buried his dead with his mother's people, and whose family may have lived in one village long enough to become biologically distinct from contemporaries up the river" (Buikstra 1980:271). The present chapter proceeds within this realm of scholarship.

The multiscalar, context-specific inferences of biodistance analysis (Stojanowski and Schillaci 2006), combined with increasing concern for population genetic variance parameters (Relethford 2003; Relethford and Lees 1982), counter claims that biodistance analysis remains grounded in nineteenth-century typology as proposed by Armelagos and Van Gerven (2003). However, biodistance analysis does generally focus on inferring basic historical facts: Who is related to whom? From where did population x migrate? While I disagree with various elements of their critique (Stojanowski and Buikstra 2005), on this point Armelagos and Van Gerven are largely correct. The point is *not* that historical issues are unimportant but rather that other things can be learned through anthropological analysis of past populations—insights that inform the modern world directly and have contemporary relevance within a broader social sciences perspective. Nonetheless, criticism (pedantic or iconoclastic) for its own sake is not productive unless alternatives are presented. Therefore, rather than further critique historical evolutionary anthropology, in this chapter I discuss a new research domain in biodistance analysis—the study of ethnogenesis.

An Ethnogenetic Research Focus in Anthropology

In the simplest terms, ethnogenesis is "the establishment of group distinctiveness" (Sturtevant 1971:92). The group, in this case, is a social collective, one of many forms of human identities based on class, ethnicity, tribe, community, nationality, religion, or political affiliation. In this sense, ethnogenesis is clearly a diachronic (not evolutionary) process of group emergence or redefinition. However, ethnogenetic processes also manifest synchronically through intertribal or intercommunity exchange, whether that entails the movement of people, genes, or material objects. Ethnogenesis, then, is a diffusionary process in which people violate "normative rules" of an endogamous tribal society. People can and often do move across social boundaries, assume new social identities, or manipulate social interactions with a multitude of identities that are called upon situationally (Moore 1994a, 1994b, 2001; Quinn 1993; Terrell 2001a, b, c). These processes effectively decouple biological and social identities in evolutionary time and defy simple reification. Such a dynamic view of human societies has long-term and broad implications for how we view human prehistory. Proponents of an ethnogenetic view posit periodic, wholesale reshuffling of peoples among social units such that "resulting new social formations are likely to have their 'roots' or 'origins' in several antecedent societies" (Terrell 2001a:31). Thus the cladistic, branching models implicit within taxonomic anthropological genetics are inappropriate given the recurrent (repeatedly separating and recombining) realities of how human societies *actually* work and persist in a historical sense. It is important to note that synchronic (diffusionary) and diachronic (formative) aspects of ethnogenetic theory are part of the same sociohistorical process. Bioarchaeology, by virtue of the temporal resolution afforded archaeological assemblages, has the ability to elucidate both synchronic and diachronic elements of ethnogenesis in past populations—the former by considering patterns of genetic/phenetic variation against socially defined criteria of group distinction; the latter by reconstructing patterns of gene flow among populations through time.

The appeal of ethnogenetic theory as an emerging research focus is its ubiquity and simultaneous invisibility. We apply it all the time in bioarchaeology and bioanthropology but without specific recognition of this fact. Its significance is far-reaching; thus it unifies anthropology and the broader social sciences within a common research theme. Transnational migrations of ethnic minorities, ethnic-based modern political movements, ethnic cleansing and genocide, genetic models of population history, race, ethnic nepotism, and the multiregional model of modern human emergence all share in this ethnogenetic focus. This emphasis encapsulates complexities of "self" (emic) and "other" (etic) group identification, historical development of social forms, transcendence of identity categories (the "idea" of what a people are), and processes of biological and social integration across population boundaries. To study ethnogenetic theory in its many faces is to study the basis of the human condition, from Pleistocene hunter-gatherer band exogamy to ethnic enclaves and disenfranchisement within multiethnic modern cities.

In this chapter, I outline the range of research orientations that fall within the purview of ethnogenetic theory and propose a specific bridging model linking human biology to the social and cultural realms of the human experience. I specifically propose that biodistance analysis can contribute to our understanding of the development of social identities (ethnogenesis in a temporal sense) and can do so in a unique manner because of our ability to study temporal changes related to transformations in social identities through time. I make no appeal to sociobiological motives, isometry between ethnic groups and breeding populations, or the conflation of race and ethnic identity. Rather I propose that human biological variation can be interpreted within the social context that created the pattern of variation as signaling changes in social group composition. Phenotypic variation, as affected by patterns of mate exchange, is an indelible imprint of human agency and action; it is an invisible (and hence passive) marker of identity symbolism manifest within the sphere of reproduction. However, I eschew Darwinian sociobiological linkages between biology and culture, which are deterministic and based on notions of fitness driving biocultural evolution in a hyper-rational model of human behavior. Instead, I draw an operational analogy from practice scholarship (Bentley 1987; Lightfoot et al. 1998; Orser 2004; Pauketat 2001) and the archaeological analysis of material culture variation and "style" (particularly Bell 2005; Voss 2005; see also Sofaer 2006).

Ethnogenesis: Forms and Definitions

Ethnogenesis is ultimately about social group composition and construction—that is, how human social groups are organized, interrelated, and defined in a historical sense (Arutiunov 1994). Ethnogenesis can result from differentiation or fissioning of social groups, thus increasing diversity (Horowitz 1975). This cladistic, bifurcating process anchors nearly all anthropological genetic research that is historical in focus (Moore 2001), as well as evolutionary archaeological approaches that consider the relative importance of phylogenetic (differentiating through descent) versus ethnogenetic (diffusion, sharing, borrowing) mechanisms of cultural evolution (Collard et al. 2006; Guglielmino et al. 1995; Mace and Holden 2006; Mace et al. 2005; O'Brien et al. 2001). Ethnogenesis can also result from assimilation or fusion (Horowitz 1975), a process that describes many examples from indigenous colonial North and South American contexts (e.g., Albers and James 1986; Davis 2001; Galloway 1995; Haley and Wilcoxon 2005; Hickerson 1996; Hill 1996; Moore 1994a, 1994b, 2001; Sharrock 1974) and perhaps most dramatically from African maroon settlements in the New World (Bilby 1996; Kopytoff 1976). In these cases, multiple historically distinct groups coalesce to form a new social identity, often within a drastically different political-economic context.

Researchers of ethnogenesis differ significantly in their orientation, depending in large part on their commitment to the uniformitarian application of ethnogenetic theory into the remote (prehistoric, not colonial) past. The differences between perspectives result in a degree of inferential coarseness, reflective of a micro versus macro scale of analysis. Historical ethnographic approaches provide a social platform for expressing the rights of modern indigenous populations and decrying the repercussions of colonialist hegemonic policies (Ferguson and Whitehead 1992; Hill 1996). For these scholars, ethnogenesis as a process of ethnic emergence and reformulation is a *known* fact. It is not to be debated. The primary focus is describing historical cases and promoting the social significance of the inferences. As a matter of law, understanding the complexities of recent colonial histories of indigenous peoples is extremely important (Davis 2001; Haley and Wilcoxon 2005; Quinn 1993; Sider 1976) because economic and political positioning is intimately linked with membership in emerging pan-ethnic identities (Espiritu 1992; Tefft 1999). In a similar vein, ethnogenesis figures prominently in the modern migration and transnationalism literatures (e.g., Roosens 1989) and also explains many of the woes in developing nations with recent colonial histories (e.g., MacEachern 2000; Vail 1989). This is an entirely postcolonial discussion engaged in by ethnographers, historians, and the traditional peoples subjected to colonial rule.

The extrapolation of ethnogenetic processes to prehistoric contexts changes the research orientations significantly. Case studies cannot be developed within a specific historical context and therefore lack some of the nuanced details of historic research. Furthermore, there is no element of activism in prehistoric ethnogenetic research. Rather, researchers are more interested in theoretical issues about the nature of human societies in the past and present: Are human societies monolithic well-bounded aggregates (Wolf's billiard ball model), or are social boundaries ephemeral and permeable (Moore's braided stream model)? How well do patterns of social interaction coincide with biological integration? How do biosocial coevolutionary processes contribute to understanding patterns of biological and social diversity in the past? These perspectives are represented by the research of John Moore, an ethnographer (1994a, 1994b, 2001); a well-defined group of archaeologists, most visibly John Terrell (e.g., Bellwood 1996; MacEachern 2000; Terrell 1988, 2001a, 2001b, 2001c; Terrell and Stewart 1996; Terrell et al. 1997); and an inordinate number of anthropological geneticists. These scholars are more concerned with the long-term importance of ethnogenetic processes in human history—how often massive reorganizations occurred in the past, the rate at which reorganizations occur, and in what contexts they occur. A key component of these discussions is the relative importance of societal collapses and reconstructions compared to group fissioning events based on demographic expansion. That ethnogenesis occurred in contexts of colonial rule is uncontested. The causes were increased mortality (often epidemic), capitalist economic systems, and the power imbalances that resulted. Uniformitarian extrapolation of these same processes into the deep past is contentious but of ultimate importance for understanding the history of the human species. Did early modern humans devolve into primitive, isolated, and xenophobic "human hordes" (Terrell 2001a:21), or were we always "involved with other aggregates . . . in weblike, netlike connections" (Lesser 1961:42) such that "[h]ardly any people can speak of their 'purity of blood' and of their 'immemorial occupation' of their present territory" (Arutiunov 1994:91). This is the essence of the race debate in anthropology; it is inherently an ethnogenetic one.

Ethnogenesis and Human Biology

One primary difference between human and nonhuman evolutionary biology is the impact of cultural factors on patterns of human mate exchange (gene flow) and population size limits (genetic drift). Human biologists have long recognized this and adopted research strategies accordingly. Biological anthropologists consider ethnogenetic theory (rarely in name) by addressing the age-old question about the relationship between biological, cultural, and linguistic patterns of variation. Strategies include examining within- versus between-cultural-group genetic variability, comparing the distributional limits of cultural and biological populations across space, or comparing the degree of cultural and genetic diversity within a region under the assumption that signals should be proportional. The use of the general term *cultural* reflects the myriad ways in which social data are constructed in comparative analyses: intertribal, interethnic, interpolity (nation, state, province), and interreligion. However, by far, genetic comparison with language group affiliation has been most common, beginning with classic studies in the 1970s and 1980s (Rhoads and Friedlaender 1975; Serjeantson et al. 1983) and continuing today on issues such as agricultural demic diffusion in Europe (Barbujani and Sokal 1990; Sokal 1988) and global patterns of genetic-linguistic covariation (Cavalli-Sforza 1997; Cavalli-Sforza et al. 1988, 1992; McMahon 2004).

These synchronic approaches study the effects of ethnogenesis (its residual) but not the process itself, which requires temporal resolution. However, here again bioathropologists have considered ethnogenesis directly for some time now. I would argue, however, that this literature is ethnogenetic in name only. It rarely considers or incorporates social processes into the inferential framework. This work, produced both by bioarchaeologists and molecular anthropologists, often reconstructs population origins not in geographical terms (such as peopling of the New World or peopling of Polynesia) but in terms of the biological composition of an often nebulously defined contemporary social aggregate. In fact, the prose indicates that *ethnogenesis* is used as a synonym for *formation*, and any study that considers the origin of a modern population in terms of the mixing of distinct antecedent populations fits this definition of ethnogenesis equally as well (e.g., Ioviţă and Schurr 2004; North et al. 2000). Similar anthropological investigations focusing on population composition and the contributing origin of represented types have been published for decades. Most visible within the bioarchaeological literature is the dual-origin hypothesis of Japanese ethnogenesis (Hudson 1999; Ossenberg et al. 2006).

An Ethnogenetic Research Agenda in Bioarchaeology

It should be clear from the preceding discussion that bioarchaeological data sets have the potential to contribute to both synchronic and diachronic ethnogenetic perspectives. Most accessible, but I would argue the least interesting, is providing a basis for understanding the patterning of synchronic biosocial variation in the past, contexts often beyond the reach of molecular anthropologists using modern DNA. Such work compares patterns of phenotypic data against a cultural analog, such as material culture design variation, to reconstruct the extent to which they are coterminous and presumably coevolved. On the other hand, temporal approaches investigate how identities formed and changed through time and identify those conditions that affect the historical trajectory of specific social identities within specific historical and cultural contexts. This focus has a direct link to modern political discourse in which issues of identity form the core of major world conflicts, ethnocides, and genocides. It is within this realm of inquiry that socially relevant knowledge that bridges the evolutionary and social sciences can be produced.

I have previously indicated that the intersection of human biology and social identity has traditionally fallen within the purview of sociobiological explanations, an approach I reject as too deterministic. Rather, I propose that the intensity of social interaction in the past can be inferred from the degree of biological integration among communities within a regional interaction sphere. That is, migration and gene flow reflect human behavior and action at the community level and therefore serve as proxies for communal recognition of "us" and "other." In other words, gene flow, intermarriage, or mate exchange between populations reflects changing emic (subjective) definitions of the people themselves. Genetic and phenotypic signatures *passively* signal ethnic emergence. Homogenization of genetic exchange networks may reflect a broadening basis of identity that may ultimately lead to ethnogenesis in a reticulate manner. It is important to stress that this association between social and biological variation is temporal in structure and focuses on *changes* in the intensity of biological integration, not the static level of integration at any particular point in time. Interpreting patterns of phenotypic variation in a temporally static framework tells us very little about ethnogenesis or ethnic group composition. Only by seeing how *patterns* of mate exchange changed through time can we infer similar changes in community sentiments, and this is the true benefit of bioarchaeological approaches to ethnogenesis.

This theoretical model departs from past biological or evolutionary consideration of ethnic identity often based in ethnic nepotism theory and a strict interpretation of evolution as Darwinian in nature (James and Goetze 2001). Instead, this work draws its operational framework from historical archaeology and the analysis of material culture variation. The work of Barbara Voss (2005) is exemplary of this approach. Voss documented the emergence of a unified "Californio" identity at El Presidio de San Francisco through an analysis of material culture, architectural structures, and dietary practices by documenting the increasing homogenization of the material world despite the biologically diverse population in residence there. In particular, the residents made an increasingly similar material culture repertoire, including plain ware ceramics, similarity in cooking implements and techniques, and similar architectural styles, and also focused on gruels that were not ethnically coded. From this, Voss (2005) inferred that ethnogenesis was occurring as the community self-referent *gente de razón* replaced the divisive "mixed-race" structure of the *sistema de castas*. Over several decades, the residents of El Presidio de San Francisco eschewed the *castas* categorizations, which objectified them and also minimized the use of material symbols of local indigenous populations, thus signaling their burgeoning sense of community solidarity. Ethnogenesis in this case constructed a community identity not an ethnic group, tribe, or nation—distinctions of scale far beyond the scope of this chapter.

This example also highlights the importance of practice in building community identities, linking quotidian actions to macro-scale patterns and processes (see also Lightfoot et al. 1998; Pauketat 2001; Shennan 1989). In addition, it is clear that elements of the debate about the interpretation of style are being applied here (Conkey 1990; Hegmon 1992; Sackett 1982; Wiessner 1990. See also Jones 2002; Shennan 1989). The major difference is the diachronic focus adopted by Voss (2005) and others interested in ethnogenesis (e.g., Bell 2005; McGuire 1982, 1983). These studies do not equate a material culture pattern with an ethnic group; they equate homogenization of material culture and life experiences with ethnogenesis. Similarly, I am not interested in identifying ethnic groups through the application of genetic/phenotypic analyses, but in documenting the process of ethnogenesis by normalizing the study of human skeletal remains within the broader realm of research on residues of the past. However, phenotypic variation is completely passive, unlike most material symbols of group identity. It is not symbolic in any active sense of the word because individuals have no knowledge of the material expression of these underlying social actions (reflected in your cranium and dentition). Rather, phenotypic variation is merely reflective of the choices one makes within the social constraints established by your community with respect to reproduction (or marriage). And, stated this way, it is evident that phenotypic variation is the product of a specific "production sequence" as well, laden with inherent meaning to the participants in the social processes. Thus it is easy to envision the complexity of social meaning imbued within something so simple as changing patterns of genetic variation within an extended breeding network.

It is important to stress that this approach to microevolutionary data is not based on racial or typological models, patterns of genetic ancestry and descent (who is related to whom), population origins (from where did Native Americans migrate), or sociobiology and ethnic nepotism theory (interest groups arise due to shared alleles). The focus is on gene flow, not natural selection. The underlying basis of patterns of biological variation is social process not subconscious psychological motivation. However, this shift to a biosocial approach (contrasted with a sociobiological one) remains uncommitted as to the actual causal linkage between behaviors and their material manifestations. In the sociobiological model it is selection for behavioral phenotypes (nepotism, altruism) that explains patterns of behavior and the formation of distinct social groups. The biosocial approach adopted here remains nonspecific as to middle-range linkages, however. Gene flow can assume a more active form and be seen as actually contributing to the ethnogenetic process, and many colonial ethnographers would seemingly agree (Albers 1996; Hickerson 1996; Sharrock 1974). However, a completely passive bridging argument could also be used. Increasing biological integration could be reflective of changing attitudes among interacting communities but contribute no causality to the process itself. Thus gene flow is not a *cause* of ethnogenesis but a *result* of it. It is the behavior itself, the process of mate choice, that signals a change in emic sentiment that may signal a redefinition of "us" but not necessarily "them." This approach rejects any a priori notion that biological homogenization necessarily *causes* feelings of shared ethnic solidarity among previously distinct ethnic groups and also eschews notions that ethnic groups are genetically cohesive biological populations. Both could be true, but neither is requisite for ethnogenesis to occur.

Ethnogenesis in Spanish Colonial Florida

I have previously published biodistance analyses for seventeenth-century mission communities in Spanish colonial La Florida (Figure 9.1) (Stojanowski 2004, 2005a, 2005b). These peoples were subject to intense sixteenth-century Spanish and French intervention, followed by a protracted period of missionary activity during the seventeenth century, which ultimately led to ethnocide for most precontact populations. Declining health combined with an increasingly maize-centric diet exacerbated mortality rates elevated by epidemics, frontier slave raiding, and the abuses of the Spanish labor draft system (see Larsen 2001 for a bioarchaeological summary). Model-bound population genetic analyses (R-matrix analysis) were used to describe the evolutionary signatures of the missionization process in La Florida, initially focusing on the expected effects of demographic collapse and genetic drift within increasingly smaller native populations. The results, as it turned out, were not so straightforward.

Biodistance analyses of odontometric (tooth size and shape) variation indicated that the transition from the precontact (circa 1400–1550) to the immediate postcontact period (circa 1600–1650) witnessed an increase in diversity among different ethnolinguistic communities subject to Spanish missionization. (Apalachee, Guale, and numerous Timucua-speaking tribes resided in northern Florida and southern Georgia during the pre- and postcontact periods; Figure 9.1.) This result is contrary to the expectations of a strict, stochastic drift model in which genetic diversity decreases within a mating network due to allele fixation and loss. Clearly, then, the populations of Florida and Georgia were not biologically integrated during the precontact period and would become less so after missionization. The transition from the early mission period (circa 1600–1650) to the late mission period (circa 1650–1700) witnessed a

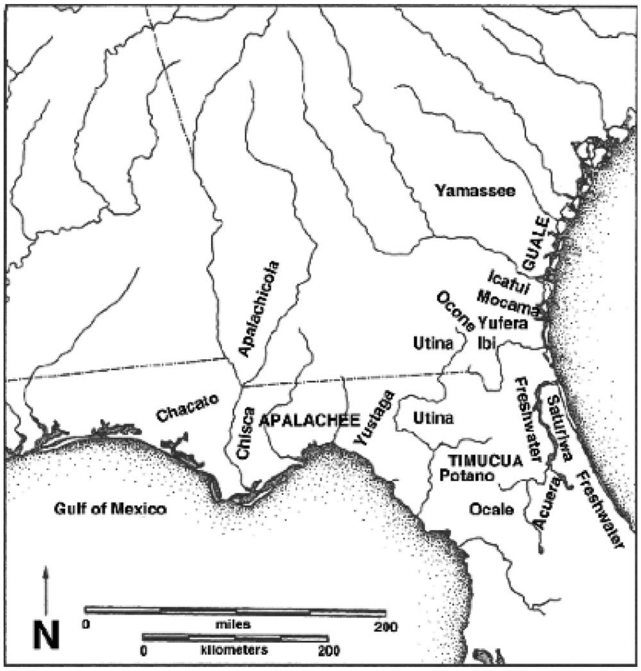

Figure 9.1. Map of Florida and Georgia showing the approximate locations of the primary ethnic groups or tribes discussed in the text. Names in bold represent the primary tribal distinctions recognized by the Spanish.

stark reversal in this pattern, however. Between-sample phenotypic variability decreased significantly, to the point that a single biological population was resident throughout the Spanish sphere of influence during the second half of the seventeenth century. From an evolutionary perspective, then, the pre- to postcontact transition is consistent with genetic drift operating within local populations not united into a single mate exchange network. In addition, migration decreased during the early mission period despite the widespread assumption of a Catholic quotidian existence. To the contrary, during the late mission period, the dramatic decline in among-population genetic variability concurrent with absolute demographic collapse can be explained only by the development of an extended pattern of migration and gene flow among formerly distinct ethnolinguistic communities.

These results are easy enough to interpret within an evolutionary theoretical context, but this is not what a "contextual analysis" of human remains prescribes (Buikstra and Beck 2006). Characterizations of migration patterns, gene flow, and genetic drift are not the most interesting things to say about these peoples, and I argue that the pattern is also reflective of changing social relations among these communities. Indeed, the diachronic changes are consistent with Nancy Hickerson's (1996) life cycle model of ethnogenetic transformation: an initial severing of ethnic ties (phase 1) precedes an exploratory phase in which new social connections are established among participating communities (phase 2), which ultimately results in nominal and mythological reification of a newly emerging social identity (phase 3).

In this chapter I proposed that ethnic realignments manifest in patterns of gene flow through either an active or passive bridging model linking the realm of social experience to the realm of human reproductive behavior (mate choice). However, to fully interpret these biodistance results, they must be further incorporated into their appropriate historical, archaeological, and social contexts. Otherwise, documentation of decreasing biological distances between distinct social communities becomes synonymous with fusion-based ethnogenesis. Such a 1:1 mapping is far too simplistic. Indeed, documenting that ethnogenesis was occurring in the past, in the absence of direct paleographic evidence for this, requires a careful reconstruction detailing historical actors, their motives, economic and political disparities, and changes in the social and material lives of the affected communities. I now turn to these data in an attempt to corroborate the biodistance analyses summarized above. I incorporate social theoretical perspectives on colonialism and ethnic identity, archaeological information on material culture patterns, and historical data on the economic and political contexts in which ethnogenetic changes are expected to occur.

Contextualization of Biodistance Analyses

I first consider why the early mission period was a time of decreasing biological integration among Christian communities. I propose that the first half of the seventeenth century was a time of "integrative devolution" as communities slowly were welcomed into the fold of the Catholic Church. Peace among soon-to-be Christian villages always immediately preceded their receiving friars (Geiger 1937:67; Oré 1936:114–117). One would think that gene flow would be more widespread once communities were united in religion and joined by amicable relationships. In fact, the opposite was documented in these analyses. This only makes sense when the role of warfare in integrating populations and chiefdoms is considered. Southeastern indigenous warfare prior to contact was characterized by two biologically meaningful practices: (1) the capture of prisoners, especially women and children, who were initially kept as slaves but eventually married into their adoptive societies; and (2) strategic alliances secured by the bestowal of brides and the establishment of fictive kinship relationships among chiefs. This was a ubiquitous feature of precontact southeastern chiefdoms. When intercommunity conflict subsided, so did the residual reshuffling of populations across social boundaries, as documented in the protohistoric chronicles (DePratter 1991. See also Bennett 1975:11, 85, 91, 105–106; Varner and Varner 1951:439, 487–489).

At the same time, Hickerson's model suggests that ethnic sentiments lose saliency during the initial phase of ethnogenesis. This second point is more theoretical and relates to the perception by scholars of ethnic units as tribes—monolithic, bounded entities. Since Barth (1969), social anthropology has embraced the notion that ethnic groups exist due to the very interactions that define their boundaries. There is no ethnicity without some degree of antagonism because "ethnic identities do not 'naturally' persist, but need to be maintained" (Banks 1996:32). Such views of ethnic groups as components within an interaction sphere rather than as isolated culture-bearing units have not permeated into biological anthropology or anthropological genetics. Nonetheless, this view does explain the decline in biological integration documented here. With the drastic changes in the sociopolitical world effected by the Spanish, old antagonisms may have lost meaning. Certainly, warfare and conflict were redefined in ways that precluded the movement of peoples among Catholic communities—indigenous-on-indigenous warfare was redefined into social revolts of the disenfranchised against the Spanish. In addition, while population sizes declined during the early seventeenth century, there is no reason to believe that widespread, massive epidemics created truly dysfunctional communities (see Kelton 2007 for one take on this). In other words, mate exchange was not expanding because the local population base, while surely smaller, was still large enough to maintain existing practices of marriage. The initial phase of ethnogenesis, therefore, was a matter of scale of political interaction. A more myopic modus may have prevailed.

The initiation of an exploratory, liminal phase of ethnogenesis is here marked by a dramatic increase in biological integration among Spanish, Christian communities. The reversal in the trajectory is stark and indicates that extensive migration and gene flow had become the norm. But what changed circa 1650 to dramatically alter the course of community engagement? The answer, as expected, is complex and multifaceted. First, the most obvious reason is that all mission communities had now experienced the effects of epidemics and were living in a postdemographic collapse environment characterized by smaller communities, rampant fugitivism, and in-migrations of peoples from the interior of Georgia and parts beyond. Small local population size generated a motive of necessity—gene flow *had* to become more expansive due to the effects of demographic collapse, regardless of any smoldering ethnic resentment among antecedent communities. However, this view by itself is mechanistic and does not afford any agency to the ethnogenetic process. That is, focus on gene flow alone assumes that people embrace a homogenous social identity because of demographic group dynamics (marriage and migration), and this is clearly an insufficient explanation. Rather, to fully appreciate the complexity of the ethnogenetic process, we must also consider the political context and scale from the top down, as well as the social environment from the bottom up. Social identities are both created by objectifying political forces and embraced and redefined by those subjected to the power imbalance.

I note first that the Spanish, through a number of actions and practices, reified a racial distinction between themselves and "Indians," who were expected to be good Christians and Catholics but always within their own *republica*—the *republica de indios* (Bushnell 2006). However, Spanish objectification of their indigenous allies does not explain the timing of the transition to the liminal phase of ethnogenesis (phase 2), in which new social connections are established among formerly distinct communities. For the Florida natives, the objectification suffered at the hands of the Spanish intensified as hemispheric geopolitics reignited the intense competition between Britain and Spain for control of eastern North America. After a protracted war with the Powhatans, the English at Jamestown began a process of frontier trade expansion. This occurred around 1644, concurrent with the initiation of the liminal phase of ethnogenesis. The Virginians were soon joined by Carolinians with mercantile roots in the Caribbean. The resources of the East Coast were aggressively pursued by the English without the encumbrances of an ecumenical charter. A plantation economy expanded, as did the trade in deerskins and Indian slaves to be sold to plantations in the Caribbean (where early-sixteenth-century epidemics had already devastated the local island populations). English traders plied their allies with superior firearms, improving hunting efficiency and establishing a debt economy that expanded in balloon-like fashion. Why is all this relevant? Because the Spanish missions were intensively targeted by slave raiders, some from as far away as the Great Lakes (the Westo; Bowne 2005). The debts encumbered in pursuit of European goods in many ways enslaved the enslavers, creating a basic distinction between well-armed, English-allied slave raiders (Westo, Yamassee, Cherokee, Creek/Uchise) and poorly armed, Spanish-allied Catholics. This dynamic situation most visibly unfolded in the expanding tribal zone of the Georgia interior and would give rise to the Creek "nation." During the latter half of the seventeenth century, indigenous populations in Florida experienced intense objectification in which ethnic nuance mattered little and a culture of fear pervaded the missions, as threats of mass suicide attest (Worth 1995:33).

At the same time, intense objectification from above does not guarantee that ethnogenesis will commence. Those participating in the dynamic must submit to the process itself and actively forge the components of a new burgeoning identity. It is here that practice scholarship is most informative (Bentley 1987). In bridging the divide between the primordial and situational theories of ethnic identity, the practice theory of ethnicity focuses on how shared experiences foster feelings of affinity among individuals. (Bentley draws from Bourdieu's *habitus* concept, a source of some criticism; Yelvington 1991.) Late-seventeenth-century Floridians had grown up within the Catholic Church, embraced a modified landscape centered around the mission *doctrina*, assumed Spanish surnames and Spanish attire, shared in the psychological devastation caused by slave raiders, and lived as fugitives or had relatives who had done so. Archaeologically, pottery types homogenized throughout the provinces, bow and arrow was traded for Spanish musket, and funerary customs coalesced toward formal Christian interments (extended, supine, with hands folded) within the *campo santo* or, more often, under the floor of the church (see Deagan and Thomas 2009; McEwan 2001). Bioarchaeologically, we see evidence for widespread morbidity, increasingly similar lifestyles and occupational practices, and a homogenization of diets (highly charged with identity symbolism)—maize-centric with the occasional input of European-imported food items (Larsen 2001). Following Bentley's approach (1987) to ethnic identity, then, Catholic Indians living during the latter half of the seventeenth century would have experienced increasingly similar lives regardless of what language they spoke or the precontact tribal identities of their ancestors. Therefore, in a context of intense objectification (by non-Catholic, English-allied tribes), we also see evidence for a region-wide shared life experience among Catholic Indian communities living in the missions, which provides indirect evidence for the emic, subjective construction of a new, broader identity through the symbolism in their material world.

I propose that these processes of ethnic amalgamation were preempted during this liminal phase, however, never to reach full reintegration in which a new ethnonym and mythological charter is adopted (Hickerson 1996). Between 1704 and 1706, a series of English and Creeks raids destroyed the mission chain stretching from Saint Augustine west into the Florida panhandle. These raids culminated a half century of escalating violence between Britain and Spain, a regional dynamic that helped define the Catholic indigenous communities in Florida. Whether a new ethnonym was ever constructed may never be known. Nothing of the sort appears in the ethnohistoric annals, and this creates the historical invisibility that leads to ethnocide and the denial of aboriginal rights for those descendant communities that had a new name chosen for them. Indeed, Patricia Wickman, a historian of the modern Seminole, noted: "The act of naming is also the act of creation and the 'right' to create is inherently the assumption of a profound power" (Wickman 1999:183). Nonetheless, I have shown here that discovery of nascent ephemeral identities is possible through the kinds of nuanced interdisciplinary analyses that bioarchaeology affords. The results presented came in a surprising form: microevolutionary analysis. However, these inferences were bolstered by appropriate contextualization within social theoretical and historical literatures. The result is a much more interesting evolutionary narrative of Florida's native peoples, one of many unheralded diasporic communities whose detachment from traditional lands perpetuates the degree of historical invisibility that anthropologists attempt to correct.

Conclusions

Ethnogenetic theory is ubiquitous in anthropology, although it is rarely identified as such. Nonetheless, a concern with variation and group definition permeates anthropological discourse, both biological and social. In this chapter I have presented a bridging model that links evolutionary mechanisms to the social world of humans by drawing an operational analogy with archaeological analyses of material culture. In the past, biodistance analyses have been used to reconstruct patterns of mate exchange within a microevolutionary framework. I would argue that such patterns provide powerful inferences about specific reproductive choices people made in the past, and these choices are heavily laden with social significance. Biodistance analysis can contribute to the literature on ethnic identity by exploring contexts in which fission or fusion forms of ethnogenesis occur. Only by understanding how social phenomena develop can we truly understand their function, and bioarchaeology—specifically biodistance analysis—can provide unique perspectives on this topic with saliency in the modern world.

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PART III

Bioarchaeological Research in Central and South America

10

What's on the Bone?

Interdisciplinary Approaches in Reconstructing the Posthumous Body Treatments of the Ancient Maya Aristocracy of Calakmul, Campeche, Mexico

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During the last few decades, the analysis of skeletal remains has become an increasingly powerful analytical tool for reconstructing the life and death of ancient Maya royals (Buikstra et al. 2004; Demarest et al. 1991; Tiesler et al. 2002, 2003, 2004; Tiesler and Cucina 2005, 2006; Wright 2005). A general awareness of the importance of osteological studies on the ancient Maya was raised in the late 1960s and the 1970s with Haviland (1967), Saul (1972), and Stewart's (1975) seminal approaches to the study of human skeletal series from the sites of Dzibilchaltún, Tikal, and Altar de Sacrificio. More recently, Maya research has received direct input from bioarchaeological agendas. This also holds true for dynastic research, which draws increasingly from sophisticated interdisciplinary tomb studies that integrate detailed taphonomic and biovital information from skeletons (Bell et al. 2004; Tiesler and Cucina 2006). The integration of skeletal research in the reconstruction of ancient rulers' lives and deaths has been actively promoted by Jane E. Buikstra. Her important work on the Classic-period aristocracy of Copan, Honduras, and Palenque, Mexico, benefits from combining detailed conventional studies and sophisticated new analytical tools (Buikstra et al. 2004; 2006; see also Price, Burton et al. 2006; Price, Tiesler et al. 2006). Examined jointly with narrated schemes of life histories that are warranted by the ancient written sources, this holistic approach allows powerfully detailed accounts of individual pre- and postmortem trajectories that facilitate cross-examination with the broader iconographic and archaeological record.

The present study, which analyzes residues adhering to two royal skeletons from Calakmul, Mexico, builds on and expands Buikstra's approach. It sets out the idea that, apart from a skeleton itself, substances adhering to skeletal surfaces also provide important sources of information that shed light on the breadth of posthumous treatments that ancient rulers received as part of public mourning and commemoration (García Moreno 2005; Hall 1989; Michelet et al. 1999; Pereira and Michelet 2004; Tiesler 2004; see also Duday 2006). Presented during the symposium "The Dead Tell Tales: Jane E. Buikstra and Narratives of the Past," this study was inspired by our quest to explore new technical applications and innovative interdisciplinary, bioarchaeological approaches, following in the footsteps of Jane E. Buikstra's pioneering integrative research on the cutting edge between archaeology and human biology. The comprehensive, collaborative approach advocated here intends to provide a fresh look at the possible roles and techniques of different posthumous body preparations of Maya royals—specifically pigmentation, wrappings, and biers—by reanalyzing tomb burials 1 and 5 from Calakmul in Campeche, Mexico, originally investigated during the 1980s by the Autonomous University of Campeche. Our results add information to earlier lab analyses on these and other elite tombs from the site conducted by Xelhuantzi-López (1985), García Vierna and Schneider Glantz (1996), García-Moreno and Granados García (1999), and García Moreno (2005), with whom we seek to develop broader insights into the postmortem timing and forms of ancient Maya elite corpse coloring and wrapping.

Two Dynastic Tombs from the Classic-Period Urban Center of Calakmul

The site of Calakmul represents a major regional and urban center in northern Petén (now in the Mexican state of Campeche). It lies along the limits of the Laberinto Bajo, which once controlled a good part of northern Petén and adjacent areas. Its 6,250 structures and more than 120 stelae were distributed over more than 30 km^2 of urban area populated by some 20,000 inhabitants (based on a 55 percent occupancy rate) during the Classic period (Figure 10.1). The site's urban space represents a concentrically organized core area divided into mosaics that extend several kilometers to the north and south and at least 10 km to the east of the central plaza (Fletcher et al. 2001; Folan et al. 1995; May Hau et al. 1990). Calakmul's aristocracy appears to have resided in the extensive acropolis areas in and around its core, where at least four royal tombs were discovered during the two closing decades of the twentieth century (Martin and Grube 2008). Two of these are the focus of this study.

Tomb 1 (Structure VII)

Previous studies indicate that the funerary chamber discovered in Structure VII dates to the Late Classic

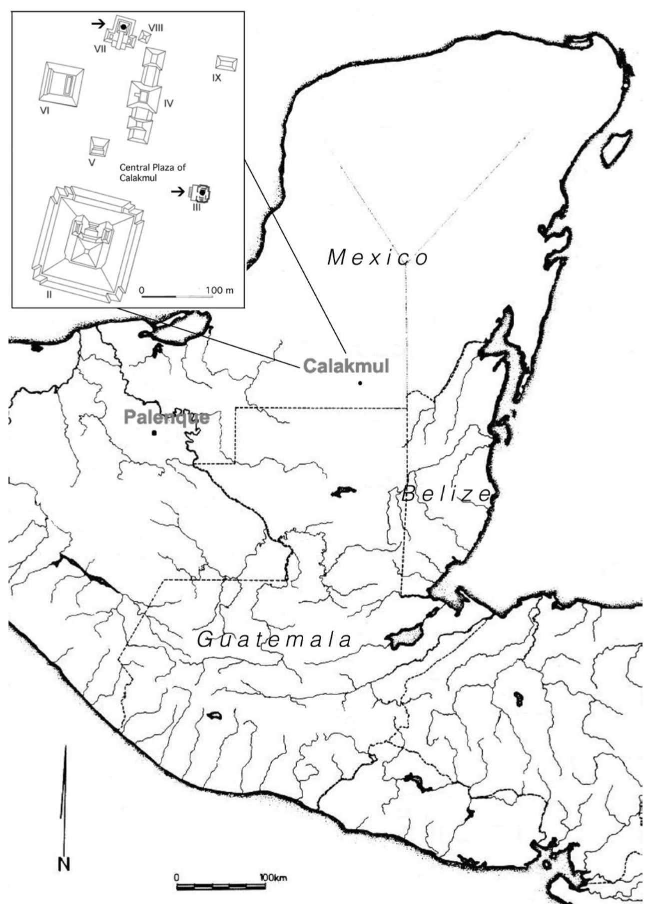

Figure 10.1. Map of the Maya area with sites described in the text, with locations of tombs from Calakmul indicated in inset (drawn by V. Tiesler).

period. Although the tomb is unidentified for lack of preserved inscriptions, its details clearly indicate the royal status of its occupant (Martin and Grube 2008:113). Structure VII is one of the massive vaulted structures that close Calakmul's central plaza to the north (Domínguez Carrasco 1994; Domínguez Carrasco and Gallegos Gómora 1989; Folan et al. 1995). The tomb contained the remains of a centrally placed body that had been laid out facing north. The corpse had been covered with red pigment and wrapped with several layers of a woven mat. Two tie holes formed of palm fibers were stabilized with *chaká* resin (*Bursera simaruba*) and with a jaguar skin with paws attached (Xelhuantzi-López 1985). Thick layers of what appeared to be a different material covered the lower extremities. The funerary bundle was laid on top of a line of four ceramic plates extended on a bed or mattress filled with *cheechém* seeds (*Metopium bownei*) (Xelhuantzi-López 1985). Before sealing, the chamber was packed with the dignitary's personal items and offerings—among them a splendidly crafted jadeite mosaic face mask; two pairs of engraved earflares; an engraved lip plug; a jadeite finger ring; and a pectoral. The body was also accompanied by several obsidian instruments, ceremonial recipients, and a small woven rectangular bundle of palm leaves placed diagonally over the thorax. Also, manta ray spines and other faunal remains were identified (Mauricio Enseñat, personal communication 1985).

The skeletal study indicates that the occupant was a male of medium height within the range of the pre-Hispanic Maya population (161 cm) who died between the age of 35 and 50. The remains, partly covered with red pigment, showed advanced deterioration due to rodent activity, which explains the disturbed arrangement of most of the skeletal segments at the time of discovery (Tiesler 1998; Tiesler et al. 2001; see also Coyoc Ramírez 1986; Lagunas Rodríguez 1985). Additional isotopic studies suggest a local origin for this ruler (Price, Tiesler et al. 2006).

Tomb 5 (Structure III)

This context consists of a large, roughly stuccoed, vaulted chamber, apparently conceived as a memorial to the royal founding ancestor of its residents during the Early Classic period (Folan 1969; Folan et al. 2001; cf. McAnany 1994). Structure III, known as the Lundell Palace after the 1931 discoverer of Calakmul (Lundell 1933), is an eight-room palatial structure, a vaulted building forming part of the Calakmul royal court (Folan et al. 2001). The tomb's occupant, who is still unidentified for lack of appropriate epigraphic texts, had probably been covered by a finely woven cloth held in place by a series of three pairs of *Spondylus sp.* shells (Figure 10.2). This cloth, similar to gauze, was registered on the front half of the cranium. The corpse had been adorned with a pectoral, ear flares, a necklace, and garments formed of thousands of small shells, including *Olivia spondae* (family Olividae) (Pincemin 1994). The body was then wrapped in a woven, mat-like material, with the right arm crossing the chest. It was laid out in a prone position on top of what might have been a wooden bier. Five ceramic plates had been placed underneath the body (Pincemin 1994; cf. Pereira and Michelet 2004). The dead ruler was also accompanied by a life-size jadeite face mask. A second, though smaller jadeite anthropomorphic mosaic mask was recorded at waist level, with a mosaic feline mask on the chest. A jadeite finger ring was also present. Other offerings consisted of a large-lidded polychrome vessel, a two-piece anthropomorphic vessel (thought to represent a portrait of the tomb occupant), and a cup in a vessel with a pouring spout. There were also the remains of polychrome stucco covering one of the supports of a tetrapod vase of perishable material, a manta ray spine, and several jadeite plaques inscribed with hieroglyphs.

The skeletal study indicates that the tomb's occupant was a robust, tall (166 cm) middle-aged male (35 to 45 years at death) who must have suffered from chronic arthritis and back ailments during the last years of his life. His cranium shows the effects of severe tabular oblique shaping, which left the forehead visibly inclined (Tiesler 1998, Tiesler et al. 2001; see also Coyoc 1992).

Sampling and Analytical Methods

Our analytical methods were designed to contribute new information on the structural properties of body pigmentation, support, and wrapping material found in the two chamber tombs. A total of 21 samples were selected from different parts of the bony surfaces of both individuals, along with residues of materials directly related to the remains. Thin sections of bone fragments and associated materials were elaborated and scrutinized by means of plain and polarized light microscopy. The slides were obtained after embedding, cutting, and polishing residue samples from both tombs following the protocol established for histomorphometric work (Tiesler et al. 2006). The structural characterization of

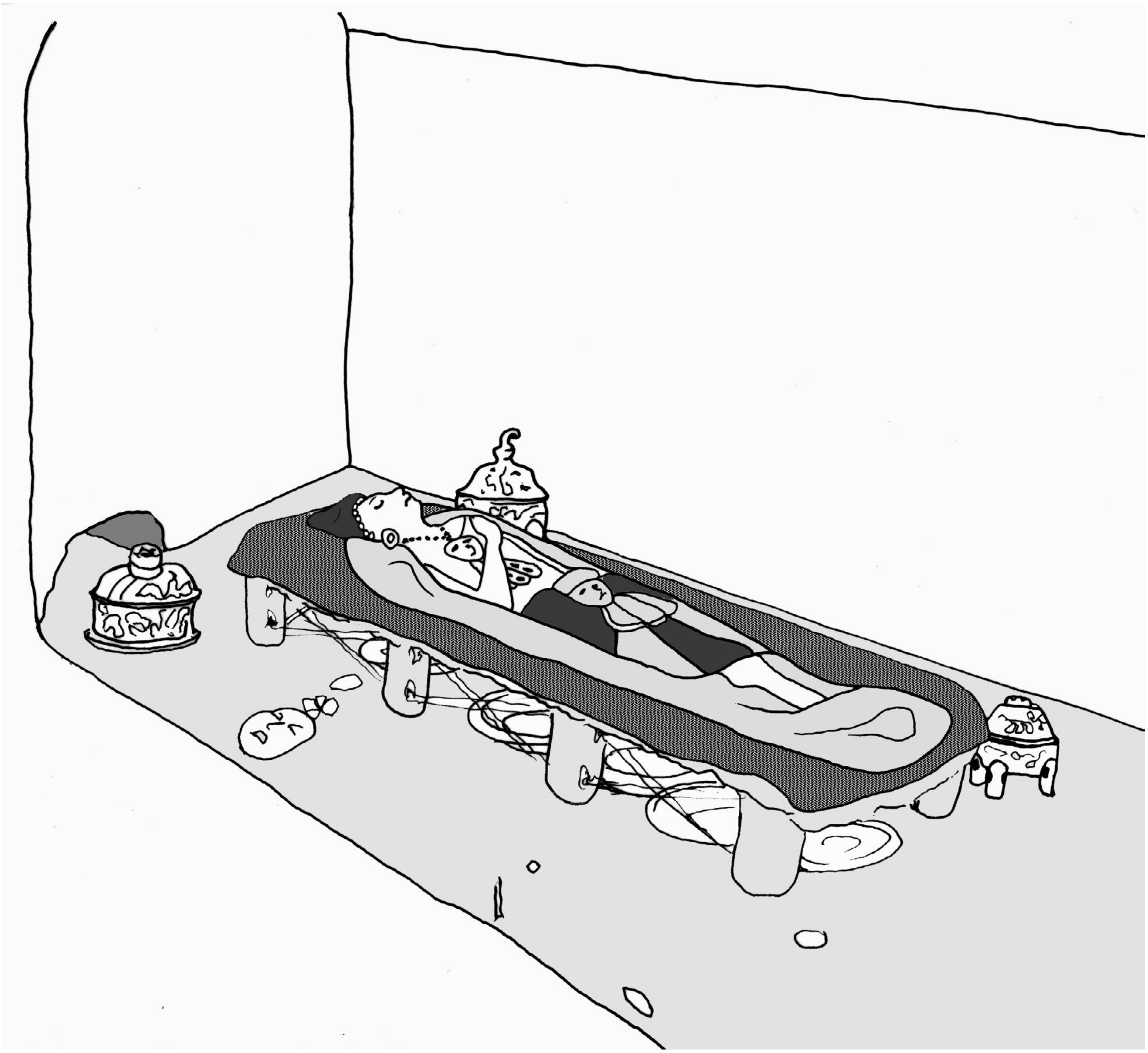

Figure 10.2. Hypothetical drawing of corpse positioning and equipment of Tomb 5 from Structure III at Calakmul (adapted and redrawn by V. Tiesler from Pincemin 1994, Figure 58).

the archaeological samples was carried out by Fourier transform infrared spectroscopy (FT-IR) and X-ray diffraction (XRD). The pigments were also analyzed in a pulverized form by X-ray diffraction after scratching the painted surface. This analysis was used to confirm the crystal structure of the pigment and to identify the mineral crystalline phases present in the pigment. The samples were registered with a diffractometer (Siemens D-5000) operated at 35 kV and 25 mA with a monochromatic Cu Ka radiation (λ = 1.5418Å). Measurements in a 2°–50° (2q) range were taken with a step time of 3s and a step size of .02°. Scanning electron microscopy (SEM) and chemical analysis by energy dispersive spectroscopy (EDS) were conducted with a JEOL 6360 LV microscope, and EDS analysis was performed on 120-x-120-µm^2 scanning areas.

Pigments, Supports, and Wrapping Materials

Tomb 1 (Structure VII)

While our analyses did not find evidence of textiles, they did confirm differences between the materials used for the two wrappings, as already observed during recovery, and they identified the red pigment as cinnabar by mineralogical X-ray diffraction analysis. Cinnabar (HgS) is a brick-red mercury sulfide, an exotic prestigious material imported from volcanic mountain mines of highland Guatemala and Honduras (Houston et al. 2009:57).

Some of the amorphous plaques found in the area of the lower extremities present a regular single surface with a slightly concave outline, probably due to contact with the corpse. Parts of the opposite surface show impressions of interwoven fibers. Microscopy of thin sections reveals a darker external and a lighter internal layer, both of which still preserve a smooth, rubber-like morphology suggestive of the wrap's original elastic properties (Figure 10.3). An interface can be clearly distinguished between the rigid, degraded external surface and the soft inner core. SEM images confirm the above impressions. The darker face exhibits an irregular surface with large amorphous areas and the presence of crater-like depressions in some parts (Figure 10.4). This rugged morphology could be related to degradation through time caused by changing humidity and temperature. Conversely, the lighter-colored surface presents several fissures, similar to those obtained when dehydration or crystallization occurs on a viscous material (Figure 10.4).

Additional EDS chemical analyses were performed on several pieces and on both faces of the wrap. Average atomic concentrations are shown in Table 10.1. The main elements—carbon (C; 53 percent average) and oxygen (O; 8 percent average)— point to an organic material. Calcium (Ca) and silicon (Si) were present in concentrations close to 3.4 percent, whereas mercury (Hg) averaged 1 percent. Calcium and silicon were present in natural minerals, as were quartz and gypsum, whereas the identified mercury is related to traces of cinnabar on the skeletal surfaces.

X-ray diffraction of both sides of the bundle and of the pulverized sample shows a wide amorphous band between 10° and 25° (2q) associated with the organic component of the wrap (Figure 10.5). Gypsum, albite, quartz, and traces of halloysite were recorded on the lighter side of the bundle, whereas only the last two were present on the opposite side. Since quartz and halloysite were detected on both sides, it is possible that these were intentionally added to the organic matrix of the bundle. On the other hand, it is not clear if gypsum and albite (a feldspar) are related to the stuccoed chamber walls or were added to the organic material to increase the resistance of the bundle. Cinnabar was not identified by XRD. Jointly with SEM and optical microscopy examination, the evidence points to the wrapping material being made from some type of latex sap, similar to other elite tombs documented from Calakmul (see, e.g., García Vierna and Schneider Glantz 1996).

The second wrapping material under study is less enigmatic and was identified as a mat made of plant fibers. The micrograph of the network of intertwined vegetable fibers confirms this interpretation. The interwoven fibers (of nearly 50 microns width) form a mesh network. XRD of the mat support showed an amorphous pattern (Figure 10.6) formed by two bands with maximum values at 14° and 20° (2q). The diffractogram

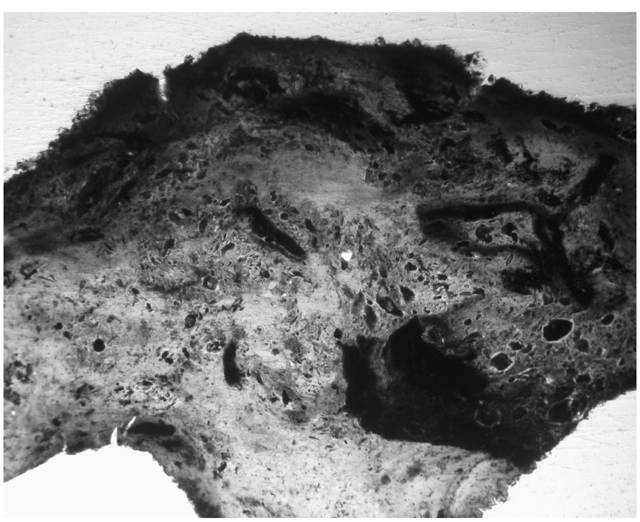

Figure 10.3. Thin section of the wrap from Tomb 1 (Structure VII), exhibiting an amorphous structure with distinctive eroded outer surfaces and an elastic, lighter inner core (x40 microscopy; photo by V. Tiesler).

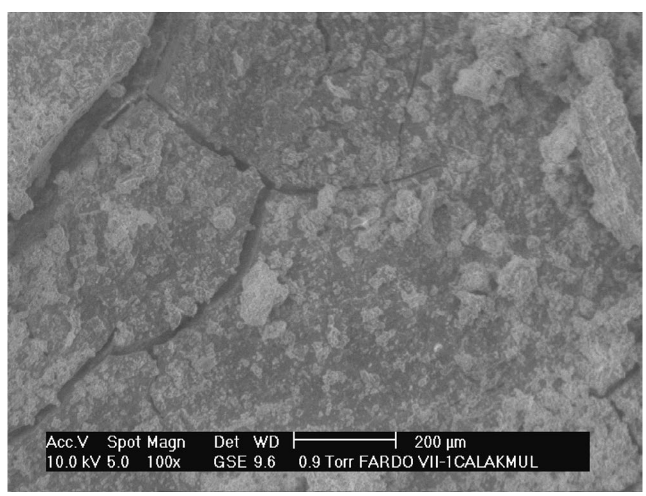

Figure 10.4. SEM image of the surface composition of the wrap (Tomb 1, Structure VII), exhibiting a fissured surface.

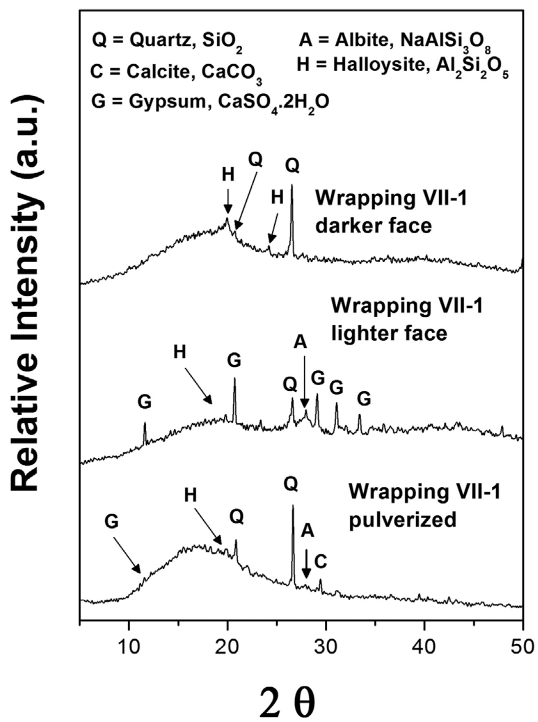

Figure 10.5. X-ray diffraction of the wrap (Tomb 1, Structure VII).

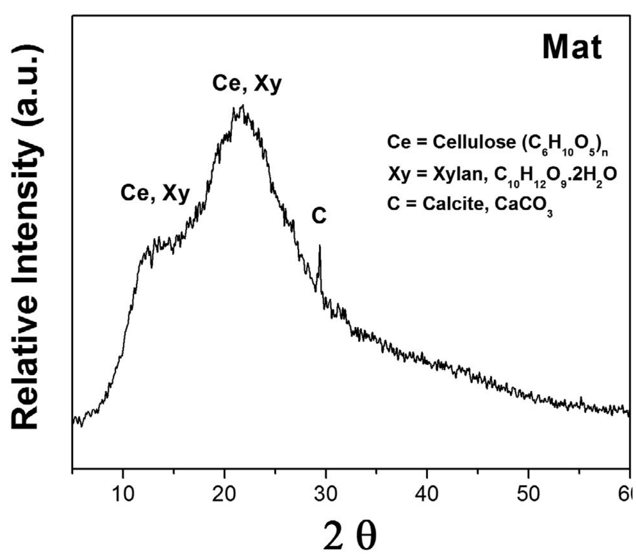

Figure 10.6. XRD pattern of the mat support (Tomb 1, Structure VII).

Table 10.1 Average Atomic Concentrations

Element	Wrapping VII-1 Average %	Fiber mat VII-1 Average %
C	53.01	44.92
O	38.09	39.27
Na	0.12	0.17
Mg	0.15	0.05
Al	0.49	0.05
Si	3.30	12.39
P	0.29	0.00
Hg	1.03	0.00
Cl	0.20	0.42
K	0.24	0.13
Ca	3.43	1.8
Fe	0.29	0.00
S	0.00	0.22

is similar to that of cellulose, $(C_6H_{10}O_5)_n$ and cellulose-derived patterns, such as xylan ($C_{10}H_{12}O_9.2H_2O$). Calcite was also detected. The chemical composition of the mat (Table 10.1) was of an organic nature, as oxygen and carbon were the main elements (44.92 percent and 39.27 percent), followed by silicon (12.39 percent) and calcium (1.8 percent).

Tomb 5 (Structure III)

The second case study refers to the chamber tomb from Structure III. As in the first tomb, no material textile evidence was preserved. We therefore focused for the residue analysis on the pigment concretions that covered most of the skeleton and the fragments of organic material identified as wood recovered beneath the remains.

A pigment nodule was sectioned in different orientations and the slides scrutinized with reflected light. The observed pattern exhibits a stratigraphy of alternating reddish and thin corrugated, amorphous, transparent to blackish layers (Figure 10.7). In this case, the vermillion pigment appears as a homogeneous, compacted deposit, which appears to have been applied together with a dark organic coating, possibly a vehicle to create a thick paste to be applied on the body. The total thickness of the layered red-and-black plaque ranges between 3 and 5 mm. As in Tomb 1, SEM and EDS analyses demonstrated that the red pigment was cinnabar. Additional X-ray diffraction of the pulverized pigmented sample on the wooden support (Figure 10.8) showed large amounts of

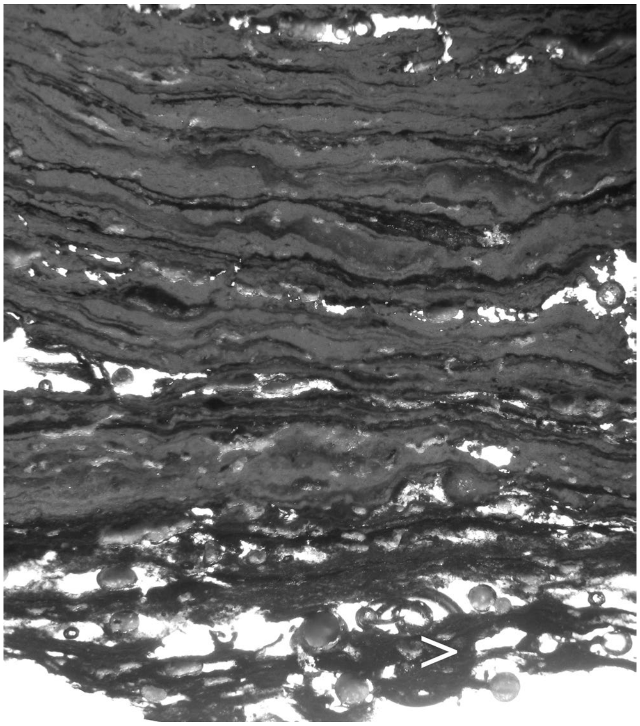

Figure 10.7. Thin section of alternating layers of cinnabar and organic black coating in the pigment concretion recovered from the support of Tomb 5 of Structure III (x25 microscopy; photo by V. Tiesler).

Table 10.2 EDXS Analysis of Pigment

EDX		
WOOD B.III-9		
Elements (%W)	**Dark seccion**	**Clear section**
C	**27.48**	**9.21**
N	0.00	0
O	**31.13**	**26.96**
Na	0.14	0.00
Mg	0.21	0.17
Al	2.40	1.83
Si	**5.01**	**3.29**
P	0.80	0.00
S (Hg)?	0.00	0.00
Cl	0.00	0.00
K	0.26	0.00
Ca	2.76	1.99
Fe	1.53	1.21
Mo?	**7.35**	**13.24**
Hg	**20.93**	**42.11**

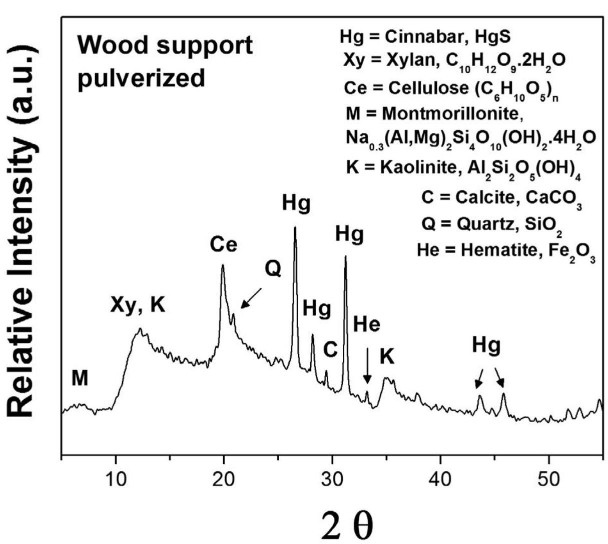

Figure 10.8. XRD of a powdered sample of wood support from Tomb 5 of Structure III.

cinnabar along with calcite, clays such as montmorillonite and kaolinite, and traces of hematite. Hematite is a mineral of iron (III) oxide (Fe_2O_3) that was used in ancient Maya mortuary treatments as a substitute for, or in addition to, cinnabar (Bolio et al. 2012; Houston et al. 2009). Similar to cinnabar, hematite from mortuary contexts is red in appearance. Additional organic components of the wood, xylan, and crystalline cellulose (peak near 20° 2q), were detected. It should be noted that cellulose appears more crystalline in the wooden sample than in the mat of Burial VII-1. The EDS analysis of the pigment-covered surface and of the wood (Table 10.2) showed a higher content of mercury and carbon, respectively.

DISCUSSION

Wrapped or elevated chamber burials of nobles such as the two from Calakmul were widespread in the Maya realm during the Classic period. Funerary bundles, some of which were very elaborate, were probably in use among the ancient Maya since the Preclassic period (Carrasco 2004; Pereira and Michelet 2004). At Calakmul, a large array of materials was employed to carry, cover, and wrap the deceased elite during the

Early and Late Classic periods. As previous studies have demonstrated, supports and bundles were tailored from textiles, resin-embedded fabrics, latex, straw mats, or a combination of materials, along with stuccoed wood for bier supports (Carrasco Vargas 2004; García Moreno 2005; García-Moreno and Granados García 1999; García and Schneider Glantz 1996; Pincemin 1994). In most elite tombs from Calakmul, where the identification of residues is hampered due to their advanced state of decay, there are taphonomic indications of bundling, as many of Calakmul's elite burials show vestiges of body "constriction" and elevation, leading to a characteristic skeletal distribution of the shoulders, pelvic girdle, knees, and ankles (Tiesler 2004). Here, as in other lowland Maya sites, the bundles could be laid on different kinds of supports, such as mattresses, wooden platforms, or "beds." Within the dominant sectors of Maya society, the use of elevated mortuary biers was especially popular during the Early Classic period (Hall 1989; Pereira and Michelet 2004). Bier elevation and wraps were probably reserved for selected ancestral shrines of the aristocracy, since commoners' filled-in graves seldom show signs of constriction or bier elevation (see, e.g., Medrano 2005). However, there survives no clear pattern in the elite mortuary record that might stand for any standardized posthumous procedures in this part of society. The observed diversity of individual body treatments suggests instead that the preparation and accommodation of a deceased body was family business.

More widespread than body wrapping in ancient Maya society was corpse pigmentation. We explored what role pigment covering might have played as part of the predepositional preparation of dead rulers by examining the consistency of applied pigments and the layering of pigments and wraps. Regarding the postmortem timing for ancient pigment application, there is still no agreement on whether it was applied directly on the dead body, on body wraps, or on already skeletonized remains. Classic-period Maya inscriptions refer to the painting of bones, pointing to the use of red pigment in secondary mortuary treatments (Eberl 2005). Other data, specifically the taphonomy of undisturbed primary burials, emphasize the coloring of freshly installed corpses rather than bones (Tiesler 2006).

For the two cases described here, along with others from Calakmul and Palenque (Tiesler 2004, 2006; Tiesler et al. 2002), we dismiss the hypothesis that the remains were painted after skeletonization, based on the pattern of pigment distribution on the bones and on placement between bones and body wrapping materials. Also, the undisturbed nature of both funerary chambers, as shown by the sealed roof and the anatomical distribution of skeletal segments, recorded during their recovery in the 1980s, rules out later entrances into the tomb and further handling of the remains. All this suggests that pigmentation did not involve skeletonized ancestral remains but instead formed part of corpse preparation, along with bundling prior to primary burial. This process is referred to in the epigraphic inscriptions as the *muhkaj* event (Eberl 2005). According to the mourning calendar, these pre-interment preparations could last several days. If we trust the epigraphy, the subsequent *muhkaj* ceremonies were performed up to 10 days after death. Since these festivities were probably public events, we can assume that the elaborate corpse preparations, like the ones studied here, would have held powerful symbolic and ostentatious functions.

A second question concerns the substances used for pigmentation, their forms of application, and their possible functions and meanings in ancient Maya ancestral treatments. Earlier studies have identified cinnabar and hematite in different proportions as the preferred colorants for painting the dead, although knowledge regarding the precise processes implied in pigment preparation and forms of application in ancient Maya mortuary treatment is still rather vague (Canto et al. 2004; Gazzola 2003; Tiesler 2006; Tiesler et al. 2004; Vázquez Negrete and Velázquez 1996; but see Houston et al. 2009). Specifically, the uses of cinnabar appear to have been restricted to elite mortuary treatments and sacred carvings and writings in temple contexts (Houston et al. 2009:57–65).

Additional substances, such as amorphic agglutinants, which we have documented in this study, could have been added as paint vehicles to improve durability and to facilitate application of the colorants as paint or paste. The alternating layers of cinnabar and dark organic coats that we recorded in one of our case studies are reminiscent of glazed blends of smooth pastes, directly applied on the skin of the deceased. This predepositional treatment is similar to what we have recorded for another Maya paramount from Palenque: Janaab'Pakal, whose body was laid to rest deep inside a monumental mausoleum in the Temple of the Inscriptions (Tiesler 2006). Here, the cinnabar showed organic and mineral impurities. It has up to five layers of coating alternated with a black organic substratum, identified as a carbon paste or bitumen, which looks similar to the pigment concretions associated with the remains of Calakmul's Tomb 5 in Structure III (Tiesler 2006).

Other forms of application, inferred in this and other studies, appear to range from powder sprinkling to blends of cinnabar and transparent organic adhesives of cinnabar and hematite (Bolio et al. 2012; García-Moreno and Granados 2000; Tiesler and Cucina 2006; Vázquez de Agredos 2006, 2007). Namely, our recent research on dynasts from the Maya Classic-period capitals of Palenque, Ek Balam, and Dzibanché has identified similarly high proportions of cinnabar as those documented in this study. In contrast, commoners' mortuary pigmentation, when present, apparently consisted of hematite, a more accessible substitute for the exotic cinnabar imports (Bolio et al. 2012).

Considering that cinnabar was employed together with different organic wrapping and support materials in the preparation of Calakmul's elite, it is reasonable to wonder whether its use in posthumous body pigmentation—like the use of latex, resin, or embedded fabric bundles—could have been employed not only for ritual but also for practical purposes, similar to the embalming techniques used in other ancient societies. (See, for example, David 2000 for materials used in Egyptian embalming techniques.) A compound of mercury and cinnabar is toxic and should have been helpful in slowing the incipient biological decomposition processes during corpse transportation or during the probably prolonged funerary ceremonies that preceded burial, which were undoubtedly public events of major importance. It is probably no coincidence that the body from Tomb 5 still had preserved hair strands below the plaques of vermilion pigmentation, a unique finding considering the generally poor preservation of hair on lowland Maya remains.

Further questions remain regarding the ritual meanings of funerary wraps, biers, and cinnabar pigmentation in local and regional traditions and their enactment according to the ritual calendar. The performance of royal corpse positioning, wrapping, binding, and concealing is itself powerfully reminiscent of the preparation of sacred bundles used for Maya ritual ceremonies (Guernsey and Reilly 2006). We think that cinnabar's exotic value and naturally occurring bright red color should have rendered it a remarkable ingredient in ancient royal ancestral rites, evoking life in the form of its primary living essence: blood. Its combination with the green jadeite ornaments on top of the royal corpse bundles expressed broader existential dualities of aristocracy, namely those of precious life and divine ancestry (Houston et al. 2009:82–83). Houston and colleagues also note the profoundly conservative quality of color choice in tomb decorations, a palette that remained essentially the same between the Preclassic and the Postclassic and that combined red and black tones on white surfaces (Houston et al. 2009). This profoundly primeval color scheme is also expressed in the cinnabar coatings we documented at Calakmul and Palenque, specifically the combination of black and red tones. These ancestral blends should have evoked the perpetual cycle of life, death, and ancestry, demise and resurrection, as expressed by the daily sunrise and sunset (Hammond 1989).

In closing, this study on ancient mortuary wraps and pigmentation among ancient Maya royals has strived to combine fruitfully the breadth of analytical tools and interpretational frameworks that the topic demands. The range of analyses presented here extends from material identification to microscopic and macroscopic morphological examination to taphonomic and behavioral mortuary analyses. These bring us to cultural interpretations of ancient Maya ancestral ideology and ritualized performance informed by coherent ideological frames and long-standing undercurrents of autochtonous Mesoamerican mortuary expressions. We were positively surprised that this study, which reexamines minute bone appositions three decades after excavation, was still possible. In this sense, we wish to encourage similar interdisciplinary inquiries of residues directly associated with skeletal remains, a source of information that we feel is still underexplored in Maya research.

Acknowledgments

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11

An Isotope Study of Childhood Diet and Mobility at Copan, Honduras

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In the 1970s, bioarchaeology emerged as a new field with goals both in archaeology and physical anthropology. With its name coined by Jane E. Buikstra, bioarchaeology emphasized populational approaches to ancient human biology (Buikstra 1977:69; Buikstra and Cook 1980) and stood in contrast to the descriptive osteobiography of the most prominent skeletal studies on the ancient Maya at that time, which served more narrowly archaeological goals (Saul 1972). Yet bioarchaeology began at the same time archaeologists of the ancient Maya emphasized settlement pattern studies over the study of tombs and temples. Truly populational approaches to the study of ancient Maya remains have been hindered by the small size of most Maya skeletal series and by the poor preservation of bone in the humid tropical lowlands of the Maya area. While populational studies of Maya remains have produced meaningful (and often highly debated) interpretations of ancient Maya health and adaptation (Wright and White 1996), osteobiographic approaches have recently found new relevance to Maya bioarchaeology. For instance, Buikstra's recent work in the Maya area reconstructs life histories of key elite skeletons at Palenque (Buikstra et al. 2006) and Copan (Buikstra et al. 2004). It is not coincidental that this renewed interest in life history followed the revolution in decipherment of the Maya hieroglyphic writing system that emerged in the 1980s and 1990s. The rich epigraphic culture-history of the Maya area tempts us to reconstruct individual lives of those few named participants of the ancient past, many of whose identities and family histories are better known now than they were during the growth of populational bioarchaeology. Moreover, bone chemical methods now offer a new approach to reconcile the historical reconstructions of Maya dynasties with the silent remains in royal tombs (Wright 2005b).

Stable isotopes can now be used to reconstruct diet at precise ages through the life span, permitting quite detailed resolution of individual dietary histories. In the last decade, we have added stable oxygen ($\delta^{18}O$) and strontium ($^{87}Sr/^{86}Sr$) isotopes to our analytic tool kit, bringing the opportunity to evaluate place of residence in addition to diet. For Sr, this is fairly straightforward because Sr comes into the body via foods. Its isotopic ratio is determined by that of the geological substrate and thus the soils on which foods are grown. In Mesoamerica, the dramatic geological differences between highlands and lowlands allow fairly good $^{87}Sr/^{86}Sr$ discrimination between these varied geological homelands (Price et al. 2000; Price et al. 2007). Although the trade of foods and sea salt (Wright 2005a) may confound this picture, there is considerable homogeneity within the Maya lowlands themselves (Hodell et al. 2004), and sorting out these influences is relatively straightforward for $^{87}Sr/^{86}Sr$.

Oxygen isotopes seemed at first to be good discriminators of geographic origin, as they vary with the composition of rainwater across the landscape. For instance, White et al. (1998) were able to identify outliers among Teotihuacan skeletons, and $\delta^{18}O$ values can be shown to vary dramatically between central

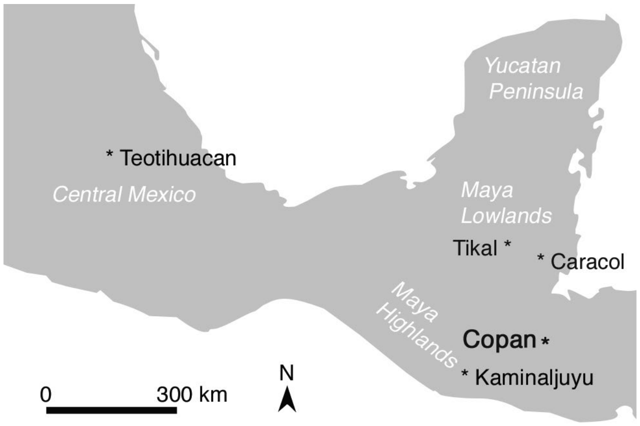

Figure 11.1. Map of Mesoamerica indicating the location of Copan and other key sites mentioned in the text.

Mexico, Oaxaca, and the Maya area (White et al. 2002). However, interpreting $\delta^{18}O$ data is a more complicated endeavor than interpreting $^{87}Sr/^{86}Sr$ data because $\delta^{18}O$ can be affected by much more than geographic variability and food trade. In this chapter, I aim to illustrate some of these complications using a small series of elite skeletons from the Classic-period (A.D. 250–900) Maya city of Copan in Honduras (Figure 11.1). My work with these remains is one component of Buikstra's multidisciplinary life history study in the region. In another publication, Price et al. (2010) found the oxygen isotope data to be less useful than strontium isotopes for studying mobility at Copan. To further explore this issue, I consider variability of both oxygen and carbon isotopes within individual teeth as well as between teeth.

Oxygen Isotopes and the Study of Geographic Provenance

The $\delta^{18}O$ ratios of bones and teeth are determined largely by the composition of water imbibed by an individual at the time the tissues formed (Kohn 1996; Longinelli 1984), offset by a fractionation that is dependent on the body temperature of the organism. The $\delta^{18}O$ of meteoric water, or rainfall, is determined primarily by the paths that weather systems take from their origins over the oceans (where rain clouds form from the evaporation of seawater) and the gradual rain-out of the heavier isotope, ^{18}O, as clouds move across continental land forms. Newly formed over the ocean, tropical clouds (and rainfall) that contain relatively more ^{18}O are referred to as heavy because they contain more of the heavier isotope, while rain from clouds that have already lost much of their ^{18}O is referred to as isotopically light. The rainwater $\delta^{18}O$ of a given location is thus influenced by its latitude, elevation, and distance from the ocean (Rozanski et al. 1993). These geochemical principles are the basis for the suggestion that $\delta^{18}O$ in human remains might shed light on geographic provenance (Schwarcz et al. 1991; White et al. 1998). The $\delta^{18}O$ of human tissues may differ from that of rain falling in the landscape where people live due to a variety of factors.

Rainwater $\delta^{18}O$ is seasonally variable, both in temperate latitudes and in tropical areas with marked dry and wet seasons. For instance, Stuart-Williams and Schwarcz (1997) documented seasonal variations of 4‰ in Canadian beaver (*Castor canadensis*) dental enamel. The amplitude of the seasonal fluctuations was much smaller than that shown in rainwater (10‰) due to mixing of rainfall with existing surface waters, as well as mixing of water imbibed with the body water reservoir of the beavers. In the tropics, seasonal fluctuation in $\delta^{18}O$ is determined more directly by seasonal variation in the amount of rainfall (Rozanski et al. 1993). Figure 11.2 illustrates the weighted mean $\delta^{18}O$ of precipitation at Ilopango, El Salvador. The inverse relationship between $\delta^{18}O$ and precipitation levels is clear; rainy-season $\delta^{18}O$ averages 7‰ lighter than the meager dry-season rainfall. Naturally, this seasonality in meteoric water carries through to water available in soils and thus available to plants. In Costa Rica, Monteverde cloud forest trees show a 9‰ seasonal variation in wood $\delta^{18}O$ (Anchukaitis 2007).

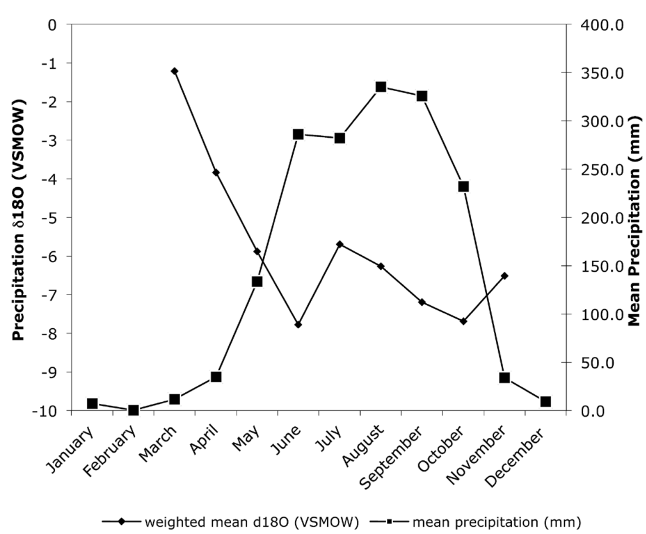

Figure 11.2. Monthly mean precipitation and weighted mean $\delta^{18}O$ of rainwater at the Ilopango weather station, El Salvador, 1968–1984. (Data are from the Global Network of Isotopes in Precipitation database (IAEA 2006).)

Rainfall levels also vary from year to year, contributing to variability in $\delta^{18}O$. Interannual fluctuations in $\delta^{18}O$ are evident in the larger Global Network of Isotopes in Precipitation database for the Ilopango weather station, with values reported ranging from zero to -13‰$_{SMOW}$ (IAEA 2006). This interannual variability is undoubtedly a major contributor to the broad variability of $\delta^{18}O$ values seen at a given site. Climate fluctuation has been shown in $\delta^{18}O$ of ostracods from lake sediments throughout the Maya area. These fluctuations are up to 2‰ over the long term and 1‰ within less than a century (Hodell et al. 1995; Rosenmeier et al. 2002). Though climate change occurs on longer-term cycles than the lives of most individuals, even skeletons buried within a single chronological period can be expected to vary somewhat due to climate fluctuation.

Rainfall recharges the natural and artificial reservoirs that were used by the ancient Maya for drinking water and for culinary needs. The degree to which seasonal variation in rainfall $\delta^{18}O$ will affect drinking water sources depends in part on the size of water reservoirs exploited by humans and their evaporation/recharge ratios. Larger water bodies show less fluctuation than small ones, and less evaporative enrichment. Cultural activities are an unknown contributor to $\delta^{18}O$ variability. In complex societies, social classes may obtain water from different sources; elites may have exclusive access to specific reservoirs, streams, or caves that can differ in evaporation/recharge rates and thus have different $\delta^{18}O$ than water imbibed by the populace at large. Cultural behaviors may also contribute to varied ratios. For instance, extended boiling can be expected to raise the $\delta^{18}O$ of beverages slightly.

At highland Kaminaljuyu, Wright and Schwarcz (1998) documented an average decline in $\delta^{18}O$ of .7‰ between first molars and third molars—probably a consequence of weaning: a dietary change from ^{18}O-enriched breast milk to water. However, we know that children nurse for hugely variable periods and intensities, and the proportions of milk and water they imbibe vary accordingly. While nursing behaviors are culturally shaped, there is substantial variability among infants and mothers within each culture (Dettwyler and Fishman 1992), and this should be expected in the isotope signatures.

These natural and cultural factors contribute to variation in $\delta^{18}O$ over time at any given site and among individuals. Moreover, considerable variability may be expected among the teeth of a single individual (Wright and Schwarcz 1998) or indeed within a single tooth. Since most permanent teeth form over the span of two to four years, seasonal fluctuations in $\delta^{18}O$ may well be visible in dental $\delta^{18}O$ values, if samples are sufficiently precise. Wright and Schwarcz (1998) analyzed samples that spanned from the cervical margin to the cusp of the tooth, thus averaging intratooth fluctuations. However, we should expect considerable variation between cervical and cuspal enamel within a single tooth due to cultural and dietary reasons alone. Hence, the precision of dental sampling is an important issue when comparing the results of oxygen isotope data collected in differing ways. As sampling methods continue to be refined, seasonality, annual fluctuations, and child feeding practices will need to be given more attention in oxygen isotope reconstructions.

In this chapter, I report inter- and intratooth variability in both $\delta^{18}O$ and stable carbon isotopes ($\delta^{13}C$) in tooth enamel from the ancient Maya city of Copan, Honduras. Maize was the primary C4 food consumed by the Maya, thus the $\delta^{13}C$ of the enamel carbonate measures the proportion of carbon that came from maize in the whole diet. Thus $\delta^{13}C$ ratios could highlight gross dietary differences that may also distinguish a foreign skeleton. The goals of this work were twofold: to determine whether or not the oxygen isotope values of the teeth would identify any of the skeletons as nonlocal to the Copan Valley, and to examine variability within and among teeth from an individual skeleton that might be due to climatic, seasonal, or dietary shifts. Because considerable variability was found in the isotope ratios, this small sample provides an interesting cautionary tale.

Copan and the Skeletal Remains

Ancient Maya civilization flourished in the tropical lowlands of Central America during the first millennium A.D. The Classic-period (A.D. 250–900) city of Copan lies in the eastern margin of the Maya lowlands in the valley of the Copan River. The city is well-known for the elaborate sculptural decoration of the monumental architecture located in the city center. The acropolis consists of successive constructions of temples and platforms containing the tombs of several of Copan's epigraphically known rulers. As the decipherment of ancient Maya writing has enabled archaeologists to learn the dynastic history of the kingdom, and as acropolis excavations have revealed elaborate iconography on buried temples and in tombs, questions have arisen about the relationship of the Copan dynasty with distant Maya cities such as Tikal and with other non-Maya states in Mesoamerica such as Teotihuacan (Bell, Canuto, et al. 2004). Thus bioarchaeological techniques that permit the identification of foreign individuals in the tombs gain relevance to a broad archaeological readership.

Tooth samples were collected from eleven skeletons from Copan by Jane E. Buikstra as part of a larger study of life history among Early Classic (A.D. 400–600) Copan elites (Buikstra et al. 2004). The skeletons were excavated from structures in the acropolis of the ancient Maya city by archaeologists from the University of Pennsylvania and Harvard University. The tomb occupants include rulers, female consorts, and perhaps other members of the royal courts, as well as possibly sacrificial victims of unknown origin. Many of these burials were accompanied by elaborate funerary goods, many implying long-distance contacts. Further details of the excavations and funerary contexts can be found elsewhere (Bell, Sharer, et al. 2004; Fash et al. 2004; Price et al. 2010; Williamson 1996).

Burial 95-2 is also known as the Hunal Tomb. Found in a lavish masonry tomb within Structure 10L-16, the skeleton is thought to be the epigraphically known founder of the Copan dynasty, K'inich Yax K'uk' Mo', and the stratigraphic context of the grave is consistent with his reign in the mid-fifth century A.D. (Bell, Sharer, et al. 2004). The skeleton is that of a male, who was older than 50 years at the time of his death, and it shows a number of injuries consistent with trauma inflicted either in battle or perhaps in the ancient Maya ball game (Buikstra et al. 2004). The elaborate structure built over this tomb shows iconographic connections with Tikal, in the central Petén, and with Teotihuacan, in central Mexico, as do the artifacts interred with the skeleton.

Burial 93-2 is commonly known as the Margarita Tomb. It was located within Structure 10L-16 and is slightly later in date than the Hunal Tomb. This tomb had two chambers: an inner mortuary chamber where the skeleton lay, and an outer chamber that contained numerous offerings, including a hieroglyphic monument known as the Xukpi Stone. This monument describes a funerary rite and mentions both Yax K'uk' Mo' and the second ruler of the city. The skeleton in the inner chamber is that of a woman, aged more than 50 years at death. Archaeologists speculate that she may have been the spouse of Yax K'uk' Mo'.

Burial 94-1 has been referred to as the Northern Guardian because this grave was located at the northern entry to the Margarita Tomb. It was probably deposited around A.D. 465 as an intrusive deposit into the Mitzil Platform. The skeleton was not accompanied by any grave goods (Bell, Sharer, et al. 2004). The remains were placed in a seated, bundled position and are those of a male, aged 30 to 50 years at death (Buikstra et al. 2004).

Burial 95-1 was found in an unlined cist, intrusive into a later Early Classic platform called Allamanda that was superimposed over the Margarita structure. It was accompanied by three ceramic vessels, five obsidian dart points, and two shell circles placed over the eyes (Bell, Sharer, et al. 2004). These "goggles" resemble the iconographic depictions of the central Mexican deity Tlaloc, hence the skeleton is sometimes nicknamed the Tlaloc Warrior. The skeleton is male, probably aged 40 years or older; however, the bone preservation was poor (Buikstra et al. 2004).

Burial 92-1 contained the disarticulated remains of a young adult male, aged 18 to 25 at death (Buikstra et al. 2004). The grave was located in the so-called Teal Platform, for which it is sometimes named. Located at the northern limit of the early acropolis and the southern margin of the Northeast Court group, the grave was a simple pit dug into structure fill below the floor of Patio 5B (Bell, Sharer, et al. 2004). The grave contained the dismembered and poorly preserved remains of an adult male.

Burial 93-1, referred to as Uranio, was a flexed interment of a single adult male skeleton located beneath an early platform below the early development of the acropolis. Associated with some burned jade offerings, the skeleton was not accompanied by further grave goods and was not deposited in a formal grave structure.

Burial 92-3 lay below a structure on the west side of the East Court 2B of the acropolis. Bell, Sharer et al. (2004:151–153) identify this grave as containing the remains of the eighth ruler in the Copan dynasty, whose name, Wil Ohl K'inich, is given in the inscriptions on the stairway of the structure containing the tomb. Commonly known as the Subjaguar Tomb, this grave was found below the sculptural Jaguar Stairway, which was added to the structure at a later time. Inscriptions indicate that Ruler 8 died in A.D. 551 after reigning 19 years (Bell, Sharer et al. 2004). However, there is no epigraphic evidence in the tomb itself to identify the occupant, so the identification is not certain. Accompanied by elaborate offerings, the grave contained the poorly preserved remains of an adult skeleton that may have been male. Age can be only roughly estimated, by the advanced state of dental attrition, as older than 35 (Buikstra et al. 2004). (Note: the Subjaguar Tomb was incorrectly published as Burial 92-2 [Bell, Sharer et al. 2004] but should correctly be Burial 92-3 [Price et al. 2010]).

Burial 37-8 is better known as the Motmot Tomb. The grave is from the earliest level of construction of Structure 10L-26, which in its final phase was decorated with an elaborate hieroglyphic stairway. Excavated into the earliest phase during a renovation, the cylindrical stone-lined cist contained the grave of a young adult female (Burial 37-8). The tomb style and seated position are reminiscent of burials at Teotihuacan. Later the grave was reopened and a fire ritual was performed. At this time, three decapitated skulls were added to the grave (burials 37-7, 37-9, and 37-10), and the cist was capped with a circular hieroglyphic monument, the Motmot Stone. Its inscriptions and context indicate that the reopening of the tomb coincided with a calendrical ritual carried out by Ruler 2 on the date 9.0.0.0.0. (A.D. 435) (Buikstra et al. 2004; Fash et al. 2004).

SAMPLING AND STABLE ISOTOPIC METHODS

Table 11.1 lists the teeth sampled from each skeleton, along with the stable carbon and oxygen isotope results. Dental samples were selected primarily for the purposes of strontium isotope analyses (Figure 11.3) (Buikstra et al. 2004; Price et al. 2010) and aimed to include a tooth that develops in early childhood as well as a third molar (M3)—which forms between 9 and 13 years of age—from each individual. M3s were sampled by cutting a section of enamel spanning from the cusp tip to the cervical margin, 1–2 mm wide, which was ground and homogenized. First molars (M1) develop between birth and 3.5 years of age. A 2-mm-wide span of enamel from the cusp to the cervical margin was removed from the M1s and divided into three samples, each approximately 2 mm wide, of equal length along the dentoenamel junction. These samples were labeled A, B, and C, with A being the cuspal sample, B the mid-coronal sample, and C the cervical sample. For several skeletons, a lateral incisor (I2, which develops from .5 to 4 years) was sampled, and the section was similarly divided into three samples. Two maxillary central incisors (I1) were sampled; because of the longer crown height of these teeth, the samples were divided into four sections of equivalent length. For some teeth, one or more of these components of the section was missing, and all samples could not be analyzed for each tooth. This is admittedly a crude sampling method, but it gave large enough samples for acid treatment to remove diagenetic carbonates. In view of the development of the teeth, each sample corresponds roughly to a year of growth.

The enamel was ground to a fine powder in an agate mortar and pestle and passed through a 50-μm sieve. The powdered enamel was soaked overnight in 1 ml of 1.5 percent sodium hypochlorite solution to remove any organic materials. After rinsing three times with distilled deionized water, the enamel was soaked in 1 M acetic acid, buffered with sodium acetate to pH 4.5, for one hour, with periodic agitation. The enamel was rinsed to neutrality with deionized water and dried at 70°C. Mass spectrometry of the enamel samples was carried out at the Department of Geology and Geophysics at Texas A&M University. The enamel was reacted with orthophosphoric acid at 80°C in a Kiel II carbonate device, and stable isotope ratios were measured on the evolved gases in a Finnegan MAT 251 mass spectrometer. The $\delta^{13}C$ and $\delta^{18}O$ were corrected to the PDB standard by comparison with aliquots of the NBS-19 carbonate standard analyzed in each run.

STABLE ISOTOPES IN COPAN TOOTH ENAMEL CARBONATE

Figure 11.3 illustrates the $\delta^{18}O$ and $^{87}Sr/^{86}Sr$ of those teeth sampled for which Buikstra et al. (2004) and Price

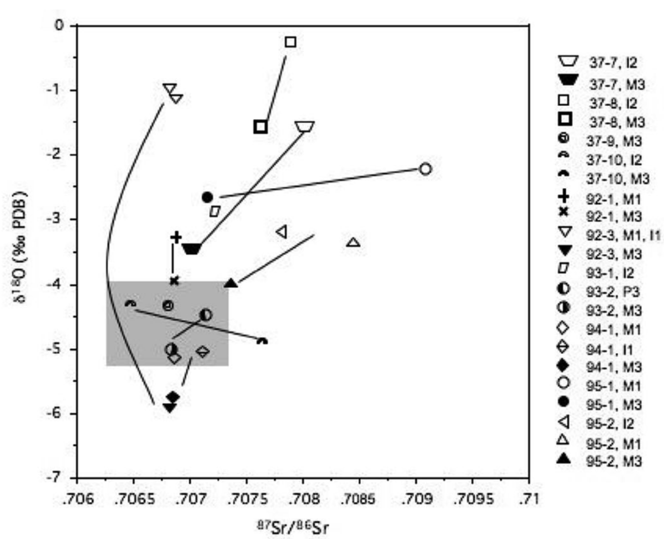

Figure 11.3. Stable oxygen and strontium isotope ratios in tooth enamel carbonate from Copan. Strontium isotope ratios are from Buikstra et al. 2004.

Table 11.1. Stable Isotopes in Early Classic Copan Tooth Enamel Carbonate.

Burial	Nickname	Tooth	Sample	$\delta^{13}C^1$	$\delta^{18}O$
92-1	Teal	M1, mand R	A	-.51	-3.19
92-1	Teal	M1, mand R	B	-.25	-3.25
92-1	Teal	M1, mand R	C	-.21	-3.70
92-1	Teal	M3, mand R	whole	-.88	-3.99
92-3	Subjaguar	I1, max R	A	-4.93	-1.10
92-3	Subjaguar	I1, max R	B	-5.94	-.99
92-3	Subjaguar	I1, max R	C	-6.20	-.88
92-3	Subjaguar	I1, max R	D	-4.96	-.25
92-3	Subjaguar	M1, max R	A	-5.23	-1.16
92-3	Subjaguar	M1, max R	B	-5.54	-1.16
92-3	Subjaguar	M1, max R	C	-5.73	-.76
92-3	Subjaguar	M3, mand L	whole	-3.30	-5.96
93-1	Uranio	I2, mand	A	-3.77	-2.81
93-1	Uranio	I2, mand	B	-3.59	-2.97
93-2	Margarita	P3, mand L	whole	-1.86	-4.52
93-2	Margarita	M3, mand R	whole	-2.99	-5.01
94-1	N. Guardian	I1, max R	A	-3.26	-5.03
94-1	N. Guardian	I1, max R	B	-3.04	-5.05
94-1	N. Guardian	M1, mand R	B	-3.46	-5.15
94-1	N. Guardian	M1, mand R	C	-3.09	-5.56
94-1	N. Guardian	M3, mand L	whole	-2.99	-5.77
95-1	Tlaloc	M1, mand L	A	-5.45	-2.28
95-1	Tlaloc	M1, mand L	B	-5.21	-2.21
95-1	Tlaloc	M1, mand L	C	-5.90	-2.29
95-1	Tlaloc	M3, mand L	whole	-4.09	-2.66
95-2	Hunal	I2, mand L	whole	-4.50	-3.17
95-2	Hunal	M1, mand L	A	-4.56	-4.10
95-2	Hunal	M1, mand L	B	-4.40	-3.39
95-2	Hunal	M1, mand L	C	-4.65	-3.36
95-2	Hunal	M3, max R	whole	-2.53	-4.02
37-7	37-7	I2, max	A	-4.47	-1.38
37-7	37-7	I2, max	B	-4.23	-1.66
37-7	37-7	M3, mand	whole	-3.27	-3.44
37-8	Motmot	I2, mand	A	-2.90	-.27
37-8	Motmot	M3, max L	whole	-4.95	-1.63
37-9	37-9	M3, max	whole	-4.63	-4.35
37-10	37-10	I2, mand	A	-3.69	-4.38
37-10	37-10	I2, mand	B	-3.01	-4.52
37-10	37-10	I2, mand	C	-2.40	-4.86
37-10	37-10	M3, max	whole	-2.14	-4.96

Note: Values for $\delta^{13}C$ and $\delta^{18}O$ are given in units permit, relative to the PeeDee Belemnite standard.

et al. (2010) published $^{87}Sr/^{86}Sr$ ratios. For M1s and incisors, the mid-coronal B sample is shown. Samples from the same skeleton are shown with similar symbols and are joined by lines. In $^{87}Sr/^{86}Sr$, Price et al. (2010) define a local range for Copan as .7063 to .7074, based on both commoner human remains and faunal remains, a range that reflects Copan's location on volcanic soils. Lower values (.704–.706) are found in the more recent volcanic soils of highland Guatemala, as at Kaminaljuyu, and in central Mexico at the important early Classic city of Teotihuacan. Higher values (.707–.709) characterize the core of the lowland Maya area, in the sedimentary limestones of northern Guatemala and the Yucatán Peninsula (Hodell et al. 2004; Price et al. 2008).

Price et al. (2010) report $\delta^{18}O$ values on M1 enamel carbonate from eight Copan commoner burials. These average -4.3 ± 0.3‰$_{PDB}$ and exclude two outlying values that must be migrants. Since these M1 values are presumably enriched by nursing, we can consider a local range for Copan enamel to be -4.0 to -5.3‰, to include teeth formed at both breastfeeding and weaned ages. Almost all the skeletons sampled by multiple teeth, including burials 37-10, 92-1, 93-2, 94-1, 95-1, and 95-2, show a $\delta^{18}O$ decline of less than 1‰ between early and later developing teeth, which is consistent with weaning from breast milk to water. This local range is illustrated by a gray box in Figure 11.3. However, its margins should be interpreted with some uncertainty. This range is only slightly higher than the local range measured for Kaminaljuyu (-5 to -6‰$_{PDB;}$ Valdés and Wright 2004), probably due to the lower elevation at Copan. Local values at Tikal overlap this range but average higher ratios (-3.2 ± 1.2‰$_{PDB}$; Wright unpublished data), while Teotihuacan (-8.3 to -5.4‰$_{PDB}$ converted from phosphate data) and Monte Alban (-10.3 to -8.2‰$_{PDB}$) show much lower $\delta^{18}O$ (White et al. 1998).

Teeth from burials 93-2 (Margarita) and the early-forming teeth from 94-1 (Northern Guardian) clearly fall within the local box for both isotopes. The M3 for 94-1 is lower and approaches the range measured at Teotihuacan, but the skeleton cannot be definitively identified as foreign based on the $\delta^{18}O$ alone. Certainly, the strontium isotopes identify it as a local skeleton. The M3 of one of the decapitated skulls in the Motmot Tomb (37-9) also shows a local value. Burial 92-1 (Teal) is slightly higher in oxygen than the local box, but given the consistent Sr value, and the fact that the M1–M3 difference is that expected for weaning, it may also be a local oxygen value.

Several individuals show considerable change between the early teeth and the M3 in one or both isotopes. From the Motmot Tomb, a second cranium (37-10) shows a local I2 value but is higher in $^{87}Sr/^{86}Sr$ for the M3, implying a move to another locale for adolescence and a return to Copan in later life. The third cranium from this tomb (37-7) is high in both $^{87}Sr/^{86}Sr$ and $\delta^{18}O$ in the I2, with M3 values close to the local box and equivalent to those of 92-1.

For skeleton 95-2 (Hunal), both I1 and M1 values are higher in $^{87}Sr/^{86}Sr$ and $\delta^{18}O$ than the local range and match values for both isotopes measured at Tikal. However, the M3 falls at the margins of the local box and may be consistent with an adolescence spent at Copan while the M3 was forming. Bone $^{87}Sr/^{86}Sr$ reported by Price et al. (2010) is lower than the M3 value. Accordingly, they interpret Hunal's move to Copan as happening after adolescence. For Hunal, the finding of a foreign childhood is significant in that it confirms iconographic evidence that Hunal was a foreign ruler sent to Copan from another Maya polity. Although the data are consistent with an origin at Tikal, and most discussion emphasizes links with Tikal, Price et al. raise the possibility that Hunal may have spent some time during childhood at Caracol because a reference on Copan Stela 63 seems to suggest he came from there (Stuart 2007). Caracol $^{87}Sr/^{86}Sr$ averages are slightly lower than at Tikal; however, we have no $\delta^{18}O$ data from the site.

The skeleton decorated with Tlaloc goggles (95-1) and placed outside the tomb of Hunal's wife shows high $\delta^{18}O$ for both teeth sampled—higher than the Copan box but in the range of central Petén—and a dramatic $^{87}Sr/^{86}Sr$ decline to Copan. The higher $^{87}Sr/^{86}Sr$ for this individual is beyond the range for Tikal and suggests an origin on quaternary soils or very high consumption of marine foods (with $^{87}Sr/^{86}Sr$ of .7092), as found on the north coast of the Yucatán Peninsula.

Skeleton 92-3 (Subjaguar/Ruler 8) shows local $^{87}Sr/^{86}Sr$ in all teeth sampled. However, the $\delta^{18}O$ of his early-forming teeth is 5‰ heavier than that of the M3, which falls slightly below the local Copan box (but approaches the range of Teotihuacan), indicating a dramatically different source of water in early childhood than adolescence, which may well have been spent at Copan. It is difficult to ascertain where these very high I1/M1 $\delta^{18}O$ ratios would match. One possibility is a Caribbean coastal area with volcanic soils, perhaps in Honduras. The strontium isotope ratios rule out Pacific coastal Guatemala, and the limited $\delta^{18}O$ data from that region are also lower than this. Price et al. (2010) discount the oxygen data for this individual as not reliable in view of the very consistent strontium ratio. Note that if the oxygen isotopes are considered, both of the suspected rulers appear not to have been born at Copan. However, little is known of Ruler 8's ancestry or reign (Martin and Grube 2000).

The M3 of the female in Burial 37-8 (Motmot) is also higher in both $^{87}Sr/^{86}Sr$ and $\delta^{18}O$ than local Copan burials, also suggesting a foreign origin, and is consistent with an adolescence spent in the central or northern Maya lowlands. Of the three skulls, only Burial 37-7 shows comparable values, with a foreign incisor value and a local M3 value. Although the strontium data are consistent with Tikal, the oxygen values are probably too high for the central Petén. Very high values are reported for coastal Belize (Metcalf et al. 2009) though not yet adequately published, so this may be a possible homeland for this royal female.

Thus it seems that more than half of the acropolis burials are those of foreigners who came to Copan from several distant places. The higher $^{87}Sr/^{86}Sr$ identifies most of these as being from areas with Cretaceous limestones (95-2, 37-7, 37-8) or perhaps coastal quaternary deposits (95-1). The higher $\delta^{18}O$ of some of these skeletons may also imply a coastal origin, though the values are quite variable and comparative data is weak.

Figure 11.4 illustrates the relationship between carbon and oxygen isotopes in the tooth enamel of the Copan skeletons, including all the data in Table 11.1. Skeletons are distinguished by symbols, and samples are joined together in order of formation, with lines that stop just short of the M3 data point. There is a broad range in both $\delta^{18}O$ and $\delta^{13}C$; the $\delta^{13}C$ variability is both broader and heavier than that reported for bone apatite (-5.5 ± 1.0‰; n = 27) of adult burials from elsewhere at Copan (Gerry 1993). Oddly, the skeletons with enamel $\delta^{13}C$ consistent with this range are primarily those identified by $\delta^{18}O$ or $^{87}Sr/^{86}Sr$ to be migrants (92-3, 95-1, 95-1). By contrast, the "local" skeleton consumed significantly more maize in childhood than the presumably local adults of Gerry's sample. It is not clear if this is an age or social distinction in diet, however, since Gerry's sample includes few elites.

As described earlier, in Figure 11.3, several skeletons show a slight drop in $\delta^{18}O$ between early developing teeth and M3s, generally less than 1‰, a drop that would be consistent with weaning. However, the age at which this shift occurs is quite variable. Some (95-1, 37-7) show little change among the M1 or I2 samples

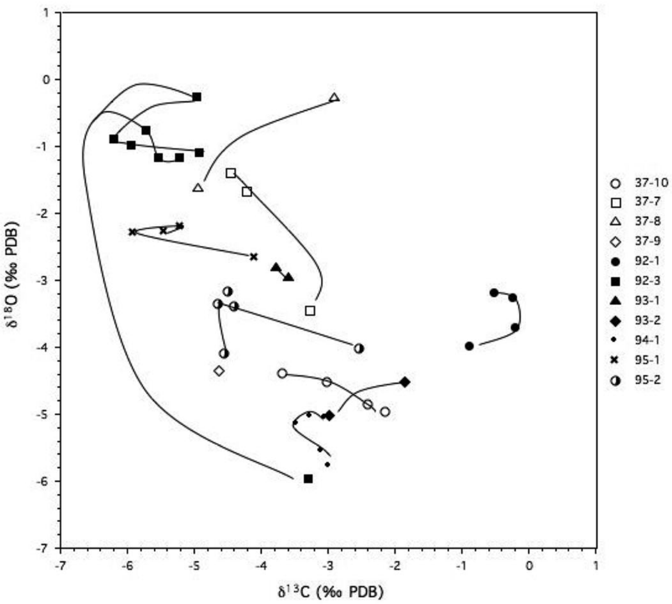

Figure 11.4. Stable oxygen and carbon isotope ratios in tooth enamel carbonate from Copan.

but a larger drop in the M3, while others (92-1, 94-1, 37-10) show greater decline between the mid-coronal and cervical samples of the early teeth, consistent with an earlier weaning age. Interestingly, skeleton 95-2 (Hunal) shows a marked rise between the cuspal and mid-coronal M1 samples, which might be explained if the cuspal sample was formed in utero in this individual and thus reflects the $\delta^{18}O$ of maternal tissues rather than consumption of ^{18}O-enriched breast milk. Given that the samples used in this study presumably average a year of enamel growth or more, seasonality is not likely a huge contributor to this anomaly, though interannual fluctuation in rainfall $\delta^{18}O$ might well be a factor.

In addition to the oxygen isotope changes, carbon isotope changes within and between teeth are variable. Some skeletons show a shift of up to 2‰ in $\delta^{13}C$ between early teeth and the M3, but the direction of this shift is not consistent, indicating quite variable dietary change with age. Among them, several of the migrants (37-7, 37-8, 92-3, 95-2) show quite large $\delta^{13}C$ changes between early teeth and the M3, corresponding to a dietary change that might reflect new dietary customs in the new home, as well as age-related dietary changes. Indeed, the local skeletons show much narrower ranges of $\delta^{13}C$, suggesting less age-related diet change among individuals who remained at Copan throughout childhood.

Implications for Oxygen Isotope Studies and for the Maya

Interpreting the $\delta^{18}O$ values of Copan tooth enamel is much less straightforward than interpreting $^{87}Sr/^{86}Sr$ data. The oxygen data confirm foreign status for several skeletons, especially that of Copan's first ruler, Yax K'uk' Mo' (95-2); a royal woman (37-8); and the Tlaloc Warrior (95-1), as well as a local origin for several others (93-2, 94-1, 92-1). However, $\delta^{18}O$ values conflict with an apparent local status as shown by $^{87}Sr/^{86}Sr$ in all teeth for the Subjaguar skeleton (92-3), which may be that of Copan's eighth ruler. When compared to his M3 values, the divergently heavy $\delta^{18}O$ and light $\delta^{13}C$ ratios of this individual's M1 and I1 suggest a childhood spent elsewhere and a move to Copan between ages four and nine. He may have come from somewhere in the southeastern Maya area, where $^{87}Sr/^{86}Sr$ is comparable to that at Copan but where $\delta^{18}O$ is substantially higher, but it is difficult to imagine where that might be. Alternately, given his high social status, he may have consumed water from a very different source (evaporatively enriched?) than other Copanecos. However, the shift seems far too large to be explained by either seasonality or local climate change at Copan. Little is known epigraphically of rulers 6, 7, and 8, so there is no reason to rule out a foreign origin for this individual (David Stuart, personal communication 2009). While they appear at times to be at odds with the strontium data, the oxygen isotope results draw attention to possible migrants from areas that are geologically similar to Copan. Although the strontium isotope data alone confirm the epigraphic identification of Copan's first ruler as foreign, only the oxygen isotope data raise the possibility that the probable ruler buried in the Subjaguar tomb may also have been foreign. Thus the data reported here both support the epigraphic record that the city's royalty was tied through marriage and migration to distant parts of the Maya area, and perhaps beyond, and shed light on political history during times that are poorly documented in the inscriptions. Although long-distance contacts between Maya cities, and indeed the exchange of elite marriage partners (Molloy and Rathje 1974), have been known since the earliest decipherments of Maya hieroglyphics, isotope studies provide the first means to identify individual migrants and to reveal the life histories of specific skeletons. The case of Copan is especially important because of its well-documented dynastic history and the recovery of so many elite skeletons.

Unlike strontium isotopes, for which local values can be inferred from bedrock geology (Hodell et al. 2004) or from nonhuman faunal samples (Price et al. 2002), local oxygen isotope signatures should be inferred only from human data. The offset between oxygen isotope ratios in bone and imbibed water differs between animal species because it is dependent on body temperature as well as variable contributions from food and physiological effects (Kohn 1996). Price et al. (2010) have observed that $\delta^{18}O$ does not vary in a simple geographic pattern across Mesoamerica. Although altitude and mountain rain shadow effects provide some predictable patterning on a large scale, within the Maya lowlands, local effects, such as reservoir size and recharge, may be more important. Perhaps due to the substantial interannual variability in $\delta^{18}O$ and the short time (months to a few years) represented by each dental sample, Maya sites studied to date show very broad, overlapping ranges. Thus better mapping of variability in $\delta^{18}O$ from human samples across Mesoamerica is needed. Stable oxygen isotope ratios in teeth should be considered a means to identify the outlying values of nonlocal skeletons rather than a means to identify a specific foreign homeland for those outliers. Although the small numbers of commoner skeletons analyzed to date from Copan help constrain the local ratio for the city, they also illustrate significant mobility in lower social classes of this city (Price et al. 2010), and they emphasize the need for large sample sizes.

Together, these Copan data paint a somewhat confusing picture of early childhood diets and water sources. The carbon isotope data indicate considerable variability in childhood diets and hint that childhood diets differed among sites, given that the migrants show different $\delta^{13}C$ ratios than the local children. In addition to broad overlapping $\delta^{18}O$ ranges among sites, the variability seen in $\delta^{18}O$ among samples from a single individual is marked. As yet, we have no way to determine the contribution of seasonality, climate change, or reservoir size and recharge to variability among $\delta^{18}O$ data from a single skeleton. While it is tempting to interpret individual isotope patterns in terms of life history events, such as weaning and migration, many possible factors could result in the erratic patterning of intratooth $\delta^{18}O$ that we see in this small sample. Undoubtedly, these burials are *not* representative of Copan's population at large, or even of Copan's elites, but they do illuminate the variability in water sources and childhood diets that can complicate the interpretation of isotopic life histories from teeth. Obviously, we must analyze larger numbers of skeletons to identify patterning in life histories using tooth enamel. It will be quite challenging to integrate these individual lives back into a populational perspective on ancient Maya bioarchaeology. Future work should emphasize large samples of skeletons from diverse social contexts to first define local ranges and then to identify outliers. Given the significant interannual variability, and the dramatic changes shown here within individual teeth, consistent and precise sampling of enamel is necessary to ensure comparable data.

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12

Mobile Bodies, Empty Spaces

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Jane E. Buikstra contributes fundamentally to bioarchaeological understandings and, indeed, to shaping the conduct of far more than this merged domain of biological anthropology and archaeology. I welcome this opportunity to celebrate our collegial friendship and shared interests in the meanings of space in the lives of peoples of the past and present. Whereas Buikstra examines bodily presence in space, the subject here is bodily absence: places where archaeologists anticipate the presence of interred bodies yet none are encountered. Such instances are often appropriately recognized as reflecting acts of disinterment. But in this exploration, I want to consider a wider range of situations that could lead to absence of bodily remains, focusing specifically on a case at the Classic Maya site of Quirigua.[1]

Attention to spatial aspects of mortuary practices has a long history, with tremendously productive expansion in recent years (e.g., Chesson 2001; Goldstein 2006). Perhaps the developments most pertinent here are an enhanced appreciation for the complexity of burial programs, and conjoined issues of social memory and emplaced social identity. Amid these advances, explanations for absence of anticipated human remains center on three alternatives: (1) that soil conditions were not conducive to bone preservation; (2) that subsequent removal took place in ancient or recent times; or (3) that no corpse had ever been interred.

Just as Buikstra and her colleagues mix science and humanism to infer cosmological meaning from Hopewell earthwork construction (Buikstra et al. 1998), I too seek meaning in the material. In this instance, I draw as well from Parker Pearson's (2002:145) comments: "Many monuments and buildings have a funerary or mortuary purpose even though they contain no human bones or bodies—cenotaphs, war memorials, and ancestor shrines are examples. One of the problems for prehistoric archaeologists is how to recognize the funerary dimension of structures and monuments that contain no corpses or body parts."

Not surprisingly, identifying ancient cenotaphs is seldom straightforward. For example, although the spectacular ship burial in Mound 1, Sutton Hoo, initially appeared to lack human remains, prevailing scholarly opinion now holds that acid soils destroyed the remains of an early-seventh-century East Anglian king (Carver 1992, 1998). Even the inclusiveness of a cenotaph *category* is not without challenge. The label *cenotaph graves* at fifth-millennium Varna is reserved for instances where grave goods are arranged as if accompanying an inferred body; when life-size clay masks stand in for human bodies at the same site, the term *mask graves* is used (Chapman 1996; Taylor 2002). Do the latter not fit the definition of cenotaphs? Where do the decedents' remains rest? At Abydos, Egypt, scholars debate which among chambers lacking bodies are "dummy tombs," which are chapels, and for which the term *cenotaph* is appropriate (e.g., Aldred 1984; O'Connor 1998, 2009; Wilkinson 2000). In short, for all these cases, what evidence suffices to infer a "funerary dimension"?

In studies of the pre-Columbian Maya, too, neither the presence nor absence of human remains suffices alone for imputing ancient intent. At Tikal, for example, investigators have grappled for decades with the ambiguity in distinguishing "burials" (that is, deliberate human interments) from "caches" (customarily interment of objects other than human remains). Becker (1992) suggested that all constitute a continuum of expressive forms and considers them alternative expressions of offerings to the earth. Building from his work, Bell (2007) proposes formalizing the neutral term *placed deposit* to encompass this range, leaving inference of meaning to detailed analysis. Among the range of documented Maya placed deposits, Freidel and Guenther (2006) recently reviewed instances they believe are cenotaphs. That is, where royal burials were anticipated, excavation either encountered none or encountered the expected decedent in a separate location. In these cases, the authors highlight particular royal accoutrements—especially jade earflares—as metonyms for corporeal remains, and they consider lidded pottery vessels potential emblems of "soul caches" (compare Houston, Stuart, and Taube 2006; Houston and Taube 2000). Even if the metonyms and lidded containers are adequate and reliable indicators of Maya funerary acts, however, they would seem unlikely to exhaust the options for such expression.[2]

Quirigua SD 6 and Classic Maya Mortuary Ritual

Let me now introduce the crypt whose emptiness prompted this paper topic (Figure 12.1). The chamber in question—Special Deposit (SD) 6—was encountered in 1976 excavations at Structure 3C-2, an earthen mound at the Classic Maya site of Quirigua, in a compound adjoining one of the civic center's two large public plazas (Ashmore 2007; Sharer et al. 1979). Structure 3C-2 was likely part of a domestic compound, found through subsequent research to overlie early civic architecture, masonry construction that Structure 3C-2 postdated by probably more than a century. Despite clear stratigraphic disjuncture, this observation hints further at importance for the location and at special social standing for its occupants.

When the capstones of SD 6 were removed, the chamber's observed contents comprised three pottery vessels (Figure 12.2). The size, form, and construction style of the chamber make it a close counterpart to contemporary burial crypts known at nearby Copan and other settlements in and beyond the region.[3] But to our great surprise at that time, no human remains were encountered. No trace of secondary removal was evident, and preservation elsewhere at Quirigua argues against simple disintegration (e.g., Ashmore 2007; Jones

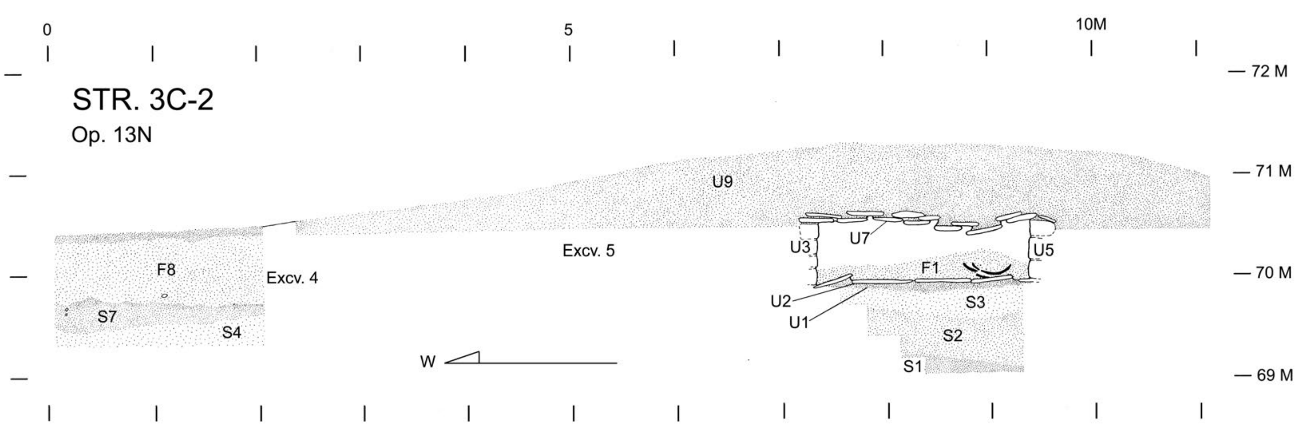

Figure 12.1. Structure 3C-2 excavation section (reproduced by permission of the University of Pennsylvania Museum, from Ashmore 2007:CD Figure 3C.3).

et al. 1983; Sharer et al. 1979). Why, then, would local residents, presumably members of the same family group, have constructed and stocked this relatively elaborate chamber if no corpse were to be interred?

To address that question, let me return to Buikstra, specifically to her critiques of a one-stage "mortuary paradigm." In collaboration with Rakita, she argues that mummification is not necessarily the mechanical step Hertz had envisioned in 1907, one completing the physical processing of corpses. Instead, Rakita and Buikstra suggest that Chiribaya mummies mark a stage in an *extended* mortuary program, whose aim and consequence is to "bridge in an uninterrupted way the divide between the world of the living and the afterworld and continue to structure the lives of [the] descendants" (Rakita and Buikstra 2005:106).

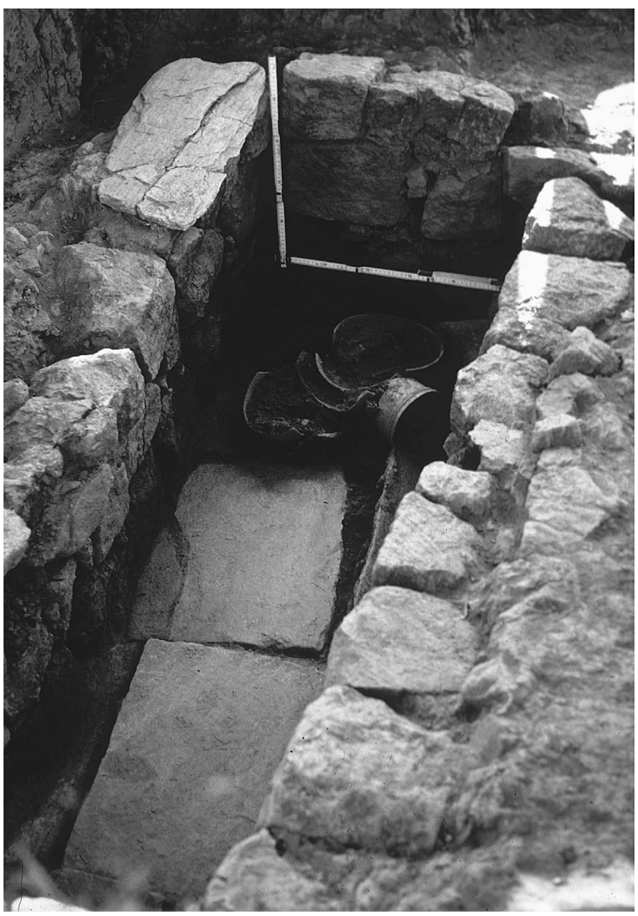

Figure 12.2. Quirigua SD 6 crypt with three vessels, from the southwest. Chamber interior 2 m by .7 m by .6 m (reproduced by permission of the University of Pennsylvania Museum, after Sharer et al. 1979:Figure 9c).

Maya Mortuary Belief and Practice

Returning to the ancient Maya, mortuary programs most commonly tied decedents to houses or shrines. And the interment act related simultaneously to passage of the decedent beyond the mundane world of the living and to continuity of the worldly domestic unit, the social house, or the polity (e.g., Geller 2004, 2006; Gillespie 2002).

Drawing from differing but complementary evidence, Fitzsimmons (2002) and Geller (2004) have detailed the intricate processes that most plausibly attended ancient Maya death, mourning, departure of multiple souls, and—in all—social transformation of both the decedent and the living. Notably, these diverse processes and stages potentially extended almost indefinitely in time, material evidence for which includes reentry of burial chambers and frequent removal of select elements of the skeletal remains. At least for royal cases, hieroglyphic texts sometimes explicitly attest to one or more liminal periods separating death, interment, and ritual commemoration of the death, the stated intervals ranging from three days to an extraordinary span slightly exceeding 24 years (e.g., Fitzsimmons 1998; McAnany 1998; Reese-Taylor et al. 2006). These varied spans supported ritual, social, and physical transformations of the corpse, souls, and mourners. We turn now to the location of the decedent while the transformations occurred.

Mobile Body Elements

Particularly important here are material punctuations bounding liminal periods: Maya customs of reentry and removal insofar as these acts relate to empty or emptied chambers. Many documented instances involved multiple, sequential interments, widely viewed as "additive" acts reaffirming social continuities. Such entries are documented both in relatively simple, small-scale interments and in elaborate crypts and tombs (e.g., Bell 2002, 2007; Chase and Chase 1996; Geller 2004; McAnany 1998).

In other cases, however, reentry allowed *removal* of body parts. Fitzsimmons (2002:368) evocatively describes the situation at Caracol as one "where skeletal remains [were] fluid in their transport over the landscape." Across the Maya world, femora, fingers, and especially skulls or faces were those elements removed with greatest frequency. Whether at Caracol or elsewhere, and especially for members of the royalty, the elements removed could be kept as "heirlooms," worn in attire of descendants, and displayed and handled during rites of ancestor veneration or earth offerings (e.g., Becker 1992; Fitzsimmons 1998; Joyce 2000; McAnany 1995; Mock 1998).

Alternately, elements could be removed before and separately from an individual's death, or held in reserve at the time of interment. "Reserving" skulls and face elements would necessarily accompany or follow death; these elements were particularly potent possessions, whether removed in reverence or triumphal capture (Duncan 2005). Epigraphy, iconography, and ethnohistoric sources indicate the paramount significance of the human head, including its standing for the person as a whole (Houston, Stuart, and Taube 2006). Other body parts held strong meaning and could mark antemortem acts. Danien (1989), for example, describes ethnographic accounts of Ixil Maya mothers severing one of their own fingers at the death of a child. Whether or not reflecting such practices in antiquity, what Chase and Chase (1998) call finger caches are numerous enough in their occurrence to be a recognized category of ritual deposition at Late Classic Caracol.

Maintaining connections between the dead and the living was critical to centering ancient Maya society at multiple scales, from house to polity. Increasingly, however, Mayanist scholars recognize that intact bodies were only one reference point for contact and *that their presence was not an absolute requirement*. Body parts, especially but not solely skulls and faces, were powerful material remains for curation and display, to venerate ancestors and as demonstration of social continuity (e.g., Freidel and Schele 1989; Joyce 1998, 2000; McAnany 1998). It becomes increasingly clear, however, that *nonmaterial* remains were equally important, especially the heritable and conjurable names, titles, and *souls* of the deceased (Geller 2004; Gillespie 2002; Taube 2004). The locale of such recognition can materialize the memorialized (Hendon 2000:50). For the Maya, discussions of soul caches certainly come to mind here (Freidel and Guenther 2006; Houston, Stuart, and Taube 2006). If and when the body or its individual elements were unavailable, the deceased could still be acknowledged. Moreover, if the lack of a corpse were due to capture or other violence, the crypt's material recognition as a place for connection and recollection could assume added importance (e.g., Duncan 2005; Metcalf and Huntington 1991). Recalling Parker Pearson's words, cenotaphs, war memorials, and ancestor shrines exemplify commemoration in the absence of bodily remains; from this observation, he urges that archaeologists more often allow for the possibility of such monuments.

Cenotaph commemoration has no single material form, and among the ancient Maya it need not take the specific form outlined earlier. For example, consider the case of Copan's thirteenth ruler, Waxaklahun-Ubah-K'awil. Classic Maya texts indicate his death in A.D. 738 at the hands of the subordinate lord of neighboring Quirigua. Although archaeologists have documented an impressive set of royal tombs at Copan, that of Waxaklahun-Ubah-K'awil has not been found. His remains might lie undetected in areas untouched by excavation. Or it might be that his death at Quirigua deprived heirs and subjects of his formal interment. Half a century later, however, the sixteenth Copan king, Yax Pasaj Chan Yopaat, acknowledged his revered if unfortunate predecessor—his dynastic ancestor—to a degree that some have characterized as near obsession (Stuart 1992). The later sovereign likely commissioned the imposing compound where the earlier king was celebrated in ritual acts and permanent architectonic display of a text bearing his name, royal title, and a date plausibly marking an anniversary of his death (Ashmore 1991; cf. Fitzsimmons 1998; Houston and McAnany 2003; McAnany 1998). That architectural compound, now known prosaically as Copan Groups 8L-10 and 8L-12, ultimately might have sheltered remains of his skull, plausibly returned by later, repacified lords of Quirigua (Figure 12.3) (Ashmore 1991; Ashmore and Geller 2005; Carrelli 1990). At least until that point, however, his memory and acknowledgment had to be maintained in other ways.

QUIRIGUA CENOTAPH

This returns discussion to SD 6, the apparent cenotaph at Quirigua. In contrast to theoretical inferences at the time of field research in 1976, it now seems highly likely that the chamber was material commemoration for an individual whose bodily remains were wholly unavailable for interment. The pottery in the undisturbed crypt tentatively places its construction in the seventh or early eighth century A.D., and pottery types, together with masonry construction and the chamber's location adjacent to a long-standing civic arena, suggest that the decedent was a prominent member of an important Quirigua family (Ashmore 2007).

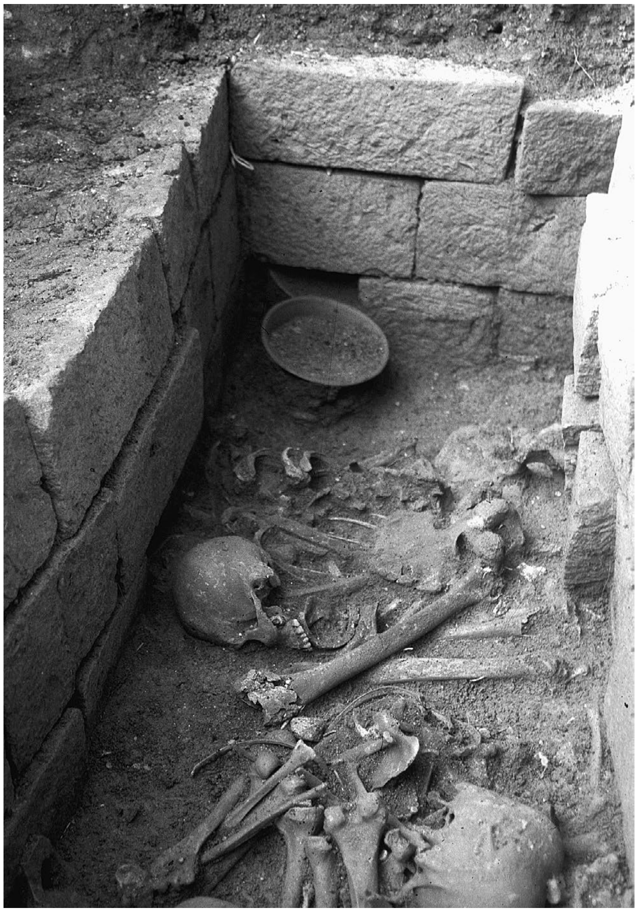

Figure 12.3. Copan burial XLII-5, Group 8L-10. Cranial fragments were encountered in a vessel in the southeastern corner, at upper left. Chamber interior 1.7 m by .75 m by .6 m (photo by author).

Figure 12.4. Vessel 13N/48-1 from Quirigua Structure 3C-2, SD 2. Height: 8.9 cm; diameter: 15.0 cm (photo by author).

More precisely, the style of one of the three vessels suggests high, if not royal, social and political standing for the person commemorated (Figure 12.4). Although location of the chamber and the structure housing it is beyond the likely physical extent of Quirigua's royal court, at least one of the vessels suggests a gift from that court or perhaps directly from overlords in the Copan court (e.g., Houston, Stuart, and Taube 1992; LeCount 1999). Of the three simple white vessels of SD 6, one was a low cylinder (Ashmore 2007). Not only is this type of cylinder rare in the Quirigua assemblage and in its counterpart version at Copan (Bullard and Sharer 2004; Willey et al. 1994), but also this particular vessel is noteworthy for its potential political message.

The relatively squat cylinder rests on *tau*-shaped feet, recalling distinctive fifth- and sixth-century cylinders emblematic of ties with the great central Mexican city of Teotihuacan (e.g., Reents-Budel et al. 2004; Sharer and Traxler 2006). Such pottery modes are now well-known from a small set of Early Classic Maya capitals, each with further material reference to Teotihuacan—especially its fourth- and fifth-century shaping of dynastic history at Tikal and Copan (Sharer et al. 2005; Stuart 2000; Taube 2004). Copan, in turn, oversaw fifth-century establishment of the Quirigua dynasty, retaining sovereignty over Quirigua until the aforementioned rebellion of A.D. 738. In the interim, prestige goods, as well as utilitarian items and tribute, likely moved between the two capitals and their support populations (e.g., Schortman et al. 2001). Chemistry identifies at least six vessels in a sixth-century royal tomb at Copan as "likely made in workshops at or near Quirigua" (Reents-Budet et al. 2004:185). Lacking chemical signatures for the SD 6 cylinder, its association with upper echelons of Quirigua-Copan society is nonetheless likely, and its role as a gift recognizing an important local person seems probable.[4]

Putative commemoration in SD 6 clearly implies considerable investment of energy and materials. Resultant archaeological traces mark only one episode in what was plausibly a more extended mortuary program. They do so in a manner that we can now understand as materializing the social memory and identity of the decedent, respectful mourners, and determined celebrants of the continued life of the person, the family, and the intricate web of sociopolitical relations of which they were part.

So a puzzle at the time of its excavation has now acquired at least tentative explanation. It is precisely the mobility of Maya bodies that can explain the empty spaces of the chapter's title. That inference is due in significant measure to new thinking and new findings within studies of the Maya past. Its relevance to this volume is the broader set of advances in understanding mortuary practices and the spaces and places of the dead, domains we collectively comprehend better in large measure because of Jane E. Buikstra's work. As the volume editors suggest, the dead *do* tell tales. And it is to Buikstra's skills in reading and translating those tales that we owe much of our ability to learn from them.

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Notes

1. The Classic period is here defined as circa A.D. 250–950. Pertinent divisions within it are Early Classic (circa A.D. 250–600) and Late Classic (circa A.D. 600–950).

2. An empty chamber at fifth-century Copan, for example, suggests relation to funerary activities at the heart of the early dynastic center, but it remains unclear whether the distinctly smoke-smudged chamber remained completely unused, was emptied of placed deposits, or was a setting for subterranean rituals connected with the nearby tomb of the dynasty's founder (Bell 2007:312; Sedat and López 2004:94–95; Sharer et al. 2005:166; Taube 2004).

3. Only two formal interments were encountered in Quirigua Project research (1975–1979). While neither resembled SD 6, the latter finds close counterparts at Guaytan (Smith and Kidder 1943) and among the hundreds of interments documented at Copan (e.g., Bell et al. 2004; Longyear 1952).

4. Three points about the shape, decoration, and number of supports on the SD 6 cylinder invite further comment. First, while the significance of their *tau* shape and incised design is not certain, it is certainly reminiscent of the Maya glyph *ik'*, which refers to "wind" or "breath" (e.g., Houston, Stuart, and Taube 2006:142–150). The latter is associated strongly with the living soul in Mesoamerica, death being the termination of both breath and soul (Freidel and Guenther 2006; Houston and Taube 2000:267; Houston, Stuart, and Taube 2006:142; Taube 2001:105). For the Maya, wind and the breath soul are identified further with quintessentially precious new green entities—"jade jewels, flowers, and even the occasional green quetzal plume"; moreover, in Maya texts, the "*ik'* [breath] sign . . . typically occurs with the floral sign in the death expression" (Houston, Stuart, and Taube 2006:147).

The second point is that the number of supports on this vessel is unusual; pottery cylinders in Mesoamerica during this time are customarily tripods (cf. Bell 2007; Bullard and Sharer 2004; Longyear 1952; Willey et al. 1994). The unusual tetrapod set on this vessel might represent the four world directions, a pervasive idea with varied iconic expression (Coggins 1980). The distinctive markings, however, suggest intentional choice; I propose that the four *tau* supports of the SD 6 cylinder are meant as *ik'* signs.

This leads to a third point, positing that the vessel design was explicitly part of courtly Maya funerary expression. Taube's remarks about a different Classic Maya context, the text of Naranjo Altar 1, may be pertinent as analog; he calls attention to a "complex glyph composed of four *ik'* signs surrounding a sky glyph, representing the four celestial winds. On closer inspection, it can be seen that these signs are actually the earspools of the wind god" (Taube 2001:109). In other cases, *ik'* signs (including sets of four) adjoin iconic images of flowers, indicating their "breathlike fragrance," and Classic Maya earspools (or earflares), especially those crafted in jade, are material icons for flowers (Houston, Stuart, and Taube 2006:147; Taube 2001:108–109). This recalls Freidel and Guenther's (2006) contention that earspools served as body metonyms in royal Maya cenotaphs. The apparent conjunction of referents—putatively to breath (*ik'*) and thereby implicitly to souls, flowers, and possibly earspools—allows at least tentative inference that this SD 6 vessel owes its form to courtly Maya mortuary expression.

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13

Andean Life Transitions and Gender Perceptions in the Past:

A Bioarchaeological Approach Among the Pre-Inca Chiribaya of Southern Peru

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Life stages represent socially and biologically constructed phases during the life span of an individual. The definition of individual stages, and the criteria for transition between them, is culturally defined by societies and often perceived through the lens of biological changes. The result is context-specific age classifications that reflect important elements of the societies that create them. An understanding of such life transitions can serve as a window into the roles and status given to individual members of society, or more broadly to society as a whole (Bernardi 1985; Stewart 1977). Within the studies of life cycles, a very important dimension is the analysis and interpretation of gender (Nemerowicz 1979). As with social age, gender has a socially constructed dimension that often dictates the roles that men and women are to fulfill in life. These gender roles are malleable and in many contexts interwoven with chronological age, social age, biological transitions, and cultural ideals. Despite the prevalence of gender studies, few researchers have examined the relationship between age and gender from past societies using archaeological contexts (Effros 1999; Sofaer 1997).

During the past several decades, mortuary archaeology has been dominated by research focused on the reconstruction of social structure, beliefs about the dead, and treatment of the body (Rakita and Buikstra 2005). In a similar vein, bioarchaeological and mortuary research, interpreted within the context of ethnohistorical data, can be used to understand life cycles and gender construction in the past. These studies may help answer key questions regarding the way societies structured their communities and enforced gender roles.

We are quite fortunate in the Andes to have a wealth of detailed ethnohistorical accounts written during and after the Spanish conquest of Peru that comment on social age and gender construction in precolonial Andean societies. Of course, historical narratives from single authors may result in biased accounts; however, colonial administrative documents and censuses from the Andes appear to be less biased and provide more objective testimonies that may be used to frame individual accounts (Dean 2001). All together, these documents are a valuable source of information that can be used to produce more contextualized models of analysis for the archaeological interpretation of past societies.

To evaluate issues of social age and gender, we have examined the pre-Hispanic Chiribaya society of southern Peru. This coastal group centralized its power on the southern coasts of Peru in approximately A.D. 700–1200 (Figure 13.1). Our previous research demonstrated that the group was a large sociopolitical unit that consisted of economically specialized communities of farmers and fishermen under the authority of a

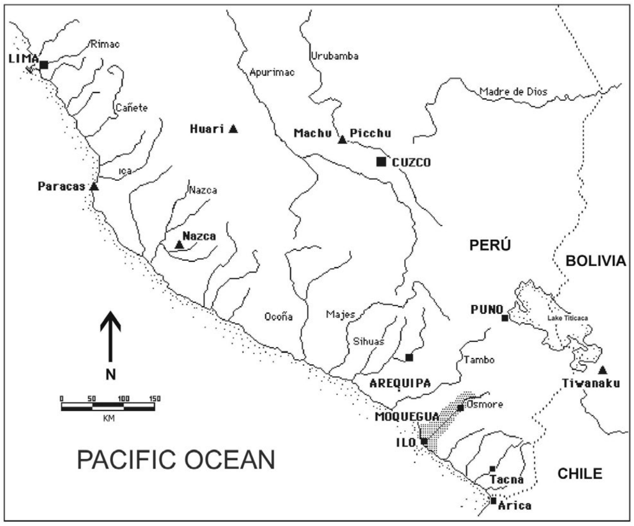

Figure 13.1. Location of the Osmore drainage in southern Peru.

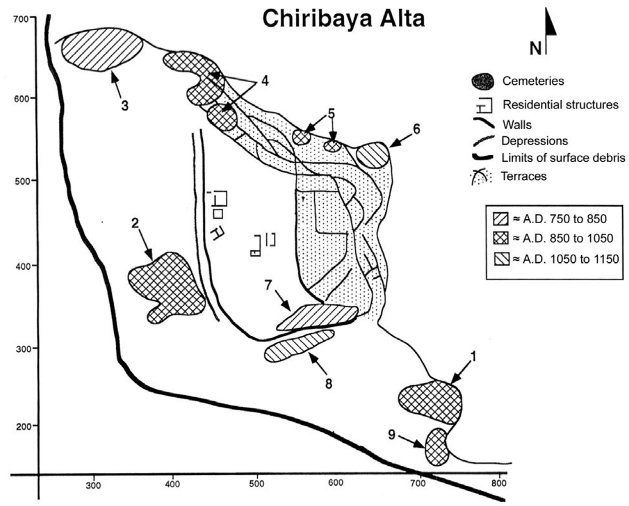

Figure 13.2. Chiribaya Alta, the Chiribaya capital in southern Peru.

paramount lord (Lozada and Buikstra 2002). This type of sociopolitical organization echoes ethnohistorical models of political economy as proposed by Peruvian historian Maria Rostoworowski de Diez Canseco and a number of other Andean scholars. This extensive bioarchaeological study incorporated mortuary data, biological distance studies, analysis of cranial modification styles, and dietary patterns. The fishing community symbolized its group identity by adopting the circumferential type of cranial modification, distinct from the fronto-occipital style used by the agrarian farmers. The cultural differences between fishers and farmers were codified in the body and were reinforced by distinctive dietary patterns, as well as the overall mortuary program (Lozada and Buikstra 2002). For instance, inland communities of farmers consumed more agricultural products, while the fishers' diet was based mostly on marine resources (Tomczak 2003). In addition, the fishers lived close to the shores of the Pacific Ocean and were buried with their fishing tools, while the farmers were excavated from their cemeteries within their communities in the valley.

As stated above, we were interested in studying life transitions and gender construction in this pre-Inca society. With this goal in mind, we examined a collection of 234 individual burials from Chiribaya Alta (Figure 13.2). Due to its size and complexity, the site has been characterized as the center of political power in this coastal society in southern Peru. The mortuary assemblages are extensive, and there is excellent preservation of both organic and inorganic material. The site was excavated by the Chiribaya Project, which was directed by Jane E. Buikstra, with detailed documentation for each of the mortuary contexts.

Through a complete analysis of this collection of burials, we sought to address two main questions:

1. Can material culture be used to identify key transitions in the life of the Chiribaya peoples?
2. Can grave goods aid in the identification of gender construction and/or gender roles in this society?

As mentioned above, life cycles are often related to specific biological or social transitions during the course of an individual's life. The Andean chronicler Guaman Poma de Ayala, born immediately after the conquest of Peru, circa 1550, describes the categories into which the Inca population was divided in both graphic depictions and short narratives. His descriptions are based on Inca census categories called *calles*, or "paths of life." There were 20 total—10 for males and 10 for females (Guaman Poma de Ayala 1988 [c. 1615]:179–189, 201–209). More importantly for our study, these categories help define the features that distinguish one age category from another, both among males and females. As noted by several specialists, these groups do not follow a clear chronological sequence. In fact, Poma's first category is composed of those individuals who contribute the most to the community, either socially or biologically. This group includes males and females between 33 and 50 years, and this classification appears to have been determined in large part by either a person's gender-specific economic activity or by key biological events. In the Inca system, life stages were not marked solely by chronological years but by changes in the functional status of individuals, resulting in a more fluid and less regimented system of age and gender categories. Tables 13.1 and 13.2 summarize the division of women and men by age groups and tasks by this chronicler.

As can be seen from the tables and descriptions, the Inca system of population classification for girls and boys under five years, when children were considered to be dependent on their parents, was mostly related to biological changes. Gender-specific roles appear to develop in Calle 8, after five years of age, when individuals were expected to contribute to their households and society at large. However, Poma depicted baby girls and baby boys differently in all the *calles*, indicating that sexual biological distinctions were very much recognized at birth and remained important throughout an individual's life.

The remaining categories were mostly based on physical activities and duties within specific gender spheres. For instance, females were involved in the production of textiles throughout most of their lives, even if they were sick or old. Females were also responsible for the production of an Andean beer called *chicha*. On the other hand, males had multiple roles, mostly outside the household realm. From Poma's depictions, males were responsible for hunting, herding, warfare,

Table 13.1. Male Life Age Categories According to Poma de Ayala.

Calle Number	**Age or Status**	**Definition of Group**
1	33–50 years	Selected group of warriors and active men
2	60 years	Old people who were not obliged to pay tribute but were still able to contribute to agricultural activities
3	> 80 years	Individuals who were inactive due to old age and sickness. If possible, they took care of small animals such as guinea pigs and ducks.
4	With a handicap or birth defect or very sick	Males, young or old, who were handicapped or sick and thus not able to work
5	18–20 years	Males who worked as messengers, took care of animals, and assisted warriors and lords
6	12–18 years	Young males who took care of animal herds, caught birds, and selected feathers for royal garments
7	9–12 years	Male children who were taught to hunt small birds, herd animals, dry meat, and cure animal skins
8	5–9 years	Children who helped their parents with household chores and the care of younger siblings
9	1–5 years	Unweaned children who could crawl
10	Just born	Breast-fed infants under the care of mothers

Table 13.2. Female Life Age Categories According to Poma de Ayala.

Calle Number	**Age or Status**	**Definition of Group**
1	33 years	Married and widowed women, responsible for the production of fine textiles for nobles and the production of ordinary textiles for the community
2	50 years	Old woman responsible for the production of ordinary textiles for the community. They also assisted in the household.
3	> 80 years	Old women who mostly slept and ate. If possible, they produced textiles for the community and assisted in the household.
4	With a handicap or birth defect or very sick	Women with birth defects or disabilities. They were expected to reproduce and if possible produce textiles.
5	30 years	Virgin women who married warriors. They were asked to participate in rituals for the sun and temples.
6	12–18 years	Young women who helped their parents and learned to produce textiles while assisting noblewomen. They took care of animals and helped with the production of chicha.
7	9–12 years	Girls who collected flowers and terrestrial and aquatic plants. They prepared dye from the flowers to dye threads for textile production.
8	5–9 years	Girls who helped their parents with household activities and assisted with the care of younger siblings, the production of chicha, and spinning
9	1–2 years	Girls who crawled
10	Just born	Breast-fed infants taken care of by their mothers

and agricultural activities. These life stages among the Inca of precolonial Peru were based on specific tasks and contributions to society, not on strict chronological categories (Rowe 1958). In addition, there are some indications that this system had its roots in pre-Inca populations, which is helpful for our analysis of the Chiribaya (McEwan 2006).

As mentioned by many chroniclers, pre-Hispanic Andean societies routinely buried the deceased with their own belongings, such as their tools of production and other personal items (Cieza de León 1922 [1553]). With this in mind, an analysis of grave goods vis-à-vis the skeletal age of an individual may help identify the life cycles described by Poma. To assess this hypothesis, all Chiribaya skeletons were aged and sexed following standard procedures in physical anthropology (Buikstra and Ubelaker 1994).

Burial items were classified into 1 of the 15 categories that incorporated all types of materials buried with individuals. The presence or absence of each artifact type was registered for each burial. These categories included typical Andean accoutrements found in mortuary contexts, such as bowls, cups, containers for water or *chicha*, pots, *keros* (ritual drinking cups), pitchers, wooden spoons, baskets, textile bags, corn, musical instruments, guinea pigs, gourds, small model boats, and textile tools.

It was possible to identify roughly 8 of the 10 categories described by Poma within the Chiribaya Alta skeletal collection. Calle 4, which included the disabled and individuals with birth defects, was excluded, as none of the skeletons exhibited any osteological evidence of such conditions. We recognize that individuals within this *calle* may have existed within Chiribaya society; however, archaeologically and osteologically this category is extremely difficult to identify, and notions of disability have to be carefully contextualized and analyzed. In addition, calle 5 and calle 1 were combined, since in Guaman Poma's system there is some overlap in the description of the roles of both men and women within these stages of life. A total of 234 skeletons were included in this study, and Table 13.3 illustrates the number of individuals by category.

In the following analyses, males, females, and individuals with undetermined sex were combined and placed into one of the above categories. Differences in associated artifacts between age categories were explored statistically using Fisher's exact test, using a bootstrapping technique to calculate significance. This statistical analysis did not reveal any correlations between types of funerary goods and specific age groups.

Table 13.3. Chiribaya and Poma de Ayala Age Categories.

Chiribaya Age Group	Chiribaya Sample	Guaman Poma's *Calle*
< 1 year	27	10
2–5 years	45	9
6–9 years	19	8
10–12 years	8	7
13–18 years	16	6
19–33 years	53	5 and 1
34–50 years	55	2
50 +	11	3

Although material culture does not appear to have reflected differences in a Chiribaya individual's life cycle, we found interesting differences between the specific age categories and the manner of burial. As illustrated in Figure 13.3, the dominant mortuary pattern among children less than six years was burial in urns. These clay containers were excavated from most of the Chiribaya Alta cemeteries; however, we found no examples of adults buried within these urns.

Apart from his graphic depictions, Guaman Poma did not discuss age categories in much detail. However, in her study regarding children in the Andean colonial period, Caroline Dean indicates that one of the most important periods in the life of an individual was when he or she was weaned and became less dependent on society both physically and socially (Dean 2001:44). In addition, current ethnographic studies in the Andes specific to children suggest that girls and boys between five and six years of age go through an important ritual that marks their social transition into the community. For instance, they are baptized in the traditional Andean sense, in a ceremony called *rutuchikuy*, by having their hair cut for the first time. For this ceremony, the hair is braided and each of the braids has a godfather and a godmother. In addition to monetary gifts, parents give children their own animals and tools of production to help ensure economic integration into the household (Isbell 1997; Portugal Catacora 1988). In sum, while grave goods do not seem to reflect life cycle stages among the Chiribaya, the very important integration of children into society is reflected by the manner in which they were treated at death; children older than six were buried following the ritual used for adults, thus integrating them into the realm of adulthood and productivity.

Although current ethnographic observations and historical records do not describe the use of urns for

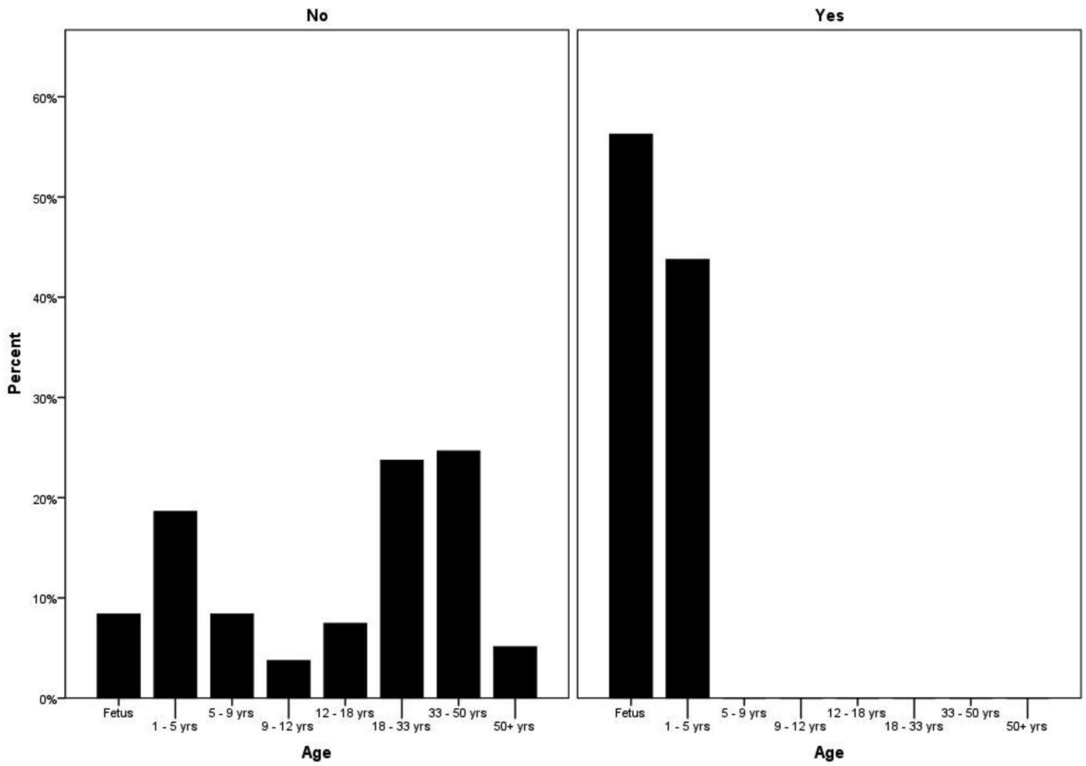

Figure 13.3. Urn burial practice by skeletal age.

the burials of children, we propose that these round clay containers, in which children were buried in a fetal position, can be seen as conceptual "wombs." In fact, in Guaman Poma's chronicles, the first five years of a child's life are characterized as periods without purpose or usefulness, and this idea may parallel the perception of children below the age of five as social "fetuses" who are entirely dependent on their immediate families and larger communities for their social, if not biological, livelihood. Altogether, this Chiribaya study highlights the importance of a multidisciplinary approach to the study of life cycles in pre-Inca societies. These patterns suggest that children were expected to transition to adulthood around age five or six. Although these important events in the lives of Andean people have been documented in contemporary and colonial studies (Dean 2001; Portugal Catacora 1988), this study shows similar ideologies regarding the development children and perceptions of adulthood among the coastal Chiribaya society in southern Peru.

As stated earlier, life cycle transitions are often interwoven with notions of gender. Some of the pioneering research in this area of study has taken place in the Andes, including studies that incorporate ethnohistorical and archaeological data. Researchers such as Joan Gero (2001) and Carolyn Dean (2001), for example, have proposed new methodologies and interpretations about gender in the prehistoric Andes, while researchers such as Isbell (1976) and Harris (1980) stress the dualistic nature of gender constructions, in which one gender is only meaningful when viewed relative to its complement. Furthermore, ethnographers in the Andes have documented the profound importance of gender opposition in indigenous Andean society and link gender differentiation to a person's biological sex. As with the first part of this paper, I would argue that bioarchaeological analyses allow researchers to examine specific nuances of gender behavior in past societies that might not be accessible otherwise.

As noted above, Guaman Poma de Ayala depicts specific roles that both males and females were expected to have as early as five years old, when a child engaged in household chores and became an adult and a "productive" member of society. According to Poma and paralleling modern ethnographies, females produced textiles throughout their lives. As stated earlier, they were also responsible for cooking and the production of *chicha*. Based on these descriptions, researchers have proposed that women kept their work within the household sphere (Dean 2001).

Table 13.4. Association Between Chiribaya Grave Goods and Skeletal Sex.

Artifact Class	Female (n = 73) % Present	Male (n = 51) % Present	Significant P-Value
Bowl	53.4	54.9	
Cup	1.4	39	
Water container	15.1	23.5	
Cooking pot	2.7	0	
Kero	**4.1**	**25.5**	**.006**
Pitcher	42.5	33.3	
Wooden spoon	42.5	43.1	
Basket	31.5	33.3	
Textile bag	26.0	37.3	
Corn	27.4	21.6	
Musical instrument	**1.4**	**15.7**	**.04**
Guinea pig	20.6	15.7	
Miniature boat	15.1	19.6	
Gourd	43.8	43.1	
Textile tool	**34.3**	**3.9**	**< .0001**

On the other hand, men were hunters, herders, and *chasquis* (official carriers) and were involved in warfare. In contrast to women in pre-Hispanic Peru, males ventured abroad, although as "seniors" they might help with some household chores. Can our analysis of funerary goods provide information regarding the gender roles described by Poma? To address this question, we conducted additional statistical tests to see if an item or a group of items was closely associated with either skeletal sex category. In this part of the study, we included only individuals whose skeletal sex was definitive: 73 females and 51 males. Using the same statistical method described in the analysis of age categories, differences in artifact frequency between males and females were explored for the 15 artifact classes. As can be seen in Table 13.4, statistical analysis revealed significant differences between males and females in three types of items.

Male individuals were associated with *keros* and musical instruments, while females were associated with weaving implements (Figures 13.4 and 13.5). These findings are in line with some of Poma's depictions of certain male and female activities in ancient Peru and reflect widespread burial practices throughout the Andes that incorporated gender-specific tasks and identity.

These findings suggest that there were gender-specific activities for males and females among the Chiribaya and that these gender distinctions were represented by the inclusion of certain artifacts in mortuary assemblages. In the Andes, *chicha* is a highly symbolic beverage mostly prepared by women. Among the Chiribaya, its consumption appears to have been exclusive to men, as manifested by the presence of *keros* in male contexts. In pre-Hispanic Peru, this alcoholic beverage, made from corn or *molle* (*Schinus molle*), was used for rituals and feasting both at the community and domestic levels (Bauer 1996; Goldstein 2003; Goldstein et al., 2009; Valdez 2006).

While Poma does not describe the nature of musicians in his work, ethnographic records in the Andes indicate that the use of musical instruments was associated with men (Harris 1980, Stobart 2008). Furthermore, in other archaeological contexts, such as Sipan in northern Peru, musical instruments are exclusively associated with men (Alva and Donnan 1993). Finally, the production of textiles in the household was a female task in pre-Columbian Peru and remains so in the current Andean household. This role seems to have been equally prevalent among Chiribaya women. These patterns of mortuary offerings are highlighted in the burial of a Chiribaya lord accompanied by two adult women. The lord was buried with *keros*, weapons, and musical instruments. In contrast, both females were interred with a vast number of textile implements (Lozada et al. 2009). It is key that we see the same fundamental gender distinctions between males and females in burials across all social strata of Chiribaya society.

Botanical studies by John Dendy (1991) add an additional perspective on the gender distinctions among the burials of Chiribaya Alta. For instance, *lucuma* (*Pouteria lucuma*), an indigenous Andean fruit, was more frequent in female burials. In the Andes, chroniclers document that the fruit was linked with women's fertility. It is said

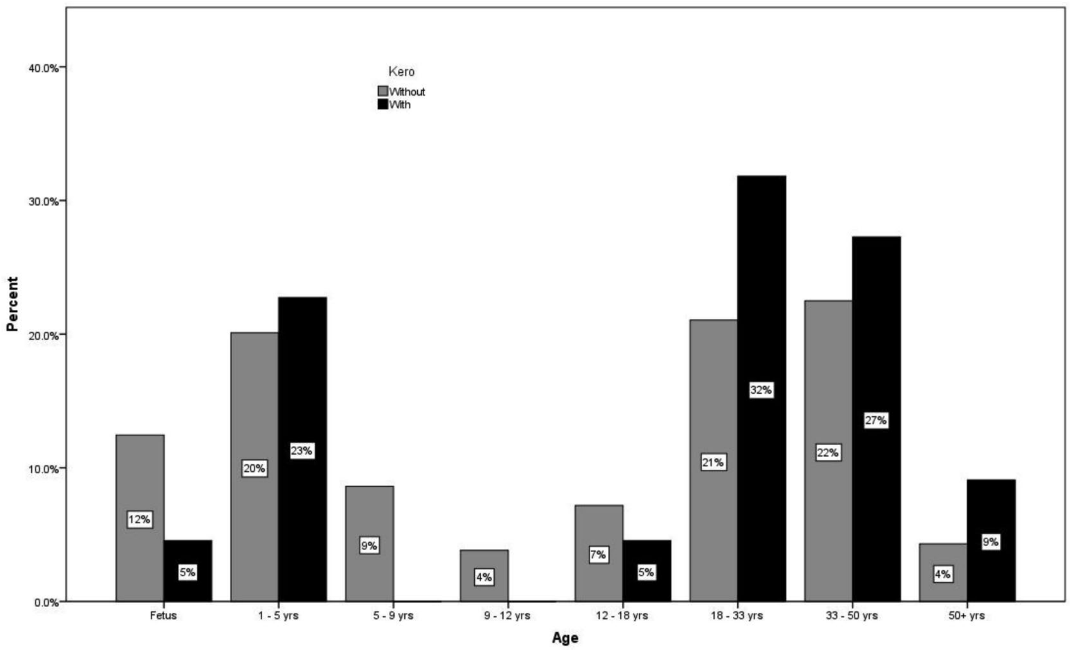

Figure 13.4. Association between keros and age categories.

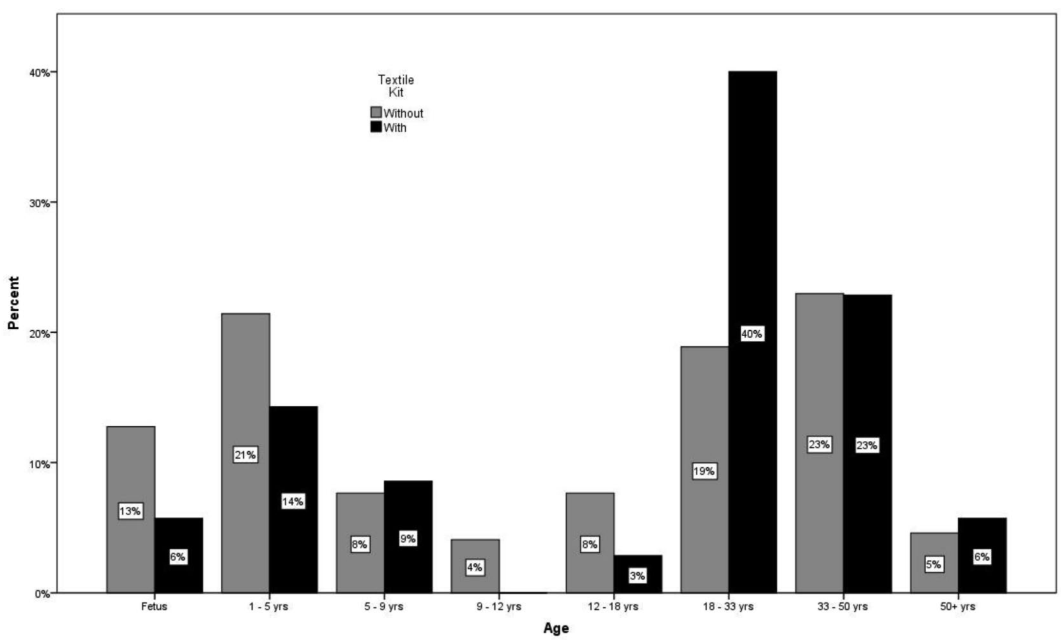

Figure 13.5. Association between textile tools and age categories.

that its form, similar to a female breast, served to nurture the world (Balbi and Cabieses 2003). This symbolic relationship between women and *lucuma* may have been important among the Chiribaya as well. Dendy (1991) also indicates that *molle* berries, also known as Peruvian pink pepper, used in the preparation of *chicha*, were more common in female burials. In this respect, and following Poma's accounts and current ethnographic studies, Chiribaya women were likely responsible for the production of this highly important ritual beverage in their communities.

Although in many cultures children are seen as genderless, it is apparent from Poma's depictions of Andean children that clear distinctions were drawn between males/boys and females/girls. In fact, ethnographic studies in contemporary Andean societies indicate that rituals designed to separate males and females begin as early as the birth of a child. For instance, after birth, the family buries the placenta of a baby boy by the right side of the front door to the home. If the baby is a girl, the placenta is interred on the left side of the door. Furthermore, in certain modern Andean communities it is believed that dreams, interest in particular foods, and the shape of the mother's stomach during pregnancy are all associated with the sex of the newborn baby (Portugal Catacora 1988). We examined burial inclusions with individuals below the age of six, specifically those interred in urns, and found that gender-specific items, similar to those found in adult burials, were included with fetuses and young children, suggesting that even though children under six may not have been active participants in Chiribaya society, distinctions between sexes were recognized early on in life and marked by specific material culture (Figures 13.4 and 13.5). None of the items associated with either adult males or adult females were found together, indicating that there were no cases of mixed gender identity from a cultural standpoint. Of course, short of using a DNA analyses to assess biological sex, it is not possible to assess these relationships further at the present time. With respect to gender construction among the Chiribaya, our results echo ethnohistorical and ethnographic accounts. Specifically, gender distinctions in the Andes appear to be based on biological sex and appear to remain constant throughout the life span of a Chiribaya person.

These two studies of social age and gender have relied on written material, archaeological data, and human biological information to assess social and biological factors that may explain differences between individual burials at Chiribaya Alta. In particular, this multidisciplinary approach underscores the need to move beyond rigid "universal" categories of both age and gender by including contextualized perceptions of individuals and society in the Andes. Furthermore, this study illustrates the fundamental sense of gender duality widespread in Andean societies long before the Spanish conquest of Peru.

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14

A Bioarchaeological Perspective on Inca Imperialism

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Jane E. Buikstra's research is epitomized by broad, holistic projects that integrate the tools and methods available to biological anthropologists, and theory derived from mortuary archaeology. Such a research design provides researchers the opportunity to consider both the biological and cultural consequences of the interaction between groups. There is considerable data, both archaeological and ethnohistorical, regarding the Late Horizon (A.D. 1470–1532) Inca Empire and how administrative policies influenced the sociopolitical infrastructure (Bauer 2004), economics and production (Hastorf 2001; LeVine 1987), and demographics (D'Altroy 2003:273; Grosboll 1993; Murra 1982; Rostworowski and Morris 1999) of conquered groups. To supplement these efforts, I will discuss how Inca imperial policies impacted population history and mortuary behavior in the Chachapoya region of northern Peru. Late Chachapoya (A.D. 1100– 1470) population structure will be reconstructed utilizing a relationship (R) matrix that provid es estimates of genetic differentiation among samples (F_{ST}) and of extralocal gene flow (Relethford and Blangero 1990; Relethford et al. 1997; Steadman 2001). Determinant ratios, which characterize the differences in multivariate variance between groups (Konigsberg 1988; Schillaci and Stojanowski 2003; Stefan 1999), will be used to discuss changes in regional heterogeneity following conquest. The Inca impact on mortuary behavior will by examined by evaluating the evidence for imperial modification of postmortem body-processing techniques. The integration of these sources complements the archaeological and ethnohistorical records and grants archaeologists a fuller appreciation of Inca imperialism.

The Chachapoya and the Inca: Archaeological and Ethnohistorical Data

The people referred to ethnohistorically as the Chachapoya inhabited a large region in the eastern watershed of the Andes in the northern highlands of Peru (Figure 14.1).[1] The broad brushstrokes of early colonial descriptions depict the region as being inhabited by several different tribes, yet they obeyed a single chief during times of war (Sarmiento de Gamboa 1999 [1572]:47) and called themselves by a single name (Garcilaso 1966 [1609]:154). Archaeologically, there appear to be regionwide similarities reflected in architectural construction, symbolic language, and ceramic traditions, yet internal social differences may have manifested in a north–south division of architectural design motifs (Lerche 1995) and mortuary structures (von Hagen 2002).

The predominance of ethnohistorical descriptions revolves around the Chachapoya and their relationship with the Inca. Several authors describe the valiant resistance offered by the Chachapoya (Cieza de León 1998 [1553]:98; Vásquez de Espinosa 1966 [1629]:385), their inevitable defeat (Garcilaso 1966 [1609]:222;

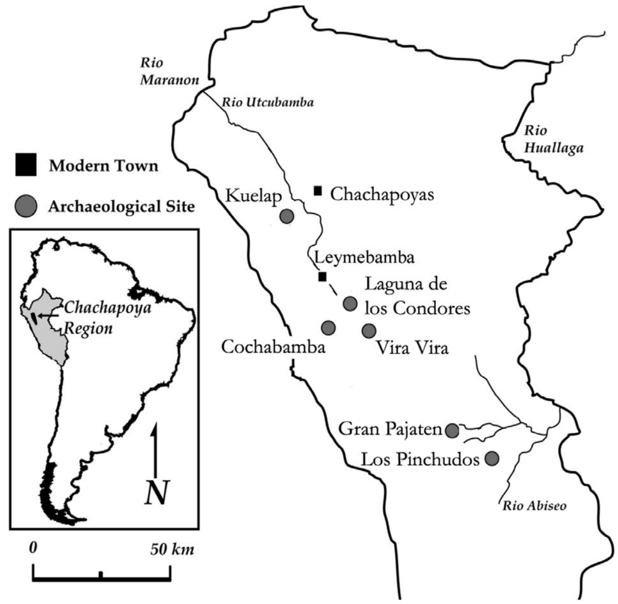

Figure 14.1. Map of the Chachapoya region, with the location of the skeletal samples analyzed and major sites discussed in this chapter indicated.

Sarmiento de Gamboa 1999 [1572]:140; Vásquez de Espinosa 1966 [1629]:387), and the severe consequences of rebellion (Garcilaso 1966 [1609]:220; Pachacuti Yamqui 1927 [1613]:222; Sarmiento de Gamboa 1999 [1572]:163). Though potentially biased, the ethnohistorical records provide insight into imperial policies that could have impacted population structure. In reference to the Chachapoya, the ethnohistorical records indicate significant internal rearrangement of groups and the exportation of large numbers of laborers (Schjellerup 1997).

Archaeological evidence attests to the significance and pervasiveness of the Inca presence, yet imperial control in the province appears to have been variable. Architectural evidence points toward more direct control in the central region, epitomized by the administrative center at Cochabamba (Schjellerup 1997, 1998). Given the size of the site and its limited storage capacity, the primary function of Cochabamba may have been military in nature rather than serving as a collection or redistribution point (Schjellerup 1998). Another site, Inka Llacta, in the northeastern periphery of the Chachapoya region, may have served as a secondary- or tertiary-level administrative center, facilitating the extraction of local resources (Schjellerup et al. 2003). Alternatively, evidence of such direct control is limited in the southern region surrounding Los Pinchudos and Gran Pajatén (Church 1997). Los Pinchudos was constructed in the Late Horizon, and while Inca-style ceramics were recovered, there is no evidence of Inca architectural influences (Morales et al. 2002). Such intraregional variability may reflect a gradual process of increasing imperial influence and consolidation through time. Evidence of an Inca presence or influence has also been noted at Kuelap (Ruiz Estrada 1969), Congona (Ruiz Estrada 1985), Laguna de los Cóndores (Guillén 1998; von Hagen 2002), Papamarca, and a number of Inca *tambos* in the Chuiquibamba region (Schjellerup 1997).

Skeletal samples drawn from mortuary contexts provide the foundation for evaluating the biological and cultural consequences of Inca conquest. Craniometric analyses allow us to consider how the loss of large numbers of individuals due to bloody wars of (re)conquest, compulsory urbanism, and the importation and exportation of laborers would have affected regional phenotypic heterogeneity. The exceptional degree of soft and hard tissue preservation in the region allows for the reconstruction of mortuary behavior and in particular how the body was treated after death.

Materials

Craniometric phenotypic data were collected from three Late Chachapoya (A.D. 1100–1470) skeletal collections (Reichlen, Laguna de los Cóndores, and Laguna Huayabamba) and one Late Horizon skeletal sample (Los Pinchudos).[2] Discussion of changes in mortuary behavior rest upon published data (Guillén 1998, 2003, 2004; Morales et al. 2002) and original observational data from the Laguna Huayabamba sample.

During the course of an archaeological survey of the Chachapoya region, Reichlen and Reichlen (1950) developed a relative chronology of what they considered to be three different cultural periods: Kuelap (circa A.D. 1000–1200), Chipurik (circa A.D. 1200–1400), and Revash (circa A.D. 1400–1470).[3] While craniometric data were generated from all the available crania, those assigned to Kuelap and Chipurik were not used in statistical analyses due to small sample size. The Revash culture was named after a collection of *chullpas* that architecturally and stylistically appear to be of typical Chachapoya construction. Unfortunately, given the nature of the recovery and the relative chronology, the allocation of this sample to the Late Chachapoya period must be considered provisional.

The site of Laguna de los Cóndores consists of a residential component exhibiting Chachapoya, Inca, and colonial-period influence (von Hagen 2002) and a funerary site consisting of six *chullpas* (von Hagen 2002). The remains of approximately 1,000 skeletonized individuals (Guillén 2003) attributed to the Chilchos, a Chachapoya ethnic group (Guillén 1998; von Hagen 2002), were recovered from two *chullpas* (von Hagen 2002).[4] Undecorated bundles, often containing skeletal material from several individuals, were also recovered (Guillén 2003). Based on a series of radiocarbon dates (Guillén 2003; Wild et al. 2007), the investigators consider these forms of body treatments to reflect typical Late Chachapoya mortuary behavior (Guillén 1998; von Hagen 2002). The mummy bundles from the site are described as anthropogenic because of evidence for evisceration and chemical treatment of the skin (Guillén 1998:47) and have been dated to A.D. 1410–1640 (Guillén 2003; Wild et al. 2007). According to the original investigators, following Inca conquest, the Chachapoya remains were relocated to two *chullpas* (Guillén 1998; von Hagen 2002). The Incas then elaborated and reused the remaining *chullpas*, interring Cuzco bureaucrats and *mitmaq* as well as local lords and their kin (von Hagen 2002:143).

The site of Laguna Huayabamba also consists of a residential and a funerary component. There is no architectural or ceramic evidence of Inca presence or influence (Muscutt et al. 1994). The mummified and skeletonized remains examined in this research were recovered from a tomb enclosed by a masonry stone wall (Briceño and Muscutt 2004) and date to the Late Chachapoya period (Fernandez-Davila 2008). Craniometric data were generated from 17 individuals, while examination of the mummified remains provided observational data on mortuary behavior (Nystrom et al. 2010).

Los Pinchudos is a Late Horizon mortuary site with no evidence for a pre-Inca presence (Morales et al. 2002). Contrary to the mortuary site at Laguna de los Cóndores, there is no suggestion that the Inca modified or influenced Chachapoya mortuary architecture. While an earlier report (Kauffmann 1980) depicted several mummy bundles, no mummies were recovered by Morales and colleagues during their more recent excavations (Morales et al. 2002), and only skeletonized remains were available for study (Bracamonte 2002). Morales and colleagues (2002) suggest that the remains were the result of a secondary interment ritual.

Summary data for the three Late Chachapoya skeletal samples examined for this research are provided in Table 14.1.

Biological and Social Consequences of Inca Conquest

To discuss how Inca conquest affected the Chachapoya, we must have an understanding of Late Chachapoya population structure. As mentioned previously, current archaeological interpretation suggests that the region was inhabited by a semi-independent confederation of groups that united only in the face of their common enemy. Measures of regional subgroup genetic differentiation are high (F_{ST} = .09; Table 14.2). The groups also differed in the degree of connection to external gene flow. The residuals in Table 14.2 indicate that the Laguna Huayabamba and Laguna de los Cóndores populations may have had stronger genetic interaction with extraregional sources. On the other hand, the individuals represented by the Reichlen collection were receiving less than average external gene flow (Table 14.2). Taken together, these results suggest that the Inca may have collapsed genetically and phenotypically heterogeneous subgroups. Imperial administrative units were presumably predicated upon some externally derived criteria, and while intraregional differentiation may have existed, the Inca considered the Chacapoya to be a single group and seemingly treated them as such.

The results of the determinant ratio analysis suggest that there was a decrease in sex-specific phenotypic

Table 14.1. Summary Data for the Three Late Chachapoya Skeletal Samples Examined for This Research.

Sample Name	n	Date	Reference
Vira Vira	17	A.D. 1000–A.D. 1150	Fernandez-Davila (2008)
Laguna de los Cóndores	151	A.D. 1100–A.D. 1420[1]	Guillén (2003)
Reichlen	78	A.D. 1350–ca. 1470[2]	Reichlen and Reichlen (1950)

1. The radiocarbon date reported by Guillén (2003) was derived from secondarily reconstructed bone bundles found in the same burial towers as the crania examined for this research.
2. This is the date range originally reported by the Reichlens for the Revash cultural period, to which the crania examined for this research have been attributed.

Table 14.2. Regional Phenotypic Distances to Centroid (r_{ii}), Observed Mean Variance ($\bar{v}_i$), Expected Mean Variance ($E_{(\bar{v}_i)}$), and Residual Variance ($\bar{v}_i - E(\bar{v}_i)$) for Late Chachapoya Populations with Trait Heritability = .55.

	r_{ii}	$(\bar{v}_i)$	$E_{(\bar{v}_i)}$	$\bar{v}_i - E(\bar{v}_i)$
Vira Vira	.2433	.931	.724	.207
Cóndores	.1108	1.006	.851	.154
Reichlen	.0348	.821	.924	-.102
F_{ST} = .0902, se = .0129				

Table 14.3. Between-Time-Period Sex-Specific Determinant Ratios.

Sex	Time Period Comparison	Determinant Ratio	More Variable Period	P-Value
Female	Late Chachapoya–Late Horizon	17.131	Late Chachapoya	.518
Male	Late Chachapoya–Late Horizon	2,034	Late Chachapoya	.394

variability following Inca conquest (Table 14.3). Though not statistically significant, the observed trend toward a loss of phenotypic variation sheds light on the impact of Inca conquest on Chachapoya populations. The most obvious social processes accompanying imperial conquest and consolidation that would have decreased genetic variation are deaths due to violent conflict and the exportation of large numbers of laborers. Indeed, ethnohistorical documents describe the loss or exportation of thousands of Chachapoya.

The exercise of ideological power was a significant theme in Inca consolidation policies (Conrad and Demarest 1984). The modification of local ideology and cosmology would have been a significant means of establishing their legitimate claim to resources. Indeed, it has been suggested that the Inca introduced anthropogenic mummification into Chachapoya. Evidence from two sites, Los Pinchudos and the Laguna de los Cóndores, suggests that pre-Inca Chachapoya mortuary behavior involved secondarily interred skeletal remains. As discussed above, Guillén (2002) suggests that at the Laguna de los Cóndores, the Inca removed the skeletonized remains of the Chachapoya, replacing them with their own anthropogenically mummified ancestors. A series of radiocarbon dates from this site provides evidence for a sequence of mortuary behavior, with skeletonized remains and the undecorated bone bundles preceding anthropogenic mummification (Wild et al., 2007).

Two points are worth considering in relation to this reconstruction (Nystrom et al. 2010). First, evidence from the Laguna Huayabamba indicates that some form of mummification existed in the Chachapoya region before Inca conquest (Fernandez-Davila 2008). While full internal examinations were not conducted, evidence of prolapsed rectums suggests that these mummies were not eviscerated in the same manner documented at the Laguna de los Cóndores (Nystrom 2005b, Nystrom et al. 2010).

Secondly, rather than viewing each type of body processing (for example, secondarily manipulated skeletal remains, bone bundles, anthropogenic mummies) as a discrete and bounded phenomenon, perhaps they should be viewed as phases within a more cohesive mortuary program that involved continued interaction with and manipulation of the remains. At the most basic level, the *chullpas* contain several generations of individuals. The dead could be physically moved, cared for, or, alternatively, destroyed by subsequent generations. Indeed, Guillén (2003:163) notes evidence of rewrapping and the presence of funerary bundles containing the remains of multiple individuals, both of which indicate long-term interaction with and manipulation of the dead. It is equally parsimonious to interpret the sequence of radiocarbon dates as indicative of continued manipulation of the ancestors, resulting in their gradual transformation from fleshed mummies to dry bones (Brown 1981). As new mummy bundles were placed within the *chullpas*, older mummy bundles may have been moved, manipulated, or even rewrapped (Guillén, 2003:163).

CONCLUSIONS

Inca expansion had more far-reaching effects than alteration to local economy or political structure. The same imperial policies that resulted in the construction of administrative centers or the relocation of conquered populations would necessarily have affected population biology. In the Chachapoya region, the impact of these policies can be seen in their effect on regional phenotypic heterogeneity. In other regions, imperial administration affected consumption and diet (Burger et al. 2003; Hastorf 1990; Hastorf and Johannessen 1993); health, trauma, and disease patterns (Andrushko 2007; Murphy 2004; Santoro et al. 2003; Torres-Rouff and Costa Junqueira 2006; Verano 2003); residential mobility (Andrushko et al. 2009); activity patterns (Andrushko et al. 2006; Toyne 2002); and even sacrifice (Eeckhout and Owens 2009). Importantly, the same samples that provide evidence on the biological consequences of conquest allow the opportunity to discuss the cultural impact of conquest, and it is the integration of these data with archaeological and ethnohistorical sources that provides a fuller understanding of the mechanisms and impact of Inca conquest.

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Notes

1. Chachapoya refers to the prehistoric province and those populations that inhabited the region. Chachapoyas refers to the modern-day capital and political department.
2. For a more thorough discussion of the steps taken to prepare the craniometric data for analysis, see Nystrom (2005a, 2006). The Reichlen skeletal material has been denoted as "Kuelap" in these publications.
3. It has been suggested that the Revash is only a modification of the Chipurik tradition. Dates are approximate and based on a chronological diagram in Schjellerup (1997:219).
4. The craniometric data utilized in this research was generated from these crania.

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PART IV

Bioarchaeological Research in Europe, Africa, and the Middle East

15

A Brief History of Soft Tissue Paleopathology

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The early history of studies on mummified human remains is inseparable from the struggle for public acceptance of human body dissection as part of the search for knowledge of human anatomy. Whatever the gifts of ancient Greece and Rome to modern civilization, these societies did not sanction the dissection of human corpses. The only exception was the famous library at Alexandria in Egypt. The usurpation of Egypt's crown early in the career of Alexander the Great included the construction of a port city in the Nile Delta, appropriately called Alexandria. After Alexander's death in 323 B.C., his principal general, Ptolemy, marched to Alexandria, appointed himself pharaoh, and built a huge library/museum. He attracted the most educated and creative scholars of the day, housed them there, and commanded only a single charge: "Create knowledge!" In the area of human anatomy, he provided his professional staff with the corpses of executed criminals for the purpose of dissection. In spite of later multiple raids and fires, this arrangement endured about seven centuries. Tragically, none of their publications dealing with human anatomy survived the vandalism of the early medieval period. The 50 generations of the Middle Ages were doomed to live their lives with an erroneous concept of disease that provided them with little if any useful medical knowledge (Long 1965:9–11; Lyons and Petrucelli 1987:399; Porter 1996:60).

It was the universities of northern Italy at Bologna and Padua that implemented the freedom of thought that swept through Europe in the 1500s. Human dissection—attended not only by physicians and anatomists but also by artists—soon became a standard option in their curriculum. Its impact is still evident in the muscular images on the Vatican's Sistine Chapel ceiling.

By the 1700s, occasional, isolated mummies were being dissected. In 1763 the British physician John Hadley (1764) unwrapped an Egyptian mummy. In 1794 John F. Blumenbach "unrolled" several Egyptian mummies, at least one of which was an ancient fraud. The physician Augustus Granville (1825) records a number of such somewhat casual examinations of Egyptian mummies by others. He himself found a large ovarian cyst in a mummy.

While the mid-nineteenth century in Europe is most well-known for advances in medical knowledge resulting from the human dissections of hospital deaths (autopsies) in Vienna and Berlin (Long 1965:102–106; Malkin 1993:114), it also documents a shift in medical viewpoints. This shift reflected awareness that mummies might be capable of adding to the developing medical database. In Britain, hucksters entertained paying customers by unwrapping Egyptian mummies in theaters and halls. Some people, however, explored mummy bodies in a serious effort to gain medical knowledge. Thomas J. Pettigrew, a British physician, taught university courses built around autopsy-based medical seminars. These included five lectures (one to two hours each) about Egyptian and other mummies, plus what today we would call a laboratory session. The latter consisted of unwrapping a mummy, followed by a meticulous dissection of the thoracic and abdominal viscera. By 1834 he had accumulated enough knowledge to share it with the public in a different form—a book (*A History of Egyptian Mummies*) that also included Gaunche mummies from the Canary Islands, as well as some Peruvian mummies, Burma priest mummies, and catacomb mummies from the Capuchin monastery of Palermo, Sicily. The intellectual climate was now receptive to the new field of mummy studies.

The Pioneers

Marc Armand Ruffer

If the field of mummy studies has a specific founder, it is Sir Marc Armand Ruffer. He was born into an aristocratic family in Lyon, France, in 1859, was educated at Oxford, and became a physician at University College in London. He was appointed professor of pathology in Cairo's new medical school in 1896 (Moodie 1921).

Ruffer's arrival in Cairo coincided with a surge of interest in Egyptian archaeology. Indeed, worldwide interest during this period is often termed Egyptomania. In 1881 a cache of pharaonic mummies was found in the Valley of the Kings, and nine more were found in 1898. Ruffer was invited to study these bodies, though only minimal dissection was permitted. In many of these mummies, however, portions of the corpse had suffered focal areas of decay that allowed Ruffer access to viscera, which could be sampled with minimal destructive effects. Most of Ruffer's diagnoses were based only on gross examination. Ruffer attempted to carry out microscopic tissue studies. He developed a method for rehydrating tissues and embedding samples in paraffin. Though his efforts had partial success and his method is still employed today, his histological observations made only modest diagnostic contributions to paleopathology. The list of diseases he identified would certainly have been much longer had he not suffered an untimely death in 1917, when, returning from a consultation dealing with public health problems in Greece, his ship was torpedoed. His principal contribution was the demonstration of the presence of information of medical interest in ancient, mummified human remains. After his death, his friend Roy Moodie (1921) collected his writings and published them in book form under Ruffer's name (*Studies in the Paleopathology of Egypt*).

Grafton Elliot Smith

Smith was a contemporary and colleague of Ruffer. A British-trained Australian neuroanatomist, Smith was head of the Department of Anatomy in the Cairo Medical School from 1900 to 1909. He was asked to examine all the royal Egyptian mummies. His enthusiastic response subsequently included not only most of those but also a vast number of other mummies. He is often credited with "examining" 30,000 Egyptian mummies (Dawson 1938), though he left us few descriptions or records of individually studied mummies outside of those of the pharaohs. Although Smith had an interest in diseases, his principal contribution to Egyptology was his focus on the methods employed by ancient embalmers to preserve corpses' soft tissues. His book (coauthored by Warren R. Dawson) *Egyptian Mummies* (Smith and Dawson 1991 [1924]) remains a classic.

Alfred Lucas

The British chemist Alfred Lucas applied his chemical knowledge to many aspects of Egyptian life as a member of Egypt's Department of Antiquities. He tested and identified coffin wood, textiles, foods, ornaments, and many other items. Compared to today's chemistry data bank, his chemical tools were primitive. Yet his book *Ancient Egyptian Materials and Industries* (1926) is even today a source of reliable information.

In spite of the exuberance that fueled the study of mummies during the first three decades of the twentieth century, a variety of factors combined to direct attention and activities away from mummy studies. Probably the most significant exception was what became known as the Manchester Museum Mummy Project (Murray 1910). That museum housed a large collection of Egyptian mummies. In 1910 its curator, Margaret Murray, launched an investigative study on two mummies that initially appeared to be well preserved. Although unwrapping their bodies revealed a degree of preservation much less than anticipated, a major examination of the textiles was a rich reward for the effort—sufficient to permit Murray's ultimate successor, Dr. Rosalie David, in 1975 to expand the project into what today is probably the largest mummy study program in the world outside of Egypt (David 1979). (For the contribution of Egyptology to paleopathology, see also Amelagos and Mills 1993.)

Paleopathology Organizations

The technological explosion following World War II spawned revived interest in mummies. A. T. Sandison (1955) plunged into a histological study of mummy tissue, while Brothwell and colleagues' *Science in Archaeology* (1963) focused interest on potential study methods for mummy diseases. Brothwell and Sandison collaborated to produce *Diseases in Antiquity* (1967), a book that summarized the state of mummy studies at that time. In recognition of the creative role played by research during World War II and its civilian applications in postwar life, Western nations structured funding sources for public projects, as did private companies. This work also rejuvenated interest in mummy studies, beginning in the early 1970s and resulting in the emergence of a number of organizations. Among the first was that founded by Marvin Allison, an infectious disease specialist in the Department of Pathology at the Medical College of Virginia, and pathologist Enrique Gerszten. Their annual visits to arid sites in Peru and northern Chile were spent dissecting human mummies, focusing particularly on infectious diseases. The Paleopathology Club, which they founded, published case reports of ancient mummies with tuberculosis, *Paracoccidioides brasiliensis*, osteomyelitis, Pott's disease, actinomycosis, congenital polycystic kidney disease, and more (Allison 1981).

Another organization devoted to mummy studies also began in the 1970s. British epidemiologist Aidan Cockburn and his wife, Eve, pursued an interest in the evolution of certain infectious diseases. Out of this grew the Paleopathology Association (Cockburn 1978). The initial group of about a half dozen scientists carried out autopsies on Egyptian mummies and published results principally in medical journals (Cockburn et al. 1975). Michael Zimmerman of the University of Pennsylvania, one of the group's leaders, studied mummies from arctic regions as well as temperate areas.

The third organization dedicated to mummy studies, the World Congress on Mummy Studies, met first in 1992 to study Gaunche mummies on the Canary island of Tenerife. It has been meeting every three years at different venues since then.

The fourth organization, the Institute for Mummies and the Iceman, evolved through the efforts of Albert Zink and others related to the Iceman mummy. Funded by the European Research Academy, its first meeting was held in March 2009 at the museum housing the Iceman mummy in Bolzano, Italy.

Membership in these organizations is growing. About 125 people attended the 2007 Meeting of the Sixth World Congress on Mummy Studies. Noteworthy was the broad array of scientific disciplines represented at the meeting.

Methods for Mummy Studies

Validation of Mummy Study Methods

Before plunging into discussions of methods employed in mummy studies, a brief but vital warning is applicable. "Blind" application to a mummy tissue sample of a technique originally designed for use on a specimen of a living individual runs the risk of producing unreliable results. For example, the lead content of a bone from a living or recently deceased person can be predictive of certain social features of that individual. These features were the product of that person's lifetime exposure to lead. In the 1990s a series of such studies on ancient bones was reported by Aufderheide et al. (1992). Some of their interpretations of the results are viewed today with a jaundiced eye because we know now that buried bone can absorb enough lead from groundwater to obliterate the antemortem values. Furthermore, the diagenetic lead cannot be separated analytically from the antemortem absorbed lead. Lesson learned: Validate the test's behavior on mummy tissues before relying on its results.

Emphasis on Mummy Soft Tissues

By the middle 1990s, the newly formed mummy study organizations had delivered a massive stimulus to research studies on mummies. Initially, the majority of studies consisted of descriptions of pathological lesions that were large enough to be detected and identified by gross observations. During the first few years after the founding of the Paleopathology Club, its newsletter featured case reports on such lesions as sinus mucocele, lung foreign bodies (projectile points), generalized hyperostosis, polycystic kidney disease, cardiomegaly, fibrinous pericarditis, skull and tibia treponematosis, and other gross pathological alterations. These were characteristic of articles reporting soft tissue paleopathology during this period.

Pseudopathology

Many articles were devoted to detailed descriptions of intact mummies, emphasizing features that revealed the various alterations that led to their soft tissue preservation. Nevertheless, investigators had to adapt to the reality that, unlike with hospital autopsies, they were seeing advanced, untreated, end-stage disease states. In addition, loss of water caused all mummified tissues to have a similar density and uniformly brown color, complicating the recognition of organs. Researchers also needed to deal with what I call organ migration, when partial decay causes an organ to detach from its normal location and move to a different location when the body is handled (Aufderheide 2003). Using a gross pathology approach, bog bodies, Chinese burials, Aleut burials, and many Egyptian burials were described with emphasis on their mummification methods.

Radiological Imagery

Early "flat plate" X-rays had a major limitation: superimposition of bones and soft tissues. This was a minor problem in supine bodies, but in a flexed body the superimposition of arms, legs, ribs, spine, and other structures profoundly reduced the amount of information one could derive from the study. This limitation was eliminated by the development of computerized tomography (CT), which effectively minimized superimposition by positioning detectors so as to generate an image equivalent to a body "slice." This technology became even more useful when the entire irradiated body area could be reconstructed as a three-dimensional image. While the use of portable X-ray machines in the field is practical, portable CT scanners are quite cumbersome, making it easier in most cases to bring the mummified body to the scanner rather than vice versa—although, as O'Brien et al. (2009) have pointed out in a review of the literature on imaging of mummies, the application of this kind of technology is most beneficial when it is hypothesis driven.

Just as postmortem changes by mummification mechanisms can cause an investigator to misjudge the nature of an altered tissue, leading to a misdiagnosis, X-rays can also produce unfamiliar changes that may lead a radiologist to erroneous diagnoses. Lynnerup (2007) has published a summary of such "diagnostic traps" in radiological image interpretations that can be avoided by an informed interpreter. Magnetic resonance imaging (MRI) designed for the study of hydrated tissues is ineffective when applied to desiccated mummies.

Elemental Analysis

Strontium isotope ratios differ in various geographic areas. With appropriate sampling of selected mummy tissue and soil (or rock) specimens in regions of interest, it may be possible to identify an individual's sites of residence in youth and in later life (Knudson et al. 2007). Measurements are performed using mass spectrometry.

Chemical Dietary Reconstruction

Isotope ratios of carbon and nitrogen as measured by mass spectrometry on mummy tissues are predictive of certain antemortem features of a mummy's diet during life (vegetal/meat fractions, dietary protein content, and terrestrial/marine diet sources) (Ambrose 1993). Postmortem compound degradation can introduce variables that can be avoided by carrying out the measurements on "purified" extracts of certain lipids that are known to resist such alterations (Evershed et al. 1999). Instrumentation employs gas chromatography and mass spectrometry.

Hair Studies

Removal from the blood of certain chemicals that circulate through hair roots results in incorporation of such chemicals into the hair shaft. After emergence of the hair shaft from the skin, the shaft can be cut and subjected to analysis. Cartmell et al. (1991) have validated both radioimmunology and mass spectrometry to quantify the amount of cocaine in hair samples from South American mummies who chewed or ingested cocaine-containing coca plant leaves in life.

Ancient DNA Studies

DNA in mummies is extensively fractured and degraded to the degree that commonly less than 1 percent of the DNA found in living persons is present in mummy tissues. Fortunately, polymerase chain reaction (PCR)—a technique for amplifying this amount to a concentration similar to that of living organisms—became available in the early 1980s (Mullis 1990). While routine recovery of ancient nuclear DNA is not yet predictable, that of mitochondrial DNA is common. The types of data studied fall into two broad groups: migration reconstruction, and detection of the presence of an ancient infectious agent in a tissue specimen of an ancient mummy. The hazards of error threaten the analyst at every step of the procedure; the principal problem is specimen contamination. In routine research laboratories, this risk is minimized by hoods with airflow controls and by carrying out certain steps in dedicated, separate rooms. When studying human DNA, the best success is experienced in laboratories that have been specifically constructed for that purpose. Identifying bacteria, viruses, and other infectious agents in mummy tissues carries less danger of contamination because DNA in mummies is not as ubiquitous in a laboratory as is DNA in living humans.

The list of applications of DNA is open-ended. Identification of DNA of the tubercle bacillus in a 1,000-year-old South American mummy established the presence of tuberculosis in the New World 500 years prior to European contact (Salo et al. 1994). A similar approach made it possible to trace the behavior of American trypanosomiasis (Chagas disease) over the past 9,000 years in South America (Aufderheide et al. 2004). Recent whole-genome sequencing methods have greatly expanded the applications of ancient DNA technology (Gilbert et al. 2008).

Proteometric Methods in Mummy Studies

Efforts to detect proteins in tissues have shown potential. To date, these employ the principle of protein digestion of tissue samples, then separation and identification of resulting peptides using mass spectrometry. Though the work is promising, it is probably too early to know what, if any, role it will play in routine ancient mummy studies.

Endoscopy

While the rigidity of desiccated ancient tissues imposes distinct limitations on the use of endoscopic applications in mummies, some bodies have yielded much useful data with this technology (Beckett et al. 2008).

The Future of Mummy Studies

Restrictive Legislation

In general, ethical concerns about mummy study activities are expressed in direct proportion to the degree to which a population or subpopulation perceives itself to be direct descendants of the studied mummies. Currently, legislation restricting mummy studies to some degree exists in the United States, Australia, and Israel. However, history indicates that once a source of knowledge has been identified, its study cannot be suppressed forever. Experience suggests that we need to be patient, but the Renaissance teaches us to actively seek our objectives if we expect to reach them. I am positive that the present impetus of mummy studies will be appropriately harnessed and lead us to our goals.

Funding

Funding is a critical factor for the future of mummy studies. The National Science Foundation and the National Institutes of Health in the United States are already struggling to meet their current needs. It will be a major challenge for leaders in the field of mummy studies to convince public funding sources such as these that mummy studies fall within their mandate. This work requires organized efforts at all levels.

Organization

It is possible but unlikely that the field of mummy studies will ever be large enough to be an independent scientific discipline. Thus, to survive and flourish, the field will need to join a related discipline. The current mummy studies organizations could bring a large number of attendees into a single, fused group. Physicians would need to learn more physical anthropology, while physical anthropologists would need to become more medically knowledgeable. Some educational supplementation would be needed to bring this about, but these concerns certainly would not prohibit progress. In addition, such a structured group would need a predictable source of mummies for training and research during summer field trips. This, too, can be arranged.

Due to their spectacular nature, mummies have often been decontextualized, because interest in the preserved body has overridden the importance of archaeological contexts. Integrative, hypothesis-driven, multidisciplinary approaches as advocated by Jane E. Buikstra are essential to a more holistic understanding of these remains. It has been my good fortune to work with Buikstra, and as a result of this collaboration, we have charted the societal impact of diseases such as primary tuberculosis, Chagas disease, osteosarcoma, and others from the Osmore drainage in Peru.

Conclusions

The scientific aspects of mummy studies have already justified their existence by demonstrating that mummies house both medical and anthropological information and that such information can be extracted and integrated with the broader databases of these disciplines. These data can be obtained from no other sources. Soft tissue paleopathology now has all the required features of a scientific subdiscipline; thus its future is promising.

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16

Social Aspects of the Bioarchaeology of Leprosy

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This chapter aims to offer perspectives on the social aspects of leprosy (or Hansen's disease) in medieval Europe, and Britain in particular, but by necessity grounds itself in the clinical literature. It is of course dedicated to Jane E. Buikstra, whose extensive interest in and research on the bioarchaeology of mycobacterial disease, particularly tuberculosis, inspired the author. However, her attendance at the "Past and Present of Leprosy" conference in the United Kingdom in 1999 (Roberts et al. 2002) proved she had an interest in mycobacterial infections beyond tuberculosis. Buikstra's tremendous contributions to bioarchaeology (in the widest sense) over more than 40 years of working in anthropology are breathtaking. However, of particular note has been her constant strong emphasis on contextualizing skeletal data, something that has been slow to develop in some parts of the world. Her research has included the publication of excavated funerary sites, incorporating both biological and archaeological data admirably (e.g., Buikstra et al. 2004); landmark works on specific health problems (e.g., Buikstra 1981); and invaluable contributions to the standardization of the recording of human remains, which have shaped the world of bioarchaeology (Buikstra and Ubelaker 1994). She has also provided bioarchaeology with a key text on its development (Buikstra and Beck 2006), as well as one on paleopathology in particular (Buikstra and Roberts 2012). Her involvement in fieldwork in the Americas and in parts of Europe is particularly illustrated in her long publication list, a list that shows the wide variety of collaborations she has nurtured at all levels and in many disciplines. Buikstra is a team player who has encouraged a great number of people in bioarchaeology, including this author, and continues to be an inspiration to all.

"It's hard to catch, easy to cure and ought to be on its way into the history books by now. So why is leprosy still blighting millions of lives?" (Nelson 2005:28). Why indeed? Leprosy is a chronic infectious disease that has plagued human populations for hundreds of years (Roberts and Manchester 2005). The first truly convincing evidence in skeletal remains comes from the Dakhleh Oasis in Egypt and dates to 250 B.C. (Dzierzykray-Rogalski 1980), although accepted documentary evidence from 600 B.C. is seen in the *Sushruta Samhita* from India (Dharmendra 1947). In some parts of the world, leprosy remains a devastating problem for many; India carries two-thirds of the world burden of leprosy (Young 2001). The problem should be considered particularly from the viewpoint of the social milieu in which many people with the disease are forced to live (Joseph and Rao 1999). This paper considers leprosy from a social perspective by comparing the present and the past, with a focus on the perception and treatment of people with leprosy in medieval Europe. However, the bioarchaeological aspects of leprosy need first to be grounded in what is known about the infection clinically.

CLINICAL LEPROSY

What Is Leprosy?

Leprosy is a chronic infectious disease that affects humans. However, it has also been recorded, although inconsistently, in other animals, such as the nine-banded armadillo (Truman et al. 1991). It is caused by *Mycobacterium leprae*, is of high infectivity but low pathogenicity, and primarily affects the peripheral nerves and secondarily the skin and other tissues, such as the eyes, testes, kidneys, bones, blood vessel endothelium, and upper respiratory tract (Jopling 1982). The main method of contracting the infection is via inhalation of bacteria-laden droplets. Therefore, prolonged close contact between affected and unaffected people is necessary for transmission; population density may play a role here. Nevertheless, leprosy is difficult to contract, unlike the related infection tuberculosis. Key to whether the infection is transmitted is the strength of the individual's immune system toward *M. leprae*. A person with a low resistance to the bacteria will develop lepromatous or multibacillary leprosy; one who is highly resistant will develop tuberculoid or paucibacillary leprosy—but there are other levels of immunity between these extremes. Leprosy can have a long incubation period, from three months to 40 years (Bryceson and Pfaltzgraff 1990), but two to five years is usual (Dayal et al. 1990). Leprosy can now be cured with antibiotic therapy, but it is important to treat it early, otherwise considerable disfigurement and disability can result.

How Does Leprosy Affect the Human Body?

Leprosy affects the body in a number of ways. In non-lepromatous leprosy, one or more skin lesions on the cooler parts of the body (buttocks, limbs, or face) are seen (Jopling 1982). In lepromatous leprosy, papules and nodules occur bilaterally and symmetrically on the face, earlobes, buttocks, and limbs, and they contain many bacilli. *M. leprae* also affects the peripheral nerves and prefers the cooler more superficial areas (Jopling 1982). Paraesthesia (pins and needles), pain, anesthesia, and paralysis also occur, with paralysis potentially affecting the face, hand, lower leg, or foot. The sensory, motor, and autonomic nerves can all be affected, usually in combination. Loss of sensation in the skin tends to occur (Bryceson and Pfaltzgraff 1990), and autonomic nerve damage affects blood vessel functioning. Loss of the ability to sweat and osteoblast-osteoclast balance, along with medullary cavity-width reduction, occurs in the metatarsals, metacarpals, and phalanges. Claw hand and foot (motor nerve damage and paralysis) and collapse of the foot arches develop. Sensory loss, repeated trauma, and bruising lead to ulceration of the soft tissues. Plantar ulcers enable secondary bacterial infection to develop, leading to osteomyelitis and septic arthritis of the bones and joints of the hands or feet. There can be multiple and varied changes in the hand and foot bones, described in Roberts and Manchester (2005). Essentially, the person may experience disintegration of the hand and foot structures, including dislocation of joints. *M. leprae* also infects the soft tissues in the nose and mouth; this process can lead to damage to the palatal and turbinate bones and the septum of the nose (pitting, perforation, new bone formation) and to the loss of maxillary alveolar bone around the incisor teeth, the anterior nasal spine, and the nasal aperture. The signs and symptoms of leprosy are variable and described in Dayal et al. (1990), onset usually being gradual and dependent on the person's resistance to leprosy.

Leprosy Today

In 1991 the World Health Organization (WHO) resolved to eliminate leprosy by the end of 2005, while the World Health Assembly agreed to set 2000 as the deadline. This meant a reduction of the global incidence to less than 1 registered case per 10,000 people. In 2001 the WHO said it had reached its target but admitted that leprosy was still a problem in parts of Asia, Africa, and South America. The WHO felt that by the end of 2005, leprosy could be totally eliminated. Now, the most prevalent endemic countries have reached elimination (World Health Organization 2012). The WHO has seen more than 14 million cases diagnosed and treated and a drop in numbers from 12 million to about 460,000 over 20 years. The WHO reports that the prevalence of leprosy at the beginning of 2011 was 192,246 cases, with newly detected cases during 2010 being 228,474. The main countries yet to reach elimination at the national level are Angola, Brazil, the Central African Republic, the Democratic Republic of Congo, India, Madagascar, Mozambique, Nepal, and the United Republic of Tanzania. Progress in elimination has been due to the use of multiple antibiotics (dapsone, rifampicin, and clofazimine), which are given free to anybody suffering from leprosy. Vaccines, particularly of BCG (tuberculosis), have also been used for preventing leprosy, but efficacy varies (Fine 1995). With a better understanding of the molecular nature of *M. leprae* (Cole et al. 2001) and the human response to infection, new vaccines will be developed. However, even if cured leprosy patients are increasing, thousands still continue to suffer the debilitating physical, social, economic, and psychological consequences (Scollard et al. 2006). Meima et al. (2004) suggest that leprosy's consequences will be with us for many years.

Unlike most other diseases, leprosy attracts much stigma and misunderstanding (Jopling 1991). This makes the social milieu in which affected people live particularly challenging, and they are often ostracized. This misunderstanding stems partly from a mistranslation in the Hebrew version of the Bible of the word *tsara'ath*—a word now accepted as being associated with any disfiguring skin disease or state of ritual uncleanliness.

Gussow and Tracy (1970:426) suggest, "Stigma is said to be worse than the disease itself," but Noordeen (1993:517) emphasizes, "Without the disability-producing potential, leprosy would not merit the attention it gets," although people disagree about how stigmatized leprosy is or was (Navon 1998). Prior to a cure for leprosy, reactions to leprosy were ambivalent. Once treatment had been developed, leprous sufferers could keep their illness a secret, but successful treatment also meant an improvement of patients' social acceptance. Disappearance from the public eye then led to the figurative use of leprosy in the English language; the word *leper* continues to be associated with social rejection, which has led to irrational perceptions of leprosy. Cochrane (1963) even suggested that charities helping those with leprosy perpetuated the negative image of leprosy in their fund-raising campaigns. More recently, the media, artists, and authors have also been accused of propagating the link between sensationalism and leprosy. A recent "educational" novel about leprosy in Greece by Hislop (2005) is a sympathetic and balanced treatment of the impact of leprosy on people in the twentieth century. Nevertheless, it is clear that stigma affects a person's lifestyle and position in society. For example, a study of people with leprosy in Nigeria found that males had problems securing and holding jobs, which led to loss of income, and divorce was usually instigated by males with leprous wives (rather than by wives of leprous men) (Awofeso 1995). Of relevance to discussions in bioarchaeology are numerous varied examples of how people view leprosy and how it affects populations (Barkataki et al. 2006). For example, Skinses (1964) notes that in China, leprosy was seen as a punishment from heaven, and it was thought to be transmitted through sexual intercourse or inherited. He records that if a person sat on a chair that was still warm from a person with leprosy using it, the sitter would be expected to contract leprosy; in one village, people kept special chairs in the rafters of houses and brought them down for healthy visitors. Concepts of leprosy in parts of the world appear strange to the Western eye but are highly relevant to the potential success of eradication programs, including acceptance of, and compliance with, treatment.

The Bioarchaeology of Leprosy

The way leprosy affects the body, and how leprosy is viewed and treated, is globally variable. This variability is highly relevant to our understanding of leprosy in the past and how it affected society, something that is often forgotten when we're faced with a skeleton with bone changes caused by leprosy. It is important to remember that "our skeletons" were once living, breathing people who functioned within societies with particular cultural values and attitudes toward disease. But how early do we see leprosy reflected in the bioarchaeological record, and how do we recognize it skeletally?

Skeletal Changes of Leprosy

Leprosy affects the skeleton in only about 5 percent of people with the disease (Resnick and Niwayama 1995), and depending on where on the leprosy immune spectrum the individual lies, these bone changes will vary in manifestation. Detection of leprosy in skeletal remains is based on the diagnostic criteria of Vilhelm Møller-Christensen, a Danish doctor who was interested in archaeology and paleopathology and who excavated and analyzed skeletons from medieval Danish leprosy hospital sites (Bennike 2002). Møller-Christensen first described bone changes in medieval Danish skeletal remains in the 1950s (1953); his criteria were extended mainly by Andersen and Manchester in a series of papers (cited in Roberts and Manchester 2005).

The facial bone damage, described above, has been called facies leprosa (Møller-Christensen 1978) and, more recently, rhinomaxillary syndrome (Anderson and Manchester 1992). This damage is considered pathognomonic for leprosy, but other disease processes may cause similar changes (Cook 2002; Manchester 1994). In lepromatous leprosy, bone damage to the nasal area and palate is seen, but in tuberculoid leprosy it is absent. In lepromatous leprosy, changes to the hands and feet are usually symmetrical, but they are asymmetrical in tuberculoid leprosy (Manchester 2002). Damage to the hands is less common than to the feet, and in archaeological contexts this is also the case, possibly because ulceration of the hands is noticed more often because of ease of observation for those living with leprosy (Manchester 2002).

Tibial and fibular new bone formation (periostitis) in leprosy is believed to be due to infection in the foot extending upward into the legs, but it has also been seen in living leprous sufferers without foot damage (Manchester 2002). In a bioarchaeological study, Lewis et al. (1995) documented 38 percent (76) of leprous individuals from a medieval leprosy hospital in Chichester, Sussex, England, with periostitis of lower leg bones, but 19 percent (60) of the non-leprous individuals also had periostitis. It is a nonspecific change that has multiple causes (for example, trauma, treponemal disease, scurvy, tuberculosis), but recent work has attempted to use histological analysis to differentiate the causes of periostitis (Schultz and Roberts 2002; but see Weston 2009).

How Common Was Leprosy in the Past?

All skeletal evidence for leprosy comes from the Old World; evidence in the New World should have appeared with colonization in late contexts. At the time of European voyages to the east coast of the Americas in the late fifteenth century, leprosy was declining in Europe; it is unlikely that lepromatous leprosy would have reached the Americas via European contact (Roberts and Manchester 2005). This is because leprosy is one of the least contagious of the transmissible infectious diseases; if it had been transported across the Atlantic, it would not have had as severe an impact on the native populations of the Americas as other infectious diseases (Browne 1970). The story of the bioarchaeology of leprosy in the New World is therefore considerably shorter than in the Old World. In the United States, leprosy was introduced to southern Louisiana by French settlers when they were expelled from Nova Scotia in 1755. In 1884 a State Board of Leprosy Control was directed to find a "leper home." Following passage of a bill to create a national leprosarium (1917) by the U.S. Congress, the Louisiana Home for Lepers was founded in 1921 at Carville, 85 miles northwest of New Orleans (at an abandoned plantation called Indian Camp). It became a sanctuary for people with leprosy throughout the United States, with admission on a voluntary basis. In the 1940s, researchers at Carville discovered the first effective antileprosy drug (promin). Leprosy is now a minor endemic disease in the states of Louisiana and Texas.

In recent years, comparative genomic research has been used to establish genealogical relationships in disease. Monot et al. (2005) suggested that *M. leprae* has undergone extensive reductive evolution and that leprosy probably originated in eastern Africa or the Near East and then spread as people moved around the globe. Early data from a third-century B.C. Chinese bamboo book suggest that leprosy was present in China (Skinses 1980), but skeletal remains indicate that by the first millennium A.D. it was likely present in Uzbekistan in west-central Asia (Blau and Yagodin 2005). Based on historical data, it has long been suggested that leprosy was brought to the Mediterranean from the Indo-Gangetic Basin by the armies of Alexander the Great (356–323 B.C.) returning from the Alexandrian campaign; there have been recent debates about this theory, however (Mark 2002). However, it is only recently that suggested bioarchaeological evidence for leprosy in India has been reported and dated to 2000 B.C. (Robbins et al. 2009), although the earliest evidence of leprosy in historical documents (*Sushruta Samhita*) is dated later, to to 600 B.C. (Dharmendra 1947), as mentioned above. Evidence from skeletal and mummified remains has also been observed in Israel and Egypt. Dzierzykray-Rogalski (1980) describes leprosy in four people from the Dahkleh Oasis of Egypt (250 B.C.), and Molto (2002) discusses more recent data at the Roman Kellis 2 cemetery (two leprous males, early to mid-fourth century A.D.).

In France and Italy, the earliest evidence of skeletal leprosy comes from the Roman period (third to 7th century A.D.—Belcastro et al. 2005; Blondiaux et al. 2002; Mariotti et al. 2005). In Britain, bioarchaeological data show leprosy increasing from the early to late medieval periods (fifth to sixteenth centuries A.D.) (Roberts 2002). In Scandinavia it is seen also in Denmark (Boldsen 2005; Boldsen and Mollerup 2006; Møller-Christensen 1953, 1969), and leprosy was present until the middle of the twentieth century in Norway (Richards 1977). While leprosy has been reported in Poland, Germany, and the Czech Republic (infrequently), in Hungary Marcsik et al. (2007) describe the first evidence for leprosy, which is dated to the seventh to ninth centuries A.D.

Despite early historical sources indicating leprosy in India and China, no bioarchaeological evidence has been found in China. The only possible skeletal evidence from Southeast Asia comes from Thailand (two skeletons dated to 300 to 200 B.C., and one to the first two centuries A.D.) (Tayles and Buckley 2004), along with evidence from Micronesia (seventh to fifteenth centuries A.D.) (Trembly 1995). On the basis of the skeletal evidence to date, leprosy has early foci in northern Europe and the Mediterranean, specifically Egypt and Nubia. If the data from India and Thailand are accepted, then early foci for leprosy are also noted in these areas. There are later appearances of leprosy in Micronesia and in other parts of northern Europe and the Mediterranean. However, it is not until the later medieval period that we see a rise in the frequency of the disease in northern Europe. Leprosy then declined from the fourteenth century onwards in Europe, possibly due to the rise of tuberculosis and the nature of cross-immunity between the two infections (Chaussinand 1953; Manchester 1991), but this possibility continues to be debated (Leitman et al. 1997; Wilbur et al. 2002). The disease has been maintained in some parts of the Old World today, as we have seen, but also in the Americas. Clearly, the movement of our ancestors enabled leprosy to be transmitted around the world, a factor that continues to affect the spread of infectious disease (Armelagos 1998; Bhatia et al. 2001).

How Was Leprosy Viewed, Diagnosed, and Treated in the Later Medieval Period?

Our understanding of leprosy in the late medieval period, and prior to Armauer Hansen's discovery of *M. leprae* (1873), is colored by those who wrote about and illustrated it, and perceptions were not helped by the mis-assignment of the infection to descriptions in the Bible (see above). As Rawcliffe (2006:17) states, "Just as cancer and AIDS have become iconic diseases of the late twentieth century, so leprosy has often been projected as representative of the 'Dark Ages,' when prejudice and superstition marched hand in hand with ignorance." Authors and artists instilled shock and fear of the leprous into society because they had found a disease that was dramatic and perceived of as dangerous. It was viewed as a punishment for immoral or unclean activities, including sexual intercourse, which led to strange diagnostic and therapeutic regimes. However, attitudes toward leprosy varied considerably around the world. For example, in the medieval crusader states, stretching from Egypt to Asia Minor, society was much more accepting of leprosy (Mitchell 2000).

A range of people—laypeople, physicians, barber-surgeons, apothecaries, monks, and the clergy—felt qualified to diagnose leprosy around Europe. They took their knowledge from works that had been translated from Latin, but, as Rawcliffe (2006:168) indicates, there was generally a common pool of knowledge that people could access regarding diagnosis and treatment, and diagnosis probably took some time and involved a combination of "medical knowledge and simple pragmatism." Diagnosis by examining blood, feces, and urine was particularly common. Cule (1970) describes the Welsh practice of dropping of a raven's egg into a person's blood. If it hardened, the person was deemed leprous. In contrast, Richards (1990) describes the French surgeon Guy de Chauliac and his *Inventarium* of 1363. Here he divided clinical features into "certain" and "uncertain" and described three stages of diagnosis: suspicion (observe at home and review continually), strong suspicion (stricter isolation at home), and certain diagnosis (consign to hospital), with anybody declared free of the disease receiving a certificate. "Certain" signs were eyebrow thickening, disfiguration and obstruction of the nostrils, scarring around the eyes and ears, and a harsh nasal voice. Clearly, many diagnostic tests were related to mythical perceptions of the disease, which then extended to treatment.

The basis of medieval European medicine lay in the doctrine of the four humors as described by Hippocrates (fifth century B.C.). A disturbance in the balance of blood, phlegm, black bile, or yellow bile was believed to lead to disease. Related to the four elements (fire, earth, air, and water) and the fundamental qualities (hot, cold, moist, and dry), the humors were also affected by astrology, especially in the fourteenth and fifteenth centuries A.D. (Rawcliffe 1997). In the later medieval period (the twelfth to sixteenth centuries A.D.), treatments for ailments, including leprosy, involved specific diets, herbal remedies, laxatives, diuretics, bloodletting, cautery, and bathing, along with treatments that seem unintelligible to the developed Western world, such as eating dead infants' flesh in China (Skinses 1964). These treatments took heed of astrology and the influences of the planets and heavens. Levels of heat, cold, dryness, and moisture associated with medications were very important (Rawcliffe 1997).

Apart from the range of potential remedies available to those with leprosy, one of the main suggested "therapies" was segregation once diagnosed; if society was afraid of people with leprosy, this was a way to rid society of leprosy. Richards (1990) describes, now controversially (see Rawcliffe 2006), that in the later medieval period in Europe, the leprous were considered dead once diagnosed and were isolated into leprosy hospitals via symbolic funerals. They had no marriage rights or property, were stripped of their citizenship, were prohibited from attending church, and were made to wear specific clothing and to carry clappers to warn people of their presence. (A full description of the rules regarding segregation is provided in Clay 1966.) Whether these rules applied to all those diagnosed in later medieval Europe is questionable. However, Rawcliffe (2006) argues that the view of leprosy was reinforced by nineteenth-century rewritings of medieval history in England by people who favored compulsory isolation. Both Rawcliffe (2006) and Demaitre (2007) emphasize the variability of reactions to leprosy in the premodern era (prior to treatment being available) and that we must not assume that everybody with leprosy was stigmatized, ostracized, and segregated. While historians are now developing more accurate views of how the leprous were treated in the past by scrutinizing historical documents more closely, bioarchaeologists are also reconsidering their data (see below).

Historical data indicate that in Europe, leprosy hospitals were founded by benefactors, usually on the edge of towns and cities and outside the walls. They rose in frequency from the eleventh century A.D., and their "popularity" was attained in the twelfth and thirteenth centuries, with a decline from the fourteenth century and with many being used as general hospitals from that time (see Roberts 1986 for a view of leprosaria in Britain). There may have been more than 200 founded in Britain between the eleventh and sixteenth centuries, but it is unlikely that everybody admitted to a leprosarium was leprous, and one certainly cannot take the number of leprosaria as indicative of the disease's frequency. Richards (1990) notes that leprosy hospitals in Britain were small and had a staff of about three people, with room for about 10 patients, but there were some larger hospitals. (Harbledown, Canterbury, Kent, for example, had room for 100, and Sherburn, County Durham, had room for 65.) Even if all the hospitals were full, there would have been a maximum of 3,000 to 4,000 leprous individuals in a population of 3 million in the late medieval period. The perception until recently was that life in leprosaria was unpleasant. However, Rawcliffe (2006) is clear that life was probably quite acceptable compared to prediagnosis living conditions: there was a guarantee of security and protection, regular food, warmth, clothing, a set routine, and even contact with the outside world. There are even suggestions that people wished to be segregated so that they had access to care and sustenance.

With respect to segregation in medieval Europe, several factors could have affected whether somebody with leprosy was diagnosed and segregated. First, the strength of a person's immune system would determine the type of leprosy he or she contracted. A person with the high-resistant type might not have shown any outward signs and therefore might have remained undetected. It is suggested that only the lepromatous leprous (low-resistant) were segregated into leprosy hospitals, and Ell (1986) argues that fourteenth-century medical writers advocated a conservative approach to leprosy diagnosis, selecting the most extreme cases. Certainly, the lepromatous leprous are seen in leprosy hospital cemeteries of the later medieval period (for example, at St. James and St. Mary Magdalene in Chichester, Sussex—Magilton et al. 2008), but people with no bone changes of leprosy at all were also buried there. Those people without evidence of leprosy may have been misdiagnosed, perhaps with skin lesions that were not related to leprosy, or they may have had high-resistant leprosy and thus no bone changes when they died. People in leprosy hospitals and buried in their cemeteries may have also had totally different diseases.

How effective leprosy hospitals were at segregating people with leprosy and controlling the infection is highly debatable. Because of its long incubation period, people with leprosy could have remained in the community long after contracting it and thus transmitted it to others. Some reports say that segregation was not strict in hospitals and that people could go to markets, have visitors, or even be expelled for bad behavior (Macarthur 1953). One also has to explore the social responses to leprosy through time and in different geographical contexts and cultures to understand how varied those responses were and can be. In bioarchaeology, the main avenue of investigation into society's response is the way the leprous were treated in death, along with the evidence of hospitals (see above). On the basis of bioarchaeological evidence, the idea that all society in later medieval Britain (or Europe as a whole) viewed the leprous with fear cannot necessarily be upheld if one considers skeletal evidence for leprosy and the cemeteries where leprous people were buried (Roberts 2002). Of the 27 archaeological sites where leprous skeletons had been found at the time of the research, only four were leprosy hospitals. There were no instances where the body had been treated in an unusual fashion (for example, a prone burial) or any evidence of a violent cause of death. This could suggest that society in general at this time tolerated those with leprosy or that they had not been detected and diagnosed and segregated (for reasons stated above). Nevertheless, one must also consider the challenges of interpreting disease from skeletal remains (Wood et al 1992). Other bioarchaeological studies have found the same picture, for example in Uzbekistan (Blau and Yagodin 2005) and during the crusading period in the Middle East (Mitchell 2000). Our Western views of past social attitudes toward leprosy, a subject with a complex picture even in developing countries today, are undoubtedly often assumed and may be inaccurate.

Conclusions

Any study of historical responses to disease "must venture far beyond the scrutiny of a particular virus or microbe and its impact on bone and tissue. It must embrace . . . the spiritual and intellectual *milieu* of the afflicted. . . . If the picture that emerges is sometimes contradictory, challenging and fragmented then it surely reflects the nature of human life itself" (Rawcliffe 2006:357). From the preceding dialogue, we have seen an infectious disease that remains prevalent in some parts of the world today and is often still poorly understood. Frequently people with leprosy are ostracized from their communities because they are stigmatized. Although there are exceptions, very little has changed over time from attitudes in late medieval Europe to today, despite better knowledge of both the clinical and social aspects of leprosy and the availability of successful treatment. However, this fact does illustrate, both from a contemporary and a bioarchaeological perspective, that people dealing with the leprous today and bioarchaeologists studying human remains of the past must consider the social milieu (in its very broadest sense) in which diseases thrive to understand the real impact of diseases on individuals and communities. Without a multidisciplinary and holistic approach, we cannot start to understand how our ancestors coped with devastating diseases.

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17

"Damnatio Ad Metallum": Investigating the Origin and Role of Phaeno Mining Camp Residents Using Multiple Chemical Techniques

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In 1977 Jane Buikstra coined the term *bioarchaeology* to refer to a multidisciplinary, problem-oriented, population-level perspective of human skeletal data (Buikstra 1977:69). One of the major tenets of bioarchaeological research was to consider multiple lines of evidence to understand the past, including utilizing different skeletal analysis techniques and considering historical, archaeological, cultural, and political contexts. In her work on the nineteenth-century Grafton Cemetery in Grafton, Illinois, Buikstra presented a critical assessment of how researchers working with historical populations incorporate textual data into their bioarchaeological results. She notes that bioarchaeologists rarely treat documentary data as fallible and biased and simply add their bioarchaeological interpretations to historical evidence (Buikstra 2000:16). Scholars instead should look to developments in anthropological archaeology and historical archaeology, which espouse a critical assessment of all data and the exploration of *why* the stories they tell may not match (Buikstra 2000:16; see also Perry 2007).

This historical-bioarchaeological case study exploring the role and origin of mining camp residents presents one example of combing historical and bioarchaeological data. Historical sources (e.g., Diodorus, *Library of History*; Pliny the Elder, *Natural History*) describe the success of the Roman and Byzantine empires in garnering and controlling resources from many points within their territories. Minerals recovered below the earth's surface were no exception. Mining camps under Roman and Byzantine imperial control were notoriously unpleasant, however. Who were the unfortunate souls sent to work in this context? Historical sources (e.g., Eusebius, *Ecclesiastical History* 8.8, 8.10; Eusebius, *Martyrs of Palestine* 7.4, 8.1, 13.1–3; Strabo, *Geography* 12.3.40) imply that a majority of individuals working in the mines were there for punitive reasons. Strabo also notes that the prisoners in the Mount Sandaracurgium mines in Anatolia were overseen by a *publicanus*, or public contractor (Strabo *Geography* 12.3.40). Other individuals affiliated with mining operations, and support staff as well prisoners, thus likely resided near the mines. In this paper, the historical information on life in Byzantine-period prison camps is combined with bone chemistry data derived from a cemetery associated with one of these camps in southern Jordan. Strontium isotope analysis and concentrations of copper and lead in skeletal systems are used to elucidate the administrative structure of Roman and Byzantine labor camps, in addition to the origin of the mine workers.

THE MINING CAMP AT PHAENO

The Phaeno mines are associated with the archaeological site of Faynan, along the eastern escarpment of Wadi Araba in southwestern Jordan (Figure 17.1). The 12 km^2 area surrounding Khirbet Faynan contains more than 250 copper mines that were extensively exploited from the fifth millennium B.C. until at least the thirteenth century A.D. (Figure 17.2) (Grattan et al. 2007; Hauptmann 2000; Hauptmann and Weisgerber 1987, 1992; Mattingly et al. 2007). A number of cemeteries from different periods, including the Byzantine-period (fourth-to-sixth-century A.D.) Southern Cemetery, partially excavated in 1996 (Abu-Keraki 2000; el-Najjar and al-Shiyab 1998; Findlater et al. 1998; Mattingly et al. 2007), are also associated with the site. Extensive looting had damaged approximately 40 percent of the estimated 1,200 graves in the Southern Cemetery (Findlater et al. 1998:71). Forty-five undisturbed primary burials were excavated during the 1996 season. Grave goods recovered with these burials include remnants of textile shrouds, leather sandals, and personal jewelry, with the exception of graves 105 and 107, which also contained glass vessels, wooden kohl tubes, and plaster disks (Findlater et al. 1998). Additionally, many burials were marked with grave stelae inscribed with personal names and/or Christian crosses.

Historical references describe the grim conditions for laborers at Roman and Byzantine mining camps such as Faynan. Eusebius of Caesarea famously details the fate of Christian martyrs at Phaeno during the great persecutions of the early fourth century A.D., describing their gouged-out eyes, cut Achilles tendons, and shackled and bound legs (Eusebius, *Ecclesiastical History* 8.8, 8.10; Eusebius, *Martyrs of Palestine* 7.4, 8.1, 13.1–3). This cheerless existence was not limited to early-fourth-century Christian prisoners. Diodorus of Sicily provided an even more dismal picture of second-century B.C. mines, describing miners, old and young, men and women, working continuously despite repeated beatings until they dropped dead (Diodorus, *Library* 18.69.2–69.9).

According to Eusebius, most of the fourth-century prisoners at Phaeno came from the Palaestina provinces in the Levantine region and Egypt. Condemnation to *metallum* in the Roman and Byzantine empires purportedly involved transportation of prisoners over long distances, imperial expenditures that reflect, to some scholars, the seriousness of their crimes (Millar 1984:139). We would therefore expect that some

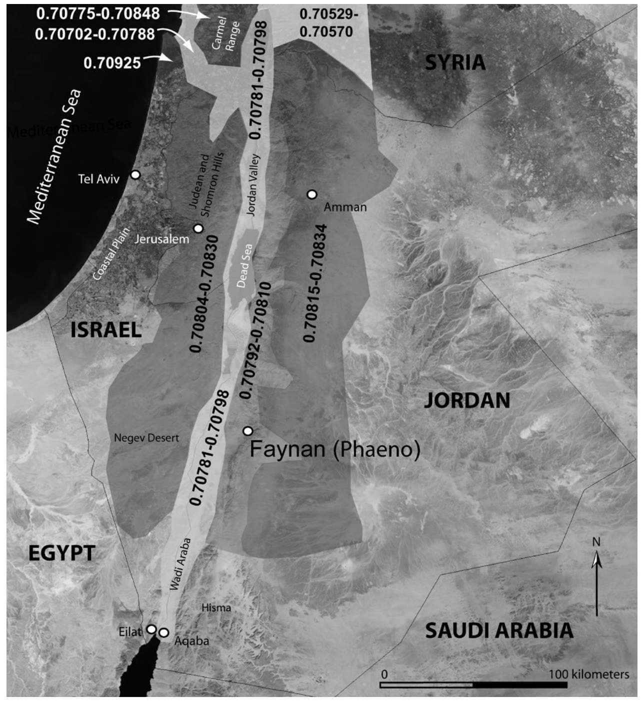

Figure 17.1. Map of the region showing the location of Khirbet Faynan (Phaeno) and $^{87}Sr/^{86}Sr$ variation based on samples of archaeological and modern fauna and modern flora (Perry et al. 2008; Shewan 2004). Unshaded areas of the map indicate insufficient strontium isotope data for these regions.

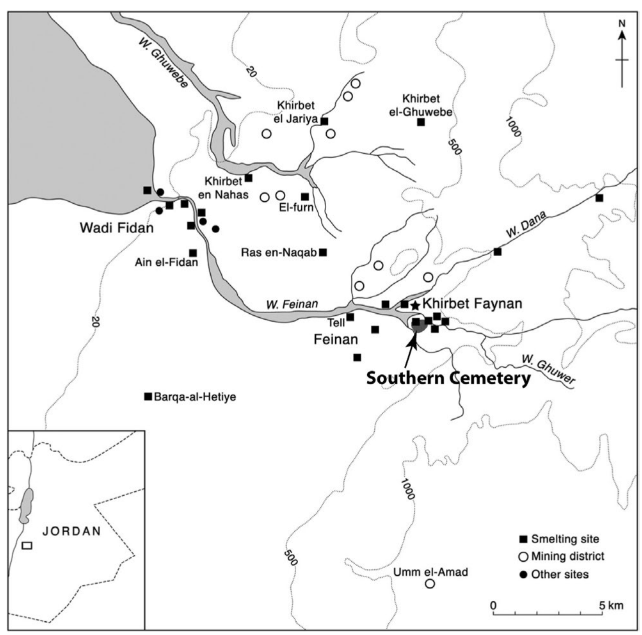

Figure 17.2. Map of Khirbet Faynan and Wadi Fidan, including location of the Southern Cemetery (from Grattan et al. 2007).

individuals in the Faynan cemetery were prisoners who had been transported to Phaeno from other regions. However, with a few exceptions, historical sources do not discuss other individuals who may have worked at the Phaeno camp, such as administrative staff, local laborers there for economic reasons, or their families.

Testing for levels of copper and lead in Faynan skeletal material by Grattan, Pyatt, and colleagues (Grattan et al. 2002; Pyatt et al. 2000, 2005) provides another indication of these individuals' roles in the mines. Bioaccumulation of lead and copper could have resulted from residing in contaminated areas surrounding the mines or from inhalation of metalliferous dusts and flue gasses generated by mining, quarrying, ore preparation, and smelting (Grattan et al. 2002, 2007; Pyatt et al. 1999, 2000). Even ancient writers recognized the health risks involved in mining activities (Lucretius, *De Rerum Natura* 6.808–815; Pliny, *Natural History* 31.49, 33.31; Strabo, *Geography* 12.3.40; Vitruvius, *De Architectura* 8.6.11). Faynan's unhealthy environment apparently persists to this day; as a result of ancient mining activity, toxic levels of copper, lead, and radon exist in local sheep and goats, bedouin tent floors, geological sediments, and plants (Grattan et al. 2003, 2004, 2007; Pyatt and Grattan 2002; Pyatt et al. 2000, 2005). Levels of copper and lead in the ancient skeletons suggest that 44 percent of these individuals were exposed to toxic concentrations of these heavy metals (Grattan et al. 2002). Tests of soil surrounding the burials (Grattan et al. 2002) and patterns of lead and copper partitioning throughout portions of the humerii and skeletons as a whole (Grattan et al. 2005) suggest that these levels do not reflect diagenetic contamination. Grattan, Pyatt, and colleagues thus surmise that individuals with high levels of lead and copper exposure actually had worked longer in the mines or resided longer at the site, while people with lower values died soon after arriving in the area.

The origins of individuals dying at Faynan and their possible role in the mines were explored through bioarchaeological data on exposure to toxic heavy metals and migration. If these individuals were mining camp prisoners, one would expect evidence for a nonlocal origin and *less* exposure to heavy metals due to childhoods spent in uncontaminated regions and death soon after arriving at the mining camp resulting from the poor working conditions described in historical sources. These data will illuminate the reality of Byzantine administrators regularly transporting criminals over long distances to mines such as Phaeno.

The Geologic Settling of Faynan

The site of Phaeno is located along the eastern edge of Wadi Araba, the southern portion of the Rift Valley system that dominates eastern Israel, the West Bank, and western Jordan. Faynan sits at the confluence of wadis Dana, Ghuwayr, and Shegar, which continue as Wadi Faynan into Wadi Araba. The sediments within these wadi systems vary slightly according to the bedrock lining each channel (Barker et al. 1998; Hunt et al. 2007; McLaren et al. 2004), although in general they are composed of Holocene-period alluvium, colluvium, and aeolian sediments eroded from Middle and Lower Cambrian dolomite limestone and shale, in addition to sandstone formations lining the escarpment and wadis (Bender 1974; Hunt et al. 2007).

Characterization of strontium isotopes based on archaeological faunal dental enamel shows that western Jordan can be split into three north–south zones: the Rift Valley ($^{87}Sr/^{86}Sr$ = .70781–.70786), the western highlands ($^{87}Sr/^{86}Sr$ = .70815–.70834), and the wadi and mountain systems in between the valley and the highlands ($^{87}Sr/^{86}Sr$ = .70792–.70810) (Perry et al. 2008). Faynan is situated within the geologically complex mountain and wadi systems that line the eastern escarpment of the Rift Valley, which includes the Jordan Valley, the Dead Sea basin, and Wadi Araba (Perry et al. 2009). The local range at the site was established through testing faunal dental enamel and snail shells recovered from a nearby site (Figure 17.3). Individuals with a $^{87}Sr/^{86}Sr$ signature outside of ± 2 standard deviations from the sample mean would therefore be identified as nonlocal.

Methods

To investigate migration into Faynan, dental enamel samples were collected from 31 burials from the Byzantine cemetery (Table 17.1). Additionally, one rodent dental enamel sample and eight land snail shells from Jebel Hamrat Fidan, about 10 km east of Khirbet Faynan, were used to establish a "local" $^{87}Sr/^{86}Sr$ value. Researchers have demonstrated homogeneity in land snail shell and faunal values within a site (Blum et al. 2000). Land snail shells were sampled from across Fidan to best reflect the local strontium, since land snails have a smaller home range than small mammals. The archaeological human and faunal dental enamel

Table 17.1. Results of $^{87}Sr/^{86}Sr$ Analysis of Archaeological Human Dental Enamel from Faynan.

Context	Age	Sex	Corrected $^{87}Sr/^{86}Sr$[1]	Tooth Sampled	Copper (μg/g)[2]	Lead (μg/g)[b]
Grave 11	35–39	F?	.707976	RM_1	9.8	1.8
Grave 12	50+	F	.707869	RM_1	8.6	112.3
Grave 25	40–44	M	.707849	M^1	109.1	170.0
Grave 63	35–39	F	.707992	LM_1	–	–
Grave 67	6 years ± 24 months	?	.707896	PM_1	181	289.2
Grave 69	Adult	?	.707979	RPM_2	17.1	27.6
Grave 70	25–29	F	.707854	RPM^2	5.0	1.0
Grave 71	20–25	F	.707906	LM^1	–	–
Grave 72	25–29	M	.708006	LM^1	3.0	13.0
Grave 73	30–34	F	.707971	UPM^2	296.2	19.1
Grave 75	45–49	F	.707939	RPM^1	7.0	42.0
Grave 78	7–9	?	.707975	?	20.0	12.8
Grave 80	35–39	F	.707978	RM^1	6.3	37.7
Grave 81	30–35	F	.708009	LM^1	7.0	27.9
Grave 83	45–49	F	.708004	LPM_2	11.0	28.9
Grave 84	35–39	F	.707926	PM	–	–
Grave 87	30–34	M	.708005	LPM^1	2.4	4.7
Grave 88	Adult	M?	.708030	LM^2	135.6	75.6
Grave 96	20–25	M	.707842	RM^1	90.7	44.3
Grave 97	20–24	F?	.707921	PM_1	5.7	13.7
Grave 99	3 years ± 12 months	?	.707952	Rm^1	–	–
Grave 100B	3 years ± 12 months	?	.708087	Lm^1	–	–
Grave 102	30–34	M	.708301	PM_1	27.5	75.6
Grave 104	7 years ± 24 months	?	.707755	LUc	43.1	17.01
Grave 105	10 year ± 30 months	?	.708006	RM_1	17.9	37.1
Grave 107	30–39	F	.707809	RM_1	–	–
Grave 108	Adult	?	.707925	LPM_1	–	–
Grave 109	25–29	M	.707853	LM^1	–	–
Grave 112	45–49	?	.707830	RPM_1	5.9	14.4
Grave 115	Adult	F	.707905	RM_1	–	–
Grave 117	30–35	F	.707913	RM^1	22.4	93.7

1. NBS 987 Standard
   Running mean .710270
   2σ standard deviation .000014

2. From Grattan et al. 2002.

samples were processed and mechanically cleaned in the Bioarchaeology Laboratory at East Carolina University. The 31 human dental enamel, one faunal dental enamel, and seven snail shell samples were then chemically cleaned, and the strontium was extracted and measured using a VG Micromass Sector 54 TIMS in quintuple-collector dynamic mode at the Isotope Geochemistry Laboratory in the Department of Geosciences at the University of North Carolina–Chapel Hill. Samples were prepared by dissolution in 7N HNO^3, dried, and redissolved in 3.5 N HNO^3 for ion-exchange column chromatography. Strontium was isolated using Sr-specific resin from Eichrom Technologies. Purified Sr was loaded on single Re filaments with TaCl and analyzed with ^{88}Sr = 3V (10^–11 ohm resistor) in triple dynamic mode. All Sr data are normalized to $^{86}Sr/^{88}Sr$ = .1194. Total procedural Sr blanks during these analyses was less than 30 pg Sr. The UNC–Chapel Hill ratios are reported relative to a value of .710270 ± .000014 (2σ) for the NBS 987 standard. Internal precision for strontium runs is typically ± .00012 to ± .00018 percent (2σ) based on 100 dynamic cycles of data collection. Fourier transform infrared (FTIR) spectral analysis on 14 randomly selected teeth discovered that tooth microstructure is minimally altered and that isotopic data are unlikely to be modified by diagenesis (see Perry et al. 2009).

Results

The estimated local $^{87}Sr/^{86}Sr$ signature at Faynan based on the faunal bone and shell sample mean (± 2σ) ranges between .707929 and .708144 (Table 17.2) (see also Perry et al. 2009). While less than half of the tested human samples fall under this range, there is no distinct cutoff in the distribution of these values indicating clear "local" versus "nonlocal" individuals (Figure 17.3). Therefore, it is likely that the faunal values do not accurately reflect the full range of Sr isotope variation at Faynan and that most of these individuals are local. Removing the one outlier (Grave 102) from the sample results in a normal distribution with no outliers (see Perry et al. 2009).

It thus appears that the individual from Grave 102 is the only nonlocal individual in the sample and that the other burials contain locally derived residents of Faynan. The Grave 102 burial had a $^{87}Sr/^{86}Sr$ value much higher than the local range, one mirroring strontium isotope ratios from the coastal region and foothills of the Galilee Range in Israel (Perry et al. 2008; Shewan 2004), although presumably other areas in the Levant have this value as well.

Copper and lead skeletal concentrations were compared with $^{87}Sr/^{86}Sr$ values to determine if local versus nonlocal origin could predict the exposure level of

Table 17.2. Results of $^{87}Sr/^{86}Sr$ Analysis of Archaeological Fauna from Jebel Hamrat Fidan.

Context	Corrected $^{87}Sr/^{86}Sr$[1]	Sample Type
1999 H 120 L. 1039 B.14345	.708071	snail shell
1999 H 120 L. 1052 B. 149412	.708005	snail shell
1999 Y 120 L. 1248 B. 35129	.707984	snail shell
2000 H 120 L. 2081 B. 56282	.707948	snail shell
2000 H 120 L. 2100 B. 56850	.708040	snail shell
2000 H 120 L. 2106 B. 57089	.708085	snail shell
2000 H120 L. 2107 B.57107	.708105	snail shell
Area C, Grave 701	.708053	rodent dental enamel

1. NBS 987 Standard
Running mean .710270
2σ standard deviation .000014

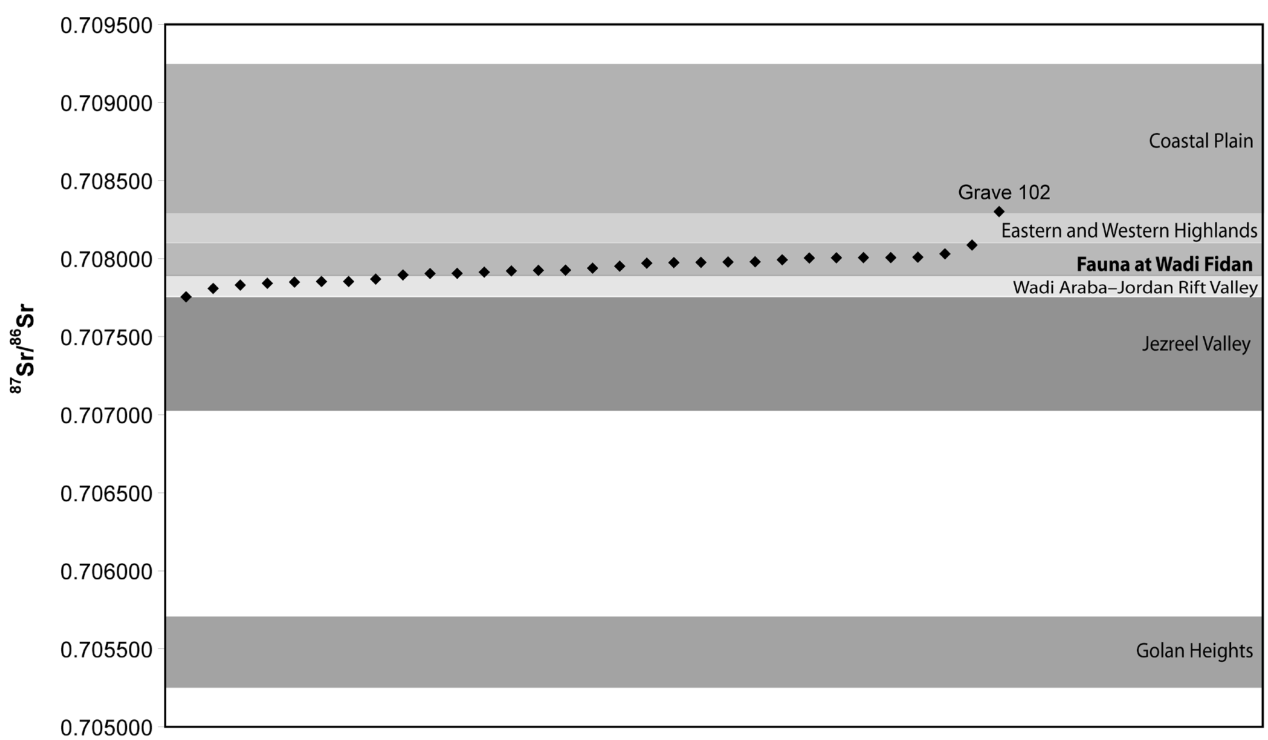

Figure 17.3. $^{87}Sr/^{86}Sr$ values of burials from Faynan compared with the range established by local archaeological faunal samples from Jebel Hamrat Fidan.

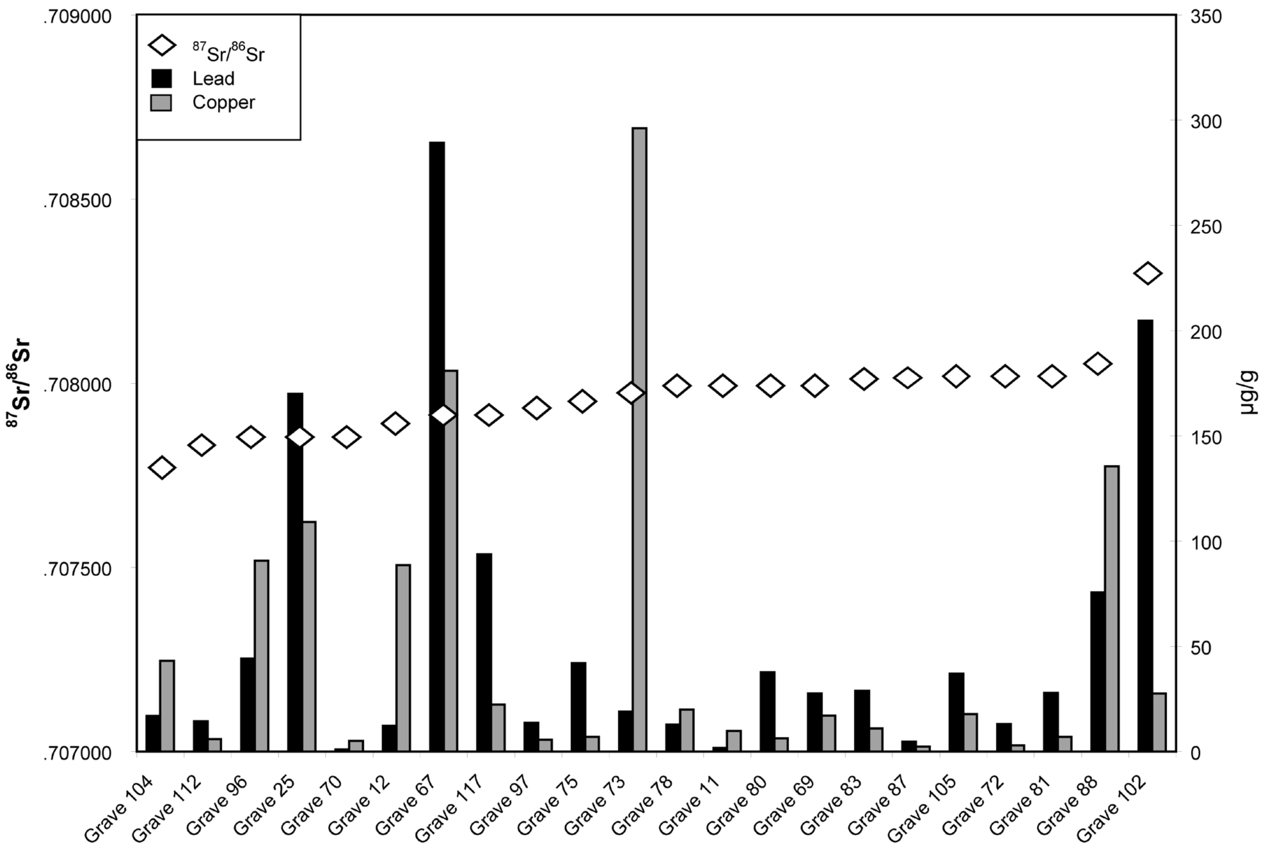

Figure 17.4. Archaeological human $^{87}Sr/^{86}Sr$ values from Faynan compared with skeletal copper and lead concentrations.

individuals to the mining environment based on their lead and copper skeletal levels (Figure 17.4). The one nonlocal individual had the second-highest lead level in the sample and a slightly elevated copper level. The local individuals, on the other hand, had a range of copper and lead levels in their skeletons. Some individuals had slightly elevated levels; others had extremely high amounts compared with a reference sample mean (see Grattan et al. 2002:301). Compared with a reference sample with "normal" copper and lead levels, 10 individuals from Faynan had copper levels greater than 2 standard deviations of the reference sample mean, and 22 had lead levels more than 2 standard deviations of the reference sample mean. These data imply that individuals of local origin carried out diverse roles at the mining camp that resulted in varied exposure to environmental pollutants.

Discussion

The Faynan sample was expected to contain a mix of locally and nonlocally born individuals. Many individuals residing at the site, however, remain invisible in historical sources. The camp certainly contained administrative staff or free mine laborers and their families, and possibly the families of prisoners. The fourth-to-sixth-century A.D. cemetery, for instance, contains children under the age of seven years, whom Byzantine courts would have exempted from many crimes (Prinzing 2006; Robinson 1995). They thus lived at Faynan because their parents resided there for employment or punishment.

Analysis of the strontium isotope data reveals only one nonlocal individual in the sample, possibly originating from the Mediterranean coast, the origin of many Phaeno prisoners during the early fourth century according to historical sources. Most burials instead likely are of individuals originating from the areas immediately surrounding Faynan or other geologically similar areas. Therefore, the most parsimonious explanation is that during the fourth to sixth centuries, Faynan residents primarily were locally born individuals, there for economic or social rather than punitive reasons. The Byzantine Empire in the later fourth century additionally may have considered the transport of prisoners from afar to the mines an unnecessary expense and sent only local miscreants to Phaeno for punishment.

These data further suggest that local individuals had varied exposure to copper and lead contaminants. Investigations at modern Faynan identified an environment markedly enriched with copper, lead, and other metals that presumably existed in antiquity (Grattan et al. 2003, 2007; Pyatt et al. 1999, 2000, 2002), suggesting that most residents of Faynan would have been exposed to a polluted environment. In fact, most Faynan individuals in this sample had elevated skeletal copper and lead concentrations, and 21 individuals had extremely elevated copper and/or lead levels. This may indicate that while everyone who lived at Faynan had slightly elevated copper and lead levels due to environmental exposure, some residents were particularly susceptible due to their direct involvement in industrial mining activities. Grattan and colleagues (2007) recently reported that varied patterns of anthropogenic, aeolian, and colluvial deposition of heavy metal pollution resulted in heterogeneous intrasite environmental copper and lead concentrations. Therefore, some differences in lead and copper concentrations should exist between site residents.

The children at Faynan displayed particularly toxic concentrations of copper and lead. Although no children under the age of 6 (± 24 months) were tested for lead and copper concentrations, three out of four children between 6 (± 24 months) and 10 (± 30 months) had extremely elevated lead levels, and half had enhanced copper levels. These high concentrations could have resulted from the primarily gastrointestinal absorption of contamination seen in children (e.g., Carrizales et al. 2006; Paoliello et al. 2002; Simon et al. 2007) or from the higher absorption of lead into the body by children's blood compared with adults' blood (Leggett 1993; O'Flaherty 1995). Another possibility is that the less mineralized and more porous subadult bones could be affected by digenetic contamination reflected as higher Cu or Pb levels (e.g., Guy et al. 1997; Lambert et al. 1979; Sandford et al. 1988). None of these individuals were tested by Grattan and collegues for diagenesis via assessing the partitioning of Cu or Pb in the humerus or soil samples from the grave context (Grattan et al. 2002). The presence of enhanced copper and lead concentrations in juvenile skeletons must be interpreted with caution.

The Faynan labor structure therefore included individuals not directly involved with industrial activities. These primarily locally born individuals could have been involved in administration or logistical support, which put them at decreased risk for lead or copper exposure. For instance, an extensive survey of agricultural fields surrounding Khirbet Faynan discovered that after the Roman period, fields were managed together, likely at the state level, for feeding administrative personnel and the labor force (Barker et al. 2000). The ancient sources' focus on the prisoners sent to mining camps therefore does not fully represent the labor structure of these camps. Emphasis on the terrible fate to befall someone sent to the mines likely served to instill fear and obedience in potential criminals or to glorify the sacrifices of Christian martyrs.

The role that individuals buried in the Faynan cemetery played in the Phaeno mining camps increasingly emerges through bioarchaeological analyses. Historical sources emphasize the grim reality of life in the mines, which frequently served as the final resting place for slaves and lower-class convicts in the Byzantine East. Researchers assume that burials in the Byzantine cemetery contain prisoners sentenced to the mines under circumstances similar to Eusebius's description. Bone chemistry tells a different story, however.

Researchers suggest that residence in the area would lead to elevated bioaccumulation of heavy metals due to the contaminated environment. Instead, individuals identified as "local" by their $^{87}Sr/^{86}Sr$ signature had varied lead levels and copper levels. This suggests that increased bioaccumulation of copper results from greater exposure to the mining environment rather than simply residing at Faynan. Strontium isotope data also do not identify wide-scale transport of prisoners to the mine, although punishment of locals at Phaeno remains a possibility. The cemetery therefore contains individuals with varied roles at the site.

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18

Taphonomy and Funerary Practices in Collective Cemeteries:

A Prehistoric Case from Menorca (Balearic Islands, Spain)

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This chapter deals with "collective burials" as a category applied to the treatment and final disposal of the dead. These burials are characterized by the following main archaeological features: the absence of individual funerary containers and a lack of any apparent order or patterning in the spatial distribution of skeletal material.

Millennia of funerary activity in many European caves and rock shelters have produced thousands of archaeological sites from different chronological periods where remains of several dozen or even hundreds of people are mixed together. These sites include not only natural or artificially made caves (hypogea, or rock-cut tombs) but also megaliths and other monuments. Many of these graveyards, spanning from the Neolithic until historical times, have been interpreted as ossuaries, implying the accumulation of secondary inhumations. Needless to say, most of them were discovered and excavated a long time ago, with little sensitive attention paid to the recovery of material remains other than artifacts and with sketchy stratigraphic/contextual information. However, more recent works have also failed to provide a proper record regarding the treatment and disposal of the dead through the time span of cemetery use. As a result, categories of mortuary analysis end up being based more upon assumptions and speculation than on positive evidence. On the other hand, collective burials have traditionally been conceived as an attribute of, in processual terms, nonstratified societies, also called small-scale segmentary societies (Renfrew 1976). The term *collective* is often understood as synonymous with *homogeneous*, and other possible dimensions of variability are frequently neglected. Clearly, this oversimplification will hardly help us explore the expected wide array of social organizations developed by extinct societies.

Our perspective, which aims at searching for meaningful patterns, has a lot to do with the pioneering work of Jane E. Buikstra in promoting an integrated bioarchaeological approach to the study of funerary practices. Our research projects on Argaric Bronze Age societies in southeastern Spain (Buikstra et al. 1990, 1995; Castro et al. 1999; Lull 2000; Lull et al. 2005) have benefited much from her expertise and collaboration. In the case study we present here, her role as the mentor of one of us (C. Rihuete) in osteological analysis has had a major influence in shaping the general approach to the funerary record under consideration.

Càrritx Cave

Menorca is located in the western Mediterranean and is the northernmost island of the Balearic Islands. Although it was peopled relatively late (by the end of the third millennium B.C.), it is extremely rich in archaeological sites. (For an updated review of the prehistory of the island, see Guerrero 2007; Micó 2005, 2006.) Menorca's geography is characterized by a flat landscape with a series of gorges running southward and with steep cliffs hollowed by numerous caves, either natural or artificially made. Difficult access, lack of close spatial relationships among settlements, and ritual use throughout prehistory are constant features in the cave record of the island.

The Càrritx Cave (Figure 18.1), lying some 30 m above the basin of the Algendar Gorge, fits the pattern of a natural cave whose entrance had been closed by what is traditionally known as a cyclopean wall, leaving a narrow space for a doorway as the only possible access. It was discovered by chance in 1995, and results of the subsequent research program have been fully published (Lull et al. 1999a, 1999b, 2002; Rihuete 2003a, 2003b, 2005). Geostratigraphical analysis has demonstrated that a collapse of the rock wall above the entrance, which happened not too long after the final abandonment of the necropolis, sealed off the archaeological deposit and contributed to the extraordinary state of preservation at the site.

The cave, with a total length of 210 m, has seven different chambers, but the cemetery was restricted to the first one: a two-level space of 32 m^2 (Figure 18.2). This funerary space contained thousands of human bones along with scattered pottery fragments, faunal and botanic remains, items of clothing, tools, and ornaments that had accumulated over 600 years (circa 1450–850 cal B.C.).

Human remains were primarily disarticulated. The total collection comprises almost 35,000 elements from 210 individuals (MNI) corresponding to a normal demographic profile (both sexes and all age categories

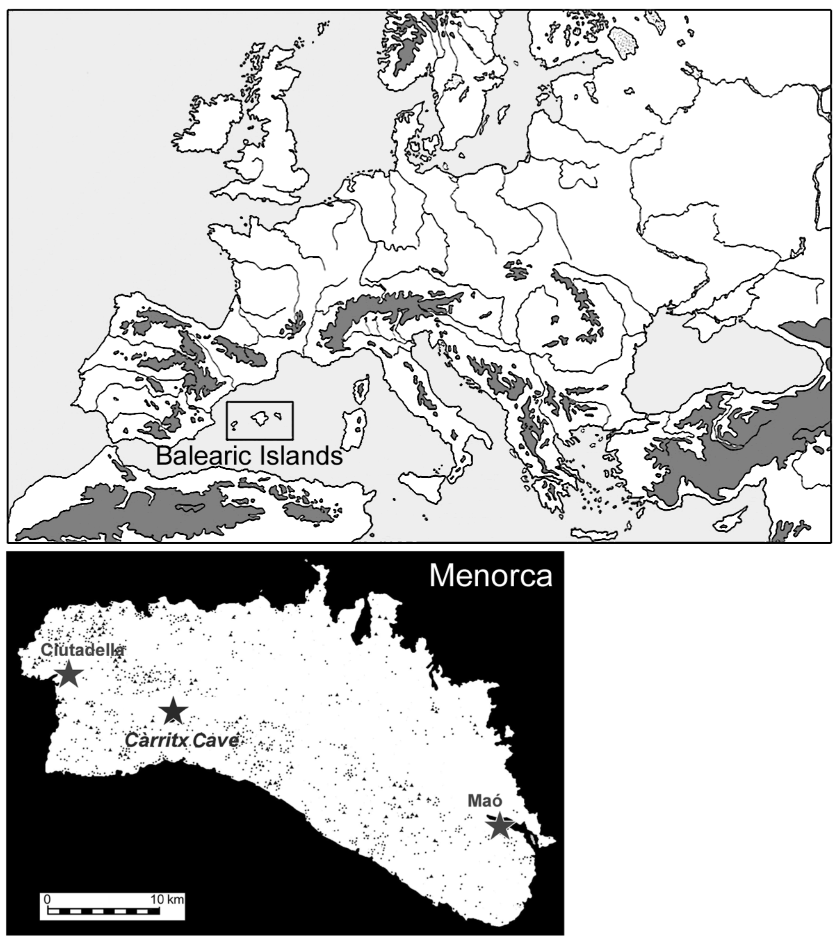

Figure 18.1. Location of Càrritx Cave.

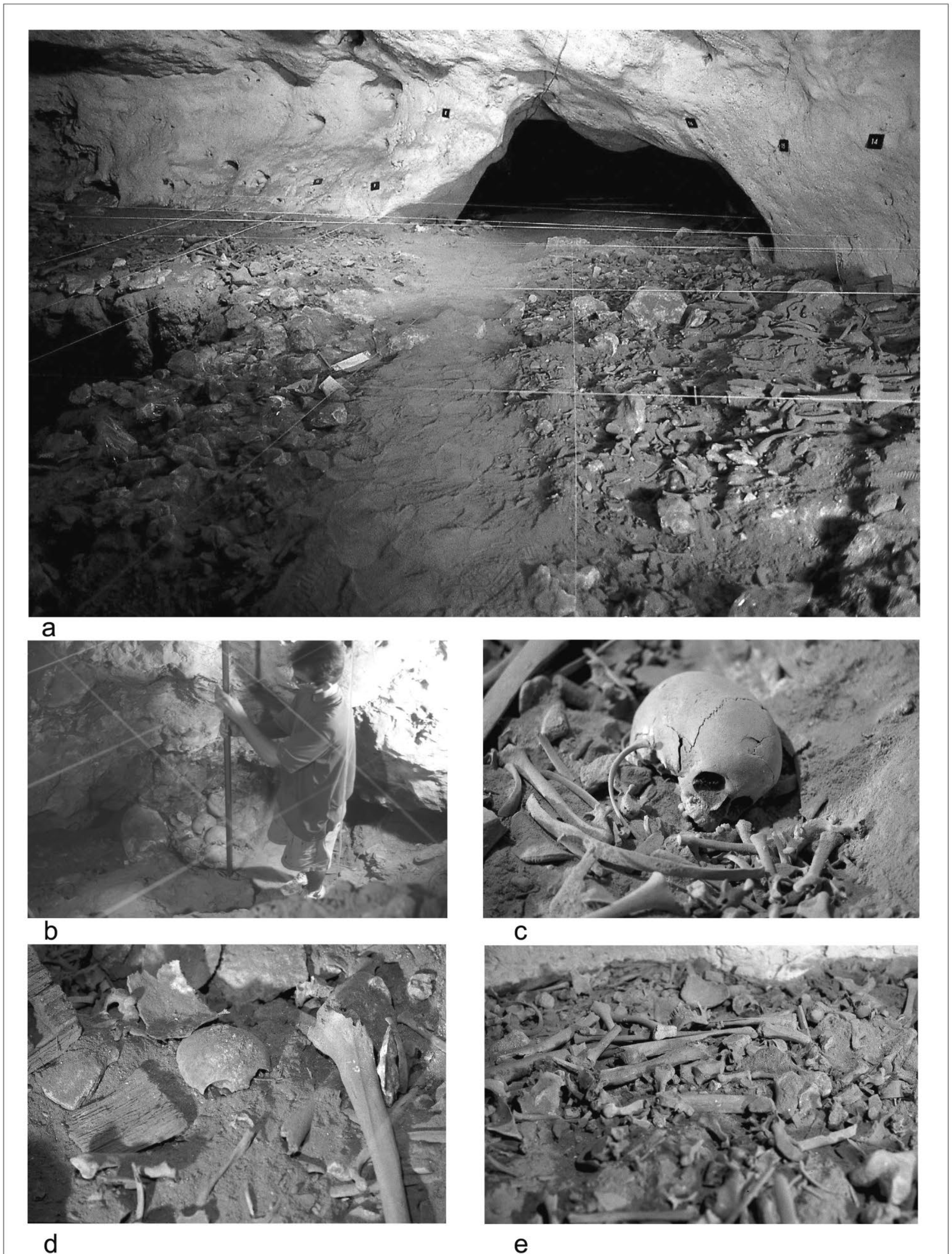

Figure 18.2. Burial chamber: (a) general view of the upper level; (b) lower level with cluster of skulls; (c–e) different aspects of the burial chamber upon its discovery.

included, except for fetuses and neonates). Some bones were burned, others appeared partially articulated, and still others, mainly skulls, were removed from their original locations and grouped together. The questions posed by these evidences can be summarized as follows:

- Were there multiple choices of burial?
- Was cremation an alternative to inhumation?
- Were commingled remains the product of spaced but continuous primary interments or the expected result of secondary inhumation in its proper sense?
- If body parts had been segregated and relocated, would that account for a differentiation among individuals?
- Were there any changes through time that would explain changes in mortuary procedures?

Cremation Versus Inhumation

In the Balearic Islands, cremation has been reported for other sites. The most well-known example comes from Son Matge (Mallorca), a rockshelter closed with a cyclopean wall during its use as a collective graveyard by the end of the second millennium cal B.C. (Waldren 1982). Some burned bones, albeit in low frequencies (2.8 percent), along with burned archaeological artifacts, charred seeds, and charcoal, were found in Càrritx. Therefore, the question arose as to whether cremations had taken place as an alternative to inhumation, either as a new type of rite or as a treatment restricted to certain individuals, or if burning of body parts was intended to accelerate decomposition as part of a process of secondary deposition.

Bone surfaces and lines of fracture showed a great variation in color, from dark brown to gray and white, which indicates different degrees of heat intensity (Etxeberría 1994). However, all burned bones showed the striation patterning typical of dry state conditions (Botella et al. 1999; Guillon 1986,), meaning that in all cases fire had acted upon already skeletonized remains. Therefore, bodies were not cremated. On the contrary, artifact, botanic, and spatial data offer a rather different explanation: accidental contact with fire. The small hearth located right at the entrance provided the first clue for this interpretation. There was also enough evidence of lighting devices in the form of wooden torches and pottery lamps scattered among the human remains. Finally, the identification of a wide array of bushes and aromatic plants, such as *Rosmarinus officinalis*, *Malva* sp., *Silene* cf. *gallica*, *Cistus* sp., and *Pistacea lentiscus* (Stika 1999), provides further support for the performance of rituals where fire must have played a prominent role in providing smoke, specific scents, and other essences.

Primary or Secondary Burials?

Now that it has been established that inhumation was the rule, let's turn to the ossuary status of the cemetery. It is true that human remains were primarily disarticulated, but body part representation (with high frequencies of hand/feet elements and other bones such as the hyoid) and the complete absence of defleshing marks are indeed sound indicators that the cave was a locus for primary inhumations. Moreover, the remains of three individuals in anatomical position (two adult males and a child, all of them located at distant points of the cemetery) also indicate that full bodies were brought into the cave.

In a situation of multiple, successive burials, we would expect the removal of previously skeletonized remains, and in that case the three skeletons preserved in anatomical position would have been the last corpses taken into the burial chamber. This supposition was contradicted by the results from absolute dating, since those individuals stand as the oldest ones for the whole cemetery, and, most interesting, two of them had their skulls and lower maxillae in place. The importance of this observation lies in the fact that many skulls (n = 32) had been carefully lined up at the junction of the floor and cave walls, sometimes arranged in rows four skulls deep. No other meaningful selection of skeletal items could be identified. The skulls correspond to both men and women and all age categories, and not a single one had its mandible in place, which means that heads were fully skeletonized when they were moved from their original positions.

As noted earlier, systematic osteological analysis of the whole skeletal sample in search of decapitation, disarticulation, and any other type of defleshing marks has produced not a single positive instance. Contrary to that, animal activity is confirmed (carnivore/scavengers and rodents), but in very low frequencies (2.4 and .1 percent, respectively) and spatially restricted, probably due to some occasions in which the cave had remained accidentally open. On the other hand, skulls appeared without mandibles, a body part that would have come along if removal of the head had taken place in a fresh condition. Thus the most likely explanation is that segregation and relocation of skulls against the cave walls were performed after natural decomposition and imply a new ritual episode that had to take place sometime after corpses had been transferred to the cave. Environmental conditions of the burial chamber such as high humidity levels (90 percent on average) and darkness (guaranteed by the closing wall and also inferred by the presence of nocturnal birds of prey), as well as clothing worn by the deceased (attested by V-perforated buttons), would have contributed to a more or less rapid disintegration of soft tissues, as forensic taphonomy has shown in comparative cases (Clark et al. 1997).

Finally, a careful spatial analysis of all archaeological materials supports the idea that successive depositions had produced a horizontal stratigraphy expanding from the area of prime exposure, which was identified by means of differential distribution and fragmentation frequencies at the northwestern corner. Funerary practices themselves were, then, the prime agent responsible for the general state of disarticulation. Full corpses were brought in the cave, but mortuary procedures involved new rites performed on the skeletons and focusing upon their heads.

STATIC VERSUS DYNAMIC: DIACHRONIC CHANGES

A series of 19 ^{14}C dates sampled from skeletal material of 18 different individuals (almost 10 percent of the MNI) shows that the cemetery was used from around 1450 to 850 cal B.C. (Figure 18.3). Uniform distribution of dates and lack of hiatus can be taken as an indication of a slow, continuous rate of deposition, without episodes of crises (higher death rates) or abandonment. Sex and age-at-death distributions account for a normal sample, and there are no archaeological reasons to believe that the cemetery is anything other than the accumulation of the deceased from a single community, either kinship or territorially based.

Contemporary settlements are found in open-air cyclopean boat-shaped structures (*naviformes*), most suitable for small residence groups. These dwellings

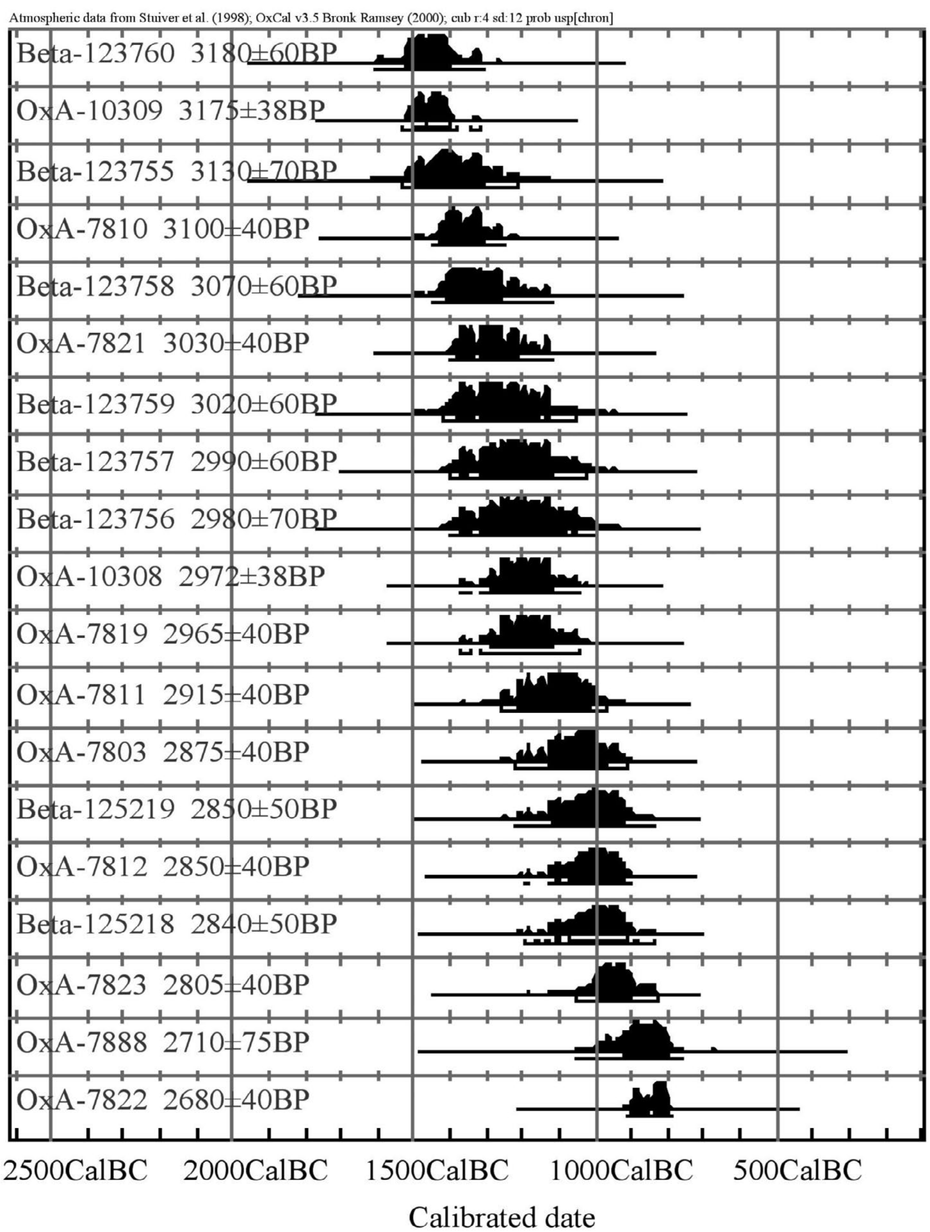

Figure 18.3. Graph of Càrritx ^{14}C probability sum from skeletal samples.

show a process of aggregation/nucleation ending up, by the middle of the eleventh century cal B.C., in new types of villages that incorporate the first attempts at monumental supradomestic towerlike buildings (Gili et al. 2006; Lull et al. 1999b, 2004). Throughout this period of 600 years there is not a single settlement unequivocally connected to its cemetery, and funerary sites display a great typological variability: natural caves, walled caves, rock-cut tombs, and open-air structures (tumular graves and *navetes*). The reasons for that variability aren't quite understood yet, especially because no detailed data on funerary practices are available. However, the sites all seem to have been used as places for communal burial and provide some interesting comparative data for the investigation of other possible dimensions of funerary variability.

In Càrritx, absolute dating and horizontal stratigraphy strongly support the idea that removal and relocation of skulls, a practice that emphasizes at a symbolical level the main physical attribute of the individual (the head), was not performed from the beginning but was introduced by the turn of the millennium. Other sites of similar recent date, such as *navetes*, have been reported as having groups of skulls clustered against chamber walls (Plantalamor and Sastre 1991:165; Veny 1976:228, 1987:447), whereas some walled caves not used after the end of the second millennium (Forat de Ses Aritges) lack evidence of this practice (Lull et al. 1999b:183).

Further support for newly introduced rites matching the chronology of changing settlement patterns and related social transformations by about 1100/1000 cal B.C. is also to be found in Càrritx. In a hidden place some 80 m away from the cave entrance, an amazing set of objects was discovered. These again focused upon the head of the deceased, but now in relation to a few individuals whose hair was dyed, cut, and finally stored in specific containers (Figure 18.4). The wealth of information retrieved from this context has a lot to do with the preservation of organic materials, namely wooden and horn artifacts, as well as human hair itself, but also with a thorough study of different sorts of evidence that allowed linking both chronologically and functionally the cemetery to the cache during the last centuries of its use. Hair and wooden containers from Càrritx were sampled for ^{14}C dating. The results provide further proof about rituals performed on the head of the deceased being introduced by the end of the second millennium. Hair containers of the same type have recently been discovered at another Menorcan burial cave, with unusual preservation of organic matter (Guerrero 2007:184). Carved lids for those containers, but made out of bone, were already known in other contemporary Balearic contexts from Menorca—Calescoves (Veny 1982) and Naveta des Tudons (Pericot 1972:79)—and from Mallorca—Son Matge (Waldren 1982). They all seem to demonstrate that hair manipulation of the deceased was not the rare habit of just one community but part of the normative rituals restricted to certain individuals and performed by the Balearic society in a time of deep social change.

Finally, the same chronological differentiation applies to a set of grave goods, namely a small number of bronze, iron, and lead ornaments found in Càrritx. Again, they all date to the last centuries of the cemetery's use, and again their association is not to an anonymous collectivity but to some specific individuals who seem to have deserved a new, special treatment unknown in previous times.

Conclusions

Following Hertz's seminal essay (1907), "secondary interments" is the corresponding category frequently used to account for mixed, disarticulated human remains in cave burials. However, a research strategy designed to account for taphonomic processes and different dimensions of variability might challenge the assumed homogeneity implied in those interpretations.

In our case study, even though collective inhumation was a standing tradition throughout the cemetery's history, uniformity in the treatment of the dead shifted to a pattern of individual differentiation whose ultimate explanation has to come from nonfunerary contexts. At a certain point, the community probably experienced remarkable changes that might have even implied a change of residence, but it continued burying its members in the same graveyard. Nevertheless, the introduction of new ritual practices in a context of multiple-stage burials seems a reliable symptom of the disintegration of the former social order.

As has been shown, research on collective burials must face the investigation of site formation processes *and* should never forget that any social practice is historically determined. Furthermore, it is our opinion that this perspective should be considered as a prerequisite of any serious investigation aimed at: (a) moving from intrasite analysis to regional scale studies and (b) providing an adequate data set for a genuine archaeological assessment of the social, political, and economical organization of past societies.

Figure 18.4. Some of the objects from the cache. Hairdressing tools: (a) wooden bowl and spoon; (b) wooden comb; (c) bronze razor. Hair containers: (d) horn container with dyed hair still inside; (e) container lids.

Acknowledgments

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19

Immigrant and Indigenous: Colonial Encounters in Early Historic Ireland

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Archaeological analyses of ancient colonial encounters tended until relatively recently to be modeled on European expansion of the post-Columbian era. This fixation on relatively recent experiences resulted in representations of colonial relationships characterized by asymmetry. Interactions between immigrant and indigenous were typically understood in terms of significant imbalances of power. A mismatch was also central to understandings of the relationship between points of origin and destination, which tended to be portrayed as metropolitan core and colonial periphery, with the former usually identified as the innovator and exporter of cultural influences and the latter as a passive consumer of such. Recent scholarship (e.g., Dietler 2005, 2009; Gosden 2004; Stein 2005) has suggested that this focus on the interactions of the last few centuries is inadequate as a means of understanding colonial encounters in earlier periods. Dietler (2005, 2009), for example, has made the fascinating suggestion that these approaches in part reflect the colonization of modern Euro-American consciousness by the classical world as a result of the central place given to the classics in the education of European and American elites over the last five centuries. He has further argued that the practices of recent colonialism grew out of selective interpretations of the narratives of the imperial expansions of classical Greece and Rome and that the resulting imbrications of ancient and modern colonial situations present archaeologists with considerable challenges in reconceptualizing earlier interactions.

Dealing with these challenges involves explicit theorization and a reflexive approach to implicit assumptions that can condition interpretation. Dietler (2005) also stressed the active role of material culture and consumption in past encounters where novel cultural sensibilities were negotiated, primarily through habituated practices. He focused on the importance of historical contingency and exhorted archaeologists to examine the diversity and uniqueness of responses among both immigrant and indigenous in colonial encounters that predate what he referred to as modern Euro-American colonialism. Gosden (2004) has also stressed the role played by material culture and has argued that colonialism is produced by a desire for things that grips the minds and bodies of people, moving them across space and producing power structures to fulfill this desire. Other recent works have focused on the body as a contact zone in this process and on the role of the intimate in the construction of colonial self (e.g., Ballantyne and Burton 2005; Creed and Hoorn 2001; Stoler 2002).

Bioarchaeology, as defined by Jane E. Buikstra (1977:69; Buikstra and Beck 2006), involving as it does the contextualized analysis of the remains of individual participants in past social processes, provides an ideal lens with which to view the complex relationship between material culture, bodies, and the movement of both through space that characterized colonial encounters. Specifically, the analysis of biological distance by means of the study of patterns of cranial nonmetric traits in archaeologically retrieved skeletons can be used to reveal patterns of morphological variability between groups. As the work of Buikstra and others has shown, morphological variability is related to genetic variability, albeit not in a linear manner (Cheverud and Buikstra 1981; Saunders 1989; Saunders and Popovich 1978), and can be used to discriminate between different populations (Stojanowski this volume; Stojanowski and Schillaci 2006). In this chapter, we use morphological variability as a basis for discussion of the nature of the relationship between immigrant and indigenous groups in Ireland during the period of Viking expansion between the end of the eighth century and the twelfth century A.D. In keeping with the contextualized approach advocated by Buikstra, this variability is considered in combination with historical and material culture data to draw inferences about the nature of the encounter between native and newcomer. In keeping with Dietler's exhortation, we begin with a brief examination of the historiography of constructions of "Irish" and "Viking" in the archaeological discourse of Ireland. Given the tainted history of physical anthropology in the construction of colonial–imperial relations, such a critical approach is particularly important in a context that considers the biological remains of past populations. Studies of the genetic affiliations of earlier "peoples" that proceed without explicitly theorizing concepts such as "Irish," "Gaelic," and "Viking" run the risk of replicating essentialist conceptualizations and perpetuating the colonial worldview. A critical approach can contribute to unmasking the prejudices and assumptions that have created dominant visions of the past. It can thereby contribute to a more well-rounded understanding of the dynamics of earlier societies while also challenging the legitimacy of ideologies that have assumed the mantle of universal truths.

Situating Irish and Viking

The use of omnibus labels such as *Irish* and *Viking* to describe early medieval societies is potentially problematic. These labels promote a view of homogenous, bounded, and self-identifying populations and project this view into the past. The term *Irish* is used here to refer to the population that was extant on the island of Ireland on the eve of direct contact with Scandinavian peoples in the eighth century A.D. The population of Ireland is not conceptualized in terms of unilineal descent from any particular earlier population that can be defined in terms of language, material culture, or biology. The population of the island is not considered as a bounded or isolated group, as significant cultural interchanges have been documented in the Irish Sea area, while contacts along the Atlantic facade of Europe may also have been significant. It is likely that these interactions facilitated both acculturation and gene flow, so it would be unsafe to assume that the island's precontact population was culturally or genetically homogenous.

We use *Viking* and *Norse* here interchangeably to refer to peoples of Scandinavian origin who settled in many areas of western Europe between the end of the eighth century and the eleventh century A.D. These terms are understood to indicate heterogeneous groups from a wide geographical area who may also have had considerable interactions with other European populations prior to and during their interactions with populations in Ireland. Contemporary early historic sources in Ireland recognized the heterogeneity of the Vikings. Since the nineteenth century, this heterogeneity has been explained in terms of diversity of origins within Scandinavia (Norway versus Denmark), but it is recognized that other processes may also have contributed to it. These could include the possibility of gene flow as a result of contacts with other European populations, while the ranks of the Vikings may have also been swelled by the accretion, either voluntary or coercive, of personnel drawn from a variety of such populations.

The earliest historical reference to contact between the inhabitants of Ireland and Scandinavian peoples is from A.D. 795, when an entry in the *Annals of Ulster* mentions that the monastery at Rechru had been raided by "heathens" (Mac Airt and Mac Niocaill 1983). The *Annals of Ulster* form part of the corpus of record keeping by early medieval clerics in Ireland, and in recent centuries this body of literature came to the fore in the formation of ideas about the Vikings in Ireland. Ambrosiani (1998) has written of the tendency among non-Scandinavian historians and archaeologists, particularly those from Ireland and Britain, to consider Scandinavia as a single, homogenous unit during the Viking Age. He has argued that this represents a misunderstanding of the cultural, linguistic, and political diversity of the region in the early historic period and reflects the homogenizing influence of the dominant image of the Vikings in the historiography of Ireland, Britain, and France prior to the 1970s: the demonic "other." The success and longevity of this image is a classic instance of where the pen has proved to be mightier than the sword in that the dominant perception of "Viking" in the Anglophone and Francophone worlds is the one promoted by early medieval clerics in the partisan and ideologically charged historical record they left behind. That record was mostly forgotten in the centuries following the reconquest of Ireland by England at the time of the Reformation and the imposition of a new colonial ruling class. Yet prior to the 1840s, most archaeological monuments in Ireland were attributed to the Danes. This was part of the colonialist narration of the Irish past, where external agents were recruited to explain cultural innovation and historical change. As a model of the past, that involving the Danes was one imported directly from Britain, specifically England, where early historic interactions with Scandinavian peoples primarily involved groups of Danish origin. The use of *Dane* as a synonym for *Viking* served to universalize the English/Danish experience, and the process of Scandinavian interactions with the early historic population of Ireland was assumed to mirror the experience of Anglo-Saxon England. Traditional attributions of archaeological monuments to the Danes were first challenged in the 1840s with the editing and publication of the more substantial of the early historic Irish ecclesiastical manuscripts. The political import of the accounts they contained was immediately recognized. In 1843 the constitutional nationalist leader Daniel O'Connell eulogized the Irish struggle against the Vikings as a role model for his generation's struggle against British domination. This nationalist interpretation was to form a strong current in the mainstream of historical research for nearly a century and a half after O'Connell's time.

In the twentieth century, archaeology played a central role in the emergence of revisionist narratives of Irish–Viking interactions. Excavations of the Viking core of the city of Dublin began in 1962 under the auspices of the National Museum of Ireland. The first serious reappraisal of the nationalist-inspired view of the Vikings came from an archaeologist, A. T. Lucas, who was intimately connected with the Dublin excavations as director of the National Museum (Lucas 1966, 1967).

The archaeological data accrued since the 1960s indicate that unlike the situation in Britain and northern France, Viking territorial gains in Ireland were limited to a small number of coastal enclaves, and the nucleated settlements established at these footholds are considered the first truly urban communities on the island. Urbanism was in its nascent stages in contemporary Scandinavia but would have been encountered by Viking groups elsewhere in western Europe. The Irish coastal towns were integrated into wider Viking trade networks and became an important source of exotic material culture for elites of both colonial and local groups (Valante 2008). Viking material found in Ireland is typical of the distinctive range of objects found among émigré Scandinavian communities in the North Atlantic (Larsen and Stummann Hansen 2001). Established on the east coast of the island in the late ninth century A.D., Dublin was the largest and most powerful of these settlements (Figure 19.1). Archaeological investigations of the medieval core of the city have provided detailed, even intimate, insights into the daily life of its inhabitants between the tenth and thirteenth centuries (Fanning 1994; Geraghty 1996; Heckett 2003; Knudson et al. 2012; Lang 1988; McCutcheon 2006; Mitchell 1987; Wallace 1992). Interpretations of the evidence across an eclectic range of material culture elements tend to agree that by the eleventh century, a considerable degree of assimilation had occurred between Irish and Scandinavian groups (Hurley 2010; Hurley et al. 1997; Wallace 1992, 2008), with the result that the archaeology of the period is referred to as Hiberno-Norse. This archaeological narrative of acculturation contrasts with the contemporary historical record in which Irish chroniclers tended to portray the Scandinavians in negative terms. Yet political and military alliances between Norse and Irish are mentioned from the ninth century, while dynastic marriages are mentioned from the tenth (Ní Mhaonaigh 1998). Readings of the archaeological, historical, and linguistic data are suggestive of processes of acculturation from an early stage in the interactions between the two groups, with a concurrent maintenance of some degree of social distance.

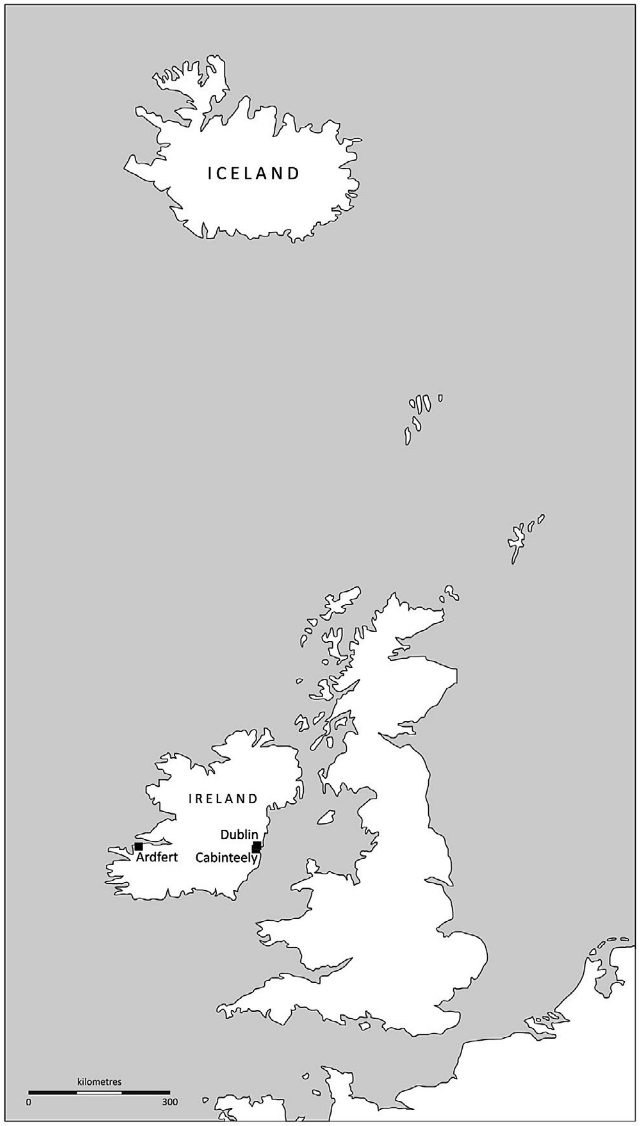

Figure 19.1. Locations mentioned in the text.

Morphological Variability in Early Historic Ireland

In a series of studies (Hallgrímsson et al. 2004; O'Donnabhain 2001; O'Donnabhain and Hallgrímsson 2001), we have interrogated the biological data to see if the flow of cultural elements was matched by a flow of genetic material between the populations. The methodologies and results of these studies are detailed elsewhere (Hallgrímsson et al. 2004), and the main results are summarized in the tables and figures below (Tables 19.1–3; Figures 19.2, 19.3). Samples of early medieval human skeletal remains from Ireland, Iceland, and Norway were analyzed, as was a later group of Inuit from Greenland that was used as a control. The Irish samples that are the focus of this discussion included skeletons from fifth-to-eighth-century levels of a cemetery at Cabinteely, a site located 10 km south of Dublin (Figure 19.1). This group (n = 101) represents the precontact population of the region in which Dublin was to develop. Skeletal remains have been found in a variety of different contexts during many excavations carried out in tenth-to-twelfth-century levels of Dublin. These represent the largest group of Viking-related human remains from Ireland that is available for study (n = 93). The bulk of this material consists of isolated bones that were found scattered in the general matrix of a number of mostly contiguous sites in the core of the medieval town. Some complete skeletons and portions of skeletons were also recovered. The latter were not found in cemeteries or areas that are likely to have been regarded by the inhabitants of the town as formal areas for the disposal of the dead. Most of the more complete remains came from excavations at Wood Quay, where most of the skeletons were found in what are best interpreted as mass graves. In contrast, the sample from Ardfert was recovered from a formal cemetery. The village of Ardfert is located on the west coast of Ireland, 1.5 km inland from the Atlantic on the flat and fertile coastal plain of north County Kerry and about 200 km southwest of both Dublin and Cabinteely (Figure 19.1). Ardfert was an important ecclesiastic and administrative center in the early Middle Ages and has a large medieval cathedral and associated cemetery. Excavations undertaken at the cathedral in advance of conservation work produced burials dating from the tenth century, while four building phases have been identified in the church beginning in the eleventh century. The skeletons analyzed (n = 64) predated the remodeling of the cathedral in the mid-thirteenth century. While there are no direct accounts of Viking attacks on or other interactions with Ardfert, it is likely that the Scandinavians were familiar with the site, as there are Viking-related sites in the general vicinity and the church site was close to sea routes connecting Viking settlements.

Analysis of biological distance indicated that the remains recovered from the Viking Age levels of the Dublin excavations were not significantly different from the local precontact population from Cabinteely or from the contemporary population on the southwest coast at Ardfert (Table 19.1; Figure 19.2). In contrast, there were significant differences between the Dublin remains and a sample of pre-Christian (that is, ninth-to-eleventh-century) burials from Iceland (n = 121), as well as from a sample of Viking Age skeletons from Norway (n = 110).

Table 19.1. Matrices of Frequency Distances Among Sites.

A. Matrix of MMD Values for All Groups.

	Pre-Christian Iceland	**Christian Iceland**	**Pre-Viking Norse**	**Viking Age Norse**	**Hiberno-Norse**	**Pre-Norse Irish**	**Late Medieval Irish**	**Greenland Inuit**
Pre-Christian Iceland		.875	1.865	1.980	**2.844**	**2.970**	**3.113**	**4.730**
Christian Iceland	-.003		**2.389**	**3.775**	**4.227**	**5.543**	**5.157**	**7.030**
Pre-Viking Norse	.038	**.050**		1.515	**2.763**	**4.108**	**3.143**	**4.457**
Viking Age Norse	.029	**.056**	.019		**2.524**	**3.608**	**2.317**	**5.154**
Hiberno-Norse	**.085**	**.122**	**.092**	**.066**		1.058	1.038	**4.326**
Pre-Norse Irish	**.071**	**.112**	**.156**	**.084**	.013		1.374	**6.872**
Late medieval Irish	**.089**	**.132**	**.115**	**.057**	.005	.014		**6.153**
Greenland Inuit	**.143**	**.161**	**.163**	**.143**	**.166**	**.226**	**.241**	

Note: The lower diagonal contains the MMD values, and the upper diagonal contains the standardized MMD, calculated as described in the text. The standardized MMD is considered significant at the .05 level if it exceeds 2 (Sjøvold 1977b). Significant values are bolded.

B. Matrix of Harpending-Jenkins Distances.

	Pre-Christian Iceland	**Christian Iceland**	**Pre-Viking Norse**	**Viking Age Norse**	**Hiberno-Norse**	**Pre-Norse Irish**	**Late Medieval Irish**	**Greenland Inuit**
Pre-Christian Iceland		.328	.065	**.006**	**p < .001**	**p < .001**	**p < .001**	**p < .001**
Christian Iceland	.026		**.007**	**p < .001**	**p < .001**	**p < .001**	**p < .001**	**p < .001**
Pre-Viking Norse	.077	**.077**		.435	**p < .001**	**p < .001**	**p < .001**	**p < .001**
Viking Age Norse	**.060**	**.077**	.056		**p < .001**	**p < .001**	**p < .001**	**p < .001**
Hiberno-Norse	**.134**	**.159**	**.146**	**.112**		**.011**	.109	**p < .001**
Pre-Norse Irish	**.113**	**.141**	**.193**	**.120**	**.067**		.117	**p < .001**
Late medieval Irish	**.129**	**.157**	**.156**	**.093**	**.066**	.064		**p < .001**
Greenland Inuit	**.205**	**.202**	**.219**	**.191**	**.232**	**.278**	**.298**	

Note: The lower diagonal contains the H-J distance values while the upper contains the p-values as obtained by the randomization method described in the text. Significant values are bolded.

Table 19.2. Univariate Measures of Within-Group Variation.

Group	n	σ	p(1-p)
Pre-Christian Iceland	121	.142	.139
Christian Iceland	265	.150	.149
Pre-Viking Norse	51	.141	.137
Viking Age Norse	110	.139	.137
Hiberno-Norse	93	.162	.157
Pre-Norse Irish	100	.148	.145
Late medieval Irish	69	.145	.142
Greenland Inuit	59	.164	.161

Table 19.3. Multivariate Individual Mean Deviation Variances for All Groups.

Group	n	Mean Number Observable Traits per Individual	σ of IMD
Pre-Christian Iceland	72	22	.0055
Christian Iceland	174	26	.0047
Pre-Viking Norse	36	26	.0054
Viking Age Norse	81	27	.0036
Hiberno-Norse	36	26	.0082
Pre-Norse Irish	74	21	.0053
Late medieval Irish	50	23	.0046
Greenland Inuit	58	29	.0048

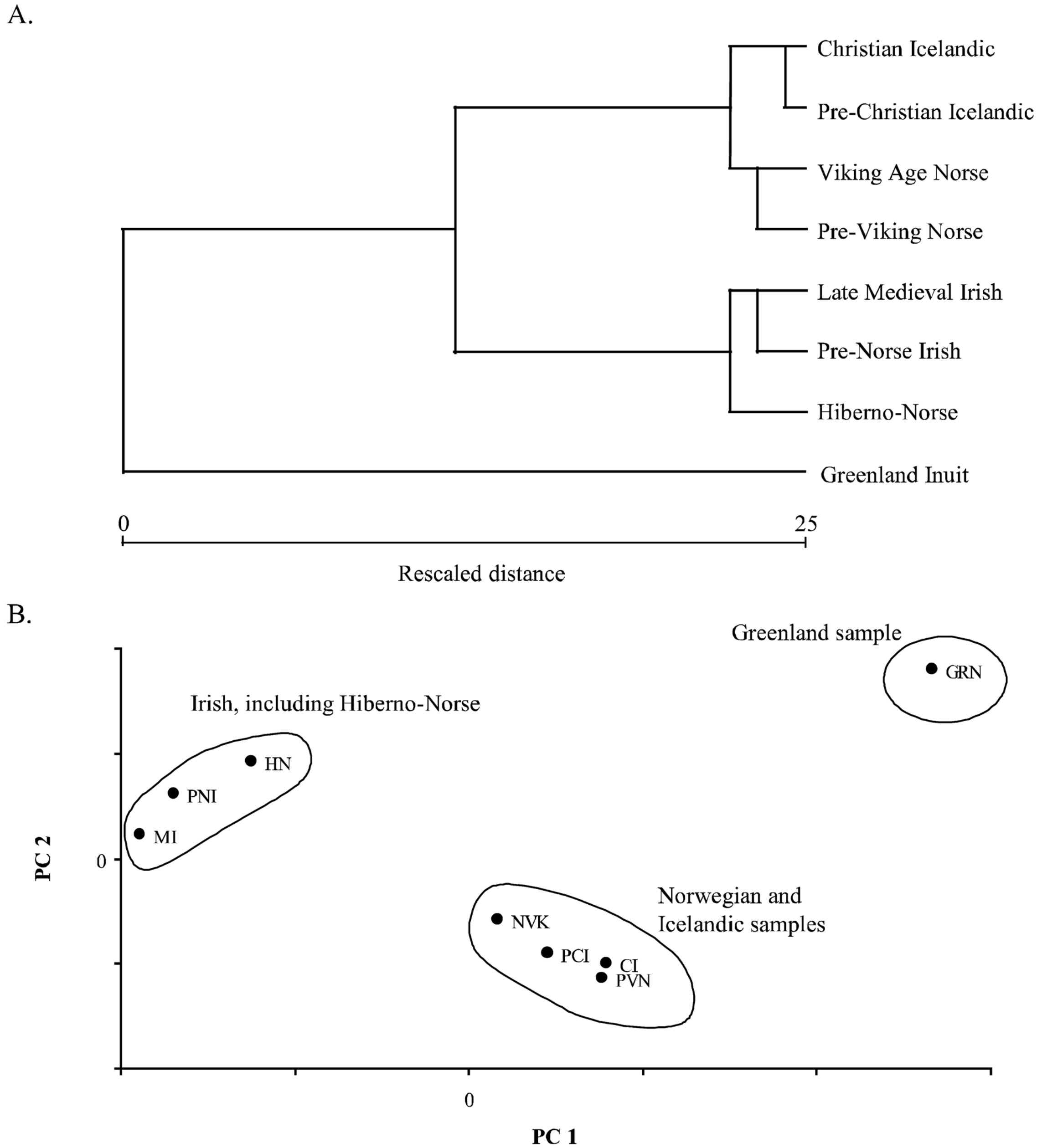

Figure 19.2. Results of principal components analysis of the R-matrix for the major groups in this study: (A) dendrogram of Ward's hierarchical cluster analysis using eight principal component scores derived from the R-matrix; (B) component score 1 plotted against component score 2; (CI) Christian Icelandic; (PCI) Pre-Christian Icelandic; (PVN) Pre-Viking Norse; (NVK) Viking Age Norse; (HN) Hiberno-Norse; (PNI) Pre-Norse Irish; (MI) late medieval Irish; (GRN) Greenland Inuit. Both A and B illustrate the phenetic groupings implied by the R-matrix values.

This suggests that local communities made a significant contribution to the population of the colonial outpost at Dublin and were to some degree ancestral to the skeletal collection recovered from the enclave.

Analyses of the within-group variability between the samples indicated that the collection from Dublin was the most heterogeneous of all the groups considered. This heterogeneity of the Viking Age sample from Dublin contrasted with the relative homogeneity of the precontact population of the region. A population of mixed geographic origins is a likely explanation of the marked heterogeneity of the Dublin sample. This does not conflict with the suggestion made on the basis of the biological distance analysis of a relatively high level of affinity with the local precontact population. It indicates rather that the latter group was only partially ancestral to the sample of the tenth-to-twelfth-century inhabitants of Hiberno-Norse Dublin.

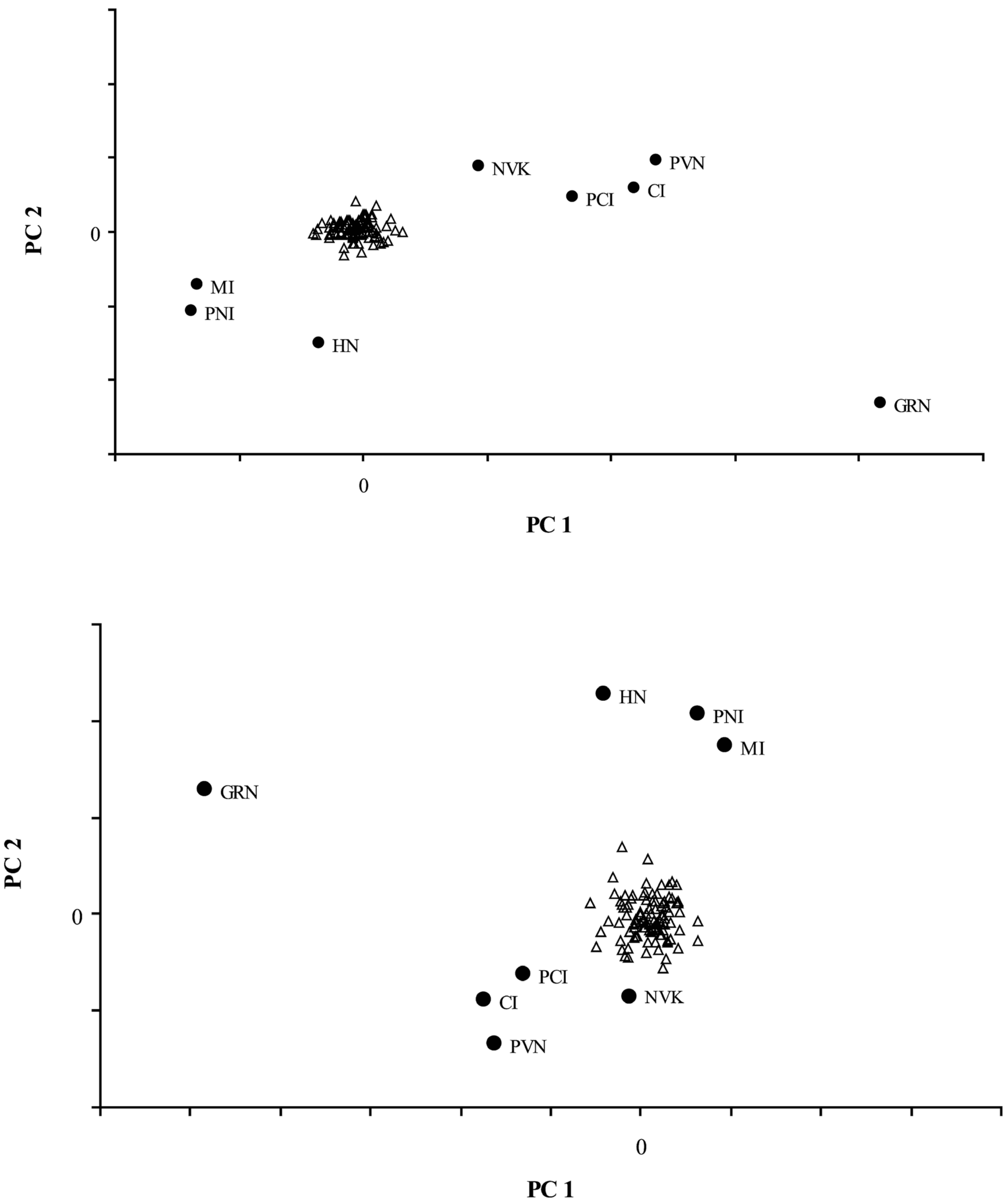

Figure 19.3. Plot of principal component scores 1 and 2 of the R-matrix for the original data and the resampled 50 percent and 66 percent hybrid populations between NVK and PNI (100 iterations). This distribution of triangles illustrates the expected position of the settlement Icelandic population (PCI) if it represented a mix of Irish (PNI) and Viking Age Norwegian (NVK). Abbreviations are the same as in Figure 19.2.

The Construction of the Colonial Self in Early Historic Ireland

The close relationship, spatially and morphologically, between the precontact sample from Cabinteely and that from Viking Dublin and the marked heterogeneity of the latter group are consistent with the occurrence of a process of gene flow between the immigrant and local populations. The results from the west coast sample from Ardfert are also consistent with such a scenario of interbreeding between native and newcomer. However, there are marked differences in material culture traditions between Dublin and Ardfert. There was no evidence of Hiberno-Norse traditions at the latter site. In contrast, Dublin maintained a distinct material culture tradition long after the period of most intense Viking activity in the North Atlantic and even after direct contacts with Scandinavia had ceased. This suggests that cultural responses to biological admixture proceeded in different ways in Dublin and Ardfert. Such admixture could have taken many forms, and explanations could range from coercion to the development of cultural proximities, sympathies, and intimacies. It is also possible that there were voluntary changes in ascription by cohorts of, in the case of Dublin, the local population or, perhaps in the case of Ardfert, the immigrant group. In Dublin, transformations in self-definition by local groups that may have chosen to identify with the economic goals and ideological values of the colonists might have been encouraged by the opportunities, economic and social, offered by a community such as the Viking Age town. The urban setting, a novel and experimental entity in tenth-century Ireland, may have presented opportunities to some segments of the local population that might not have been attainable in the context of the rigidly hierarchical society described in the early medieval Irish law tracts (Kelly 1988).

The archaeological data from Dublin and the other Viking ports suggest that their inhabitants maintained a distinct political and cultural identity up until the Anglo-Norman invasions of Ireland in 1169 and probably beyond that date. Not only did the population of Dublin maintain a distinctive material culture with a strong Scandinavian flavor, but the persistent use into the twelfth century of terms such as "Gall Átha Cliath"—the foreigners of Dublin—by writers of the Irish-language chronicles of the same period indicates that the attitudes and behaviors of those living outside the Viking settlements contributed to the maintenance of this social distance. Yet the biological data suggest that boundaries were transgressed and that there was a flow of genes across the cultural cordon. The maintenance of a distinctive lifestyle by some subgroups within early historic society in Ireland despite evidence for significant gene flow is a reflection of the dynamic, opportunistic, and pragmatic nature of identity construction and sense of ethnic affiliation (see also Stojanowski this volume). In the context of the fragmented and cellular political landscape that characterized early medieval Ireland, the privileging of a particular set of roots of the family tree at the expense of other kinship ties could have been an effective device in the maintenance of political and economic autonomy. This suggests that identity construction in Hiberno-Norse Dublin was located in the past. The representation of the past can be a source of power in the present. A dominant version of the past, irrespective of it being fictive or characterized by selective amnesia, can be a powerful tool in the legitimation of a particular set of social relations and the political status quo. In a context of increasing biological and linguistic entanglements, this selective privileging of a Scandinavian past had both overt and nondiscursive symbolic expressions that would have been meaningful to all the cultural groups who shared the island. These expressions ranged from the maintenance of distinctive settlement forms and economic strategies to the use of material culture items whose exotic nature and decorative styles evoked particular genealogical origins. These symbolic expressions also operated at the level of the individual inhabitants of the Viking enclaves. In this way, a distinctive Hiberno-Norse self was cultivated and affirmed through mundane sets of habitual activities. Daily actions with a particular material culture were not only microsites in which a distinctive identity was maintained and affirmed. Habituated practices were also the means by which distinctive Hiberno-Norse bodies were molded by the "natural" modus vivendi of the inhabitants of Dublin.

Nationalist fantasies, essentialist conceptualizations of "Viking" or "Irish," and asymmetrical models of colonization based on post-Columbian European expansion provide inappropriate lenses with which to view the cross-cultural and colonial encounters that occurred in Ireland toward the end of the first millennium A.D. The distinctive material culture of the Viking West suggests that the process of colonization was less about core–periphery relationships than about a novel network for circulating objects, ideas, values, and people. The encounters between colonists and locals in Ireland are perhaps best understood in terms of a meeting of equals where individuals and groups, operating with different cultural logics, interacted and were agents of change, transforming the values of all those involved as new cultural sensibilities were forged in the colonial setting. Not without considerable stress, both immigrants and indigenous created an elaborate network of economic, political, social, and genetic ties to meet the demands of their particular historical situation. The strategic, innovative, and symbolic use of material culture was central to this process, and through interbreeding and habituated practices, the political and the personal were enmeshed, with the result that the bodies of the inhabitants of the Hiberno-Norse enclaves can be seen as an archive of this particular colonial encounter.

The different cultural responses to processes of interbreeding seen in Ireland imply that material culture change was not a predictable outcome of biological admixture and that identity was not necessarily constructed solely around biological descent. Daily actions as visual performances of identity involving material culture and bodies' habitual activities were critical to the self-conscious construction of identity on both sides of the cultural divide in early historic Ireland. The colonial encounter resulted in processes of syncretism that left influences in technology, art, language, and social practices, as well as in the gene pool. All participants were radically changed by the experience, which was less about a mismatch between core and periphery and more about the transformation of bodies and minds.

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20

Trauma in the Medieval to Early Modern Sortebrødre Skeletons from Odense, Denmark

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Skeletons are the primary source of information on the health of people who lived during the prehistoric and early historic periods, and they contribute much to knowledge about workload, cultural customs, social organization, and intergroup relations. Collectively, this research is commonly referred to as bioarchaeology, following Jane E. Buikstra's (1977:69) use of the term more than 30 years ago. With regard to the disease experience of past populations, the work undertaken over the past few decades has tended to focus on two pivotal transformations in human existence: the origins of agriculture and the emergence of complex societies, commonly called chiefdoms and states. Of particular interest are the effects of changes in dietary content, breadth, reliability, and sufficiency; settlement size and duration; population size and density; differential intragroup access to food and exposure to occupational hazards; and interaction among variously constituted groups at local and regional levels.

Since the early 1980s, it has been widely accepted that these two watershed events came at a great biological cost to our ancestors, with consequences rattling down to the present day (Armelagos et al. 1991; Cohen 1989, 2008; Cohen and Armelagos 1984; Cohen and Crane-Kramer 2007; Gibbons 2009; Larsen 1995; Steckel and Rose 2002; Steckel et al. 2002). Each step along the way is thought to have been accompanied by poorer nutrition, greater exposure to a wider array of infectious diseases, higher mortality, and a heavier workload for most people. In the fullness of time, the quality of life worsened as populations grew and regional densities increased. Infectious diseases became a major problem when settlements got larger and were inhabited for longer periods, fostering the contamination of local soil and water supplies and allowing more people to have regular face-to-face contact that facilitated direct pathogen transmission. Malnutrition became more common as diets changed and their breadth diminished when ever-greater reliance was placed on domesticated animals and plants. Finally, most people eventually came to be dominated, and thereby disadvantaged, by a privileged few who enjoyed disproportionate access to the necessities of life, often forcibly acquired from a downtrodden majority.

Not all researchers, however, agree that a simple stepwise deterioration in the human condition adequately captures the complexities of what took place. One major difficulty with what can now be characterized as Conventional Wisdom—that health progressively deteriorated as people began to farm and, later, to live in large, hierarchically organized societies—is that individual skeletal samples are treated as representative of entire ways of life subsumed within a few stages defined by subsistence strategy or sociopolitical system. Comparisons are drawn between hunter-gatherers and subsistence agriculturalists, or other such categories, as if societies in each group were similar with respect to the many aspects of cultural behavior and natural settings that influence disease experience.

The way Conventional Wisdom is framed poses difficulties, as there is in fact no reason to imagine that any one such society, often represented by a single cemetery, was the same as the next in terms of whether people were particularly healthy or not. One might just as easily expect considerable variation in disease experience within a societal category such as subsistence agriculturalist (that is, Neolithic). After all, differences that affected pathogen transmission and nutritional adequacy surely existed among groups classified as belonging to the same societal, archaeological, or temporal category.

If that was indeed the case—if variability, not uniformity, in disease experience was the norm within broadly defined societal categories—then there is much to be said for investigating thoroughly what took place in individual communities before using skeletons to construct grand narratives about trends in disease experience. An initial focus on communities and what happened within them conforms to Buikstra's (1977) call for a contextually nuanced approach to bioarchaeology. This theme is identifiable, however faintly, in a few early osteological studies that set a high standard for subsequent work (Angel 1971; Hooton 1930; also see overviews by Beck 2006; Ubelaker 1982).

Focusing first on variation in disease experience within communities is of special interest because overall frequencies of pathological bony lesions, as generated from cemetery samples considered in their entirety, might well mask much of what we would like to know about life in the distant past. That is true even if data are presented according to sex or long age intervals such as juvenile and adult. Such aggregated data are likely to obscure the diverse life experiences of various segments of past communities. It is precisely this diversity that our archaeological colleagues would like to know something about when reconstructing what took place in ancient societies and why it did so. An additional twist to the tale centers on using mortality samples (skeletons) to describe the disease experience of past populations (living people). This difficulty, a result of the selective effect of mortality, is sometimes referred to as the osteological paradox, after the title of an article where the issue was first discussed in depth for a bioarchaeological audience (Wood et al. 1992).

Intragroup heterogeneity in life experience with regard to age and sex is explored in this chapter using skeletons from a graveyard associated with the Sortebrødre (Black Friars) Monastery in Odense on the Danish island of Fyn. Some skeletons might date to as early as the thirteenth century, but the majority of them are from people who probably died in the sixteenth century following the Reformation (Boldsen and Mollerup 2006). One of the principal objectives of the pilot study, conducted in 2000, was to determine if it would be worth pursuing the subject of trauma further with the human remains available from this period in Denmark. Trauma was chosen as the focus of study because broken bones are relatively easy to identify in archaeological skeletons, the damage and bony response are correspondingly unlikely to be mistakenly attributed to some other cause, and fractures in archaeological samples are reasonably common relative to many other pathological conditions. The age distributions of broken bones were of special interest because they potentially provide us with perspectives on the experiences of different segments of past populations. Of particular concern was the reasonable, but not necessarily correct, assumption that examples of healed fractures simply accumulate with age in cemetery samples.[1]

Conventional Wisdom

For the most part, comprehensive bioarchaeological studies focusing on the health of past peoples have been undertaken only over the past several decades. Yet the assumption underlying much of this work—that things got worse with the advent of new ways of life—has deep and tangled roots in Western thought. There are, in fact, distinctly eighteenth-century Rousseauian (1950 [1755]) overtones in much of the modern scholarship that generalizes from osteological findings. Most important in this regard is a unidirectional stepwise degradation in the human condition that is said to have accompanied the adoption of new subsistence strategies and sociopolitical systems, and all else that followed in train. There is also a political advocacy dimension. Even raising methodological concerns over using skeletons to infer whether one group of people were healthier than another, and thereby questioning the factual basis of Conventional Wisdom, invites accusations of "pro-state" or "procivilization" agendas, as shown by Cohen's (1992:359) response to Wood et al. (1992).

Conventional Wisdom—that things got bad when people started growing crops and tending animals, and deteriorated still further when societies got larger and more complex—resembles in one important respect the frequently and rightly criticized notion of progress over time toward ever-improving conditions. Both ideas feature universal unidirectional change, only one represents an improvement in people's lives whereas the other is quite the opposite. These essentialist positions rest squarely on the identification of disease experiences that are said to typify certain classes of societies, however they might be defined.

Osteological support for Conventional Wisdom comes from individual cemeteries that are explicitly or implicitly treated as representative of particular types of societies classified by archaeological culture (distinguished by artifacts and architecture), time period (perhaps spanning many centuries or even millennia), sociopolitical stage (such as band or chiefdom), or subsistence strategy (including hunter-gatherer or subsistence agriculturalist). Such classifications imply the existence of readily separable sets of societies, considerable uniformity in each category, and clear distinctions between those groups. Regardless of how useful such categories might be as deliberate simplifications of messy reality, they each encompass considerable variation in community size, population density, residential mobility, technological sophistication, dietary practices, sociopolitical organization, intergroup relations, environmental productivity, subsistence security, and much else. These dimensions of variability no doubt had an effect on nutritional adequacy, pathogen transmission, and habitual activities, and hence what might be seen in skeletons. But treating individual cemeteries as singularly representative of larger categories of societies means that truly interesting differences in the health of roughly contemporaneous, geographically proximate, and culturally similar groups remain underappreciated.

Not all osteological studies, however, conform neatly to Conventional Wisdom, indicating that the situation is more complex than commonly depicted. Using various skeletal proxies for infectious disease load and nutritional status, researchers have noted exceptions to a progressive deterioration of health, as well as variability among roughly equivalent societies from the same general area and time horizon (Cook 1984; Douglas and Pietrusewsky 2007; Jackes et al. 1997; Meiklejohn and Babb 2011; Milner 1992; Pietrusewsky and Douglas 2001; Smith and Horwitz 2007; Wittwer-Backofen and Tomo 2008). Even when health is thought to have declined over time within a particular region, there could be considerable variability among populations inhabiting different local settings (Ubelaker and Newson 2002). So whatever happened in such an area during the shift from hunting-and-gathering to agricultural economies, it must be interpreted within the context of the specific groups of people, represented by mortality samples, that took part in the transition (Wittwer-Backofen and Tomo 2008). Yet despite acknowledging mixed results, especially in close examinations of multiple skeletal collections from culturally well-characterized regions, the general trend—things got bad, and then they got worse—is still touted as the essential story (Cohen 2008; Cohen and Crane-Kramer 2007).

Archaeologists, in particular, are understandably chary about broadly defined societal stages, since they are acutely aware of the considerable variability that exists in the nature and success of adaptations to local settings among societies otherwise similar in size, cultural background, sociopolitical organization, economic underpinnings, group mobility, and technology. Moreover, long-term cultural change within particular areas with well-documented archaeological sequences was not always in the same direction (for example, toward greater sociopolitical complexity). Given such diversity, it follows that one cannot assume uniform responses to what, in the fullness of archaeological time, were more intensive subsistence practices, elaborations in sociopolitical and economic systems, and greater numbers of people.

A prelapsarian state of good health followed by several millennia when people were sicker and died earlier is difficult to reconcile with a trend toward a larger global population. After all, large numbers of individuals can be considered a measure of a species' success. If Conventional Wisdom is correct, then people who were generally worse off than their ancestors managed to produce enough offspring to offset whatever losses resulted from early death. In short, sicker people routinely outreproduced their healthier ancestors, and they did so within shorter life spans. Perhaps that is indeed what happened, but then it would have to be explicitly accommodated by our models of cultural evolution, including the eventual replacement of hunting-and-gathering ways of life by those based on agriculture.

General Trends Versus Local Circumstances

In any generalization of what took place over great periods of time, there is a problem with reconciling overall trends with specific samples, including skeletons from cemeteries associated with individual communities.

What tended to happen over several millennia across entire continents, or large portions of them, was not necessarily the same as what occurred locally over a few decades, generations, or even centuries. Since cemetery samples are for the most part drawn from one or at most a few nearby settlements, they necessarily represent little more than local conditions. Differences in pathogen exposure, dietary adequacy, and activity patterns can be expected to have been present among geographically, temporally, and culturally similar populations, as living conditions would not have been identical from one place to the next.

It might be imagined that more intensive subsistence systems, once widely adopted, resulted in transitory periods of plenty before increases in population once again dampened growth by pushing hard against production capacity ceilings (Wood 1998). Thus it is conceivable, even likely, that in any particular region, periods of growth alternated with stasis (or even decline), and people's health tended to vary accordingly. The general model—it features elements of both Malthus's (1970 [1798]) and Boserup's (1965) insights into the relationship between population size and productive capacity, and its consequences (Wood 1998)—gains some archaeological support from the American mid-continent. Two thousand years ago, there was a marked shift to a heavier use of native cultigens that, from an archaeological perspective, took place rather suddenly, and it was accompanied by greater interregional contact, more permeable territorial boundaries, and a dampening of intergroup hostilities despite general population increase (Milner 1999, 2004). To judge from this single preindustrial example, more people do not necessarily mean a correspondingly poorer quality of life, at least as measured in this instance by cultural conditions that influenced whether fights would break out among groups (a Malthusian misery). If a scenario such as the one proposed by Wood (1998) indeed characterizes what generally took place, we would expect alternating periods when people were better or worse off, and their skeletons would presumably show it. That sort of temporal variation in health within particular regions would produce a picture quite different from Conventional Wisdom, in which health inexorably and universally deteriorated with the adoption of new ways of life.

Before sweeping conclusions are made about the biological impact of changes in sociopolitical and economic systems, it would seem prudent to start with contextually secure, fine-grained snapshots of what took place in individual communities. That is because extrapolations from single cemeteries, or just a few of them, to broadly defined archaeological groups, cultural stages, or temporal horizons risk mischaracterizing general conditions. In part, the problem stems from the archaeological samples available for study. Cemeteries are rarely, if ever, systematically chosen for excavation from a well-characterized universe of sites—that is, numerous sites where it is possible to control for variation in function, size, duration, and the like. So osteologists must base their conclusions on samples composed of whatever has been excavated for any number of reasons. That is because excavations, especially large and expensive ones that yield many skeletons, are often undertaken as a result of circumstances that have little to do with a research question, since fieldwork is frequently dictated by modern land-use needs.

Saying it is necessary to have a thorough understanding of specific settings and populations before constructing grand narratives about the past should not be misinterpreted as a retreat to particularism. Instead, it is acknowledgment that we must be thoroughly familiar with the nature of our samples—the specific cultural and environmental circumstances that conceivably affected past communities, and hence skeletons—before treating cemetery samples as representative of general cultural types (for example, hunter-gatherer). In fact, community-focused skeletal studies nicely complement archaeological investigations of societal change that emphasize the interplay of cultural and environmental conditions within historically contingent contexts.

Living Populations Versus Mortality Samples

There are also methodological problems with interpreting the skeletal data widely regarded as supporting Conventional Wisdom. The difficulty in going from pathological features of skeletons to the characteristics of once-living populations, the fundamental point of the osteological paradox, boils down to how well a mortality sample reflects the population from which it was drawn (Wood et al. 1992). That is, we must ask ourselves what can truly be said about a group of living people when all we have available are those who happened to have died (excavated skeletons). While various aspects of this problem have been discussed in the literature, there is no consensus over the magnitude of its effect and how to deal with it effectively (Byers 1994; Cohen 1992, 1994, 1997, 2008; DeWitte and Wood 2008; Goodman 1993; Jackes 1993; Ortner 1991, 1998; Saunders and Hoppa 1993; Usher 2000; Wood and Milner 1994; Wood et al. 1992; Wright and Yoder 2003).

By their very nature, mortality assemblages are highly selected samples of people who were once alive at a given age. That is because each individual does not experience the same risk of dying at each age. So cemeteries are filled with the weakest, sickest, and most vulnerable members of every cohort at each step along the way from birth to the maximum life span. As a group, people who died in a particular age interval, say between 5 and 10 years, are not the same as those who survived for a while longer only to die at some later age. It follows, therefore, that the frequencies in skeletons of pathological lesions that are related in any way to a greater risk of dying do not equal the prevalence of those same lesions among the living (Wood et al. 1992). There is nothing original or surprising about such a statement, yet it is curiously what remains controversial about the osteological paradox. Cohen, for example, recently claimed that "straightforward interpretations of *relative* pathology frequencies in two or more past populations can generally be made from their cemeteries without concern for paradoxical interpretations" (2008:495; emphasis in original). The emphasis on *relative*, presumably in contrast to *absolute*, does not allow us to skirt the fundamental issue. The distinction between mortality samples and the population from which they were derived is important because we are interested in the life experiences of people in the past. Skeletons are of concern only insofar as they provide the primary data used to reconstruct what life was once like.

There are, of course, other issues that must be tackled, most importantly estimating the ages of skeletons without bias (Bocquet-Appel and Masset 1982; Chamberlain 2000, 2006; Hoppa and Vaupel 2002; Konigsberg and Frankenberg 1994; Konigsberg et al. 1997; Milner et al. 2008). The age estimation issue is particularly problematic for adults, especially the elderly. It too is the subject of continuing research, and some promising new directions are being pursued (Chamberlain 2006; Hoppa and Vaupel 2000; Milner et al. 2008; Weise et al. 2009).

Intracommunity Distinctions

A natural outgrowth of treating individual cemetery samples as representative of particular types of societies is the tendency to consider past communities as essentially homogenous except to the extent that skeletal samples are crudely broken down by sex, age (commonly juvenile and adult), and social status (typically an elite group in contrast to everybody else). Archaeologists, however, would welcome greater attention being directed toward within-community heterogeneity to identify the differential effects of infectious disease, nutrition, and workload on various segments of society.[2] Such an emphasis is consistent with one of the principal aims of archaeologists: to understand the functioning of past societies, including the opportunities and hazards faced by variously constituted social, activity, age, and sex groups. A firm understanding of how people were actually living—specifically, who became sick and died and what effect that had on households and communities—would seem desirable before making sweeping generalizations about human health. At the very least, closer attention to what happened in particular populations complements the archaeological interest in characterizing life experiences in individual communities, as well as the variation that existed among those local groups.

Excessive reliance on summary figures when comparing one cemetery sample to another does not encourage an examination of untested beliefs about what mortality samples indicate about the lives of past people. One such assumption is that signs of nonlethal (healed) skeletal trauma simply accumulate with age in archaeological samples. At first glance, that seems to make sense—the numbers of broken but healed bones would be greater in individuals of advanced age simply because older people have more years in which to accumulate injuries (Buzon and Richman 2007; Glencross and Sawchuk 2003; Lovejoy and Heiple 1981; Mays 1991, 1998; Torres-Rouff and Junqueira 2006; Tung 2007). Yet that seemingly reasonable assumption need not be correct, because, once again, osteologists derive their data from death assemblages (Wood et al. 1992). If having experienced injuries earlier in life was in any way associated with an increased risk of dying, then healed fracture frequencies will not necessarily increase with age in graveyard samples. One must keep in mind that people do not need to have died from such an injury—only that the presence of a healed fracture marks someone who for any number of biological or cultural reasons experienced a higher risk of dying than many other members of his or her cohort.

It is certainly true that for any single individual, the chances of ever having had a broken bone increase with advancing age; that is a function of years of exposure to conditions that might result in severe trauma. But it does not necessarily follow that in mortality samples the skeletons of old people, in aggregate, must have more healed fractures than those of younger adults. In the death assemblage, young to middle-age adult skeletons could show more signs of injuries than the skeletons of elderly people if survivors of trauma, those with healed fractures, experienced a greater risk of dying than other members of the community who were the same age. Should that occur, the individuals at each age who were most likely to enter the morality sample would include many with healed fractures. People with previously broken bones would be winnowed out of the population, leaving behind those who, for the most part, were free of fractures.

Sortebrødre Skeletons

The Sortebrødre sample consists of numerous partial to complete skeletons. Many additional bones were also found scattered through grave fill. That was because when new graves were dug, people often disturbed old ones; the bones that were encountered were simply tossed into open holes as they were subsequently filled. A few bones even have marks made when gravediggers chopped through earlier skeletons. During the pilot study, all the adult bones were examined, yielding a sample of 8,640 complete bones: frontal bones, parietals, occipitals, temporals, zygomatics, maxillae, nasals, mandibles, clavicles, scapulae, humeri, radii, ulnae, ilia, ischia, pubic bones, femora, tibiae, and fibulae.[3] They were for the most part well preserved, although many were broken postmortem.

With regard to trauma, attention focused on various kinds of bone fractures and damage from weapons such as swords. The latter featured distinctive smooth surfaces where heavy, sharp-edged weapons had cut cleanly through bone. Healed and unhealed examples of both fractures and blows from swords or other sharp weapons, such as axes, were recorded. Several instances of trauma that lacked signs of remodeling—that is, there was no evidence of survival in the form of bone resorption or proliferation at the sites of injuries—resulted from blows with sharp weapons. In contrast, people survived most injuries that produced ordinary fractures, as indicated by extensive remodeling where broken bones had healed. Healed fractures were typically poorly aligned, sometimes resulting in a noticeable shortening of a bone relative to the corresponding one on the opposite side. It is possible that some perimortem bone fractures were not recognized, so deaths directly related to severe trauma would be underestimated accordingly. It is unlikely, however, that many such fractures were overlooked since these breaks tend to have a distinctive appearance that is unlike the damage that can take place long after death through pressure exerted by grave fill, tree roots, or building foundations.

Ages at death were estimated on the basis of standard osteological measures, along with other age-informative skeletal characteristics (Buikstra and Ubelaker 1994). The ages used here are best regarded as experience-based estimates because they depart from a strict, mechanistic application of one or more formally described age-estimation methods. Osteologists have generally shied away from relying heavily on such estimates because of a concern over their accuracy and replicability. Yet as a first approximation of age—and in the absence of age-estimation refinements currently undergoing development (Weise et al. 2009)—experience-based estimates can be considered adequate for the present purposes. That is, they are sufficient to judge from blind tests on hundreds of known-age skeletons of people who died from the nineteenth to twenty-first centuries, such as the Bass Donated Collection at the University of Tennessee (Milner 2010). Estimates of sex were based primarily on pelvic and cranial structures summarized in Buikstra and Ubelaker (1994). Long bone dimensions, most notably the sizes of the heads of femora and humeri, also contributed to these assessments.

For convenience, adulthood was broken down into 10-year age intervals. Skeletons that did not entirely fall in a single age interval were apportioned among the appropriate age categories. The principal advantage of long age categories is that having many bones in each interval dampens the quirky effects of a rather small number of specimens showing signs of trauma despite an examination of several thousand bones. In short, the coarse intervals are a reflection of the size of the data set that could be collected within the time constraints of a pilot study.

Skeletal Trauma

The people buried in the Sortebrødre cemetery had survived most injuries that caused fractures, as indicated by broken but healed bones (Figure 20.1). Some

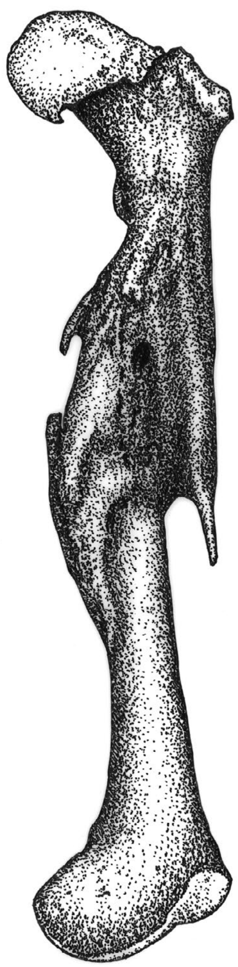

Figure 20.1. A healed but poorly aligned fracture of a left femur from a man's skeleton in the Sortebrødre graveyard. Quite remarkably, this individual survived major injuries that resulted in a number of badly fractured bones, probably attributable to the same incident (Jakobsen 1978).

of them, however, died immediately from their injuries or soon thereafter. Four of five people with evidence of unhealed trauma, all on crania, were male. Three young to middle-aged men had been struck with sharp, heavy weapons such as swords or axes. Two others, a man and a woman, had massive cranial fractures consistent with blunt-force trauma. Both were somewhat more than 50 years of age, so they were older than the men hit with sharp weapons.

As expected, many more bones showed signs of having healed after injuries had taken place. So for the most part, people survived trauma that was severe enough to affect the skeleton. Of the entire sample of complete bones, about .9 percent (81 of 8,640) had been broken and subsequently healed. Turning to long bones and combining both sexes and sides, radii were broken most often (3.1 percent; 15 of 478). Other commonly fractured long bones were clavicles (1.9 percent; 10 of 532), fibulae (1.5 percent; 6 of 407), and ulnae (1.1 percent; 5 of 469). Humeri (.6 percent; 3 of 516), femora (.5 percent; 3 of 596), and tibiae (.2 percent; 1 of 585) were broken less often.

Men and women were not equally likely to experience trauma that ended in broken bones that healed. Combining cranial bones and the major long bones of the upper and lower limbs, the men had more fractures that had subsequently healed than women: 1.6 percent (58 of 3,576) of male bones and .7 percent (22 of 3,339) of female bones ($p < .001$, chi-square test). The sample age distributions, however, are not the same: under 40 years, males 39.9 percent, females 49.6 percent; 40 to 65 years, males 54.1 percent, females 43.1 percent; greater than 65 years, males 6.0 percent, females 7.3 percent. While different age distributions complicate matters, they are not dissimilar enough to account for males having twice as many fractures as females. So discrepancies in bone fracture frequencies cannot be attributed to an equally great disparity in sample composition, at least in terms of age. It is reasonable to conclude, therefore, that males had a different experience with serious trauma than females.

Looking more closely at age, frequencies of broken but healed bones generally increase with advancing years for adult males (Figure 20.2). The pattern for women is similar, except in old age when there are relatively few broken bones.

Combining all fractures, regardless of origin, is not the best way to proceed because different injuries, with presumably different causes, are lumped together. It is, of course, usually impossible to determine precisely what caused a bone fracture in an archaeological skeleton. Nevertheless, some injuries likely resulted from intentional violence, most clearly blows from swords

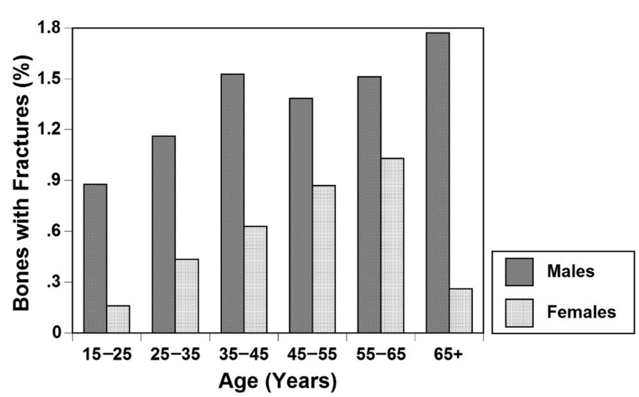

Figure 20.2. Age distributions of fractured but healed bones for males and females.

or axes, but also many examples of blunt-force cranial trauma, broken noses, and so-called parry fractures of ulnae. The majority of the other fractures probably came about through various accidents. Many clavicles and radii, for example, might have been broken when people fell (for example, Colles' fractures of radii). Falls were presumably common, as people would have routinely walked across uneven and at times slippery surfaces, frequently in poor light. The separate tabulations of fractures presented below, however, should not be taken to imply that all broken bones in each category occurred during either a fight or an accident. All that is intended is that many fractures allocated to one group, perhaps the majority of them, were attributable to interpersonal violence, whereas the other category likely had a larger proportion of fractures caused by accidents.

When frontal bones, parietals, nasals, and ulnae are combined—many of these bones could have been broken in violent encounters between people—healed fractures were observed in 3.9 percent (33 of 846) of male and .9 percent (8 of 859) of female bones ($p < .001$, chi-square test). A similar result is obtained when crania as a whole are examined rather than skull bones tabulated individually. Counting only individuals with at least three-quarters of the frontal and parietals present, 11.2 percent (18 of 161) of the males had some form of healed trauma, in contrast to only 3.6 percent (6 of 165) of the females. So men as a group experienced more trauma of this sort than women ($p < .01$, chi-square test). This tendency is accentuated when the five cases of unhealed cranial trauma, four of them occurring on men, are also included.

The distribution by side of frontal bone trauma is consistent with the intentional nature of many injuries. When examples of unhealed and healed trauma are combined for both sexes, the left side of the frontal bone was struck more often than the right: left 3.5 percent (12 of 345) and right 1.2 percent (4 of 343). While the patterning of injuries is consistent with victims facing right-handed assailants, and this difference approaches statistical significance, the sample size precludes definitive statements about whether one side showed more signs of trauma than the other ($p > .05$, chi-square test, correction for continuity). Trauma frequencies on the left and right parietals, however, were essentially identical: left 2.6 percent (9 of 341) and right 3.0 percent (10 of 337) ($p > 0.05$, chi-square test). A strong tendency toward one side or the other was not expected for the parietals since some people struck on the sides of their heads might have been hit from behind, whereas others could have been facing their attackers or were oriented sideways to them.

When the frontal bones, parietals, nasals, and ulnae are considered together and divided into 10-year intervals, the frequencies of healed bones for men vary somewhat throughout adulthood, although there is no clear trend one way or the other (Figure 20.3). Women show a different pattern: trauma frequencies increase early in adulthood and then decline. Neither men nor women show a tendency for these forms of trauma to increase steadily throughout the entirety of adulthood.

Turning to bones likely to have been broken in accidents, a difference between the sexes is not apparent for healed fractures of clavicles and radii. For these two bones, the difference between males at 2.8 percent (15 of 539) and females at 2.2 percent (10 of 453) is negligible ($p > 0.05$; chi-square test). So when only sex, not age, is taken into account, men were no more likely than women to break these two bones and to survive their injuries.

The skeletons of both sexes show an increase into middle age of broken, but healed, clavicles and radii (Figure 20.4). Overall patterns of increase, however,

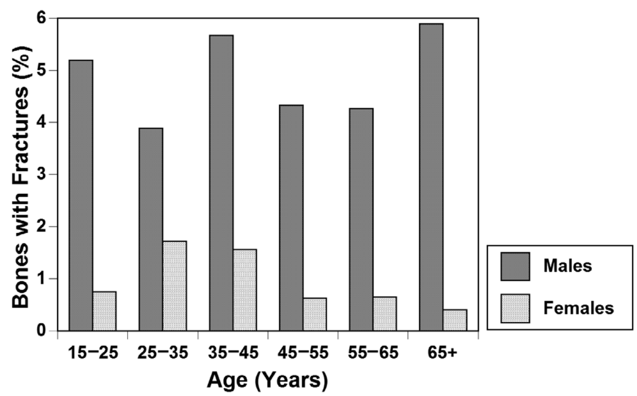

Figure 20.3. Age distributions of fractured but healed frontal bones, parietals, nasals, and ulnae for males and females.

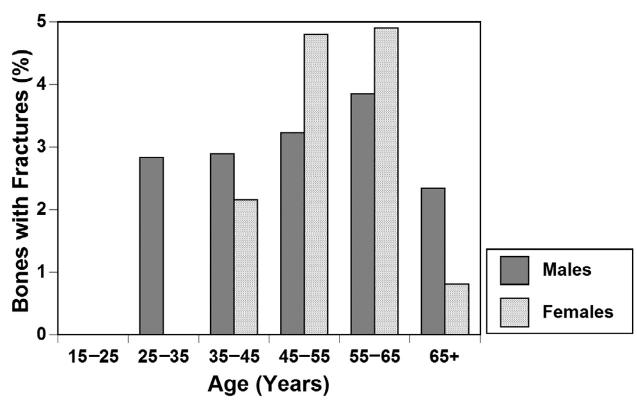

Figure 20.4. Age distributions of fractured but healed clavicles and radii for males and females.

appear to be different for men and women. Healed fractures in young men began higher relative to the middle-aged maximum than they did in women. Female skeletons also show more trauma concentrated in the 45-to-65-year period than those of males. In contrast to the skeletons of middle-aged people, those of the elderly had fewer healed fractures of clavicles and radii. Thus old-age fracture frequencies for both sexes depart from what would be expected if signs of trauma simply increased with age in individuals comprising the mortality sample. So it seems likely that many people with broken but healed bones were removed by death from the living population before they had reached old age.

The Community

Studies of European early to late medieval cemeteries indicate that male skeletons often show more signs of trauma than those of females, although that does not always occur (Angel 1974; Arcini 1999; Djurić et al. 2006; Gejvall 1960; Grauer and Roberts 1996; Judd and Roberts 1998, 1999; Mays 1991, 2007; Møller-Christensen 1982; Roberts and Cox 2003; Stirland 1996; Stroud and Kemp 1993; Weber and Czarnetzki 2001). The people buried in the Sortebrødre graveyard, therefore, conformed to a common pattern. The magnitude of the difference between male and female fracture frequencies varied from one place and time to another, as did the distribution of trauma throughout the skeleton. Assuming that such variation is not entirely an artifact of differential bone preservation, sex estimation, trauma identification, sample age composition, and burial circumstances—that is, to the extent information gleaned from separate studies is directly comparable—then there were measureable differences in the likelihood of suffering from severe trauma across medieval Europe. That conclusion is not at all surprising considering the diverse life experiences of people in rural and urban settings, regional differences in ways of life, and changes in life experience over several centuries. Nor is it new to this study. But it does underscore problems with treating only one skeletal sample, or at most a handful of them, as characteristic of entire ways of life or particular time horizons.

Overall trauma figures, however, do not tell the entire story. The Sortebrødre men were more likely to experience injuries that were probably often a result of intentional violence, specifically those affecting crania and ulnae. All five healed fractures of ulnae were from men (2.1 percent of 244 male ulnae). Although all of these fractures were not necessarily defensive injuries, the fact that males also experienced by far the largest proportion of cranial trauma is consistent with at least some ulnae being broken when fending off blows. In contrast, individuals of both sexes commonly fractured their clavicles and radii, probably most often in falls. In fact, these were the two most frequently broken bones when both sexes are combined. So it seems that women as well as men were prone to accidents, but men were far more likely to suffer from interpersonal violence that was serious enough to result in visible damage to bones.

The number of people who died when attacked or shortly afterward is too small to say anything definitive about which segments of society were most at risk. Yet the fact that four of five individuals with unhealed trauma were men suggests that they were more likely than women to be killed in such a fashion. Interestingly, the three men struck with sharp weapons were less than 50 years old. Old people, those over 50, were still vulnerable to attack, as shown by the man and woman with unhealed blunt-force trauma. These individuals, however, were struck with weapons that would have been available to everyone, perhaps staffs, cudgels, or even sticks. So it appears that the people most likely to fall victim to swords, axes, or other such weapons were men in the prime of life. But anybody, including the elderly, might be knocked about by whatever was readily at hand.

For the people who ended up in the Sortebrødre graveyard, the distribution of trauma by age shows that men and women did not experience the same chances of suffering from severe injuries throughout life, age-specific risks of dying associated with healed injuries, or both. Teasing these possibilities apart is not an easy task. Yet it is clear from these skeletons that healed fractures in mortality samples need not simply accumulate with advancing age; nor is the patterning of trauma by age for males and females the same.

Previous studies of adult skeletons from various places have likewise reported either no marked increase in fracture frequencies with advancing age, or old-age declines in whatever terminal, open-ended age interval is used (Buzon and Richman 2007; Judd 2002, 2004; Jurmain 2001; Lambert 1997; Robb 1997; Stroud and Kemp 1993; Walker 1989). In other skeletal collections, trauma of one kind or another increases with age. A mixed picture is precisely what is expected, as the skeletal samples no doubt represent diverse life experiences. So perhaps the only things we can be confident about are that the chances of breaking various bones over the course of a lifetime varied among these societies and that survivors of trauma did not face the same risks of dying at each age throughout life. Whatever real variation existed among these populations is complicated by differences in the kinds of data collected, how they were compiled, and the ways the resulting information was reported.[4] Published figures are also affected to some unknown extent, presumably some more so than others, by biases in age estimates, especially an underestimation of the elderly.

To judge from the Sortebrødre skeletons, everyone in medieval to early modern Denmark was hounded by death, but the people who survived earlier bone fractures were often weeded out of the population before they reached what at the time passed for old age. That is true even of injuries that today might be considered to have had little lasting consequence, such as fractured but healed clavicles and radii.

For the clavicles and radii, frequencies of trauma increased through middle age but then decreased. The early increase is consistent with the traditional interpretation that healed fractures in skeletal samples accumulate with age. That would not be surprising because of what might be regarded as the comparatively minor consequences of these injuries, at least when compared to fractures of other bones. It could also be the case that previously injured people experienced a somewhat higher mortality during that part of adulthood. But as long as the chances of breaking bones and surviving exceeded any increase in the risk of death if previously injured, each age interval would still have more skeletons showing evidence of trauma than the preceding one.

Whatever differences might have existed between men and women, the old-age picture for radii and clavicles is the same for both sexes (Figure 20.4). Healed fractures marked people who for one reason or another were removed from the population before reaching advanced age (greater than 65 years). Relatively few skeletons from old people showed signs of broken bones.

It is interesting that the skeletons of 45-to-65-year-old women had more fractures than their male counterparts. In medieval to early modern Denmark, that time of life (starting in the fifth decade) was when many women had osteoporosis that was far enough advanced that it can be noticed by simply picking up their bones. Mortality seems to have acted selectively on these middle-aged women, so their skeletons appear commonly in cemeteries (Jesper L. Boldsen, personal communication 2000). Perhaps women with bone loss or fractures, including those stemming from comparatively weak bones, were the ones most likely to have been culled from the population in middle age. The contrast between male and female fracture frequencies from 45 years onward indicates a stronger selection against women with healed clavicles or radii in the 45 to 65 interval, so fewer survived to the oldest age category (65-plus years) relative to men.

A decline in fracture frequencies for crania and ulnae throughout much of adulthood is seen in women (Figure 20.3). That patterning is only likely to occur if women who survived such trauma experienced a greater risk of dying than those who had not been similarly injured. For male crania and ulnae, there is no apparent increase in trauma frequencies with age. Yet men who had survived earlier violent encounters must still have experienced a somewhat higher risk of dying than other men if it is assumed that this kind of trauma might occur at any point throughout adulthood. The other possibility—that fractures of crania and ulnae were mostly restricted to the youngest age interval and that having healed trauma was not associated with an increased risk of dying in subsequent age intervals—is inconsistent with the few men with unhealed trauma where it is known when the injuries took place (death occurred at the time of the injury or shortly thereafter).

Thus it seems bones that had broken but healed have more to tell us than that some greater or lesser fraction of a cemetery sample experienced severe trauma at some point in their lives. Partitioning the sample by age across all of adulthood permits one to identify differences in the life histories of men and women (Boldsen 1997). Furthermore, it should be possible to use trauma—or, for that matter, any frequently occurring pathological condition—to identify segments of societies that faced a greater risk of dying because they were either biologically compromised, culturally disadvantaged, or both. Whatever happened to these people, they did not experience the same age-specific mortality patterns as other segments of society.

That finding, of course, is not unexpected, and it has been detected in other skeletal samples. For example, people killed in ambushes involving members of a late prehistoric village in the American Midwest were not representative of their community as a whole (Milner and Ferrell 2011; Milner et al. 1991). Individuals with prior conditions that interfered with their capacity to fight or flee were more likely to die in attacks than villagers who were, on average, more fit. It is precisely this heterogeneity within populations that is of real interest when trying to characterize the life experiences of people in the past and when untangling the effects of social, political, and economic conditions on groups defined in various ways. By adopting such a community-based approach, more can be learned from skeletons than just saying that one population was, in aggregate, sicker than another, even if such a statement could be made with some assurance.

CONCLUSION

The distribution of trauma by age is but one example of the counterintuitive consequences of dealing with mortality samples (archaeological skeletons). Although the Sortebrødre trauma sample is small, the commonly assumed increase-with-age scenario is not supported. The results are instead consistent with survivors of injuries being more likely to succumb to earlier death in comparison to many other members of their community, at least during some parts of the life span. People typically survived trauma that led to fractures, but these unfortunates tended to be among the ones who experienced a greater risk of dying as they passed through subsequent age intervals.

Such patterning, which says much about what took place within communities, is the sort of richly nuanced appreciation of the past that captures the spirit of Buikstra's (1977) sense of bioarchaeology. At the very least, this kind of information is of great interest to archaeologists as they strive to identify what life would have been like for people in the distant past.

In the best of circumstances, a skeletal collection drawn from a single cemetery would be a representative sample of deaths that occurred in the nearby area, quite possibly a single community. Whatever happened to these people when they were alive, it took place within the context of local cultural and natural settings. It is possible, perhaps even likely, that the particular challenges they faced bore only a tangential relationship to what might be said to characterize broadly defined types of societies. To the extent that is true, it is arguably best to start with well-characterized samples to understand life in specific communities before using the skeletons to bolster grand narratives of the human experience. While frequently couched in terms of the origins of agricultural economies and, later, organizationally complex societies, changes in morbidity and mortality patterns across these major transformations in ways of life quite likely resulted from a combination of alterations in population size and density, group mobility, purposeful and unintentional modifications of local settings, dietary composition and sufficiency, and intergroup contact. Identifying what had the greatest effect on disease experience in specific circumstances will require close attention to what took place in archaeologically well-characterized communities, as well as in cultural sequences within particular geographical regions.

Perhaps some version of Conventional Wisdom will one day be shown to be essentially correct. By that I am referring to the overall direction and timing of changes in health, not the way that conclusion is reached by simply counting skeletal lesions in mortality samples and using age-estimation procedures that yield biased results. But even if that is the case, the research question as it is commonly expressed is inherently limiting, as it does not naturally lead to more sophisticated questions. Little remains to be done once it is demonstrated to everyone's satisfaction that the situation got progressively worse over time, and that is how the situation is typically portrayed in current scholarly and popular writing.

In contrast, exploring temporal and geographical variation in health in relation to the wide variety of economic and sociopolitical systems, ecological settings, and population densities that once existed is a more interesting endeavor than setting out to show that conditions simply deteriorated with the introduction of new ways of life. It is also more amenable to studies that explore models of the rate and processes of cultural change (e.g., Wood 1998). Those kinds of analyses are precisely what will provide deeper insights into the conditions and events that contributed to cultural change, including the benefits and costs to human health of different ways of life.

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Notes

1. To my knowledge, Buikstra (1975) was one of the first to explore the effects of sample origin on fracture frequencies, in her case how a group of macaque skeletons were collected.

2. In this context, *heterogeneity* is arguably better than the more commonly used archaeological terms *hierarchy* and *heterarchy* because it refers to both, and it encompasses biological as well as cultural dimensions of variability.

3. There were also many incomplete bones, but they are not discussed here.

4. Such problems crop up whenever previously published skeletal information, no matter how meticulously collected, is used for comparative purposes, as pointed out by Ortner (1991) and Wittwer-Backofen and Tomo (2008), among others.

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Honoring Jane Buikstra's pioneering work in the development of bioarchaeological research, the essays in this volume stem from a symposium at the annual meeting of the Society for American Archaeology. Multiple generations of Buikstra's former doctoral students and other colleagues gathered to discuss the impact of her mentorship. The essays are remarkable for their breadth, in terms of both the topics discussed and the geographical range they cover. The contributions highlight the dynamism of bioarchaeology, which owes so much to the strong foundations laid down over the last few decades. The volume documents the degree to which bioarchaeological approaches have become normalized and integrated into anthropological research: bioarchaeology has moved out of the appendix and into the interpretation of archaeological data. New perspectives have emerged, partly in response to theoretical changes within anthropology, but also as a result of the engagement of the broader discipline with bioarchaeology.

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