
Bioarchaeology

The Contextual Analysis of Human Remains

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Chapter 2

The Old Physical Anthropology and the New World: A Look at the Accomplishments of an Antiquated Paradigm

Della Collins Cook

It is indeed difficult to imagine an all-wise Providence, after having by the Deluge destroyed all mankind excepting the family of Noah, should leave these to combat, and with seemingly uncertain and inadequate means, the various external causes that tended to oppose the great object of their dispersion: we are left to the reasonable conclusion that each Race was adapted from the beginning to its peculiar local destination. In other words, it is assumed, that the physical characteristics which distinguish the different Races are independent of external causes. (Morton 1839:3)

Craniology in the work of the 18th- and 19th-century anthropologist-physicians Blumenbach, Morton, and Warren serves largely as a descriptive tool, and analysis for these early typologists was confined to evaluating individual specimens. Variability was unimportant, and the approach is primarily one of classification. The typological study of Indian and Eskimo crania became the dominant enterprise as American physical anthropology emerged as a profession around 1900. The contributions of Hooton, Hrdlička, Rivet, Oettinger, and Neumann are reviewed. Among these, Hrdlička and Rivet built on the

19th-century French school that begins with the work of Paul Broca. Oettinger and Neumann built on the Boasian school, and through it as well as independently on the German school. American craniology is distinct from similar work in Europe in the degree to which these researchers interacted with archaeologists, in part because the Boasian race–language–culture model encouraged such interaction. The cultural and historical questions that motivated the typologists remain with us today.

I. INTRODUCTION

The twin problems of the origins and diversity of American Indians emerged in the earliest European accounts of the New World (Arensberg, 1995). Were Indians fundamentally similar or were they diverse? Were they closely related to one or to several peoples of the Old World? The most balanced and detailed account of this history remains that of Juan Comas (1960, 1974); he presented Hrdlička's model for a single northwest Asian origin for American Indians as a novel formulation that contrasts the various hypotheses for multiple origins that had and continue to have considerable currency in Latin America. Hrdlička's model has been and remains the dominant or only model among North American anthropologists (Stewart, 1960a, 1981; Stewart and Newman, 1951; Crawford, 1998). Thus, key issues that were formulated in the earliest literature in our field have persisted to the present day, despite theoretical and methodological transformations that might have been expected to influence them. This chapter focuses on some issues of method in the typological research.

The typological paradigm in physical anthropology gave way in the middle of the 20th century to a concept of human variation grounded, on the one hand, in the emerging field of population genetics and, on the other, in the powerful new statistical tools of biological distance. From its origin our field was wedded to typological thinking. In rejecting this outdated paradigm, we have turned away from much of what our discipline accomplished before the latter half of the 20th century. This chapter reviews and reevaluates this past.

Much of what has been written about the typological era in physical anthropology has been couched in a disciplinary critique of racism in the latter half of the 20th century. The focus — often implicit rather than stated — has been on the cultural freight of White/Black or, more accurately, White/other racism that the typologists brought to their science. These are important issues, but the result is a sort of presentism. In holding our intellectual ancestors to the standards of the present, the rhetoric of late 20th-century social context distracts us from an appreciation of the questions that motivated the craniologists, questions that are peculiar to Americanist anthropology. Where did the Indians come from? How diverse are they and how is that diversity related to their origins? How is their

biological diversity related to linguistic, cultural, and ecological diversity in the New World?

II. JOHANN FRIEDRICH BLUMENBACH 1752–1840

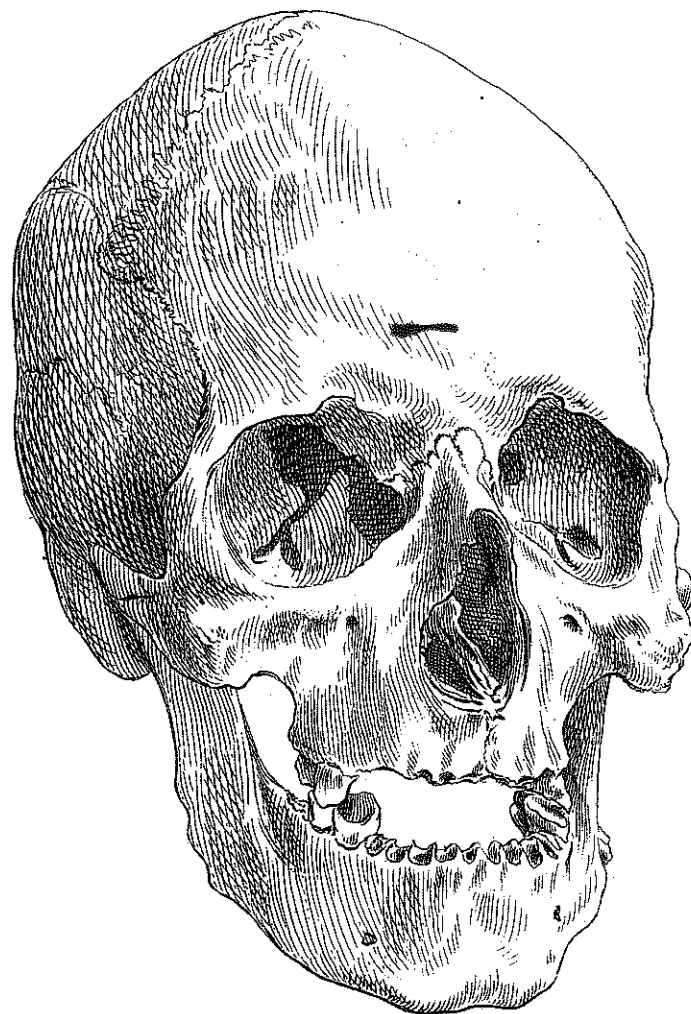
Typological characterization of the newly discovered American peoples appears in the earliest work that we can label physical anthropology: the craniological research of the German anatomist Johann Friedrich Blumenbach. Blumenbach has been the focus of considerable attention in recent work on the history of science regarding the origins of the biological concept of race and critiques of scientific racism.

Blumenbach's work is couched in the degenerationist paradigm that dominated biology in his day. His understanding of race combined elements from the work of Kant and Buffon (Larson, 1994). Emmanuel Kant had attributed human variability to the effects of climate on an ideal, created or ancestral type. Variability was thus the result of the degeneration — here an accommodation to local conditions that foreshadows the modern concept of adaptation — of a single original type that was of intermediate skin color. Kant recognized four Old World races and considered the Americas too recently settled to have given rise to a constant type. Larson summarized the resultant concept of variation thus: "A race was a class or series of individuals issued from one another and distinguished by a variation that had become constant. Naturalists considered these subtypes distinct branches, in spite of their common origin, and recognized that yet further subtypes might arise from them" (Larson, 1994:63). Georges-Louis Leclerc, Comte de Buffon, introduced the concept of reproductive isolation as the defining feature of species, and he expected to find infertility in crosses between human races. His view of the Americas was degenerationist in a different — and quite negative — sense. He argued that the American fauna, the Indians included, was smaller, weaker, and less vigorous than its Old World counterparts (Larson, 1994; De Waal Malefijt, 1974). His discussion of human diversity also contributed to a third sense in which the degeneration came to be used in the 19th century, which Stepan has labeled "the race out of place" (1985). Buffon expected rapid change in migrant populations toward the characteristics of native groups. Marks (1995) argued that Buffon's concept of human variation was adaptationist and thus modern, in contrast to the misguided "anti-anthropological, anti-biological and anti-historical" (1995:52) typological concept of Linnaeus and Blumenbach. Any Americanist will find this rosy view of Buffon difficult to reconcile with Buffon's highly negative view of American Indians.

Blumenbach generated a remarkably modern account of the continuous and trivial nature of human variation. A succinct statement of his race concept is this quotation from the English translation of his 1775 work *De generis humani*

varietate nativa: “The variations of skin color, stature, body proportions, etc., which we have been able to observe, considerable though they may appear at first sight, have no absolute value; they all merge gradually one into another and, accordingly, classification into human races is arbitrary” [Bendyshe (1865), quoted in Comas (1960:16)]. A race concept of underlying unity did not prevent him from defining five races: the Caucasian, the Mongolian, the Ethiopian, the American, and the Malayan, corresponding to five skulls he illustrated as exemplars of these races. Gould (1994) and Marks (1995) point out that the Malay race is a late addition; the first edition of *De generis* presents only four categories, as do the classifications of Blumenbach’s contemporaries, Cuvier and Linneaus. Gould inferred that the addition was made for reasons of symmetry in fitting the scheme to degenerationist theory, serving as “the transitional form between Europeans and Africans” (1994:69). It seems equally likely that Blumenbach did not have a skull from the Pacific in 1775. In his third edition (1795) of *De generis*, Blumenbach thanks Joseph Banks for providing him with a skull from Botany Bay, and he dedicates this edition to Banks. Banks, the British botanist and patron of scientific explorations, had accompanied Captain James Cook to Australia in 1770, and the Cook expedition represented the first opportunity for Europeans to accommodate Australian Aborigines in their accounts of natural history.

The ordering of these categories has been a subject of much recent discussion. While Blumenbach discusses Camper’s facial angle at some length, he rejects it as a criterion for assigning skulls to races. Nevertheless, he illustrates his five races in order of facial projection: that is to say Mongolian, American, Caucasian, Malayan, and Ethiopian. He discusses his five races in a different order, beginning in the middle with the Caucasian, proceeding to the extremes of flat Mongolian and projecting Ethiopian faces, and ending with the intermediate American and Malayan faces (Fig. 1). Gould argued (1994) that this is the first ranking of races in science and that the ranking itself is perniciously hierarchical, even if Blumenbach himself was not racist. This seems to be an inappropriately quantitative reading of Blumenbach’s work, and it is perhaps not trivial that Gould’s critique appears in a popular magazine in a collection of essays responding to *The Bell Curve*. Gould singles out the facial angle in a way that Blumenbach explicitly rejects in this passage from the 1795 edition of *De generis*: “It very often happens that the skulls of the most different nations, who are separated as they say by the whole heaven from one another, have still one and the same direction of the facial line: and on the other hand many skulls of one and the same race, agreeing entirely with a common disposition, have a facial line as different as possible. We can form but a poor opinion of skulls when seen in profile alone, unless at the same time account be taken of their breadth” (Bendyshe 1865:235). Blumenbach’s most important contribution to theory in biology is the concept of *habitus* in systematics (Farber, 1982). The whole organism, not a



XXXVIII. *Americani Illinoisi.*

Figure 1 Blumenbach’s cranium from Illinois (Blumenbach, 1800).

single character, should be used in assessing affinities. It is thus particularly inappropriate to represent him as having ranked races on a single scale.

The language that Blumenbach uses in *De generis* has been a lively subject in recent literature on scientific racism. Schiebinger (1993) reads his choice of skulls for description as a complex text that conflates religious meanings attached to the mountains of central Asia with lubricious accounts of the Turkish slave trade, expressing a species of sexism that she finds pervading Enlightenment science. Gould takes Blumenbach to task for the language he uses in describing his Caucasian exemplar: "Blumenbach's descriptions are pervaded by his subjective sense of relative beauty, presented as though he were discussing an objective and quantifiable property not subject to doubt or disagreement" (Gould, 1994:69) and he quotes the description of the Georgian female skull as if it were the description of the whole Caucasian race. Here both Gould and Schiebinger have missed Blumenbach's allusion to the historical context in which *De generis* was written. Blumenbach's colleagues at Göttingen University included S. T. Soemmerring, an anatomist who had dissected the cadavers of Africans, arguing that they were intermediate between apes and Europeans, and the philosopher C. Meiners, who ranked the races on relative beauty in building a justification for slavery (Jahoda, 1999). Female skeletons were similarly aestheticized and stereotyped in anatomical literature until the early 20th century (Fee, 1979), and Buffon and other contemporaries of Blumenbach used aesthetic language in describing human variation. The author prefers to read the gushing language Blumenbach applies to the skull of his Georgian woman as irony aimed at these colleagues, a reading that Jahoda supports from Blumenbach's correspondence (Jahoda, 1999).

The concept of variation expressed in the 1795 text quoted earlier does not accord with the prevailing 17th-century definition of races as constant varieties, and in this regard it approaches variability in a novel way. Blumenbach's method was also novel, a novelty for which he used the term *anthropology* for the first time in its modern sense because he tested his models of human variation using observations on skulls. Most recent discussions to the contrary, Blumenbach did not measure skulls (Bowles, 1976; Ubelaker, 1982; Burke, 1998; Joyce, 2001). Rigorously defined measurements and tools for making them are a product of 19th-century anthropology. He proposed the *norma verticalis* as the best perspective from which to view the skull, but the calipers and the craniophor were still in the future. While many features of the skulls are described, only the relative projection of the face is treated analytically, and Blumenbach's observations were visual, not metric.

All recent scholarship on Blumenbach of which the author is aware has focused on his *De generis*. This book is a natural history in the sense that it belongs to a genre of science writing in which the writer presents a comprehensive, literary account of humans of the natural world. Natural histories were popular in a way that is difficult for modern readers to comprehend in our age

of scientific specialization. The 19th-century translation of this work into the major scholarly languages reflects this popular audience. The exclusive focus on *De generis* misrepresents Blumenbach's methods. His science lies in his craniology. He collected skulls by corresponding with travelers to various parts of the world and stimulated scientific collection on the part of travelers. This method, if we wish to use the word, was novel. In his fascinating study of travel and natural history, Liebersohn says of Blumenbach that "As a scientific entrepreneur, he linked the burgeoning interest in travel to university learning and powerful patrons" (1998:135). By the end of his career, Blumenbach had amassed a collection of 245 skulls at Göttingen, 43 of them from the Americas (Bendyshe 1865:348). Between 1790 and 1828 he published a series of detailed descriptions of 65 crania, including provenience information and an engraved illustration of each. The title of the series varies somewhat: *Decas prima collectionis sua craniorum diversarum gentium illustrata* appeared in 1790 and *Nova pentas collectionis suae craniorum diversarum gentium* in 1828. The author refers to this work collectively as *Decas*.

The *Decas* includes nine crania of American Indians. Specimen 9, which Blumenbach describes among the 10 presented in the first installment of the *Decas* 1790, is a Cherokee sent to him by a Dr. Michaelis of Philadelphia. Blumenbach comments on cranial deformation and on the size of the nasal aperture, relating the volume of the nasal cavity and the complexity of the turbinals to reports of the acuity of the sense of smell among Indians. Numbers 10 and 20 are Caribs from the island of St. Vincent contributed by Joseph Banks. Cranial deformation is again noted. Skull 38 is from Illinois near Cahokia, contributed by a Dr. Barton. This is perhaps Benjamin Smith Barton (1766–1815) of Philadelphia, who had studied at Göttingen. Blumenbach remarks on Caucasian features in his Illinois specimen, thus prefiguring the Kennewick Man controversy in the 1990s. Blumenbach's illustration of this skull appears in Fig. 1. Specimen 46 is a skull from the upper Orinoco donated by Alexander von Humboldt, 47 is a decorated trophy head, 48 a native woman, origin unspecified, and 57 is a Coroa woman, all from Brazil. Specimen 58 is a Botocudo from Brazil donated by Maximilian, Prince of Wied, the ethnographer and explorer (Liebersohn, 1998). Specimen 65 is a deformed Inca skull excavated by Alexander Caldcleugh, a British diplomat and travel writer. There are in addition four Eskimo, two from the North American Arctic and two from Greenland, an Aleut, and several representatives of Siberian peoples. Blumenbach has been credited as the first scholar to recognize the Asian affinities of the Eskimo and Aleut [Harper and Laughlin (1982:282); Szathmary and Ossenbreg (1978) pointed out that David Cranz made the same inference a decade earlier]. They had been previously understood by the natural historians as most closely related to Europeans and appear in the earliest anthropological literature among the Hyperboreans along with the Lapps, Picts, and Scots.

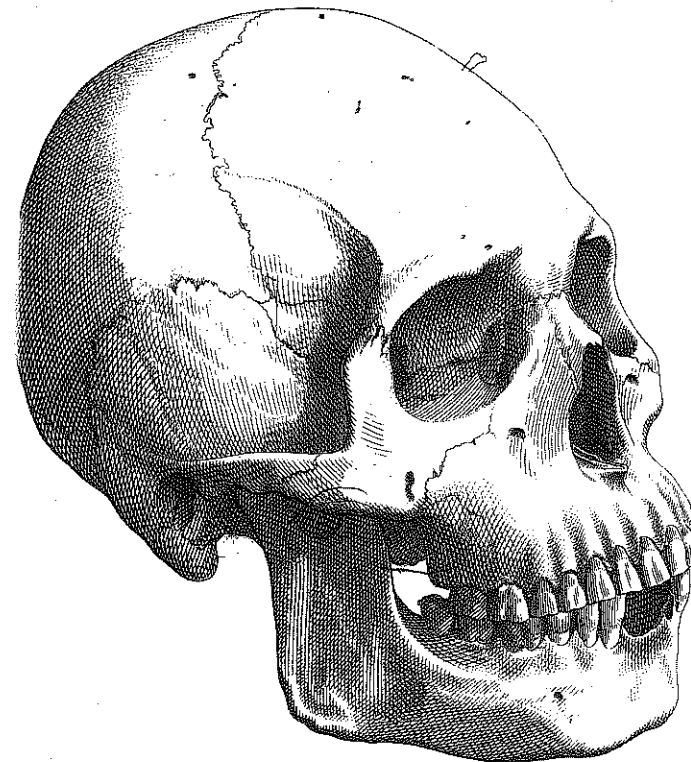
The only portion of the *Decas* that appears to have been translated into English is an excerpt from the description of the Botocudo skull that appears in Samuel Morton's *Crania Americana*:

The age of this man was about five and twenty. During the war between the Botocudos and the Portuguese, he was accustomed to join his countrymen in their hostile incursions; but after the hostilities ceased, he frequently visited the garrison on the Rio Doce, where he not long after fell sick and died. The cranium, which is large, is also very ponderous from the thickness of the bones, and their dense and hard texture: and as a whole, if you disregard for a moment the under jaw, the figure and interval of the orbits, the elevated nasal spine, and other particulars peculiar to man, the general aspect approaches nearer to that of the Orang Outang than any other skull from a barbarous nation to be seen in my collection. I have indeed one or two specimens of the Negro, in which the upper jaw is more projecting; but this skull differs from them in other respects, besides having the cheek bones more prominent, and a greater swell of the parietal bones. But what deserves particular notice is an indentation, shaped like the point of the finger on wax, which remains after the loss of the front teeth, the sockets of which are compressed, or rather completely absorbed. So universally, the Prince of Wied assures me, does this happen to the youth of this nation from wearing the wooden lip-ornament, already mentioned, that you will scarcely find one of them arrived at the age of thirty who retains these teeth. (Morton, 1839:140)

This passage illustrates the character of Blumenbach's descriptions. The attention to provenience is typical of his work. This description is unusual in remarking on resemblances to a nonhuman primate. In sharp contrast to the work of his contemporaries, the likeness he draws is limited, qualified, and without any suggestion of affinity. The passage is also interesting for its notice of pathological conditions. Blumenbach has been credited as the founder of craniology and calumniated as the inventor of racial classification. Perhaps we ought also to claim him as an early contributor to paleopathology, as this passage in the *Decas sexta* of 1820 is the first published description of alveolar pathology resulting from the wearing of a wooden labret or botoque, for which the Botocudo were named (see Fig. 2). Blumenbach's collections eventually contained many American Indian crania not included in *Decas*, e.g., two Arikara skulls collected for him by Karl Bodmer in 1834 (Bass *et al.*, 1971).

III. SAMUEL GEORGE MORTON 1799–1851

Blumenbach's work served as the model for the efforts of the Philadelphia physician and anthropologist Samuel G. Morton. His 1839 *Crania Americana* tested then-prevalent accounts of New World peoples that attributed the ancient monuments of high civilization to an extinct race of immigrants from Europe or elsewhere in the Old World (Silverberg, 1968; Buikstra, 1979). Morton's research



LVIII. Botocudi.

Figure 2 Blumenbach's Botocudo skull (Blumenbach, 1820).

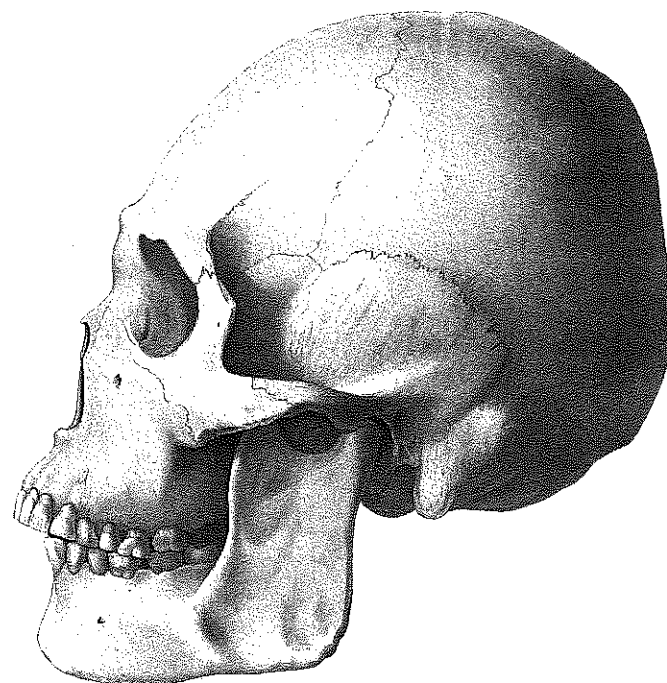
soundly discredited this "Moundbuilder" myth. He grouped specimens from Peru, Mexico, and the ancient earthworks of the Ohio Valley into his Toltecian Race and found this group to be essentially similar to the other American crania in his collection, which he assigned to the category Barbarous Nations. He concluded that "the American nations, excepting the Polar tribes, are of one Race and one species, but of two great Families, which resemble each other in physical, but differ in intellectual character" (1839:260). Much recent scholarship on Morton has largely focused on constructing his methods as racist and has reduced his work to a ranking of races (Browne, 2000; Bruce, 1988; Joyce, 2001; Gould, 1996; Worden, 2002). The author argues that in the context of his day, his research was grounded in ethnology, and his view of the unity of ancient and recent American

Indians was antithetical to that of many of his contemporaries, who denigrated Indian intellectual and cultural capacity in the long tradition extending from the *Book of Mormon* to the alien fantasies of van Daniken.

The literature on scientific racism has largely ignored Morton's scientific contributions, but physical anthropologists claim him as an intellectual ancestor. Brace (1982) traced a genealogy from Blumenbach to Morton and from Morton to the French founder of physical anthropology, Paul Broca (1824–1880). Building on Morton's concept of anthropology, Broca professionalized the discipline. The profession was then returned to the Americas through the efforts of Aleš Hrdlička in Brace's view (Brace, 1982). Hrdlička himself (1918) stressed the detailed continuity between Morton's techniques and the standards for craniometry that emerged at the end of the 19th century. Ten linear measurements, one angle, and an internal capacity with four component measurements are defined in *Crania Americana*, and four instruments, a "facial goniometer" (Morton, 1839:252), a graduated cylinder, a device for finding partial cranial volumes (Morton, 1839:254), and a "craniograph" (Morton, 1839:294) for drawing skulls are described.

The illustrations in *Crania Americana* are remarkable for their beauty and precision. Unlike the illustrations in Blumenbach's *Decas*, they have great anatomical detail, perhaps because Morton used his craniograph to do the rough drawings. Lithographs prepared by John Collins are among the earliest examples of the use of lithography for scientific illustration in the United States and reflect the rapid improvement of visual presentation of natural science during the 19th century (Blum, 1993). Folio publication was funded by William Maclure, the last of several lavish publications he supported (Porter, 1986). Figure 3 is Collins' lithograph of the same Botocudo skull that appears in Blumenbach's engraving in Fig. 2. The illustrations in *Crania Americana* are detailed and anatomically precise. Most are lateral views. Several three-quarter views show exaggerated shallow perspective that reflects the use of the craniograph, an equivalent of the camera obscura (see Hockney, 2001). The many plates drawn from "nature" are printed at 1:1 scale, and measurements in the text that can be checked on the plates are remarkably accurate, an innovation comparable to the scaled-down engravings in Cuvier's *Le Règne Animal: Races Humaines* (1836). Pathological changes, such as trephination (Plate 11D), taphonomic alterations, e.g., rodent gnawing (Plate 68), and anatomical variants, e.g., epipteric bones (Plates 34 and 37), are illustrated, although not necessarily noted in the text.

Morton's race concept is founded on Blumenbach's in the sense that he uses Blumenbach's five races as the framework for his analysis. Morton elides these five distinct races with the three sons of Noah in the introductory pages of *Crania Americana* (1839:1). He divides these races into 22 families, with the Toltec and the Barbarous Nations representing the two subdivisions of the American race at the level of the "family." The American race is contrasted with the



BOTOCUDO of BRAZIL

Drawn from Nature by WE Berleini

On Stone by John Collins

Figure 3 Morton's Botocudo skull (Morton, 1839).

Mongol-Americans or "Esquimaux." The Barbarous Nations are further subdivided into Appalachian, Brazilian, Patagonian, and Fuegian branches, the first of these accounting for all of North American north of Mexico.

A question that requires further investigation is the source of the model for the presence of several races in Native North America that Morton confronts in *Crania Americana*. Gruber points out that Morton was a Quaker and that the prominent Quaker intellectuals senior to and contemporary with Morton had argued that the Indians were remnants of the lost tribes of Israel (Gruber, 1967). Silverberg credits Caleb Atwater's 1820 report on Ohio Hopewell remains as the first scientific claim that more than one race was present in ancient North America. Atwater contrasted crania from the mounds with contemporary Indians: "Their foreheads were low, cheekbones rather high; their faces were short and broad; their eyes were very large; and, they had broad chins. . . . The limbs of our fossils

are short and very thick and resemble the Germans, more than any Europeans with whom I am acquainted" (Silverberg, 1968:107). Morton corresponded with Atwater, and cited his work, and it is certainly possible that Atwater's formulation of the problem of who the Moundbuilders were provided the stimulus for Morton's work. Atwater was not alone in his opinions, as Silverberg has shown, and it is argued later that Morton was stimulated by his contemporary and scientific rival, Dr. J. C. Warren, who followed Atwater closely in claiming that the Indians represented more than one race. Hrdlička's assessment of Morton's contribution deserves emphasis: in finding that the Indians constituted a single race, Morton "subverted the numerous loosely formed but commonly held theories respecting the racial complexity of the American natives, as well as those of a racial separateness of the "Moundbuilders" from the rest of the American Indians" (1918:141).

Was Morton a polygenist? Morton's ideas concerning race origins were unremarkable in 19th-century America and are a very minor part of his work. As Arensburg (1995) has shown, notions of separate creation of races can be traced back as far as Columbus and were especially pervasive in the Iberoamerican world. The radical notions of separate creation of the races expounded in Nott and Gliddon's account of Morton's work are primarily Nott's work, not Morton's (Brace, 1974; Porter, 1986). Morton has surprisingly little to say on the subject of polygenesis, given the extent to which recent accounts have stressed his adherence to this model for human diversity. His strongest statements in this regard are found in his correspondence with Nott (Horsman, 1987). His findings, both in *Crania Americana* and in his smaller parallel study of Egyptian antiquities (1844a), are cautiously phrased and limited to the observation that ancient crania are as distinct racially as are recent ones, so much so that it is difficult to find a passage in his published work that clearly expresses a commitment to the concept of polygenesis. Stanton quotes a statement from his correspondence: a skull obtained from Squier's excavations in the Ohio mounds was "a perfect type" of the race "indigenous to the American continent, having been planted there by the hand of Omnipotence" [Stanton (1960:84), quoting Morton to Squier 1947]. In *Crania Americana* there is only one allusion to the concept of separate creation of human races, and it follows a discussion of the conflict of the five-race and four-race models of Blumenbach and Cuvier with the Biblical three-race model. This passage appears as the epigram of the present chapter (Morton, 1839:3). Divine providence is otherwise notably absent from the remainder of the text of *Crania Americana*.

Was Morton a phrenologist? Spencer finds evidence for a long-term commitment to phrenology as well as polygeny in Morton's doctoral thesis written at Edinburgh in 1822 (Spencer, 1983). In the author's view, Spencer's case is circumstantial: the portions of Morton's thesis that he chooses to translate make no claims concerning race. For example, stoicism in American Indians is placed in

a context of human nature in general: "all over the world examples have been found of people suffering . . . without uttering a single moan . . . among the aborigines of America, a prisoner, condemned by the enemy to torture and slow death, sings his funeral song unmoved . . ." (Spencer, 1983:335). A similar passage appears in *Crania Americana* (1839:77), where Morton likens the courage of Indian captives to that of European martyrs and denies that Indians are less sensitive to pain than others.

Similarly, De Waal Malefijt (1974) suggested that Morton became interested in craniology and the relationship between skull size and shape and mental ability because he was a correspondent and colleague of the phrenologist George Combe. This scenario also seems unlikely. Morton's acquaintance with Combe began rather late, shortly before *Crania Americana* was published, and Morton had begun to collect skulls in 1820 (Stanton, 1960:27). Hrdlička characterizes Morton as an "investigator" of phrenology rather than as a "promoter" (1918:138). Combe's assessment of the phrenology of Morton's collection is appended to Morton's study rather than integrated with it. While Morton occasionally remarks on the development of one of the phrenological landmarks in some of his specimens (1839:169, 202) he does not cite Combe in the text, except to acknowledge him as the donor of several Eskimo and Plains Indian specimens. The broader question of cerebral localization did not begin with Combe, and it was very much normal science during Morton's career (Young, 1990). It is an anachronism to view localization or, for that matter, phrenology as the bizarre pseudoscience it seems today.

In contrast to his circumspect treatment of phrenology, Morton discusses Blumenbach's craniology extensively, and he cites both *De generis* and several of the descriptions from the *Decas*. If we trust Morton's own account of the beginnings of his interest in the subject, he wanted specimens to illustrate his anatomy lectures on the varieties of mankind (Stanton, 1960:27), an enterprise he shared with many less ambitious anatomists of his day.

Was Morton a racist? Stephen J. Gould has misled many to a conception of *Crania Americana* that centers on cranial capacity and on its use of the relative ranking of races (Gould, 1978a, 1981, 1996). For example, a recent history of anthropology claims "[i]n 1839 Morton published *Crania Americana*, in which the inherent capabilities of a race of people was scientifically determined by skull size and capacity" (Joyce, 2001:8). Another author opines with more generosity "the Quaker physician inadvertently opened the door for others to associate cranial shape with brain size and brain size with mental capacity and social station" (Porter, 1986:70). Others dismiss Morton as a racist skull collector or an apologist for slavery, citing only Gould (Bruce, 1988; Blakey, 1987). These summations, like many in recent literature, seriously misrepresent Morton's work. The overwhelming majority of Morton's text is concerned with natural history. An "introductory essay" of 95 pages is devoted to a lengthy ethnological and

historical discussion. Description of the skull collection occupies 253 pages, including detailed accounts of provenience and funeral customs. What we would now call metric methods and results occupy only 12 pages, and Combe's phrenology results another 7. Cranial capacity is 1 of 12 measurements Morton tabulated in his collection. While it is true that only cranial capacity is analyzed in detail, Morton's measurements are supportive of his typological analysis rather than central to it. It remains a puzzle that Morton devoted his energies to the other 11 measurements, but failed to discuss the results. His complaints about the accuracy of calculations conducted by his assistants (1849) hint that the sheer magnitude of the task was a factor!

Was Morton a cheat? Gould accuses Morton of conscious or subconscious falsification of his data through the use of grouped means, both in *Crania Americana* and in his later essay on cranial capacity. Gould also suggests that the measurements may have been manipulated in favor of the hypothesis of Caucasian superiority. Gould's supposition that the measurements may have been manipulated consciously or unconsciously was tested directly by Michael (1988), who replicated Morton's measurements for a portion of the collection. It is noteworthy that in the second edition of *Mismeasure of Man*, Gould failed to respond to Michael's demonstration that Morton's measurements were accurate. Gould's most interesting argument concerns his allegation that Morton manipulated his data through the use of different proportions of males and females and of large-statured and small-statured peoples in the groups he compared. Gould recognizes that Morton's discovery that Peruvian mummies had smaller crania capacities than other Indians, particularly those of his so-called Barbarous Races, contradicted the hypothesis that cranial capacity constrains cultural capacity, and that this discovery argues for Morton's scientific objectivity, but he remains convinced that "Morton's summaries are a patchwork of fudging and finangling in the clear interest of controlling a priori convictions" (1996:86). The author finds Gould's argument unpersuasive because it views Morton's work through the lens of 20th-century quantitative sophistication. Morton worked before the invention of statistical methods appropriate to his research. While it may be difficult for anyone educated in the sciences today to understand that Morton may have been blind to the effects of sexual dimorphism and body size differences on his means, the author's experience in teaching Gould's paper to undergraduates has been that Gould unfairly brands Morton as racist. The concept of grouped means is exceptionally difficult for students who lack a quantitative bent. Sorting out the relative contributions of sex, body size, latitude, and subsistence on cranial capacity has required multivariate statistics, as well as samples far beyond Morton's considerable efforts in collecting crania. Indeed, anthropologists did not complete this task until the late 20th century (Beals *et al.*, 1984; Smith and Beals, 1990), an accomplishment that Gould also fails to note in his second edition. The necessary statistical tools were unavailable to Morton.

Morton's principal anthropological accomplishment was the demonstration that the Moundbuilders were Indians and that American Indians constituted a single race. As Stanton showed more than 40 years ago, this was an antiracist point of view. It credited the Indians with the capacity for high culture. Stanton's case that Morton was motivated by a desire to refute the popular culture claims for various migration legends is less convincing. The scale of his research suggests a more scholarly, scientific target. It seems more plausible that Morton's concept of two races among American Indians was stimulated instead by the work of his Boston contemporary, John Collins Warren.

IV. JOHN COLLINS WARREN 1778-1856

Dr. John Collins Warren was a Boston physician, surgeon, and anatomist whose family had a long association with Harvard University. Hrdlička acknowledges Warren as a pioneer: "Inspired evidently by Blumenbach's works, Professor Warren began to collect and examine skulls of different races, and in 1822 he published an *Account of the Crania of some of the Aborigines of the United States*, the first publication in this field on the continent . . . while of no permanent value scientifically . . . is nevertheless remarkable for the systematic, technical description of the specimens" (1918:136). With such faint praise he has consigned Warren to relative obscurity. Hrdlička gives pride of place to Morton as the first American physical anthropologist, but fails to explore any connections between Morton and Warren, apart from pointing out that Morton had read Warren. Perhaps because Hrdlička is dismissive of Warren's physical anthropology, the historical literature on Morton and on race in the Americas has ignored Warren's earlier work.

Both Morton and Warren were natural historians. Warren wrote a natural history of an anatomical region, the nervous system, whereas Morton wrote two natural histories of human races. Anthropology is a secondary concern in Warren's work, whereas it is the primary focus of Morton's. Both were institution builders, but Warren was the more prominent in this regard.

The publication that Hrdlička cites is an appendix to a monograph, *A Comparative View of the Sensorial and Nervous Systems in Man and Animals* (1822). Warren's theory of the multiple origins of North American Indian populations is presented in this brief appendix. The author argues that Warren's theory provided a motive for Morton's work and focused Morton's attention on skulls. Silverberg (1968) has shown that the multiple origins of North American Indians, specifically the attribution of all high culture in the New World to an Old World immigrant group (Atlantean, Egyptian, Phoenician, Israelite, or whatever), were pervasive in the United States in the early 19th century, but curiously omits Warren from his account. We will see that Morton had read Warren carefully.

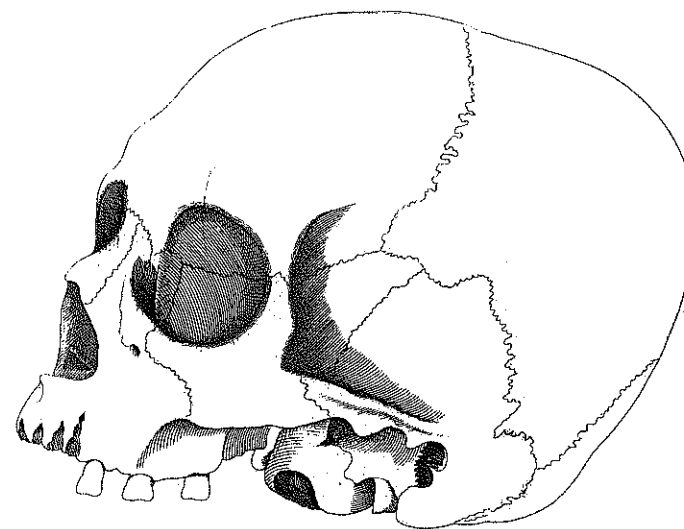
Warren's *A Comparative View of the Sensorial and Nervous Systems in Man and Animals* (1822) is a natural history of the neurological system. Most of the work consists of a literature review with strong preference for the work of Lamarck and follows Lamarck in viewing the brain as the prime mover in anatomy. The section of interest to anthropologists is Warren's original contribution, a neurology of several New World forms. It includes dissections of a lobster, a centipede, and an oyster, together with descriptions of four human skulls: a "Caucasian," two Indians from the Columbia River, and a "South Sea Islander" from the Marquesas. American Indian skulls from the vicinity of Boston and from Marietta, Ohio, are discussed, but are not illustrated.

Warren's theory of the peopling of the New World appears in his footnotes: "All who have turned their attention to the subject have, I believe, satisfied themselves that the ancient inhabitants of the Ohio and the Mississippi, of the middle and southern part of the United States, were a different people from the aborigines found here by our ancestors" (Warren, 1822:138). He gives a lengthy account of the Heckwelder's version of the Delaware or Lenni Lenape migration legend, in which three linguistically distinct migrations account for the diversity among the Indians.

The collections of the Warren Anatomical Museum at Harvard were assembled in part by the Boston Phrenological Society 1832–1842 (Bowles, 1976) and in part by Warren himself. Warren and Morton acquired skulls from many of the same sources. For example, Schoolcraft collected for Warren (Hrdlička, 1918) as well as Morton. Robert Bieder (1986) has criticized Morton's collecting practices and those of 19th-century anthropology as a whole as racist, but fails to explore the extent to which the various collectors and institutions were competitors or collaborators. Stanton discusses Warren as a member of the scientific community that appreciated Morton's research and states without citation or elaboration that Morton and Warren exchanged specimens (1960). Examination of their published accounts and illustrations shows that this was not the case. Morton presents figures of eight Northwest Coast and Columbia River specimens. Morton's plate 42 is similar to Warren's plate 6 (Fig. 4), but lacks postcoronal depression that Warren notes and has a canine that is missing in Warren's specimen. Morton's plate 43 is similar to Warren's plate 6 in both these regards, but details, e.g., the form of pterion, do not match. Morton's plate 48 Clickitat shares a fissure and missing anterior teeth with Warren's plate 7, but pterion is dissimilar. Morton notes the fissure as a healed fracture (1839:214). Warren credits T. H. Perkins for Columbia River specimens, whereas Morton credits J. K. Townshend. If one compares Morton's Naumkeag (1839:plate 33) and Warren's Nahant, the descriptions of the skulls do not match, although the descriptions of mortuary practices are very similar. Morton's robust male (plate 63) from a cave near Marietta, Ohio, is clearly not the female skull described by Warren from Marietta, even though both credit Dr. Hildreth

for specimens. While both Morton and Warren appear to be describing the same cave site, Morton credits the skull he figures to Andrews. There is thus no direct evidence that Morton and Warren shared specimens. Warren's illustrations lack the detail and accuracy of Morton's.

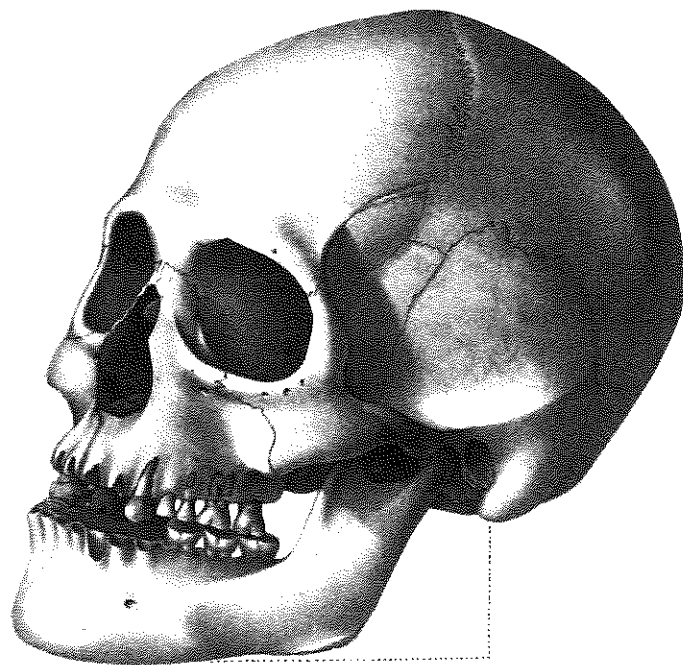
Warren had studied in Paris in 1799–1801 and he encouraged his son to seek out Cuvier during his son's tenure in Paris in 1832–1835, a period that coincides with Morton's most intense collecting activities. Warren and his son exchanged a lively series of letters that include many references to acquisitions of crania and other anatomical specimens (Jones, 1978). There are no references to Morton in the correspondence. This is surprising if Warren and Morton were advancing each other's collections, because Morton's many articles in the *Proceedings of the Academy of Natural Sciences of Philadelphia* in this era make the nature of his research quite clear. Similarly, Morton makes few references to Warren in the *Proceedings*. Strikingly, the only quotation from Warren's work in *Crania Americana* is a description of the intellectual abilities of American Indian students at Harvard (Morton, 1839:82). Warren is not cited in Morton's discussion of crania from the Columbia River region. Warren cites Heckwelder's work on Delaware migration legends from the *Transactions of the American Philosophical*



(a)

Figure 4 (a) Warren's Columbia River skull, plate 6 (Warren, 1822), and (b) Morton's Chinook skull (Morton, 1839).

(continues)



CHINOOK:

OF COLUMBIA RIVER.

Natural Form.

(b)

Lab. of John Collins 2719 South Third Street Philadelphia.

Figure 4 (Continued)

Society and must have seen Morton's many contributions to that journal. Warren and Morton were aware of one another's projects, but may have been competitors more than colleagues, much as were the larger competing scientific communities of Boston and Philadelphia.

V. LATE 19TH-CENTURY CRANIOLOGY IN NORTH AMERICA

In tracing a scientific genealogy linking Blumenbach to Morton, Morton to Broca, and Broca to Hrdlička, Brace has argued that Darwinism and the Civil

War resulted in "the effective eclipse of interest in Morton's work in America" (Brace, 1982:18). In so doing he has deflected attention from a group of scholars who contributed to craniology in the latter half of the 19th century and the first decade of the 20th. This group of scholars is interesting in their diverse perspectives and in their responses to new ideas reaching them from Europe. This chapter is limited to North America, but a similar history could be traced in South America. That history would begin with the Brazilian anthropologist João Batista de Lacerda (1846–1915), who cited Morton and—through Morton—Blumenbach in discussing crania in the collections of the Museu Nacional in Rio de Janeiro (Lacerda and Rodrigues, 1876).

Daniel Wilson (1816–1892), a professor of literature at University of Toronto and founder of the discipline of anthropology in Canada (Wilson, 1863; Trigger, 1966; Kehoe, 2002), contested Morton's claims for the racial unity of American Indians, stressing cranial index as the most important variable. Wilson collected eight linear measurements following Morton and Warren, making comparative use of their published data. Like theirs, his measurements are reported in inches. He collected crania throughout eastern Canada. Crania from a given region are grouped into "dolichocephalic" and "brachycephalic" types and means are reported for these groups. These two terms, with no intermediate category, come from the work of the Anders Retzius (1796–1860), who had earlier proposed three American races: the dolichocephalic Eskimo allied to the peoples of Northeast Asia, a round-headed race allied to peoples of the Pacific, and a long-headed one allied to the Guanches of the Canary Islands and perhaps the Lost Tribes of Israel via Atlantis (Retzius, 1859). Wilson cites Retzius as claiming that "it is scarcely possible to find a more distinct separation into dolichocephalic and brachycephalic races than in America" (Wilson, 1863:244), but he neglects to note Retzius' extreme diffusionism. It is noteworthy regarding Stephen J. Gould's criticisms of Morton that Wilson seems as oblivious to the effects of grouped data on comparisons of means as Morton was.

Language groups are a prominent feature in Wilson's analysis. He compared Huron, Iroquois, and Algonquian crania with Morton's moundbuilders and found the differences comparable to those seen among disparate European groups. He was particularly interested in cranial deformation as a contributor to differences between groups. He summarizes his reservations about the existence of a unitary American Indian race:

But the legitimate deduction from such a recognition, alike of extreme diversities of cranial form and of many intermediate gradations, characterizing the nations of the New World as well as the Old, is not that cranial formation has no ethnic value, but that the truths embodied in such physiological data are as little to be eliminated by ignoring or slighting all diversities from the predominant form, and assigning it as the sole normal type, as by neglecting the many intermediate gradations, and dwelling exclusively on the examples of extreme divergence from any prevailing type. (Wilson, 1863:264)

Wilson argued that there had been three migrations to the New World: from Asia via the Bering Strait, from Polynesia across the Pacific, and from Europe across the Atlantic. Kehoe (1999) has related this model to his earlier research on the possibility that there had been an ancient group in Britain that preceded the migration of Celtic peoples.

A second proponent of diversity among New World populations was James Aitken Meigs (1829–1879). Meigs was Samuel Morton's student and successor as curator of the collection Morton had amassed. Like Morton, Meigs was a Philadelphia physician and professor of medicine and an active participant in the Philadelphia scientific community. His earliest publication on Morton's collection is a lengthy review of international craniological literature and is largely hagiographic in its account of Morton's work (Meigs, 1857). It appears in Nott and Gliddon's *Indigenous Races of the Earth*, but pointedly fails to engage in the polygenist and racist agenda of the remainder of their volume (Horsman, 1987). A noteworthy point is Meig's objection to the label "Mongol-American" for Eskimo as misleading. He argues that there is no close resemblance with crania one might associate with the historical Mongol peoples.

By 1866 Meigs had assimilated Retzius' and Wilson's critiques of Morton's work (Meigs, 1866). He reanalyzed Morton's collection, classifying the American Indian crania as dolichocephalic, mesocephalic, or brachycephalic, and according to eight skull shape categories, six of these applying to the most common long-headed, or dolichocephalic skulls. Individual crania or small series of crania identified by tribe are his unit of analysis. Measurements are not presented directly. He concludes "that these ethnical or typical groups are founded upon osteological differences as great as those which, in Europe, suffice to separate the Germanic and Celtic stocks on the one hand, from the Ugrian, Turkish and Slavonian, on the other" (Meigs, 1866:235). In his 1866 paper he again situates his study with respect to international literature in anthropology. For example, he points out that d'Orbigny's *L'Homme Américain* appeared in the same year as *Crania Americana*, but reached the opposite conclusions about the diversity of Indians. For Meigs the radical aspect of Morton's work was his argument for the unity of American Indians. Meigs even suggests an interesting link between Morton and Benjamin Smith Barton, a Philadelphia physician and academic who wrote a philological treatise arguing that all American Indian languages sprang from a single ancestor, an insight that deserves further investigation.

Meigs' most surprising paper is a description of a low, heavy-browed skull from Illinois that suggests that he was familiar with the Neanderthal find just a decade after its discovery: "If the position in which it was discovered be any evidence of its age, it belongs, in all probability, to an earlier inhabitant of the American continent than the present race of Indians" (Meigs, 1867:415). Meigs is thus an early contributor to the claims for great antiquity in the Americas that Hrdlička would spend his career combating.

Neither Wilson nor Meigs was in any way isolated from developments in anthropology in the Old World, and this is equally true for late 19th-century scholars. The legacy of Morton and Meigs in Philadelphia passed to Harrison Allen (1841–1897), a physician and professor of medicine. Allen published several anatomical and pathological studies of the skull (Hrdlička, 1918; Spencer, 1997c). His principal contribution to our subject is his monograph on five crania from Moore's shell mound excavations in Florida (Allen, 1895a). It rivals Morton's work in its beautiful 1:1 engravings, with four views of each skull, and in its meticulous descriptions following the conventions of Broca's French school. Crania representing 17 tribes from the Philadelphia collections and from the Columbia University medical department are used in comparison. He notes a moderate frequency of metopism and cites—without making behavioral inferences—the work of Lombroso and others on the very high frequencies of this condition among European criminals.

Meanwhile in Boston, Frederic Ward Putnam (1839–1915) gathered around him at the Peabody Museum several physical anthropologists who worked in Broca's paradigm (Mark, 1980; Brew, 1968). Cordelia A. Studley (1855–1887) published a single paper (1884), a description of skeletons from four caves in Coahuila, Mexico, from the museum collections. Her craniology consists of 62 measurements, including angles, indices, and cranial capacity. Skulls are grouped as "dolichocephali," "mesaticephali," and "brachycephali" following Retzius, and means and ranges are reported separately for these three groups. She is perhaps the first person to point out that crania from cave site mummy bundles in the greater Southwest are markedly more dolichocephalic than recent peoples of the region. Lucien Carr (1829–1915) may be better remembered for his archaeological explorations on behalf of Putnam's museum, but he also produced three descriptive papers on craniology that are similar in method to Studley's (Carr, 1878, 1879, 1880).

The first American doctorate in our field was awarded in 1896 to Putnam's student Frank Russell (1868–1903). Russell was curator of physical anthropology at the Peabody Museum until his early death (Hrdlička, 1914a; Brew, 1968). He contributed two craniological papers: a comparison of New England Indian and Labrador Eskimo crania and long bones (Russell and Huxley, 1899) and an application of what we now call discrete trait analysis to the problem of American Indian diversity. The latter paper used "nearly two thousand skulls in the Peabody Museum at Harvard University" (Russell, 1900:737) and has sample sizes of 1200 to 1500 for most comparisons. He presents frequencies for nine characters across nine regional series, but concludes: "I hope that the facts presented may prove suggestive and interesting, but do not expect them to establish firmly any hypotheses regarding the origin or affinities of the Amerinds" (1900:743). His series include all those that Studley and Carr had measured.

Further afield, the Southwestern studies of Washington Matthews (1843–1905) posited a relationship among the Zuni, the Hohokam, and ancient Peru, a pet theory of Matthews' colleague Frank Cushing (Merbs, 2002). The Inca bone and brachycephalization are the keystones in Matthews' edifice (Matthews *et al.*, 1893). Matthews' work is indirectly connected to Harvard via Putnam's encouragement, support, and curation of some of the materials, and Putnam encouraged Cushing in his racial theories. George Langford's (1876–1964) demonstration that dolichocrany was older than brachycrany on stratigraphic grounds in Illinois mounds (Langford, 1927) was perhaps inspired in part by Putnam (Browman, 2002:261; Kullen, 2000). A descriptive craniology of Ontario Indians produced by Susanna Boyle (1869–1947) is similarly connected to and influenced by Putnam (Boyle, 1892; Killan, 1983). Even Harrison Allen (1895a), who was Meigs' successor in Philadelphia, produced his monograph on Florida crania with encouragement from Putnam via Putnam's support for the archaeologist C. B. Moore.

Putnam articulated his vision of American Indians in an 1899 address before the AAAS: "The facts show diversity—of race" (Putnam, 1899:12). He recognized nine types: Eskimo type, northern and central so-called Indian type, Northwest brachycephalic type, Southwestern brachycephalic type, Antillean type, Toltec brachycephalic type, Ancient Brazilian, Fuegian, and pre-Inca (1899:8). He thus retains Morton's three races and adds six more. Putnam's commitment was clearly to a model of multiple origins of American Indians, and his influence is visible among the late 19th-century craniologists with whom he interacted. Putnam encouraged both Hooton and Hrdlička in their early 20th-century efforts in craniology. Hrdlička even refers to himself as one of Putnam's "boys" (1918:155). Hrdlička went on to rebut the concept of multiple origins of American Indians (Spencer, 1979), while Hooton endeavored to reinforce it.

VI. ALEŠ HRDLIČKA 1869–1943

The typological study of Indian and Eskimo crania became the dominant enterprise as American physical anthropology emerged as a profession after 1900. Aleš Hrdlička, founder of the American Association of Physical Anthropologists and first editor of its journal in 1918, as well as first curator of physical anthropology at the Smithsonian Institution, developed the new science using the race concepts of the 19th-century French school of anthropology, as Brace has shown (1982).

What did Hrdlička import from France? Brace argued that Hrdlička's resistance to Darwinian explanations and his static race concept are attributable to his admiration for Broca's anthropology. Paul Broca wrote little about the

New World, and his views were largely based on Morton's work. For example, Broca's disciple Paul Topinard devotes only a few sentences to the subject:

If one trusts the cranial capacity method followed by Morton, the American cranium is one of the least capacious among humans. It is more often dolichocephalic than brachycephalic, with respect to the collection in Philadelphia. Judging by the collection of the Museum, it would be on the contrary mesaticephalic, what could be had from a mixture in equal proportions of brachycephals and dolichocephals. . . . Dolichocephaly is more extensive, following Morton, among the tribes that originally lived east of the Alleghenies, and brachycephaly among those west of the Mississippi. The same condition is reproduced on the coasts of South America. (Topinard, 1876:507, author's translation)

At the time the collections of the Musée de l'Homme were largely South American. Topinard follows Morton in excluding the Eskimo from the American race as defined earlier. In the early 20th century, American craniology becomes distinct from similar work in Europe in the degree to which these researchers interacted with archaeologists and others, in part because the Boasian race–language–culture model encouraged such interaction and in part because Hrdlička's work was grounded in the multidisciplinary perspective encouraged by the institution with which he was affiliated throughout his career.

Hrdlička's concept of race in the Americas is difficult to characterize. He stressed the relative unity and recent origins from North Asia throughout his career, but the details vary. In an early paper heavily influenced by Putnam, he admits the possibility that there was a low-vaulted race that preceded the historic peoples of the Delaware valley, assigning two crania from one of several sites studied by Russell to this group (Hrdlička, 1902c). As late as 1912 he opined that "it is also probable that the western coast of America, within the last two thousand years, was on more than one occasion reached by small parties of Polynesians, and that the eastern coast was similarly reached by small groups of whites, but these accretions have not modified greatly, if at all, the mass of the native population (Hrdlička, 1912b:12). By 1917 he recognized four subtypes scattered among the native populations of the Americas: dolichocephals, eastern brachycephals, western brachycephals, and the Eskimo (Hrdlička, 1917b; Rivet, 1943:57). That this model is little advanced beyond Broca's is readily apparent, but it is less complex than Putnam's. He consistently minimized New World variability: "There are, it is true, subraces of the American Indians, a number of them; but the differences between them are less than the differences between, for instance, the Italian and the Scandinavian in Europe" (Hrdlička, 1928:815). Near the end of his life he summarized his views: "The Chinese present at least two types, the American Indians five or six, the Eskimo two, but these do not deserve the name 'races,' unless the use of the term be much stretched" (Hrdlička, 1941:184).

In later life his extensive fieldwork in Alaska led him to complexities. Harper and Laughlin (1982) point out that Hrdlička's view of the relationships among Eskimo, Aleut, and Indian peoples was novel, a concept that they label the Eskimo wedge hypothesis. Hrdlička saw the eastern Eskimo as quite distinct from Indians, but found less evidence for distinctiveness in the western Arctic, arguing for a common ancestry separate from most Asian peoples, and further differentiation in the Americas.

Letters exchanged during Hrdlička's lifetime by Georg Neumann and Charles E. Snow criticize Hrdlička for ignoring archaeological provenience and lumping crania by state (Jacobi, 2002). Hrdlička was remarkably insensitive to subtleties of archaeological provenience and there is a frank recent literature critical of his field technique (Krupnik, 2003; Loring and Prokopec, 1994). Hooton refers tongue in cheek to the dogma of isolation of the New World from the Old as "a sort of *ex post facto* Monroe Doctrine" (1973:133):

In fact, it seems glaringly improbable that the Bering Straits and the Aleutian Islands should have strained out all prospective incomers except Mongoloids . . . there was no Dr. Hrdlička standing on the Aleutian equivalent of Ellis Island, acting as Prehistoric Commissioner of Immigration to enforce an alien exclusion act applicable to all save Mongoloids. (Hooton, 1946:650)

Hrdlička's single mindedness regarding the racial prehistory of the New World is difficult to overstress. In the lengthy essay on the history of physical anthropology in the United States that appears in the first volume of *American Journal of Physical Anthropology*, he says of Herman ten Kate (1858–1931), a Dutch Americanist trained by Broca, "He has the distinction of being perhaps the last living anthropologist of note who defends the theory of a multiplicity of races on the American continent, though this is largely if not entirely due to his interpretation of the term 'race'" (Hrdlička, 1918:379). Hrdlička's assessment was premature.

VII. EARNEST ALBERT HOOTON 1887–1954

E. A. Hooton is the most quixotic of the prominent contributors to physical anthropology in North America. Unlike his predecessors, his training was in classics, not medicine. His doctoral thesis betrays interest neither in physical anthropology nor in human variability (Hooton, 1911). Hooton's sojourn in England as a Rhodes scholar was a watershed experience. He studied first with Robert R. Marett and then with Arthur Keith. He returned to the United States to teach physical anthropology at Harvard for the remainder of his life, mentoring most of the prominent contributors to mid-20th-century physical anthropology (Giles, 1997; Garn and Giles, 1995; Shapiro, 1981). Oddly, those students have

written relatively little about his contributions to our understanding of the diversity of Native American peoples.

Hooton's first contribution to craniology relevant to the peopling of the Americas appeared as the third article in the first volume of Hrdlička's new periodical, the *American Journal of Physical Anthropology*. It is remarkably modern in its method and tone. He compared Viking remains from Iceland with Eskimo, California, Chukchi, Italian, and Libyan crania with respect to frequencies of mandibular torus, palatine torus, thickened tympanic plate, and sagittal keel, attributing similarities between Icelanders and Eskimo to functional consequences of "habitual chewing of very tough food" (Hooton, 1918:76). Here we see an adaptationist perspective on variation: features shared by disparate groups in similar environments are adaptive and not useful in assessing affinities.

Hooton's descriptions of skeletal remains from two sites in southern Ohio, Madisonville (1920), a Fort Ancient Late Prehistoric site, and the Turner Group (1922), a Hopewell mound complex, constitute his contribution to the physical anthropology of the ancient Midwest. Both are quite conventional in tone and content, proceeding from age and sex composition to cranial measurements, discrete variation, and postcranial metrics. There is little discussion of pathology. Hooton's study of Turner incorporates Cordelia Studley's unpublished observations, although he did not find her measurements useful. The intrusive component at Turner is differentiated from primary series in lacking brachycephaly but exhibiting cranial deformation. The salient point for this paper comes at the end of the Turner Group paper:

The primary and secondary series resemble each other much more closely than either resembles the Madisonville series. It may be said positively that the people of the Turner Group show practically no physical affinities with the people who live on the Madisonville site, beyond those which are common to all Indians. (Hooton, 1922:132)

Here, Hooton demonstrates the morphological distinctiveness of Middle Woodland and Mississippian populations in the Midwest in a context in which cultural distinctiveness and chronological sequence, if not its magnitude, are clear from the accompanying archaeological analysis.

Hooton's most substantial contribution to Americanist anthropology is his *Indians of Pecos Pueblo* (1930). Several aspects of this project are discussed at length in Lane Beck's contribution to this volume and are not repeated here. In it, he takes the theme of variability among American Indians to an extreme that has stimulated a continuing critique of his work as racist, and his version of the typological paradigm is certainly remarkable in its complexity and eccentricity. The 129 suitable male crania from Pecos were sorted into seven morphological types: Basket Makers¹, Pseudo-Negroids, Pseudo-Australoids, Plains Indians,

¹This is the archaic spelling. The current accepted spelling is Basketmaker.

Long-faced Europeans, Pseudo-Alpines, and Large hybrids, as well as residuals not accommodated in the seven types, without regard for cranial deformation. The "validity" of these types was established by comparing means for cranial measurements and indices with means for the group as a whole: if the type mean deviates by more than one standard error from the group mean in several variables, Hooton accepted the type as valid (1930:203). The types were then compared to one another and to crania from elsewhere in the Americas and the world, again by examination of means. While all types persist throughout the site sequence, proportions of the types shift through time, with dolichocephalic types predominating in the earlier horizons.

What all this meant to Hooton is a puzzle. Clearly, he demonstrated that the Pecos crania are quite variable and that the variability is not explained by cranial deformation or by change in stature through time. In *Indians of Pecos Pueblo*, several interpretations are presented, for example:

Of course, if one wishes, he may argue with considerable plausibility that the earliest strata of American Indians may have carried among other strains some of the Australoid blood and that these Pecos "Pseudo-Australoids" represent a segregation of such strains. Candidly however, I do not think that our Pecos Australoids sufficiently resemble real Australians to justify even this moderate opinion. Large brow-ridges and platyrrhine noses together with short, broad faces may not always mean Australians, although they suggest such a type. The total absence of prognathism in our "Pseudo-Australoids" is a strong argument against the identification. I am much more impressed with the resemblance of our "Pseudo-Australoids" to the Ainu, since here the indicial similarities are very marked. (Hooton, 1930:262)

As in this quotation, the comparison groups are selected consistent with the racial identification of each type. No attempt is made to compare each type to the whole range of comparison groups. "Basket Makers" are compared with crania from the Coahuila Caves, California and Egypt, "Pseudo-Negroids" with groups ranging from the Andaman Islands to Zulu, "Pseudo-Australoids" to Tasmanians and Peruvians, "Plains Indians" to Arikara and Illinois Algonkians, "Long-faced Europeans" to Eskimo and Chinese, "Pseudo-Alpines" to Burmese and Tibetans, and "Large hybrids" to Tennessee Stone Grave and Madisonville crania, among many others. Hrdlička's *Catalog of Crania* (1924, 1927, 1928) is conspicuous among the citations.

Hooton's Harvard colleague Roland B. Dixon (1875–1934), whose work is clearly a source for a portion of the Pecos typology, recognized three North American races more or less consistent with Hrdlička's: Northeastern Dolichocephals (including Hrdlička's Eskimo), Southwestern Dolichocephals, and Central Brachycephals, and recognized within these varying proportions of his eight Old World types (Dixon, 1923), despite hewing to the Bering Straits orthodoxy. Dixon (1923:419) first noted on an Alpine and a Proto-Negroid type at Pecos and in the Coahuila crania. In what is perhaps a response to criticism,

Hooton comments on their relationship 3 years after the publication of *Indians of Pecos Pueblo*:

"The method which I have employed in segregating cranial types differs quite radically from that of my colleague, Professor Dixon. He utilized only combinations of the conventional subdivisions of the length-breadth, length-height, and nasal indices . . . I, on the contrary, used morphological judgments in selecting the types, and, after establishing their statistical integrity, sought their affinities with other crania by utilizing the means of all available cranial measurements and indices and appraising the sum total of significant differences" (1973:161) and he insists that "the American race is a composite race . . . composed of heterogenous strains welded together by mixture, not of wonderfully adapted types made out of common clay by a creative environment." (Hooton, 1973:162)

Hooton insisted on the multiple origins on American Indians throughout his life (Hooton, 1946). Pecos was a lens through which he saw a grand and unorthodox scheme for the peopling of the Americas. Modern morphometric studies support his views at several levels. For example, Brace still finds evidence linking the most ancient American specimens with the Ainu on the one hand and Oceania on the other (Brace *et al.*, 2001), and a recent reanalysis of Hooton's craniometric and discrete data finds considerable variability and little evidence for change through time among the chronological components at Pecos [Weisensee (2001); see Beck's demographic reanalysis in this volume].

Hooton's method at Pecos and in his more general schemes for race classification is at root an application bertillonage, a primitive form of multivariate classification. While he may have borrowed the technique from Dixon, whose three indices, each trichotomized, yield 27 possible types, or from Francis Galton (1822–1911), who had adapted the methods of criminologist Alphonse Bertillon (1853–1914) in his study of dermatoglyphics (Gillham, 2001), the similarity is clear. Bertillon appears to have invented this antecedent of contingency table analysis. Hooton also appears to have borrowed the method of composite photographs of the Pecos types from Galton, although Galton is not cited in the bibliography, which is quite limited. Connections via Hooton's extensive work on criminal typology are likely. Howells argued that Dixon's work was largely independently developed and that Dixon's influence on Hooton flowed largely through their earlier collaboration on racial assessment of crania from the Canary Islands. Dixon himself cited sources with regard to data rather than ideas, suggesting that his analysis is largely original. However, Dixon's race labels correspond closely to those in general use in Europe. His method of casting crania from a single site or region into a series of types is unusual, and Hooton adopted this practice. Both represented a group of people as consisting of various percentages of types. As Dixon's colleagues noted at his death "he was the first anthropologist to show by scientific data the composite character of the American Indians as being primarily Mongolian but with admixtures which can be affiliated with early white and negroid strains. Recent archaeological investigations have borne out

this thesis" (Tozzer and Kroeber, 1945:105). Those investigations were surely Hooton's.

The last of Hooton's contributions relevant to the present topic is his paper on skeletal material from the Cenote of Sacrifice at Chichen Itza (1940). There is a lengthy discussion of various mechanisms for cranial deformation and some interesting paleopathology antithetical to the interpretation of the remains as sacrifices that has been largely overlooked by Mesoamerican archaeologists. Echoing Matthews and Putnam on brachycephaly and high civilization in the Americas, Hooton remarks on the similarity of Peruvian, Maya, and Southwestern remains. He reiterates his distain for the Bering Land Bridge model and suggests Armenian and even Toda contributions to the remote ancestry of the Maya!

It is noteworthy that Hooton's work extends the projects of Putnam's protégés at the turn of the century. Cordelia Studley had begun a study of the Turner material, although her AAAS address on this series was never published. Hooton's publications on the Turner and Madisonville series build directly on her work, although she is not acknowledged through citation. Similarly, Hooton's Iceland study mirrors Russell's Labrador paper in its logical structure (1899). *Indians of Pecos Pueblo* cites none of Putnam's protégés, but Studley's Coahuila series and Russell's California series are used in comparisons. Dixon might be added to this list.

The larger connections of Hooton's craniology are similarly difficult to trace through his citations. Hooton's mentor Sir Arthur Keith is central to recent critiques of scientific racism, and some have seen close correspondences in their work (e.g., Brace, 1982; Barkan, 1992). Whatever one's opinion of his racial politics, Keith was certainly a taxonomic splitter whose ideas are often congruent with Hooton's. Keith accepted the Punin skull from Ecuador as evidence for "a pleistocene invasion of America by an Australoid people" (Keith, 1931:312), and Hooton refers to it as "a skull any competent craniologist would identify as Australian in type" (Hooton, 1946:650).

Much of the work of Hooton and Dixon seems fanciful to modern readers. It is a useful corrective to presentism to note that T. D. Stewart took their case for multiple late migrations accounting for brachycephalization in the New World to develop his own argument that these late migrants brought with them, not only round, high heads, but the practice of cranial deformation and the pathogen responsible for syphilis (Stewart, 1940f).

While there are several modern summaries of Hooton's work, some written with great affection (Garn and Giles, 1995; Shapiro, 1981), there is as yet no full biography of this remarkably interesting figure. Critical assessments of Hooton's work are astonishingly varied in their focus. Wolpoff and Caspari (1997) call him racist and Lamarckian, and his work polygenism, blaming his association with both Dixon and Keith for these faults. Brace (1981) calls Hooton's scheme "polyphyletic," not "polygenetic," and suggests that "Hooton and his students were less than fully conscious of the strains of romantic racism that

constituted a major part of their background" (Brace, 1982:15). One of the few positive recent assessments is Stewart's (1981) demonstration that Hooton demolished Hrdlička's claims for morphological dating. Stewart also pointed out that despite their apparent intellectual differences, Hrdlička counted Hooton as his closest friend. It is perhaps less surprising than some have found it (Wolpoff and Caspari, 1997) that Boas enlisted Hooton and Hrdlička in trying to move American physical anthropologists to speak out against Hitler's race policies.

VIII. PAUL RIVET 1876-1958

Hooton's work may look a bit more mainstream when viewed from the perspective of contemporaneous work in France. Paul Rivet was a polymath anthropologist of the Boasian style, publishing in all four subfields. He was particularly influential in South America and contributed to the organization of physical anthropology as a discipline in Mexico, Ecuador, Bolivia, and Brazil (Leon, 1977).

Christine Lauriere (2000) has written an insightful analysis of Rivet's early career. He became interested in anthropology while serving as an army physician in Ecuador. Between 1906 and 1912 he established himself as a professional anthropologist. His research on prognathism was part of a campaign aimed at securing a position, first at Societe d'Americanistes, then at the Museum d'Histoire Naturelle, and eventually as founder of the new Musee de l'Homme. Lauriere showed that Rivet's demonstration that the facial angle produced no systematic hierarchy of races was strategic as much as scientific and was a key element in his rejection of Paul Broca's 19th-century physical anthropology. Rivet left Broca's Société d'Anthropologie in 1911 and founded, with colleagues who constitute a roster of the memorable figures in French social thought, a new Institut Français d'Anthropologie that integrated all the human sciences. Lauriere summarizes the importance of the prognathism studies: "He had to construct for himself a most convincing curriculum vitae in looking toward the next candidacy at the museum that he knew from experience was very attached to the pre-eminence of the biological over the cultural."

However, once nominated, Paul Rivet took advantage of the global conception of anthropology defended by Paul Broca and Armand de Quatrefages to take his work in a completely different direction: "He devoted himself henceforth to studies of American Indian linguistics, ethnography and archaeology" (Lauriere, 2000:20, author's translation). The parallels to Franz Boas' career path in the United States are remarkable: the legitimizing role of early research in physical anthropology, a revolutionary concept of an integrated field, and an emphasis on institution building are shared features of Boas and Rivet.

Rivet's four papers on prognathism are a remarkable tour de force, both with regard to sample size and with regard to exhaustiveness (1909b,c, 1910a,b).

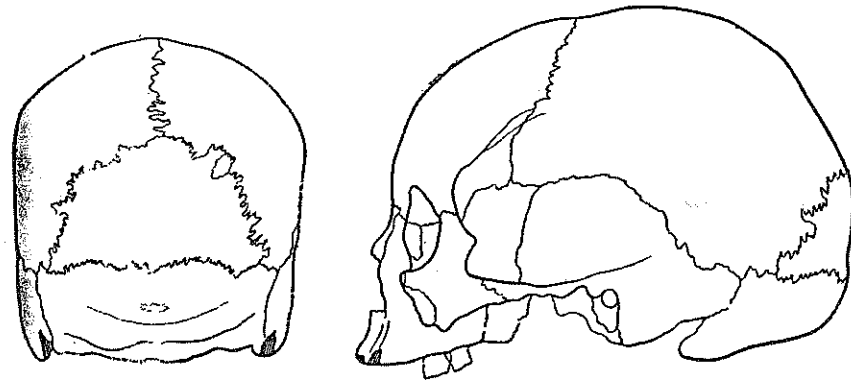


Figure 5 Rivet's Pericue skull XVI (Rivet, 1909a).

He compared the several measures of facial angle, beginning with 5615 humans, 151 apes, and 334 monkeys (1909c) and adding series as the study progressed. A table in the final study includes 665 crania from the Americas: 11 Amazonians, 18 Zuni, 30 Ancient Peruvians, 17 Ancient Mexicans, 73 Ancient Ecuadorians, 29 "Peaux-Rouges" (presumably Plains Indians), 44 Eskimo, 25 Tierra del Fuegians, 31 Moundbuilders (including Hrdlička's series from Arkansas and Louisiana), 21 Andeans from Argentina, 18 Pampians, 36 Northwest Coast, 17 continental California, 17 Pericue (Baja California; Fig. 5), 240 Channel Islands, California, 21 Aleuts, 9 Carib-Arawaks, and 7 Yucatecs, listed in order of facial angle from mesognathic to prognathic (Rivet, 1910b:642)! He summarizes:

In America, a great center of prognathism occupies the Northwest Coast, represented by the Aleuts, the Californians and the Indians of the Northwest. In the Eskimos and above all in the Peaux-Rouges prognathism diminishes clearly. It is the same with the Zuni and the ancient Mexicans. On the other hand, the Moundbuilders and above all the Yucatecs are distinguished by the small size of their naso-alveolo-basilar angle. (Rivet, 1910b:648, author's translation)

The prognathism papers are of interest here because this large sample constitutes the experiential basis for Rivet's concept of race as expressed in the skull. One notes a bit of bias toward California. Statistical analysis is limited to comparison of means and ranges by inspection, and extensive use is made of tripartite categorization of continuous measures, e.g., orthognathic-mesognathic-prognathic for the facial angle. Nevertheless, the scope, energy, and complexity of Rivet's study is impressive: he demonstrated that facial angle varies with age and sex, that it has no consistent relationship to cranial index and facial index, that geographical races include populations that differ enormously in facial angle, and that the various measures of facial projection are far from equivalent one to another, thus laying to rest the enterprise begun by Blumenbach: arranging races in order of facial projection.

Rivet's other contribution to the craniology of North American groups is his description of 18 skulls from five localities in Baja California (1909a). It is the third of a series of studies, with the earlier two concerning ancient crania from Paltacalo, Ecuador, and Lagoa Santa, Brazil (Rivet, 1908). This paper makes extensive use of bivariate plots of the principal cranial indices to separate Baja California from other North American Indian groups, making use of published and unpublished data from Carr, Allen, and Hrdlička, among others. He then links the Baja California series, first, with the ancient population from Lagoa Santa in Brazil, and thence with Melanesia and Australia in a *type hypsistenocephale*, characterized by a high, narrow skull. This type is contrasted with American Indians. He then proposes a trans-Pacific migration accounting for his findings:

I have searched without success for an explanation of how Melanesian migrations could have reached the coast of California, whether voluntarily, or by way of sea currents. It suffices to recall that numerous and indisputable observations have shown the possibility of great voyages, even for uncivilized populations. Besides, we are more or less completely ignorant of the exact configuration of the north Pacific in the geological period that followed the appearance of humans. (Rivet, 1909a:247)

Spencer (1997c) implies that Rivet used Mendes Correa's map of Antarctic migration routes, but this is an anachronism, perhaps misunderstood from Stewart's (1973) popular account of these ideas. Recent work revisiting the question of the affinities of Baja California populations using modern morphometric techniques have resurrected Rivet's thesis (González-José *et al.*, 2003).

Rivet's ideas about the peopling of the New World are laid out in their fullest in a monograph he produced late in his career that integrates his craniology with his ethnographic and linguistic research. *Les Origines de l'Homme Américain* was published simultaneously in translation in Mexico as well as in Canada (Rivet, 1943). Neither historical linguistics nor ancient DNA supports his views today, but this does not lessen their historical interest, and he has been cited quite frequently in recent literature on the peopling of South America.

Like Hooton, Rivet confronted Hrdlička's dogma of a single migration of American Indians across the Bering land bridge. In his view the Indians were too diverse to fit Hrdlička's model. He recognized, marshaling craniometric and linguistic evidence, late Asian affinities in the Eskimo, Polynesian, and Indonesian affinities in the Hokans, and Australian affinities in the peoples of Tierra del Fuego and Lagoa Santa in Brasil (Rivet, 1943). Rivet's ideas have been revived recently in the controversial new morphometric studies surrounding the "Luiza" specimen from Brazil (Powell and Neves, 1999), and his views have continued to be accepted as mainstream in Latin America (Comas, 1960, 1974).

Rivet's craniology is more quantitative than Hrdlička's and more facile in its use of geometric techniques than Hooton's (e.g., Rivet, 1909a,b,c). Rivet's use of bivariate plots of the principal cranial indices is an intriguing precursor to

multivariate statistics in that they visually summarize three or four linear variables at once. Indices were of course central to late 19th- and early 20th-century craniology and to a quest for measures of shape—of morphology—independent of size. They have largely disappeared from our science, partly because of their refractoriness to statistical analysis and partly because multivariate methods have supplanted them.

Historians of anthropology are discomfited by the failure of early 20th-century anthropologists whom they regard as liberal and antiracist to reject the concept of race, and Rivet is no exception in this regard. Despite his liberal role in the history of French anthropology and his heroism in the Resistance, he was paternalistic toward his ethnographic subjects and opposed the decolonialization of Algeria (Lauriere, 2000; Reynaud-Paligot, 2001).

IX. BRUNO OETTEKING 1871–1960

If the French and British traditions in early 20th-century physical anthropology reached the New World in such diverse forms, we may expect similar variety from the German tradition. Bruno Oettking did his doctoral work under Rudolf Martin (1864–1925) at the University of Zurich, completing his dissertation on the craniology of ancient Egyptians in 1908. He held positions at several German institutions. In 1913, Franz Boas (1858–1942) recruited Oettking for his research group at the American Museum of Natural History, which focused on the Arctic and Northwest Coast collections. Oettking moved with Boas to Columbia University in 1920, where he held an appointment as lecturer until his abrupt dismissal 1938. He also served as curator of physical anthropology at the Museum of the American Indian. At Columbia, Oettking taught the physical anthropology courses that made Boas' program a four-field department (Weiant, 1960).

Oettking's work is meticulously, perhaps obsessively, descriptive. His most ambitious project was the study of skulls collected by the Jesup North Pacific expedition, *Craniology of the North Pacific Coast* (1930a), published in a volume shaped by Boas' interests and published under Boas' editorship. The series of 560 skulls is divided among four groups: those evidencing Cowichan, Chinook, or Koskimo styles of cranial deformation and undeformed crania. Ethnic groups are distributed unevenly across these four categories, and the last category includes Siberian Eskimo and Chuckchee crania, a strategy that makes comparison among groups problematic. The analysis is grounded in Boas' article on cultural patterns of cranial deformation (Boas, 1890). Oettking's principal question is metric and non-metric distinctions among the three varieties of artificial cranial shaping. There are 107 figures illustrating discrete variations in exhaustive detail. Most recent citations of Oettking's work draw on his descriptions of variants and

on his somewhat questionable demonstration that cranial deformation affects the frequency of many discrete traits. The question of race occupies a small part of this monograph. The undeformed group is compared with data from Oettking's own previous studies of Egyptian and Californian crania at the Museum of the American Indian (1925) and Eskimo crania in the collections at Dresden, as well as Hrdlička's data on Mongol crania. A form of pattern profile analysis is an interesting innovation in these comparisons (Oettking, 1930a). His conclusion is remarkably brief and qualified:

Of a number of crossproducts the narrowing of the face and nose have been recognized as progressive and would have to be attributed in our case to the blending with another morphologically different and, as it were, superior racial group, such as early caucasoid elements It was not intended by the author to draw into his study of a rather limited but at the same time all the more important anthropologic domain, the problem of Polynesian or other origin. From his present investigations, however, he derives the conviction of North Asiatic migration, the Mongolian affinity, the premigratory cross-breeding with distant (precaucasid?) elements, and finally the phaenotypical differentiation of the American Indian on American soil. (Oettking, 1930a:376)

Oettking's reluctance to reach conclusions afflicts his other typological publications to an even greater degree than in this tortured prose (1925, 1930b, 1931, 1934, 1945). Certainly, the labeling of features as "primitive" or "superior" strikes a discordant note in the work of a protégé of Boas, and the failure to develop an ethnic or linguistic dimension to the analysis is surprising. His many publications in *Indian Notes and Monographs* seldom venture beyond description, and he was notably slow in producing them.

The extensive literature on the career of Franz Boas is essentially silent on his relationship with Oettking. One is curious about their long professional association and the issues that led to Oettking's dismissal. The focus on cranial deformation in *Craniology of the North Pacific Coast* is certainly consistent with Boas' agenda of demonstrating environmental plasticity in skull shape (Holloway, 2002). One looks in vain for the statistical sophistication that characterized Boas' publications in physical anthropology (Tanner, 1959; Howells, 1959). There are many eccentricities in statistical language. For example, Oettking uses the term "correlation" for tables reporting means of facial measurements grouped by tripartite categorizations of cranial length, cranial breadth, and cranial index (Oettking, 1930a:102), but he uses no correlation statistics.

In a late publication on Arctic crania he assigns three skulls from a single site to three different types: "the crudest and most robust ones as the morphologically most inferior and belonging to an old, perhaps primarily pre-Columbian ethnic stratum." He attributes the lack of "homogenous racial integration" (1945:307) to Russian admixture and "extraneous derivation" (1945:308), citing Georg Neumann (1942) on low vaults. He omits the cranium with the most gracile face and rounded vault as pathological, diagnosing hydrocephalus to explain

the anomaly. The Dayak are used as a comparison group in this study. This hardly strikes one as the work of a committed Boasian!

While Oetteking's overview papers, many in German language journals, advocate the orthodox view—Boas and Hrdlička's Bering Strait scenario (1928, 1932)—one finds some remarkably old-fashioned claims, e.g., orthogenesis: "Nature herself always progresses from the crude to the more refined, and from the simple to the more complex" (1928:817). He flirts with ideas from Hooton, Rivet, and ten Kate in his choice of comparative samples and in his hair-splitting, tentative approach to typology. If we remember him for nothing more, he documented a great many skulls now threatened with deaccession, and he did so in a very transparent way.

Boas' extensive correspondence contains a few hints concerning his rapport with Oetteking (Boas, 1972). Oetteking wrote to Boas on January 18, 1936, protesting his dismissal. There is no letter in response, and other correspondence shows Boas negotiating his own retirement and arranging lectureships for others, notably the ethnomusicologist George Herzog and the ethnographer Frans Olbrechts, both refugees from Nazi Europe. A letter to Dean H. L. McBain dated March 12, 1936, rates these two candidates among others, including Rivet: "Rivet does not speak English. He is an agreeable dabbler in many different subjects and has a good knowledge of the archaeology of the most northern part of South America." In an intriguing letter to Boas dated Feb. 27, 1936, Alfred Tozzer of Harvard wrote "I am delighted to learn that you are not going to have Columbia humiliated by the presence of our ex-tutor and instructor." This person is not identified, but one suspects that Tozzer refers to Oetteking.

X. GEORG KARL NEUMANN 1907–1971

Georg K. Neumann was among the last physical anthropologists committed to the typological concept. His dissertation *Racial Differentiation in the American Indian* was the last grand effort at defining races in native North America. The University of Chicago dissertation was accepted in 1950. Preliminary versions were circulated and cited earlier (Neumann, 1941; Martin *et al.*, 1947). The dissertation was immediately and widely published (1952, 1954a,b) and was critically reviewed (Angel, 1954; Stewart and Newman, 1951, 1954; Comas, 1960). It remained the paradigm for the remainder of Neumann's scholarly output. This work is particularly situated in culture history and language, reflecting Neumann's mentors.

Neumann's training in physical anthropology began under Fay-Cooper Cole (1881–1961), even though Krogman rather than Cole supervised his dissertation. Neumann was a student in Cole's archaeological field program at the University of Chicago, and he excavated cemetery sites for Cole from 1928 through 1934.

Cole's field projects put North American archaeology on its modern footing, establishing standards for data collection and excavation, although Browman has recently questioned whether Putnam deserves credit for many of Cole's (2002) innovations. Cole has received less attention than one might expect in the history of anthropology. Cole was one of the founding members of the American Association of Physical Anthropologists and was a father figure for North American archaeologists. He was the lone archaeologist among Boas' successful students, and he transplanted the Boasian program to the University of Chicago. Griffin discusses his central role in establishing archaeology as a scientific discipline in the United States (Griffin, 1996). Krogman (1981:470) writes of Cole as a teacher: "Dr. Cole was almost 100% a disciple of Rudolf Martin's osteometry and somatometry. We who majored in physical anthropology became first 'measurers of man' in purely osteological and morphometric terms and only later in functional terms: physiological, biochemical, and genetic. But these latter were not taught to us in depth, for their relevance to physical anthropology was yet to be clarified and developed." Krogman adds that Cole sent him to study with T. Wingate Todd at Western Reserve University. Similarly, Cole sent Neumann to work with Todd in 1932–1933. Neumann's relationship with Todd was apparently problematic; e.g., he was the uncredited anonymous illustrator of the Todd public phases (see Stewart, 1979a:159). W. M. Krogman eventually served as Neumann's dissertation supervisor. We now think of Krogman as a pioneer of growth studies, but early in his career he made several contributions to physical anthropology and archaeology in the Midwest. At the time he was a partisan of the view that "the American Indian—the First American—has also emerged from a racial 'melting-pot'" (Krogman, 1941:812).

Neumann's 1937–1942 sojourn at the University of Michigan and the early years of his employment at Indiana University were supported by Eli Lilly (1885–1977), a philanthropist who was deeply interested in American archaeology. He was a founder and major supporter of the Indiana Historical Society. On the advice of the archaeologist James B. Griffin, Lilly supplied funding to Indiana University to hire Neumann, the linguist Carl Voegelin, and the archaeologist Glenn Black as faculty. He later helped establish a department of anthropology for them. Erminie Wheeler Voegelin, an ethnohistorian and specialist in Indian land claims, was hired in the history department; finally the ethnologist Harold Driver and ethnomusicologist George Herzog were recruited in anthropology (Griffin, 1972; Jones, 1976). Mr. Lilly had a project in mind for his department: the authentication of the *Walam Olum*, a purported Delaware migration legend, and in 1954 his document was published along with essays written by each of his anthropologists (Voegelin and Rafinesque, 1954). Most modern scholars regard the *Walam Olum* as a forgery, and the scholarly essays and their authors have been ridiculed (Oestreicher, 1996, 2002); however, the Delaware remain convinced of its authenticity and accept it as a true account of their

ancestry (McCutchen, 1993). In defense of the author's own institutional ancestors, the scholarly essays are best read as exercises in stating one's contradictory conclusions in a manner designed to give as little offense as possible to one's sponsor.

To the end of his life Lilly remained convinced that the *Walam Olum* would eventually prove to be authentic. At a 1974 lecture celebrating the Glenn A. Black Laboratory of Archaeology at Indiana University, Black's successor, James A. Kellar, suggested that the team had shown it to be inauthentic. Mr. Lilly rose and said that he considered "the jury to be still out" (author's notes on the lecture).

Neumann's contribution to the *Walam Olum* project is confined to a comparison of 10 male putative Munsee skulls with 20 male Seneca skulls. He finds that the Munsee (a Delaware Algonquian-speaking group) differ from the Seneca (an Iroquoian-speaking group), but that the former also differ from other presumed Algonquian speakers in ways consistent with details of the *Walam Olum* text and, one notes, equally consistent with the multiple-group model for late prehistoric peoples that Griffin favored.

In *Archaeology and Race in the American Indian*, Neumann fused race, language, and culture in a manner that reflects the culture-history interests of the archaeologists who were his mentors and colleagues. The type, not the population, was Neumann's unit of analysis, and he defined eight such types using the term variety. This effort was a refinement of the taxonomy produced by Egon von Eickstedt in the German tradition reaching back to Blumenbach. Neumann's variety is the penultimate taxon in what must be the ultimate splitter's taxonomy. The species is divided at five levels: subspecies, series, pars, varietas, and subvarietas (von Eickstedt, 1940:65). These would replace the varieties von Eickstedt proposed for North American Indians: Pacifid, Centralid, Silvid, and Margid, as well as Eskimid from the Arctic series. We can thus understand Neumann, via the Cole genealogy, as the flowering of the Boasian four-field concept in physical anthropology and connect him to Virchow and thence to liberal, monogenist German physical anthropology of the 19th century (Massin, 1996), but his taxonomic choices ally him with the polygenists via Gmelin and Haeckel to Linnaeus.

The eight varieties were Otamid, widespread and ancient but surviving in Coastal Texas and the eponymous Tohono O'Odham; Iswanid, also widespread and ancient, typified by Archaic Indian Knoll and named linking it to Catawba; Ashiwid in the Southwest; Walcolid in the Southeast extending to the Midwest in Adena and Mississippian groups and to the Pacific Coast; Lenapid in the Northeast; Inuid for the eponymous Inuit and their precursors; Deneid for Aleut and wide-ranging Athabaskans; and Lakotid for peoples of the northern Plains. Note that the Eskimo are not set apart from other New World groups and that distinct Asian connections are discussed for Otamid, Iswanid, Inuid, and Deneid. In summarizing, the author has touched on the range of each type, but

failed to convey the fluidity and complexity of Neumann's concept. This intricate picture of population movements is all the more remarkable in that at that time archaeology as a whole was in the process of purging itself of migrationism (Adams *et al.*, 1978) and that James B. Griffin played a major role in this process.

Neumann discusses the work of Hooton and Hrdlička extensively. Madisonville is Walcolid, whereas Hopewell is Lenapid. Hrdlička's unitary views of the Plains and the Northeast are dissected. Rivet is not cited, but his Pericue are assigned to Otamid. *Archaeology and Race* was published just as radiocarbon dating was becoming available, and some sense of the ferment this engendered is reflected in the frequent discussion of chronological relationships. Neumann may have felt some ambivalence about the lack of securely dated early series, and there are several interesting conjectures in this vein. One, a putative Paleoindian skeleton from Clark's Fork, Idaho, assigned to the Otamid variety, has been confirmed as ancient, if not quite as old as Neumann believed (Pennefather-O'Brien and Strezewski, 2002).

Oddly, given the importance of archaeological context and time depth in Neumann's scheme, site, population, and specimen identifiers were not salient. Indeed one of the frustrations of dealing with his output is that after 1928 he did not publish the detail that would allow one to know which crania were measured in any study. While he measured crania thoroughly—one might think obsessively—his analysis was limited to tabular presentation of means and standard deviations. He did not use the multivariate techniques that became the standard for biological distance studies during his lifetime. While he measured female skulls, his typological analyses used males exclusively. As for Hooton, the type was the unit of analysis, but unlike Hooton, an archaeological site or component was expected to yield a single type. Measurements are used only to support findings of the typologist's eye. *Archaeology and Race in the American Indian* was criticized for the subjectiveness of the types, for the arbitrariness of his choice of just 471 crania from the 10,000 he claims to have studied, and above all for his delay in publishing (Angel, 1954; Stewart and Newman, 1954). Stewart and Newman are remarkably sanguine in their account of Neumann's work, given their own investment in adaptationist models for change in skull shape (Newman, 1953, 1962). They accept much of the typology and point out improvements over Hrdlička's scheme, but argue that Iswanid and Ashiwid are not sufficiently different: "such evidence leads us to conclude again that these particular varieties have more archaeological rather than craniological validity" (Stewart and Neumann, 1954:141).

Neumann tinkered with his types over time. Varieties were renamed and subdivided chronologically into an ancestral Paleoamerind series and a descendent Mesoamerind series. Lenapid was renamed Ilinid, perhaps in response to doubts about the authenticity of the *Walam Olum*, Otamid branched off

Lenid in the east, Lakotid became Dakotid, and Walcolid became Muskogid (Neumann, 1960, 1966; H. Neumann, 1960a,b; Robbins and Neumann, 1972). A Uinicid variety for the Maya and Nootchid for the Great Basin were added, and the Deneid and Inuid varieties were put in a separate Cenoamerind series for the most recent immigrants (Neumann, 1960). Interestingly, the illustrated specimen for Neumann's Lenapid in 1952 becomes Lenid for Robbins and Neumann in 1972. In Fig. 6, the author arranges the illustrations from *Archaeology and Race in the American Indian*, plus one Illinid illustration from Robbins and Neumann, to illustrate this scheme.

Neumann channeled most of his graduate students into craniometric dissertation projects aimed at testing details of his typology. He asked that his students work with measurements Neumann himself had taken as part of his dissertation project and insisted that he measure any new material side by side with the student. His students Constance Omoto (1960), Holm Neumann (1960a,b), Kenneth Smail (1964), David Skomp (1965), James F. Metress (1971), Ralph Alexander (1971), Robert Blakely (1971, 1973), Louise Robbins (1964, 1968; Robbins and Neumann, 1972), Elizabeth Glenn (1965, 1974), and Judith Droessler (1975) published local or regional studies that evaluated boundaries between Neumann's types using modern statistical techniques. Robbins participated in adding a variety, Illinid, to the later prehistory of the Midwest (Robbins and Neumann, 1972; Neumann, 1966). Three of his students addressed Neumann's typology as a whole. Joseph Long (1966) tested the eastern North American types using multivariate analysis, a project that began as a University of Kentucky M.S. thesis directed by Neumann's close colleague, Charles Snow, and found limited support for the typology, if not for Neumann's interpretations of his types as evidence for migrations. Kenneth Smail (1964) asked whether female crania supported the model Neumann proposed for male crania and found mixed results, with females showing clearer Plains or Oneota affinity than males. He interpreted these findings as reflecting gender differences in the population structure. Matthew Brennan and W. W. Howells, in an unpublished paper meant for the ill-fated physical anthropology volume of the *Handbook of North American Indians*, used principal components analysis to discern groups among 68 series of Siberian and North American crania measured by Hrdlička. Brennan had been an undergraduate student of Neumann's, and this project was part of his graduate work under Howells at Harvard. They conclude (Brennan and Howells n.d.:33):

These results do not coincide particularly with older attempts to classify North Americans. . . . Our groups do, however, correspond quite well with varieties discerned by the Experienced [loc. cit.] G. K. Neumann. . . . On the basis of mean figures and general morphology, he examined many samples large and small (as here), and selected particular ones which seemed both representative and clearly characterized, and suggested the distribution, origin and final development of each. Here we approach

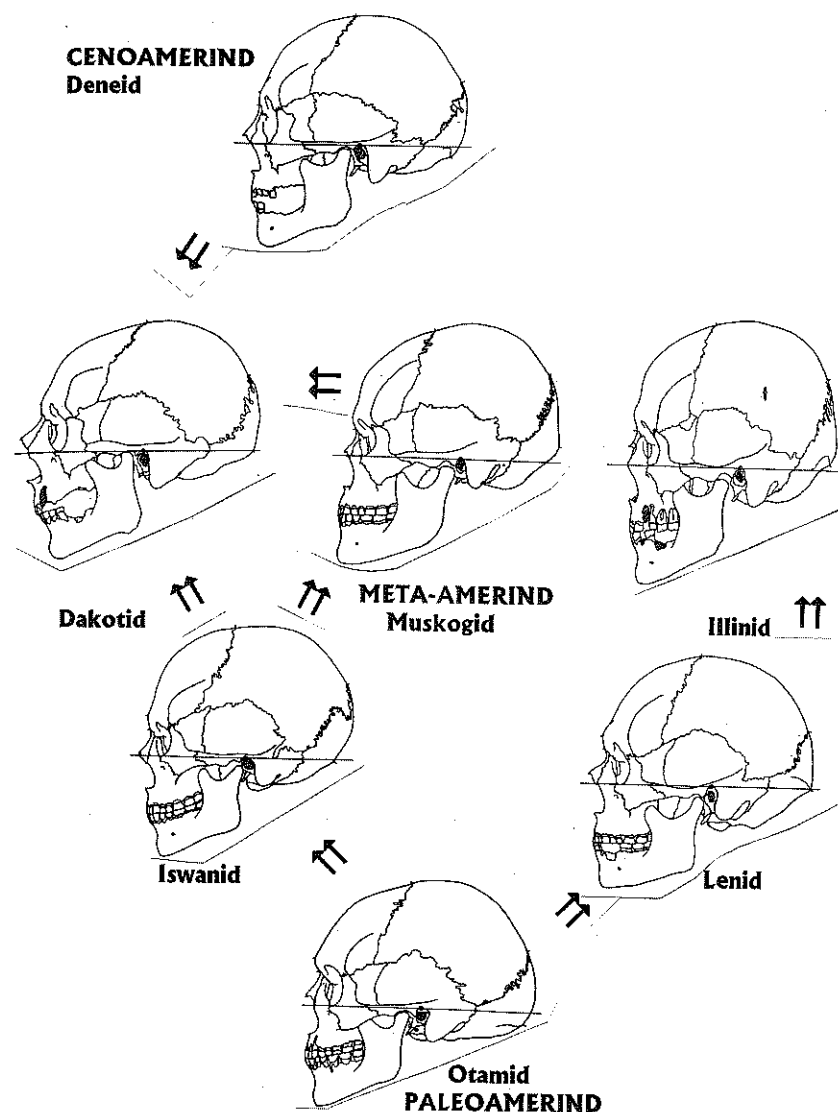


Figure 6 Neumann's varieties arranged to correspond to the evolutionary scenario proposed by Neumann and Robbins (Neumann, 1952; Robbins and Neumann, 1972).

similar series from the other direction, letting groups form (the essence of the study) and then examining their characters and relationships.

Howells and Brennan found five clusters and analogized them to Neumann's Inuid, Lakotid, Deneid, and Walcolid varieties. The fifth group "General United States . . . seems to merge Neumann's Iswanid, Ashiwid, and Lenapid varieties, though not closely fitting his descriptions, especially the last" (Brennan and Howells n.d.:35). They attribute this failure to limitations of the series they analyzed and to Neumann's use of temporal distinctions. A more recent discussion of this study suggests that unrecognized cranial deformation contributed to conflating the latter varieties (Howells n.d.).

Perhaps the most widely cited of Neumann's (1942) works is his paper on types of cranial deformation. It bears an interesting relationship to his racial typology; in order to assign a group to a variety using Neumann's scheme, one had to omit deformed skulls, a major factor in the reduction of his study series from 10,000 to just 471. The deformation study is itself typological in that it assumes discontinuities among the eight types, an assumption that does not stand up to rigorous testing (Droessler, 1981). The type is communicated primarily through craniophor drawings of typical exemplars, much like the varieties in Neumann's larger study. Oddly, he chose a skull with a bipartite parietal that Putnam (1884) had published as abnormal as one of his exemplars. The problem—accounting for intentional cranial shaping as well as positional plagiocephaly—remains a vexing one in metric studies of the cranium and is still generally handled typologically.

The last of the racial typologists, Carlton S. Coon, cited Neumann's (1965) work as the authoritative bibliography on North American Indians. This is a surprising choice because Coon, a radical splitter in other regions, adhered to Hrdlička's dogma of a single migration across the Bering land bridge. Neumann's work is still cited as normal science, often in some surprising places (e.g., Wolpoff and Caspari, 1997:393, n.123; Stewart, 1981; Haskell, 1987; Ousley, 1995). There has been relatively little recent assessment of his contributions to our literature (but see Buikstra, 1979; Crawford, 1998; Griffin, 1996; Howells n.d.). Perhaps his most important role was in salvaging the human skeletal collections when Sherwood Washburn dismantled Cole's laboratory at the University of Chicago and in providing a home for skeletal collections from Gregory I. Perino's excavations for the Gilcrease Institute of American Indian History and Art. As his successor, the author is grateful.

Robert Meier (personal communication, 2004) recalls a conversation he had with Neumann in 1968: "He did ask me as we were driving to the AAPA meetings held in Michigan if I thought that the typological approach would be supplanted by the population/variation approach, and when I said that I was sure that it would, he simply shrugged and seemed not very keen to contest the statement on what he probably considered the inevitable."

XI. GÖTTERDÄMMERUNG

The end of the typological paradigm was very much in sight during the careers of Hrdlička, Hooton, Rivet, Oettinger, and Neumann. The first application of multivariate statistics to the question of American Indian races is a 1938 paper that Gerhardt von Bonin (1899–1964) and Geoffrey M. Morant (1890–1979) published in Karl Pearson's journal *Biometrika*. Neither author rated an entry in Spencer's *History of Physical Anthropology: An Encyclopedia* (1997c), an oversight that speaks to the unfortunate provincial biases of American physical anthropology. When the paper was written, von Bonin was a neuroanatomist at University of Illinois, Chicago, and participated in Fay Cooper Cole's circle. Morant spent a long career at the Galton laboratory and was a prolific contributor to the literature on anthropometry and craniometry.

Their paper applies Pearson's coefficient of racial likeness to data from Hrdlička's *Catalogue of Crania* and Hooton's *Indians of Pecos Pueblo* to comparisons among American Indian series and to comparisons with Asian and Eskimo series. While the language is still typological, the analysis is a biological distance answer to the question of New World affinities. Some highlights include the discovery that Hrdlička's Kentucky Algonkin differed markedly from other Algonkin and Iroquois series. One would now point out that the Indian Knoll series is archaic, several thousand years older than the others (see Neumann, 1952), and that its linguistic affiliations are a surmise at best. Von Bonin and Morant found that it resembled a Japanese series among those included in the larger analysis. California crania were found to differ from other U.S. series, and "the Pecos Pueblo series was not included in the second group because its standard deviations are obviously peculiar . . . its peculiarity may be due either to the fact that the measurements selected because they were believed to be unaffected by artificial deformation were not uninfluenced by this disturbing factor, or to the fact that the population represented was racially more heterogenous than all the others" (von Bonin and Morant, 1938:124). Some California crania were linked to Ainu and other Japanese series. "A surprising diversity is found among the Indian populations of the country. . . . On this account it will be necessary to have considerably more material than that available at present to reveal their interrelationships in a completely satisfactory way" (von Bonin and Morant, 1938:127).

An appendix to the paper analyzes Neumann's data from Cole's excavations in Fulton County, Illinois (Neumann, 1937), and concludes that "the total series must hence be supposed racially heterogenous" (von Bonin and Morant, 1938:128) and fairly distinct from all other groups included in their study except Algonkin East-Central. Because the series includes Archaic, Early, Middle, and Late Woodland and Mississippian components, the heterogeneity is hardly surprising. Neumann had not yet developed his typology in 1937, and his analysis linked the earlier components to Hooton's Pseudo-Australoids.

Later Neumann (1952) would assign the chronological components variously to his Otamid, Lenapid, and Walcolid varieties.

Hrdlička actively resisted statistical innovations as editor of his journal. Hooton wrote of his statistical objectivity with obvious pride, but ignored the first studies in the new biometric paradigm. His later work does not cite von Bonin and Morant. Neumann (1952) cited them, but he discussed only their Indian Knoll and Eskimo results. He ignored the appendix reanalyzing his own work and made no mention of the paper's statistical advances. In contrast, von Eickstedt (1940) devoted several pages to von Bonin and Morant and reproduced their graphics. Both *Biometrika* and *Die Forschung am Menschen* are available in Indiana University's library, but there is neither evidence that Neumann used them nor that he encouraged his students to do so. He cited and taught from von Eickstedt's (1937) earlier *Rassenkunde und Rassengeschichte der Menschheit*, a work not available here. Perhaps Neumann did not know that von Bonin and Morant had reanalyzed his Fulton County data. Perhaps he was unready to face the paradigm change. It was left to Hooton's student and successor at Harvard, W.W. Howells, to champion the biometric paradigm in the United States. Howells begins an early foray (using data provided by Morant) thus:

It is surprising that the natural variation in recent human head form—and let us consider particularly the cephalic index—remains a generally uncomprehended phenomenon. Many of the functional explanations offered can only be called fantastic today; and in general these, and phylogenetic explanations as well, fail to give an answer to the really notable differences in this prominent characteristic, especially as between populations of the same racial stock such as the European. Even extreme forms, such as that of the most long-headed Eskimo groups, have not been given any satisfactory explanation, in spite of some celebrated discussion. (Howells, 1957:19)

The eclipse of the typological concept had begun.

XII. WHERE HAVE WE BEEN?

In a forum very different from this one, Adam Gopnik (2000) contrasted “sizzlist” histories with “steakist” histories. “Sizzlist” histories are written from the perspective of social constructionism and address various contemporary social agendas as means of illuminating the past. In contrast, “steakist” histories are written from a technical perspective and emphasize, to use a concept from the vocabulary of anthropology, processual explanation. Of the former, Gopnik (2000) writes: “The trouble with this kind of reading . . . is that it vastly underestimates the difficulty of doing things as opposed to thinking about them.” The latter are what historians often label—perjoratively—as “insider histories,” and they are prone to positivistic bias. This essay is an insider history and it has focused on the craft of doing typology.

Measurement in the work of both Blumenbach and Morton serves largely as a descriptive tool, and detailed analysis in each researcher's work is confined to a single variable. Variability is unimportant, and the approach is primarily one of classification. Variability becomes the important focus among the late 19th-century practitioners of the typological paradigm. However, the uses of measurement from these early efforts through the mid-20th century are curiously limited and secondary to the definition of types or varieties. As Andrew Lang may have quipped about politicians, they used “statistics as a drunken man uses lampposts—for support rather than for illumination” (Ratcliffe, 2000).

The typological era was anything but monolithic in its paradigm. There was lively controversy over the origins of North American Indians that is certainly not settled today, as the contributions to this volume on morphometrics and mitochondrial DNA witness. There was remarkable disagreement about many issues. Was the unit of analysis the individual, the population, the site, or the type? Should both male and female crania be evaluated? Should one exclude deformed skulls? If so, what was the appropriate threshold?

The typological paradigm did, however, set the rules of the game. There was a shared sense of what needed to be measured and of shared methods, thanks to the craniometric conferences at the turn of the century. The typologists shared collections, and the 20th-century figures discussed here even shared forms for collecting craniometric data. For example, Neumann used Harvard University/Peabody Museum craniometric data forms, and Snow's and Angel's forms are only slightly modified versions of the Harvard model.

The typological paradigm had certain advantages we may have difficulty appreciating: one could type a fragmentary or immature skull, or a small series that cannot be evaluated using biological distance techniques. It is to that extent inappropriate to expect a morphometric study to validate a typological one given the same data base because the statistical requirements for sample size and preservation are such that efforts such as Long's are compromised at the outset.

Early 20th-century physical anthropology was a very small field. Its practitioners knew one another better and corresponded more extensively than we do today. Teaching methods and research methods were widely shared. For example, Neumann taught a version of Fay Cooper Cole's excavation manual throughout his career, and among his legacies to his department was a file drawer full of 19-page course handouts on Hooton's racial taxonomy from *Up from the Ape*. Paul Gebhard's notes from Hooton's 1948 course in physical anthropology at Harvard show that Hooton returned the compliment. His students read a preliminary summary of Neumann's dissertation project that included a version of Neumann's eight varieties.

The grand, old-fashioned typological studies of the pre-Columbian peoples of North America failed to discover ethnic or tribal boundaries because their statistical tools were inadequate and because they had no real concept of populations

and little chronological control. On the one hand, most modern biological distance studies have been either too local (e.g., Szathmary and Ossenberg, 1978; Steadman, 2001) or too global (e.g., Howells, 1989; Brace *et al.*, 2001) to model ancient populations as cultural systems in the way that Neumann attempted. In part this is a technical limitation of the population paradigm: morphometric statistical techniques require samples orders of magnitude larger than the typologist's eye. On the other hand, the obsession with remote origins and with a concept of race as stable through time deflected the attention of the typologists from such anthropologically meaningful concepts as ethnic or tribal boundaries that have become the focus of much recent biological distance research.

Were Indians fundamentally similar or diverse? Were they closely related to one or to several peoples of the Old World? These questions are racially charged—perhaps all questions in American social life have some racial valence—but to reduce the work of the physical anthropologists who practiced the typological paradigm to mere racism is to lose its meaning. The Moundbuilder myth was a species of racism, and we should celebrate Morton for undermining it.

Is the study of race necessarily racist? There is controversy within and beyond physical anthropology. Most of us have given up the word “race” for less loaded formulations such as ‘population history’ or ‘ancestry,’ although the meaning of this trend is itself controversial [Cartmill and Brown, 2003; see Bocquet-Appel (1989) for an earlier parallel in France]. The typological paradigm rested on a concept of races as having discernible boundaries and persisting through time as bounded entities; this concept has been abandoned, but the questions that motivated typological anthropologists are still very much with us.

Massin (1996) has written of the “crisis of classical physical anthropology” in the context of German science at the beginning of the 20th century. Cranial measurements, whether taken singly or as indices, failed to differentiate races. We have seen a similar developmental sequence in North America. The extreme diversity of assumptions and race concepts in the work of Hrdlička, Hooton, Rivet, and Neumann is a symptom of this crisis. Massin and others write as if craniology had disappeared after the middle of the 20th century. Indeed, several authors with insiders' knowledge of physical anthropology adopted similar language, as if wishing it so would make what continues to constitute a major focus of research in our discipline disappear (Adams *et al.*, 1978; Armelagos and van Gerven, 2003). The crisis was resolved through the shift from the race concept to the population concept and through the introduction of multivariate statistical techniques that continue to generate detailed and rigorous accounts of the natural history of our species.

Despite Foucault's argument that biology replaced natural history in the mid-19th century (Larson, 1994), natural history persists to the present day as an organizing concept in anthropology (cf. Cavalli-Sforza, 1997). All of the work reviewed here is natural history. If that paradigm is an increasingly contested

one in modern anthropology—witness the schisms at Connecticut and Berkeley, among others—it continues to be a richly productive one, and it lies at the heart of Boasian, or four-field, anthropology. Foucault has emphasized institutions in the rise of natural history and its replacement by specialized disciplines, and most anthropologists writing about our history have likewise emphasized the role of institutions—departments, associations, journals—in professionalization. The building of collections that were publicly held, properly curated, and accessible to researchers was an equally important condition for professionalization (see Farber, 1982). Everyone whose work the author has reviewed contributed to building collections and relied on the collections and data of his predecessors. As we witness the wholesale destruction of these resources through repatriation, we must insist on the importance of study and restudy in our science (Buikstra and Gordon, 1981).

Where did the Indians come from? How diverse are they, and how is that diversity related to their origins? How is their biological variability related to linguistic, cultural, and ecological systems in the New World? Twenty-first-century answers to these questions await us. Let us hope that adequate collections will remain to permit these studies.

ACKNOWLEDGMENTS

I write this essay as a nonspecialist in the question of American Indian origins. I have never published a biological distance study, and I spent much of my early career trying to distance myself from Georg Neumann's brand of anthropology at my home institution. The task of documenting our collections for NAGPRA compliance required me to read Neumann's work carefully and to understand it in its historical context. The task has deepened my respect for him and his contemporaries. My visits to Brazil and to Museo Nacional and Museo do Homen Americano helped me approach Rivet and his critique of the Bering Strait dogma that was central to my training with an open mind, as well as a forum to present a discussion of Morton that is the basis for a portion of this chapter. I thank the Fulbright Foundation and CNPq for their support. My colleagues Robert Meier and Paul Gebhard provided helpful comments.