Histories of Scholars, Ideas, and Disciplines of Biological Anthropology and Archaeology

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Franz Boas defined anthropology over a century ago as a four-field discipline in danger of fragmentation. It has endured with stresses and strains by maintaining many integrative ties among the sub-disciplines. Vibrant links indicate that a healthy endeavor remains among anthropologists dedicated to an interdisciplinary approach. Little and Kennedy suggest that the story can be told by the histories of individuals, institutions, ideas, and traditions. However, they focus on individuals and institutions and seldom consider the histories of ideas, leading them to omit significant events. Fagan describes the origins of archaeology more thoroughly and situates individuals within the larger context.

KEYWORDS archaeology, biological anthropology, general anthropology, history

The St. Louis World’s Fair, more precisely the Louisiana Purchase International Exposition, provides an example of the overt racism that pervaded the fabric of American life at the turn of the 20th century when anthropology was in its incipient phase of its modern development. Expositions were important social movements designed to reassure order to a population that was “unsteadied by the overwhelming changes in world society” (Mathe 1999:54). Indigenous groups were brought to St. Louis,

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following a practice that began with the 1893 Columbian Expedition in Chicago where “tribes” were viewed as “living fossils” providing indisputable visual evidence of the evolutionary advancements of the “white races.” These expositions showcased modern technology contrasted with that of the “savages,” as they were referred to in the newspapers.

“Anthropology Days,” a highlight of the 1904 Olympic Games held in conjunction with the Exposition, reinforced racial stereotypes typical for that time (Brownell 2008; Parezo and Fowler 2007). W. J. McGee, one of the United States’ most prominent anthropologists and who referred to himself as “Ethnologist-in-Charge” at the Bureau of American Ethnology, organized “Anthropology Days” (Rydell 1987:160). McGee, who was later elected as the first president of the American Anthropological Association, organized “Days” as a two-day event to demonstrate the evolutionary progress that humans have made (Anonymous 1903:29), “from the dark prime to the highest enlightenment, from savagery to civic organization, from egoism to altruism. The methods will use living people in their accustomed avocation as our great object lesson.”

For McGee, the enlightenment was the movement away from a reliance on traditions and customs to a belief in rationality and science. McGee was expressing a philosophy that was a culmination of an evolution of thought that began in the 18th century (Wolff 2007). McGee saw the “wild tribes” as fitting into four evolutionary grades: savagery, barbarism, civilization, and enlightenment. He thought that those with the simplest culture were closer to nature and therefore would be the best athletes. To test his hypothesis, McGee organized their participation in traditional Olympic events such as the shot put, sprints, distance running, and pole vault.

The tribal competitors failed miserably in the Olympic events. The official history of that Olympic Games (Bennitt and Stockbridge 1905) judged the endeavor to be a failure. A “Pygmy” put the shot 13’ 7” and an Ainu tossed the 56-pound weight a distance of 3’ 2”, destroying the belief that the greatest natural athletes were to be found among the “uncivilized” tribes in various parts of the world. McGee, reconsidering his earlier prediction, surmised that civilization is not only good for the mind but for the body as well.

FRANZ BOAS AND THE FOUNDING OF ANTHROPOLOGY

As the events of “Anthropology Days” were being played on the fields of the Olympics, Franz Boas was certainly aware of the charade that portrayed the evolutionary history of the human species at the Exposition. His experience with the 1893 Columbian Exposition in Chicago turned Boas against the prevailing evolutionary framework that linked race and cultural development. Boas (1896) forcefully presented his view of cultural relativism in “Limitation of the Comparative Method,” in which he argued for what has become known as historical particularism. He claimed that each culture was a unique
expression of historical and environmental events and consequently any hierarchal evaluation of them was meaningless. Boas was certainly arguing for moving beyond race and evolution to understand cultural development, though the level of Boas’s resistance to evolutionary mechanisms remains a matter of debate.

An important part of the St. Louis Exposition was played out of public view in scientific sessions. Franz Boas presented a paper on “The History of Anthropology” which defined the field of anthropology:

The historical development of the work of the anthropologists seems to single out clearly a domain of knowledge that heretofore has not been treated by any other science. It is the biological history of mankind in all its varieties; linguistics applied to peoples without written language; the ethnology of people without historic records, and prehistoric archaeology. (Boas 1904:523)

This definition is noteworthy in that anthropology was defined as a four-field endeavor, but it was his comments a few paragraphs later that reverberate to this day:

The field of research that has been left for anthropology in the narrower sense of the term is, even as it is, almost too wide, and there are indications of its breaking up. The biological, linguistic and ethnographic-archaeological methods are so distinct that on the whole, the same man will not be equally proficient in all of them. The time is rapidly drawing near when the biological branch will finally be separated from the rest, and become a part of biology. (Boas 1904:523).

Boas was aware of the fragile nature of the four-field union and even within his statement suggests that anthropology was beginning to disintegrate. Stocking (1988) has argued that, given anthropology’s accidental beginning and its fragility at its origin, it might be time to give up the illusion that it is thriving as a four-field endeavor. He argues that the notion of four-field anthropology is a fiction that we maintain as our “sacred bundle” that is brought out in times of intellectual crisis. As a challenge to this perspective, I will evaluate two books, Histories of American Physical Anthropology in the Twentieth Century (Little and Kennedy 2010b) and A Brief History of Archaeology: Classical Times to the Twenty-First Century (Fagan 2004).

In Unwrapping the Sacred Bundle: Reflections on the Disciplining of Anthropology, Segal and Yanagisako (2005b) challenge the validity of the four-field perspective by accepting Stocking’s assessment of anthropology’s origin as a historically contingent phenomenon that at its inception showed “indications of breaking up” (Boas 1904:523). Through this position, they advocate jettisoning the four-field approach that has defined the discipline since its inception (Segal and Yanagisako 2005a; Yanagisako 2005).
Segal and Yanagisako (2005a) show an especially strong contempt and antipathy for the biocultural perspective, which they see as reductionist and deterministic. They argue that the biocultural perspective privileges biological explanations, which at best are narrow and crass and at their worst are sociobiological. In addition, they call for a rejection of biocultural anthropology, arguing that it is tainted by an evolutionary perspective. While evolution in early anthropology was tinged with racism, this argument misinterprets the essentials of biocultural anthropology and instead paints contemporary practitioners of the perspective with what Lévi-Strauss (1952) called the “original sin” of an earlier era in our discipline. These criticisms make it necessary to understand the origins of anthropology with all its imperfections. The criticism of the biocultural perspective is important since it became an integrative force in physical anthropology that necessitated ties to cultural anthropology, linguistics, and archaeology.

It may appear that I am waving the sacred bundle as Stocking suggests; however, I am arguing not that anthropology must be a strict four-field discipline a la Franz Boas or Alfred Kroeber, but that it is instead a discipline that has been transformed by specialized branches of academics that share their identity as anthropologists with a common understanding of the need to interpret and examine our past, present, and future (Kuper and Marks 2011). Boas was correct that few individual anthropologists could control all four fields as he or Kroeber did, but indeed anthropology has retained its interdisciplinary flavor. Four-field anthropology has changed dramatically, but there is significant evidence of the integration of physical anthropology, archaeology, and linguistics, all with strong ties to cultural anthropology. Little and Kennedy (2010b) and Fagan (2004) both attest to the viability of the sub-disciplines of biological anthropology and archaeology and their commitment to an integrative anthropology.

HISTORY OF PHYSICAL ANTHROPOLOGY

Histories of American Physical Anthropology in the Twentieth Century (Little and Kennedy 2010b) discusses the development of physical anthropology from its beginnings in the late 1800s, when its primary concern was the justification of slavery, to a modern science that considers human variation and human evolution. Thirteen chapters trace the journey that moved biological anthropology to its modern incarnation. The introductory chapter provides an overview of the history of American physical anthropology (Little and Kennedy 2010a). Little and Kennedy discuss the early history of the discipline with respect to its static nature, which mired the discipline and strapped it with a reliance on racial typology as a methodological tool and a perspective that supported a eugenicist and racist agenda. Even with Little and Kennedy’s critical evaluation in the initial chapter, subsequently there is a disjunction in
which many of the authors shield the founding fathers’ prevailing cultural views and their role in supporting a typological and racist perspective. One could make a case for understanding the cultural pressures that led many physical anthropologists to endorse a racial typology that supported a racist perspective. While these scientists were products of their times, others such as Boas rejected racial typology and the racism it engendered and were marginalized by the scientific majority for their views. It is the prevailing perspectives, methods, and theories that demand explanation irrespective of the men and women who carry them.

The first five chapters of Histories treat the early years in greater depth. C. L. Brace (2010) accounts for developments at the turn of the 20th century with a discussion of the impact of French anthropology on Americans’ views on race and the place of Neanderthals in human evolution. However, his earlier treatment of the issue (Brace 1982) was a more thorough discussion of the role of race in anthropology at the beginning of the 20th century. A number of the contributions consider the impact of great names on the development of early American physical anthropology. Little (2010) discusses the contribution of Franz Boas in four-field anthropology with special emphasis on physical anthropology. D. J. Ortner (2010) examines Ales Hrdlicka’s role in the founding of the American Journal of Physical Anthropology (AJPA) and E. J. E. Szathmary (2010) discusses how Hrdlicka founded and organized the American Association of Physical Anthropologists (AAPA). These chapters have an annoying repetition of Hrdlicka’s role in founding AAPA and AJPA. Later in the book, Relethford (2010) repeats material from the previous chapter by Marks (2010) concerning Carleton Coon’s role in supporting the racist rants of Carleton Putnam (Putnam 1961; Putnam 1967). These contributions in Histories were originally presented at the 2005 annual meeting of the AAPA, which should have given the editors and contributors five years to reduce the duplication that detracts from the book.

The authors who provided biographical data on Boas, Hrdlicka, Hooton and others missed an opportunity to delve deeply into their subjects. Lohmann (2008) notes that biographies of anthropologists can have as much empirical and theoretical importance as cultural anthropologists’ ethnographies, the grammars of linguists, and archaeologists’ and biological anthropologists’ monographs. Biographies, according to Lohmann, can depict “cultural dynamics from a person-centered, intimate, experience-near, and diachronic perspective on anthropology’s cluster of sodalities” (Lohmann 2008:89). The biographer has the responsibility to provide an accurate rendering of events. Lohmann suggests:

Avoiding ethnocentrism while providing accuracy in written accounts is a central tenet of anthropological wisdom; applying the cognate principle to biography, anthropological biographers are compelled to write
sympathetically yet honestly of individual biographical subjects, repre-
senting subjects’ own perspectives and analyzing the causes and con-
sequences of their actions. Doing this well in biography is made more
complicated by the convention in some cultures that the recently dead
are not to be criticized, and by the fact that those yet alive are likely to
disapprove of at least some of the ways they have been portrayed.
Awareness and separation of emic and etic perspectives are as important
in biographies as they are in ethnographies. (2008:90)

**BOAS AND PHYSICAL ANTHROPOLOGY**

Little (2010) provides an excellent review Boas’s contribution to physical
anthropology, but his account is flawed by his unwillingness to confront
the unpleasant ideas that prevailed at the time. Little does consider the racism
and anti-Semitism that clouded the treatment of Boas. As Little shows, Boas
made contributions to anthropometry, biometrics, growth and development,
migration studies, skeletal biology, and the study of race. In his contributions
to racial studies, Boas argued against fixity of racial typology and the subject-
vivity of racial classification, and was also vocal in the fight against racism.
However, as Little notes, Boas’s legacy regarding his impact on biological
anthropology was minimal given that he produced only six graduate
students in the subfield. Little further suggests that Boas was marginalized
in physical anthropology, despite publishing 180 articles, because his
extensive research in cultural anthropology, folklore, and linguistics oversha-
dowed his contributions to physical anthropology. Despite Boas’s commit-
ment to developing the integrative approach, his legacy did not retain all
four fields to the same degree.

There are, however, more compelling reasons for why Boas was mar-
ginalized. His marginalization requires a political economic analysis of the
academic environment in which anthropologists such as Boas were working.
Boas was marginalized by fellow scientists and the popular press for his
position on race and racism, as well as his outspoken political views. His
message was lost in the din of the prevailing racist view of society, and he
was overtly attacked for his scientific ideas. Boas’s influence was challenged
from the beginning by anthropologists such as Daniel G. Britton, whose view
of race reflected a consensus of the scientific community (Baker 1994).
Brinton’s presidential address to the American Association for the Advance-
ment of Science (AAAS) (Brinton 1896) was printed in its entirety in *Popular
Science Monthly*, one the most prestigious magazines for the lay audience. In
it, Brinton (1895:246) claimed that the anatomical differences in the guts of
the “the black, the brown and red races” are so great that even with equal
brain power they could never rival the white majority. He asserted,
“especially in their splanchnic organs, that even with equal cerebral capacity,
they never could equal their results with equal effort” (Brinton 1895:249). Boas’s challenge to Britton’s racist argument was given only a single column in the *Monthly* (Baker 1994).

Boas (1940:173) emphasized the peculiarities of racial classification, which were dependent largely on the previous experience of the observer, not upon the morphological value of the observed traits. This explains the diversities of opinion in taxonomic classification. Following this reasoning, Boas argued that racial classifications are social constructs. By using biological traits, one gives the impression that race is a natural biological unit, when in reality it remains a cultural construct that is based on biological descriptors. Williams (1996) rightly claims that Boas paradoxically criticized race but frequently used racial analysis in discussing human variation, often using the terms superior and inferior in describing features that were by definition racial traits.

There is another example of Little’s failure to consider an important political event in the life of Boas’s scientific career. Boas’s voice with respect to race and other issues was diminished after he was censured by the American Anthropological Association (AAA) in 1919 and removed from the influential National Research Council (NCR) committees (Price 2000; Price 2002). This action was the result of a letter that Boas (1919) wrote to *The Nation* in which he challenged President Woodrow Wilson’s claim that democracies do not employ spies. Boas pointed out Wilson’s hypocrisy by charging that not only did America employ spies, but that anthropologists, whom he did not name were using their role as field researchers to cover their service as spies. The censure, which was voted on the day after the letter’s publication, was used to attack Boas as an anti-American pacifist whose views on racism were “antiscientific” (Baker 1994:150). A case can be made that the action against Boas was driven by anti-Semitism (Hyatt 1990) and collective professional jealousy. Those in the AAA who voted against the resolution of censure, his students and colleagues, were prohibited from further service on any of the important NRC committees and self-interest groups that felt threatened by his ethical statements on anthropological practices. On June 15, 2005, the AAA uncensored Boas (AAA 2005), 86 year after the damage was done. Given the extensive coverage of the censure (Baker 1998; Darnell 1988; Price 2000, 2001, 2002; Stocking 1968), it would be hard to imagine that Little was not aware of the event nor of its significance in marginalizing Boas’s early contributions to biological anthropology.

Little (2010) claims that Boas was supportive of an evolutionary approach to human populations. Boas believed that race, racism, and evolutionism were inextricably linked; he needed only look out the window of the scientific sessions to the Olympic playing fields at the St. Louis Exposition to see the links among race, racism, and evolutionism in action. While Boas would have discussed ways in which evolutionary mechanisms could result in change at the level of the trait or gene, he remained committed to rejecting
broad evolutionary changes in biology and was adamant in his rejection of cultural evolution. He is clear on this point saying, “the grand system of the evolution of culture, that is valid for all humanity, is losing much of its plausibility” (Boas 1904:252). Furthermore, his brand of historical particularism rejected attempts to generalize about the cultural and social processes.

ALES HRDLICKA AND THE FOUNDING OF THE AAPA

Ales Hrdlicka is considered another hero of physical anthropology. Ortner (2010) implies that he was relatively uninfluenced by the prevailing racist views of his contemporary scientists. Ortner and others have not cited Blakey’s (1987, 1997) critical evaluation of Hrdlicka’s scientific views which embraced some of the prevailing racist views and the role that anthropologists played in supporting the eugenics movement. Ortner was well aware of Blakey’s research since he had a role in supporting it. Anthropologists such as Davenport (1911) were pushing the eugenics agenda as a way to resist what they saw as the “tide” of racial mixing. Blakey details the influence that non-scientist eugenicists such as J. H. Kellogg, a co-founder of the cereal company that bears his name and a key figure in his Race Betterment Foundation, had on the field. He and Davenport were charter members of the editorial board of the *AJPA* at its founding. In all of the discussions of Hrdlicka, there was no mention of his statement on the goals of physical anthropology that reflected the influence of the prevailing world view of America. In the inaugural issue of *AJPA*, he states: 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. (Hrdlicka 1918:18)

Caucasians in this view represented the “normal” type to which the other pathological types were to be compared as the standard. Later, in line with this view, Hrdlicka (1927:208) stated, “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.” These issues do not detract from his contribution to physical anthropology but reflects the eventual advances made in the field as it moved away from its adherence to a system that supported and strive to maintain racial inequality.

Emoke J. E. Szathmary offers an insightful recounting of the origin of the AAPA. She recounts Hrdlicka’s skills in forming the association but failing in his goal to establish an Institute of Physical Anthropology to be modeled on Paul Broca’s institute in Paris. The institute was a life-long goal of Hrdlicka and the failure to establish it represented a major disappointment in his life. The formation of the institution was opposed by Boas and T. Wingate Todd, a well-respected anatomist who claimed it would weaken existing scientific
societies. Hrdlicka, despite this opposition, was able to build a coalition of anatomists by encouraging them to publish in the *AJPA*. This plan was successful since the majority of the founders were biomedically trained scientists and not anthropologists (Szathmary 2010). Only one-fifth of these founders were anthropologists and only 10 percent were physical anthropologists. What drew this disparate group was the shared topic of study: humans. As noted in the initial article in the initial issue of *AJPA*:

A branch of science may be defined as a portion of systematized research that extends to closely related phenomena and has become the special function of a class of qualified observers. One of the most interesting and far-reaching of such branches is Anthropology. This has been frequently but somewhat vaguely defined as “the science of man;” perhaps a more fitting definition would be “the comparative science of man,” for its main characteristic, the criterium [sic] in fact, which differentiates it from many closely related branches of science, is that of comparison. More specifically Anthropology may also be defined as that portion of systematic research which deals with the differences, and causes of the differences, in structure, in function, and in all other manifestations of mankind, according to time, variety, place, and condition. (Hrdlicka 1918:3)

The anatomical bias of this perspective was evident in the journal’s early years. Goldstein (1940), when conducting a retrospective study of the early decades, found that more than 50 percent of the articles were anatomically oriented.

**FORENSIC ANTHROPOLOGY**

Kennedy’s (2010) chapter on early figures in physical anthropology emphasizes the development of forensic anthropology. This eclectic chapter is a somewhat disjointed (no pun intended) discussion and again includes the obligatory material about Hrdlicka repeated from earlier chapters. Kennedy does review interesting material covering the rise of academic anthropology, though it is unfortunately self-limited to data from the 1950s that detail the number of courses offered in the various subfields of anthropology. There is no follow-up to any recent data on course offerings in the contemporary setting. The chapter focuses on the contribution of these founders of forensic anthropology as a specialty. This objective is problematic since there is no steadfast footprint of forensic anthropology as an academic discipline, although it has become an object of academic investigation just recently. Kennedy (2010) lists 14 scholars (“grandparents”) who were founders of applied forensic anthropology, and it is interesting to note that their
contributions to forensic anthropology were incidental to their major research focus. Wilton Krogman was a major figure in growth and development (Krogman 1941, 1972), while making significant contributions to forensic anthropology (Krogman 1939, 1962). In a similar fashion, T. Dale Stewart made significant contributions to skeletal biology and human evolution while providing input to forensic science (Stewart 1979). Forensic anthropologists are making incredible contributions to identifying remains in mass disasters and testifying in human rights cases (Kahana and Hiss 2009), but as an independent academic discipline it has limits.

Kennedy attempts with limited success to make the case for forensic anthropology as a scientific endeavor rather than a set of skills. I find his position difficult to support. A committee of the NRC (2009) has presented a scathing analysis of the lack of science in “forensic science.” In addition, forensic anthropologists perpetuate the notion that they can identify the race of a cranium. The growing interest and subsequent desire to focus on forensic anthropology is further problematic because of the lack of opportunities for individuals to use their education as forensic scientists. However, while most forensic anthropology programs provide an obligatory disclaimer that the employment opportunities for forensic anthropologists are limited, they continue to train and produce large numbers of graduates. Even those with graduate-level forensic anthropology degrees are not able to find positions in their field and are forced to take positions as a criminologists, for which they are overqualified.

The last seven chapters cover new ground with the growth of the AAPA following World War II. Eugene Giles (2010) considers later historic figures that laid the foundation for the modern incarnation of the profession. Gabriel Lasker's (Little and Kaplan 2010) and S. L. Washburn's (Stini 2010) roles in the recent expansion of the discipline are described in detail. Two chapters (Marks 2010; Relethford 2010) consider the controversial concept of race, a core consideration in physical anthropology. Kaye B. Brown and Matt Cartmill (2010) present an interesting content analysis of 75 years of papers presented at the AAPA meetings. They show a dramatic increase in membership from 400 in 1960 to over 1,600 in 2000. The attendance reflects the shift in number of presentations from fewer than 100 presentations in 1960 to over 600 presentations in 2004. The health of the AAPA is evident with future potential for growth given that 45 percent of registrants at AAPA meeting are students. The last chapter, by C. S. Larsen (2010), is a valuable discussion of how the descriptive nature of physical anthropology is moving into a more analytical phase.

Giles (2010), in his obligatory comments on Ales Hrdlicka, offers two interesting intellectual positions taken by Hrdlicka. He was adamant in claiming that the North American continent was colonized relatively recently and no earlier than 10,000 years ago, and in fact had some quirky views on race crossing. In response to a direct inquiry from President Franklin D. Roosevelt concerning the consequences of race crossing, Hrdlicka claimed
that European matings with Chinese and Japanese were “bad.” In another interaction with Roosevelt, who suggested that the Japanese’s Ainu heritage might explain their imperialistic expansion, Hrdlicka supported the president’s view by claiming that the Ainu skull was 2,000 years less developed than “ours” and that may help to explain the nefarious nature of the Japanese (Giles 2010).

E.A. HOOTEN AND PHYSICAL ANTHROPOLOGY

Giles provides some insights into the career of E. A. Hooton in the same chapter. He cited Hooton’s record as a popular undergraduate teacher and a successful mentor of graduate students, with at least two generations of Hooton’s graduate students subsequently becoming influential professionals and assuming leadership positions within the profession. Giles details the commitment that Hooton had to typology in his research. Hooton was an avowed typologist in his analysis of skeletal and living populations using race as a methodological tool. His study of the skeletal remains from Pecos Pueblo (Hooton 1930) and the Canary Islands (Hooton 1925), living populations that included American criminals (Hooton 1939), the Irish male (Hooton and Dupertuis 1955) and analysis of individual morphology using William H. Sheldon’s somatotypes (Sheldon 1940, 1954) were deeply indebted to typology. Hooton developed a method of typological analysis that greatly influenced physical anthropology for decades, arguing that an individual could be racially typed and that this racial typology would provide information on the biological history of that individual and in this way the population.

Even when publications seemed to anticipate the modern era, race overshadowed the innovative feature of the studies. E. A. Hooton’s *The Indians of Pecos Pueblo* (1930) has been described as one of the most important publications of its time. It is frequently cited as the defining moment in the birth of modern skeletal biology. Hooton established the paleoepidemiological approach that introduced quantitative analysis as a methodology. In retrospect, Hooton’s perspective had little impact until recently. James Roney (1966) was more influential in stimulating an interest in paleoepidemiology, though to his credit, Hooton enumerated the observations that were used to determine the frequency of a specific lesion. Although this may seem a trivial innovation, until the late 1960s researchers routinely failed to provide this information. It was not until the 1970s that rudimentary epidemiological methods became a basic research methodology in bioarchaeology.

Hooton (1926) believed that racial analysis was more reliable in the skeleton. Nonetheless, in *The Indians of Pecos Pueblo*, Hooton distilled a racial typology that described the skeletal population as a composite of a number of racial groups. He accepted the premise of “pure races,” defined by fixed
immutable genetic traits present since their origins. Although Hooton claimed that the racial typology of Pecos was only an exercise that did not necessarily reflect population history, he argued later in the book that the “African” racial types found at Pecos were features that the population retained as a result of their African heritage. In Pecos, Hooton (1930) defined a “Pseudo negroid” type in the sample of skeletons. He states that if an “undiluted” Negro type were found, it did imply a “full African Negro type” which implied no indication of a genetic relationship. However, the similarities exist and begged to be explained. Hooton resorted to the obvious explanation that the “Pseudo Negroids” were the result of “earlier invaders who worked their way up northwest Asia across the Bering Straits down the New World, (and) carried with them a minor infusion of Negroid blood which had trickled in from the tropical parts of the Old World” (Hooton 1930:356). In this way, the typological classification becomes an explanatory tool (Armelagos 1968). Even with its innovative approach to paleopathology, the epidemiological perspective of Pecos remained a footnote to history. It was the racial typology that is Hooton’s legacy.

The culmination of Hooton’s typological studies was an analysis of 9,521 Irish men, which he parsed into 11 types that included Pure Nordic, Predominately Nordic, Keltic, Dinaric, Mediterranean, and Pure Mediterranean (Hooton and Dupertuis 1955). Their taxonomic scheme made it necessary to assign every individual to one of these types, with the assumption that genetic linkages existed between their phenotypic traits and the taxonomy. In the study of the Irish men, the linkage included traits such as hair color, eye color, cephalic index, nasal index, and stature. In retrospect, Hunt noted that “only meager” and “contradictory demonstrations” of linkage have been demonstrated and there were no linkages with Hooton’s sorting criteria. Hunt saw no genetic reason “why...ancestral morphological types would persist in an ancestral population more than one might expect by chance” (Hunt 1959:81). Hunt was especially critical of the use of morphological types to establish relationships between populations that were geographically distant.

The influence of Hooton waned after World War II. Physical anthropology experienced growth after the war, driven by veterans returning to the universities, the establishment of the Yearbook of Physical Anthropology, and Summer Institutes supported by the Wenner-Gren Foundation (Little and Kaplan 2010). The Wenner-Gren foundation has had a profound and continuing impact on the field by acting at a catalyst for interdisciplinary cooperation.

WASHBURN AND THE NEW PHYSICAL ANTHROPOLOGY

Washburn, as Stini (2010) noted, defined the “new physical anthropology,” in which he rejected classification and description for a more dynamic
investigation of process and hypothesis testing (Washburn 1951). Genetic
traits such as blood groups would be the key to the meeting of these goals
(Boyd 1950). While the racial classification of the 1950s had the trappings of
dynamic populations processes, they remained typological and static (Las-
ker 1970). The benchmark of the 1950s that heralded the beginning of
the biocultural approach was the volume published (Warren 1951) from
the “Origins and Evolution of Man” symposium held at the Cold Springs
Harbor (CSH) Biological Laboratory (CSH 2010). The conference high-
lighted the differences between population approaches to typology in
human evolution. The discussions were extremely critical of typology in
physical anthropology and argued for a population-level analysis in which
the genetic construct of race was featured in half of the papers. Unfortu-
nately, by today’s standards the papers on the racial traits and racial classi-
fications were typological.

The seeds of the transformation of the sub-discipline can be seen in
“Anthropology Today,” a symposium sponsored by the Wenner-Gren Foun-
dation that produced an encyclopedic volume (Kroeber 1953) in which
Washburn (1953) contributed “The Strategy of Physical Anthropology.”
Washburn proposed revolutionary changes in biological anthropology that
would define its future, meant to replace the “old physical anthropology”
that was characterized by endless descriptions used to classify individuals
and populations into racial groups. Previously, fossil anthropoids and homi-
noids would be described and typed without much interest in developing a
synthetic analysis. In “new physical anthropology” this descriptive classificat-
dory approach would be displaced by theory-driven analysis. Washburn saw
evolutionary theory that could link to experimentally verifiable results as
the salvation of physical anthropology. Hypothesis testing using models of
adaptation and evolution was a major objective of Washburn’s vision for
the future (Washburn 1951:299).

Stanley M. Garn’s (1962) take on the “newer physical anthropology,”
which did not mention Washburn, seemed to support Boas’s vision of what
would happen to physical anthropology. Garn argued the new physical
anthropologist requires new biomedical technology. He allied physical
anthropology with biomedical sciences and did not believe that graduate
programs in physical anthropology would be able to train their students
with the equipment that they would require to push forward the discipline.
In one sense, Garn was correct. There has been an emphasis on the appli-
cation of new technologies in the solutions of old problems, but training
has occurred in anthropology programs with a biomedical orientation.
While giving voice to the new physical anthropology, it remained mired
in a methodology committed to typological analysis. This perception was
confirmed by Gabriel Lasker who observed that while physical anthropol-
ogists may talk about a processual science, the decade of the 1960s
remained descriptive. Lasker cites Eliot Chapple’s assessment that physical
anthropology had become a “handmaiden to history, a tool for archaeologi-
gical reconstruction of culture history through racial analysis of crania”
(Lasker 1970:2).

The movements out of the typological morass discussed by Larsen in
this volume can be attributed to three factors (Armelagos 2003:56). The first
was the application of a population perspective to skeletal biology; second,
the recognition that culture is part of the human environment and that it is
inextricably linked to biological adaptation; and, finally, a method for testing
alternative hypotheses that involves the interaction between the biological
and cultural dimensions of adaptation (Platt 1964).

Marks, in his illustrious career, has made substantial contributions to the
study of race (Marks 1995). He has argued convincingly that racial classifica-
tions may have originated in attempts to provide a biological explanation for
behavioral differences in human groups (races) that were perceived to be
different. In his chapter on race, Marks (2010) examines two crises in racial
studies in anthropology. As physical anthropology grappled with the validity
of the race concept that Marks describes in broad detail, the United Nations
Economic, Social, and Cultural Organization (UNESCO) decided to formulate
a statement on race. UNESCO’s dissatisfaction with pre-World War II race
science, which was based on a flawed genetic rationale that grew out of
the eugenics movement, encouraged them to deal with the problem of race
(Zuckerman and Armelagos 2010). UNESCO’s official statement, “The Race
Question” (UNESCO 1950) was the first attempt by an international
community to achieve consensus on, and provide the public with a new
conception of race.

The crisis ensued because of concern over confusion on the differences
between race as a biological fact and the concept of race as a social
phenomenon. There was also concern by some in which they “declined to
acknowledge as a proved fact that there are no mental differences between
racial groups, stressed that there was insufficient evidence to support that
view, and urged the need for keeping an open mind on the subject”
(UNESCO 1952:7).

There was concern about the composition of the drafters of the
statement. Sociologists predominated in the group that framed of the state-
ment. The fact that biological scientists such as geneticists and physical
anthropologists were missing from the writing of the statement meant that
the statement did not carry the conviction or the support of biologists.

A second meeting in 1951 was convened in which 12 anthropologists
and geneticists formulated a second statement that reflected the view of these
scientific circles. The final document rejects the doctrine of pure race and
racial hierarchies. The statement emphasized race as a biological concept
and left suggested the possibility that races differed in their innate capacities
for emotional and intellectual responses. Even though there were voices that
criticized the race concept in the 1940s (Montagu 1942), the 1960s was a
decade in which the validity of race as a tool to describe and understand human variation gained adherents (Brace 1964a, 1964b; Livingstone 1962).

MARKS ON RACE

The second crisis, according to Marks, began with the publication of racist tracts written by Carlton Putnam (1961, 1967) at the height of the civil rights movement and in response to anthropologists criticizing the validity of the race concept. Putnam was being aided surreptitiously by Carleton Coon, then President of the AAPA. Putnam claimed that those who criticized race were social and cultural anthropologists who had little understanding of race. At the 1962 business meeting of the AAPA, presided by Coon, a resolution condemning Putnam’s book was passed. Coon stormed out of the meeting rather than “preside over such a craven lot” (Jackson 2001:265).

Controversy continued to surround Coon with the publication of *Origin of Races* (Coon 1962) that argued for a differential evolution of racial groups. Coon stated that the African evolved 200,000 years “behind” the development of Caucasians. This model provided more grist for the racist’s mill. Physical anthropology, in one of its finest moments, closed ranks and provided critical evaluation that limited its impact. Putnam and his colleagues lost this battle in the United States with the passage of the Civil Rights Act of 1964. Jackson (2001) has summarized the controversy and shows Coon’s duplicity in aiding the segregationist’s cause.

Teamed with the Mark’s contribution, Relethford (2010) rehashes the controversy surrounding the Carleton Coon-Carleton Putnam fiasco. He does provide some original material on changing views in the 1960s, during which there was a major assault on the race concept. He concludes with a defense of the race concept, though maintains that it has little explanatory power. Relethford (2010:215) claims that human variation can be explained by race, specifically arguing that “Geographic origins can be described... without need for the arguments about number and defining characteristics of racial categories.” Given the fact that race has little explanatory power and that variation can be explained without resorting to race, why geographic races are needed to explain variation seems to beg the question. Relethford’s endorsement seems hollow in light of evidence that questions the utility of geographical race.

While Rosenberg and colleagues (2002) argue that genomic analysis show human variation clusters in six geographic races, their methods have been questioned. Blotnick (2008) claims there were a number of clusters possible using the same data. She shows that while Rosenberg’s study identified six genetic clusters it, “is not significant in and of itself—the program also identified 2, 5, 10, and 20 genetic clusters using the same set of data” (Bolnick 2008:76). Serre and Pääbo (2004) show that when variation is
analyzed homogeneously it is continuous and does not cluster in geographic races.

LARSEN AND BIOARCHAEOLOGY

Larsen (2010) provides a model of how the history provided in this volume could have been written. Larsen considers the static approach to physical anthropology reflected in Armelagos and colleagues’ (1982) and Lovejoy and colleagues’ (1982) discussions of the persistence of the typological approach in the discipline. In an analysis of ideas that were missing from earlier chapters, Larsen considers the criticism that theoretical perspective has failed to keep pace with the rapid change in technology. As Larsen points out, though the descriptions have become more sophisticated they remain focused on identification and sorting. Larsen is well qualified to respond to these criticisms since he was editor of AJPA for six years. He did a content analysis of AJPA publications from the early 2000s and found that the percentage of “descriptive” publications has decreased considerably to 37 percent. Assuming the objective of physical anthropology is to move from description to analysis, the trends indicate that skeletal biology and likely other areas of physical anthropology have matured.

Histories provided a sanitized version of the discipline. Little and Kennedy (2010a) make the distinction that there are different histories that tell the story of physical anthropology; there are histories of individuals, histories of institutions, and histories of ideas and traditions. Unfortunately, most of the contributions to this volume focus on the histories of individuals and institutions and only a few of the papers consider the histories of ideas. Many of the contributions gloss over the ideas that supported the earlier prevailing racist worldview of the United States. It has been argued that these scientists were “men of their times” and thus are justified in their views. The problem with this perspective is that it avoids issues that are relevant to understanding physical anthropology’s beginnings and how it was transformed to a modern science.

ARCHAEOLOGY AS ANTHROPOLOGY

Brian Fagan’s (2005) A Brief History of Archaeology shares with Histories a propensity for presenting the history of archaeology by focusing on the great archaeologist. The first six chapters of Fagan’s text describe early antiquarians and the founders of archaeology. The last seven chapters are relevant to bioarchaeology. Fagan presents the readers with famous archaeologists (Mortimer Wheeler, V. Gordon Childe, the Leakeys, Graham Clark, Walter Taylor, and Julian Stewart), famous sites (Harappa, Sutton Hoo, and Pecos), and great discoveries (the Dead Sea Scrolls, Tutankhamun, and
Australopithecines). Importantly, Fagan weaves in the ideas that defined archaeology into his discussion of the greats. For example, in his description of the great Egyptologist Grafton Elliot Smith, Fagan (2005:111–113) discusses the development of diffusionism at the beginning of the 20th century as an alternative to the then-discredited theory of progressive evolutionism. Through diffusionism, culture change was viewed as the result of contact between groups with the transmission of ideas or artifacts. Smith assumed that the pyramid was an Egyptian invention that was spread throughout the world, becoming a proponent of hyperdiffusionism with Egyptians carrying culture to all areas of the world where pyramids are found.

Fagan describes the era from 1920 to 1950 as the period of professionalization driven by archaeologists such as Mortimer Wheeler, who initiated scientific methods as standard excavation procedures. It was a time of spectacular discoveries at Harappa and Mohenjo-Daro that created excitement among professionals and the lay public. It was a time when V. Gordon Childe made remarkable contributions with his ability to synthesize massive data sets that weaved together broad sweeps of history. He tackled some of the most difficult issues in archaeology, including the origins of agriculture. Childe (1928) argued that desiccation at the end of the Ice Age led to populations coalescing at oases (the Oasis Theory) where the domestication of plants and animals became inevitable because of the propinquity of humans, plants, and animals. In a book that expanded on his earlier work, Childe (1934) described the agricultural revolution that led to the urban transformation of society. His most influential work was to complete a broad synthesis of human development (Childe 1951a, 1951b).

Fagan disappoints the bioarchaeologist with his cursory treatment of the study of skeletal remains. He briefly discusses the Piltdown fiasco, *Homo erectus*, *Australopithecus*, Zinjanthropus, the Bog bodies, and discoveries of the Leakeys without providing a broader context of the material. However, in his discussion of recent developments, Fagan does present areas of common concern for archaeologists and biological anthropologists.

In two chapters, Fagan discusses the New Archaeology and the postprocessual response to it. The 1960s and 1970s saw the movement away from the site as the center of attention to a focus on regional analysis. The movement was influenced by the evolutionary frameworks of Leslie A. White (1943, 1959) and Julian Steward (1949, 1977) who proposed models of multi-linear evolution. While White proposed the capture of energy as the key to cultural evolutionary change, Steward and his followers suggested that specific cultural adaptations were unique to ecological settings. Thus, the New Archaeology proposed to uncover evolutionary generalizations to move beyond description and the former reliance on diffusion or migration as the source of cultural change. Evolutionary generalizations or deductive models were applied with strict scientific principles to archaeological data (Binford
1962, 1964; Binford and Binford 1968). While processual archaeologists claimed scientific objectivity and that the knowledge gained was ethically neutral and independent of the values of contemporary culture, they had as their goal the formulation of generalizations that would be relevant to understanding contemporary cultural adaptation.

Fagan discusses the impact of postprocessual criticism during the last three decades as influenced by a rapidly changing world in which archaeology was becoming more specialized. While processual archaeology adhered to the principles of functionalism, structural and critical archaeology took root in understanding the symbolic role of thought in cultural development. Against this credo, postprocessualism argued that meaning is more important than materialism, that archaeologists have to examine their social responsibilities, and that they should focus on areas that processual archaeologists had neglected. Issues such as gender, race, inequality, decision-making, and the role of individuals and groups in societies became objectives of study. An outgrowth of processual archaeology was the development of cognitive archaeology. In light of these theoretical developments, the methodological toolkit became more complex with the addition of high-tech methods producing remarkable findings.

The origins of bioarchaeology are linked with the “new archaeology” by blending methods and data from archaeology with the approach of hypothesis testing from biological anthropology and a theoretical basis supplied by cultural anthropology. Hypothesis testing as required by the methods and suggested by strong inference (Platt 1964) were the focus of both approaches. The postprocessual criticism of archaeology focused on the construction of theory that could not be independently verified. In bioarchaeology, skeletal material could be used to independently test hypotheses. For example, if one hypothesized that agriculture was detrimental to health, there would be independent means of testing the hypothesis. Societies undergoing the Neolithic transformation should show an increase in pathological conditions indicative of stress. In empirical studies, agriculturalists show an increase in nutritional deficiencies and infectious diseases as compared to their non-agriculturalist counterparts.

As archaeology responded to the postprocessual critique, bioarchaeology retained its methods and its commitment to hypothesis testing. Postprocessual theory had an impact on bioarchaeology by framing hypotheses in a political-economic context, with skeletal biology employed to determine the impact of social, political, and economic transformations on health and illness. Postprocessual archaeology that reflects a more political perspective (Leone 1995, 2009) and bioarchaeology share overlapping interests such as race, violence, power, gender, and inequality.

Fagan predicts that archaeology will insure a strong commitment to inter-subdisciplinary cooperation with cultural, biological, and linguistic anthropology, supporting the integrative nature of anthropology. Three
areas of future development, evolutionary archaeology, genetics, and cognitive processual archaeology, will require cooperation with biological anthropologists. While Fagan in his short book does not provide examples of this cooperative research, there is ample evidence of such interaction. The genomic analysis of human populations, domesticates, and pathogens has revolutionized archaeological interpretation (Armelagos and Harper 2005a, 2005b). Renfrew (2001, 2010) is so positive about the profound advances made in genomics that he has proposed a new field called archaeogenetics, which provides an important tool for understanding the origins of agriculture and its implications for subsistence transformation (see also Renfrew and Boyle 2000). Archaeogenetics uses molecular genetics to examine issues related to population prehistory (Soares et al. 2010), and has been applied to questions of agricultural origin (Palmer et al. 2009) and population expansion (Sykes 2003). Genomic analysis expands the realm of domestication research, revealing a much more complex pattern of domestication in which plants began the process of domestication earlier than once thought.

There are a number of domesticates that have been uncovered in regions outside the originally heralded centers of domestication. Gruhn’s (2006) recent review of Bellwood and Renfrew’s edited volumes (Bellwood and Renfrew 2002; Renfrew 2000) on language and genomic dispersal with agriculture demonstrates a vigorous interdisciplinary interaction among historical linguistics, archaeology, and biological anthropology. This level of cooperative research reflects the nature of anthropology today.

Cognitive archaeology, a recent development in archaeology, includes a focus on the neurological basis of human behavior using brain imaging (Malafouris 2009). For example, when neuroimaging subjects making Oldowan tools, Stout and colleagues (2008) see the activation of ventral premotor and inferior parietal elements of the parietofrontal praxis circuits with activation of right hemisphere homologue of Broca’s area. These patterns of activation, which overlap with language circuits, suggest that tool making and language share a capacity for complex, goal-directed action. They also suggest a co-evolutionary hypothesis that links the emergence of language, tool making, and functional lateralization on a population level, which is associated with cortical expansion in human evolution.

The diversity of American anthropology as a discipline could not have been imagined by Boas in 1904. Boas was certainly correct in his assessment that specialization would strain the ability of any individual to control all four fields of anthropology, but as a discipline it has persisted for over a century of development and the identification of the practitioners as anthropologists has endured. One-hundred and thirty years ago, Edward B. Tylor (1881:439) concluded his book *Anthropology* with the following observation, “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."

ACKNOWLEDGMENTS

I would like to thank Amanda Mummert, Emile Esche, and Joshua Robinson
for critical readings of this review.

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