

of the discipline reflects the strengths that it brings to the table in addressing issues about the past as well as the success of the discipline in adopting and applying developments — technological, methodological, and theoretical — from other sciences in new, innovative, and highly creative ways.

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Chapter 14

Mortuary Analysis and Bioarchaeology

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I. INTRODUCTION

In her preface to the volume *Biocultural Adaptation in Prehistoric America*, Gwen Kennedy Neville, editor of the Southern Anthropological Society Series in which the volume was published, noted that the papers in the volume represented an important attempt to combine and share knowledge (1977):

(T)he participants converge in their genuine interest in applying multiple perspectives and in their commitment to the creative sharing of knowledge. . . . (I)n the search for answers they are not afraid to look beyond their own disciplinary boundaries into the promising territory of the holistic study of human beings. (Neville, 1977)

The question or issue that I try to address in this chapter is whether we have made progress since the early 1980s in bioarchaeology in the creative sharing of knowledge and holistic study of human beings. The six authors whose work is represented in the 1977 volume attempted to demonstrate the value of physical anthropology and archaeology working together to address problems of the past, and although this may have been a somewhat ambitious and perhaps even naïve goal, it was certainly a worthy one.

My own perspective is as an archaeologist, so my bias is perhaps different than others in this volume. In order to address the current state of knowledge and to assess the range of research being done, I examined eight journals over the period 1995–2000, as well as some more recent issues of a few other journals. The eight journals were *American Antiquity*, *American Anthropologist*, *Current Anthropology*, *Journal of Anthropological Archaeology*, *Antiquity*, *Journal of*

Archaeological Research, Historical Archaeology, and World Archaeology. With a few exceptions, I did not systematically examine the *Journal of Physical Anthropology* or other specifically physical anthropology journals because Larsen (1997, 2002) has provided relatively recent reviews of much of this work that could be incorporated into this analysis.

The simplest way to summarize what I found is to say that archaeology and physical anthropology¹ have followed very different trajectories since the early 1980s, without extensive or creative sharing of knowledge. There were a total of 87 articles that had some discussion of mortuary practices, although this may not have been the primary focus of the article. The work in both areas has been extensive, interesting, and innovative, but it has not necessarily been collaborative or interactive to the degree that the participants in the 1977 Blakely volume had hoped. There are a variety of reasons for this state of affairs, and this divergence is worth exploration and examination.

II. CURRENT BIOARCHAEOLOGICAL RESEARCH DIRECTIONS IN PHYSICAL ANTHROPOLOGY

The simplest explanation for the lack of shared and combined research between archaeology and physical anthropology is that research in each of these areas has taken very different directions. In physical anthropology, researchers realized that they could use the tools of skeletal biology, DNA research, and chemistry to answer new questions that had never before been considered. Research became more and more science and laboratory oriented (Larsen, 1997). To be a physical anthropologist focused on skeletal remains from archaeological sites meant that one had to know much more than how to determine age, sex, trauma, and obvious signs of disease. As Larsen notes in a 2002 article documenting current directions in bioarchaeology, the field is now focused on using human bone to study such topics as dietary reconstruction from bone chemistry, infectious disease and health, physiological stress and disruption of growth, violence and trauma, masticatory function and tooth use, lifestyle reconstruction and interpretation, population history and biological relatedness, and paleodemography (Larsen, 2002:120). In this framework, the physical anthropological researcher does not necessarily need the archaeologist once the archaeologist has excavated the

¹I use the terms "physical anthropology" and "biological anthropology" to refer to those researchers who are anthropologists who study human remains because of the way in which Larsen has appropriated the term "bioarchaeology." Because I use the term bioarchaeology differently and more broadly [and more like the way in which Buikstra (1977) used the term], I feel the need to distinguish it here. I choose not to use the term osteology, as an osteologist may not necessarily be an anthropologist.

bones — research can be accomplished by the physical anthropologist working alone or with other scientists. Indeed, Larsen has redefined bioarchaeology as something exclusive to physical anthropology — he sees it solely as the study of human remains recovered from archaeological settings (Larsen, 1997, 2002). This is in dramatic contrast to the definition and interpretation of several other scholars [such as Buikstra's (1977:69) "active participation of both archeologists and physical anthropologists in all phases of research design"], but because of Larsen's impressive and prolific publication output, his definition has *de facto* become the most common, or at least the most ubiquitous, definition of bioarchaeology.

Larsen emphasizes the point that skeletons are especially important in making statements about the past because they are the physical remains of the people themselves and "are the most direct evidence of the biology of past populations" (Larsen, 2002:145). While this is an important point and one that is difficult to argue, he then goes on to discuss the issue of determination of gender and social inequality, noting that the sex of an individual is nearly always revealing about their gender. "Indeed, the jump from sex identification to social identity and behavioral inference is not a big one" (Larsen, 2002:145). Unfortunately, while perhaps it may not be a big leap in some instances, it is a huge leap at other times, and is precisely the reason that one should never rely on biological data alone — sex and gender are *not* the same. Context is everything, and while physical anthropology can do a lot with the bones once they are out of the ground, there is much that cannot be done without context and other nonosteological data. Indeed, there are several articles in the literature review conducted for this chapter that focus on the topics of gender, status, identity, and the changing role of gender over time; in each case, context is key in interpretation (e.g., Crown and Fish, 1996; Sofaer Derevenski, 1997, 2000; several articles in Arnold and Wicker, 2001; see also Parker Pearson, 1999).

It is possible, and even likely, that some physical anthropologists have started to ignore archaeological data because physical anthropology has gotten complicated and requires such specialized training that researchers do not have time to do everything they have to do for their specific analysis, plus work with the archaeological data as well. It is also possible that archaeological data are different enough that they are hard to interweave into the osteological analysis. Archaeological data are often "messy," requiring more interpretation and more work than osteological data.

A survey of the physical anthropological literature on bioarchaeology (Larsen, 2002) makes it clear that the field has become much more scientific or laboratory oriented and, with that move, has come the idea that the field is more absolute in what it can do and in how accurate it is. This idea has not been without its critics. Jurmain (1999) has perhaps been among the most vocal of these, demonstrating that some applications of work taken from biology and anatomy have not been carefully done and can be called into question for their conclusions.

Most recently, a dissertation by Bice (2003) critically analyzes the biomechanical model for reconstructing behavior from archaeological skeletal remains. She examines the use of cross-section geometry as representative of physical activity, sexual division of labor, and differential usage of the upper limb. All of these have been used extensively in the bioarchaeological literature since the 1980s. Bice demonstrates that ongoing nonanthropological theoretical, experimental, and clinical research on the adaptation of bone to mechanical loading has shown that the assumptions routinely made in bioarchaeology do not hold, and a number of alternative explanations are possible. Bice also provides evidence that the conclusions reached in bioarchaeological publications about kinds of activities conducted are likely false and that data presented in published papers often do not support the conclusions presented; behavioral inferences are sometimes based on statistically insignificant group differences in the cross-sectional geometric properties analyzed. In other words, Bice's work demonstrates that for one area of physical anthropology, conclusions about past human behavior are based on simplistic and unsupported assumptions. The researchers have borrowed work from another discipline, but have failed to keep up in that discipline and did not realize that more recent research has demonstrated that their measurements and analyses can have alternative explanations. Science is rarely absolute; alternative explanations are important to consider, and multiple lines of evidence are critical.

Larsen and many other scholars have developed bioarchaeology in exciting and interesting new directions. However, many have not taken note of what was said back in 1977—that to do biocultural or bioarchaeological work well requires a sharing and combining of information and questions. Biological data may appear more scientific and more precise, and it may be more precisely measurable, but it is not inherently better data—it is just different, and we need to learn how to work with different kinds of data. As the field has become more seemingly scientific, some bioarchaeologists have decided that they no longer need to work as closely with archaeological data, which is a huge mistake. When we work with data from the past we are working with parts of the whole so we cannot afford to dismiss any of it.

Perhaps worse or more dangerous than totally ignoring archaeological data is treating these data simplistically. Archaeology has changed significantly since the early 1980s and it is an even greater mistake to reduce archaeological mortuary theory to such simplistic assumptions as "there is always a one-to-one relationship between grave wealth and social status," or the sex and gender assumption cited earlier. While these assumptions may hold true sometimes, they do a serious disservice to theoretical developments in archaeology; they are culturally insensitive and they fail to recognize the complex social fabric of the past and the present.

In the set of literature examined for this chapter, I found few examples in which physical anthropologists acknowledged that there were important theoretical

developments in anthropological archaeology that need to be taken into account in their work, which, in fact, could bias bioarchaeological interpretations if ignored. One example is examined in a later section of this chapter, and another example is Buikstra (2000:15) in her discussion and critique of historical bioarchaeology, where she notes that, especially for historic contexts, there is a "complex interplay between social, economic, and ideational factors" (Buikstra, 2000:17). As Parker Pearson (1982) and Shanks and Tilley (1982) noted a number of years earlier, many different factors can enhance, distort, or mask relationships among social standing, burial treatment, and cemetery structure.

III. CURRENT MORTUARY RESEARCH DIRECTIONS IN ARCHAEOLOGY

At the other end of the spectrum are archaeologists. What happened to them since 1977? In mortuary studies, Hodder (1982, 1984, 1986) and others (Parker Pearson, 1982; Pader, 1982) argued that the focus on social ranking and social organization was far too narrow and inappropriate and was also too mechanistic as applied by some analysts. Further, others argued that areas such as gender and symbolism were largely ignored (Conkey and Spector, 1984; Ehrenberg, 1989; Shanks and Tilley, 1982; Wylie, 1991). In terms of what was missing from mortuary analyses, such critiques were certainly accurate, and the 1980s and 1990s saw a shift away from the social ranking—social organization orientation of mortuary analysis. Unfortunately, instead of supplementing or enriching mortuary analysis with additional approaches, the analysis of social organization was abandoned by many, only to be supplanted by topics such as emotive analyses (e.g., Tarlow, 1999), which is fine, but also limited. ~~There is no reason why one cannot focus on both or at least develop aspects from each, creating a richer and more complex picture of the past, but this tended not to happen, with a number of notable exceptions, such as Chesson (1999), Kuijt (1996), and Kus and Raharijaona (1998).~~ With this shift away from social ranking and organization was a shift away from most statistical analyses and work that appeared to be scientific or positivist; research tended to be more humanistic and symbolic [see a number of examples cited and discussed in Parker Pearson (1999)]. Interestingly, one of the few calls for an integrated approach to mortuary analysis or biocultural study was by Ian Morris in his book on how he thought burials in the ancient Graeco-Roman world should be analyzed (Morris, 1992). Morris thoughtfully and carefully put together an integrated plan for analysis of the skeletons and the archaeological materials.

As noted earlier, two important areas of study that had been largely ignored in the 1970s and 1980s were the study of gender and the study of symbolism.

Although mortuary analysts certainly noted the sex of burials, there was little discussion of the meaning and importance of gender in any way. At most, there might be some surprise when a woman was buried with lots of grave goods or was placed in a high status position. The topic of gender as a focus of study was largely ignored, as was the idea of a gendered past. As far as symbolic issues were concerned, archaeologists had long considered it an area of interest, but one that was beyond what they could extract from the archaeological record. Work such as James A. Brown's early research at Spiro (e.g., Phillips and Brown, 1983) was considered a notable exception. In the 1990s, these two areas developed both theoretically and practically, with a number of researchers demonstrating that excellent research could be accomplished in different ways and from different directions. Arnold and Wicker's (2001) edited volume on gender and mortuary practices presents a wide range of some more recent examples on approaches to gender analysis in mortuary sites, and people have also taken a variety of approaches to symbolism, religion, and ideology (e.g., Bradley, 1998a; Brown, 1996, 1997; Burley, 1995; Hill, 1998; Pollex, 1999; Siegel, 1996).

Another interesting direction of recent mortuary studies has been to focus on the individual and on the emotive (Tarrow, 1999; Meskell, 2000). While the archaeologist may see such a study as most likely limited to those instances in which one has documents or inscriptions or evidence about specific individuals, it is possible that one could propose or develop an emotive analysis based on individual or idiosyncratic treatments at a particular mortuary site. Sets of behavior could lead to such an interpretation, and Tarrow (1999) in particular outlines some of the ways in which an emotional archaeology might be possible further back in the past, through the analysis, for example, of metaphors and meanings. Most interesting here, however, is that nowhere in these published papers or examples did anyone suggest that such studies could be improved if they were done in conjunction with a physical anthropologist. Who better to assess and determine details about an individual? Working with a physical anthropologist who has additional information about the person, this area seems a natural one for collaboration, yet it rarely happens unless the archaeologist uses some already published skeletal data. One can only imagine how much more might be gained if Meskell's (2000) rich and impressive narrative analysis of Deir el Medina could be interwoven with a detailed osteological analysis.

Perhaps some of the most exciting of the developments in the archaeological mortuary analysis arena have been in the area of landscape studies. In the edited volume *Regional Approaches to Mortuary Analysis* (Beck, 1995), a variety of authors attempted to place mortuary analysis in a larger perspective, providing a regional context for mortuary sites and allowing archaeologists to place their work in a broader setting as part of the overall landscape in which people live. Creating a sense of place and developing the notion of social memory have been critically important research directions because researchers can connect

disparate and messy data from a region and link it in ways that had not been possible previously. Notable in this regard is Bradley's work on votive deposits in the Thames River (Bradley, 1990), as well as his work on megaliths and their relationship to the natural landscape (Bradley, 1993, 1998b). Equally impressive and influential has been Alcock's (1993) work on Roman Greece. Papers in the Beck (1995) volume have provided a variety of examples of the kinds of regional mortuary analyses and landscape research that are possible.

In the literature review for this chapter, there were at least four papers that focused on landscape issues and mortuary sites in a way that highlights one of the reasons why collaboration and context are so critical. Coming out of Bradley's (1993) seminal work on megaliths and landscape, a number of archaeologists have begun to examine monuments as a continuing part of the landscape rather than as an artifact built at one time and abandoned. We know these monuments were not abandoned, but were used again and again in patterned ways, and are still being used today. This behavior toward monuments comes from cultural memory. "Ancient monuments represent the past in the landscape and cultural memory gives them meaning and cultural significance" (Holtorf, 1998:24). Holtorf goes on to note that practices such as secondary burial are examples of a closely related phenomenon that he terms "history culture." History culture includes all of the appearances of the past in everyday social life (Holtorf, 1998:24). Holtorf (1998:24) notes: "Ancient monuments in the landscape influenced cultural memories of subsequent societies whose history cultures, in turn, transformed the monuments." These monuments not only link the present with the past, but they also link the present with the future. While Holtorf focused on megaliths, Semple (1998) examined how Anglo-Saxons reused Bronze Age burial mounds and Neolithic long barrows. In these cases, the reasons were different than those developed by Holtorf, but Semple was able to outline a strong case for the Anglo-Saxon perception of the landscape and its meaning. Hingley (1996), in his focus on Atlantic Scotland, makes a strong case that Neolithic peoples were deliberately reinventing monumental aspects of the past as a strategy related to developing a contemporary identity. The fourth case of reuse of the landscape is examined separately later.

The question to address here is why are these studies important to the development of a proper bioarchaeology or biocultural approach? Precisely because they demonstrate the importance of context and the fact that human bone is not simply placed in the ground or in a tomb and forgotten. Mortuary sites are active places and they are used and changed. If the analyst does not understand this, it is unlikely that the analysis will properly reflect what actually happened. There is not a single or simple answer or interpretation at a mortuary site, but possibly many.

The fourth example of a monument serving as a continuing part of the landscape is documented in an article by Moss and Wasson (1998); this is perhaps

the most remarkable case of all. The site is known as the Pistol River Site and is located on the Oregon coast. The site was the main village of the *Chet-less-chun-dunn-dunne*, an Athapaskan group. In 1856, a party of 34 Euro-American men burned the village. There are materials at the site from the AD 1600s through the 1800s. The site was of interest to 19th-century archaeologists, as well as to amateur archaeologists. The Oregon Archaeological Society excavated it, as did some professionals during highway construction. Later, professionals from the University of Oregon tried to return and do more work, but some locals also dug here. Among the excavators are local residents who are Native Americans and descended from those killed at the site. These people are friends of another family who are descended from one of the vigilantes who burned the village in 1856. The site yielded many artifacts as well as burials, and the artifacts have attracted many collectors. The story of the site is too complicated to repeat here, but is intertwined with Native ties and affiliation, archaeological significance, local interest, materials not curated or reported, a site claimed by everyone, and a place that today is important for locals, archaeologists, and wind surfers. It is definitely a story of use and reuse that cannot be understood without context.

I would be remiss at this point if I did not raise the possibility of politics as a reason why archaeologists may have moved further away from physical anthropology. As the call for new laws increased, and in particular, as the Native American Graves Protection and Repatriation Act (NAGPRA) was implemented, physical anthropology was seen in a negative light by many native peoples and was cast as a colonial endeavor. In part this was a misunderstanding, in part this was because few physical anthropologists had relationships with tribes, and in part this was because of some past misdeeds. Physical anthropologists were also not quick to publicly stand up and defend themselves and their work. Many archaeologists found it easier to avoid burials, and even physical archaeologists began to look at other portions of the subfield rather than continue to focus on bioarchaeology.

As interesting and important as the different kinds of landscape work have been, it has moved archaeologists further and further away from the bones themselves. Rejection of the processual approach to mortuary analysis with its ranking and focus on social status apparently accompanied rejection of integration of the physical anthropological data and physical anthropologists, perhaps in part as a rejection of science and statistics. I also have no doubt that politics played a role in the avoidance of physical anthropology as well. Whatever the basis, it is relatively rare to find an article written by an archaeologist about a mortuary site that incorporates skeletal data or is coauthored by a physical anthropologist. Instead of making progress at integration of archaeology and physical anthropology since 1977, a review of the literature reveals that physical anthropology and archaeology have diverged even more since the early 1980s. In the nearly 90 articles reviewed for this chapter, there are few articles that are biocultural or

bioarchaeological in the sense that was meant by Buikstra in her original 1977 definition.

The one bright spot of cooperation and integration of physical anthropology and archaeology is perhaps less an integration than it initially appears, and that is the work that uses bone chemistry to address issues of diet and residence patterning. This area of research brings together archaeologists and physical anthropologists to examine patterns of dietary change and possible measures of agricultural intensification, as changes happen in bone chemistry when maize becomes available in the diet. This kind of research has been ongoing since the 1980s, but in this review of the literature, at least three articles—Price and co-workers (1998), Schurr (1997), and Schurr and Schoeninger (1995)—demonstrate an integrated approach to data, attempting to employ a range of data, examining context, and exploring a variety of alternative explanations. Price *et al.* (1998) asked new questions about prehistoric migrations in central Europe, concluding that migration was substantial during the Bell Beaker period. Schurr (1997) compared demographic measures of fertility with age-related changes in stable nitrogen-isotope ratios and concluded that the relationship between fertility and weaning behavior is a complex one. I found the Schurr and Schoeninger (1995) article most impressive, as it examines the association between social complexity and agricultural intensification for late tribal and chiefdom level societies, but uses many, many different lines of evidence and examines alternative explanations. Of course, the irony of these analyses is that this time the archaeologists can use the bone samples without doing much more than noting that they are human bone.

IV. TOWARD AN INTEGRATIVE APPROACH

Integrative bioarchaeological work is not impossible, but it can be somewhat impractical and difficult. There is no question that for both archaeology and physical anthropology the work being done today is time-consuming and complex. To add another layer to that work adds more difficulties, more time, more cost, and so on. However, even though the overall benefit should be great, the individual researcher often has to worry about publications, raises, grants, tenure, and often calculates that maybe this is something that can be done at a later time. Perhaps only scholars who have infrastructures that support such integrative work can afford to do this research, but if this were true, that would be even more distressing. In this volume, Larsen discusses the increasing interdisciplinary work of bioarchaeology, but unfortunately, he does not include archaeology in that interdisciplinary or multidisciplinary umbrella, nor does he really include archaeology under the rubric of bioarchaeology, except as a source for data.

The lack of integration means that both archaeologists and physical anthropologists are ignoring data. Almost by definition, archaeological data are messy data—it is incomplete, it is imperfect, and it can be interpreted in multiple ways. By ignoring one or more aspects of data, the researcher is likely ignoring context or multiple lines of evidence or, at the very least, is not exploring alternative explanations. In other words, while there is nothing inherently wrong with physical anthropologists or archaeologists working on only one aspect or subset of data, that subset can never provide a full or complete picture of what happened in the past. Further, until those data are incorporated with other information, one cannot be sure of one's interpretations. As one example, some researchers have realized that the addition of geologists or geomorphologists is also important for mortuary analysis for these very reasons; their ability to understand site formation processes and construction can often provide critical information for overall interpretation (e.g., Buikstra *et al.*, 1998).

In a book focused on the anthropology and culture history of death, Elizabeth Hallam and Jenny Hockey (2001) explore the relationship among death, memory, and material culture. Their work is primarily cultural anthropology, but they explicitly explore memory through material objects that acquire meanings through practice, ranging from items such as mourning clothing to objects that represent the body, such as photographs or effigies, to memorials. Although the majority of the book focuses on Western death rituals, the authors also examine some non-Western ethnographic accounts of funerary ritual. In summarizing the ways that people bring material culture into death rituals, Hallam and Hockey comment on the real diversity in how different peoples sustain materialized relationships between the living and the dead; they note that "the body and its material surroundings become significant in the orientation of persons, both deceased and alive, in relation to their past, present and future" (Hallam and Hockey, 2001:190). One of their fundamental points is that the "processes of memory making, in relation to death and the dead, are not confined to institutionalized, public rituals" (Hallam and Hockey, 2001:201), but can be a factor of everyday life.

In 2000, Hendon wrote an article bringing together several different lines of evidence to talk about the past, and specifically about storage. She argues that storage is a situated practice "through which groups construct identity, remember, and control knowledge as part of a moral economy" (Hendon, 2000:42). Using this model, she discusses the social meaning embodied in other kinds of storage, such as burials. Burials are spots on the landscape that are remembered so "their presence informs a locale with meaning" (Hendon, 2000:47). Hendon (2000:50) goes on to discuss the rich symbolism that such burials may contain, arguing that groups, using both the natural and created landscape, construct a setting that gives permanent expression to both identities and relationships. At the same time, because not everyone knows about all of these places, some of that mutual knowledge is imagined, and burials are one set of the features that are

imagined, remembered, and discussed. Her paper is a very powerful one and would have been even more so if her examples had included osteological, as well as archaeological and architectural, data.

Perhaps because so much of my own work has focused on the importance of the spatial dimension of mortuary practices (e.g., Goldstein, 1980, 1981, 1995, 2000), I find myself particularly concerned that much of the bioarchaeology practiced today has ignored the context and the spatial location of the human remains. If the relative placement of a burial has anything whatever to do with that person's life, ties to others, and social memory, it does not appear that that information will ever be determined from much of the modern bioarchaeological research done today, or at least it seems that independent lines of evidence verifying that relationship will be ignored. Similarly, archaeologists have focused so intently on the structural, social, symbolic, and landscape aspects of sites that they have tended to ignore the individuals themselves, except when those individuals left behind items of personal adornment or inscriptions. Still, archaeologists have not tended to ignore the context of the materials they have found and they have examined the relationships of things to each other.

Michael Blakey has prepared a very thoughtful review of the bioarchaeology of the African diaspora in the Americas (Blakey, 2001). In this review, he notes the well-known early work of scholars such as Boas and Herskovits and also the significant work conducted during the first part of the 20th century by African-American researchers such as W.E.B. DuBois, Jean Price Mars, Cheikh Anta Diop, Katherine Dunham, W. Montague Cobb, Fernando Ortiz, and Irene Diggs (Blakey, 2001:390). In contrasting the work of these early black scholars with white scholars, Blakey comments on the different intellectual trajectories and traditions. He points out that at certain institutions and under certain anthropologists, there was intellectual cross-fertilization with black scholars and collective scholarship, whereas at other institutions, this was not the case (Blakey, 2001:391). While it is beyond the scope of this chapter to summarize Blakey's arguments, he is trying to draw a parallel between what happened in black scholarship generally and what has happened in African diasporic bioarchaeology. He notes that "(a)rchaeology and physical anthropology have experienced even less interaction with the black intellectual traditions than did American sociocultural anthropology" (Blakey, 2001:394).

Blakey (2001) provides a history of "physical anthropology and the Negro" and, not surprisingly, there is a dominant racial deterministic trend to many of the studies that were done. Blakey rightfully criticizes many studies for focusing on race rather than on people and notes that this focus really does not change until the National Historic Preservation Act of 1966 and the emergence of cultural resources management (CRM). Once CRM took off and projects happened across the country and in cities everywhere, many firms began to focus more and more on historical archaeology and on African-American sites.

Blakey (2001) makes an interesting distinction between what he calls the "biocultural" approach and the "forensic" approach in physical anthropology. Indeed, his point is very similar to the one made earlier in this chapter; his forensic approach is what I am concerned about with some of the current trends in bioarchaeology, and his biocultural approach is what I would call good bioarchaeology. He also makes a historical linkage or connection, intellectually associating biocultural work with the University of Massachusetts—Amherst and forensic work with the Smithsonian. In fact, I think this distinction is somewhat of an overstatement, and there are people trained by other individuals and institutions in the country who can be said to practice each approach, but his point is worthy of note in terms of the history of the discipline. The University of Chicago, for example, was training physical anthropologists in the biocultural approach at the same time. Blakey did not himself coin the terms "biocultural" and "forensic" approaches.

More significantly, I am uncomfortable with such labeling of an approach. I certainly agree with Blakey (2001) that there is an approach in physical anthropology that focuses on bones and tends to ignore context, history, and the people themselves. However, I do not agree that it is useful to call that approach "forensic," as it suggests that all forensic work is also problematic. At one level one could argue that this is true, but there are instances when one does not have much context or history and one only has the human remains. At that point, a forensic analysis is what one does. Even though the point being made about lack of context and focus is absolutely correct, there is no reason to make the term "forensic" derogatory. Blakey's success with the New York African Burial Ground project provides an excellent example of why context and background are so important to good biocultural analysis.

A final irony in the comparison of archaeologists and physical anthropologists ~~vis-à-vis mortuary sites is that, largely as a result of politics and changes~~ in the culture of archaeology, archaeologists are conducting fewer excavations and analyses of mortuary sites, but physical anthropologists are probably doing more analyses of skeletons. Implementation of the NAGPRA and other laws in the United States (as well as similar laws in other countries) has meant that archaeologists excavate fewer burials and mortuary sites; when they do such excavations, the work is generally limited in extent and scope. While one might surmise that the same would be true for physical anthropologists, this has not necessarily been the case, as so many skeletons in museums and other institutions have had to be examined before they could be reported or returned to tribes or affiliated to specific tribes. Indeed, since the implementation of NAGPRA, a considerable amount of physical anthropology has been required, at least at the level of basic recording of standardized data (Buikstra and Ubelaker, 1994). At least for the United States, there is now a large set of detailed, roughly comparable, data on human skeletons that has never before been available. We should

be able to now ask and answer questions that have never before been possible, as well as make comparisons across a number of different regions.

As exciting or interesting as these directions in physical anthropology may be, there are several things that dampen one's enthusiasm: (1) the bones themselves may no longer be available and (2) archaeologists have not done an equivalent job of recording context and detail on the artifacts and other information from these sites; in some instances, sites were excavated many years ago and context is poor at best. While we may hope that someone has field notes and records of how the site was excavated, as well as maps of what was done, archaeologists as a group or as a discipline have not carefully or systematically considered how to record artifacts and items that may be repatriated in the same way that physical anthropologists have done. Of course physical anthropologists have been able to focus on recording the skeleton, while archaeologists have to worry about every possible kind of artifact, but it would be helpful if there were at least some set of standards considered or some consistency in recording applied.

This chapter began with a quote from the Blakely 1977 volume on the importance and value of combining and sharing knowledge in the development of biocultural anthropology. A truly integrated project that incorporates the active participation of archaeology, physical anthropology, other scientists, and indigenous people in all phases of research design would still be a new form of research and an innovative way to proceed — while it was perhaps a naïve concept in 1977, it remains difficult today, but it is still a worthwhile goal.