A Century of Skeletal Biology and Paleopathology: Contrasts, Contradictions, and Conflicts

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ABSTRACT For the first half of the 20th century, biological anthropology stagnated in a state in which racial typology was its major theoretical and methodological focus. In 1951, Sherwood Washburn proposed the "new physical anthropology" that would move biological anthropology beyond description. Washburn repositioned it into a science that focused on process, theory, and hypothesis testing. The commitment to a process-oriented biological anthropology has been slow, but there has been progress. Biocultural studies and functional anatomy have produced a more dynamic science characterized by hypothesis testing and a heightened concern for causality. Unfortunately, a return to historical particularism has limited progress. An increasing interest in forensic application and resurgent interest in measures of population distances and migrations represents a reversion to an earlier descriptive past. [Keywords: adaptation, osteology, evolution, history]

> There is no present or future, only the past happening over and over again
>
> —Eugene O'Neill

HUMAN SKELETONS REPRESENT ANSWERS, and the goal of osteology is to frame the questions. There are important questions that ancient skeletons will not answer, and there are unimportant questions that they will. The quest, of course, has always been to discover meaningful questions—questions central to knowledge and the human condition, solvable through the analysis of human skeletal remains. The search continues and the stakes are high. We are searching for nothing less than the identity of our science defined by that small space in which the possible meets the meaningful.

The space, of course, is an ever changing landscape of possibilities. Osteologists once limited to simple techniques of counting and measurement are now armed with chemical assay techniques, imaging technology, and multivariate statistics programs for high-speed desktop computers. Studies of biological distance and multivariate morphometrics compete for journal space with neutron activation analyses and dietary reconstructions. New techniques have led to new questions and reconsideration of old ones. This volatile mix of old and new defines the contrasts, contradictions, and conflicts of our time, and this also leads to an important insight. Where we are today is very much a reflection of where we have been.

It is interesting, then, that osteology, a science directed so much to the past, has often failed to reflect on its own. Put simply, an understanding of skeletal biology's history may help us evaluate the importance of the questions we ask and methods we apply today.

Our interest follows in the tradition of earlier studies by Gabriel Lasker (1970) and C. Owen Lovejoy et al. (1982). Like them, we intend to explore the apparent disconnection between the questions asked and the techniques employed by contemporary osteologists. In our view, the promise of a "new physical anthropology," driven by the convergence of new methods applied to new questions, has failed to take solid hold in osteology. The discipline finds itself awash in new and increasingly sophisticated techniques applied to old questions with roots deep in the past but with little importance to contemporary anthropology.

We, therefore, have several objectives in this article. We first examine the concept of race and racial determinism that drove both the earliest questions as well as the earliest methods of osteology. We then consider the transformation of osteology into a new science of skeletal biology armed with new methods and directed toward new wider-ranging questions of process and causality. Finally, we discuss the discipline's retreat back to a neoracial approach and with it a resurgent interest in the methods of description.

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PRELUDE TO 20TH-CENTURY RACIAL STUDIES

> It is a capital mistake to theorize before one has data. Insensibly one begins to twist facts to suit theories, instead of theories to suit facts.
>
> —Sherlock Holmes, *A Scandal in Bohemia*

To consider any aspect of early anthropology, and, most particularly, osteology, demands a consideration of race. Questions of race were entwined in all aspects of the discipline's beginnings. Claude Lévi-Strauss described anthropology's "original sin" as the misconception that race was essential in understanding what has been termed the "production of civilization" (1952:1–3). Anthropology has wrestled with the question of race as a tool for understanding behavior for much of its history. Even as anthropology moved beyond racial determinism, race remained a core concept (Lieberman et al. 1989) and continued as the primary method for explaining human variation in both living and ancient populations.

The roots of the race concept run much deeper than anthropology. Across the millennia of recorded history, race has been an amalgamation of observed biological differences interpreted through the lens of cultural prejudice. For example, the Egyptians, as early as the 14th century before Christ, assigned humans to four color categories. Red represented themselves, yellow their Asian enemies to the east, white the people to the north, and black the African populations to the south. Prejudices associated with skin color were largely political. When light-skinned rulers held power, the Blacks were the "evil race of Ish." When Blacks ruled, Whites were "the pale, degraded race of Arvad" (Gosset 1963:4).

In the centuries before Christ, Greek philosophers envisioned a *scala naturae* along which all the productions of nature could be arrayed in an upward progression from inanimate matter through the varieties of humanity to God (Mayr 1988:420). By the 18th century, the scala naturae became temporalized into the "the Great Chain of Being" (Lovejoy 1936: ch. 1), and race once again took its place in this scheme. The placement of humans along the Chain of Being was enhanced in the 1790s by Petrus Campers's development of the facial angle. The lowest races had the most projecting (animalistic) faces while the higher races had flatter faces. The ideal was the flat face represented in Greco-Roman statuary (Meijer 1997:242).

It is not surprising that biological hierarchies reinforced behavioral hierarchies. For example, Carolus Linneaus classified racial types that inhabited the four regions of the earth associated geographically with humors that effected behavior (Stocking 1968:5). Essentialist thinking of the time argued that the four humors that influenced behavior (blood, phlegm, black bile, yellow bile) were keyed to geographic locality: American Indians had reddish skin, were *choleric,* and regulated by custom; Africans had black skin, flat noses, were *phlegmatic,* and governed by caprice; Europeans were white, *sanguine,* muscular, and governed by law (Slotkin 1965:177–178). Indeed, while we think of Linnaeus today for his biological constructs, Marks (1995) has convincingly argued that when it came to humanity, Linnaeus was more concerned with explaining behavior than understanding biology.

Two central ideas came into sharp focus during this period—races were real and races were rankable. These ideas breathed life into an old question: Where did races come from? Did human races have a monogenic or a polygenic origin (Greene 1959: ch. 8; Harris 1968: ch. 4)? Polygenists, such as the French philosopher Voltaire and the U.S. scholars Louis Agassiz, Samuel G. Morton (1839, 1844), and Josiah Nott and George Gliddon (1854) believed in the separate origin of races as "primordial types." Others maintained the view expressed by Saint Augustine a millennium earlier:

> We may hear of monstrous races—people who have one eye in the middle of their foreheads, people with no mouths, people with dog-like heads . . . but whoever is born a man, that is, a rational mortal animal, no matter what unusual appearance he presents in colour, movement, sound, nor how peculiar he is in some power . . . no Christian can doubt that he springs from that one protoplast. [Gosset 1963:6]

Skeletal biology found its place in the debate and in so doing fueled a love affair between race science and the skull. Morton (1844) measured crania from around the world in an attempt to both rank races and determine the antiquity of racial types. Differences in features such as cranial capacity appeared to have great antiquity and, thus, supported polygenesis. God, it appeared, had created not one humanity but many unequal kinds.

Johann Friedrich Blumenbach stood squarely on the side of monogenesis but was no less committed to ranking. The monogenist view simply required evidence for degeneration from God's original creation. His approach had both diachronic and synchronic elements. Living races were categorized into one of five color categories (black, African, Aethiopisea;[1] brown, Malayan, O-tahetae; white, Caucasian, Georgianie; yellow, Mongolian, Tungusae; and red, American, Caribaei). Corresponding cranial features were then identified as a means for tracing racial ancestries. Referring to ancient and modern crania, Blumenbach states:

> When skulls of the Mongolian, American, Caucasian, Malay, and Ethiopian races were viewed together . . . the Caucasian was seen to have the most beautiful and symmetrical form . . . in like manner, the white color of the Caucasian skin was the *norm* from which *degeneration* toward darker shades had taken place. [Greene 1959:224, emphasis added]

This notion of racial origins, and with it idealized racial types, fit well with biblical interpretations. Indeed, degenerationists such as Blumenbach maintained a strictly theological view of creation with a White race (Adam) created in God's image. As Caucasians expanded into new regions, they were exposed to environmental elements and cultural factors that caused degeneration from a primordial type to form new races. Degeneration explained what

was clearly viewed as both a biological (white to dark) and social (civilized to savage) fall from grace.

Two intellectual events, the development of evolutionary theory after 1859 and the discovery of Mendelism in 1900, had the potential to force a reevaluation of the race concept. That potential, if born with Darwin and the gene, was stillborn. While Darwinism ended the monogenesis-polygenesis debate in favor of a new *scientific* monogenesis, degenerationists simply turned their theory upside down. The fall from Adam simply became an ascent from the ape.

In this sense, it is not surprising that racial determinism continued to prevail in the post-Darwinian era. Roger Lewin echoes this point,

> Inequality of races—with blacks on the bottom and whites on the top—was explained away as the natural order of things: before 1859 as the product of God's creation, and after 1859 as the product of natural selection. [1999:3]

The quickness by which such diverse fields as medicine, anthropology, education, sociology, and paleontology lent support to "proven" racial inferiority shows that racism and racial hierarchies continued to be an integral part of the intellectual climate after Darwin (Haller 1971:xi–xii).

There can be no question that the intellectual shift from racial degeneration to evolution was important for osteology, but not for the reasons traditionally given (Armelagos et al. 1982). Evolutionism did not shift science away from Linnaean taxonomy but actually reinforced taxonomic descriptions and definitions. Taxonomy became the method for creating phylogeny. This was no less true for the study of race. Racial typologies were simply cast in the form of phylogenies as metaphors for race history. Thus taxonomy, as an inherently static, preevolutionary concept, did not give way to evolutionism after 1859; rather, evolutionism became cast in the form of traditional descriptive historicism (Armelagos et al. 1982).

Post-Darwinian osteology was far from ready to abandon race. At a time when archaeology and paleontology were contributing little more than curios, the comparative study of race seemed the only way to reconstruct our evolutionary past. In an age with few fossils, primitive races became "living fossils" and were viewed quite literally as evolutionary survivals of the various stages through which more "advanced" races had evolved. The key was finding some cranial trait or combination of cranial traits by which a growing number of races could be classified and ranked into an evolutionary hierarchy.

To this end, there was a rapid proliferation of measurements and instruments concerned with racial assessment. Paul Broca developed many of the anthropometric instruments in the late 1880s. He defined many of the cranial landmarks that were essential in establishing measurement standards. Standardization was the goal of conventions held in Germany, Monaco, and Geneva in the late 1800s to 1900s. In 1934, an international agreement resolved national differences in measurements (Spencer 1997), giving a further impetus to description.

The methods of anthropometry failed to provide answers to even the most basic questions: How many races are there? What is their relative ranking? Race science needed a new approach, and the promise lay in the new science of genetics. The impact of genetics had to wait until the development of a synthetic theory of evolution in the 1930s.

Most importantly, genetics did less to challenge the race concept than spawn the quest for new and more *scientific* racial traits. In this context, evolutionary theory worked against a genetic–racial approach. The problem was this: If racial categories were to be miniphylogenies, the traits chosen must reflect what Darwin called "propinquity of descent." However, if the traits used to build the phylogeny were evolving, then similarity may or may not reflect "propinquity of descent." Similarities could reflect parallelisms and evolutionary convergence.

If nonadaptive traits could be found and linked to specific racial groups, then race–science and classification could be used to establish racial histories. More than ever, race–science became the means to uncover culture history. And, ironically, in its search for nonadaptive traits, osteology became antievolutionary.

THE FIRST FIVE DECADES OF THE 20TH CENTURY

During the first half of the 20th century, biological anthropology was shaped by the contributions of Franz Boas (Baker 1994), Aleš Hrdlička (Blakey 1997), and Earnest A. Hooton (Spencer 1981). Boas and Hooton were instrumental in establishing academic anthropology at Columbia and Harvard, while Hrdlička built the Division of Physical Anthropology in the National Museum of Natural History.

Questions of culture history, and the history of human races, were of central importance to anthropology. Hooton and Hrdlička envisioned a historical process driven by the forces of human migrations, diffusion, and racial admixture, and for each the key to unlocking that history lay in the bones of antiquity. Their quest for culture history intertwined with the constant ebb and flow of human races came to define much of osteology's role in physical anthropology.

In the case of Boas, his thoughts on race and his opposition to racial constructs of the time were mixed at best. On the positive side, he criticized the most basic aspects racial typology. He asked simple but important questions. For example, how could the average represent the norm when all averages are derived from the sum of deviations? How could we accept the fixity of races if traits such as cephalic index (the most sacred of racial traits) changed by the magnitude of a race in one generation, as was demonstrated by Boas (1912) in a classic comparison of Jewish and Sicilian immigrants and their U.S.-born children? If races are in a constant state of transformation, how can we

ever know their number or hope to establish a ranking among them?

Boas also launched a sustained attack against attempts to link race and cultural achievement. He was a major force in the promotion of racial equality (Baker 1994). His criticism of evolutionists such as L. H. Morgan and E. B. Tylor led him to a strong antievolutionary stance (Harris 1968) that repelled all aspects of evolutionary thought. Thus, for all of his positive contributions to modern anthropology, his antievolutionary position was overwhelming and left his students and followers with few questions to ask beyond questions of diffusion, and few methods to apply beyond description.

Hrdlička's major goal at the beginning of the century was to establish an institute of biological anthropology similar to that founded by Broca in France. When his efforts were thwarted (Boas was a major force in impeding funding), he moved to establish the museum as a major research institution. Hrdlička succeeded in transforming the Smithsonian's National Museum of Natural History into a major force in skeletal biology and built a vast collection of skeletal remains.

Hrdlička's (1907) earliest contribution to skeletal biology was the systematic analysis of New World skeletal material. The data were used to refute claims of a pre-Pleistocene occupation of the New World. He spent a considerable portion of his later life examining the Asiatic origins of Native Americans. These works led him to field studies in Alaska and Aleutian Islands that established the shovel-shaped incisor as a racial hallmark linking Asian and New World populations (Hrdlička 1920).

Hrdlička's greatest contribution was the founding of the *American Journal of Physical Anthropology (AJPA)* in 1918. The journal was established with the blessing of Robert Lowie, then-editor of the *American Anthropologist (AA)*, and Hrdlička's editorship would last for 24 years (Glenn 1997: 59). His vision was made clear in the inaugural issue:

> The paramount scientific objective of physical anthropology is the gradual completion, in collaboration with the anatomist, the physiologist, and the chemist, of the study of the normal white man living under ordinary conditions. [Hrdlička 1918:18]

Twelve years later, Hrdlička spearheaded the effort to organize the American Association of Physical Anthropologists (AAPA). Only eight professional physical anthropologists were among the 18 anthropologists that comprised the 85 charter members of the association. Anatomists were the largest professional group with 47 members.

Caucasian biology was the norm against which other races were to be compared. To this end, Hrdlička expressed concern regarding the rudimentary state of racial studies (Blakey 1987:10), and this was no idle concern. Where Boas argued for the independence of race, language, and culture, Hrdlička saw race as a force of nature shaping and constraining the progress of culture. In his own words, "The real problem of the American Negro lies in his brain, and it would seem, therefore, that this organ above all others would have received scientific attention" (Hrdlička 1927:208–209).

While Boas's and Hrdlička's accomplishments were legion, Hooton, a classicist, trained the first generation of leaders in physical anthropology. Hooton trained seven of the eight presidents of AAPA serving from 1961–77. As great as his teaching was, his research reflects the contradictions of the past. For many, *The Indians of Pecos Pueblo* (1930) laid the foundation for modern skeletal biology. In it, Hooton used an epidemiological approach that foreshadowed modern paleopathology. His innovative use of simple statistics such as percentage frequencies would not become common for another 30 years. He was a prime mover in interdisciplinary interpretations based on a solid knowledge of host, pathogen, and environmental relationships.

At the same time, he worked with blinders imposed by a racial typological approach. Fixed racial traits were a reality in his view, and the presence of all such traits required an explanation in strictly racial–historical terms. For example, the presence of Negroid racial features among the Indians of Pecos Pueblo led to a preposterous theory in which he envisioned "pseudo-Negroid" types making their way from northwest Africa, across the Bering straits, and then down to Pecos carrying "a minor infusion of Negroid blood . . . with them" (Hooton 1930:356). Sadly, Hooton's innovative approach to paleopathology remained a footnote to history while his racial typology captured the interest of many researchers.

To his credit, Hooton spearheaded what has been described as the "most sophisticated 'data crunching' operation that anthropologists had seen until the 1950s" (Giles 1997:500). The Statistical Laboratory at Harvard, equipped with state-of-the-art IBM computers, was the forerunner of data analysis that transformed biological anthropology. The new instrumentation did not, however, result in more innovative research. W. W. Howells (1954, 1973, 1989), Hooton's successor at Harvard, continued his legacy of descriptive typology.

SKELETAL BIOLOGY IN THE MODERN ERA: LIFE AFTER THE 1950s

> The test of a first-rate intelligence is the ability to hold two opposed ideas in mind at the same time and still retain the ability to function.
>
> —F. Scott Fitzgerald, *The Crack Up*

> Circumstantial evidence is a very tricky thing. It may seem to point very straight to one thing, but if you shift your own point of view a little, you may find it pointing in an equally uncompromising manner to something entirely different.
>
> —Sherlock Holmes, *The Boscombe Valley Mystery*

The early 1950s can be seen as a watershed for biological anthropology in general and osteology in particular.

Discovery of the double helix set the stage for anthropological genetics, and population studies began to make inroads. But osteological studies continued to reflect the conflicts and contradictions of times past.

In 1952, Georg Neumann's "Archeology and Race in the American Indian" appeared in James B. Griffin's *Archeology of the Eastern United States* (1952). While the book developed new ground in U.S. archaeology, Neumann's contribution (1952) provided nothing more than an old-time treatise on racial typology. Cranial types such as the Lenid and Walcolid reified race and reaffirmed the use of craniometry as a tool for racial–historical reconstructions. Yet Neumann's chapters (1954a, 1954b) were considered sufficiently important to be published in the *Yearbook of Physical Anthropology*.

The linkage between Neumann and typology is in no sense a stretch. He was described by a close associate as "the last and best of the typologists" (Hall 1997:731), who was able to bridge the gap between typological and populational paradigms (Hall 1997). The so-called bridge to population study was apparently based on his use of large collections (over 10,000 total). In reality, a population approach exists nowhere in the work, and two of his types were based on fewer than 20 skulls.

It is stunning to realize that a year before Neumann's treatise, the *Yearbook* published Sherwood L. Washburn's (1953) "The New Physical Anthropology"—an essay originally published in 1951 that became a manifesto for the modern era (Washburn 1951). Washburn presented a promise of a "new physical anthropology" profoundly different from the old. Where the "old physical anthropology" remained locked in endless description, new theoretical perspectives would dominate the new. Most importantly, hypothesis testing based on concepts of adaptation and evolution would be the hallmark of modern research.

The moment was right for new data and a new approach, and William C. Boyd's *Genetics and the Races of Man* (1950) seemed to provide both. Boyd saw the blood groups as a panacea for anthropological research. Their inheritance was understood and their frequencies could be measured with precision. They could be studied objectively without the prejudice associated with features such as skin color. Additionally and most essentially, they were nonadaptive. Thus the old took root in the new.

From the very outset, Boyd viewed the blood groups as unlikely targets of natural selection and, thus, of great potential for tracing population movements and reconstructing historical connections among human races. Rather than seeing the blood groups as an opportunity to break new conceptual ground, Boyd simply replaced an old osteological approach with a new genetic one.

Boyd specifically targeted osteology on methodological grounds. He argued that it is difficult to study skeletal morphology in the living because bones respond rapidly to environmental influences. Their genetics is complex, and the old measurements were never logically conceived. Boyd, in short, asserted that osteology was passé.

Ironically, Boyd's "cutting edge" genetic research remained as devoted to description and typology and as committed to the search for nonadaptive (racial) traits as the osteology he decried. Even when the blood types were shown to be adaptive (Buettner-Janusch 1960; Otten 1967), researchers continued to use them as racial markers. They simply combined multiple blood types (Edmonson 1965) in an attempt to somehow cancel evolutionary influences. We had little more than "new wine in old bottles."

LIFE AFTER THE 1950s: FUNCTIONAL MORPHOLOGY AND BIOARCHEOLOGY

> The pattern of disease or injury that affects any group of people is never a matter of chance. It is invariably the expression of stresses and strains to which they were exposed, a response to everything in their environment and behaviour.
>
> —Calvin Wells, *Bones, Bodies and Disease*

In spite of Boyd's view of skeletal biology as passé, new theoretical developments were beginning to emerge. A functional anatomical approach to morphology and the rise of bioachaeology provided the stimulus. Developments in these areas increasingly came to reflect Washburn's proscriptions for a new biological anthropology, and within it a new skeletal biology.

Functional Morphology

The tools used to understand functional morphology have been available for years. Many of the statistics essential for teasing out functional relationships began before the 20th century. Indeed, skeletal remains provided an important source of data for the development of both regression and correlation techniques. It was statisticians (Pearson and Davin 1924) who used cranial measurements to distinguish between "organic" and "spurious" correlations. Organic correlations measured relationships between distinct regions of the crania while spurious correlations reflected redundant measures within the same cranial (functional) system. This distinction could have laid the foundation for functional craniology; however, its application remained largely statistical.

It was not until decades later that Melvin Moss (1972) and his colleague R. W. Young (Moss and Young 1960) extended the research of C. J. van der Klaauw (1945, 1952) by modeling a "functional components" approach to cranial morphology. In this model, cranial systems such as the masticatory, neurological, and visual were analyzed functionally relative to the soft-tissue organs they supported and protected. Functional craniology provided a powerful tool for the analysis of prehistoric skulls.

David S. Carlson, Dennis P. Van Gerven, and colleagues measured crania from ancient Nubia using a functional craniometric approach (Carlson and Van Gerven 1977, 1979; Van Gerven et al. 1976). They then used discriminant functions to identify patterns of facial reduction and

cranio-facial evolution across some twelve thousand years of Nubian history. The biological data were then used to develop a dietary hypothesis relating facial reduction to the cultural evolution of food production and preparation technologies. William C. Hylander (1975) applied a similar approach to the analysis of Eskimo crania. In this sense, functional morphology became fertile ground for a growing biocultural approach in skeletal biology.

A shift away from race and description would not come easily. Howells rejected such attempts. He stated, "My purpose is not the study of growth but of taxonomy, of the variation between existing recent populations in the dry skull" (1971:210), even though he admitted, "we do not know whether . . . (the) variation is of taxonomic, or functional importance" (Howells 1973:3). What Howells (1973, 1989) provided was a way to bend the potential of discriminant function statistics to the will of old racial classifications.

In this sense, complex statistics, including multivariate analyses, do not insure a nontypological approach. R. E. Blackith and R. A. Reyment (1971) described the difficulty of breaking away from a description and typology even when using elaborate statistical procedures (Armelagos et al. 1982:313–314). Typology continues despite our understanding of adaptation and the processes of morphological change confirming the "superficial nature of biology at the classificatory level" (Blackith and Reyment 1971:5).

Functional analyses of postcranial remains have been less controversial since postcranial morphology has been less central to racial classification. Thus, while there are many forensic methods for racial determination of long bones (Dibennardo and Taylor 1983; Komar 1996), there have been extensive functional analyses as well. For example, C. Owen Lovejoy (1978) and C. B. Ruff and colleagues (Ruff 1984, 1993, 2000; Ruff and Hayes 1983; Ruff et al. 1984) have found an important link between climate, locomotion, subsistence, and cross-sectional geometry of the femur and tibia. Lovejoy has used the approach to address questions of locomotion in early hominids while Ruff and colleagues have used their data to consider the link between activity patterns in food getting and the mechanical properties of bone.

Even features linked most closely to forensic description can be a rich source of biocultural analysis. The human pelvis has been subjected to a number of studies that provide qualitative and quantitative discriminations between male and female pelves (Bass 1995). However, the pelvis can be examined from an adaptive perspective as well—one that models its role in birth and bipedalism (Sibley et al. 1992; Tague 1989, 1994). For example, Sibley et al.'s (1992) study of ancient Nubian pelves revealed high frequencies of pelvic contraction in females. This has, in turn, led to new questions concerning infant mortality at the site. Could there be an interaction between pelvic morphology, neonatal size, and infant mortality? The question is intriguing given that the modal age at death is birth to six months among these ancient Nubians.

An obstetric approach to pelvic morphology has been applied to fossil remains as well. Robert G. Tague and Lovejoy (1986) examined the pelvis of A.L. 288-1 (Lucy) from this perspective and with Karen Rosenberg and Wenda Trevathan (Rosenberg 1992; Rosenberg and Trevathan 1996) developed a broader evolutionary perspective on the birth process in early hominids.

Bioarcheology

In the 1950s, skeletal biology and archeology were stagnating in an era of descriptive particularism that created a moribund state for both disciplines. The "new archeology" transformed archeology by moving it beyond its fixation on description and cultural diffusion. The new approach (Binford 1962, 1964, 1977; Binford and Binford 1968) embraced a concern for the ways in which cultural systems (the technology, social, and ideological systems) adapted to their environments. This, in turn, led archaeologists toward the development of general principles of adaptation that could be applied to both archeological and contemporary cultures. Hypothesis testing and the application of scientific methodology became the hallmarks of this new process-oriented archeology.

Skeletal biologists, propelled by the "new physical anthropology," began developing a biocultural approach to the analysis of skeletal remains that paralleled and supported the trends in archeology. These developments occurred at a time when anthropology was a four-field discipline. Physical anthropology was becoming an interdisciplinary and intradisciplinary undertaking committed to an adaptive and evolutionary perspective often in a cross-cultural setting. In this sense, skeletal biology provided time depth to understanding the adaptive process. Skeletal biology incorporated methodology that it shared with processual archeology to spawn bioarcheology (Buikstra 1977; Larsen 1987, 1997; for a more complete discussion of these developments, see Armelagos in press).

The promise of bioarchaeology required three factors: (1) a population perspective; (2) a recognition of culture as an environmental force effecting and interacting with biological adaptation; and (3) a method for testing alternative hypotheses that involves the interaction between the biological and cultural dimensions of adaptation.

This emergent biocultural view embraced the notion that a society's technology, social organization, and even its ideology could play a major role in inhibiting or creating opportunities for biological events such as patterns of disease. It is not surprising that this new approach found fertile ground in paleopathology (Armelagos 1997). In fact, this relationship is so strong that bioarcheology and paleopathology are linked in the minds of most skeletal biologists.

The traditional focus of paleopathology had been the differential diagnosis of specific diseases such as tuberculosis,

leprosy, and syphilis, but the approach was inherently limited. Bones and teeth do not often respond with the kind of specificity necessary for a clinical diagnostic approach to all diseases. Skeletal and dental remains do, on the other hand, record stress reaction to a vast array of insults. Responses such as trauma, patterns of growth and development, periosteal inflammation, enamel hypoplasia, and differential mortality can be used to ask a host of interesting questions. Their meaning does not lie in the diagnosis of individual cases but, rather, in their pattern by age, sex, and environmental (cultural and natural) setting. All that is required for their analysis is a single a priori assumption: *Patterns of stress response evidenced in ancient populations are the result of systematic environmental forces.* The goal of the analysis is to develop and test hypotheses concerning the forces in play.

The power of bioarchaeology derives from the linkage between archaeological and skeletal analyses. This linkage has made it possible to answer significant questions concerning the adaptation of ancient populations. Examples include the regional investigations of Della Collins Cook (1979), Jane E. Buikstra (1977), and Clark Spencer Larson and George R. Milner (1994), as well as population-specific studies of health and mortality in relation to subsistence (Cohen and Armelagos 1984), trade (Goodman et al. 1992), social stratification (Goodman et al. 1995), political organization (Van Gerven et al. 1981), and contact (Baker and Kealhofer 1996). As with functional morphology, bioarchaeology shifted the focus away from simple description toward analytical questions of biocultural adaptation and in situ evolution. The question is, given the promise of analytical research, has our commitment to description actually given way?

The Conflict

Given the successes of functional and biocultural approaches, the continuing attraction of simple description is surprising. The conflict between description and higher-level analytical (functional and biocultural) analyses reflects in many ways the tension between the new and old physical anthropology. This conflict was noted by Gabriel Lasker some thirty years ago (1970). In Lasker's view, physical anthropology was little more than "the hand maiden to history" (1970:1–2), with little interest in analytical investigations of function, and adaptation. Even when such questions could be asked, description remained the preferred goal.

Lovejoy et al. (1982) conducted a content analysis of *AJPA* a decade later and found Lasker's concern to be well founded. While analytical research increased from 1930–80, descriptive studies remained in the majority among all publications related to osteology.

For the purpose of this discussion, we expanded Lovejoy's survey to include two more recent five-year samples (1980–84 and 1996–2000). Following Lovejoy and colleagues, articles were considered analytical if they proposed and tested specific hypotheses or if they addressed issues of process, function, or attempted to place the analysis into a broader theoretical context (see Table 1). Articles were considered descriptive if they focused primarily on description, sorting methods, or identification without placing the results into a broader theoretical context. What we found reaffirms the concerns expressed by Lasker over 30 years ago. If anything, our survey suggests a shift toward rather than away from description. Furthermore, the pattern does not appear to be changing.

TABLE 1. Content analysis of human osteology articles in the *American Journal of Physical Anthropology,* 1930–84 and 1996–2000. Modified and extended from Lovejoy et al. 1982.

	Osteology %	Analytical %	Descriptive %
1930–1939	36.8	13.5	86.5
1940–1949	33.7	21.1	79.0
1950–1959	42.0	29.7	70.3
1960–1969	44.3	35.4	64.6
1970–1979	51.3	44.1	55.9
1980–1984	55.0	43.0	57.0
1996–2000	56.0	43.0	57.0

There is, however, a certain coarseness to both ours and Lovejoy's surveys. The articles included reflect all aspects of osteological research including paleontology and primate anatomy. There is no question that the historical and theoretical context in which they are framed is extremely diverse. For example, the importance of description when the subject is the remains of a new fossil does not compare easily to the contribution of yet another description of a well-known lesion in a modern human skeletal series.

With this limitation in mind, we conducted a second survey (1980–84 and 1996–2000) focused entirely on modern human osteological remains (see Table 2). We also categorized the articles into four categories according to their major intellectual thrust(s). The categories were analytical, descriptive, methodological, and racial. In cases in which the research had more than one emphasis, such as descriptive and racial, it was counted in more than one category. Thus the percentages do not add to 100 percent.

As with osteology in general, articles devoted to modern human osteology have increased over time relative to all publications in the journal. However, unlike the trend for all osteology research, the amount of description in human osteology has increased by 12 percent compared to an increase of only seven percent in analytical. The frequency of articles devoted to methodology has dropped by 26 percent, and those devoted to or utilizing racial categories have dropped by 14 percent.

These data suggest several things of interest. First, interest in human osteology is not declining. That said, the research is actually becoming more descriptive and relatively less analytical. Interest in race has declined, but such analyses are still abundant. The interesting question is this: If skeletal analyses are more descriptive than ever, yet at the same time less interested in methodology and

TABLE 2. Content analysis of human osteology articles in the *American Journal of Physical Anthropology,* 1980–1984 and 1996–2000.

	Osteology	Descriptive	Analytical	Method	Race
1980–1984	19%	59%	22%	43%	26%
1996–2000	29%	71%	29%	17%	12%

race, what exactly is the nature of the work being published? Ironically, while many anthropologists have decried the use of race, the race concept continues to provide one real, although limited, conceptual framework. Alternative biocultural analyses appear to have stalled, leaving a paucity of alternatives. As a result, many osteologists have returned to the old questions of racial history (often cast in terms of biological distance), migration, and diffusion. In the case of paleopathology, interest has returned to the old questions of differential diagnosis.

The resurgence of description has also been encouraged by the emergence of new techniques and technology. Washburn predicted that the new physical anthropology would develop new techniques as part of its advancement. In fact, new technologies have often impeded rather than promoted a new perspective. Indeed, much of the published work reflects what the philosopher Abraham Kaplan (1964) calls the law of the instrument, that is, *Give a child a hammer and everything in their world needs pounding.* Give an osteologist a CAT scan, and every specimen is scanable.

THE CHALLENGE

The population is the last bastion of the typologist.

—C. Loring Brace

21st-century technology applied to 19th-century biology [comment on the Human Genome Diversity Project]

—Alan Swedlund

The challenges to skeletal biology come from within and beyond the discipline. For example, Public Law 101-601, the Native American Graves Protection and Repatriation Act (NAGPRA), has been a powerful external influence, but its impact has differed from that which was anticipated. It was initially believed that collections would be lost and that new excavations would be limited or possibly eliminated altogether. Neither outcome has come to pass, but the concern led to action. Protocols were developed (Buikstra and Ubelaker 1994) and data were collected quickly and systematically. Collections that languished unstudied for years were carefully described using new standardized techniques. In addition, anthropologists collaborated with Native American groups in conducting new excavations (Rose et al. 1996).

NAGPRA's impact beyond anthropology was in many ways more negative. A perception of anthropologists challenging the rights of Native Americans to bury their dead spread throughout the academy. This perception was reinforced by earlier images of osteologists using craniology to support racial stereotypes prompted an editorial in *Nature* promoting the Human Genome Diversity Project:

> With physical anthropology under a cloud for its habit of using measurable skeletal indices as proxies for less tangible attributes (cranial capacity as a measure of intelligence, for example), it would be better to invest what goodwill there is in a quite different field. [1995:183]

At the same time, images of forensic anthropologist "bone detectives" received positive play in the media. Osteologists are frequently portrayed as key figures in solving the most intractable cases. The demands of NAGPRA and forensics are in many ways the same. The emphasis is on description with a view to practical application. Research and training with little or no applied value,[2] become secondary even in the academy.

Currently, some thirty departments of anthropology offer programs in forensic anthropology. Even the National Science Foundation has jumped on the bandwagon by featuring "forensic paleontology" in its FY 2002 request to Congress. The result has been a shift away from Washburn's "new physical anthropology" back to the traditional techniques of human identification. Once again the diagnosis of age, sex, and race are paramount. Racial diagnosticians, armed with new techniques and technology, map the terrain of cranial morphology much as their forebears did over a century ago. Indeed, confidence in the dry skull for racial diagnosis is little changed from the time of Blumenbach. Osteologist George W. Gill (2000) has gone so far as to proclaim greater confidence in skeletal features than soft tissue ones. He says, "I am more accurate at assessing race from skeletal remains than from looking at living people standing before me" (2000). Unfortunately, Gill's confidence belies the objective evidence.

Goodman (1997) demonstrated that the 85–90 percent accuracy claimed by forensic anthropologists is seriously misleading. High levels of accuracy can be achieved, but only when the skulls meet extremely limiting criteria. For example, Giles and Elliot's (1962) discriminant function formula is based on a reference sample of known composition, and it can indeed achieve 85–90 percent accuracy. This level of accuracy is reached only when tested against additional specimens from the same reference sample. When applied to independent samples of known composition (the true measure of its success), the method is less than 20 percent accurate (Goodman 1997)—a figure that hardly inspires confidence in forensic anthropology's ability to race a skull notwithstanding Gill's confidence.

Poor performance has not disabused forensic anthropologist from selling the method. Fordisc 2.0 (Ousley and Jantz 1996) is a computer program designed to diagnose any skull into one of Howell's geographic populations. The program, however, is seriously flawed (Kosiba 2000). When applied to a cranial from a known African population (Belcher et al. 2002; Leathers et al. 2002), some fifty percent were placed in non-African categories.

The failure is interesting if we allow ourselves to think beyond the applied box. The program *forced* a solution

based on a priori racial criteria presumed (as all racial schemes do) to delimit patterns of real human variation. What we see with the African test is the result of an astounding mismatch between actual cranial variation and the variation modeled by racial constructs. As we have known for decades, so-called racial traits are nonconcordant, and the races we get are little more than a function of the trait or traits we use. Sadly, the response has been directed more toward fixing the program than fixing the approach.

The second challenge to skeletal biology came from the postmodern critique of science in general. The rejection of evolutionary theory, theories of cultural adaptation, as well as ecological interpretations, have cast a pall across much of anthropology (Johnson 1999). Changes in archeology have been profound. Bioarcheology has survived the postmodern onslaught because it has had a means of objectively testing hypotheses. Biological outcomes in the form of patterns of pathology and morphology have been a major factor in maintaining an adaptive and evolutionary perspective. Forensic anthropology, descriptive by nature and devoid of sociocultural content, has remained largely off the postmodern radar screen. For this reason, description with an eye to practical application provides a safe harbor for osteological research. The attraction is indeed twofold. It is outside the net of postmodern critique, and it enjoys wide appeal among the public at large. The result has been an ever narrowing research agenda that many osteologists find comfortable.

This is not to suggest that bioarchaeology and biocultural analyses are beyond legitimate criticism. Indeed, critics are essential to a vibrant science, and harsh criticism should not make osteologists timid. For example, J. M. Wood et al. suggested that biocultural reconstructions suffered from what they saw as an "osteological paradox" (1992:345). Skeletons with the most pathology may represent not the sickest members of a population, as bioarcheologists contend, but, rather, the healthiest. The rationale is simple. Long-lived (healthier) individuals survive to accumulate the greatest abundance of skeletal damage. In other words, paradoxically, bad skeletons mean good health. While this may seem logical, it is not necessarily the case.

Likewise, porotic hyperostosis, a lesion interpreted by most osteologists as a sign of disease, has been interpreted by others as a sign of adaptation (Stuart-Macadam 1992). Such conflicts and contradictions do not threaten the discipline nor are they beyond resolution. Goodman (1993) has used multiple lines of evidence against Wood's "paradox" and in the process clarified many aspects of biocultural research. R. P. Mensforth et al. (1978) demonstrated a direct relationship between porotic hyperostosis and systemic infection (periosteal reaction), and Diane M. Mittler and Van Gerven (1994) found a significant link between the lesion and reduced life expectancy. Both studies lend strong support to the disease hypothesis.

Many students, believing that bioarchaeology has been mortally wounded, shy away from both the risk and the controversy by pursuing more conservative research. Our point is this: Criticisms of bioarcheology and biocultural reconstructions do not require a retreat back to race, descriptive typology, and diffusionism. They represent opportunities to develop and test alternative hypotheses in the finest scientific tradition. However, there seems to be a force that draws us back to descriptive historicism. Current interest in mtDNA is reminiscent of interest in the blood groups a half century ago. Expensive high technology research has the cache of cutting-edge science. But where is mtDNA research taking us? The questions are the old ones of diffusion and descent, grounded in a view of culture history driven by ancient migrations and the admixture of ancient populations. For example, one of the most celebrated studies in mtDNA (Cavalli-Sforza et al. 1994) research is a study of the racial history of human populations. Questions of in situ evolution and population adaptation remain as always antithetical to the methods at hand. Even as evidence grows for the operation of selection on mtDNA, we are assured, as we were with the blood groups, that all is well with studies of origin and population distance (Torroni et al. 2001).

CONCLUSION

It has been almost four decades since Leslie A. White (1965) provided a retrospective and prospective view of cultural anthropology in his American Anthropological Association (AAA) Presidential Address. Today, his concerns apply with equal force to biological anthropology. White took issue with anthropology's obsession with the repetitive analysis of certain things. He states:

> As the number of excavated Ohio mounds or Southwestern pit houses increases, the significance of one more "dig" decreases; the law of diminishing returns sets in . . . I am simply raising the question of the law of diminishing returns as it has been operating in our science in recent decades. I am raising the question: Can cultural anthropology do anything more valuable and significant, and should it try to do so? [1965:630]

We are simply asking this: As the number of diagnoses and racial–biological distance schemes increases, does the significance of yet another diagnosis or distance study diminish? Does the law of diminishing returns set in? Most importantly, can biological anthropology do anything more valuable and significant, and should it try? We question the need to publish the report of another single pathological specimen to understand the chronology or geography of a specific disease. Is the search for origins using mtDNA and discrete dental traits the best use of research time and research dollars?

In White's prospective view of anthropology, he suggested that we had to address problems more relevant to contemporary society. White was not the first to argue for an anthropology that is relevant to everyday life. One hundred and twenty years ago, Edward B. Tylor concluded

his book *Anthropology* with, "The knowledge of man's course of life, from remote past to the present, will not only help us forecast the future, but may guide us in our duty of leaving the world better than we found it" (1881:439).

To meet these goals, we have to reclaim skeletal biology as the means to understand morphology from a functional perspective and adaptation and evolution from a biocultural perspective. This implies an interdisciplinary and intradisciplinary approach that is integrated with cultural anthropology, archeology, linguistics, and other aspects of biological anthropology. We believe that skeletal biology has much to offer in understanding issues that are relevant to contemporary society. Rather than being obsessed with constructing racial classification, we should be examining the biological consequences of racial analysis. Skeletal biology can help us understand the factors in evolution that have led to the global patterns of emerging disease. Nutritional problems that are affecting developed nations and the Third World can be better understood from an adaptive and evolutionary perspective of bioarcheology. Issues of inequality that are a part of many of the contemporary problems should be the focus of bioarcheology. Inequality had its beginning in our remote past, and we should be able to chart its course. Widening gaps in social, political, and economic inequality need to be understood from an adaptive and evolutionary perspective.

Reclaiming physical anthropology as anthropology will require that we reevaluate our past and recast the field for the future. The leading journals in the field, *AJPA, AA,* and *Evolutionary Anthropology* have to take a more proactive position in promoting discussion of what our *futures* may become. The start of a new century should be a good time to begin.

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NOTES

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1. The third descriptor refers to cranial type.

2. We use *applied* in the broader anthropological sense. Forensic anthropology has made significant contributions to issues of human rights.

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