

Networking Ohio valley archaeology in the nineteenth century

by

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for the degree of Doctor of Philosophy,

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Abstract

In the nineteenth century, the Ohio valley contained dense concentrations of spectacular earthwork monuments and other sites attributed to the mysterious “moundbuilders.” For North American ethnologists, positioning the moundbuilders within accounts of the pre-Columbian peopling of the New World was seen as a fundamental theoretical problem. At the same time, regions such as southern Ohio witnessed the accelerating dispersal and loss of archaeological data to development, agriculture, and increasing numbers of hobbyist archaeologists, relic-hunters, and others. This dissertation situates nineteenth century Ohio valley archaeology within a broader context of ethnological science. More importantly, it views the history of this archaeological work as a function of centralization. Efforts toward this goal became most pronounced in the 1880s through the involvement of the Smithsonian Institution’s Bureau of Ethnology and the Peabody Museum of American Archaeology and Ethnology. Centralization was not at all a straightforward sort of process. It involved establishing complex networks of associations linking field spaces with institutional centers. Viewing Ohio valley archaeology in terms of centralization clarifies the effects of institutions such as the Smithsonian and the Peabody upon archaeological practice, and it allows for a broad understanding of the nature of fieldwork in the nineteenth century context. This perspective helps make sense of the actions of individuals within the networks, and it explains the sometimes intense inter-institutional competition underlying the Smithsonian and Peabody efforts. It also suggests that the production of archaeological knowledge cannot be disentangled from the networks facilitating centralization.

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Chapter 1.

Native, American:

Mounds, archaeology and ethnology in the early nineteenth century.

1.1 Introduction: The mounds and earthworks of prehistoric America.

Many North Americans today are entirely unaware that tens of thousands of spectacular prehistoric mounds and earthworks once covered a vast portion of the continent. The monuments were the visible remnants of sophisticated indigenous societies that functioned throughout the greater Mississippi River valley for nearly two millennia, societies from which many modern Native Americans have descended. Very sadly, few of these remains have survived the ravages of post-colonial development.

In the early decades of the nineteenth century, after the territory west of the Appalachians had been opened for Euro-American settlement, many of the newcomers became very intrigued by the mounds and earthworks and some rather interesting theories about the sites began to take hold. Some people thought the structures indicated an ancient American history rivaling that of Celtic Britain, Egypt, or even Greece. Although historians and other commentators disagree as to how it happened, what essentially emerged was a conceptual disconnect between the moundbuilders and modern Indians.¹

¹ Especially comprehensive in detailing the emergence of the “moundbuilder myth” are: Robert Silverberg, Mound Builders of Ancient America: The Archaeology of a Myth (Athens, Ohio: Ohio University Press, 1968), chapters 2 and 3; Thomas Tax, The Development of American Archaeology 1800-1879, diss., University of Chicago, 1973 (Ann Arbor: UMI 1973. T24741), chapters 2-4; Stephen Williams, Fantastic Archaeology: The Wild Side of North American Prehistory (Philadelphia: University of Pennsylvania Press, 1991), chapter 2; Barbara Alice Mann, Native Americans, Archaeologists and the Mounds (New York: Peter Lang, 2003). A number of other sources situate the early development of the “moundbuilder

Simply put, many thought extant Indians were too unsophisticated to have been responsible for the earthworks and therefore the moundbuilders must have been a separate and more advanced pre-Indian people.

The emergence of this view is at least partly attributable to the fact that most Indian groups had been displaced further west and so the mounds were viewed in a context that lacked a visible extant aboriginal presence. Also likely at some level is the bleaker scenario presented by historian Barbara Alice Mann in her much-needed effort at integrating a Native American perspective into the history of North American anthropology.² In her view, the period of overlap between the native and colonial presences was long enough that many historically practiced Indian traditions—such as moundbuilding—were widely enough known by white Americans. Mann points out that travel accounts by some sixteenth century European explorers documented instances of moundbuilding among living native groups, and also that native oral traditions lasting well into the nineteenth century told of the practice. According to Mann, the myth of the

myth” within a broader history of archaeology context, including: Gordon Willey and Jeremy Sabloff, A History of American Archaeology (London: Thames and Hudson, 1974), 28-48; Bruce Trigger, A History of Archaeological Thought (Cambridge: Cambridge University Press, 1989), 67-72 and 104-6; Robert Bieder Science Encounters the Indian, 1820-1880: The Early Years of American Ethnology (Norman, OK: University of Oklahoma Press, 1986), 108-114; William Stiebing, Jr., Uncovering the Past: A History of Archaeology (Oxford: Oxford University Press, 1993), 170-178. A number of archaeology and anthropology texts presenting historical background to current theories mention the emergence of the idea of a lost race of moundbuilders, including: Brian Fagan, The Great Journey: The Peopling of Ancient America (London: Thames and Hudson, 1987), 28-31; Stuart J. Fiedel, Prehistory of the Americas (Cambridge: Cambridge UP, 1992), 1-4.

² Mann, Native Americans, Archaeologists and the Mounds (New York: Peter Lang, 2003).

moundbuilders was a convenient fiction rooted in racist attitudes toward Indians that was intentionally adopted by white Americans and became accepted uncritically. Certainly, as other historians have argued, the mounds provided a heroic past that was appealing to many citizens of the new nation.³ The moundbuilders served certain kinds of functions in narrative accounts of the pre-colonial American past, and they did so largely because colonists could not avoid encountering the spectacular and awe-inspiring ruins that had been left in the Mississippi valley.⁴

Whatever the case, a concept of moundbuilders caught on and it became very important within a developing science of ethnology, something which achieved a certain disciplinary identity in the United States with the formation in 1842 of the American Ethnological Society. Ethnology was generally thought by its leading practitioners to provide the framework for the comprehensive study of mankind. It included the comparative study of the languages, customs, and physical characteristics of different groups of people. It also included the study of the distant past through archaeology. In its overall scope, then, ethnology did not differ much from what began to be called “anthropology” after the middle of the century, and the two terms were frequently used

³ Daniel Boorstin, The Americans: The National Experience (New York: Random House, 1965), 362; Stephen Williams, Fantastic Archaeology: The Wild Side of North American Prehistory (Philadelphia: University of Pennsylvania Press, 1991), 37. For an account of how archaeology in the middle east served to tell a particular kind of nationalistic history, see Nadia Abu El-Haj, Facts on the Ground: Archaeological Practice and Territorial Self-Fashioning in Israeli Society (Chicago: University of Chicago Press, 2002).

⁴ Eviatar Zerubavel talks about the mnemonic power of ruins, relics, and memorabilia in constructing accounts of the past. See, Eviatar Zerubavel, Time Maps: Collective Memory and the Social Shape of the Past (Chicago: University of Chicago Press, 2003), especially pp. 43-5.

interchangeably in the American context after this point in time.⁵ That said, precise definitions of either were few and far between.

Of course, ethnologists were not free of the biases that shaped many Americans' ideas about mounds, but by mid-century they self-consciously demarcated their accounts from others as rational and empirical—in other words, as science and not storytelling. Working out how the moundbuilders fit into broader theories about the patterns of human history and development, in fact, came to be seen as one of the central problems of American ethnology. As the moundbuilders became objects for scientific analysis, they were defined in different ways depending upon who wrote about them and what kinds of archaeological evidence was used.⁶ Eventually, the usefulness of “moundbuilders” as a clearly definable entity began to break down, forcing investigators later in the nineteenth century to re-conceptualize the analytical categories they employed to understand human

⁵ For the most part in this dissertation, I will refer to “ethnologists” rather than “anthropologists.” To trace out the exact usages and meanings of the terms “ethnology” and “anthropology” in the nineteenth century would be a major project in its own right. I will, however, frequently distinguish between “ethnologists” and “archaeologists.” Many people interested in archaeology were not necessarily interested in the synthetic theorizing done by ethnologists, though many ethnologists paid particular attention to archaeology.

⁶ For an interesting set of perspectives about the coming into being of scientific objects, see the collection of essays presented in Lorraine Daston, ed., Biographies of Scientific Objects (Chicago: University of Chicago Press, 2000). On my reading of Daston's epistemological and ontological objectives, the biographical approach serves a two-fold function: on the one hand, to think of scientific objects as having life spans forces us to historicize the ways in which scientists come to understand and conceptualize those objects; on the other hand, scientific objects regardless of how they come to be are subject to multiple characterizations depending upon who is writing about them.

diversity in the prehistoric past. As we shall also discover, what in fact made ethnology in any of its often ambiguous guises *scientific* was often a matter of intense debate and controversy, and generally revolved around the presentation of data and, more importantly, the methods by which that data was collected. This was especially so in the case of archaeological work on the mounds in southern Ohio, where the production of archaeological knowledge was entangled with the processes by which data was moved out of the field.

The mounds and earthworks themselves were distributed largely throughout the greater Mississippi valley, roughly from Pennsylvania to Kansas and the Dakotas, down to the Mississippi delta, and could also be found further to the southeast into Florida and north into Canada. The Ohio River valley was home to particularly dense concentrations of the works and became an area of great interest to nineteenth century archaeologists. To adequately describe these sites in words is a nearly impossible feat, and it would be helpful to flip ahead to Chapter 2 to get a sense of how some nineteenth century archaeologists depicted them. Figures 2.1 through 2.11 in particular give a rather astonishing impression of the size and extent of the structures, which generally appeared as complexes of linear earthen embankments in conjunction with conical mounds. As it turned out, the images reproduced in Chapter 2 proved to be very important for the course of Ohio valley archaeology, but we will get a sense of that later.

Most of the ancient works, as I mentioned, were destroyed in the nineteenth century, doomed by colonial expansion into the west. General urban development and agriculture accounted for much of the destruction. Colonial settlers established themselves in precisely the same choice locations where many of the mounds' builders had lived—

along broad transportation-friendly river valleys layered with loamy, fertile alluvial soil. Many nineteenth century towns and cities consequently sprang up literally right on top of mound and earthwork complexes. Some places such as Moundsville, West Virginia, and Circleville, Ohio, were named for the ancient works in immediate proximity. The construction of roads and railway lines connecting new urban centers further cut through many of the sites. Agriculture proved to be an exceptionally devious culprit in the destruction of mounds and earthworks, as the incremental effects of plowing and erosion had devastating accumulative consequences for the ancient sites. Farmers often did not avoid tilling over the massive structures, and several decades of cultivation and weathering could greatly reduce even the largest of works. The accelerated pace of agriculture after 1870 under Gilded Age economic policies only speeded the damage. These combined forces of development had an immense impact upon archaeology in southern Ohio, the region to which I will paying most attention throughout this dissertation. By the third quarter of the century, Ohio emerged as an economic powerhouse with numerous rapidly growing urban and industrial areas and a population of over three million people and counting. It had become a principal transportation hub linking the northeast and mid-Atlantic states to regions further west and south. Railway lines radiated from Ohio's cities and towns in all directions. Ohio also maintained vast areas of productive rural farmland, especially throughout the southern half of the state.⁷

⁷ One of the best sources I have found for general historical, geographic, economic, and other demographic data for the state of Ohio is a two volume county-by-county encyclopedic collection put together state historian Henry Howe. See, Henry Howe, Historical Collections of Ohio (Cincinnati, C.J. Krehbiel & Co. for the State of Ohio, 1904), hereafter cited as Howe, Collections. The complete subtitle of this source deserves to be cited in full: "An Encyclopedia of the State: History both general and local, geography with

In the 1880s, archaeologists from both the Peabody Museum of American Archaeology and Ethnology as well as the Bureau of Ethnology located at the Smithsonian Institution became very concerned by the loss of ancient works to the plow.

And yet, out of colonial encroachment and destruction came archaeological knowledge in a very tangible sense. One noteworthy antiquarian scholar with a strong interest in American archaeology, Samuel Haven, observed around mid-century that the restoration of peace after the War of 1812 meant that with the “interior of the country tranquilized, a lively spirit of inquiry [had] sprung up in the midst of the antiquities to be investigated.”⁸ As the stalwart librarian since 1838 for the American Antiquarian Society in Worcester, Massachusetts, Haven was not only a fixture of eastern U.S. ethnological circles, he was also in command of one of the largest collections of archaeological literature to be found in the country.⁹ Haven recognized that the growth of the “spirit of

descriptions of its counties, cities, and villages, its agricultural, manufacturing, mining and business development, sketches of eminent and interesting characters, etc., with notes of a tour over it in 1886. Illustrated by about 700 engravings. Contrasting the Ohio of 1846 with 1886-90. From drawings by the author in 1846 and photographs taken solely for it in 1886, 1887, 1888, 1889, and 1890, of cities and chief towns, public buildings, historic localities, monuments, curiosities, antiquities, portraits, maps, etc.”

Another very good source for similar information is the published proceedings of the 1888 Marietta, Ohio Centennial Celebration in the Ohio Archaeological and Historical Quarterly 2 (no. 1, June 1888), 1-242.

⁸ Samuel F. Haven, Archaeology of the United States or Sketches, Historical and Biographical, of the Progress of Information and Opinion Respecting the Vestiges of Antiquity in the United States, Smithsonian Contributions to Knowledge, vol. 8 (Washington, D.C.: Smithsonian Institution Press, 1856), 33.

⁹ Some biographical information on Haven may be found on-line at the America Antiquarian Society website. See, <http://www.americanantiquarian.org/Exhibitions/Portraits/samuelhaven.htm>. Haven’s specific

inquiry” to which he referred was driven at least as much by the forces of development as it was by pure curiosity, and he noted the pronounced increase after 1830 in the numbers of encounters with the archaeological monuments of the central west.¹⁰ As more and more of the works were encroached upon, their locations were documented.

Occasionally, some attempt at a systematic investigation was performed on individual mounds. In cases where mounds were churned, opened or ripped apart, their abundant contents spilled forth—stone tools, projectile points, decorated pottery, clay figurines and other sculptural objects, copper implements, mica ornaments, beads, and bones all emerged into the light of day, then into the hands of collectors and the curious. Gradually, over the course of the century, archaeologists became increasingly more organized as they struggled to counter the ongoing dispersal and loss of archaeological specimens and other information by centralizing it to particular institutional locations. A considerable portion of this dissertation focuses on the consolidation of archaeological networks that aided the cause of centralization during the Peabody and Smithsonian efforts of the 1880s.

1.2 Background to the emergence of ethnology in nineteenth century America.

The background of speculation regarding the natives of the Americas is, of course, directly associated with the long process of European imperialism. The various

interest in southern Ohio archaeology during the 1840s is discussed in David Meltzer, “Ephraim Squier, Edwin Davis, and the Making of an Archaeological Classic,” introduction to reprint edition of Ephraim G. Squier and Edwin H. Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press 1998 (1848)), 14-7.

¹⁰ Haven, Archaeology of the United States, 105-6.

intellectual currents and contextual factors important for understanding the broad range of developing human sciences from the early modern period through the early nineteenth century has been addressed by numerous historians and a full survey lies outside the scope of things here.¹¹ Since interpretations of the moundbuilders were for the most part inevitably linked to attitudes about extant Native Americans, it would be helpful to get at

¹¹ See, for instance: George Stocking, Victorian Anthropology (New York: The Free Press, 1987); Stocking, "From Chronology to Ethnology: James Cowles Prichard and British Anthropology, 1800-1850," introduction to reprint edition of James Cowles Prichard, Researches Into the Physical History of Man (Chicago: University of Chicago Press, 1973, originally published 1813); Stocking, "Paradigmatic traditions in the history of anthropology," in R.C. Olby, G.N. Cantor, J.R.R. Christie, and M.J.S. Hodge, ed., Companion to the History of Modern Science (London: Routledge, 1990); Annemarie De Waal Malefijt, Images of Man: A History of Anthropological Thought (New York: Knopf, 1974); Roger Smith, The Norton History of the Human Sciences (New York: W.W. Norton, 1997); Christopher Fox, Roy S. Porter, and Robert Wokler, ed., Inventing Human Science: Eighteenth Century Domains (Berkeley: University of California Press, 1995); Nicholas Jardine, "*Naturphilosophie* and the kingdoms of nature," Michael Dettelbach, "Humboldtian science," Janet Browne, "Biogeography and empire," and Michael T. Bravo, "Ethnological encounters," in N. Jardine, J.A. Secord, and E.C. Spary, Cultures of Natural History (Cambridge: Cambridge University Press, 1996); Richard Olson, "The Human Sciences," and Rob Iliffe, "Science and Voyages of Discovery," in Roy Porter, ed., The Cambridge History of Science, Volume 4: Eighteenth-Century Science (Cambridge: Cambridge University Press, 2003); Theodore M. Porter, "Genres and Objects of Social Inquiry, from the Enlightenment to 1890," Johan Heilbron, "Social Thought and Natural Science," Harry Liebersohn, "Scientific Ethnography and Travel, 1750-1850," Johnson Kent Wright, "History and Historicism," Adam Kuper, "Anthropology," and Elazar Barkan, "Race and the Social Sciences," in Theodore M. Porter and Dorothy Ross, ed., The Cambridge History of Science, Volume 7: The Modern Social Sciences (Cambridge: Cambridge University Press, 2003).

least a general sense of a range of approaches for understanding New World natives that had developed during this period of time.

A first wave of European writings on Native Americans appeared in the wake of the Spanish Conquest. Around 1600, Jesuit and Dominican missionaries in the southerly Americas considered the origins of Indians—that they could have been descended variously from Iberians, Carthaginians, Lost tribes of Israel, Canaanites, Tartars, Atlantis, the Welsh, East Asia, and so forth.¹² In the context of the Voyages of Discovery and Euro-centric attempts to make sense of the world's diversity within a *scala natura*, these explanations were not always considered far-fetched. The idea of a gradual migration from northeast Asia across a land bridge was hypothesized as early as 1590.¹³ It was helped much later by the discovery of the Bering Strait in the 1730s, although the Asian migration theory remained somewhat disputed scientific terrain through the nineteenth century (and, indeed, in certain respects, it remains so today).

After the initial imperial encounters, Native Americans, along with many of the world's non-European peoples, became objects of study by various European scholars in the late seventeenth through early nineteenth centuries. This phase corresponded to the acceleration of colonialism and to the rise of scientific expeditions by the late eighteenth century. Theologians continued to wonder if non-European peoples could be Christianized, or whether and in what manner they could be related to accounts given in the bible. Moral and political philosophers sought out elemental social units in native

¹² Williams, *Fantastic Archaeology*, 32-2; Trigger, *History of Archaeological Thought*, 67-8; Fiedel, *Prehistory*, 2.

¹³ The Jesuit missionary Jose de Acosta proposed the idea in his 1590 treatise *Historia natural y moral de las Indias*. See, Williams, *Fantastic Archaeology*, 32; Tax, *Development*, 33.

societies in order to develop naturalistic laws of social cohesion and development. Naturalists and anatomists debated the taxonomic status of human beings. Philologists sought to track the migration of human populations through the comparative study of languages. Taken as a whole, these approaches might rightly be subsumed under the category of “the natural history of man,” though this label maybe does not quite capture the full range of nuances within the developing human and social sciences. Often, too, such various studies were based on second-hand descriptions from travel or missionary accounts, and authors frequently relied upon a hierarchical chain of being for ordering their accounts of human beings throughout the world.

One popular way of thinking about the Americas was literally as a New World untainted and unadulterated by the trappings of European history and civilization and closer to some original state of nature. The notion is perhaps most directly traceable to the philosophical pronouncements of John Locke, who in 1690 wrote that “in the beginning all the world was *America*.”¹⁴ Such a position, maintained by many after Locke, made it philosophically defensible to base analyses of society in the belief that many non-European natives—such as those of America—existed in some arrested state of primitive existence, serving as a foundation for many naturalistic studies of social development. Here were the roots of what would come to be known as the “comparative method” of the nineteenth century, the basis of analogy between living and prehistoric “primitives.” But, here, too, were roots of polygenist ways of thinking about human

¹⁴ Quoted in Roger Smith, The Norton History of the Human Sciences (New York: Norton, 1997), 170, although all of his chapter 5 addresses the context of Locke’s philosophy for the developing human sciences. Additionally, see Alice Beck Kehoe, The Land of Prehistory: A Critical History of American Archaeology (New York: Routledge, 1998), chapter 3.

diversity that would become much more dominant toward the middle of the nineteenth century: the state of nature—whatever that was—may have been how each human group began its existence, but this did not mean all humanity started off as a single undiversified population. An alternative explanation was that the various groups were distinct from the beginning and developed at different rates.

Other approaches, however, emphasized the potentially degenerative effects of environment and interpreted North American native peoples in this light—they had migrated across the Bering Strait into the western hemisphere long ago, deteriorating physically and mentally in the process. The degenerationist view acquired substantial scientific underpinning through the natural history program of French naturalist Georges-Louis Leclerc, comte de Buffon in the late eighteenth century. Buffon emphasized the degrading effects of the environment on all living beings as they migrated about the world.¹⁵ Buffon's views directly evoked reactionary research by the circle of scholars associated with Thomas Jefferson, members of which believed human history to have an inherently progressive trajectory.¹⁶

Given the differences in approach to theorizing about non-European people that saw them either as noble or degenerate savages, it is not surprising that attempts by naturalists at taxonomic classifications of human beings should have been widely divergent. Increasing colonial encounters with both natives as well as unfamiliar forms of “higher” apes certainly shaped discussions along these lines in the late eighteenth and early nineteenth centuries. Buffon, Swedish naturalist Carl Linnaeus, French naturalists

¹⁵ Peter Bowler, *Evolution: The History of an Idea* (Berkeley: University of California Press, 2003), 75-80; Haven, *Archaeology of the United States*, 94.

¹⁶ Bieder, *Science Encounters the Indian*, 4-6.

Georges Cuvier and Jean-Baptiste Lamarck, and German naturalist Johann Blumenbach, among others, all differed greatly in their opinions on the taxonomic status of man, especially whether mankind should be grouped into one species or many, and what physical characters should be considered in order to determine human different types.¹⁷

As for the duration of human existence on the earth, until the second half of the nineteenth century, humanity was thought to be no more than a few thousands of years old. A movement of German historical criticism posed serious objections to literal interpretations of the bible, but until just after mid-century, no widely agreed-upon evidence existed to suggest that human history could be much more than something on the order of several thousands of years at most. Historians have pointed out that the excavations at Brixham Cave in England in the late 1850s, conducted under the auspices of the Geological Society of London, proved something of a turning point. Afterward, enough of a consensus had been reached by leading British geologists and archaeologists that the idea of a glacial or pre-glacial human presence became considerably more acceptable.¹⁸ Before this episode the possibility of extreme human antiquity was not taken very seriously.

The biblical time frame was especially important within the philological tradition, where the comparative study of languages rested on a biblical, monogenist framework. Philology addressed the geographic and temporal diversification of languages from a

¹⁷ John C. Greene, The Death of Adam: Evolution and Its Impact on Western Thought (New York: Mentor Books, 1961), 177-202.

¹⁸ Donald Grayson, The Establishment of Human Antiquity (New York: Academic Press, 1983); A. Bowdoin van Riper, Men Among the Mammoths: Victorian Science and the Discovery of Prehistory (Chicago: University of Chicago Press, 1993).

common source, and it provided the core methodological framework for what would come to be known as a more comprehensive science of ethnology by the early 1840s. The term “ethnology” itself derives from the pioneering work of English Quaker James Cowles Prichard, whose success at synthesizing disparate approaches to the historical study of man into a single methodological framework proved of lasting influence.¹⁹ For Prichard, ethnology was based on the essential unity of mankind, and the task of ethnology was “to show how all the modern tribes and nations of men might have been derived from one family, and so far as possible to trace them back historically to a single source.”²⁰ Extensive knowledge of the present geographic distribution of the range of human traits, with language structures held above all, facilitated this goal, and allowed one to infer the historical patterns of migration and diffusion that had occurred. Archaeology was critical as it supplied the empirical support for historical patterns. Although the outlines of ethnology had been implicit in Prichard’s work from at least 1813, his more explicit formulation of the science was in large measure a response to what he perceived as the distasteful growth of polygenist anatomical science. For Prichard, biblically-rooted monogenism not only satisfied moral imperatives, it served as a stable methodological tool by subsuming human historical development into a single process of diversification from one original source.

One person with whom Prichard corresponded was the Swiss-American philologist Albert Gallatin, a man who deserves considerable credit for establishing ethnology in the United States. Gallatin arrived in America in 1780 and worked his way into the upper

¹⁹ Stocking, “From Chronology to Ethnology,” especially pp. lxxiv-lxxvi.

²⁰ Stocking, “From Chronology to Ethnology,” p. xciv.

levels of American politics by becoming secretary of the treasury in the administrations of both Thomas Jefferson and James Madison, where he was chiefly responsible for the systematization of the government's finances. He later served as state diplomat to both Russia and France.²¹ He was friends with the Prussian explorer, naturalist and geographer Alexander von Humboldt, from whom he developed an intense interest in the geographic distribution of languages, and of American Indian languages in particular.²² Like Prichard, Gallatin rigidly adhered to a belief in the underlying unity of mankind and was a staunch monogenist. Unlike Prichard, however, Gallatin was never very interested in tracing commonalities back to an ultimate biblical origin. His belief in unity came from other sources, namely an overarching faith in universal human reason as expressed through language, something Gallatin felt was increasingly confirmed in his comparative studies of the highly diversified range of Native American languages.²³ Although he was most interested to understand the historical relationships between these languages, his views on social development were informed by a belief in the inherently progressive direction of human history. Environments could either stimulate or retard development; accordingly, under the right set of conditions, agrarianism would emerge, forming the basis for civilization.²⁴ In these respects, Gallatin was a true Jeffersonian. Gallatin, too, saw philology as the methodological cornerstone for a more comprehensive comparative science of man, and in 1842 founded the American Ethnological Society in New York as

²¹ Bieder, Science Encounters the Indian, 16-24.

²² John C. Greene, American Science in the Age of Jefferson (Ames, Iowa: Iowa State University Press, 1984), 403-4.

²³ Bieder, Science Encounters the Indian, 33.

²⁴ Bieder, Science Encounters the Indian, 16-54.

a place where this diverse range of approaches could come together. Importantly, the AES provided a vital forum for ethnological discussions within which the topic of the mounds rose to prominence. Any accounts of the historical distribution of aboriginal Americans had to include the moundbuilders.

Later, I will say more about Gallatin, the AES, and one of its other well-known members, an anatomist named Samuel Morton. In the meantime, a number of other considerations need to be addressed, as the AES was not the first venue for addressing questions about the origins of American Indians in the New World, nor was it the only organization in the United States to facilitate discussions about the mounds. In fact, by 1842, the moundbuilders were fairly well-known to many Euro-Americans, not just ethnologists.

1.3 Organizing archaeology in the early United States.

Of the early colonial learned associations, probably none was more important than Philadelphia's American Philosophical Society, founded in 1769. Its membership, including Thomas Jefferson, was a roll-call of the leading American intellectuals of the era, and they were very interested in the topic of American Indian origins, among many others.²⁵ After excavating a mound on his Virginia estate in 1784 and reporting on the work in his Notes on the State of Virginia (1780-1), Jefferson influenced the APS to form

²⁵ One of the best available accounts of American science in the early national period and of the immense influence of the Jeffersonian circle is Greene, American Science, especially chapters 12-14. See also Tax, Development, 54.

a Standing Committee on Antiquities in the 1790s.²⁶ According to Greene, in fact, the “decade of 1780s,” which corresponded with the early phase of western colonial expansion, “was a banner one for American publications on Indian mounds and related topics.”²⁷ In 1799, Jefferson drafted and distributed a circular with guidelines for the collection of drawings, descriptions, and contents of western mounds and earthworks that had mildly successful results. Jefferson’s influence extended to researches into Indian languages as well, and a number of APS affiliates took up the call over the next couple of decades, including Benjamin Smith Barton, Peter Du Ponceau, and Gallatin.²⁸ The Jeffersonian circle was thoroughly influenced by an Enlightenment optimism in human progress, and they were eager to demonstrate the quality of America and its science.²⁹ The study of America’s antiquities and native peoples was central to the Jeffersonians’ vision of national science. These men also strongly disagreed with degenerationist views by followers of Buffon, since degenerationism had negative connotations for all living

²⁶ Greene, America Science, chapters 1-2, especially p. 29, pp. 41-6. As Greene portrays the situation, in fact, Jefferson’s influence on the APS after Benjamin Franklin’s death in 1790 was vast, and it served as a primary venue for his vision of a national science. According to Greene, the comprehensive Notes on the State of Virginia “set the tone and foreshadowed the content of much of American science for the next three decades” (p. 29).

²⁷ Greene, American Science, 345. Greene enumerates some of these on pp. 346-50.

²⁸ Greene, American Science, chapter 14; Bieder, Science Encounters the Indian, chapter 2; Tax, Development, 50-2 and 56-7; Williams, Fantastic Archaeology, 34-5; Trigger, 69 and 105-6; Willey and Sabloff, 36-8; Silverberg, 44.

²⁹ Greene, American Science, chapters 1-2. Also see Williams, Fantastic Archaeology, 37-8.

immigrants to North America.³⁰ Here was a philosophical dilemma—Jefferson, Barton, and Gallatin all believed Indians came to America in a single migration from Asia via the Bering land bridge, but linguistic research by Barton and Gallatin, combined with archaeological evidence of the moundbuilders, indicated to them that the Indians might well have degenerated significantly. These men therefore stressed the need for ongoing investigations of the nation's natives and its antiquities.

The APS was greatly aided in its work along these lines by its close association with Charles Willson Peale's Philadelphia Museum, an institution which served its own important role in facilitating archaeology.³¹ Peale's museum, formed in 1786, was the central repository for many of the early government explorations of the west such as the Lewis and Clark expedition. As such, it acted as one of the most significant early conduits for the transmission of antiquities and natural history specimens out of the trans-Appalachian west to the east where these materials could be accessed for study.³² It also supported the publication of the major American natural history texts of the day. In 1812, the Academy of Natural Sciences formed as an offshoot of the museum, a group which also fostered connections to numerous individuals and upstart organizations in the west. Peale himself participated in ethnographic research during western expeditions, and many artifacts came back to rest in Philadelphia, where they whetted archaeological appetites. Whether Peale's museum served as a model to be emulated or simply embodied a broad

³⁰ Tax, Development, 53; Charlotte Porter, The Eagle's Nest: Natural History and American Ideals, 1812-1842 (Alabama: University of Alabama Press, 1986), chapter 1; Sally Gregory Kohlstedt, "Curiosities and Cabinets: Natural History Museums and Education on the Antebellum Campus," Isis 79 (1988), 410.

³¹ Porter, The Eagle's Nest, chapters 2-5; Greene American Science, 52-7.

³² Greene, American Science, 195-217.

growing interest in natural history, numerous smaller scale museums devoted to natural history sprouted up in connection with eastern colleges, where the didactic value of objects—including archaeological specimens—took on a new significance in science curricula.³³

As Thomas Tax reports, the Jeffersonian influence at the APS also directly resulted in the formation of a number of antiquarian, historical, and scientific organizations in the east in which the topic of the mounds was discussed.³⁴ Mostly, these were fairly small-scale sorts of organizations, but at least one in this early period had a national vision, with membership elected from every state.³⁵ This was the American Antiquarian Society, which was founded by the patriot printer Isaiah Thomas in 1812 in Boston, and moved to Worcester, Massachusetts in 1815. According to Thomas, the purpose of the organization was to "encourage the collection and preservation of the Antiquities of our country, and of curious and valuable productions in Art and Nature [that] have a tendency to enlarge the sphere of human knowledge."³⁶ After somewhat of a decline in the activities of the American Philosophical Society beginning around 1818, this group operated as a principal outlet for historical, archaeological, and philological studies for a number of years.³⁷ As a social catalyst, Thomas was in the thick of the ethnological scene, as was the Society's librarian Samuel Haven a little later on, for some time before the Society

³³ Kohlstedt, "Curiosities," 408-415.

³⁴ Tax, Development, 132.

³⁵ Greene, American Science, 356.

³⁶ As quoted in "A Brief History of the American Antiquarian Society," found at the website of the AAS. See, <http://www.americanantiquarian.org/briefhistory.htm>.

³⁷ Tax, Development, 132.

refocused its purpose to the collections of documents of colonial history after mid-century.

As in the east, so went the settled region of the country west of the Appalachians, though on a much smaller scale. After around 1820, numerous learned societies sprang up in provincial western towns and cities during a period of rapid intellectual growth. Archaeological interests were well represented, especially in settled regions where mounds could be found in abundance such as in the Ohio valley.³⁸ Nonetheless, many organizations found it difficult to maintain a long-term membership for support, and few proved very durable until much later in the nineteenth century. Even in those that enjoyed at least slightly more than a purely ephemeral existence, such as the Western Museum Society of Cincinnati or the Historical and Philosophical Society of Columbus, individuals sometimes struggled to both maintain some status in relation to eastern organizations, while simultaneously asserting their independence from them.³⁹

Take, for example, the case of the physician Daniel Drake, who founded the Western Museum Society in Cincinnati in 1818, as a way of promoting the city as *the* intellectual and cultural capital of the west.⁴⁰ Cincinnati, located at the confluence of the Little Miami and Ohio Rivers, was a thriving young city sitting squarely amidst some of the densest concentrations of moundbuilder sites found anywhere. Drake's museum was the first repository for artifacts in the west, and he himself undertook reasonably cautious investigations and interpretations of mounds and moundbuilder specimens. He also actively networked with a number of individuals throughout Ohio who were doing

³⁸ Tax, Development, 137.

³⁹ Tax, Development, 139 and 144-7.

⁴⁰ Tax, Development, 138.

archaeological investigations and building specimen collections of their own.⁴¹ Drake's influence was such that, according to Tax, it was simply perceived as disloyal to send artifacts out of the west after the formation of his Western Museum.⁴² And yet, the museum was a rather short-lived endeavor. Drake simply could not generate and maintain enough support to keep it going at his envisioned scale.

One of Drake's archaeological correspondents was a man named Caleb Atwater, the postmaster of Circleville, Ohio.⁴³ Atwater had established an active correspondence with the American Antiquarian Society, and in 1819 Thomas commissioned him to produce an extended study of the Ohio mounds. Atwater himself had moved from Massachusetts to Circleville, where his position as postmaster allowed him to carry out extensive series of correspondence free of charge.⁴⁴ The result was a lengthy 162-page study, entitled "Description of the Antiquities Discovered in the State of Ohio and Other Western States," which was published in the first (1820) volume Archaeological Americana, the journal of the American Antiquarian Society.⁴⁵ Here, Atwater presented a number of maps and descriptions of many of the ancient works that he and others had surveyed, and he discussed numerous artifacts as well as skulls. The publication derived from Atwater's

⁴¹ Greene, American Science, 359-60. Drake also belonged to both the American Philosophical Society and the American Antiquarian Society. See, Daniel Drake, An Anniversary Discourse, on the State and Prospects of the Western Museum Society; Delivered by Appointment, in the Chapel of the Cincinnati College, June 10th 1820, on the Opening of the Museum (Cincinnati: Western Museum Society, 1820).

⁴² Tax, Development, 138.

⁴³ Greene, American Science, 359-60.

⁴⁴ Greene, American Science, 358.

⁴⁵ Greene, American Science, 362-75.

own knowledge and experience of mounds, and it synthesized a fairly broad range of additional information supplied by many of Atwater's archaeological colleagues—including Drake—who were also doing similar work. Atwater's "Description" was more than a straight description of the mounds and moundbuilder specimens, however. It also presented more speculative views about the origins of the moundbuilders. After a lengthy discourse on Old World history, he concluded that the moundbuilders were not related to American Indians, but were descended from the "Hindoos" of India. Evidence for this conclusion hinged largely on a single three-headed idol found in Tennessee.⁴⁶ Atwater, however, additionally drew upon established Orientalist scholarship of his day in seeking cultural similarities.

Some historians have portrayed Atwater's interpretive work as being rather methodologically naive.⁴⁷ Certainly, when weighed against later standards this is true. Other historians have presented Atwater's scholarship in a more positive light, and many American archaeological and ethnological scholars of the mid-nineteenth century saw it as a valuable contribution.⁴⁸ Despite its shortcomings and eventual criticisms, Atwater's study was seen by Gallatin and other nineteenth century archaeologists as the first synthetic, general treatment of ancient monuments, having given the best "conception of the number, magnitude, and more obvious characteristics" of mounds and earthworks to date.⁴⁹ Even Alexander von Humboldt took a great interest in it, and cited it within in his

⁴⁶ Williams, Fantastic Archaeology, 39-42.

⁴⁷ Williams, Fantastic Archaeology, 39-42.

⁴⁸ Greene, American Science, 362-75.

⁴⁹ Ephraim G. Squier and Edwin H. Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press 1998 (1848)), xxxiii; also, Albert Gallatin to Joseph Henry, June 16,

own studies.⁵⁰ At the least, Atwater's "Description" represented an honest attempt to present to a large audience all at once the potential sheer scale of an ancient moundbuilder civilization. Within two decades, however, Atwater's work would come to be seen by most ethnologists and archaeologists as a model in need of considerable revision, loaded with fanciful claims based on faulty or sloppily gathered evidence, even if it had been a good start.

Atwater and Drake were only two of many individuals who lived in Ohio mound territory and who were actively engaged in creating a networked community of archaeological scholars in the early decades of the nineteenth century. At this point in time, the means for maintaining these networks relied heavily upon the willful activities of motivated individuals such as Drake, although, as we have seen, support from eastern organizations such as the American Antiquarian Society went a long way toward facilitating research in the west. However, none of the people mentioned thus far could pursue archaeological work as a full-time activity. Investigating a mound was time-consuming and relatively labor-intensive hobby. This limited participation to those with the time and money to do so. Even then, these individuals—Drake, Atwater, Edwin Hamilton Davis, about whom we will learn much more in the next chapter, and many others—relied on a considerable amount of field help that went completely unacknowledged. Hobbyist archaeologists needed help digging, and they needed to negotiate access to sites and artifact collection. As I pointed out at the start of this chapter, farmers became extremely important for archaeology within this context.

1847 in Marc Rothenberg, ed., The Papers of Joseph Henry, Volume 7: January 1847-December 1849, The Smithsonian Years (Washington, D.C.: Smithsonian Institution Press, 1996), 116-7.

⁵⁰ Greene, American Science, 369.

Farmers' very livelihood often resulted in plowing up specimens and the accumulation of large collections. The Albert Gallatins and Isaiah Thomases of the east would have had precious little archaeological data to discuss without the productivity of farmers at the other end. As we shall see in later chapters, southern Ohio farmers continued to be essential contacts for later archaeologists coming into the area looking to gain access to sites.

1.4 Moundbuilders and Indians in political, religious, and popular discourse.

Developing networks of hobbyist archaeologists in western states such as Ohio certainly brought greater attention to the moundbuilders. But these mysterious, lost people became famous in the first half of the nineteenth century for many other reasons, too. The moundbuilders seemed “mysterious” to Euro-Americans by virtue of comparison to the present condition of American Indians. By the 1840s, most Indian tribes had suffered horribly under the crushing weight of American western expansion and the 1830 Indian Removal Act of the Andrew Jackson administration. The end of the War of 1812 kicked off a second phase of western expansion, a period which was characterized by the rapid pace of Euro-American settlement and ultimately resulted in the “Manifest Destiny” ideology of the Jacksonian era. The stage was set for decades of ongoing military conflict in the far west as well as for a variety of responses to the “Indian problem.”

Martial tensions over frontier land between whites and Indians certainly contributed to strong anti-Indian sentiment and hostility. Such attitudes, however, were not uniformly shared by all white Americans. A number of politicians and religious reformers at least did not wish to see Indians treated inhumanely, believing that they could be civilized,

Christianized and assimilated into American society. Earlier in the century, Gallatin had advised Jefferson on that president's Indian policies, espousing the notion that education would lead Indians to the agrarian lifestyle that formed the basis of civilization.⁵¹ In 1824, the Jeffersonian influence figured into the creation of the American Society for Promoting the Civilization and General Improvement of the Indian Tribes in the United States.⁵² The stated purpose of the organization was "to secure for these tribes instruction in all branches of knowledge, suited to their capacities and condition," and it called for ongoing studies into the "origin, history, memorials, antiquities, traditions, governments, customs, manners, laws, languages, and religions" in order to facilitate this process.⁵³ The Society was set up under the direct patronage of U.S. Presidents, and listed John Adams, Jefferson, and James Madison as patrons. Although Indians hardly reaped any tangible benefits from such thinking, their situation only became more desperate under the decidedly unenlightened policies of Andrew Jackson, whom Gallatin personally despised. Other politicians disagreed with the course of action that the Jackson administration had chosen in clearing the west and south for white settlement. Congressman Edward Everett, for one, condemned federal Indian removal policies as early as 1828, going so far as to accuse the government of repeatedly and blatantly violating treaties.⁵⁴

⁵¹ Bieder, Science Encounters the Indian, 20-2.

⁵² First Annual Report of the American Society for Promoting the Civilization and General Improvement of the Indian Tribes in the United States (Millwood, NY: Kraus Reprints, 1975 (1824)), hereafter cited as Report of the American Society for the Improvement of Indians.

⁵³ Report of the American Society for the Improvement of Indians, 4.

⁵⁴ Edwin Scott Gaustad, A Religious History of America (San Francisco: Harper Collins, 1990), 161.

A great deal of concern for the plight of the Indian came from religious quarters, though, again, this was a sympathy to be qualified. The opening of the west went hand-in-hand with the broad religious movement in America known as the “Second Great Awakening.” This was the era of the Methodist circuit rider and the Baptist farmer-preacher, all seeking to have a part in civilizing and redeeming primarily the intemperate and vice-ridden frontier pioneers, but non-Christian Indians as well.⁵⁵ A religious pluralism unprecedented in America erupted in the western territories, beginning in Ohio, as Protestants and then Catholics and other denominations surged into the new lands in the first half of the century. Many of the leaders involved in the movement were in fact highly respectful of Indian life and saw the introduction of Christianity as a necessary move for Indian assimilation—and survival. Reporting around 1814 to the Society for Propagating the Gospel Among the Indians, John Schermerhorn not only expressed wonder at the diversity of Indian ways, he was careful to point out that European contact seemed to have had highly detrimental effects upon Indian life.⁵⁶

Sympathizers respected Indian intelligence and lifestyles generally insofar as it meant that they believed Indians should be assimilated into an ever-expanding American society at large. Such a course, it was believed, was in the Indians’ own interest. Few, if any, individuals advocated that Indians should (or even could for that matter) freely maintain their “uncivilized” ways of life in the face of a growing nation of “civilized” citizens. Although there was no widely unified pro-Indian movement, sympathizers did share the attitude that Indians, at the least, were human beings who possessed the innate capacity to

⁵⁵ Gaustad, Religious History of America. Chapter 14 outlines in general how religious reform movements looked toward “civilizing and redeeming the west.”

⁵⁶ Gaustad, Religious History of America, 147.

improve their condition should they so choose. They were not hopeless brutes deserving of extinction in the name of economic expansion.

A striking number and variety of books that dealt with mounds and the moundbuilders were written by Protestant clergymen in the decades before 1840.⁵⁷ Some were intended as fiction, others presented as fact. At times the line between blurred. One interesting work that was intended as fiction was the Reverend Solomon Spaulding's Manuscript Found (1809), in which the fourth century exploits of Christian Romans in America were detailed based on a "translation" of some long lost parchments. The Romans discovered two nations of moundbuilders, one light skinned the other dark, bent on destroying each other.⁵⁸ It has been suspected that Joseph Smith, the founder of Mormonism, plagiarized from Spaulding by capitalizing upon some idiosyncrasies surrounding the publication of Spaulding's tale. In Smith's hands, both moundbuilders and Indians factor into a complex saga of multiple immigrant waves of Israelites to the New Jerusalem that was America, a tale which appeared in his Book of Mormon.⁵⁹ Smith was driven out of most everywhere he lived for his beliefs, continually moving westward toward some envisioned promised land, until he was ultimately murdered in Carthage, Illinois, and his followers declared him a martyr. Mormonism moved to Utah and thrived. At first take, Smith's manipulation of America's native history seems downright bizarre (at least to a non-Mormon). One historian has pointed out, however, that at least the Book of Mormon succeeded on some level in bringing "the New World and its native people

⁵⁷ A great many are enumerated and discussed in Silverberg, chapters 2-3.

⁵⁸ Silverberg, Mound Builders, 89.

⁵⁹ Silverberg, Mound Builders, 90-6.

into the whole divine plan, acclaiming America to be a 'land choice above all other lands.'"⁶⁰

In the 1820s and 1830s, many popular writings appeared in which the moundbuilders were set against negative stereotypes of Indians.⁶¹ In William Cullen Bryant's poem The Prairies (1833), for instance:

The red man came—
The roaming hunter tribes, warlike and wild,
And the mound-builders vanished from the earth.
The solitude of centuries untold
Has settled where they dwelt.⁶²

Popular periodicals such as the Knickerbocker printed poems of a similar tone, some supporting the idea of moundbuilders as Lost Tribes of Israel, and many fixing romantically upon the funerary nature of many mounds. Lengthier works appeared as well, such as Cornelius Mathews's Behemoth, a Legend of the Mound-builders (1839) in which moundbuilders battled against a monstrous and terrifying mammoth that was ultimately slain by a moundbuilder hero named Bokulla.⁶³ According to Silverberg, Mathews was a fervent nationalist looking to create for America a "tradition worthy of Greece's Iliad and Rome's Aeneid," and that Behemoth might be interpreted as an important early contribution to the literary genre that produced Moby Dick.⁶⁴

⁶⁰ Gaustad, Religious History of America, 160.

⁶¹ Silverberg, Mound Builders, 82-8.

⁶² Quoted in David Meltzer, "Ephraim Squier, Edwin Davis, and the Making of an Archaeological Classic," introduction to reprint edition of Squier and Davis, Ancient Monuments, 72 n. 6.

⁶³ Tax, Development, 72-4; Silverberg, Mound Builders, 89.

⁶⁴ Silverberg, Mound Builders, 88.

Books were not the only means of popularizing moundbuilders. Philadelphia physician Montroville Wilson Dickeson had investigated mounds throughout Ohio, Louisiana and Arkansas and capitalized on his endeavors by touring the country between 1837 and 1844 with a massive painted panorama which depicted a sort of archaeological history of the west.⁶⁵ The panorama showed mounds in their natural settings, and it depicted excavations in progress. Dickeson charged a twenty-five cent admission, and, as Silverberg reports, it was a very popular attraction.⁶⁶ One portion of the panorama, which is now housed at the City Art Museum of Saint Louis, is reproduced in Silverberg's account.⁶⁷ It shows in spectacular fashion a the excavated profile of a massive burial mound, with numerous skeletons and various funerary artifacts lying in place within different horizontally stratified levels of soil comprising the mound's interior. In the foreground, two well-dressed white men, one of them presumably Dickeson, stand taking notes on the mound profile towering before them. At least a half dozen black men in work clothes toil away around them with shovels and picks, clearing away the last bits of excavated debris.

The moundbuilders also filtered through into government policy. Jackson justified the fateful extermination of Indians in an address to Congress in 1830 by appealing to the mounds. He stated:

⁶⁵ Silverberg, Mound Builders, 98-9. The panorama, it seems, was gradually painted over some period of time. At least portions of it, if not the entire thing, was painted by John J. Egan. Additional information on Dickeson may be found in Meltzer, "Making of an Archaeological Classic," 17-9.

⁶⁶ Silverberg, Mound Builders, 99.

⁶⁷ Silverberg, Mound Builders, 99.

In the monuments and fortresses of an unknown people, spread over the extensive regions of the west, we behold the memorials of a once powerful race, which was exterminated, or has disappeared, to make room for the existing savage tribes. Nor is there anything in this [the subsequent removal of the Indians], which, upon a comprehensive view of the general interests of the human race, is to be regretted.⁶⁸

What comes around, goes around, Jackson reported: just as the Indians pushed the moundbuilders out of the way, so, too, shall America sweep away the Indian. It was their historical fate.

Even through this cursory survey, it is understandable that a “moundbuilder myth” might gain popularity. It made for good storytelling, it served the interests of Protestant Christians, and it helped ease the guilt of Indian removal.

1.5 Ethnology and the moundbuilders.

By the early 1840s, the moundbuilders had entered so widely into various discourses that leading ethnological scholars began to grow concerned. The situation was further exacerbated by the “discoveries” of Mayan ruins around the same time.⁶⁹ Vague travel accounts about the Central and South American ruins had been in circulation for some time, but this changed decisively when New York traveler and writer John Stephens, accompanied by English artist Frederick Catherwood, undertook two excursions into the Central American jungles in 1839-42 and made rough surveys of some fifty Mayan cities. The descriptions and, especially, images of these ruins that appeared in Stephens’s Views of Ancient Monuments in Central America, Chiapas and Yucatan (1844) had a powerful impact on just about any discussions concerning the aboriginal populations of the New

⁶⁸ December 7, 1830 message to Congress, as quoted in Bieder, Science Encounters the Indian, 112 n. 17.

⁶⁹ Trigger, History of Archaeological Thought, 105.

World.⁷⁰ The spectacular Mexican ruins were attributed to an ancient and mysterious race of “Toltecs,” the moundbuilders of the south, though it was widely accepted that the Toltec civilization had attained a far higher degree of civilization than the moundbuilders. Catherwood’s images shifted the focus of ethnological debate away from the Bering Strait and toward the relationship between moundbuilders and Toltecs, though the question of Asian origins certainly remained important. A number of new questions could be posed: Had moundbuilders drifted south, becoming more advanced as they went, or had the initial migrants into the New World made their way first to Mexico and then north into the Mississippi Valley, retrograding in the process? Alternatively, the presence of moundbuilders and Toltec populations could be explained as the result of separate migrations into the New World. Was this the case? And, lastly, how did living America Indian groups fit into this more complex framework of human history?

These questions were central for the diverse group of scholars brought together in Albert Gallatin’s American Ethnological Society. Although members were hardly unified in terms of their answers, they did adhere to the belief that ethnology could be conducted in a rigorous, methodical way. Broad comparative studies based on exhaustive descriptions and classifications of more particular phenomena—language structures, architecture or arts, archaeological remains, or, increasingly, anatomy—could reveal the patterns and, perhaps, origins of human history in the New World. On this model, similarities between groups indicated the possibility of some common origin, whereas variations could be enumerated and taken as a measure of the time-span of

⁷⁰ Tax, Development, 100-1.

differentiation.⁷¹ The methods of ethnology were seen to be every bit as rigorous an analytical as those of comparative anatomy, or, especially, of uniformitarian geology.⁷² The problem that emerged very quickly within the AES was that, although its members could basically agree on what properly belonged to ethnology, a rift had begun to appear between adherents of the philological and anatomical approaches, one that widened and deepened very quickly. Chief among the disputants were Gallatin himself and the anatomist Samuel Morton.

Gallatin's monogenist views were reinforced by his philological studies, which built from earlier foundational studies by American Philosophical Society president Peter Du Ponceau.⁷³ Du Ponceau had determined all American Indian languages to be of a "polysynthetic" nature. Indians, according to this view, expressed complex concepts by compounding basic units of speech.⁷⁴ Gallatin's most significant linguistic publication was "A synopsis of the Indian tribes of North America" (1836) for the American Antiquarian Society, a study which greatly elaborated upon a language map he had

⁷¹ Curtis M. Hinsley, The Smithsonian and the American Indian: Making a Moral Anthropology in Victorian America (Washington, DC: Smithsonian Institution Press, 1981), 24.

⁷² Smith, History of the Human Sciences, 378-91. John Pickstone is particularly interested in emphasizing the analytical nature of those sciences which have typically been portrayed as purely descriptive and classificatory. For Pickstone, the acts of description and classification generally involve the reduction of complex nature to elemental entities. See, John Pickstone, Ways of Knowing: A New History of Science, Technology, and Medicine (Chicago: University of Chicago Press, 2001).

⁷³ See Greene, American Science, 388-408.

⁷⁴ Bieder, 27-8; Hinsley, 24.

produced earlier.⁷⁵ Stimulated greatly by intercourse with Alexander von Humboldt, Gallatin directly modeled the “Synopsis” on Jefferson’s ideal of an extensive comparative geography of Indian vocabularies.⁷⁶ In it, he classified twenty eight American Indian language families that demonstrated certain similarities and therefore pointed to some common origin, possibly in Asia. Gallatin was consistently hesitant to speculate on ultimate origins, however, remaining content to find evidence for universal human reason.⁷⁷ “Synopsis” also addressed in some detail how a commingling of environmental and cultural factors in the New World facilitated the practice of agriculture among Indians of Central and Southern America, as well as among the moundbuilding populations of the Mississippi Valley.⁷⁸ Later, Gallatin would argue for the diffusion of agriculture from Mexico northward to the moundbuilders.⁷⁹ Gallatin could not accept that the historical relationship between moundbuilders and Indians was indicative of degeneration. And yet, he found it difficult to provide evidence to the contrary. Gallatin thus increasingly thought mound archaeology was going to have to play a bigger role in resolving the issue.

In the final decade of his life, Gallatin, who died in 1849, came to disagree with proponents of an increasingly popular polygenist stance on human origins. Chief among them was Philadelphia anatomist Samuel Morton, who by this time had become one of

⁷⁵ Albert Gallatin, “A synopsis of the Indian tribes of North America,” American Antiquarian Society Transactions and Collections, 1836.

⁷⁶ Greene, American Science, 403-4.

⁷⁷ Bieder, Science Encounters the Indian, 33.

⁷⁸ Bieder, Science Encounters the Indian, 36-7.

⁷⁹ Meltzer, “Making of an Archaeological Classic,” 46.

the most respected scientists in the country.⁸⁰ Morton, a native Philadelphian, was a fixture of the Academy of Natural Sciences, and also a member American Ethnological Society.⁸¹ Beginning with his Crania Americana of 1839, Morton's research gradually polarized not only the ethnologists, but the American scientific community in general, on the nature of human species.

Polygenism in its eventual strong form contained a number of basic tenets that were in direct opposition to Gallatin's philosophical ideals. For one thing, polygenists argued the existence of distinct types of people largely on the basis of the cold, hard quantitative data of comparative anatomy, and especially of skulls. Furthermore, they held that these racial differences were original to Creation, since the accepted time frame for human history given by Scripture was not long enough to account for existing diversity. Some polygenists went so far as to justify the classification of humans into multiple species. Polygenism struck a devastating blow against the core methodological precept of monogenist ethnology, and critics attacked the position in general as heretical for its rejection of a single seat of creation. Polygenism received some further scientific underpinning from the work of Harvard naturalist Louis Agassiz, who held a theory of nature according to which every distinct species of organism was specially created by

⁸⁰ According to the New York Tribune obituary of Morton after his death in 1851, "probably no scientific man enjoyed a higher reputation among scholars throughout the world." As quoted in Stephen J. Gould, The Mismeasure of Man (New York: Norton, 1996), 83.

⁸¹ General biographical information may be found in, Whitfield Bell, Jr., "Samuel Morton," in C.C. Gillespie, ed., Dictionary of Scientific Biography (New York: Scribner's, 1974), Volume 9. Also, see Bieder, Science Encounters the Indian, 55-64; and, Meltzer, "Making of an Archaeological Classic," 23.

God to occupy a specific geographic zone.⁸² He applied this theory specifically to humans in an article that appeared in Josiah Nott and George Gliddon's Types of Mankind, a comprehensive defense of polygenism based on the work of Morton.⁸³

Morton came to support polygenism gradually. As a young man, he studied medicine in Philadelphia, then in Europe at Edinburgh and Paris, and returned to Philadelphia, eventually to take up a chair of anatomy at Pennsylvania Medical College in 1839.⁸⁴ Not coincidentally, English ethnologist James Cowles Prichard thought that Edinburgh was precisely where a dangerously polygenist style of anatomical science was developing. Morton was a gifted comparative anatomist and adopted elements of phrenology then in vogue. Skulls, he came to believe, represented the most stable essential character of mankind, and were least subject to environmental alteration. Consequently, he amassed one of the largest crania collections in the world, and became particularly interested in American Indian crania, both ancient and recent. Working his connections with eastern scientific societies, he cultivated network of collectors who often resorted to unscrupulous means to get skulls from mounds and recent Indian burial grounds.⁸⁵ Morton claimed to have found a correlation between cranial capacity (quantifiable as a measure of volume) and race, as well as mental character. Although certain aspects of

⁸² Gould, Mismeasure, 75; Mary P. Winsor, Reading the Shape of Nature: Comparative Zoology at the Agassiz Museum (Chicago: Chicago University Press, 1991), chapter 3, especially pp. 74-5.

⁸³ Louis Agassiz, "Of the Natural Provinces of the Animal World and Their Relation to the Different Types of Man," in Josiah Nott and George Gliddon, Types of Mankind (Philadelphia: Lippincott, Grambo and Co., 1854), pp. lviii-lxxvi.

⁸⁴ Bieder, Science Encounters the Indian, 56-8.

⁸⁵ Bieder, Science Encounters the Indian, 64-67.

Morton's methodology—as well as the basic principles of phrenology—were eventually discredited as seriously flawed, Morton was a well-respected scientist and his claims held considerable weight.⁸⁶

For his Crania Americana (1839), he drew upon extensive measurements of hundreds of skulls of North and South American Indians, including many thought to be ancient, arguing for the qualitative and quantitative classificatory separation of an “American race” that was distinct to the New World.⁸⁷ Volumetric similarities between ancient and recent skull forms demonstrated to his satisfaction that the racial type was highly variable but ultimately stable and had been unaffected by the environment over time. The “American race,” as he classified it, was distinct from Caucasian, African, Mongolian, and Malayan forms; phrenology additionally gave him the leverage to claim for it a lowly state of mental development.⁸⁸

If Morton saw *all* New World Indians (except the far northern “Eskimaux”) as belonging to a single race, how did he account for what many saw as a gulf between

⁸⁶ Bieder, Science Encounters the Indian, 68. On the dubious quality of Morton's methodology, see Gould, Mismeasure, 82-104. Gould actually re-measured the volumes of many of Morton's crania using the same methods in order to demonstrate Morton's “a priori conviction about racial ranking so powerful that it directed his tabulations along preestablished lines” (p. 101). Gould's point was not to suggest that Morton willfully set out to reinforce his own prejudices, but, rather, to comment on the “unconscious finagling” that took place in Morton's work, and that occurs regularly in science.

⁸⁷ The full title is Crania Americana; or, a comparative view of the skulls of various aboriginal nations of North and South America, to which is prefixed an essay on the varieties of the human species.

⁸⁸ Meltzer, “Making of an Archaeological Classic,” 10-2; Gould, Mismeasure, 82-101; Bieder, Science Encounters the Indian, 69-70. Morton's 1844 study, Crania Aegyptica, extended a similar line of arguments to blacks and the African race.

moundbuilders and Indians? Morton accepted this evidence, and subdivided his American race into the “Toltecan” and “Barbarous” *families*. The culturally more sophisticated Toltecan included moundbuilders, as well as Indians of Central and South America, whereas North American Indians principally comprised the Barbarous category.⁸⁹ Interestingly, Morton’s representative Toltecan skulls possessed smaller average cranial volumes than the Barbarous. This problematic anomaly to his phrenological principles was something Morton struggled to explain away in later writings by claiming either that Indians had in fact undergone some form of degeneration from the more advanced form that predated them, or that the more warlike Barbarous family somehow overcame the semi-civilized Toltecan moundbuilders at some point in the past.⁹⁰ Neither response sat all that easily, which is one major reason that Morton’s research pointed the way toward further mound studies that might shed light on the matter.⁹¹ He especially wanted to acquire intact moundbuilder crania, which were not easy to come by due to the fact that skeletal remains did not preserve well in the moist Mississippi valley.

⁸⁹ Meltzer, “Making of an Archaeological Classic,” 10.

⁹⁰ Meltzer, “Making of an Archaeological Classic,” 11. See also the portions of Nott and Gliddon’s Types of Mankind (1854) attributable to Morton. The work was essentially a heavily edited and annotated collection of posthumous writings by Morton. Chapter 9, and particularly pp. 172-197, clearly outlines the view that the Barbarous Indians with larger heads (and smaller intellects) had overrun the semi-civilized Toltecan. This chapter incorporates extensive evidence from Squier and Davis’s Ancient Monuments of the Mississippi Valley.

⁹¹ Meltzer, “Making of an Archaeological Classic,” 10-1; Gould, Mismeasure, 82-101.

The spur to action was articulated more clearly by Morton in a paper published in The American Journal of Science and Arts in 1846.⁹² After acknowledging the recent Mayan discoveries, Morton called for more archaeological work “in tracing the primitive condition and cognate relations of the several great branches of the human family,” and especially in understanding the American race.⁹³ Importantly, Morton urged that “ethnography and archaeology should go hand in hand,” and that all those so disposed should preserve

every relic, organic and artificial, that can throw any light on the past and present condition of our native tribes. Objects of this nature have been too often thrown aside as valueless; or kept as mere curiosities, until they were finally lost or become so defaced or broken as to be useless. To render such relics available to science and art, their history and characteristics should be recorded in the periodicals of the day; by which means we shall eventually possess an accumulated mass of facts that will be all-important to future generalization.⁹⁴

He even provided some practical advice for collectors of mound skulls, specimens which were almost invariably found in advanced states of decomposition due to humid conditions. Morton advised that “skulls taken from the mounds, should at once be saturated with a solution of glue or gum, or with any kind of varnish, by which precaution further decomposition is effectually prevented.”⁹⁵

Morton went on to reiterate his basic conclusions from Crania Americana about the stability of the American race, adding that since the publication of that volume he had examined another four hundred or so American crania “derived from the repositories of

⁹² Samuel Morton, “Some Observations on the Ethnography and Archaeology of the American Aborigines,” The American Journal of Science and Arts 2 (1846) 1-17.

⁹³ Morton, “Observations,” 1.

⁹⁴ Morton, “Observations,” 2.

⁹⁵ Morton, “Observations,” 3.

the dead in different localities over the whole length and breadth of both Americas.”⁹⁶

Commenting in some detail upon Indian skull deformation practices as a cause for some variation among skulls, Morton claimed the new cranial evidence continued to “present a conformable and national type of organization, showing the origin of one to be equally the origin of all.”⁹⁷ He further emphasized that the American race was “peculiar and distinct from all others,” a point of contention with those who supported the Asian migration hypothesis.⁹⁸ The remainder of the paper marshaled evidence from Indian architecture in the southwestern United States, as well as from a class of discoidal stone artifacts found in mounds as evidence for the stability of the American race. This he directed squarely at theories advocating advanced ancient races (for both moundbuilders and ancient Mexicans) that were separate from recent Indians.

Morton’s studies successfully added a new dimension—the anatomy of race—to ethnology in general and to archaeology in particular. In these years between the start of Indian removal and the start of the Civil War in 1861, the growing rift between advocates of monogenist and polygenist approaches served to highlight the need for further cautious research. Some important questions were being raised about the nature of humankind in general, and within all of this, archaeology and the mounds offered promising leads. In the second half of the 1840s, a new scientific organization was rising to prominence that would unalterably shape the direction of archaeological research, both on the mounds and in more generally. This was the Smithsonian Institution, whose early reputation rested in

⁹⁶ Morton, “Observations,” 4.

⁹⁷ Morton, “Observations,” 4.

⁹⁸ Morton, “Observations,” 7.

large measure on the activities of two archaeologists working out of Chillicothe, Ohio by the names of Ephraim George Squier and Edwin Hamilton Davis.

1.6 Further calls for a systematic mound archaeology.

In their introduction to Ancient Monuments of the Mississippi Valley, published in 1848 under the aegis of the Smithsonian, Squier and Davis despaired at the impossibility of listing all of the “detached and incidental observations” about mounds that continued to be published in piecemeal ways through various channels.⁹⁹ There was, they thought, little uniformity to these accounts except that they were generally short and suitably vague. Despite Atwater’s attempt at synthesis, much remained to be done in terms of standardizing accounts. Squier himself earlier wrote of Atwater’s work to Smithsonian secretary Joseph Henry that it lacked empirical rigor and ultimately sustained the “marvelousness of the public” rather than throw “any clear and certain light upon our monuments.”¹⁰⁰

Ethnologists were beginning to hold out great hope for the potential this new Smithsonian Institution could have in supporting their science. In 1847, Gallatin wrote to Henry of the mounds that “the most vague and fabulous reports...and even flagrant impostures are so mixed with true accounts as to render it almost impossible even for the American reader to make the proper discrimination.”¹⁰¹ He was writing with a particular

⁹⁹ Squier and Davis, Ancient Monuments, xxxii-xxxiii.

¹⁰⁰ Ephraim Squier to Joseph Henry, March 24, 1847, in Marc Rothenberg, ed., The Papers of Joseph Henry, Volume 7: January 1847-December 1849, The Smithsonian Years (Washington, D.C.: Smithsonian Institution Press, 1996), 57.

¹⁰¹ Albert Gallatin to Joseph Henry, June 16, 1847, in Rothenberg, ed., Papers of Joseph Henry, 116.

reason—to persuade Henry to publish the results of the archaeological work being done by Squier and Davis. Not only was Ancient Monuments of the Mississippi Valley published by the Smithsonian, it was published as the inaugural volume of Henry’s projected Contributions to Knowledge series, the expensively produced flagship serial that was intended to bring Smithsonian science to all the world.

Chapter 2.

Ancient Monuments of the Mississippi Valley:

The visual impact of Squier and Davis's landmark study of 1848.

2.1 Introduction.

By the mid-1840s, leading American ethnologists Albert Gallatin and Samuel Morton had become explicitly concerned about methodological standards for archaeology.

Gallatin, a philologist, and Morton, an anatomist, were both interested in one of the biggest questions of contemporary American ethnology: how did aboriginal people get here? Either they had migrated and diversified into the continent, or they were truly autochthonous and had literally always been here. This, essentially, was the difference between monogenism and polygenism. Underlying their attitudes was a deep concern for the integrity of archaeological data, since archaeology provided vital corroborating evidence. Gallatin worried about the presentation of data—he thought it had become very difficult to discriminate between reliable and unreliable reports on the mounds given the proliferation of literature on the subject. Morton, on the other hand, was vexed by the loss of artifacts and osteological specimens by careless or ignorant collectors who discarded or lost track of them.

At the same time, two men who were very interested in the archaeology of the Ohio valley began a collaboration in 1845 that resulted in the appearance three years later of Ancient Monuments of the Mississippi Valley, arguably the most influential publication of nineteenth century American archaeology. Ephraim George Squier and Edwin Hamilton Davis, the collaborators, performed original fieldwork and synthesized a considerable body of extant information on the mounds for this study. In so doing, they

formalized the concept of a “race of the mounds.” The highly visual nature of the text, especially the plan view maps of earthwork sites, left little to the imagination: these were not the works of extant Indians. What made Ancient Monuments so influential was that it was published by the Smithsonian Institution and promoted as a model of careful, inductive science. In fact, Smithsonian Secretary Joseph Henry was so convinced of its merits as a piece of scientific ethnology that he published it as the inaugural, precedent-setting volume of the Institution’s Contributions to Knowledge series. These volumes were expensively produced, distributed widely, and were intended to establish the Smithsonian as a leading supporter of scientific research. For well over three decades, Ancient Monuments remained the standard reference point for most Ohio valley archaeologists seeking to do fieldwork there.

2.2 A profile of Edwin Hamilton Davis.

I want to begin this chapter with a profile of Edwin Hamilton Davis. Historians generally focus on the character of Squier in the collaboration largely because Squier went on to do further notable research in ethnology, whereas Davis did not—at least nothing that was published, unlike Squier whose publication record was fairly extensive. Davis, however, was arguably the more important of the two in terms of the work accomplished for Ancient Monuments. Davis was a life-long resident of south central-Ohio—right in the very heart of mound country—whereas Squier was a transient New Yorker who lived in Ohio only for the three years they worked together. Davis, a doctor, had also been a long-time avocational archaeologist and was well-connected to others with similar interests. He was part of the same lively, growing community of Ohio archaeologists that had included people like Daniel Drake and Caleb Atwater.

Born in 1811, Davis grew up in the southern Ohio town of Hillsboro.¹ He was fascinated by the mounds and earthworks of the region from an early age. Davis attended college at Kenyon, northeast of Columbus, where, around 1831 or 1832, he began excavating and studying some nearby mounds, paying particular attention to their construction. This activity resulted in a paper in which Davis refuted some claims made in an earlier study by Caleb Atwater about finding bricks in the mounds.² Davis delivered the paper first to the local Philomethesian Literary Society, and then again at his Kenyon commencement in 1833, where it attracted the attention of the famous orator Daniel Webster, a member of the Worcester, Massachusetts based American Antiquarian Society.³ Both men were distraught by the destruction of mounds by the plow, and, encouraged by Webster, Davis made an ongoing commitment to himself to document the sites while pursuing a career in medicine.⁴ In 1835, he moved to Bainbridge, about midway between Hillsboro and Chillicothe in the Paint Creek valley, and over the next four years attended Cincinnati Medical College, graduating in 1837 or 1838. In 1839,

¹ General biographical information on Davis comes from Terry Barnhart, Of mounds and men: The early anthropological career of Ephraim George Squier, diss., Miami University, 1989 (Ann Arbor: UMI 1989, T24741), 33-6; David Meltzer, "Introduction: Ephraim Squier, Edwin Davis, and the Making of an Archaeological Classic," introduction to reprint edition of Ephraim G. Squier and Edwin H. Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press, 1998[1848]), 7-9; and Edwin Davis to Charles Rau, 28 May and 5 June 1884, Smithsonian Institution National Anthropological Archives (SI/NAA) MS 7065.

² Davis to Rau, 28 May and 5 June 1884, SI/NAA MS 7065.

³ Davis to Rau, 28 May and 5 June 1884, SI/NAA MS 7065; Barnhart, Of mounds and men, 33-4; and Meltzer, "Making of an Archaeological Classic," 7.

⁴ Barnhart, Of mounds and men, 34; Meltzer, "Making of an Archaeological Classic," 7.

Davis relocated to Chillicothe, married the daughter of a prominent banker, and set up a medical practice.⁵

In the few years prior to settling in Chillicothe, a number of associations and events transpired to further his experience as an archaeologist. In 1836 or 1837, he participated in surveys of the massive complex of earthworks at Newark, just east of Columbus, with Charles Whittlesey of the Geological Survey of Ohio.⁶ In Cincinnati, Davis socialized with Daniel Drake of the Western Museum.⁷ But the most important event during these years for him was the construction of the Milford and Chillicothe turnpike, which passed through Bainbridge (Milford was just outside Cincinnati). A large twenty-four mile segment of the turnpike followed the valley of Paint Creek, an area replete with ancient works. Road construction passed directly through a number of earthwork complexes and their associated mounds, and Davis committed himself to watching the operation very closely especially where mounds were being bisected. The road work was destructive, but it gave him the opportunity to study the structure of a relatively large number of mounds and to collect many specimens.⁸

Davis later recalled that he was struck by what he saw there. In the first place, these mounds did not appear to contain any human bones. They had not been constructed for burial purposes. And yet, all of them had been built in essentially the same manner as burial mounds, that is, with successive domed layers of different soils, except that instead

⁵ Davis to Rau, 28 May and 5 June 1884, SI/NAA MS 7065; Meltzer, "Making of an Archaeological Classic," 7-8 and 73 n. 19; Barnhart, Of mounds and men, 34-5; Tax 178-9.

⁶ Barnhart, Of mounds and men, 34.

⁷ Meltzer, "Making of an Archaeological Classic," 73 n. 19.

⁸ Davis to Rau, 28 May and 5 June 1884, SI/NAA MS 7065.

of having skeletal remains at the core of the mound, these structures contained a central burnt clay basin.⁹ They also mostly contained numerous artifacts of a specific type—hundreds of pipes in one, as many spear points or pieces of pottery in another, for instance.¹⁰ Davis was intrigued. Why had such care been taken to construct mounds that did not serve a burial function? The answer was that these seemed to have been built for some other ceremonial purposes. They seemed to have been carefully placed in relation to earthwork embankment structures and they were apparently built according to some basic structural plan. There was also nothing accidental about the sorts of artifacts they contained and the burnt clay centers, Davis thought, might have been a form of altar.

As it turned out, these were significant observations. Burial mounds were known to consist of layered soils, even if it was just one layer over the remains.¹¹ Because they were burials, there was no doubt as to their artificial construction. There was, however, some disagreement as to whether all mounds were indeed artificial creations. Edward Hitchcock, for one, was a prominent eastern geologist professor of same at Amherst College, Massachusetts, and he thought that many structures thought to be mounds were in fact of natural origin, created by sedimentation from repeated flooding in that part of the country.¹² It was fortuitous for Davis that the turnpike cut through these Paint Creek

⁹ Davis to Rau, 28 May and 5 June 1884, SI/NAA MS 7065.

¹⁰ Davis to Rau, 28 May and 5 June 1884, SI/NAA MS 7065.

¹¹ At least by around 1780, Thomas Jefferson had reported on the stratified nature of a burial mound he excavated on his Virginia estate. William Stiebing, Jr., Uncovering the Past: A History of Archaeology (Oxford: Oxford University Press, 1993), 172-3.

¹² Hitchcock was in charge of the Massachusetts geological survey and he played a significant role in the transition of the Association of American Geologists and Naturalists into the American Association for the

valley sites. He saw very clearly that the non-funerary mounds had indeed been constructed.

Davis did not publish his views, but he shared them with Virginia state geologist William Rogers, who had visited the valley on one of his exploring expeditions in 1837.¹³ Rogers examined one of the mound sections and agreed that it appeared to be the work of artifice. It was Rogers who likely brought Davis's observations to the attention of Hitchcock, who remained unconvinced. According to Davis's recollection, at least, Hitchcock continued to explain mound stratigraphy as the result of diluvial processes. Elsewhere, the Amherst geologist reasoned that the mounds and earthworks were too large and far too numerous to have been built by people, even over very long periods of time.¹⁴ As far as Hitchcock was concerned, these were not for the most part human productions.

Soon enough, the debate over the artificial or natural origin of many mounds drew Samuel Haven of the American Antiquarian Society and Edward Robinson, an officer of the American Ethnological Society, to Ohio in order to make their own determination on

Advancements of Science. See, Sally Gregory Kohlstedt, The Formation of the American Scientific Community: The American Association for the Advancement of Science, 1848-60 (Urbana, IL: University of Illinois Press, 1976), chapters 1-2. On Hitchcock's views about the diluvial origins of mounds, see Edward Hitchcock, "Final Report on the Geology of Massachusetts," 1841, 369. Also, see, Davis to Rau, 28 May and 5 June 1884, SI/NAA MS 7065; Marc Rothenberg, Paul Theerman, Kathleen Dorman, John Rumm, Deborah Jeffries, ed., The Papers of Joseph Henry, Volume 7: January 1847 – December 1849, The Smithsonian Years (Washington, DC, Smithsonian Institution Press: 1996) 59 n. 7, 510 n. 1.

¹³ Davis to Rau, 28 May and 5 June 1884, SI/NAA MS 7065.

¹⁴ According to Davis, Bancroft repeated the error in the Illinois Magazine, volume 1, p. 252. Davis to Rau, 28 May and 5 June 1884, SI/NAA MS 7065.

mound stratigraphy.¹⁵ Davis gained considerable support for his side from the easterners, though he later saw Hitchcock's views repeated in the literature by scholars such as the historian George Bancroft.¹⁶ This was particularly irksome to the Ohio doctor. He later recalled that, as far as he knew, Hitchcock had never even seen an actual mound in person.

When Davis relocated to Chillicothe—a town situated right at the confluence of Paint Creek and the Scioto River, another area where some of the densest concentrations of ancient works could be found—he had earned a reputation as a serious archaeologist.

2.3 Davis, Squier and the Smithsonian.

Chillicothe had been the official birthplace of Ohio statehood and served as the state capital for some years in the early part of the century. It was a leading papermaking center and a major transportation hub, with a number of roads intersecting there, the Ohio and Erie canal running north, and railways in the making during the 1840s.¹⁷ For those who were interested, the Chillicothe region (see Figure 2.1) was thought to have been one of the major centers of ancient population, the moundbuilder ground-zero for the Ohio valley.¹⁸ When Davis moved here, he began corresponding with members of the eastern

¹⁵ Davis to Rau, 28 May and 5 June 1884, SI/NAA MS 7065. Davis also expressed surprise that there remained some doubters as to the artificial character of mounds as of 1884.

¹⁶ Davis to Rau, 28 May and 5 June 1884, SI/NAA MS 7065.

¹⁷ G. Richard Peck, Images of America: Chillicothe, Ohio (Charleston, SC: Arcadia Publishing, 1999).

¹⁸ Squier and Davis, Ancient Monuments, xxxiv.

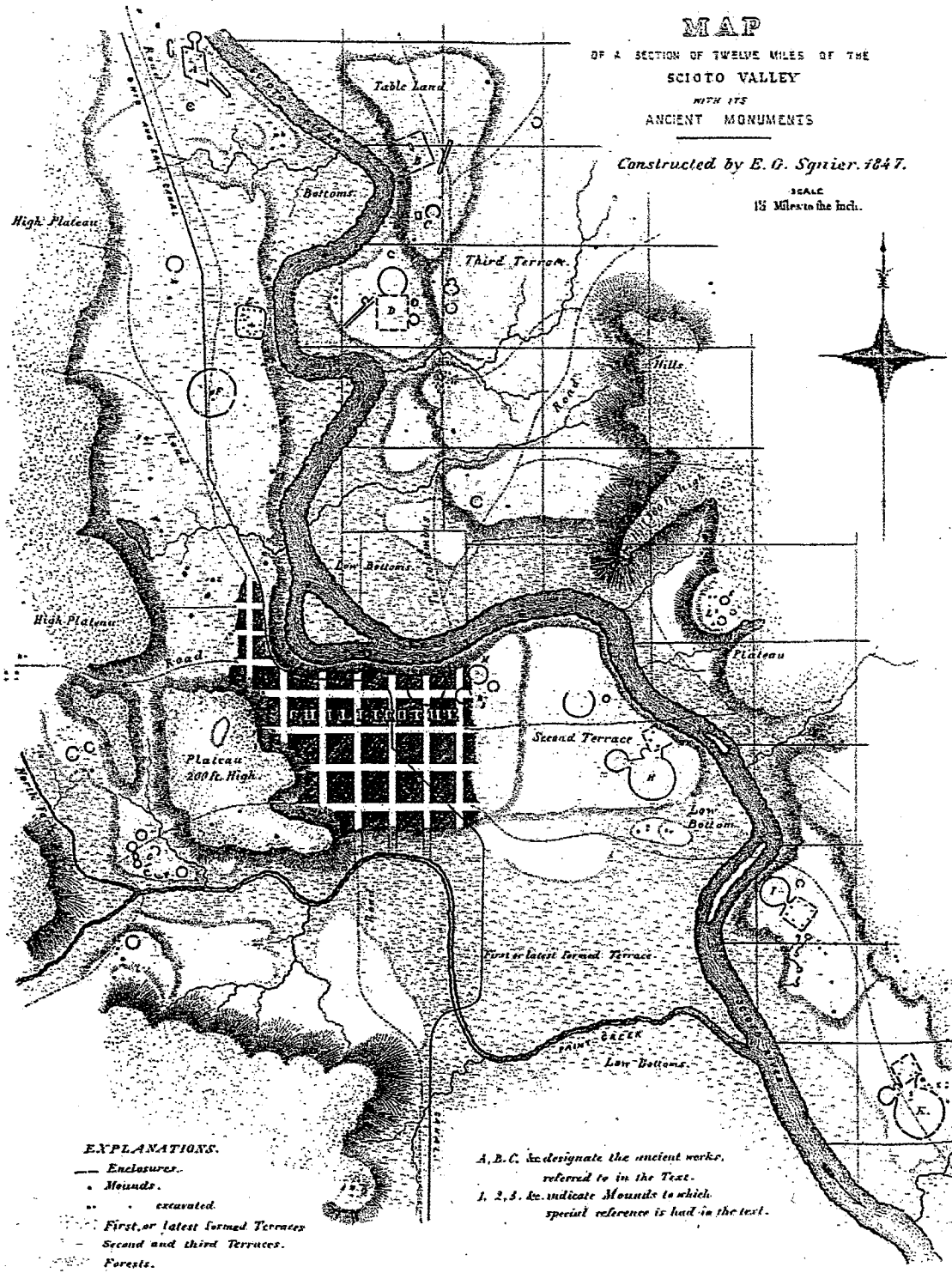


Figure 2.1. Ancient works in the vicinity of Chillicothe. Most of these earthwork groups were designated as sacred enclosures. Not to original scale. Source: Squier and Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press, 1998 [1848]), plate between pp. 2-3.

ethnological community, including Samuel Morton, who sent him a copy of Crania Americana.¹⁹

In 1845, Davis became acquainted with Ephraim George Squier, a younger man who had arrived in town early that Spring to take up an editorial position with the local newspaper. Squier, originally from New York, was a writer who had restlessly wandered the east largely in search of elusive literary fame.²⁰ He arrived in Chillicothe knowing a bit about the region's archaeological monuments; however, he soon enough became fascinated, if not utterly obsessed, by them.²¹ Here, he thought, was something he could write about and make a name for himself. Like Davis, Squier, too, was particularly unhappy with the accuracy of Atwater's study. Atwater had described a number of supposedly ancient wells in the slate bed of Paint Creek; Squier found these very clearly to be natural geological formations.²² Davis and Squier thus found in each other a mutual dissatisfaction with the state of affairs in archaeology. They also believed that, outside of Atwater's work, the reports about mounds and earthworks appearing in other publications were too brief and unsubstantiated to be of any real scholarly value.

The two men then began a more substantial collaboration. At first, they began collecting only artifacts.²³ Soon enough, however, they conceived a broad investigation

¹⁹ Meltzer, "Making of an Archaeological Classic," 9 and 74 n. 28.

²⁰ Biographical information on Squier comes from: Meltzer, "Making of an Archaeological Classic," 5-7; Barnhart, Of mounds and men, 1-30; Tax, Development, 175-8.

²¹ Meltzer "Making of an Archaeological Classic," 9; Ephraim G. Squier to Joseph Henry, 24 March 1847, in Rothenberg, et al., Papers of Joseph Henry, Volume 7, 57-8.

²² Squier to Henry, 24 March 1847, in Rothenberg, et al., Papers of Joseph Henry, Volume 7, 57-8.

²³ Meltzer, "Making of an Archaeological Classic," 9.

of southern Ohio mounds and earthworks that involved methodically surveying as many earthworks and excavating as many mounds as possible. Quite ambitious, given that the work would have to be done in their spare time. The execution of the project overall was propelled by Squier's increasingly relentless ambitions, while the day to day conduct of fieldwork relied heavily upon Davis's valuable wealth of local and regional experience, as well as his money. Ultimately, the collaboration culminated in the Smithsonian-sponsored publication of Ancient Monuments of the Mississippi Valley in 1848, which was hailed as an instant landmark in American archaeology, one that put Chillicothe squarely on the map as an epicenter for much subsequent archaeological research.

The history of the Squier and Davis project, as well as the significance of the Smithsonian's role in supporting their publication, has been examined in detail by a number of historians.²⁴ The whole episode eventually devolved into a saga of epic proportions rife with intrigue, breach of trust, personal and institutional machinations, and intense personality conflicts, mainly revolving around the increasingly temperamental character of Squier.

The relationship between Davis and Squier began amicably enough, however, as they took to the field and amassed a sizeable cabinet of moundbuilder specimens.²⁵ As their aspirations for the scale of the project grew, so did the need for additional funding. In the Spring of 1846, Squier traveled to the east coast with the specimen collection and letters of introduction from Davis, hoping to attract patronage. In Washington, Philadelphia,

²⁴ An excellent full account is Meltzer's extended introduction to the 1998 reprint of Ancient Monuments, cited above. Also Barnhart, Of mounds and men, chapters 2-5; Tax, Development, chapter 5.

²⁵ Barnhart, Of mounds and men, 38. According to the author, they had collected at least 6,000 specimens by July of 1846.

New York, Boston, and Worcester, he drew the attention of members of the Academy of Natural Sciences, the American Ethnological Society, the American Academy of Arts and Sciences, and the American Antiquarian Society. Among those who were especially interested in the proposed project Squier described were Morton, Gallatin (who by this time was frequently ill), John Russell Bartlett (acting head of the Ethnological Society), and Samuel Foster Haven (librarian and central figure of the Antiquarian Society). Despite widespread enthusiasm, and attempts at coordination of funds between the various organizations, only the American Ethnological Society offered to support publication of a substantial paper. Squier returned to Chillicothe in August, and he and Davis resumed work still funded by Davis's income.²⁶

Over the course of 1846-7, Squier and Davis were able to produce a number of publications. Five short papers appeared in the American Journal of Science (one by Davis, four by Squier).²⁷ The American Ethnological Society made good on its promise and published a long paper by Squier in late 1847.²⁸ In retrospect, this was a blueprint of

²⁶ Meltzer, "Making of an Archaeological Classic," 10-7; Barnhart, Of mounds and men, 40-9; Tax, Development, 182-90; Squier to Henry, 24 March 1847, in Rothenberg, et al., Papers of Joseph Henry, Volume 7, 58 and 59 n. 8.

²⁷ Ephraim Squier, "On the discoidal stones of the Indian mounds," American Journal of Science 2(1846):216-8; Ephraim Squier, "Pipestone of the ancient pipes of the Indian mounds," American Journal of Science 2(1846): 287; Ephraim Squier, "Discoidal stones," American Journal of Science 2(1846):287-8; Ephraim Squier, "Observations on the uses of the mounds of the west, with an attempt at their classification," American Journal of Science 3 (1847):237-48; Edwin Davis, "Footprints and Indian sculpture," American Journal of Science 3(1847):286-88.

²⁸ Ephraim Squier, "Observations on the aboriginal monuments of the Mississippi Valley," Transactions of the American Ethnological Society 2(1847):131-207.

what was to come in Ancient Monuments. It appeared without Davis's name—resulting in a dispute over authorship and proper credit that was the real beginning of the end between the two (and was exacerbated when Squier's name appeared first on the Smithsonian text).²⁹ More importantly, by the time the AES paper appeared in print, Joseph Henry, secretary of the fledgling Smithsonian Institution, had agreed to support the publication of a monograph based on Squier and Davis's work as the inaugural volume of the Smithsonian's Contributions to Knowledge series.

In early 1847, Henry had learned of Davis and Squier's work largely through American Ethnological Society members Bartlett and George Marsh (who would later sit on the Smithsonian's Board of Regents). Gallatin, for whom Henry held great respect, wholeheartedly endorsed the project and thought it deserved more support than the AES could give.³⁰ Henry thus began corresponding with Squier, not realizing that Squier had begun positioning himself to appear as the principal investigator in the project.³¹ As it happened, the Smithsonian Board of Regents had just appropriated to Henry one thousand dollars for the production of the Smithsonian Contributions to Knowledge.³² A very important part of this story is the fact that Henry was a physicist (specializing in terrestrial magnetism) and was widely considered to be one of the leading American scientists of the day. It came as a bit of a surprise to many of Henry's scientific peers

²⁹ Meltzer, "Making of an Archaeological Classic," 26.

³⁰ Gallatin to Henry, 16 June 1847, in Rothenberg, et al., Papers of Joseph Henry, Volume 7, 116-7.

³¹ Meltzer, "Making of an Archaeological Classic," 21; Squier to Henry, 24 March 1847, Rothenberg, et al., Papers of Joseph Henry, Volume 7, 57-60; Henry to Squier, 3 April 1847, Rothenberg, et al., Papers of Joseph Henry, Volume 7, 76-8.

³² Rothenberg, et al., Papers of Joseph Henry, Volume 7, 78 n. 2.

when he opted to stake the Smithsonian's yet unproven scientific reputation on an archaeological project as its initial effort.³³

Henry viewed archaeology as part of a broader science of ethnology, a part which seemed especially conducive to the kind of rigorous, inductive scientific ideal that was promoted through Smithsonian rhetoric. By supporting the work of Squier and Davis, he wanted to showcase the Smithsonian as a place for the "increase and diffusion" of a very broad range of subject matter. Archaeology also fit within the model he had envisioned for a nationally coordinated network of meteorological observations—in like manner, the Smithsonian might serve to coordinate a broad geographic series of archaeological studies.³⁴ Squier and Davis accepted the patronage, unaware of the laborious editorial negotiations that lay ahead (during which process Squier became increasingly distrustful to the point of paranoia and obsessed with gaining recognition, much to the expense of Davis's name).³⁵

There were no precedents to follow for anyone involved in the production of what was planned to be a lavish quarto volume on the order of 300-400 pages with numerous illustrations. So it is understandable that process was not entirely smooth. With Ancient Monuments, Henry felt it necessary to establish a clear precedent for peer-review procedures that would apply to all future Smithsonian publications. However, he had already agreed to publish the manuscript even before receiving it. What also made things

³³ Meltzer, "Making of an Archaeological Classic," 21-2.

³⁴ Curtis Hinsley The Smithsonian and the American Indian: Making A Moral Anthropology in Victorian America (Washington, DC: Smithsonian Institution Press, 1981) 34-40.

³⁵ Henry to Squier, 24 November 1847, in Rothenberg, et al., Papers of Joseph Henry, Volume 7, 232-4; Squier to Henry, 3 January 1848, in Rothenberg, et al., Papers of Joseph Henry, Volume 7, 252-5.

awkward was that—at least in Henry’s mind—Gallatin was the logical choice for outside appraiser, but Gallatin’s was hardly an unbiased perspective on the Squier and Davis project at this point. Nonetheless, it was exactly the plan Henry followed. With the open consent of Squier and a group of American Ethnological Society reviewers, some letters were fabricated and backdated. The letters made it seem as though the manuscript had been received before any definite arrangements were made about publishing, and they made the AES reviewers out to be a completely objective third party.³⁶ Henry could then reprint these correspondences at the front of the published text. Other factors complicated the situation as well. In early 1847, Henry and the Board of Regents were debating the functional essence of the Institution. The debate came to a head in December of 1847, when Henry drafted his “Programme of Organization of the Smithsonian Institution,” a critical document in the early history of the Institution. In the main, it emphasized research and publications over library and museum programs.³⁷ As far as Henry was concerned, the primary mission of the Smithsonian was to operate mainly as a publishing house for peer-reviewed scientific monographs that could then be distributed widely. Ancient Monuments would serve as the model, so at the least it had to appear to have been done right.

³⁶ Meltzer, “Making of an Archaeological Classic,” 24. Also see, Henry to Squier, 18 August 1848, in Rothenberg, et al., Papers of Joseph Henry, Volume 7, 385-7; and, also, Rothenberg, et al., Papers of Joseph Henry, Volume 7, 59 n. 9.

³⁷ This document was reprinted as an advertisement and placed conspicuously at the front of Ancient Monuments, pp. iii-viii. It has also been reprinted in Rothenberg, et al., Papers of Joseph Henry, Volume 7, as an insert between pp. 248-9.

2.4 The ancient monuments of Ancient Monuments.

In its final form, Ancient Monuments of the Mississippi Valley was both a presentation of extensive original research and a major synthesis of work done by others on the mounds. Squier and Davis personally investigated some three hundred sites. A vast majority of these were located along sections of the Scioto River or Paint Creek valleys of Ohio, despite the broad-ranging title of the text. They surveyed and mapped around one hundred earthworks, and approximately two hundred mounds were excavated, their stratigraphy generally noted and relics obtained.³⁸ Several thousand artifacts of many forms were collected, as well as some skeletal material. Incorporated into the text were numerous surveys and observations made by a number of other individuals, including topographic engineer Charles Whittlesey of the Ohio Geological Survey (with whom Davis had earlier worked), and surveyors James McBride, Samuel Hildreth, and John Locke, all three residents of southern Ohio and avocational archaeologists. McBride lived just north of Cincinnati and had done notable survey work on many of the sites in the Little Miami River valley.³⁹

One of the most enduring features of Ancient Monuments was that it formalized a classification system for the mounds and earthworks that was essentially based on tacit functional categories in common use among collectors and scholars. These categories were based on assumed functions for works with different structural characteristics.

³⁸ Squier and Davis, Ancient Monuments, xxxiv.

³⁹ Squier and Davis, Ancient Monuments, xxxv-xxxvi; Barnhart, Of mounds and men, 82-3. Some controversy would also emerge over Squier later giving proper credit to these—and other—contributors during the editorial process with Henry. The net result was the addition to Ancient Monuments of a lengthy preface enumerating all those who made valuable contributions.

Ancient works were thus divided into two general subclasses, “Enclosures” and “Mounds.” “Enclosures” were large-scale structures comprised of systems of earthen embankments and were further subdivided into principle categories of “Works of Defence” and “Sacred Enclosures.” Defensive structures such as the aptly dubbed Fort Ancient and the Fortified Hill (see Figures 2.2 and 2.3) irregularly conformed to hilltops and were assumed to have some sort of martial function. Sacred enclosures such as the Newark Works and others (see Figures 2.4 to 2.7) were characterized by their geometric regularity and seeming complexity and were thought to be sites of ceremonial or religious activity. Some attention was given to monuments of the southern and northwestern states, especially to the animal effigy earthworks found in Wisconsin (Figure 2.8) and to a few other works not easily classifiable (Figure 2.9). “Mounds,” on the other hand, were conical constructions further broken down into categories of “Altar Mounds” (or “Mounds of Sacrifice”), “Mounds of Sepulture,” “Temple Mounds,” or the catch-all “Anomalous Mounds” (see Figures 2.10-2.12). Mounds generally contained a great number of specimens, whereas enclosure embankments did not. As a matter of course, both kinds of works could generally be found in association with each other, especially in the case of sacred enclosures, despite the fact that Squier and Davis treated them as separate and distinct kinds. Many altar and temple mounds, for instance, were located within or adjacent to sacred enclosures. A little over one half of the text was devoted to the enclosures and mounds themselves, and a vast majority of that was focused on Ohio sites.

The bulk of the rest of the text was devoted to descriptions of objects found within the mounds (see Figures 2.13-2.17), although Squier and Davis occasionally included

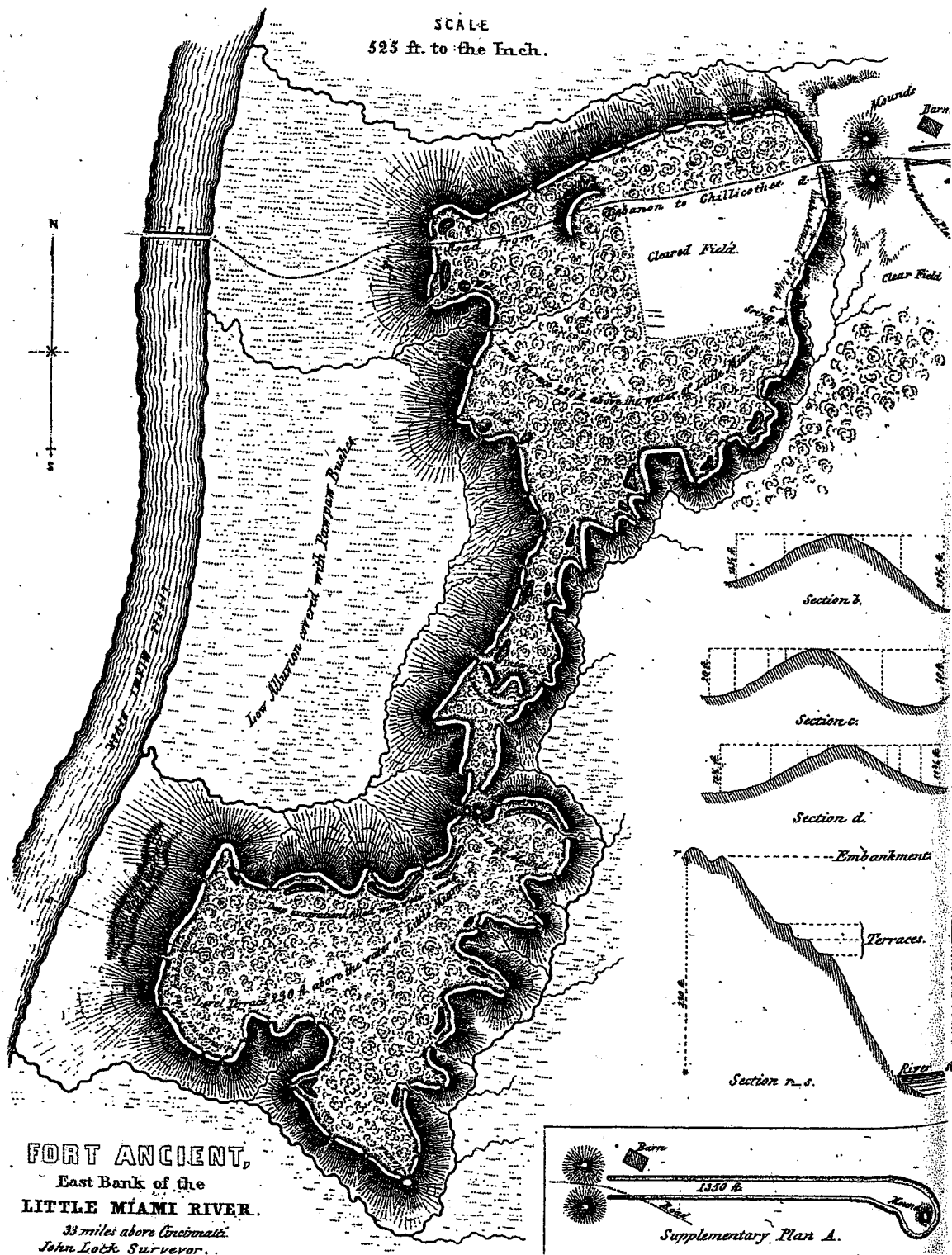


Figure 2.2. "Fort Ancient." For obvious reasons, perhaps, Squier and Davis classified this as a defensive work. Not to original scale. Source: Squier and Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press, 1998 [1848]), plate between pp. 18-9.

FORTIFIED HILL,

SUTLER COUNTY OHIO.

(3.3 Miles S.W. of the
TOWN OF HAMILTON.)

Surveyed by Jas. M^c Bride 1836.

*Mound
10 ft. high.*

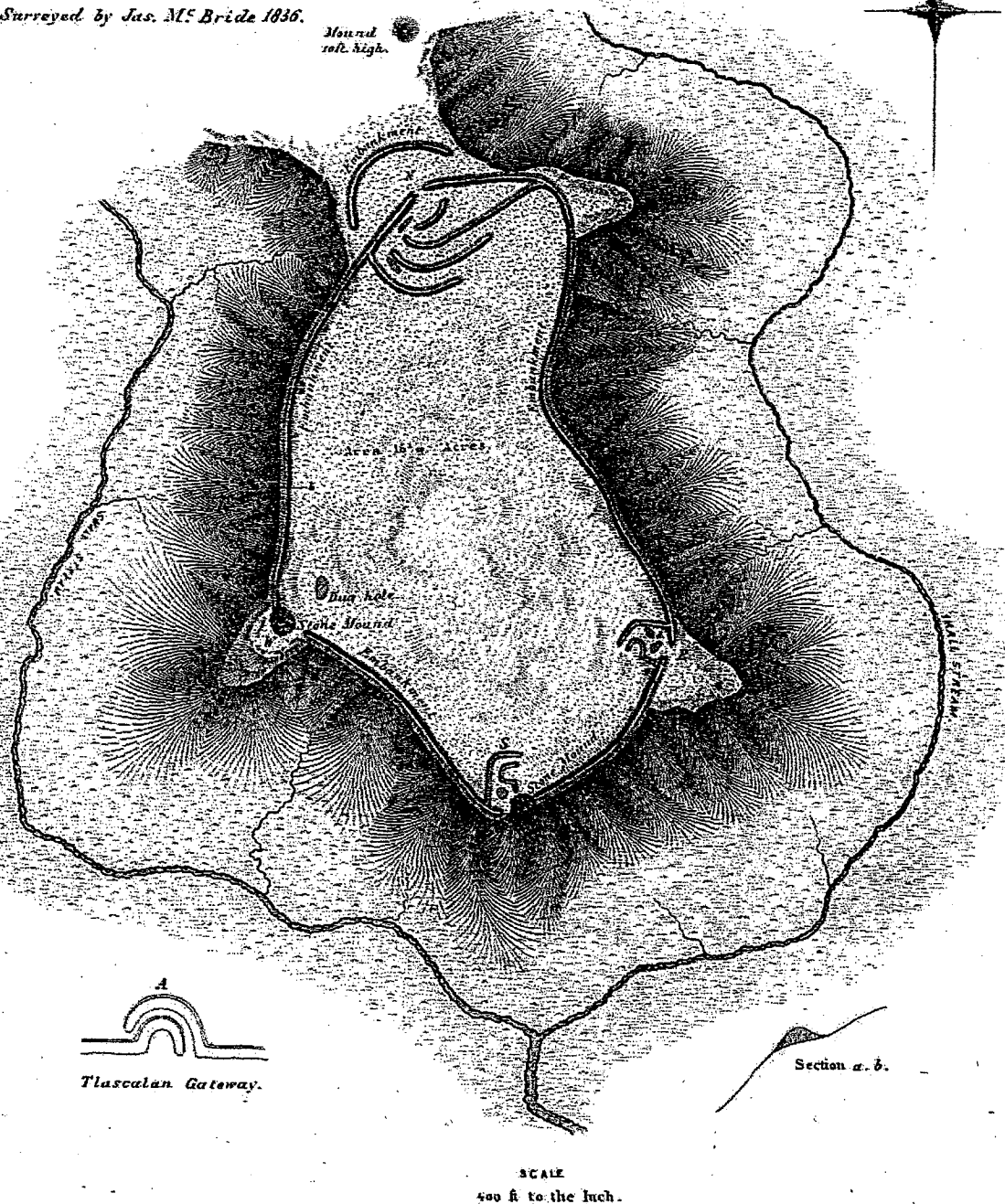


Figure 2.3. "Fortified Hill," another of Squier and Davis's "Works of Defence." Not to original scale. Source: Squier and Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press, 1998 [1848]), plate between pp. 16-7.

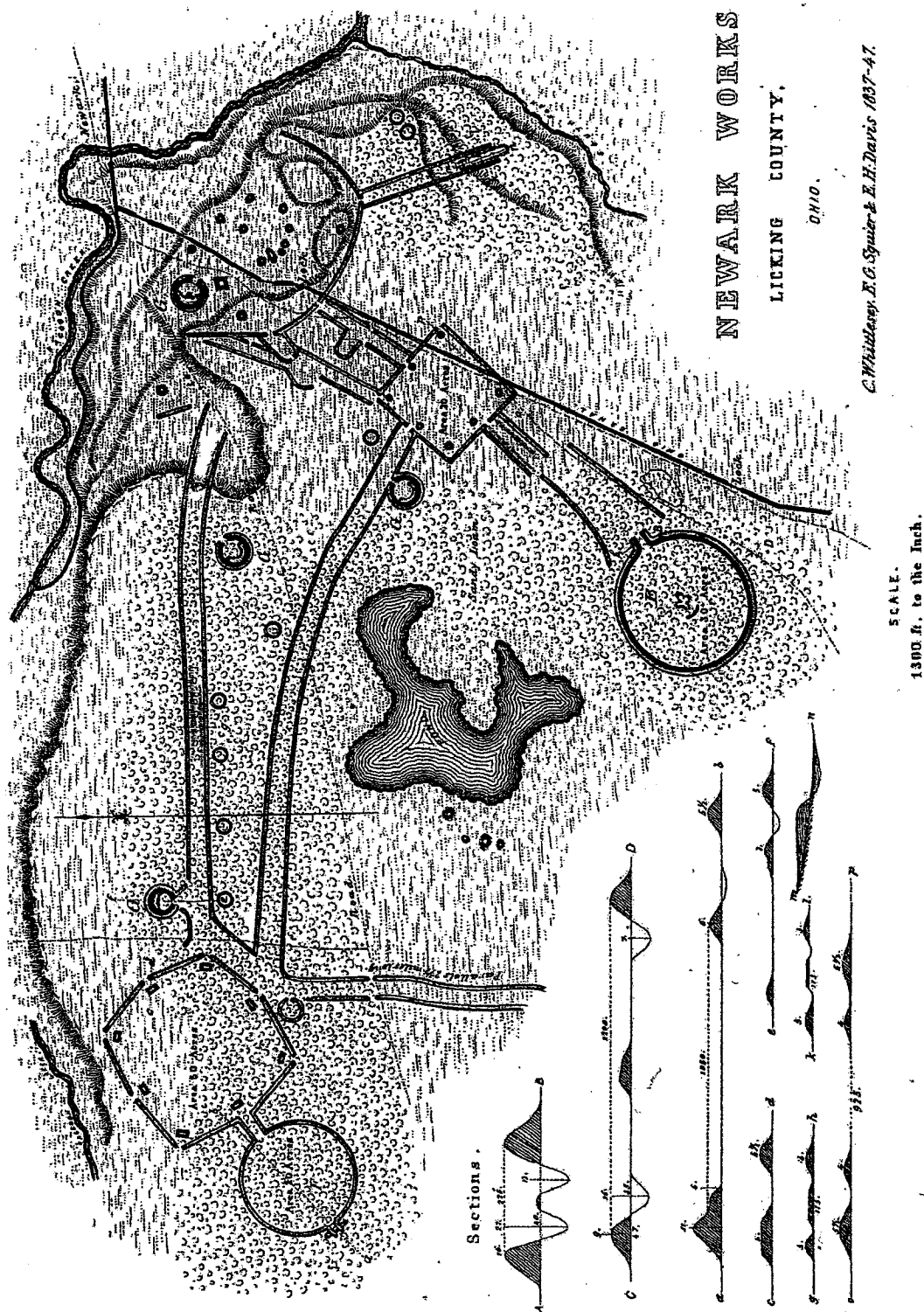


Figure 2.4. The ancient metropolis known as the "Newark Works," one of the most extensive of the Ohio valley sites. Designated by Squier and Davis as a sacred enclosure. The actual enclosed areas are as great as fifty acres. Not to original scale. Source: Squier and Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press, 1998 [1848]), plate between pp. 66-7.

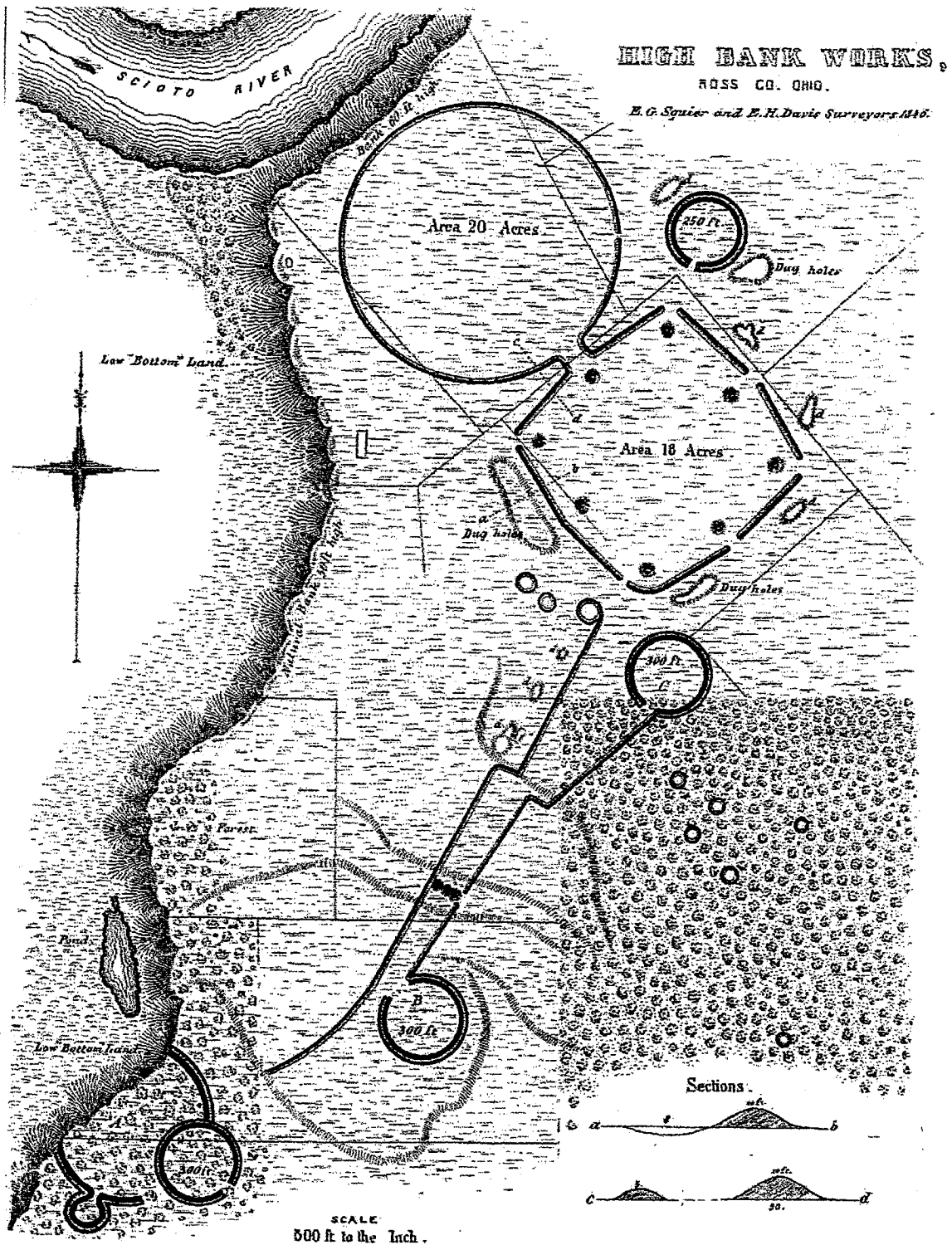


Figure 2.5. The "High Bank Works," another sacred enclosure. Not to original scale. Source: Squier and Davis, *Ancient Monuments of the Mississippi Valley* (Washington, DC: Smithsonian Institution Press, 1998 [1848]), plate between pp. 50-1.

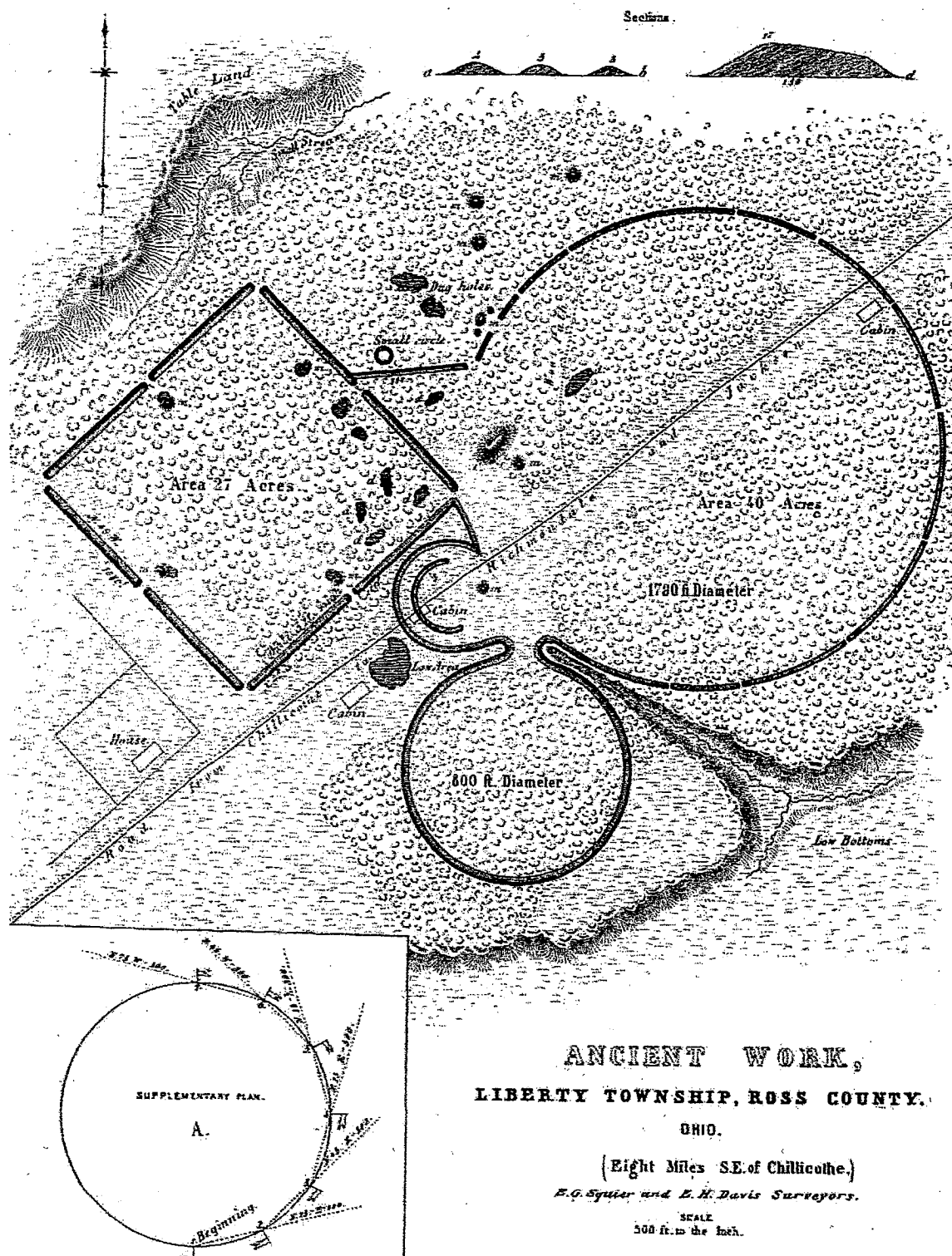


Figure 2.6. The "Liberty Works," a sacred enclosure with seemingly perfect circular and square elements. Not to original scale. Source: Squier and Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press, 1998 [1848]), plate between pp. 56-7.

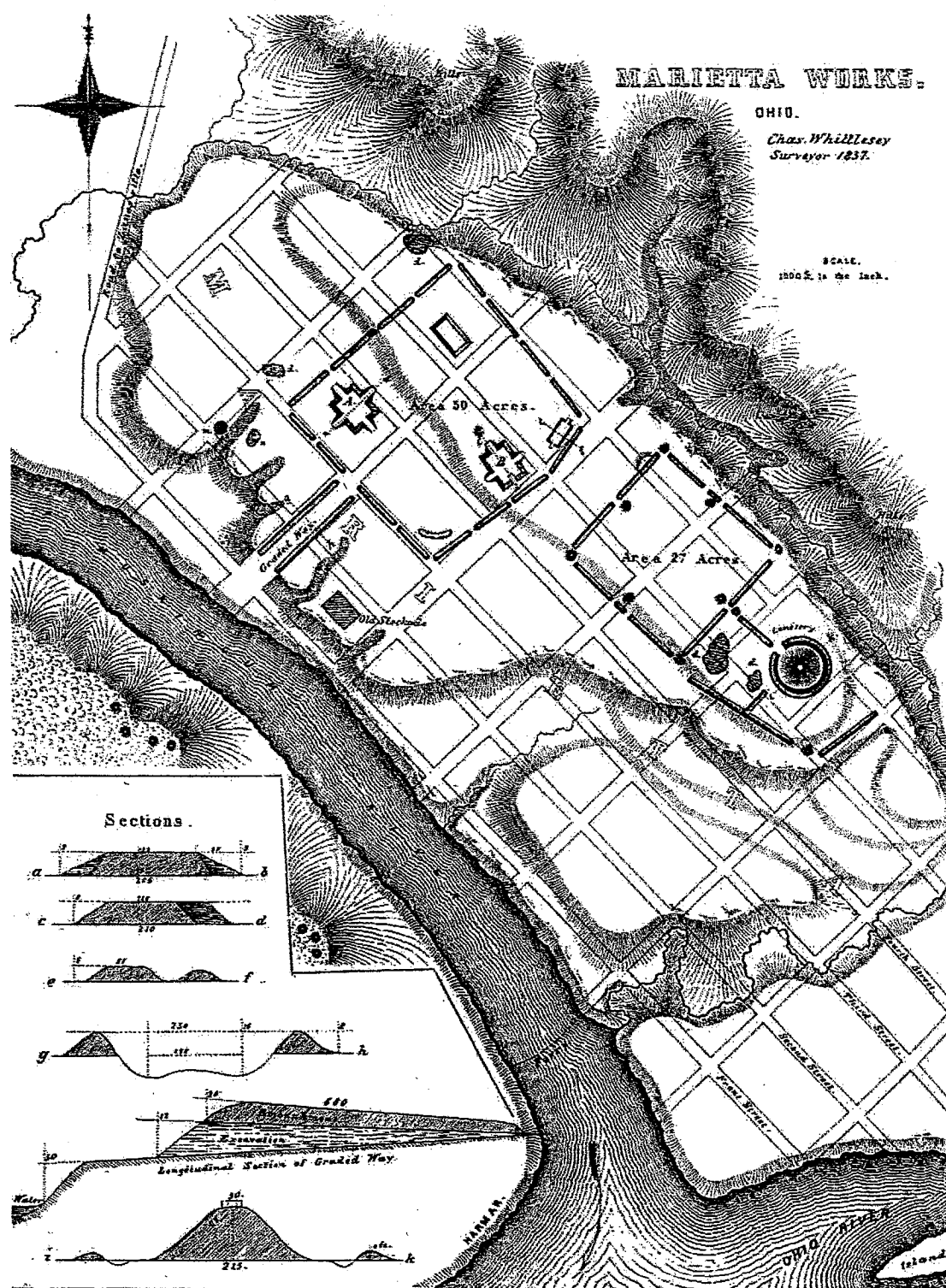


Figure 2.7. Marietta Works, a sacred enclosure. A view of the “Great Mound” (the circular feature at center right) is presented in Figure 2.10. This site was somewhat unusual in that the rectangular level-topped “temple mounds” (at “A” and “B”) were more characteristic of earthworks in the southernmost portions of the Mississippi Valley. Not to original scale. Source: Squier and Davis, *Ancient Monuments of the Mississippi Valley* (Washington, DC: Smithsonian Institution Press, 1998 [1848]), plate between pp. 72-3.

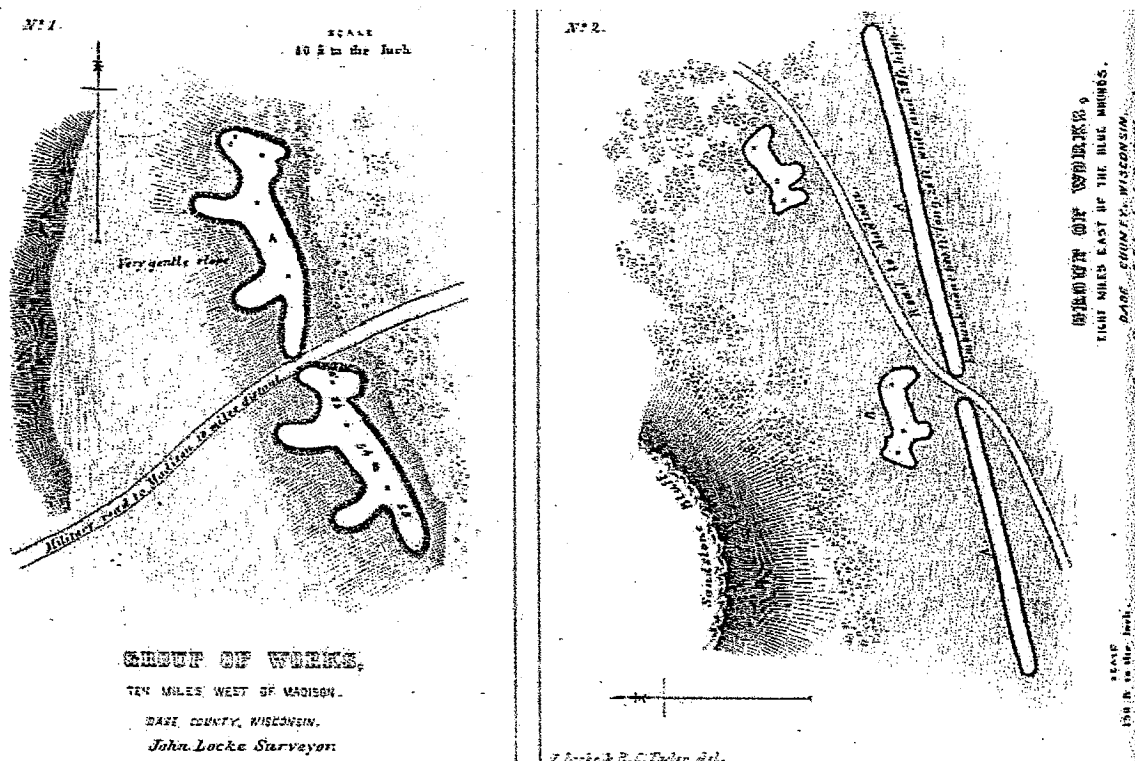


Figure 2.8. Examples of effigy works found in Wisconsin. Not to original scale. Source: Squier and Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press, 1998 [1848]), plate between pp. 126-7.

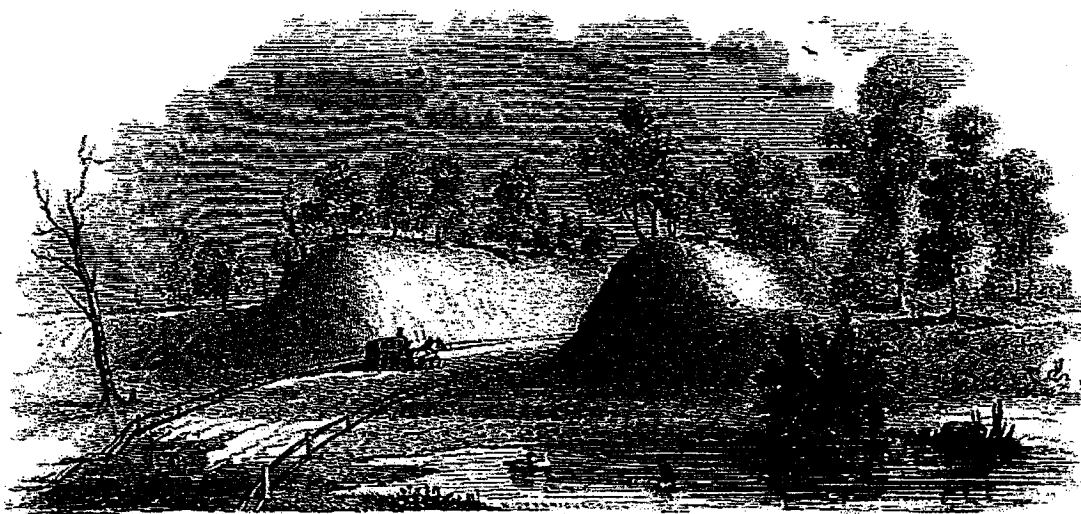


FIG. 20.—VIEW OF GRADED WAY NEAR PIKETON, OHIO.

Figure 2.9. The mouth of a moundbuilder “graded way” running some 1000 feet long and smoothly connecting river terraces of different elevations. Source: Squier and Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press, 1998 [1848]), p. 88.



GREAT MOUND AT MARIETTA, OHIO.

Figure 2.10. Romantic depiction of the Great Mound of the Marietta Works (see Figure 2.7). The image was reproduced from a privately owned painting. Interestingly, the area was used as the town cemetery. Marietta was one of the oldest frontier outposts east of the Appalachians, and served as the capital of the Northwest Territories. The earthworks located there were well known from the late eighteenth century. Squier and Davis's plan view of the site is intriguing because much of it had to be inferred. Substantial portions of the works had been destroyed by town development. From their depiction, one would hardly guess there was much of a town there when in fact there was. Source: Squier and Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press, 1998 [1848]), plate between pp. 138-9.

in circumference at the base. It was excavated by the proprietor in 1838. He

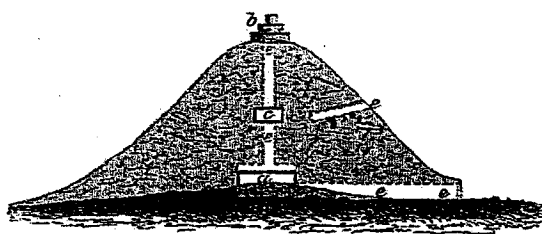


FIG. 55.

sank a shaft from the apex of the mound to the base, (*b a*, Fig. 55,) intersecting it at that point by a horizontal drift (*a e e*). It was found to contain two sepulchral chambers, one at the base, (*a*,) and another thirty feet above (*c*). These chambers had been constructed of logs, and covered with stones, which had sunk under the superincumbent mass

as the wood decayed, giving the summit of the mound a flat or rather dish-shaped form.* The lower chamber contained two human skeletons (one of which was thought to be that of a female); the upper chamber contained but one skeleton in an



FIG. 56.—GREAT MOUND AT GRAVE CREEK.

advanced stage of decay. With these were found between three and four thousand shell beads, a number of ornaments of mica, several bracelets of copper, and various articles carved in stone. After the excavation of the mound, a light three-story wooden structure was erected upon its summit. It is indicated by *b* in the section.

Figure 2.11. Page with depictions of Grave Creek Mound, near Wheeling, West Virginia. The owner had turned the site into something of a tourist destination in the late 1830s. Source: Squier and Davis, *Ancient Monuments of the Mississippi Valley* (Washington, DC: Smithsonian Institution Press, 1998 [1848]), p. 169.

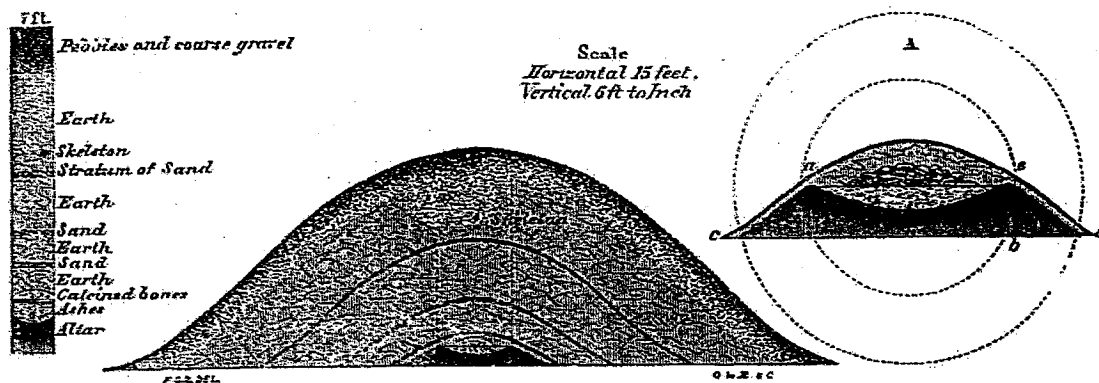
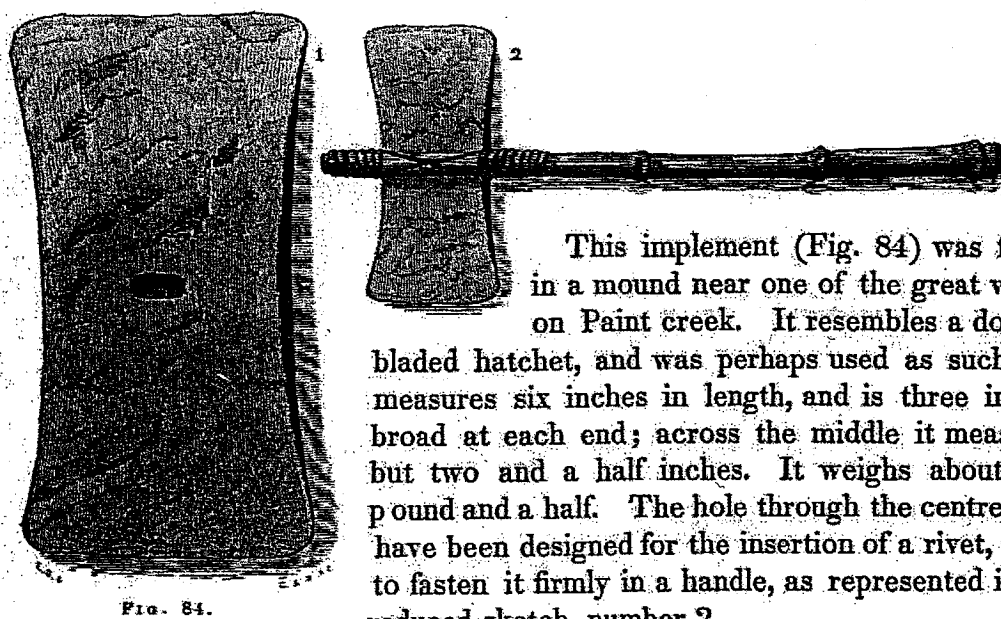


Figure 2.12. Profile of an "altar mound." A skeleton was located in the upper layer of the mound, but it was not directly associated with the burned clay basin and ashes located at the mound's core. Not to original scale. Source: Squier and Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press, 1998 [1848]), p. 144.



This implement (Fig. 84) was found in a mound near one of the great works on Paint creek. It resembles a double-bladed hatchet, and was perhaps used as such. It measures six inches in length, and is three inches broad at each end; across the middle it measures but two and a half inches. It weighs about one pound and a half. The hole through the centre may have been designed for the insertion of a rivet, so as to fasten it firmly in a handle, as represented in the reduced sketch, number 2.

Figure 2.13. A copper implement, presumed to be a hatchet. Source: Squier and Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press, 1998 [1848]), p. 199.

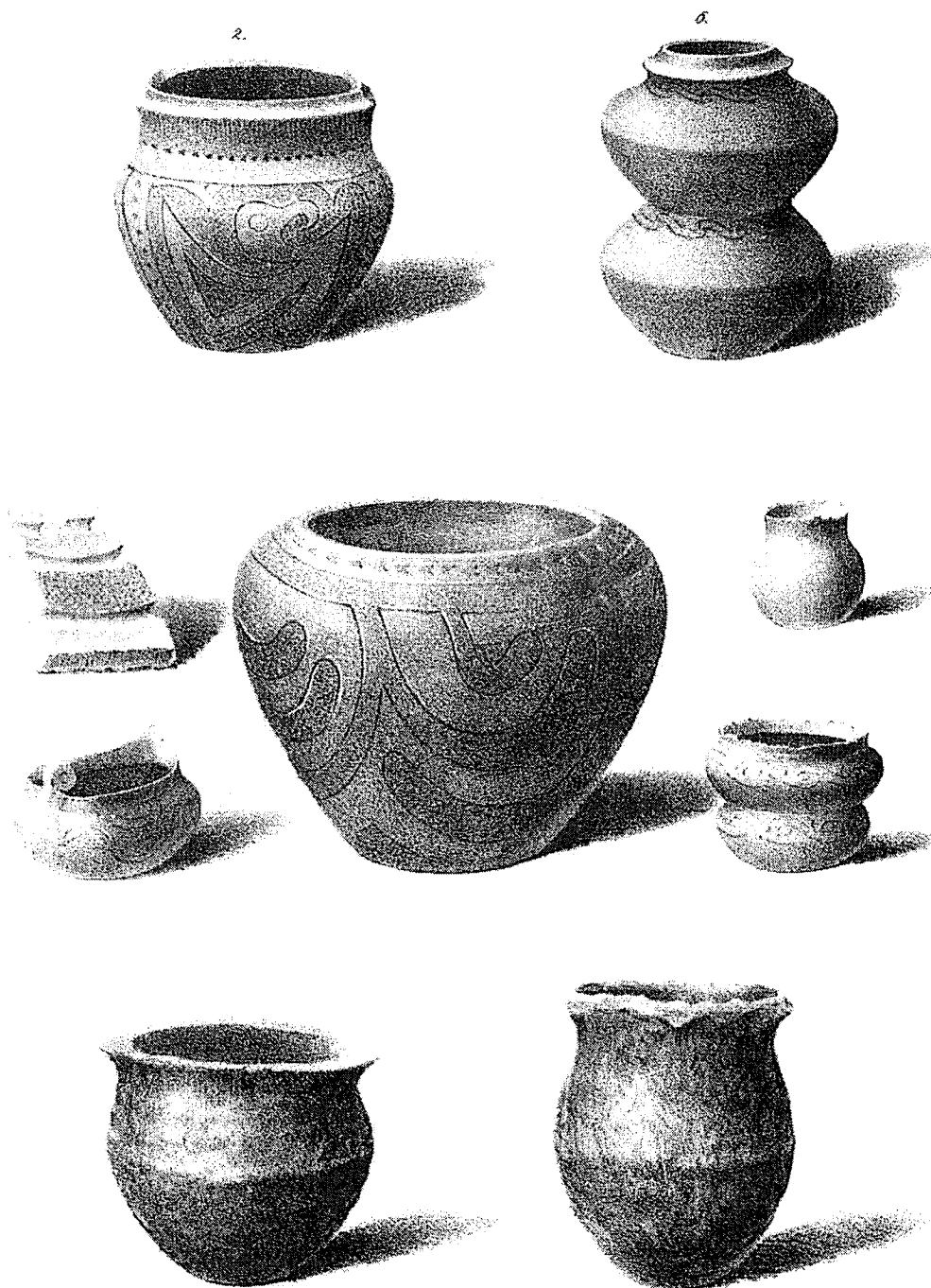


Figure 2.14. Examples of pottery from the mounds. As a testament to how unimportant provenance information was regarded to be, consider this: The text accompanying this plate consists entirely of descriptions of the objects themselves and tells readers nothing more specific than that these pieces came from Ohio, South Carolina or Florida mounds. On top of that, the illustrations are supposed to be individually numbered 1-9, but only the top two are labeled. Source: Squier and Davis, *Ancient Monuments of the Mississippi Valley* (Washington, DC: Smithsonian Institution Press, 1998 [1848]), plate between pp. 188-9.

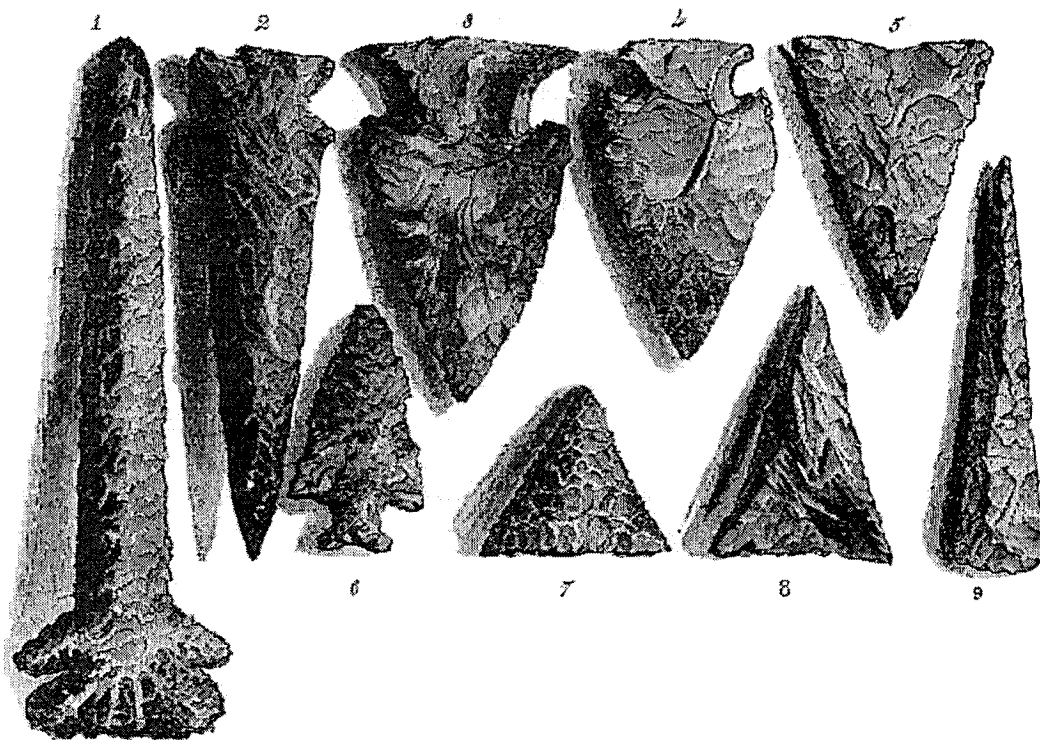


Figure 2.15. Examples of stone projectile points recovered from mounds. Source: Squier and Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press, 1998 [1848]), p. 212.

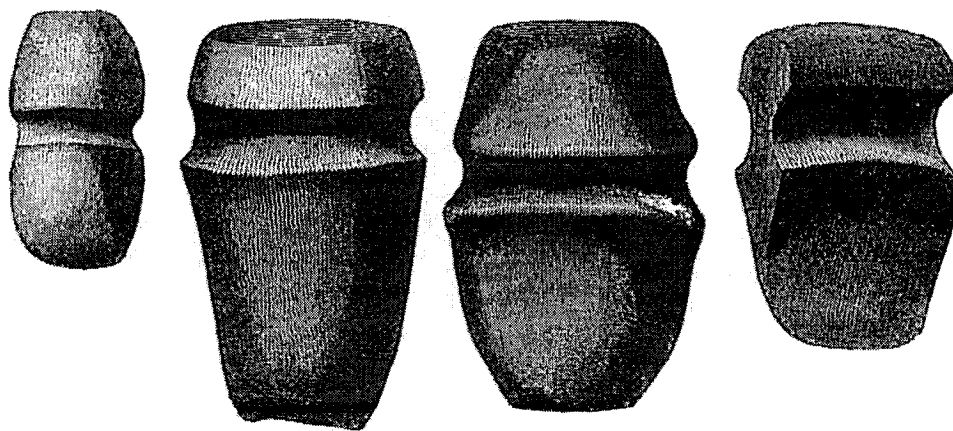


Figure 2.16. Examples of stone axes recovered from mounds. Source: Squier and Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press, 1998 [1848]), p. 217.

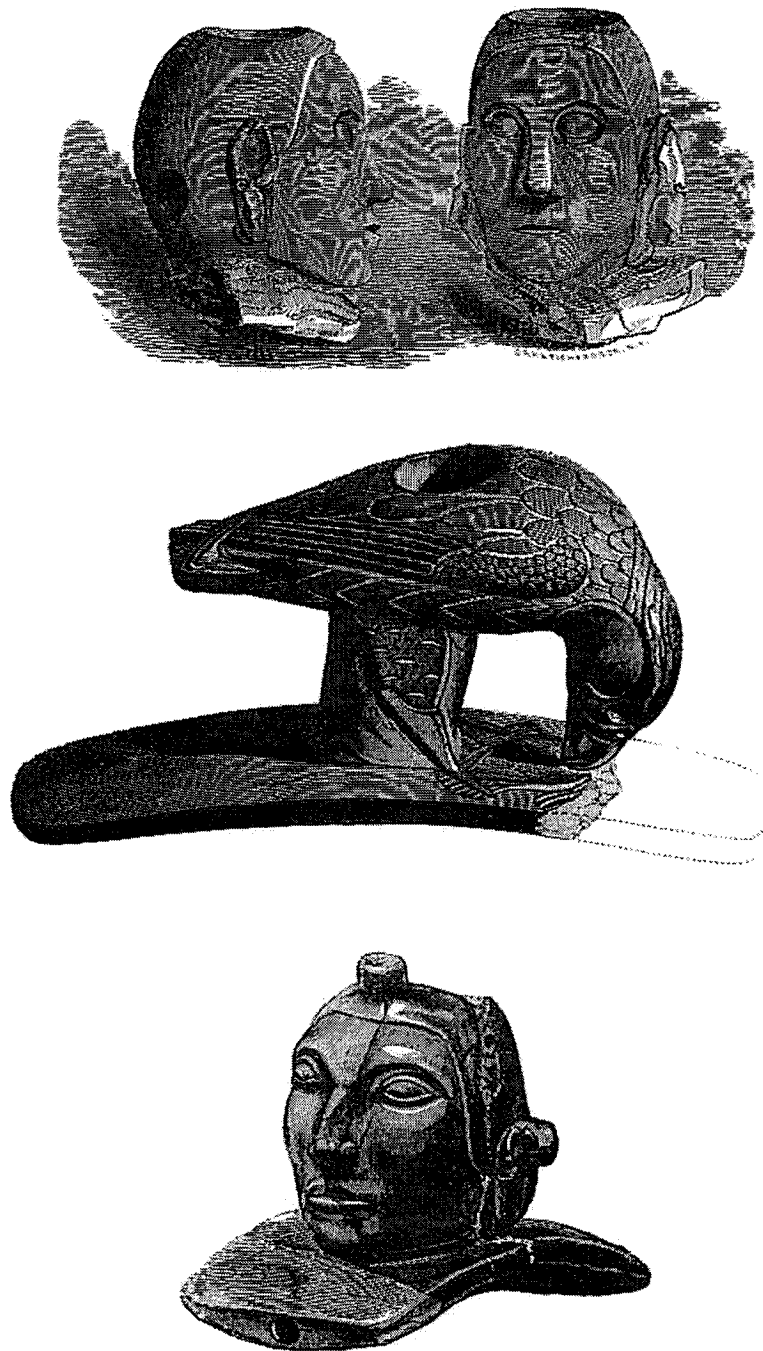


Figure 2.17. Three examples of carved stone pipes from mounds. Source: Squier and Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press, 1998 [1848]), pp. 245, 259.

depictions of specimens from other parts of the world for the purpose of comparison. This part of the monograph was as lavishly illustrated as the first half, and the accompanying textual descriptions of individual objects were often quite detailed. There was a chapter on pottery. Four chapters covered metal and stone or bone objects, which were each divided into subcategories of “implements” (tools, weapons, and so forth) and “ornaments” (decorative objects). Another chapter contained descriptions of sculptural objects, yet another on miscellaneous inclusions such as other metals, fossils, and minerals. As it turned out, the moundbuilders had a particular penchant for working with copper ore and were quite skillful at carving intricate hematite sculptures. A further chapter treated of the sole intact moundbuilder cranium recovered during the project. All told, Squier and Davis provided a remarkably detailed catalogue of specimens that attests to the great creativity and craftsmanship of their creators.

To the eyes of a modern reader, something is missing from these descriptions—context. With rare exception, Squier and Davis made no mention whatsoever of the locations of finds. The overriding assumption governing the treatment of specimens in Ancient Monuments was that these were all moundbuilder relics, and that it did not generally matter to distinguish artifacts taken, for example, from Cincinnati area sites from those found in around Chillicothe. Occasionally, the authors broadly distinguished between specimens from the upper Mississippi valley and those from southern regions, but even such general distinctions got blurred in the presentation (see, for instance, Figure 2.14 and caption). Once in a rare while, somewhat more specific provenance information was related, as in the authors’ discussion on “discoidal stones.” Here, readers were informed that two of the depicted specimens “were taken, in connection with

numerous other remains, from a mound numbered 1 within the great enclosure on the North fork of Paint Creek (see Plate X, and also page 157)."⁴⁰ Plate X, given much earlier in the text in the section on "Works of Defence," was a plan view of the "North Fork Works" (also known as "Clark's Works," after the lawyer on whose estate the expansive work was located). This large, rectangular enclosure contained numerous conical mounds; mound 1 was in the northeast corner. The second reference to page 157 led readers to the section on "Sacrificial Mounds," where some details about this particular mound were given—it was quite small, constructed of three distinct strata overlying an "altar" area noteworthy because it actually contained no altar, and its innermost stratum contained "hundreds of relics, and many of the most interesting and valuable hitherto found," including "several coiled serpents, carved in stone, and carefully enveloped in sheet mica and copper; pottery; carved fragments of ivory; a large number of fossil teeth; numerous fine sculptures in stone, etc." This was followed by the comment, "Notice will be taken of some of the most remarkable of these, under the appropriate heads," though only in the discussion on discoidal stones was any explicit reference made to the North Fork Works mound 1. Simply put, there was no way to determine from the text what even a representative sample of individual specimens from this seemingly rather important mound might have looked like.

The method of presentation thus utilized by Squier and Davis disarticulated their archaeological sites into distinct sub-classes of moundbuilder evidence that were not then related back to each other. In the example just given, the earthwork as a whole stood apart from the individual mound found within it, both of which in turn stood apart from

⁴⁰ Squier and Davis, Ancient Monuments, 221-2.

any specimens that were recovered. Earthworks, mounds, and artifacts were all moundbuilder productions, so to identify that a certain specimen had been recovered from a particular location within a particular mound was an extraneous and unnecessary detail, at least in terms of how this data was presented. This is not to say that Squier and Davis did not record more specific contextual information as part of field investigations. As I pointed out earlier, Davis paid meticulous attention to the internal structures and contents of mounds, since these details were critical to his designation of a non-funerary ceremonial function to some of them. The issue here has everything to do with the presentation of data, and even if, as we shall see, Ancient Monuments was advertised by the Smithsonian as a work of purely inductive investigation, there is no question that certain gross assumptions about the character of the moundbuilders framed the construction of the final published product. When archaeologists in the 1880s re-examined many of the sites first documented by Squier and Davis, there was a much greater emphasis on the necessity of keeping track of detailed contextual information, and that this information needed to be presented in published accounts of work as it was every bit as essential as any other form of data.

Returning to the actual work performed by Squier and Davis, field methods employed by the pair differed depending on the type of site they believed they were examining. Enclosures were surveyed and mapped. They were not excavated because they contained few artifacts and were considered mainly for their external appearance. Mounds on the other hand were excavated when possible, both to recover specimens and to examine interesting facets of their construction (see Figure 2.12).⁴¹ The fieldwork was completed

⁴¹ Squier and Davis, Ancient Monuments, 139.

between late 1845 and early 1847 in predominantly warmer months. Given the number of sites visited (and considering that both men had other professional obligations) they could not have spent more than a few days at any one site, so there is ample room to question the thoroughness of the work. Excavations were performed hastily, and surveying was often hampered by thick vegetation growing over earthworks. Various references are scattered throughout the text to give the reader a further sense of how important local connections were for the conduct of fieldwork. As the project got underway, notice of it spread and numerous individuals—landowning farmers especially—contacted Squier and Davis with information about the location of sites worth exploring. This kind of networking brought to their attention many sites that had never been documented before. We can also tell that Squier and Davis often hired locals as additional field hands for a days' work or two, usually for help in digging. Unfortunately, these people are never mentioned by name.

Beyond the fieldwork itself, remaining tasks were divided between the two men. Davis, in addition to financing the bulk of the project, cared for the specimen collection. Squier wrote the text and prepared the illustrations. Most of this was done in late spring and early summer of 1847, after he had moved from Chillicothe back to New York. At this point, relations had already begun to deteriorate between himself and Davis.

The general conclusions of Ancient Monuments are easily enough summarized. First and foremost, the mounds and earthworks of the Mississippi valley comprised, in Squier's words, "a single grand system, owing its origin to a family of men, moving in the same general direction, acting under common impulses, and influenced by similar

causes.”⁴² This population was “essentially homogeneous, in customs, habits, religion, and government,” it was exceptionally ancient (on the order of numbers of centuries old), and was quite numerous.⁴³ The large-scale construction of the monuments could only have been undertaken by a dense, sedentary, and agriculturally-based population of some considerable social complexity. The existence of “sacred enclosures” indicated a rather advanced state of religious development. There was also undeniably some connection between the “race of the mounds” and the “semi-civilized nations” of ancient Mexico, but this association, according to Squier, required further investigation.⁴⁴

A homogenous, semi-civilized race of the mounds. While Squier did not come right out and say so in the concluding chapter, there was no question as to what he thought about the association between these moundbuilders and historic or more recent Indians: there was none. Here was the message that most readers of Ancient Monuments would take away from it for the next thirty five years. The bulk of evidence and the manner in which it was presented in the text clearly supported this view. The evidence additionally included a rigid distinction between “moundbuilder” burials and more recent “intrusive” or superficial Indian graves with no allowance for a continuum between the two. Intrusive burials were those deemed to have no association with the original construction of the mound. They were uniformly dismissed as being associated with recent Indians,

⁴² Squier and Davis, Ancient Monuments, 301.

⁴³ Squier and Davis, Ancient Monuments, 301, 306. Squier and Davis considered evidence from tree growth on mounds (pp. 14, 16, 85, 305-6) and constructed an interesting, and somewhat complex, argument based on the absence of monuments upon the lowest (and most recently formed) river terraces (p. 304-5).

⁴⁴ Squier and Davis, Ancient Monuments, 301.

they were given little attention during fieldwork, and they merited barely a passing mention in the text of Ancient Monuments.

Squier's plan view maps, particularly of the sacred enclosures, had perhaps the greatest overall impact in terms of conveying an impression of the moundbuilders as an advanced, separate race. All of his plan views of enclosures have a rather otherworldly feel to them, especially in the way that embankment walls were often depicted in stark, surreal relief to the surrounding landscape. The sacred enclosures, such as the Newark Works or the Portsmouth Works pictured above, went a step further. These elaborate works were comprised of coordinated systems of linear and geometrically regular embankments. As Squier would have it, the circular and square structures were often perfect circles and squares, no mean feat considering the embankments in question often exceeded twenty feet in height and delineated areas of fifty acres or more.⁴⁵ He thought the only way such perfect engineering could have been accomplished was if the moundbuilders had mastered certain principles of geometry and were in possession of standards of measurement. One look at the plan views of some of these sites certainly conveys this impression.

Sacred enclosures were also taken to be sites of sophisticated religious ceremony. What evidence was there for this determination? First, they did not appear to be defensive structures because they were mostly located on low-lying areas and seemed too intricately designed for the purpose. Also, associated conical mounds, especially those that had been carefully spaced within enclosures, often seemed to be of the sort Davis had studied when the turnpike was built. That is, they contained burned clay areas thought to

⁴⁵ Squier and Davis, Ancient Monuments, 47-9.

be altars. In the end, sacred enclosures just had an aura of sacredness about them. No other explanation other than that these sites fulfilled some sort of ceremonial religious function adequately accounted for their structural appearance.

I should note that the supposedly perfect geometry of sacred enclosures became a matter of some controversy in the 1880s when a later generation of archaeologists from the Smithsonian re-surveyed some of Squier and Davis's sacred enclosures and found that they were not nearly so geometrically perfect after all. This was a problem both because Ancient Monuments remained such a widely cited reference, and also because the later Smithsonian archaeological authorities wanted to demonstrate unambiguously that moundbuilders and Indians were one and the same people, and not separate races. They were not so keen to make the moundbuilders look like an advanced race. These issues will be addressed further in Chapters 4 and 5.

2.5 The most sacred enclosure: the Great Serpent of Adams County.

Of all the supposed kinds of sites documented by Squier and Davis, none gripped the imaginations of subsequent archaeologists more than the sacred enclosures. And, as spectacularly intriguing as most of these sites appeared in print in Ancient Monuments, none was more spectacular than the Great Serpent of Adams County, Ohio (Figure 2.18). It had never been documented and was entirely unknown outside its secluded setting in a remote, south-central part of the state.⁴⁶

Sometime after the project got underway, Squier and Davis received a notice about the site in which the structure was described in “exceedingly vague and indefinite” terms,

⁴⁶ Squier and Davis, Ancient Monuments, 96.

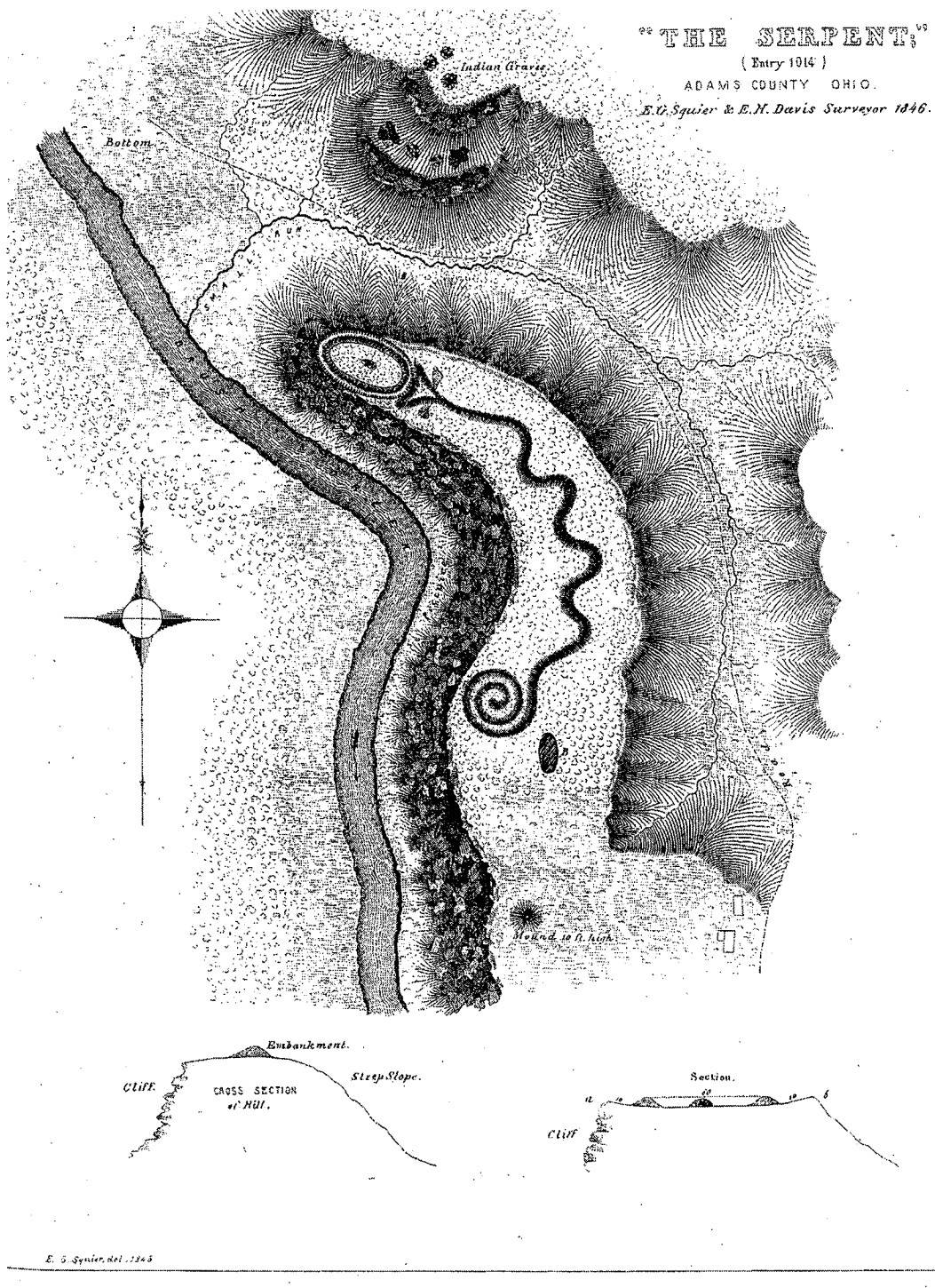


Figure 2.18. The "Serpent" of Adams County, generally thought to be the most significant Sacred Enclosure. Not to original scale. Source: Squier and Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press, 1998 [1848]), plate between pp. 96-7.

as though it were a hill-top defensive work “with bastions at regular intervals.”⁴⁷ What the two men found when they visited in late summer or autumn of 1846 was no fortress, but something far more unusual. Situated atop a ridge-spur overlooking Brush Creek, a meandering cobble-strewn Ohio River tributary, sat an earthwork in the form of an enormous serpent. Large, figurative effigy earthworks were somewhat common in Wisconsin, but none had been documented in Ohio. Besides, the Serpent was utterly unlike the Wisconsin effigies. It was of a much larger scale, it appeared to exhibit a very distinct relationship to its topographic setting that others lacked, it was a more elaborate construction, and, for various reasons, it appeared to have been a site of unparalleled ceremonial significance for the moundbuilders. It was, Squier and Davis thought, an utterly unique site not just in America but as far as they knew in all the world.

The two men probably spent no more than a few days there, but it was more than enough time for them to have created an indelible account of it for Ancient Monuments. Squier’s map depicts a principal serpentine embankment (“five feet high, by thirty feet wide” near the midpoint), terminating in a triple-coiled tail at the southern end that was “not less than one thousand feet” long if extended.⁴⁸ Topographically, the work was

situated upon a high, crescent-form hill or spur of land, rising one hundred and fifty feet above the level of Brush creek....The side of the hill next to the stream presents a perpendicular wall of rock, while the other slopes rapidly, though it is not so steep as to preclude cultivation. The top of the hill is not level but convex, and presents a very even surface, one hundred and fifty feet wide by one thousand long...Conforming to the curve of the hill...is the serpent.⁴⁹

⁴⁷ Squier and Davis, Ancient Monuments, 96.

⁴⁸ Squier and Davis, Ancient Monuments, 97.

⁴⁹ Squier and Davis, Ancient Monuments, 96-7.

At the northern extremity of the effigy, the “mouth is opened wide as if in the act of swallowing or ejecting an oval figure, which rests partially within distended jaws.” The oval, some one hundred and sixty feet long by eighty feet wide, might possibly represent a “circle, egg, or globe.”⁵⁰ Within the oval, the ground appeared to have been slightly elevated; at the center there appeared to have once been a large pile of burned stones that had long since been scattered about. Squier next describes a pair of “smaller triangular elevations ten or twelve feet over” jutting out from the base of the serpent’s head; these were “too distinct to be overlooked...yet too much obliterated to be satisfactorily traced.”⁵¹ South of the tail, the authors documented a strange “level oval terrace” (at point “B” on the map) and a small conical ten-foot-high mound further to south of that.

Squier and Davis performed no excavations at the site. They certainly did not expend much effort in examining the “Indian graves” plotted on the map to the north of the serpent across the ravine. As I also mentioned above, embankment works were generally thought to contain little by way of artifacts, and so they did not dig into the Serpent. The small conical mound was probably thought to be too small to be worth their while.

Besides, the two men thought the real value of the site lay elsewhere. It had all the hallmarks of a sacred enclosure, and more—the oval exhibited perfectly regular geometry, and the burned stones at its center were thought to have been part of an altar area.⁵² But no other known sacred enclosures exhibited such overt expressions of religious symbolism on anything close to the scale of the Great Serpent. Furthermore, the prominent setting of the work had to be more than mere coincidence. Not only was it

⁵⁰ Squier and Davis, Ancient Monuments, 97.

⁵¹ Squier and Davis, Ancient Monuments, 97.

⁵² Squier and Davis, Ancient Monuments, 97, 101-3.

prominently situated to overlook the creek valley below, but it seemed to have been constructed in direct relation to the landform itself. As we shall see in Chapter 6, ethnologists in the 1880s attached great significance to the apparent topographic symbiosis between serpent and landform, and claimed that the moundbuilders must have recognized a resident serpent spirit there. The problem was that these same ethnologists made such claims solely on the basis of Squier's map. At the same time, other archaeologists were revisiting the site and finding that Squier's depiction grossly misrepresented the true topography of the surrounding land.

These difficulties lay well in the future. By the time Squier finished preparing the final text of Ancient Monuments, he had begun to regard the Great Serpent as a site of immense ethnological significance. He had also become increasingly open to the idea of polygenic human origins. Well before the time he relocated to New York City in 1847, Squier was corresponding regularly with anatomist Samuel Morton. Morton by this time was fully endorsing polygenism, and he had high hopes that Squier and Davis's work in the Ohio valley would result in the recovery of numerous moundbuilder skulls.⁵³ As it turned out, Squier and Davis recovered only one in good condition (Figure 2.19). The brief chapter of Ancient Monuments devoted to this specimen was full of references to Morton's work and his craniometric data, all in the service of demonstrating that the moundbuilder skull approximated most closely to Morton's "Toltecan" family of the "American race." Morton and his followers took this discovery as evidence of the long-term durability of a distinct Toltecan type. The significance of this conclusion within the

⁵³ Meltzer, "Making of an Archaeological Classic," 11-2, 44; Barnhart, Of mounds and men, 61-7.

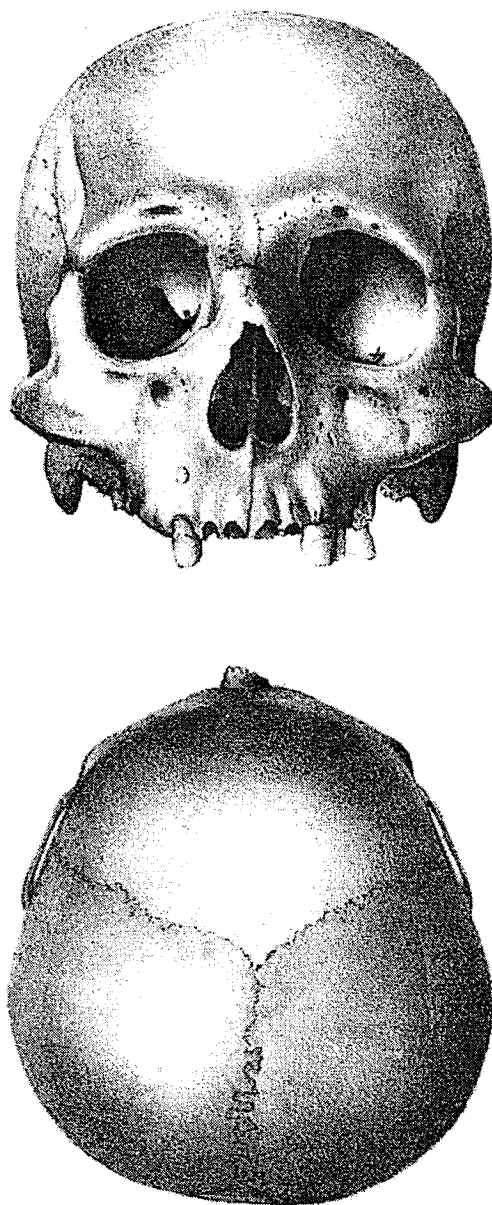


Figure 2.19. The sole intact cranium recovered from a “confirmed” moundbuilder grave. It was excavated from the central portion of a mound located immediately south of Chillicothe. Source: Squier and Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press, 1998 [1848]), plate between pp. 288-9.

often convoluted logic of polygenism is something we will consider in more detail in the next chapter.

While still in Chillicothe, Squier had also invited Egyptologist George Robins Gliddon to give a series of lectures there in May of 1847. The two men began a close and lasting friendship.⁵⁴ The author of Ancient Egypt (1844), Gliddon was one of Morton's followers—in fact, he was a close confidant and had provided the anatomist with many Egyptian and Nubian skulls.⁵⁵ Gliddon was also much more polemical and openly racist in arguing the polygenist cause, and he delighted in rousing controversy with his opposition to monogenism.⁵⁶ Along with Josiah Nott, he was also later responsible for assembling and publishing Types of Mankind under Morton's name after the anatomist died in the early 1850s.

Squier came to adopt a form of polygenism that was considerably “softer” than that of Morton's followers. Human groups may have been created separately, but all possessed the same essential innate capacity for mental development. Some groups had progressed further than others. Within a framework polygenism, Squier's appeal to the “psychic unity of man” offered a workable methodological principle: by comparing the appearance of certain mental traits from one group to another, one might be able to infer more general patterns of development and yield insights as to why some groups developed at different rates than others.

How did Squier go about doing this? By the time he finished Ancient Monuments, he had also begun to think that any given people's system of religious beliefs and practices

⁵⁴ Barnhart, Of mounds and men, 61 n. 33.

⁵⁵ Barnhart, Of mounds and men, 58.

⁵⁶ Barnhart, Of mounds and men, 61-2.

could be broken down into constituent parts and analyzed just as surely as could languages, except that religion offered a glimpse into a far more primeval condition than could other traits. Squier quoted from another scholar in support of his view along these lines:

of all researches that most effectually aid us to discover the origin of a nation or people, whose history is unknown or deeply involved in the obscurity of ancient times, none perhaps are attended with such important results, as the analysis of their theological dogmas, and their religious practices. In such matters mankind adhere with greatest tenacity, and though both modified and corrupted in the revolutions of ages, they still preserve features of their original construction, when language, arts, sciences, and political establishments no longer retain distinct lineaments of their ancient constitutions.⁵⁷

The sacred enclosures of the Ohio valley, Squier thought,

indicated the prevalence among their builders of religious beliefs and conceptions, corresponding with those which prevailed among the early nations of the other continent, and which *in their elements* seem to have been common to all nations, far back in the traditional period, before the dawn of written history.⁵⁸

In the serpent, Squier found what he thought was a most pervasive elemental unit of religious expression. It seemed truly to speak to the psychic unity of mankind.

The serpent, separate or in combination with the circle, egg, or globe, has been a predominant symbol among many primitive nations. It prevailed in Egypt, Greece, and Assyria, and entered widely into the superstitions of the Celts, the Hindoos [sic], and the Chinese. It even penetrated into America; and was conspicuous in the mythology of the ancient Mexicans, among whom its significance does not seem to have differed materially from that which it possessed in the old world. The fact that the ancient Celts, and perhaps other nations of the old continent, erected sacred structures in the form of a serpent, is one of high interest. Of this description was the great temple of Abury, in England,—in many respects the most imposing ancient monument of the British islands.⁵⁹

⁵⁷ James H. McCulloh, Researches, philosophical and antiquarian, concerning the aboriginal history of America (Baltimore: Fielding Lucas, 1829), 225. Quoted in Squier and Davis, Ancient Monuments, 304.

⁵⁸ Squier and Davis, Ancient Monuments, 304. Emphases added.

⁵⁹ Squier and Davis, Ancient Monuments, 97.

And here, on a remote hilltop setting in rural southern Ohio, was the largest known construction of serpent symbolism in all the world. It appeared in a compound form along with the oval and with what might have been wing-like projections jutting out from the base of the head.

In Ancient Monuments Squier only hinted at a possible future course of action, but it was clear that a broad comparative study of serpent symbolism was on his mind already. It would be, he wrote, “an investigation fraught with the greatest interest both in respect to the light which it reflects upon the primitive superstitions of remotely separated people, and especially upon the origin of the American race.”⁶⁰ For any readers who may have understood Squier’s perspective, and who read these passages with an awareness of the divide between polygenism and monogenism, these words carried a heavy weight of possibilities.

Squier could not develop his speculative views on comparative ethnology any further in Ancient Monuments. Joseph Henry, the ultimate editor of the volume, simply did not allow it because it violated his ideal of strictly empirical research for Smithsonian publications.⁶¹ Squier’s ethnology also seemed to touch directly upon the issue of race, which was not something Henry wanted prominently showcased by the Smithsonian. And so it was that Henry excised many of Squier’s more speculative passages even though Squier thought he had fairly presented his ideas without leaning toward any particular

⁶⁰ Squier and Davis, Ancient Monuments, 98.

⁶¹ The laborious editorial negotiations, particularly between Squier and Henry, are discussed in Meltzer, “Making of an Archaeological Classic,” 30-3, 44; also Rothenberg, et al., Papers of Joseph Henry, Volume 7, 272 n. 4.

theory (such as polygenism).⁶² In the end, Henry had his way, and Squier, angered by the censure, would have to present his theoretical views without the aid of the Smithsonian. Just a few years later, after having spent some time in Central America, Squier completed The Serpent Symbol, and the Reciprocal Principles of Nature in America, which was published in New York in 1851. This substantial work of comparative ethnology represented Squier's attempt at fulfilling the course of action to which he hinted in his entry for the Great Serpent of Adams County in Ancient Monuments.

It is worth pointing out that Henry's editing really had little effect. As it turned out, readers could interpret the conclusions of Ancient Monuments in different ways. Gallatin, the arch-monogenist, saw in Squier and Davis's researches the potential to elucidate "what man insulated, and almost without aid from intercourse from other Nations, can do by his solitary efforts," as well as evidence for the agricultural basis of civilization.⁶³ For him, Ancient Monuments shed light on how groups of people who became separated in the course of migration developed in response to new environments. Morton, on the other hand, liked the implications of antiquity for a singularly homogeneous native race, and particularly of the "Toltecan" family of that race. In his view, the Toltecan were the older and more advanced of two families, the other being the "Barbarous" to which modern Indians belonged. The one intact skull recovered by Squier and Davis was

⁶² For some of the editorial exchange between Henry and Squier, see Henry to Squier and Davis, 16 February 1848 in Rothenberg, et al., Papers of Joseph Henry, Volume 7, 270-2; and, Squier to Henry, 21 February 1848, in Rothenberg, et al., Papers of Joseph Henry, Volume 7, 278-80. Although Davis was included in some of the negotiations, he was increasingly left out in the wake of Squier's machinations.

⁶³ Gallatin to Henry, 16 June 1847, in Rothenberg, et al., Papers of Joseph Henry, Volume 7, 116-7.

eagerly taken up by Morton, but so too was the general portrayal of moundbuilders through their earthworks and artifacts as exceptionally advanced people.

2.4 Concluding remarks.

After its publication in 1848, Ancient Monuments was received in scholarly quarters with very high praise, in both America and Europe.⁶⁴ As one historian has put it, “Hyperbole aside, Ancient Monuments was instantly recognized as a milestone in American archaeology and, indeed, in American science. With the emphasis on ‘American’”⁶⁵ The relationship between Squier and Davis continued to break down, and they parted ways for good. Nonetheless, the end result of their collaboration unquestionably remained the authoritative document on the archaeology of the greater Mississippi valley for decades afterward. The scale of the project undertaken by the two men was unprecedented. Nowhere else had so much valuable archaeological information been assembled into a single text. The expensive production of Ancient Monuments, especially in terms of its profusion of maps and illustrations, without doubt contributed to its long-term impact. Of course, that production would not have been possible without the financial support of the Smithsonian Institution. However, the Smithsonian did more than merely facilitate publication. It gave Ancient Monuments an equally unprecedented imprint of scientific credibility.

Historians have paid much more attention to the post-Ancient Monuments trajectory of Squier than they have to that of Davis. In one sense this is not surprising. Davis published nothing of consequence related to archaeology, whereas Squier’s publication

⁶⁴ Barnhart, Of mounds and men, 160-5; Meltzer, “Making of an Archaeological Classic,” 51-4.

⁶⁵ Meltzer, “Making of an Archaeological Classic,” 52.

record after 1848 is respectably long. Squier spent time in Central America pursuing ethnological research and he returned to New York where he studied mounds and earthwork in the western part of the state. Davis may have kept to his own, but he did not abandon active involvement in archaeology. Decades later in 1884 he showed that he was very much aware of developments in the field when he wrote to archaeologist Charles Rau of the Smithsonian National Museum.⁶⁶ His main reason for writing was to correct erroneous claims made by British anthropologist John Lubbock. Lubbock, in a review of American archaeology written in the early 1860s, had mistakenly classified the sculptured stone pipes (see Figure 2.17, above) recovered by Squier and Davis as pottery. To Davis, this detail had significant interpretive consequences. A far greater amount of skill was necessary to carve such intricate objects out of stone than to have molded them out of clay. It was, Davis thought, a careless blunder made by an armchair theorist who spent little time in the field and who had never actually seen an American mound with his own eyes. The paper in which Lubbock made the error had been reprinted verbatim in the Smithsonian Annual Report for 1862, and was repeated in Lubbock's popular Prehistoric Times of 1865.⁶⁷ Twenty years later, after five editions of Prehistoric Times and after its translation into a number of different languages, the mistake was still there. Surprisingly, Lubbock would not have had to leave England in order to verify whether the pipes were stone or pottery, or to view many other moundbuilder artifacts. Davis's valuable collection was housed in Salisbury.

⁶⁶ Edwin Davis to Charles Rau, 28 May and 5 June 1884, SI/NAA MS 7065.

⁶⁷ John Lubbock, "North American Archaeology," Smithsonian Institution Annual Report for 1862 (1863), 318-336; John Lubbock, Prehistoric Times as Illustrated by Ancient Remains, and the Manners and Customs of Modern Savages (London: Williams and Norgate, 1865).

This brings us to a rather sad post-script to the Ancient Monuments story. As early as 1851, Davis wanted to find a good home for his collection, at the time the most important single group of archaeological specimens to be found in the United States. According to historian Terry Barnhart, it had become too much for him to take care of, and needed to be housed somewhere permanently. The objects represented years of work, and so Davis was looking to sell rather than donate them.⁶⁸ He wished the Smithsonian would take the collection. Henry certainly showed interest, but could do little more. There was at this time no official museum associated with the Smithsonian, even though the Institution had been obliged to take on collections from government surveys of the west. To spend money on collections, however, was not part of its mandate. Only after the 1876 Philadelphia Centennial would the promise of a funded National Museum come to fruition.

In late 1863, William Blackmore, a wealthy English entrepreneur with a robust interest in archaeological subjects, traveled to the United States and inquired about Davis's collection. Blackmore himself felt the objects belonged in their home country, but when no American buyers came forth he purchased the entire collection for ten thousand dollars.⁶⁹ It then became the focal point at Blackmore's newly-established museum in Salisbury, England for "illustrating the material progress of 'stone age' man

⁶⁸ Terry Barnhart, "An American Menagerie: The Cabinet of Squier and Davis," Timeline 2 no. 6 (December 1985-January 1986), 2-17, p. 6.

⁶⁹ Barnhart, "Menagerie," 7; Meltzer, "Making of an Archaeological Classic," 68.

in Europe and the Americas.”⁷⁰ Practical considerations certainly may have guided Davis’s decision to sell the collection, but ten thousand dollars is quite a lot of money.⁷¹ Whatever the case, the removal of this collection was seen for decades afterward by American archaeologists as a tragic event. Henry was truly distraught over the loss and he used the incident as a platform to scold Congress for not funding the purchase. At least he was able to secure from Davis a set of plaster casts of many of the artifacts.⁷² Davis, for his part, was also saddened that the collection would not remain in the United States, but the money surely eased the sorrow.

The Smithsonian set an important precedent in supporting the publication of Ancient Monuments of the Mississippi Valley. In hindsight, we might consider this level institutional involvement as an important step in a larger process of establishing boundaries between experts and hobbyists in archaeological matters. The names of Ephraim Squier and Edwin became synonymous with archaeological expertise, and yet

⁷⁰ Barnhart, “Menagerie,” 8; Meltzer, “Making of an Archaeological Classic,” 68. The collection ultimately made its way to the British Museum after Blackmore’s venture closed in 1931 (according to Barnhart). According to Meltzer, the British Museum purchased the collection in 1924.

⁷¹ There is reason to think that Davis may have sold his collection to Blackmore at least partly out of spite toward the Smithsonian. Keeping in mind that Davis and Squier parted ways largely because Squier seemed to be taking most of the credit for the project, I suspect that Davis held Henry at least a little responsible for facilitating Squier’s ambitions. In any case, in a communication with Henry that was printed in the Smithsonian Annual Report for 1866, Davis—after lamenting the sale of his collection—clearly states his intention to “again fill up my cabinet with specimens illustrating the development of the arts on this continent” (p. 370). I think Davis did genuinely wish to keep the collection in the United States, but nobody was offering as much as Blackmore.

⁷² Smithsonian Annual Report for 1866, 370; Annual Report for 1868, 27.

the men themselves were fairly representative of a large number of individuals who led similar lives and held similar interests and who were making archaeology happen. To our modern sensibilities, the loss of Davis's collection seems shocking. How could the Smithsonian publish Ancient Monuments but not take the collection, even if there was no official museum? The answer to this question is complicated but boils down essentially to this: before around 1880, this level of large-scale, coordinated centralization of archaeology did not exist. Monographs could be published, but there were no institutional resources to stop the ultimate dispersal of archaeological data. Not until after 1880 did the situation begin to change, largely through two institutions. The Smithsonian was one, but this time through a Mound Exploring Division within its newly organized Bureau of Ethnology. The other was the Peabody Museum of American Archaeology and Ethnology in Cambridge, Massachusetts. These institutions were able to exert much more effective control over archaeological practice, though as we shall see in Chapters 4-6, centralization was by no means an easy or straightforward process.

Chapter 3.

Indians, moundbuilders, race, and deep-time:

Evolving contexts for mid-nineteenth century American archaeology.

3.1 Introduction.

After the publication of Squier and Davis's Ancient Monuments of the Mississippi Valley in 1848, the Smithsonian continued to promote researches in the developing science of ethnology. Because it remained the most important American organization to do so until after the Civil War, it will continue to occupy a central role in the present account. From mid-century through the 1870s, the direction of Smithsonian ethnology—and American ethnology in general—was shaped by many factors, which included the following: the continued influence of polygenism; European discoveries attesting to the extreme antiquity of man; a growing alliance between archaeology and geology; the articulation and refinement of a concept of prehistory; and, the emergence of theoretical frameworks for understanding human history explicitly rooted in evolutionist theories of organic development. Notwithstanding the various cognitive developments in ethnology, the science in America also continued evolving in direct response to its socio-political context. This was especially the case at the quasi-governmental Smithsonian Institution. In the decade before the U.S. Civil War (1861-5), the topic of race received heightened attention in the nation's capital where discussions took place concerning the status of blacks as well as Indians in American society. Mid-century America saw the accelerating pace of western expansion, and, consequently, a continuation of Indian removal and the decline of tribal population numbers. The Civil War itself may have settled in principle the legal status of slavery, but for Indians, little would change. American ethnology

developed within such a remarkable set of contextual forces at mid-century that some scholars treated it as an entirely new science by the mid-1860s.¹

At precisely the same time, some European scholars, particularly in France and England, were even beginning to give the comprehensive science of man a new name—“anthropology.” Historians have addressed how anthropology was promoted in these contexts mainly as an anatomical science, precisely what its advocates claimed distinguished it from the “softer” science of ethnology.² In early 1860s England, a protracted conflict emerged between members of the Ethnological Society of London and a newer Anthropological Society of London. As Stocking points out, the conflict had political dimensions, and at its heart lay a seemingly unbridgeable divide between the ethnologists’ emphasis on the centrality of history in a science of man and the anthropologists’ emphasis on a physical science of race.³ Such particular disagreements aside, however, the terms “ethnology” and “anthropology” were used interchangeably just as often as not. One need look no further than anatomist Paul Broca’s account of the transactions of his Société d’Anthropologie de Paris during the first years of its

¹ Curtis M. Hinsley, The Smithsonian and the American Indian: Making a Moral Anthropology in Victorian America (Washington, DC: Smithsonian Institution Press, 1994), 44.

² See, for instance, George Stocking, Victorian Anthropology (New York: The Free Press, 1987), especially chapter 7; George Stocking, Race, Culture, and Evolution: Essays in the History of Anthropology (New York: The Free Press, 1968), especially chapters 2-3; Adam Kuper, “Anthropology,” and Elazar Barkin, “Race and the Social Sciences,” in Theodore M. Porter and Dorothy Ross, ed., The Cambridge History of Science, Volume 7: The Modern Social Sciences (Cambridge: Cambridge University Press, 2003).

³ Stocking, Victorian Anthropology, chapter 7.

existence.⁴ Broca, admired as a pioneer of craniometric techniques, was one of Europe's leading proponents of physical anthropology.⁵ Nonetheless, he saw his work as part of a comprehensive science of mankind, a science which he referred to as either anthropology or ethnology. As he outlined it, this grand science differed little in its essential components from the kind of scientific project promoted within Albert Gallatin's American Ethnological Society two decades earlier. In Broca's estimation, however, these components were to be held subordinate to the hard data of anatomical measurements.

In the United States, the context of the Civil War meant that scholars had to be cautious about the topic of race. "Anthropology" thus came into usage with an awareness of its European, physical overtones, but was used more or less synonymously with "ethnology" unless stated otherwise. In the late nineteenth century, two of the most important American "anthropological" institutions were the Peabody Museum of American Archaeology and Ethnology and the U.S. Bureau of Ethnology, both of which will be discussed in more detail in later chapters. As in the pre-Civil War era, ethnologists afterward still considered the moundbuilders as a central problem within frameworks for reasoning about human history in the New World.

This chapter will take as a starting point Smithsonian Secretary Joseph Henry's commentary on the importance of ethnology that appeared in the Smithsonian Annual

⁴ Paul Broca, "History of the Transactions of the Anthropological Society of Paris from 1865 to 1867," translated by C.A. Alexander, Smithsonian Annual Report for 1868, 376-91.

⁵ On Broca's methods of craniometry, see, Stephen Jay Gould, The Mismeasure of Man (New York: Norton, 1996), chapter 3.

Report for 1865. I will use his commentary to frame my own review of evolving contexts for ethnological approaches to the mounds in the 1850s and 1860s.

3.2 Lost histories.

On April 14, 1865, the four-year American Civil War had drawn to a close with the surrender of Robert E. Lee's forces to Ulysses S. Grant at Appomattox Courthouse, Virginia. The practicalities of reunification now suddenly fuelled political discussions in Washington. Joseph Henry, secretary of the Smithsonian Institution, had more than politics on his mind in the spring of 1865. He was, at the time, particularly frustrated for other more immediately pressing reasons. In January, a serious fire caused by an improperly installed stove pipe destroyed a large portion of the Smithsonian building and a fair amount of its contents.⁶ He had never been all that happy with the gothic-inspired structure to begin with, believing it rather extravagant in appearance for what he saw as the Institution's more sober research needs. But the Board of Regents and members of Congress had had their way years ago in the early stages of Smithsonian planning, and Henry often found himself expressing some concern at this state of affairs ever since. By the time the flames were doused, the upper floors of the main building and much of the north and south towers were largely ruined. Afterward taking stock of the situation, Henry stated clearly to the Board his view that their plan for the building had resulted in an edifice "much admired for its architectural effect...[but] too expensive to admit in its construction of the exclusive use of fire-proof materials."⁷ In effect, I told you so. At least

⁶ Joseph Henry, "Report of the Secretary," Smithsonian Annual Report for 1865, 14-5.

⁷ Henry, "Report of the Secretary," Smithsonian Annual Report for 1865, 14.

he now had better leverage to argue for additional Congressional appropriations for the upkeep of the building.

Structural damages could be repaired easily enough. The many valuable scientific apparatus and workshop tools lost to the flames were costly but for the most part replaceable (except for a few instruments of historic significance). Some losses, however, were permanent. Among those, the personal effects of James Smithson were destroyed, as were a number of in-storage library materials, including the entire public library of Beaufort, South Carolina (stored there for safe-keeping for the duration of the war under request of Secretary of War William Stanton). More devastating was the loss of the contents of the offices of the secretary, consisting basically of the entire set of records and correspondence of the Institution to date. Copies of 35,000 pages of outgoing correspondence and some 50,000 pages of incoming documents were forever gone. Records of experiments, diaries, manuscripts, account books, and various other personal papers, all lost. Many of the original plates for figures in Squier and Davis's Ancient Monuments of the Mississippi Valley were destroyed. Henry was crushed. In a flash, much of the history of the Smithsonian had been deleted.

But even that was not all. Although the museum and library proper both escaped damage, the Indian portrait gallery on the upper floor did not. The large collections of paintings by John Mix Stanley and George Catlin, the famous artists of the American West, were completely lost. Luckily, a similar collection of Catlin's works was housed in Philadelphia, but Stanley's unique portrayals of individual Indians could not be reproduced.⁸ The result of many years spent among Indian tribes somewhat earlier in the

⁸ Henry, "Report of the Secretary," Smithsonian Annual Report for 1865, 17.

century, Stanley's works—like those of Catlin—represented the artist's interpretations of the dress, customs, and general physical appearance of Indians whose tribal lifestyles were changing rapidly and irrevocably. Given the current situation out west, there was little chance that the portraits could ever be redone.

For two decades, the Smithsonian had been in the vanguard of American organizations promoting researches in the sciences, and ethnology—including archaeology—had been prominent among those. Under Henry's guidance, the Institution's influence in promoting ethnology came largely through its role as publisher, although the museum aspects of Smithsonian science uneasily gaining momentum as well. Henry did not particularly want the responsibility of a museum without additional guaranteed money from Congress, and yet materials continued to accumulate in the Smithsonian building. As a publisher, both through its more exclusive Contributions to Knowledge series and through its regular Annual Reports, the Smithsonian was creating a way for loosely knit communities of avocational ethnologists to pay attention to what they were each doing. Archaeology had been a central component of Smithsonian ethnological scholarship from the organization's earliest formative days when Henry took on Squier and Davis's Ancient Monuments. This was also the first major publication of any sort by the Institution. Henry remained keen to promote ethnology in 1865, except that he was by then far more willing to allow the explicit subject of race within Smithsonian publications and proceedings.⁹ In his report to the Board of Regents for 1865, Henry wrote that ethnological science

⁹ Hinsley, Smithsonian, 27-8; David J. Meltzer, "Ephraim Squier, Edwin Davis, and the Making of an American Archaeological Classic," introduction to Ephraim G. Squier and Edwin H. Davis, Ancient

has been a subject to which the Institution has given particular attention. Indeed, it is one which especially commends itself to the science and literature of this country, since it is intimately connected with our history and politics. Unfortunately, however, the subject of races is one which involves questions as to their origin and characteristics which can scarcely be discussed at the present time with that dispassionate logic and strictness of induction which is necessary to the establishment of truth. Still, much can be done in the way of collecting and recording facts which may serve as the basis of future investigation. Some of these, such as those relative to the Indians, are rapidly passing away into oblivion; and others, which regard the negro, require to be disentangled from much prejudice and misrepresentation. The peculiarities of these races should be critically examined and truthfully recorded; to do this is a duty we owe to science and humanity.¹⁰

Henry was perceived by many to be the most respected American physicist of his day, proud of the fact that his beloved Smithsonian openly promoted such a wide range of researches outside the exact sciences.¹¹ Benefactor James Smithson's original wish was that his money be used for the "increase and diffusion of knowledge among men," a statement whose exact interpretation subsequently required many years of debate in Washington after Smithson's death in 1829. When Henry was made secretary in 1846, he integrated a literal interpretation of Smithson's statement into his "Programme of Organization," a document that defined the philosophy of the Institution. According to

Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press, 1998 [1848]), 57-8; Robert V. Bruce, The Launching of American Science 1846-1876 (Ithaca, NY: Cornell University Press, 1987), 274-5.

¹⁰ Henry, "Report of the Secretary," Smithsonian Annual Report for 1865, 45-6.

¹¹ On Henry's status as a scientist, see Marc Rothenberg, Kathleen Dorman, John Rumm, Paul Theerman, ed., The Papers of Joseph Henry, Volume 6: January 1844—December 1846, The Princeton Years (Washington, DC: Smithsonian Institution Press, 1992), 553. At the time of his election as secretary, he was perceived to be America's leading scientist. Also, Henry's choice of an archaeological topic for the Institution's first big project was publicly controversial—Scientific American, for instance, reported disappointment that the money was being wasted on an insignificant subject (February, 1848).

Henry, Smithson was “fully impressed with the philosophical fact, that all subjects of human thought relate to one great system of truth.”¹² Henry whole-heartedly concurred—according to his Programme the Smithsonian would embrace physical sciences, subjects of a moral and political class, and literature and fine arts.¹³

Nineteenth century Smithsonian science, and much of American science in general, was underlain by a deeply moral sensibility. As it was for so many peers of his day, Henry saw the pursuit of science and letters in strongly moral terms: it improved the individual, it improved society, and it did not have to be incompatible with religion. It was *good* work. Every valid contribution added “new and important truths to the existing stock of knowledge,” and slowly aided the betterment of mankind. As he once remarked, “We may rest satisfied that religion and true science cannot be at variance; the one properly understood, and the other rightly interpreted, must agree in the final results.”¹⁴ In Henry’s view, ethnology was perhaps the one place where all of this best came together, “a common ground on which the cultivators of science and of literature might

¹² Smithsonian Annual Report for 1847, 179.

¹³ Smithsonian Annual Report for 1847, 175-6. It is worth noting the full range and classification of scholarship as Henry saw it. The “Physical Class” included: “1. Physics, including astronomy, natural philosophy, chemistry, and meteorology. 2. Natural history, including botany, zoology, geology, &c. 3. Agriculture. 4. Application of science to arts [presumably, technology].” The “Moral and Political Class” embraced: “5. Ethnology, including particular history, comparative philology, antiquities, &c. 6. Statistics and political economy. 7. Mental and moral philosophy. 8. A survey of the political events of the world; penal reform, &c.” Lastly, “Literature and the Fine Arts” consisted of: “9. Modern literature. 10. The fine arts, and their application to the useful arts. 11. Bibliography. 12. Obituary notices of distinguished individuals.”

¹⁴ Smithsonian Annual Report for 1868, 33.

harmoniously cooperate.”¹⁵ In fact, in no other department of Smithsonian science did the moral imperatives of the nineteenth century shine through so clearly as they did through its ethnology. It was as true in 1848 as it was for some time after 1879, when the science was fully institutionalized within a separate Bureau of Ethnology under John Wesley Powell.¹⁶ After the Civil War, Henry felt better situated to direct Smithsonian science to race questions, particularly since the rise of polygenism had left many unanswered questions.

Polygenism was not merely some historical aberration of ethnological thought, a short lived and unpopular “wrong turn” during the rise of anthropology that disappeared after the appearance Charles Darwin’s Origin of Species. Most if not all commentators on the history of American anthropology certainly recognize that polygenism—especially through Samuel Morton and his followers—played a significant role during the 1840s and 1850s in offering an alternative theory of human diversity, one that came to be criticized for its racist as well as its heretical implications. In a nation of immense religious pluralism and deeply rooted Christian fundamentalism of many forms, the “higher criticism” emanating out of Germany did not have quite the same effect in America as it did in Europe. Many God-fearing Americans got their first exposure to the implications of biblical revisionism through the writings of a few men who claimed to be

¹⁵ Smithsonian Annual Report for 1868, 26.

¹⁶ The best work on the moral underpinnings of Powell’s Bureau of American Ethnology is Curtis Hinsley, The Smithsonian and the American Indian: Making a Moral Anthropology in Victorian America (Washington, DC: Smithsonian Institution Press, 1994). Hinsley’s outstanding account focuses on the linguistic and ethnographic aspects of the Bureau’s work; mound archaeology is not considered in any depth even though an official mound survey was undertaken within the Bureau under Cyrus Thomas.

furthering ethnology through polygenism. Polygenist ethnology was such a crucial facet of nineteenth century American ethnological thought that the subsequent history of American ethnology or anthropology must be viewed in some sense as a reaction to it. And by reaction, I do not simply mean a refutation. The polygenists' arguments were complex and sometimes shrouded in personal opinions that were ugly. These men were masters of synthesizing vast amounts of scientific and historical data in cunning ways. And, although general public reaction was swift, it took some time for scholars to fully work out the implications of what the polygenists were saying. This is important to a history of mound archaeology because the concept of a race of the mounds had served the causes of polygenism very well.

In an earlier chapter, I described how ethnology emerged in the earlier decades of the nineteenth century as an all-inclusive science of man, characterized predominantly by an historical approach to understanding such problems as the distribution of languages, myths, archaeological remains, or even of anatomical characteristics. This historical approach was generally rooted to a biblically-oriented monogenic theory of human migration and diversification within a relatively short time frame. The idea of human unity formed the central paradigm of the earlier ethnological model, and it reached its greatest expressions in the writings of scholars like James Cowles Prichard in England, and Albert Gallatin in America.¹⁷ According to the theory of human unity, perceived similarities between different human groups could be interpreted as indicators of common historical origins. In the American setting, it was largely Samuel Morton's

scientific work, equally rooted in comparative anatomy and phrenology, that shifted the focus of ethnology more strongly toward biology. For an increasing number of scholars, the hard data of comparative anatomy seemed far more reliable than vague descriptions of language structures or religious beliefs. Morton himself, as I have pointed out, did not at all reject the “softer” forms of ethnology, but rather saw his brand of physical ethnology as a complement to an overall science of man which sought to understand some basic principles regarding human history. In his day, as I pointed out in Chapter 1, Morton was very well-respected by his scientific peers for his anatomical work. It was his developing ideas on the polygenic origins of man that drew increasing response, although publicly Morton tried to keep religion and politics out of his science.¹⁸

Even before coming to Henry’s attention in the 1840s, the importance of Squier and Davis’s project for illuminating the unity of man problem had been elevated by both Albert Gallatin and Morton.¹⁹ Morton, particularly, wanted to establish the stability of an

¹⁷ George Stocking, “Paradigmatic traditions in the history of anthropology,” in R.C. Olby, G.N. Cantor, J.R.R. Christie, and M.J.S. Hodge, ed., Companion to the History of Modern Science (London: Routledge, 1990).

¹⁸ Stephen Jay Gould has pointed out that Morton’s methodology for measuring cranial volumes was at least sometimes deeply flawed. See Stephen Jay Gould, The Mismeasure of Man (New York: Norton, 1996), 82-104. We must keep in mind, however, that comparative anatomy was a rising science of its day, and Morton had studied under some of the best of anatomists in Europe. Whatever assessment we may level on Morton in hindsight, he was a remarkably well-respected scientist in his day.

¹⁹ Both men participated in the review process for Ancient Monuments. See David Meltzer, “Ephraim Squier, Edwin Davis, and the Making of an Archaeological Classic,” introduction to reprint of Ephraim G. Squier and Edwin H. Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press, 1998[1848]), 23.

“American” racial type over time, and archaeological studies of the ancient mounds (which might uncover skulls among other clues) could help greatly. Henry saw the relevance of the mound project along these lines, but he also worked to edit out any overtly theoretical speculations in the text. Squier himself had associated with Morton prior to publication and was working out his own brand of polygenism. (Squier’s view differed from the “strong” polygenists’ in that, although he accepted the idea of multiple points of creation, he still maintained that all humans possessed a shared psychological nature—he still maintained unity in diversity.) Long before the end of the Civil War, Henry wanted to maintain an aura of neutrality on the topic within the Smithsonian.²⁰ Appearing to take sides might be perceived to have some bearing on the slavery question, and thus inevitably entailed becoming enmeshed in politics. This was not a wise move for someone in charge of a national institution in the decade or so prior to the outbreak of the war.

Morton died in 1851, but the ideological legacy of his “American school” of polygenist ethnology did not fade away, even if many in the pro-slavery south and elsewhere disagreed with the theory for religious reasons. For many southern Americans, the strong polygenist refutation of the Bible on chronological grounds was too offensive to take seriously.²¹ Under the guidance of Josiah Nott and George Gliddon, two of

²⁰ See, Hinsley The Smithsonian and the American Indian: Making a Moral Anthropology in Victorian America (Washington, DC: Smithsonian Institution Press, 1981) 27-8; Meltzer, “Making of an American Archaeological Classic,” 57-8; and, Bruce, Launching of American Science, 274-5.

²¹ Morton’s argument, based on evidence such as that derived from Egyptian hieroglyphs found in his Crania Aegyptica, was that racial types had been stable for at least several thousand years. Accordingly, a

Morton's most hard-edged followers, Types of Mankind appeared in 1854. This was a major synthesis of American polygenist thought.²² Nott was a true son-of-the-south, an Alabama surgeon who had studied under Morton and was known for his hellfire-and-brimstone ability to deliver his message to a willing audience. Gliddon, an Egyptologist with interests in the archaeology of classical remains, had an skillful command of biblical and Old World history and languages. He collected many skulls from Africa for Morton's collection. He also gave traveling lectures on these subjects, one of which took place in Chillicothe, Ohio by invitation of Squier in 1847.²³

In late 1848, by invitation of a professor of political economy at the University of Louisiana, Nott gave two lectures to the Louisiana legislature that formed the basis for his 1849 book, Two Lectures on the Connection Between the Biblical and Physical History of Man.²⁴ Publication of the book was encouraged by many of his listeners at those sessions. The title speaks volumes, for here, Nott sought to integrate recent developments in ethnology with a revisionist interpretation of the bible in order to

literal Biblical chronology was vastly insufficient to account for the diversification of races (from the Tower of Babel) under a monogenic theory of diversification.

²² Josiah Nott and George Gliddon, Types of Mankind: or, Ethnological Researches Based Upon the Ancient Monuments, Paintings, Sculptures, and Crania or Races, and Upon Their Natural, Geographical, Philological, and Biblical History (Philadelphia: Lippincott, 1854). There were others in the so-called "American school," but Nott and Gliddon were by far the most active and vocal.

²³ Terry Barnhart, Of mounds and men: The early anthropological career of Ephraim George Squier, diss., Miami University, 1989 (Ann Arbor: UMI 1989, T24741), 58 and 61-2.

²⁴ Josiah C. Nott, Two Lectures on the Connection Between the Biblical and Physical History of Man (New York: Negro Universities Press, 1969). Originally published by Bartlett and Welford, 1849.

discredit “the mooted question of *unity* of species.”²⁵ Having lived in the South all his life convinced him that deep moral and intellectual differences existed between blacks and whites. By his own reckoning, twenty-five years of studying the diseases, anatomy, and physiology of black and white southerners had convinced him of the truth of multiple seats of human creation.²⁶ In the book, Nott haphazardly surveyed evidence from geology, comparative anatomy, phrenology, biblical history, and other forms of ethnology from around the globe in support of polygenism, evidence he used to support the assertion of qualitative differences in mentality existing between races. Even the moundbuilders (who were “very much in advance of the strictly savage tribes”) were brought into the discussion.²⁷ Although described as intellectually superior to Indians, they were yet “immeasurably below the Caucasian race in civilization,” and nevertheless part of a highly variegated family of American races.²⁸

Although Nott’s aim was to demonstrate the universality of polygenism, Two Lectures was as much about the author’s concern over the differences specifically between whites and blacks at a time in the nation’s history when these differences were increasingly being called into question. Consequently, the text is often suffused with a white Southerner’s sense of impending unease and isolation. For example:

A fearful crisis must come, sooner or later. With three millions of negroes crowding down upon the Gulf States and rapidly increasing—cut off, too, as we are from the sympathies, not only of the rest of the world, but of our own government, which should have some parental feeling for us—what, let us ask,

²⁵ Nott, Two Lectures, 6. Italics in original.

²⁶ Nott, Two Lectures, 5.

²⁷ Nott, Two Lectures, 32.

²⁸ Nott, Two Lectures, 32-3.

will be the condition of our children before three score and ten years have run out?

The time must come when the blacks will be worse than useless to us. What then? *Emancipation* must follow, which, from the lights before us, is but another name for extermination.²⁹

As utterly incomprehensible as it may seem, however, Nott did not consider himself to be in favor of slavery. He was so fully convinced that blacks were suited *by nature* and according to the “laws of God” for being enslaved that to set them free would only send them onto a path of inevitable self-destruction.³⁰ The “slaves of the South,” Nott pronounced,

as a mass, are more intelligent, more moral, and more happy than any free negroes on the face of the globe; and yet we are taunted and insulted, by Northerners and Europeans, for perpetuating this institution.³¹

The twisted logic of polygenism could be employed very craftily, indeed.

It is worth noting, finally, some of Nott’s comments regarding biblical revisionism, because they point to something deeper about the impact of his work that is less than apparent on the surface. He articulated high praise for Germany, the home of the “best educated people of the Old World” and birthplace of the Reformation, where “philology, archaeology, and all those studies which form the groundwork of biblical criticism have been most advanced.”³² Nott believed it was time to bring it home—really *home*—to America. Although biblical criticism had found some notable proponents among a handful of New England theologians, Nott saw himself as offering something more than a

²⁹ Nott, Two Lectures, 18.

³⁰ Nott, Two Lectures, 19-20.

³¹ Nott, Two Lectures, 20.

³² Nott, Two Lectures, 16.

new way of thinking about the bible and human history. He was making biblical criticism *applicable* to the ills of American society, and he was marching it right into the halls of government. This is why his views were so dangerous.

Where Two Lectures presented Nott's views in a rather condensed format, Types of Mankind was an expansive 738-page defense of the stability and distinctness of racial types. It was built largely upon a series of unpublished and unedited manuscripts by Morton, and bolstered with writings by Nott and Gliddon themselves, as well as by Louis Agassiz and William Usher. This monument to the deceased anatomist is probably most famously remembered for its contribution from renowned Harvard zoologist Agassiz, whose sketch "On the natural provinces of the animal world and their relation to the different types of man" lent the work scientific respectability above and beyond that given by Morton's name alone.³³ The other notable contribution came from Usher, an Alabama physician whose extensive knowledge of recent developments in geology formed the basis of a chapter on "Geology and palaeontology, in connection with human origins." What made Types so controversial overall was the manner in which Nott and Gliddon situated the full import of Morton's anatomical theories about racial types—including his phrenology-derived scale of mental capacity based on cranial volumes—within their own radically revisionist critique of Mosaic chronology and Biblical literalism in general. Morton's own words were heavily edited and often qualified, usually by Nott.

³³ Meltzer, "Making of an American Archaeological Classic," 54-8.

Joseph Henry, for one, found the work exceptionally distasteful and thought Nott and Gliddon had slandered the names of Morton and Agassiz.³⁴ He also did not like the fact that data from Ancient Monuments of the Mississippi Valley was prominently deployed within the polygenists' argument for the stability of an autochthonous American racial type from ancient times to the present.³⁵ Chief among the archaeological evidence was the single moundbuilder skull recovered by Squier and Davis from a prominently situated sepulchral mound four miles south of Chillicothe.³⁶ This lone cranium had been analyzed by Morton for Ancient Monuments. Morton thought it represented a perfect example of the Toltecan form, one of two great families within his American race. Nott and Gliddon followed Morton's subdivision of the American type into the "semi-civilized" Toltecan and the "restless and warlike" Barbarous families.³⁷ Archaeology gave the authors further leverage to claim that the moundbuilders were an agricultural people who existed at least as early as the time of the Egyptian pharaohs.³⁸ Most likely, they thought, these early Toltecans migrated southward to flourish in Central and South America. Archaeology was not the only source of evidence employed from outside the realm of anatomy here—

³⁴ Meltzer, "Making of an American Archaeological Classic," 57-8.

³⁵ Nott and Gliddon, Types of Mankind, 271-298. Nott and Gliddon referred to Ancient Monuments as Squier's work—Davis was never acknowledged. Nott and Gliddon also included lengthy discussions on the "Negro types," "Egyptians," and "Caucasians."

³⁶ For a description of the mound, its excavation, and the recovery of the skull, see Squier and Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press, 1998[1848]), 288-9.

³⁷ Nott and Gliddon, Types of Mankind, 276-7. The classification, following Morton, did not include the northernmost "Eskimaux" who were not seen as part of the autochthonous American race.

³⁸ Nott and Gliddon, Types of Mankind, 286-7 and 273.

monogenist Albert Gallatin's philological research was turned on its head in the service of polygenism. Whereas Gallatin's quest for linguistic unity among American Indians was intended ultimately to demonstrate the diffusion of language into the New World from Asia, Nott and Gliddon read it at face value.³⁹ The unity of American Indian languages merely proved the autochthonous nature of those tongues.

Surprising as it may seem, Nott and Gliddon allowed for a significant degree of plasticity within this system. In order to account for the diversity found within real human populations, the authors brought Agassiz's theory of nature (to which Morton subscribed) to bear on the matter: "all mankind do not spring from one pair, nor even each race from distinct pairs; but...were created in *nations* [of individuals], in the different zoological provinces where history first finds them."⁴⁰ Within zoological zones, there was room for a certain range of individual variability as well as mixing by "amalgamation" of blood. Inter-zonal mixing gradually homogenized the type. But individual humans often departed their native zoological provinces for others to which they were less well-adapted. Such movement generally met with physical and mental deterioration.⁴¹ So far, this was a fairly straightforward creationist view of nature's economy, in which degenerationism played a significant causal role.

Beyond this much, however, Nott and Gliddon's reasoning became considerably more complicated. In overall statistical terms, the authors stressed that the basic

³⁹ Nott and Gliddon, *Types of Mankind*, 283-4.

⁴⁰ Nott and Gliddon, *Types of Mankind*, 283, emphasis in original. Agassiz had developed a theory of nature based on the principal that every distinct species of organism was specially created by God to occupy a specific geographic zone to which it was suitably adapted.

⁴¹ Nott and Gliddon, *Types of Mankind*, 79.

primordial types persisted over time, as shown, they claimed, by history. At the very least, these types represented not just races but distinct human species.⁴² At this point, it would do well to emphasize that Nott and Gliddon's reasoning on human types was underpinned by the view that strict qualitative differences existed between them. This much, they thought, had been proven by Morton's craniometrical analyses, studies which demonstrated the supposedly superior intellect of the white Teutonic form, measuring in with a mean cranial volume of ninety-two cubic inches. "Negroes" measured in at eighty three, the "American type" at seventy-nine.⁴³ Could intermixing occur between different fixed types? The answer was yes. The addition of a small amount of black blood should in principle help the white man adjust to more tropical climes. By the same token, white blood improves the intellectual and moral character of blacks.⁴⁴ Although mixing was possible, Nott and Gliddon nonetheless denied that successful hybridization broke down the distinction of types.⁴⁵ Hybridization was not for them an open-ended and cumulative process. Instead, it resulted in the swamping of inferior characters—and ultimately, too, of inferior races. As long as human types persisted within their God-given natural provinces, they would remain stable and statistically true to form. The principal obfuscating factor was that white races, "the representatives of civilization," had been restless in recent centuries and prone to fits of colonization.⁴⁶ No matter, however, as this was part of the ordained order of things. "The Creator has implanted in this [the

⁴² See especially their discussion on human classification, pp. 80-7.

⁴³ Nott and Gliddon, Types of Mankind, 454.

⁴⁴ Nott and Gliddon, Types of Mankind, 68.

⁴⁵ Nott and Gliddon, Types of Mankind, 74.

⁴⁶ Nott and Gliddon, Types of Mankind, 77.

Caucasian] group of races an instinct that, in spite of themselves, drives them through all difficulties, to carry out their great mission of civilizing the earth. It is not reason, or philanthropy, which urges them on; but it is destiny.”⁴⁷ In America, blacks and Indians were doomed to extinction. All one had to do was look west to see “as clear as the sun at noon-day, that in a few generations more the last of these Red men will be numbered with the dead.”⁴⁸ They “are fading away before the exotic races of Europe...[and] no philanthropy, no legislation, no missionary labors, can change this law: it is written in man’s nature by the hand of his Creator.”⁴⁹

Nott and Gliddon may have castigated the monogenists as “Theological Naturalists” in contradistinction to “Naturalists proper,” but it should be apparent that this is no way meant they had abandoned God. Their problem was with literal interpretations of the bible; God’s rigid laws—which operated “not through a few thousand years, but throughout eternity”—still ruled supreme in nature.⁵⁰

To emphasize the extreme pre-biblical antiquity of human existence, the authors included Usher’s chapter, a skillful litany of recent developments in geology and palaeontology. Frequently overlooked by historians within the context of Types of Mankind, Usher’s contribution to the book is in many respects the most important. Marshalling extensive evidence from Charles Lyell’s uniformitarian geology and other sources, Usher emphasized that the vast amount of time required for the operation of

⁴⁷ Nott and Gliddon, Types of Mankind, 77.

⁴⁸ Nott and Gliddon, Types of Mankind, 69.

⁴⁹ Nott and Gliddon, Types of Mankind, 79.

⁵⁰ Nott and Gliddon, Types of Mankind, 79.

geological processes, likely on the order of millions of years if not more.⁵¹ Fossil indicators proved the appearance of life at remote epochs in the primary formations; the appearance of novel fossil forms in ascending strata through the tertiary beds pointed to successive creations of new and unique zoological forms (this was consonant with Lyell's beliefs on organic origins).⁵² However, it was to the more recent geological deposits superposed on those of the tertiary known as the Diluvium, or "drift," that Usher turned his attention.

Geologists had identified these geographically extensive formations, marked by clay and sand deposits as well as itinerant gravels and erratic boulders, and interpreted them to have marked the last significant event in the geological history of the earth.⁵³ Many fossils found in these deposits, often associated with caverns, were of extinct animals; by

⁵¹ Nott and Gliddon, Types of Mankind, 327-34.

⁵² Factoring into Usher's discussion here were the discoveries of fossil footprints of giant birds and quadrupeds in the Connecticut sandstone by Edward Hitchcock, a natural sciences professor and president of Amherst College who had also co-founded the American Association of Geologists and Naturalists (parent organization of the American Association for the Advancement of Science). In the late 1830s, Hitchcock had called into question Edwin Davis's interpretation of the artificial character of mound stratigraphy. In Hitchcock's view at the time, the mounds were products of natural processes.

⁵³ The exact cause of the drift deposits was still a matter of much debate. Before moving to America, Agassiz had worked out a theory of glaciation (and corresponding concept of an "Ice Age"). The drift gravels also served natural theologians and biblical literalists well as evidence of the Noachian flood. On Agassiz's theory, see Martin Rudwick, "The Glacial Theory," History of Science 8 (1970), 136-57. On the natural theological context for geology in the first half of the nineteenth century, especially in Britain, see Charles Coulston Gillispie, Genesis and Geology: A Study in the Relations of Scientific Thought, Natural

the 1850s, a number of startling European discoveries of fossil human bones seemingly bedded within with the drift had excited no little controversy among the geological communities there. American polygenists took the discoveries very seriously—more so, perhaps, than anybody else at the time until those finds were validated by controlling interests within the Geological Society of London in 1859.⁵⁴ Prior to this, many of the finds were written off as intrusive or the result of shoddy excavation techniques, criticisms leveled often by Lyell himself.

Usher provided an exhaustive list of important discoveries: William Buckland's British cavern finds in the 1820s; Philippe-Charles Schmerling's discoveries in Belgian caves between 1829 and 1834; Jacques Boucher de Perthes's gravel bed finds in northwest France during the 1830s and 1840s. These were among the most famous; Usher included many more examples. Buckland and Schmerling had found fossil human

Theology, and Social Opinion in Great Britain, 1790-1850 (Cambridge: Harvard University Press, 1996), especially chapter 4.

⁵⁴ The process of legitimizing the fossil evidence for human antiquity within the European context was very complex. In the Continental setting, scholars were willing to take them somewhat more seriously than were members of the powerful, controlling British geological community, and thus their views were marginalized. After William Pengelly carefully excavated human remains from Brixham Cave in southwest England under the microscopic scrutiny of the Geological Society of London did British geologists begin to accept the evidence for extreme antiquity and re-evaluate numerous earlier discoveries. For excellent full accounts, see: Donald Grayson, The Establishment of Human Antiquity (New York: Academic Press, 1983); and, A. Bowdoin van Riper, Men Among the Mammoths: Victorian Science and the Discovery of Human Prehistory (Chicago: University of Chicago Press, 1993). Also see, William Pengelly, "History of Cavern Exploration in Devonshire, England," American Journal of Science and Arts third series 114 (1877), 299-308 and 387-94.

bones in association with extinct faunal remains, both within complex and confusing cave stratigraphic contexts.⁵⁵ These discoveries were thus more easily dismissed on methodological grounds. De Perthes's discoveries were much more controversial—stone tools in direct association with extinct animal fossils, found in an open context along the Somme River and announced in published form in his 1849 Antiquités Celtiques et Antédiluviennes. Whereas archaeology in Europe had largely been the province of Classical studies, de Perthes had married it to geology and palaeontology.⁵⁶ Usher took de Perthes's work quite seriously, and devoted much ink to it, reproducing several of the French excavator's detailed stratigraphic profiles of the deposits within which his finds were made. Usher also included quite a few reproductions of the stone tools found by de Perthes, and addressed the seeming similarity between those artifacts and some moundbuilder axes described by Squier.⁵⁷ Consistent with polygenist thought, Usher saw this to indicate that similar human wants merely produce similar technological responses.

In 1854, much of the European evidence for extreme human antiquity was widely regarded as highly debatable—and not a little seductive. Although “diluvial man” was mainly perceived as matter of geology, it was fast becoming apparent that the drift constituted a common ground upon which geology and archaeology might meet.

⁵⁵ Although Buckland steadfastly denied the extreme antiquity for his human finds. See Gillispie, Genesis and Geology, chapter 4.

⁵⁶ According to both Grayson and van Riper, de Perthes's work was marginalized as much for the outmoded catastrophist geology to which he adhered, especially within British circles. It is important to point out the significance of the transformation occurring in European archaeology during the years 1840-1855, as van Riper has indicated (Chapter 2).

⁵⁷ Nott and Gliddon, Types of Mankind, 361. Again, there was no mention of Davis.

Significantly, this newer brand of archaeology was increasingly less oriented to Classics, which is not to say that this sort of “historical” archaeology disappeared. The polygenists may have been keen to make broad denunciations against the dogmatic reliance on biblical chronology within scientific matters pertaining to human history. The truth of the matter is that biblical chronology had loosened considerably by the mid-1850s.⁵⁸ Whether one accepted the evidence for antiquity or not, many members of the scientific communities to whom this evidence mattered most were suspicious of a literal biblical chronology. All the new evidence demanded further serious attention.

Usher’s contribution to Nott and Gliddon’s book introduced many American readers to developments regarding the antiquity of man for the first time. It stood as one of the most impressive English-language syntheses of this information, and by taking finds such as de Perthes’s seriously it lent considerable weight to the polygenist argument for the extreme duration of human history.

Without question, Types of Mankind was a catalyzing agent in scientific debates on race in the United States. The fact that mound archaeology—the Smithsonian’s mound archaeology, no less—figured so extensively into its various arguments did not sit well at all with Joseph Henry. In addition, Squier—a man whose ethnological career Henry had personally done so much to advance—praised the book, having come around by this time to openly embrace polygenism as a plausible theory.⁵⁹ Types of Mankind sold quite well and invoked an immediate critical response mainly from religious orientations.⁶⁰ The

⁵⁸ See, for example, van Riper, Men Among the Mammoths, chapter 3.

⁵⁹ Meltzer, “Making of an American Archaeological Classic,” 57.

⁶⁰ Meltzer, “Making of an American Archaeological Classic,” 57. Meltzer suggests that its sales were likely due more to “notoriety than its scientific standing.”

scientific reaction was considerably more measured. Whatever else could be said of them, Morton and Agassiz were both held in high esteem within American scientific circles. Admittedly, Morton was dead; the Swiss immigrant Agassiz was still very much alive and active in establishing Harvard as one of the principal centers for zoological research in the United States.

More likely, however, was the disturbing possibility that much of Nott and Gliddon's science was in fact plausible. For one thing, the evidence for human antiquity had been mounting rapidly. In just a few short years there would be little question on the matter. As Grayson and van Riper have demonstrated, the 1858-9 excavation of Brixham Cavern in southwestern England was an important event in converting the British geological community to the possibility of extreme human antiquity.⁶¹ After quarry workers broke into the cave and found fossil bones, the Geological Society of London staked a claim to the site and oversaw a highly controlled excavation. This work revealed stone tools in undeniable association with extinct faunal remains. Participants in the project, including Charles Lyell and excavation supervisors William Pengelly and Joseph Prestwich, each presented papers on the finds in 1859.⁶² Humans, it seemed, coexisted with mammals now extinct at a time when the earth was not in its modern form. The Brixham Cavern episode had a number of ripple effects—discoveries made earlier were re-evaluated in a new light, and it put a much higher premium on rigorous field methodology and

⁶¹ See fn. 54, above.

⁶² Papers were presented to the British Association for the Advancement of Science, the Geological Society of London, the Royal Institute of Great Britain, the Royal Society of London, and the Society of Antiquaries (Grayson, Establishment of Human Antiquity, 188-90).

authoritative witnessing.⁶³ The polygenists seemed to be correct on the matter of antiquity, at least for the time being.

In addition to these estimates, Nott and Gliddon's predictions about inevitable Indian extinction also seemed to be coming true. Nobody in their right mind could deny that it appeared to be the case. With the increasing pace of American expansion into the west, Indians continued to suffer horribly for not abandoning their ways of life, and despite the best efforts of reformists and sympathizers to improve their situations, nothing seemed to be working to end conflicts and peaceably acculturate Indians to American society.

By the time Types of Mankind appeared, little more than a few small Iroquois populations remained east of the Mississippi River as a result of the legacy of the removal policies instituted under the Andrew Jackson administration. Between 1830 and 1855, the five so-called "civilized tribes" of the southeast—the Chickasaws, Choctaws, Cherokees, Creeks, and Seminoles—had been removed to "Indian Territory" (present-day Oklahoma); virtually all of the Eastern tribes originally residing north of the Ohio River had been relocated and concentrated first into the Kansas Territory, and then to the Indian Territory when the Kansas-Nebraska Act of 1854 further eroded those Indian land claims. Resettlement of tribes meant inevitable conflict: with each other, with pre-existing western tribes of the Great Plains, and with ever increasing numbers of white settlers who continued to move west and occupy designated Indian lands. It also meant a U.S. military presence throughout these regions to enforce pacification and protect settlers. Conflict existed also in the southwest, especially in the vast New Mexico territory, where U.S. cavalry campaigns against the Navajo had been ongoing since the

⁶³ Pengelly, "History of Cavern Exploration in Devonshire, England," 389.

close of the Mexican War (1848), and which drew the involvement of the Hopi, Pueblo, and Ute Indians as cavalry allies. Indian groups on the west coast similarly felt the press of U.S. expansionism especially after the discovery of gold in the 1840s.⁶⁴

These years represented a period of intense unrest, dislocation, and cultural annihilation for most of America's Indian tribes who were dealt with in a piecemeal fashion by the federal government as dictated largely by the necessities of white settlement. Indian removal, which began under a treaty system of negotiation and land exchange, by mid-century had begun mutating into something of a different character. On both federal and state levels, respect for Indian sovereignty (which at least was sometimes recognized under the treaty system) had eroded precipitously. The fact that Indians remained so defiantly aggressive in the face of such overwhelming political pressure—and in the face of their own declining numbers—was not, suffice it to say, seen as the heroic actions of peoples struggling to maintain identity. It was generally interpreted as stubborn refusal to assimilate, the mark of savagery.

⁶⁴ Donald Fixico, "Federal and State Policies and American Indians," in Philip J. Deloria and Neal Salisbury, ed., A Companion to American Indian History (Oxford: Blackwell, 2002), especially pp. 379-84; R. David Edmunds, "Native Americans and the United States, Canada, and Mexico," in Deloria and Salisbury, ed., Companion, especially 397-401; John M. Murrin, Paul E. Johnson, James M. McPherson, Gary Gerstle, Emily S. Rosenberg, and Normal L. Rosenberg, Liberty, Equality, Power: A History of the American People (Fort Worth, Texas: Harcourt, 2001), 491-3; Roger L. Nichols, Indians in the United States and Canada: A Comparative History (Lincoln: University of Nebraska Press, 1998), chapters 7-8, especially pp. 200-220; Michael D. Green, "The Expansion of European Colonization to the Mississippi Valley, 1780-1880," in Bruce Trigger and Wilcomb Washburn, ed., The Cambridge History of the Native Peoples of the Americas: Volume 1, North America, Part 1 (Cambridge: Cambridge University Press, 1996), 461-538.

Through all of this, ethnology held promise for facilitating at least some level of understanding about Indians to policy makers. Nott and Gliddon's science left no ambiguity that Indians should be left to die out, but their views were too controversial to have become the basis of official policy. Yet, to put the matter fully in perspective, one need not have been a polygenist to hold such nihilistic views about the Indians. A glaring example worth noting came in the form of Henry Rowe Schoolcraft's six-volume Historical and Statistical Information Respecting the History, Condition, and Prospects of the Indian Tribes of the United States, appearing between 1851 and 1857.⁶⁵ Schoolcraft had a long history of association with the Department of War (which housed the Bureau of Indian Affairs until 1849), had acted as Indian agent in the Michigan territory for some time after 1822, and had married into a mixed-blood Chippewa family in Sault Ste. Marie.⁶⁶ An interest in Indian languages, myths and legends, combined with an intense and sudden Presbyterian awakening led Schoolcraft also to see Indians in increasingly moral terms—to him, they were as children in need of education and guidance.⁶⁷ Commissioned by the War Department specifically in order to help formulate policy, Schoolcraft's History was at base a defense of monogenism—he found polygenism morally repugnant on religious grounds.⁶⁸ However, unlike the more Enlightenment-

⁶⁵ Henry Rowe Schoolcraft, Historical and Statistical Information Respecting the History, Condition, and Prospects of the Indian Tribes of the United States (Philadelphia: Lippincott, 1851-1857).

⁶⁶ Robert E. Bieder, Science Encounters the Indian, 1820-1880: The Early Years of American Ethnology (Norman, OK: University of Oklahoma Press, 1986), chapter 5 ("Henry Rowe Schoolcraft and the Ethnologist as Historian and Moralist), pp. 146-93, especially 146-9.

⁶⁷ Bieder, Science Encounters the Indian, 156-62.

⁶⁸ Bieder, Science Encounters the Indian, 183 and 187.

rooted monogenic views of an earlier generation of theorists (like Albert Gallatin), Schoolcraft did not believe Indians capable of progress—they had Old World origins, but analysis of myths and grammatical patterns showed to his satisfaction that Indians possessed a fixed mental and moral nature that was fundamentally different from that of the European.⁶⁹ Indians should be helped, he thought, for humanitarian reasons, but in the big picture they were doomed to eventual extinction because they simply did not possess the mental capacity to fully adopt Christianity and civilization. Seen in such terms, there was little reason to alter basic removal policy and the maintenance of a military presence on Indian lands.⁷⁰

Indians were disappearing, and their histories were vanishing with them. In the eyes of white investigators, America's aboriginal inhabitants—living or dead—were so remarkably different from Europeans that it seemed a foregone conclusion.

This brings me to a final comment here on the impact of polygenism on ethnology. The rise of polygenic theory created somewhat of a crisis amongst ethnologists in regard to the value of historical knowledge by relegating the study of a peoples' history to an incidental role within its explanatory framework for human diversity. For whatever else can be said of the various monogenist approaches to the study of mankind, history was essential. Whether to illustrate common biblical origins or to trace the diffusion and development of traits without committing to the question of ultimate origins, the historical record underlying particular human traits formed the basis for understanding

⁶⁹ Bieder, *Science Encounters the Indian*, 187-92.

⁷⁰ There is a legitimate argument to be made that, despite outer appearances, ethnological studies like Schoolcraft's actually had little practical effect upon altering basic policy. When it came right down to the heart of the issue, the economics of expansion usually trumped all other considerations.

them. In the wake of Morton, Nott, and Gliddon, historical details became incidental to the seemingly hard data of anatomical measurements. Humankind, under polygenism, was comprised of distinct types that could best be identified through craniometric analysis. Granted, history was not entirely unimportant under polygenism—it could after all be employed to show how the non-white races had given way to the press of European imperialism, or to indicate through archaeology or palaeontology the durability of types over long stretches of time. And as we saw in the last chapter, Squier himself worked out a softer form of polygenism that assumed a psychic unity to all mankind. History was relevant for Squier in that groups may have been created separately and they may have developed along parallel lines, but something had to account for the existence of different levels of progress. Exhaustive comparative studies of elemental conceptions such as the serpent symbol could ultimately reveal a sort of grand history of serpent symbolism. But within the rigid strictures of the strong form of polygenist typological thinking, the value of historical knowledge ultimately became secondary and inessential.

If Squier and Davis contributed to the idea of moundbuilders as a separate race, then Nott and Gliddon brought the moundbuilders fully within the politicized framework of race debates in mid-century America. Insofar as science was part of these debates, the core issue became the suitability of the concept of the unity of man. As far as the strong polygenists were concerned, unity had outlived its usefulness.

The moundbuilders now more than ever became one piece of a much bigger human puzzle, the solution to which would not be possible until many of the broader contours of aboriginal history were worked out. To this effect, the 1856 publication of Samuel Haven's Archaeology of the United States in volume 8 of the Smithsonian's

Contributions to Knowledge represented a thorough attempt to bring everything to date together into one account. Haven was the long-standing librarian of the American Antiquarian Society, a position which had kept him in the thick of the east-coast ethnological scene. Despite its title, Haven's study was a remarkably up-to-date synthesis of virtually all ethnological work (not just archaeology) completed in regard to the Mississippi valley mounds. Under editorial direction from Henry, Haven did not attack polygenism overtly nor put forth any strong theoretical commitment otherwise, even if he quietly left the door open for the Asian origin of man in the New World.⁷¹ His discussion of moundbuilder achievements downplayed their seemingly advanced qualities over those of Indians, a subtle way of denying the popular notion of a lost civilization. The underlying message was that moundbuilders and Indians were equally part of the same history of the continent. Haven's summary of the researches of Morton, Nott, and Agassiz was fair and uncritical.

Archaeology of the United States did not mark the fact that any wholly satisfactory sort of resolution had been achieved regarding the ancient history of the New World, even if the publication did signal something of an end to an initial flurry of Smithsonian

⁷¹ Meltzer, "Making of an American Archaeological Classic," 58; Hinsley, Smithsonian and the American Indian, 40; Robert Silverberg, The Moundbuilder of Ancient America: The Archaeology of a Myth (Athens, OH: Ohio University Press, 1968), chapter 4; Samuel Haven, Archaeology of the United States, Smithsonian Contributions to Knowledge 8 (1856), 145-158. Haven concluded with a lengthy and sophisticated discussion of wind and ocean current information, suggesting that "casual passages from the eastern to western continents have been possible in very rude ages" (p. 146).

sponsored researches in the Ohio and Mississippi valleys over the previous decade.⁷²

Between the appearance of Squier and Davis's Ancient Monuments in 1848 and Haven's review in 1856, three other volumes of the Smithsonian Contributions to Knowledge presented the results of original field research in the upper Mississippi valley. Squier's "Aboriginal Monuments of New York" appeared as part of volume 2 (1850), Ohio geologist Charles Whittlesey's "Ancient Works in Ohio" appeared in volume 3 (1851), and Wisconsin geologist Increase Lapham's "Antiquities of Wisconsin" appeared as part of volume 7 (1855).⁷³ Although each of these was a valuable contribution to archaeology in its own right, they were all modeled on Ancient Monuments and essentially added more details to the framework established by that study.

Squier and Davis's work had pointed to a number of generalizations about moundbuilders, and these had taken hold. The moundbuilders were a numerous, sedentary, and extended population, and they practiced agriculture. They were highly creative, building not only massive earthworks and mounds, but smaller works of artifice, too, such as decorated earthenware pots, intricately carved stone pipes and other sculptural objects, and implements of worked raw copper, a metal commonly found

⁷² Thomas G. Tax, The Development of American Archaeology, 1800-1879, diss., University of Chicago, 1973 (Ann Arbor, UMI 1973) 246. Technically speaking, Haven's work did not build from novel fieldwork or research. Despite the title, it was a synthetic summary and overall assessment of virtually all ethnological work on mounds to date. See my comments on Haven's study in Chapter 1.

⁷³ Ephraim G. Squier, "Aboriginal Monuments of New York," Smithsonian Contributions to Knowledge 2 (1850); Charles Whittlesey, "Ancient Works in Ohio," Smithsonian Contributions to Knowledge 3 (1851); and Increase Lapham, "Antiquities of Wisconsin," Smithsonian Contributions to Knowledge 7 (1855). All

within excavated mounds.⁷⁴ Construction of the mounds and earthworks themselves seemed to require a complex stratified social structure in order to orchestrate such large scale productions. Furthermore, the earthworks appeared to many a surveyor's eye to evince sophisticated knowledge of geometry. Although the geometry was downplayed by Haven, anyone who deferred to Squier and Davis's survey maps saw perfectly regular circular or polygonal works constructed on a massive scale. Most archaeological scholars liked Squier and Davis's classification of ancient works and placed great importance on the "sacred enclosures" that seemed to have been sites of ceremonial activity. This pointed to the existence of a relatively sophisticated level of religious development. Lastly, Ancient Monuments made it fairly clear that some sort of connection existed between the moundbuilding people of the Ohio valley and the ancient populations of Central America, most likely that the older moundbuilders had gradually migrated southward. Many readers saw this association to further indicate that no physical link connected the race of the mounds with Asian races.

In the midst of discussions about the status of Indians in American society, the romance of the moundbuilders remained very great indeed. After 1856, however, Smithsonian sponsored ethnology became focused more on the disappearing Indian tribes of the west and southward to the Central American remains. Because of the

essentially followed the model of Squier and Davis's original Ancient Monuments of the Mississippi Valley, and are very similar in terms of overall visual appearance.

⁷⁴ Squier and Davis (1848) had commented extensively on the moundbuilders' use of copper. Charles Whittlesey, additionally, had documented ancient copper mines along Lake Superior in the 1850s, which eventually became the basis of his second Smithsonian monograph, Ancient Mining on Lake Superior, in volume 13 (1863) of Smithsonian Contributions to Knowledge.

Smithsonian's quasi-governmental institutional status, both the military presence on Indian lands and an increase in government expeditions in the west would increasingly play central roles in shaping the direction of the organization's ethnological endeavors, particularly after the Civil War.

3.3 Archaeological diversity in a new light.

The polygenists were by no means immune to scientific criticism. One of the most direct and carefully measured responses originated not from Washington, Philadelphia, New York, Boston, nor any of the scientific centers of Europe. It came from the provincial scholarly backwater of Toronto under the pen of a recent Scottish transplant to the chair of history and literature at University College named Daniel Wilson. For the remainder of his life and career there, Wilson conducted many notable ethnological studies and became a central, active member of North American scientific societies. Having been steeped in the currents of European antiquarianism and archaeology, he became an important conduit for introducing to North American audiences many of the European developments in archaeology, and is perhaps best known for having made explicit in his work a conceptual analytical distinction between "history" and "prehistory." Although Wilson was recognized as an important ethnologist by his contemporaries, his work bore somewhat of an "outsider" status, especially since he remained cool to Darwinian theory.⁷⁵ At least partly because of this, his theoretical work in ethnology stood apart from many of the other synthetic studies to emerge in the 1850s

⁷⁵ See, Bruce Trigger, "Daniel Wilson, 1816-1892," in Tim Murray, ed., Encyclopedia of Archaeology: The Great Archaeologists, Volume 1 (Santa Barbara: ABC-Clío, Inc., 1999), 79-92.

and 1860s. What particularly distinguished Wilson's ethnological views was his thinking on race and the prehistory of the New World.

Wilson had moved from Edinburgh in 1853, where he had been a central organizational member for some time of the Society of Antiquaries of Scotland. His mentor there, Robert Chambers, the anonymous author of Vestiges of the Natural History of Creation (1844) was a man with whom Wilson remained very close friends for the rest of his life.⁷⁶ In Edinburgh, Wilson became interested in archaeology, had consorted with phrenologist George Combe, and facilitated social ties and an artifact exchange program between the Scottish antiquarian society and the Danish National Museum of Antiquities. As Trigger states, Wilson and Chambers together were seeking to transform the Society of Antiquaries "into a significant research institution" for the methodical study of the past.⁷⁷ Wilson himself employed the "three age" classificatory system institutionalized at the Danish Museum in classifying objects in the collection at the museum of the Society of Antiquaries. Arranging the Society's archaeological materials in terms of temporal

⁷⁶ Trigger, "Daniel Wilson," especially p. 80 on relationship with Chambers. Other biographical information on Wilson may be found in: Bruce Trigger, A History of Archaeological Thought (Cambridge: Cambridge University Press, 1989), 82-3; Bruce Trigger, "Sir Daniel Wilson: Canada's First Anthropologist," Anthropologica 8(1966), 3-28; Bruce Trigger, "Giants and Pygmies: The Professionalization of Canadian Archaeology," in Glyn Daniel, ed., Towards a History of Archaeology (London: Thames and Hudson, 1981); Alice B. Kehoe, "The Invention of Prehistory," Current Anthropology 32(1991), 467-476; Marinell Ash, "'A fine, genial, hearty band': David Laing, Daniel Wilson, and Scottish Archaeology", in A.S. Bell, ed., The Scottish Antiquarian Tradition: Essays to mark the bicentenary of the Society of Antiquaries of Scotland and its Museum, 1780-1980 (Edinburgh: Donald Publishers, 1981), 86-113.

⁷⁷ Trigger, "Daniel Wilson," 80.

sequences of stone, bronze and iron ages would prove of lasting consequence to the development of Wilson's thought on human history.

Wilson came to North America eager to expand the breadth of his archaeological and ethnological interests. He had been fully aware of the range of existing ethnological studies on American aborigines, and particularly those of Morton. Almost immediately after his arrival in Toronto, Wilson began amassing his own set of measurements of aboriginal crania procured from nearby graves in Canada. Wholly expecting to find them approximating to the brachycephalic (or rounded) form determined by Morton to be representative of the American type, he was gradually forced to admit that, "to whatever extent the affirmed typical form of the American cranium is found to prevail in other parts of the continent, the crania most frequently met with along the north shores of the great lakes are deficient in some of its most essential elements."⁷⁸ Something, it seemed, was amiss. From this starting point, Wilson took it upon himself to analyze as many crania as he could—including many from Morton's own collection still in Philadelphia—to show that the range of physical variation did not at all support the view of a clearly definable and distinct American type. In other words, Wilson thought, Morton and others who had focused on skull measurements had forced these real differences into a pre-determined category. The mistake, however, had not been limited only to polygenist anatomists, and Wilson then took it upon himself to point out how American ethnology in general had been characterized by its failure to account for apparent differences in a meaningful way. Polygenists and monogenists alike, he thought, anatomists and

⁷⁸ Daniel Wilson, "On the Supposed Prevalence of One Cranial Type Throughout the American Aborigines," Edinburgh New Philosophical Journal January 1858, 13-4.

philologists—all had been operating under one grand fiction. All sought unity where in fact there was considerable diversity. “Very few and partial exceptions,” Wilson wrote,

can be quoted to the general unanimity of American writers,—some of them justly regarded as authorities in ethnology,—in reference to this view of the nations of the whole American continent, north and south. With the solitary exception of the Esquimaux, they are affirmed to constitute one nearly homogeneous race, varying within very narrow limits from the prevailing type, and agreeing in so many essentially distinctive features, as to prove them a well-defined variety, if not a distinct species of the genus Homo.⁷⁹

Wilson spared nobody—Morton, Nott, Gliddon, and Agassiz as well as Gallatin and Prichard all were critiqued. Morton, in particular, he considered “the fountain-head of all such opinions and views,” and so it was mainly against Morton’s craniometry that Wilson directed the brunt of his own findings.⁸⁰ But Agassiz’s contribution to Types of Mankind was not spared a lengthy criticism, and Wilson even commented on Gallatin’s philological findings regarding the seeming unity of linguistic structure among Indians. It was a rather remarkable position to articulate.

Wilson published his results, “On the supposed prevalence of one cranial type throughout the American aborigines,” in January, 1858 in the Edinburgh New Philosophical Journal. Likely he published there because at the time he was struggling to maintain some status within the European circles he had left behind, and had not yet integrated into the American ethnological scene.⁸¹ Perhaps, too, this had something to do with his strong stance. Wilson was not overtly accusatory, however—though he did

⁷⁹ Wilson, “Prevalence,” 3.

⁸⁰ Wilson, “Prevalence,” 3.

⁸¹ Kehoe (“Invention of Prehistory,” 469) suggests that Wilson felt “exiled” to Canada for lack of finding any comparable academic work in Britain. Wilson had already established quite a name for himself there, and did not want to lose that status.

explicitly suggest that Morton had been too selective with some of his data.⁸² The concept of a homogeneous American type served Morton and his followers well as a working hypothesis, Wilson admitted, but “the evidence was totally insufficient for any such absolute and dogmatic induction as it has been made the basis of.”⁸³ They had built a system, and forced their data to fit into it.

Although Wilson found Squier and Davis’s moundbuilder skull to be a good approximation of a supposed brachycephalic Toltecan form, he found Morton’s rigid distinction of two families (Barbarous and Toltecan) to be wholly arbitrary and without basis.⁸⁴ Wilson was perplexed by the fact that close attention to Morton’s own data showed clearly that subtle gradations existed between Eskimo, Iroquois, Peruvian, Mexican, Huron, and Moundbuilder forms.⁸⁵ These were differences in degree, not kind, and quite likely “amount to no more than variations within the normal range of a

⁸² Wilson, “Prevalence.” For example, Wilson stated that Morton ignored the significance of regional differences caused by fairly common practices of skull-deformation (p. 25), and that Morton’s samples were simply too small in number to have derived any sound conclusions upon (p. 28). Interestingly, in his critical re-assessment of Morton’s methodology, Stephen Jay Gould does not mention Wilson (See Gould, Mismeasure of Man, chapter 2). This is unfortunate, since it leaves the impression that Morton’s work was widely and uncritically accepted, that Morton’s sole legacy to the history of science was the racist “‘American school’ of polygeny” (p. 83), and that this “school” represented the consensus view of mid-century American ethnology. Gould’s account of Morton thus additionally fails to acknowledge the full breadth of ethnological science to which Morton believed he was contributing, and thus does little to help us understand how particular scientific beliefs emerge from periods of controversy.

⁸³ Wilson, “Prevalence,” 31.

⁸⁴ Wilson, “Prevalence,” 5, 15-6.

⁸⁵ Wilson, “Prevalence,” 28-9.

common type....”⁸⁶ Further research, Wilson strongly believed, would show “that the crania everywhere, and at all periods, have conformed or even approximated to one type.”⁸⁷ There were no distinct cranial types, only variations.

In subsequent years, Wilson was to become an increasingly prominent ethnologist with even stronger archaeological interests in the aboriginal inhabitants of North America. He published additional papers on the topic in the *Smithsonian Reports* (in 1862) and the *Proceedings of the American Antiquarian Society* (1863), but his real impact was in Toronto where he remained central to the operations of the Canadian Institute, published extensively through its journal, and became a head administrator at University College (later the University of Toronto). As a long-time editor of the Institute’s scientific periodical, Wilson introduced to Canadian audiences many of the European developments bearing on ethnology, from de Perthes’s ancient human finds to Darwin’s views on evolution. His two-volume *Prehistoric Man* of 1862 brought him the most recognition as an ethnologist. This work synthesized his more recent studies in North America with views on human development that he had fostered before leaving Scotland.

The background to Wilson’s theories of human development merit additional comment. In Edinburgh, his work at the Society of Antiquaries importantly involved applying the Scandinavian “three age” system to the arrangement of the Society’s archaeological collections. The Danish museum worker Christian Thomsen had developed the schema as he arranged groups of artifacts according to criteria of

⁸⁶ Wilson, “Prevalence,” 31.

⁸⁷ Wilson, “Prevalence,” 31.

manufacture into stone, bronze and iron categories. These series in turn gradually suggested temporal sequences.⁸⁸ Stone tools, the most primitive in appearance, invariably seemed to be of greatest antiquity. Wilson's Archaeology and Prehistoric Annals of Scotland (1851) was the expanded product of his classification of the Society museum's collection based on these premises, and it was the work that has since been credited by historians as having introduced the term "prehistoric" into the English language in a meaningfully analytical sense.⁸⁹ Importantly, for Wilson, the Danish tripartite system was not so much something to be taken as a given, but was an analytical tool to be used, as one historian has put it, for "getting beyond the classical and literary bias of so much Scottish antiquarian and archaeological thought," and into the realm of prehistory.⁹⁰

By the time he wrote Prehistoric Annals, Wilson had melded three-age classification with developmental theory to demonstrate the progress of European civilization out of a

⁸⁸ The "Three Age System" (stone, bronze, iron) first emerged from Christian Thomsen's meticulous work in serially arranging groups of artifacts and paying attention to their provenance of recovery. Gradually, a developmental chronology presented itself as a succession of stone, bronze and iron "ages." This system was gradually refined by Thomsen and other archaeologists associated with the Danish Museum (notably Jens Worsaae), and seemed to be borne out by archaeological excavations that correlated stratigraphy with age—the deeper one dug, the more primitive and ancient the implement. For further background on the development of the Danish three age system, see: B. Gräslund, The Birth of Prehistoric Chronology (Cambridge: Cambridge University Press, 1987); B. Gräslund, "The Background to C. J. Thomsen's Three Age System," and Judith Rodden, "The Development of the Three Age System: Archaeology's First Paradigm," both in Glyn Daniel, ed., Towards a History of Archaeology (London: Thames and Hudson, 1981).

⁸⁹ See for instance, Kehoe, "The Invention of Prehistory," 467; and Trigger, "Daniel Wilson," 80-1.

⁹⁰ Ash, "David Laing, Daniel Wilson and Scottish Archaeology," 109.

primeval prehistoric state through a succession of ages. In so doing, he had elaborated a neat methodological framework for linking the human present with the past.⁹¹ From some primeval point of origin, mankind had migrated and diversified across the globe, splintering into smaller groups, some of which developed more rapidly than others. Environmental factors played the major role in determining rates of progress. The existence in the present of primitive groups thus offered a window directly onto the prehistoric past of more advanced groups, since these were equivalent “stages” and analogies could be drawn between them.

Wilson brought out this method of analysis more fully and explicitly in his two-volume Prehistoric Man: Researches Into the Origin of Civilisation in the Old and the New World (1862). Whereas Prehistoric Annals mainly focused on the development of Old World civilization, Prehistoric Man represented an intensive study of North American natives, because the New World offered a valuable contrasting vantage point for observing, in his words, a “parallax of man,” a clear line of sight directly onto the primitive condition of mankind.⁹² The work was strongly anti-polygenist, as one might imagine from his earlier research on cranial types, but also because Wilson’s method of analogical comparison only worked under the monogenist model of human history. For data, in addition to his own researches, Wilson made extensive use of libraries and

⁹¹ Ash, “David Laing, Daniel Wilson and Scottish Archaeology,” 108-112.

⁹² Daniel Wilson, Prehistoric Man: Researches Into the Origin of Civilisation in the Old and New World, Volume 1 (London: MacMillan and Co., 1862), xii.

specimen collections throughout the United States as well as a few in Canada. Chief among those he thanked were Joseph Henry and Edwin Davis.⁹³

The basic premise of Prehistoric Man was that American aboriginals were only just emerging from a primeval state and entering into their own historic period, existing literally at the cusp of prehistory and history. Accordingly, the work was permeated with the sense that America's natives retained some element of the noble savage largely unadulterated by the trappings of civilization even if recent contact with Europeans had led to drastic and unalterable change. It would not be long before all living traces of the primeval condition vanished. For better or worse, North America had entered a European phase—the Indians were becoming “historic” and their primitive customs and artifacts needed to be documented. Prehistoric Man was Wilson's attempt to do just that comprehensively, and he devoted chapters, therefore, to topics such as: speech and language, as well as letters; technological ability, especially the building of canoes, the use of tools, and metallurgy; and artistic capacities and the use of symbolism. Four chapters were devoted to the mounds and earthworks of the Mississippi valley, including detailed discussions about the various uses to which these structures might have been put, especially those of a religious nature. Much of Wilson's mound discussions relied exclusively on Squier and Davis's work. A chapter on “the American cranial type”

⁹³ Wilson was impressed enough with the help he received to claim “that in no country in the world are public and private libraries and collections made available to the scientific inquirer with the same unrestricted freedom as in the United States” (Wilson, Prehistoric Man, Volume 1, xiv.) Other than the Smithsonian, he also visited the Academy of Natural Sciences, the Boston Natural History Society, the American Ethnological Society (where Davis was still active), and the Historical Society of New York.

reiterated his views in opposition to polygenist thinking, and he considered in one other some issues concerning hybridization and the amalgamation of races.

Wilson's basic conclusions regarding American aboriginals were relatively straightforward. The native populations were as a whole quite old, and arrived in the western hemisphere likely via the Bering Strait long ago subsequently to spread across the continent through processes of migration and diffusion. The archaeological record attested to these wanderings. In the relative geographic isolation of the North American continent, they had greatly diversified and subsequently progressed toward certain stages of civilization, free of the artificial constraints imposed by European civilization until the recent arrival of Europeans. Despite these more recent influences, America nonetheless was

a continent where man may be studied under circumstances which seem to furnish the best guarantee of his independent development...[and where] man is still seen...in a condition which seems to reproduce some of the most familiar phases ascribed to the infancy of the unhistoric world. The record of its childhood are not obscured, as in Europe, by later chronicles; where, in every attempt to decipher the traces of an earlier history, we have to spell out a nearly obliterated palimpsest.⁹⁴

Comparative analysis of man's infancy in both the New World and the Old gave Wilson the methodological leverage to extrapolate such grand propositions as the likely original seat of civilization, somewhere between the Tigris and Euphrates Rivers.

Central to Wilson's philosophy was his concept of prehistory, which for him did not necessarily indicate extreme human antiquity, but referred only to the pre-literate stages of existence. Defined more clearly in the second 1865 edition of Prehistoric Man, Wilson wrote:

⁹⁴ Wilson, Prehistoric Man, Volume 1, x-xi.

Man may be assumed to be prehistoric wherever his chroniclings of himself are undesigned, and his history is wholly recoverable by induction. The term is in no sense equivalent to preadamic; nor has it, strictly speaking, any chronological significance; but, in its relative application, corresponds to other archaeological, in contra-distinction to geological periods. There are modern as well as ancient prehistoric races; and both are available for solving the problem of man's true natural condition.⁹⁵

Prehistory corresponded, literally, to those phases of existence where mankind had not yet developed the ability to write history. It was the role of archaeology to deal with prehistory as a chronological phase (or series of phases) recoverable through artifacts. Other branches of ethnology embraced the “prehistoric present” through the study of customs and languages. By positing man's steady progressive development over time as a necessary operating natural law, presently observable phenomena could be employed to explain and illustrate past historical stages lying beyond the reach of direct observation.

Overall, in conjunction with his earlier Archaeology and Prehistoric Annals of Scotland, Wilson had articulated a powerfully comprehensive methodological approach for the study of man, one that was incompatible with a polygenist model of human history. In keeping with his earlier work, it also struck at polygenism by highlighting throughout (and in great detail) the immense diversity of customs, archaeological remains, and physical characteristics evident among the native peoples of the New World. The polygenists could still counter that Wilson's estimates of only a few thousand years for human history fell far short of the mark to explain diversity—in this regard, Wilson embraced his strong Protestant sensibilities and took a cautious stance toward claims of extreme antiquity, even after works such as Charles Lyell's 1863 Geological Evidences for the Antiquity of Man gave much greater credence to the idea. Regardless,

⁹⁵ Wilson, Prehistoric Man (1865 second ed.), 3.

Wilson's methodological model could still work no matter what time frame was placed on the duration of human existence. Not so for the polygenists—as the antiquity of man got pushed back beyond even their wildest imaginings, a crucial pillar of their argument crumbled. The seeming stability of types over time, Darwin would respond, merely indicated the slow, minutely incremental pace of evolutionary change. The dogmatic assertions by polygenists about separate creations faced a major challenge.

I should point out that Wilson's views were not entirely novel in the English speaking world, but his clear, extended articulation of them was, pre-dating John Lubbock's remarkably similar expressions in that author's Prehistoric Times (1865). Historians have argued over the priority of Wilson's work (over that of Lubbock) in establishing the "comparative method" as a lasting methodological feature in anthropology.⁹⁶ As Kehoe has pointed out, there is a very legitimate argument to be made that Lubbock's status as a prominent member of the Darwinian "X-Club" allowed him to marginalize Wilson's work in Britain. Whatever the case may be, Wilson's work appeared precisely when a new generation of scholars were explicitly placing the data of human diversity into sprawling theoretical frameworks underpinned by evolutionary theory. The comparative method became standard to anthropological and sociological works appearing in the 1860s and 70s from John Lubbock, Herbert Spencer, Henry Summer Maine, Thomas Henry Huxley, John McLennon, Edward Burnett Tylor, and Lewis Henry Morgan, to name a few.⁹⁷ Insofar as archaeology was concerned, Lubbock's work did stand out at

⁹⁶ See Kehoe, "The Invention of Prehistory;" also, Bruce Trigger, "On Giving Lubbock His Due," Current Anthropology 35, 46-7; Alice Kehoe, "Reply," Current Anthropology 35, 47-8.

⁹⁷ For example, some of the more famous of these titles included: John Lubbock, Prehistoric Times (1865); Herbert Spencer, First Principles of e New Philosophy (1862), The Study of Sociology (1873); Henry

least for the fact that he resolved the stone age into an older “Palaeolithic” and a more recent “Neolithic” period.⁹⁸ Morgan’s research, which included resolving human social development into successive stages of “savagery,” “barbarism,” and “civilization,” importantly became central to Smithsonian ethnology.⁹⁹ Many of these men, all Brits with the exception of New Yorker Morgan, considered themselves proponents of Darwinism. Yet, despite Darwin’s own emphasis on the largely non-progressive character of evolution, the evolutionist anthropologists afforded mankind special status over the rest of brute nature. According to these theorists, including Wilson, human social and cultural evolution followed a linear path of progressive development, ascending toward “Civilization,” embodied in a European ideal.

Summer Maine, Ancient Law (1861); Thomas Henry Huxley, Man’s Place in Nature (1863); John McLennan, Primitive Marriage (1865); Edward Burnett Tylor, Researches Into the Early History of Mankind and the Development of Civilization (1865), Primitive Culture (1873); Lewis Henry Morgan, Systems of Consanguinity and Affinity of the Human Family (1871), Ancient Society (1877).

⁹⁸ The French geologist, palaeontologist, and archaeologist Gabriel de Mortillet, for one, importantly further refined Lubbock’s Palaeolithic into a number of successive periods, each characterized by a dominant tool-making industry and named after a prominent archaeological site. See, Trigger A History of Archaeological Thought (Cambridge: Cambridge University Press, 1995) 95-101; Michael Chazan, “Conceptions of Time and the Development of Paleolithic Chronology,” American Anthropologist 97 (3: September 1997), 457-67.

⁹⁹ Particularly, it was his extensive analyses of Indian kinship systems that provided a methodological model for linguistic and ethnological work among living Indian tribes. See Bieder, Science Encounters the Indian, chapter 6 (“Lewis Henry Morgan and the Evolution of an Iroquois Scholar”), 194-246; Hinsley, The Smithsonian and the American Indian, 125-43.

The dispute over Wilson's priority is instructive, however, and certainly has done much to restore some degree of recognition to a scholar who has been largely neglected by historians. Prehistoric Man sold well and went through three editions (even the second edition predated Lubbock's work). Wilson created ties with many of America's leading ethnological scholars and institutions, including the Smithsonian. Like Henry, Wilson was an institution-builder himself, largely responsible for shaping the scientific programs of the Canadian Institute and making ethnology a strong central component of its promoted researches.¹⁰⁰ He was also a long-standing member of the American Association for the Advancement of Science who, along with Frederic Ward Putnam of the Peabody Museum of American Archaeology and Ethnology, helped carve out a subsection of the AAAS devoted to anthropology. Wilson disseminated his ideas to North American audiences along with accounts of the European developments that factored so prominently into his own work. Lastly, unlike many of the British arm-chair theorists who did little actual fieldwork, Wilson had fairly direct access to the archaeological and ethnological data he employed. Kehoe points out that his work was "clearly tempered by firsthand observations of Ojibwa on the western Ontario frontier and of prehistoric sites in the American Midwest."¹⁰¹ The same could hardly be said of Lubbock.

Reasoning by analogy between present and past particularly suited ethnologists in North America. Wilson only discovered what many others already knew—that the simultaneous existence of intriguing archaeological sites as well as living Indian traditions demanded that both be fit into ethno-historical accounts of American

¹⁰⁰ Trigger, "Daniel Wilson."

¹⁰¹ Kehoe, "The Invention of Prehistory," 472.

aboriginals. Without a doubt, Wilson was influential in North American ethnological circles, but he was by no means individually responsible for importing the use of the comparative method so strongly suggested by the three age system to this continent. A number of Continental European scholars had also embraced the heuristic power of the Scandinavian system for some time before 1860, molding it to archaeological ends. One of these men was Adolphe Morlot, whose paper, “Some General Views on Archaeology,” was published almost simultaneously in the American Journal of Science and Arts and the Smithsonian Annual Reports in 1860.¹⁰²

Morlot was a geology professor at Lausanne Academy and had done much to promote the three age classification there, much as Wilson had done in Edinburgh. More so than Wilson, however, Morlot synthesized archaeology and geology in light of the recent ancient discoveries in Europe.¹⁰³ Pronouncing that the “development of archaeology has been very similar to that of geology,” Morlot acknowledged Lyell’s role in establishing the use of presently observable forces to understand and reconstruct the remote, inaccessible past as a fundamental method in the geological sciences.¹⁰⁴ In a similar fashion, “we can...consider the antiquarian as a geologist, applying his method to reconstruct the first ages of mankind, previous to all recollection, and to work out what may be called pre-historical history. This,” Morlot defined, “is archaeology pure and

¹⁰² Adolphe Morlot, “Some General Views on Archaeology,” American Journal of Science and Arts Series 2, 29 (1860), 25-33; Morlot, “General Views on Archaeology,” Smithsonian Annual Report for 1860.

¹⁰³ “General Views on Archaeology” was actually the introduction to a longer paper entitled, “Geologico-Archaeological Studies in Denmark and Switzerland” which had appeared the previous year in Switzerland.

¹⁰⁴ Morlot, “Some General Views on Archaeology,” American Journal of Science and Arts, Series 2, 29 (1860), 26.

proper.”¹⁰⁵ The Swiss geologist then provided one of the most concise formulations of archaeological methodology to appear in print, and it deserves to be quoted in full. “So,” he began,

to understand the past ages of our species, we must first begin by examining its present state, following man wherever he has crossed the waters and set foot upon dry land; the different nations, which inhabit our earth at present, must be studied with respect to their industry, their habits and their general mode of life. We thus make ourselves acquainted with the different degrees of civilization, ranging from the highest summit of modern development to the most abject state, hardly surpassing that of the brute. By that means ethnography supplies us with what may be called a contemporaneous scale of development, the stages of which are more or less fixed and invariable, whilst archaeology traces a scale of successive development with one moveable stage, passing gradually along the whole line. Ethnography is consequently to archaeology what physical geography is to geology, namely a thread of induction in the labyrinth of the past, and a starting point in those comparative researches of which the end is the knowledge of mankind and its development through successive generations.¹⁰⁶

Within this framework, archaeology was not simply about the recovery of fragmentary traces and evidences of human antiquity. It was the far more comprehensive endeavor of reconstructing the ages of man through stone, bronze, and iron. Morlot thus credited the “Scandinavian savans” for “unravelling [sic] the leading features in the progress of pre-historical European civilization.”¹⁰⁷ Civilization, it seemed, was not merely the end-product of human progress, but represented the very process itself.

Left at that, these were intriguing statements, indeed, for American archaeologists to grapple with. But Morlot went even further in pointing out the unique character of American prehistory. Stone implements were certainly common to New World contexts, so there was no doubt as to the existence of a stone age there. The use of bronze in the

¹⁰⁵ Morlot, “Some General Views on Archaeology,” 26.

¹⁰⁶ Morlot, “Some General Views on Archaeology,” 26-7.

¹⁰⁷ Morlot, “Some General Views on Archaeology,” 27.

Americas, however, seemed to have been preceded by an extensive copper-age not witnessed in Europe, potentially representing an important intermediate phase in the development of metallurgy. Morlot incorporated evidence from the Smithsonian researches of Squier and Davis and Lapham to support the view—the moundbuilders became America’s “race of the copper-age.”¹⁰⁸

The examples of Wilson and Morlot illustrate that the relativistic concept of “prehistory” and the idea of developmental ages had been employed in some tacit sense by ethnologists for some time. By the early 1860s, however, these concepts were being articulated and elucidated much more explicitly. In conjunction with a dawning awareness of the significance of European discoveries of human antiquity, the newer methodological perspectives had an immediate and pronounced impact on the direction of archaeology in the United States, largely through the support and promotion of the Smithsonian. The developmental approach seemed to strike against polygenism by assuming some sort of unity to the human race and to human history, a unity that the polygenists could not abide. Second, the style of reasoning implicit to the comparative method—that is, the use of analogy to link present and past—meant that both Indians and archaeological remains could be placed more surely within a system underpinned by a rational methodological core. The Smithsonian had begun to direct much of its ethnological efforts to the Indians out west for “salvage” reasons, but this in no way meant that archaeological activity ceased. Archaeological publications continued to be promoted by the Smithsonian through the 1860s, though on a somewhat smaller scale than earlier. For the time being, the Institution would shy away from the monograph-

¹⁰⁸ Morlot, “Some General Views on Archaeology,” 30.

length archaeological studies found in the Contributions to Knowledge and instead focus on utilizing the Annual Reports to disseminate shorter papers and methodological circulars among its readers and correspondents.

Following closely after Morlot's paper, George Gibbs's "Instructions for Archaeological Investigation in the United States" appeared in the Smithsonian Annual Report for 1861. Gibbs was a restless spirit who, after studying law at Harvard, had drifted west and spent some twelve years in the Washington Territory, working in various capacities.¹⁰⁹ He became very interested in Indian languages and in creating lists of Indian vocabularies, activities that ultimately attracted the attention of Joseph Henry after Gibbs moved back east in 1861. Henry increasingly brought Gibbs within the Smithsonian fold, eventually granting him a position as resident collaborator and researcher on Indian languages. Gibbs's "Instructions" is highly informative not only for how it embodied the current state of ethnology, but also because it demonstrated how these developments tangibly influenced practice and, further, how that practice was wholly embedded in the politics of the time. The circular opened with an appeal to data collectors:

The Smithsonian Institution being desirous of adding to its collections in archaeology all such material as bears upon the physical type, the arts and manufactures of the original inhabitants of America, solicits the co-operation of officers of the army and navy, missionaries, superintendents, and agents of the Indian department, residents in the Indian country, and travellers [sic] to that end.¹¹⁰

¹⁰⁹ Early biographical information on Gibbs can be found in Hinsley, Smithsonian, 50-2.

¹¹⁰ Gibbs, "Instructions for Archaeological Investigation in the U. States," Smithsonian Annual Report for 1861, 392.

Here, we get a sense not only of the extent to which the Smithsonian had come to rely upon correspondents for the accumulation of ethnological data, but also of the complexion of this pool of individuals. Henry and his assistant secretary Spencer Baird had cultivated a vast network of correspondents throughout the country for the collection of all manner of scientific data from meteorological records to natural history specimens.¹¹¹ Keeping in mind the situation for Indians in the west, there was no shortage of military men, religious reformers, or other government agents on Indian lands who were willing to oblige the interests of Smithsonian ethnology.

The collection of crania—of a “full series of skulls of American Indians”—came first on Gibbs’s agenda. The author wisely cautioned against taking crania from contexts which might offend living Indians, and, while recognizing the “extinction” and relocation of many tribes, insisted that collectors make a best determination as to the “nation or tribe to which a skull belongs,” if recovered from a recent tomb or grave.¹¹² Ancient skulls—

¹¹¹ For a good description of the overall character of the Smithsonian correspondent system, see, Daniel Goldstein, “‘Yours for Science’: The Smithsonian Institution’s Correspondents and the Shape of Scientific Community in Nineteenth-Century America,” *Isis* 85 (1994), 573-99. The Smithsonian’s use of correspondents provided an inexpensive way to collect scientific data, and, at a time when little formal education and training existed for would-be practitioners, it provided aspiring scholars the opportunity to participate in an increasingly broader American scientific community. As the title of Goldstein’s article suggests, the Smithsonian correspondent system had a remarkable influence on shaping the social dimensions of scientific practice in general in nineteenth century America. In the remaining chapters, I will be considering how correspondents formed a central part of the greater archaeological networks that were essential for the success of archaeological work in the Ohio valley in the 1880s by both the Smithsonian and Peabody Museum of American Archaeology and Ethnology.

¹¹² Gibbs, “Instructions,” 392.

such as those of the moundbuilders—should be duly noted. In either case, Gibbs instructed that additional important details about the circumstances surrounding the skull's interment be recorded carefully. All this was articulated with at least some view toward establishing tribal or racial affiliations between Indian groups—relationships made complex, Gibbs noted, by inter-tribal marriages and practices of skull-deformation (by flattening or elongating) according to sex or social rank.¹¹³

Gibbs next turned his attention to the collection of specimens of art, which might, by his reckoning, “naturally be arranged under three periods” relative to the European conquest of the continent. Not to be confused in this case with the Scandinavian three ages, Gibbs outlined the following: a class of recent or presently-existing Indians; another of “tribes who have disappeared with the settlement of the Atlantic States and the country between the Alleghanies and the Mississippi” (what would now approximately be called the contact period); and a class of ancient tribes who had disappeared prior to or coeval with the arrival of the first Europeans.¹¹⁴ The customs of modern Indians were vanishing fast—either literally or by miscegenation, and needed to be documented immediately. The intermediate classes were hard to distinguish archaeologically due to their somewhat more transient duration. To the oldest class, of course, belonged not only the moundbuilders and all ancient Americans, but more specifically “those articles found under conditions which connect archaeology with geology.”¹¹⁵ Gibbs brought many

¹¹³ Gibbs noted that depending on the tribe, skull deformation could be a feature of one or both sexes; generally it was a sign of rank. Non-deformed skulls, according to Gibbs, were often the mark of a stranger or of an individual of slave status. See, Gibbs, “Instructions,” 393.

¹¹⁴ Gibbs, “Instructions,” 394.

¹¹⁵ Gibbs, “Instructions,” 395.

European developments regarding human antiquity directly to bear in shaping his specific instructions for collectors of American antiquities.

One relatively new circumstance which drew the Smithsonian's interest, according to Gibbs, was quite recent work by Scandinavian and Swiss archaeologists in identifying extensive shell-heaps in sea coast or lacustrine settings. These coastal mounds were quite massive and previously thought to be of natural origin, but Northern European archaeologists (including Morlot) had discovered numerous stone and bone implements mixed throughout what now appeared to be gigantic stone-age refuse heaps. Many bones of extinct animals were also identified.¹¹⁶ Similar kinds of shell mounds had been noted along the American Atlantic coast, and Gibbs put out a call for specific investigations of these as little or no careful work had been done.¹¹⁷ The implications of such researches had great potential significance for tracing the movements of prehistoric peoples—especially if these shell heaps proved older than the inland mounds due to an absence of metallurgical implements. Beyond that, the remarkable similarity between American and European shell mounds might speak to universal processes of human progress through necessary stages of development.

After the shell mounds, Gibbs called for further attention to the prospects of finding human remains or implements within deep cave settings, in conjunction with extinct faunal remains, and deep within sand or gravel banks.¹¹⁸ The potential for finding ancient “drift man” in America had taken hold. Gibbs next went into some detail as to how best to excavate shell beds and within cavern settings, clearly emphasizing the need for

¹¹⁶ See also Joseph Henry, “Report of the Secretary,” Smithsonian Annual Report for 1865, 48-9.

¹¹⁷ Gibbs, “Instructions,” 395.

controlled attention to the stratigraphic depth and relative positions of finds.¹¹⁹ Concisely outlined by Gibbs, a whole new realm of possibilities now lay before the American archaeologist.

Charles Rau was one to take up the search for human antiquity in America's stone age. Rau was a Belgian-born immigrant to the States in 1848 who made a meager living teaching German to school children, but aspired to study archaeology.¹²⁰ By 1863, he had become one of the Smithsonian's corresponding archaeological investigators with "Agricultural implements of the North American stone period," published in the Report for that year, followed over the next fifteen years by numerous articles on the subject of human antiquity. Rau was too poor to support himself financially at what he saw as his true calling, and, after much perseverance and hardship, was eventually granted a resident position at the Smithsonian by Henry in 1876. Soon after, he was given the curatorial position in the Department of Antiquities in the Institution's National Museum, where he remained until his death in 1887.¹²¹ Rau was as aware of anybody else in the United States of the European developments in archaeology and devoted his career to fitting the data of American archaeology into a developmental classificatory system. Beginning especially with his 1864 paper on "Shell-deposits in New Jersey," Rau sought to demonstrate the similarities of man's development in both Old and New World

¹¹⁸ Gibbs, "Instructions," 395.

¹¹⁹ Gibbs, "Instructions," 395-6.

¹²⁰ Biographical information on Rau appears in Hinsley, Smithsonian, 42-7.

¹²¹ Hinsley, Smithsonian, 42.

settings.¹²² Rau's work showed the immediate fruitfulness of the approaches outlined by Gibbs.

Shortly after its initial publication, Gibbs's paper was expanded to include extensive instructions for collecting linguistic data and widely distributed as a circular to correspondents (1863). Later, it was republished yet again within the Smithsonian Miscellaneous Collections for 1867. It served an important role in imposing some level of methodological and centralizing control on a diverse body of individual fieldworkers, some of whom, like Rau, became integral to Smithsonian archaeology in later years. According to Hinsley, Gibbs's linguistic instructions were in the end more successful, as data collected under that head ultimately served the long-term linguistic enterprises of the Smithsonian quite well.¹²³ In terms of archaeology, the Gibbs circular stimulated a great deal of activity in hopes of finding evidence of stone-age occupation on a par with that of Europe—and it was a substantial amount of activity. In due time, it would become increasingly apparent that the geological settings for archaeological sites in the New World were much different from those of the Old—especially in regard to cave finds.¹²⁴ The United States simply did not seem to have parallels to Brixham Cavern, with undeniable evidence of fossil man living among the extinct animals of a remote era. Gradually over the decades after 1861, archaeologists were frustrated in their attempts to place American archaeology within the same geology-derived framework as that of Europe. Rau persisted, but many others realized that substantial revisions were necessary in order to place the archaeological diversity of the New World within some rational

¹²² Charles Rau, "Shell-deposits in New Jersey," Smithsonian Annual Report for 1864, 374.

¹²³ See, Hinsley, Smithsonian, 50-6.

system for charting and understanding the grand contours of human history. At the same time, however, a growing number of archaeologists and other ethnological scholars were beginning to appreciate the recognition of diversity as an end unto itself.

3.4 The jolt of war.

At exactly the same time that Gibbs sat drafting his “Instructions for Archaeological Investigations,” the United States was ripping apart in a clash of sectionalist ideologies at the heart of which lay some of the very same race issues that had preoccupied many ethnologists for over a decade. Following the election of Abraham Lincoln, in early 1861 seven southern states seceded as the Confederate States of America under their own chosen President, Jefferson Davis.¹²⁵ Four more states were to follow after Confederate artillery opened fire on federal troops stationed at Fort Sumter, South Carolina on April 12 and the garrison fell to CSA forces.

Few would argue that slavery became the most visible and most divisive issue of the Civil War when Lincoln issued the Emancipation Proclamation in 1862. Yet, the war had a remarkable impact on the “Indian situation” out west, a fact which is often overshadowed in historical accounts by the intricate military campaigning and politics of the east, where most of the war was actually waged. The outbreak of hostilities had the effect of drawing federal troops out of the west for service against the Confederate army, and this directly contributed to a surge of widespread unrest in virtually all of the western

¹²⁴ Thomas Tax, Development of American Archaeology, 258.

¹²⁵ Earlier, Davis had been a Mississippi senator, and he sat on the Smithsonian Board of Regents in 1848 (the list of Regents’ names may be found printed at the head of the Smithsonian Annual Reports).

Indian lands.¹²⁶ The Apache campaigns of the southwest, the Sioux uprisings in Minnesota and the Dakota territory, the Cheyenne and Arapaho campaigns in Colorado, to name but a few, all began during the period of the Civil War, and most of these conflicts turned into prolonged and bloody affairs lasting for decades afterwards. During the four years of the War itself, many tribes within the Indian Territory (present day Oklahoma) allied with the Confederacy—in fact, a number of individuals within certain tribes owned black slaves.¹²⁷ Both the North and the South, however, sought to recruit Indians as troops (with varying degrees of success), with a net effect of further fracturing Indian communities. The Cherokees and Creeks, for example, each provided soldiers to both sides.¹²⁸

The federal victory over the southern Confederacy with Lee's surrender on April 14 of 1865 ensured both the preservation of the Union and the abolition of slavery, but at a very great cost—the war had claimed the lives of over 620,000 soldiers and cost the government billions of dollars, and the nation remained a wounded and deeply conflicted place. Five days after Lee's surrender, Republican President Abraham Lincoln was assassinated in a Washington theater by a radical pro-south sympathizer. The more Democratic-leaning Vice-President Andrew Johnson was left with the unenviable job of initiating policies for the re-assimilation of the secessionist southern states, an arduous process that would ultimately get him impeached by the House of Representatives.¹²⁹

¹²⁶ Nichols, *Indians in the United States and Canada*, 213-20.

¹²⁷ Murrin, et al., *Liberty, Equality, Power*, 491.

¹²⁸ Nichols, *Indians in the United States and Canada*, 213.

¹²⁹ General historical background in this and the following two paragraphs is drawn loosely from the following sources: Murrin, et al., *Liberty, Equality, Power*, chapters 13-18; Randall Woods and Willard

With the end of the war, as part of the general reconstruction of the nation, various abolitionist and reform groups became more thoroughly involved in attempting to alleviate the conditions of both blacks and Indians, both of whose standing in the new order was not clear. In the South, over three million emancipated slaves found themselves with few or no prospects for work other than sharecropping, which was little different than working under slavery. Blacks faced rising waves of southern racism, manifested most viciously in newly formed organizations like the Ku Klux Klan, but instituted as well in various discriminatory Black Codes enacted by southern legislative assemblies.¹³⁰ For many former slaves, daily life after emancipation was no better or even worse than it was before, and the idea of an integrated American society remained a practical impossibility. Organizations such as the Freedmen's Bureau, as well as various other missionary societies, sought to help by providing education to former slaves.¹³¹

Indians faced similar prospects. Despite pronouncements of policy shapers like Schoolcraft about the inability of Indians to progress, Christian missionaries and other humanitarians soon enough became more heavily involved in efforts to acculturate

Gatewood, The American Experience: A Concise History (Forth Worth, Texas: Harcourt, 2000), chapters 10-14.

¹³⁰ Murrin, et al., Liberty, Equality, Power, 468-9 and 479-80.

¹³¹ Murrin, et al., Liberty, Equality, Power, 471-2 and 493; Fixico, "Federal and State Policies and American Indians," 382; Edmunds, "Native Americans and the United States, Canada, and Mexico," 400-1; Nichols, Indians in the United States and Canada, 215-20.

Indians into American society by “civilizing” them.¹³² Acculturation meant further abandoning the tribal way of life, adopting Christianity and agriculturalism, and sending children to often distant boarding schools. Forcibly instituting such changes, obviously, did not often occur easily—if at all—and it ensured an ongoing military presence on western Indian lands for a long, long time to come.

With the end of the Civil War, the status of both blacks and Indians within American society remained indeterminate. Both groups faced their own sort of scrutiny from politicians, theologians, ethnologists, and, really, from many white Americans. Could blacks be assimilated into American society? Could the annihilation of Indian populations be stopped before it was complete, or was this the inevitable and natural course of things?

3.5 Expanding possibilities.

In the Smithsonian Annual Report for 1865, the first after the end of the Civil War, Joseph Henry commented that ethnology demanded special attention because “it is intimately connected with our history and politics.”¹³³ Those few words masked so very much. As one of the nation’s leading scientific organizational figures, Henry could appreciate that ethnology in all its various facets had taken on a renewed significance in matters of American history and politics. An astonishing conflation of events, from the crushing impact of western expansion on Indian tribes, to the emancipation of slaves

¹³² Murrin, et al., Liberty, Equality, Power, 471-2 and 493; Fixico, “Federal and State Policies and American Indians,” 382; Edmunds, “Native Americans and the United States, Canada, and Mexico,” 400-1; Nichols, Indians in the United States and Canada, 215-20.

¹³³ Henry, Smithsonian Annual Report for 1865, 46.

during the war, in addition to the discovery of fossil humans in Europe and the subsequent awareness of immense human antiquity, had all come together to make ethnology a much more visibly relevant scientific activity. Given the Institution's proximity to the center of American government, it was particularly well-placed to continue as a leading promoter of ethnological research.

In Europe, the Société d'Anthropologie in Paris and the Anthropological Society of London had been established between 1859 and 1862. Founders Pierre Broca and John Hunt, respectively, were both anatomists who, like the American polygenists, wanted to move beyond what they saw as an outmoded, "soft" ethnology anchored to philology. Broca's efforts, in particular, were remarkably successful in revitalizing phrenology there and instituting French anthropology as a physical science of human typologies.¹³⁴ In America, Ephraim Squier attempted to reform the floundering American Ethnological Society in New York along similar lines in the early 1860s, but his promotion there of polygenism fell largely on antagonistic ears. Later, in 1869, he would form on his own the Anthropological Institute of New York. In the first volume of its journal, Squier included an article by Broca defining the physical contours of anthropological science. After one year, Squier's Anthropological Institute disintegrated.¹³⁵

In the United States, however, the sting of Nott and Gliddon was felt too sharply as a result of the Civil War. Physical anthropology thus did not take hold quite the way it did across the Atlantic, at least not through the rest of the nineteenth century. Thanks to the American polygenists, the subject of human anatomy had to be handled cautiously.

¹³⁴ Roger Smith, The Norton History of the Human Sciences (New York: Norton, 1997), chapter 11, especially p. 398.

Nonetheless, as before the war, anatomy was still regarded as an important component of the broader field of ethnology.

In that same 1865 Smithsonian report, Henry outlined in some detail a program for future ethnological research in the United States. Coming full circle from where I began this chapter, it would be good to re-iterate and expand upon Henry's words along these lines. He wrote:

the subject of races is one which involves questions as to their origin and characteristics which can scarcely be discussed at the present time with that dispassionate logic and strictness of induction which is necessary to the establishment of truth. Still, much can be done in the way of collecting and recording facts which may serve as the basis of future investigation. Some of these, such as those relative to the Indians, are rapidly passing away into oblivion; and others, which regard the negro, require to be disentangled from much prejudice and misrepresentation. The peculiarities of these races should be critically examined and truthfully recorded; to do this is a duty we owe to science and humanity.¹³⁶

Ethnology as moral imperative, indeed.

The study of languages remained most prominent within Henry's vision as a guide "in the labyrinth of ethnological research," particularly in regard to the study of American aboriginals.¹³⁷ Artifacts and earthworks aside, Henry thought that the immense plurality of Indian dialects still offered the best "indications of affiliation or diversity not otherwise attainable."¹³⁸ It would be incumbent upon Smithsonian researchers to develop a surer system for recording vocabularies as well as the identity of sounds that have few or no parallels in English (or the mother tongue of any white researcher). Think of the

¹³⁵ Tax, Development, 299-300.

¹³⁶ Henry, Smithsonian Annual Report for 1865, 46.

¹³⁷ Henry, Smithsonian Annual Report for 1865, 46.

¹³⁸ Henry, Smithsonian Annual Report for 1865, 46.

difficulty faced by these linguists—how, exactly, would one document a vocal utterance that one could not audibly express with accuracy? There were no wax cylinder phonographs, no technological means whatsoever to capture sound faithfully. It had to be done with written symbols. Henry credited Gibbs's work along these lines for making a good start of it; the problem was that many Indian dialects were disappearing. The work needed to be done quickly.

Next, Henry addressed anatomy, which he certainly believed relevant to the progress of ethnology. He had, in fact, made arrangements for the Smithsonian to obtain records of the physical characteristics of the soldiers in the U.S. army during the war. These included "measurements of different parts of the body [in order] to ascertain the peculiarities of the different nationalities represented."¹³⁹ The collection of records was initiated by the Smithsonian, but the enormity of the task necessitated that it be taken over by the U.S. Sanitary Commission, a group that had strong connections with the Army Medical Museum formed during the war.¹⁴⁰ Henry further proposed to extend the program of measurements to Indians, blacks, and "the descendants of the civilized inhabitants of different parts of Europe, thousands of whom are now flocking to our shores."¹⁴¹ He shared the view of many ethnologists who saw the American mixing pot to offer "a better opportunity to study the peculiarities of a number of races than is perhaps

¹³⁹ Henry, Smithsonian Annual Report for 1865, 47.

¹⁴⁰ On the formation of this museum, and on its connections with the Sanitary Commission, see, Conor Burns, Recording the Medical Lessons of War: The Formation of the Army Medical Museum and the U.S. Civil War, unpublished master's thesis, University of Toronto, Institute for the History and Philosophy of Science and Technology, 1999.

¹⁴¹ Henry, Smithsonian Annual Report for 1865, 48.

to be found in any other single portion of the earth.”¹⁴² The commingling of races, of people from many places, offered unparalleled possibilities for those interested in studying hybridity. Nott and Gliddon, perhaps, had not foreseen the explosive levels of immigration that the United States would witness in the decades after the war; for someone like Daniel Wilson, the prospects of so much mixing offered untold advantages for research.¹⁴³

Henry finally commented on the importance of archaeology. Making special note of the interesting shell mound discoveries in northern Europe, he credited Rau’s recent important work in conjunction with the Smithsonian along similar lines. Henry himself was particularly fascinated by the ethnological significance of these structures, noting that the

occurrence of the Danish shell-heaps, whose history is merged in the twilight of civilization, and those of a similar character in America, show that the early condition of man is everywhere essentially the same, while the rude implements which are obtained from them indicate a similarity of wants and an identity of mental characteristics by which these wants are supplied.¹⁴⁴

¹⁴² Henry, Smithsonian Annual Report for 1865, 48.

¹⁴³ See, for example, Daniel Wilson, “Some American Illustrations for the Evolution of New Varieties of Man,” The Journal of the Anthropological Institute of Great Britain and Ireland 8 (1878-9), 338-59. Wilson noted that since European discovery, “a constant transfer of races from the Old to the New World has been taking place, alike by voluntary and enforced migration; with results involving a series of undesigned yet exhaustive ethnological experiments carried out on the grandest scale”(p. 338). There had been prolonged periods of intermixture between different races, including of “the most highly cultured races of Europe with purely savage tribes” (p. 338).

¹⁴⁴ Henry, Smithsonian Annual Report for 1865, 50.

He was well aware of the potentials of developmental classificatory systems, and clearly saw how such approaches could further provide some organizational basis for managing specimens. He wrote,

An effort will be made during the present year to properly arrange and fully display [our archaeological collections] for study. The comparison of the early savage implements in different countries is full of interest. We see from this that what is called the stone age is not a period of absolute time, but a stage of civilization, long past in one portion of the earth, while existing at present in another.¹⁴⁵

The Civil War did have the effect of bringing visitors to Washington in great numbers, and Henry (along with assistant secretary Spencer Baird), ensured that public displays of archaeological materials were included within the Institution's very limited museum space.¹⁴⁶

Until late in the 1870s, however, the role of the museum within the Smithsonian was ambiguous. Henry himself had always been cool to the idea—from the beginning he thought that, without additional appropriations from Congress, it ran the danger of draining the financial resources of the Institution. As a result, Henry was wholly unwilling to spend much money on artifact collections, relying mainly upon donations or the spoils of government expeditions. Even the great Squier and Davis collection—the fruit of what Adolphe Morlot called “one of the most splendid archaeological works ever published”—could not be retained.¹⁴⁷

¹⁴⁵ Henry, Smithsonian Annual Report for 1865, 50.

¹⁴⁶ Tax, Development, 273-4.

¹⁴⁷ Morlot, 29. Davis, who possessed the collection, sold it to a wealthy English entrepreneur in 1863 for the tidy sum of ten thousand dollars, a bitter loss for a fledgling American science. See, Terry Barnhart,

Underlying Henry's program was a perceived need for greater control over the centralization of ethnological data. At this point in time, the Smithsonian's means were actually rather limited along these lines, at least to do so at the scale required by a comprehensive science of ethnology. Henry's program would especially see some form of fruition in 1879, a year after his death, in the formation of the Bureau of Ethnology under John Wesley Powell.

If anything, Henry's 1865 report on ethnology highlights the extent to which the science had diversified. And yet, the essentials of this plan—the emphasis on linguistics, the inclusion of comparative anatomy, the interest in ethnography and archaeology—had been integral to American ethnological research for quite a long time. What was changing was the greater perception of ethnology as a unified science that had immediate relevance to contemporary problems in American society and to issues of American history. The impact of evolutionism certainly helped. Increasingly, ethnology was attracting a great deal of attention beyond what in reality had been a rather limited scholarly community, even if numerous individuals, for example, collected relics and studied mounds as a hobby. Gradually over the next two decades many new outlets would appear for the promotion of ethnological research. At the time in 1865, however, it seemed at least somewhat reasonable to Henry that a single institution—the Smithsonian—might be able to embrace an entire “science of man,” but this, too, would ultimately prove entirely frustrating. Even by 1868, the Institution had transferred its human osteological

“An American Menagerie: The Cabinet of Squier and Davis,” *Timeline* 2 no. 6 (1985-86), 2-17, p. 7; Meltzer, 68.

collection to the Army Medical Museum, although Henry hoped that by keeping the specimens in Washington at least they might remain “part of one harmonious system.”¹⁴⁸

Others shared similar concerns. Under Wills DeHass, for instance, a special committee on archaeology had been formed within the American Association for the Advancement of Science as early as 1858, but the disruption of the war meant that little archaeological work was accomplished until well afterward. Only at the 1868 meeting in Chicago could DeHass report on the “rising importance of American Archaeology” and the need for vastly increased research on American antiquities.¹⁴⁹ Of great concern to him was the fact that a “well organized society devoted to the investigation of *American Archaeology*” had been active in Paris (this was the International Congress of Americanists).¹⁵⁰ DeHass’s sense of nationalism was gravely wounded by this, as well it would have by the loss of Davis’s collection to England. DeHass went on the offensive:

Whilst the enlightened Governments of Europe are zealous and active in efforts to trace out the beginning, progress, and primordial life of man, to develop his remains of art, what are we doing?...It is discreditable to our intelligence as a cultivated people, that so little attention is given this important branch of scientific inquiry.¹⁵¹

DeHass wanted the full membership of the AAAS to recognize and support archaeology. As a testament to how much more work the Smithsonian needed to do, he particularly called for the formation of a “National Museum for American Antiquities,” one that would require a sizeable financial base. DeHass’s committee certainly did promote much

¹⁴⁸ Joseph Henry, “Report of the secretary,” *Smithsonian Annual Report for 1868*, 15.

¹⁴⁹ Wills DeHass, “Report on Archaeology and Ethnology,” *Proceedings of the American Association for the Advancement of Science for 1868*, 303-4, p. 303.

¹⁵⁰ DeHass, “Report on Archaeology and Ethnology,” 304. Italics in original.

¹⁵¹ DeHass, “Report on Archaeology and Ethnology,” 304.

archaeological research (particularly on the stone age in America), but within the open structure of the AAAS, it ultimately lacked the institutional stability to do more than merely advocate the promotion of research. Even after the 1875 formation of a formal Anthropology Section under the guidance of Frederic Ward Putnam, and the fact that numerous prominent anthropologists occupied leadership roles in the AAAS, the society did not have the “bricks and mortar” infrastructure to house a central repository of anthropological materials.¹⁵² As the Smithsonian anthropological initiatives came to fruition around 1880 with the creation of both the Bureau of Ethnology and a dedicated National Museum building, so, too, did those of another major institutional player in the United States—the Peabody Museum of American Archaeology and Ethnology.

The Peabody Museum was founded in 1866 after a wealthy New England expatriate banker named George Peabody donated \$150,000 to Harvard for the formation of a museum of archaeology.¹⁵³ Peabody was himself interested in American history, having financed the American department at the 1851 Great Exposition in London after

¹⁵² Lewis Henry Morgan, Daniel Wilson, and Frederic Ward Putnam are three of the more notable examples of such leadership figures within the AAAS.

¹⁵³ On the formation of the Peabody Museum, see: Curtis Hinsley, “The Museum Origins of Harvard Anthropology, 1866-1915,” in Clark Elliot and Margaret Rossiter, ed., Science at Harvard University: Historical Perspectives (Bethlehem, PA: Lehigh University Press, 1992), 123-45; Hinsley, “Frederic Ward Putnam,” in Tim Murray, ed., Encyclopedia of Archaeology: The Great Archaeologists, Volume 1 (Santa Barbara: ABC-CLIO, Inc., 1999); Tax, Development, 307-16; Frederic Ward Putnam, “The Peabody Museum of American Archaeology and Ethnology in Cambridge,” Proceedings of the American Antiquarian Society for 1889, 180-90; “Mr. George Peabody’s recent gifts to science,” American Journal of Science and Arts series 2, 43 (1867), 131-33; “Death of George Peabody,” American Journal of Science and Arts series 2, 48 (1869), 442-5.

Congress failed to do so, and his gift to Harvard was but one of a number of similarly liberal donations. The formation of the museum was a remarkable event, and not without controversy among Harvard administrators, for here was an institution devoted exclusively to the subject of ethnology and archaeology. Jeffries Wyman, a trained Harvard anatomist, was appointed curator, and he immediately undertook his own winter researches on shell mounds in Florida despite increasingly poor health. Wyman ran the museum conservatively in its tenuous early years, but even by the time of his early death in 1874, he had established a system “of teaching by correspondence and collaboration with workers in the field” that would remain an integral component of Peabody archaeological practice for the rest of the century.¹⁵⁴ It was a system much like that utilized by the Smithsonian. There were many avocational archaeologists out there who continued their work, and some of them were paying attention to the involvement of these institutional centers. And they wanted to participate. Under Wyman, the Peabody’s research focused on coastal shell mounds, Tennessee mounds, as well as ancient man. Under Frederic Ward Putnam, who became curator and director after Wyman’s death, the Peabody greatly expanded its scope and the scale of its investigations to rival, and even surpass, those of the Smithsonian. Putnam himself took a great interest in the Ohio valley area, and his work there in the 1880s had a major impact on the organization of archaeological practice. At this point in time, he, along with the archaeologists of the Smithsonian’s Bureau of Ethnology, had begun articulating the need for large-scale institutional centralization of archaeology.

¹⁵⁴ Hinsley, “Museum Origins,” 123.

3.6 Concluding remarks.

I will close this chapter with some final commentary on the diversification of ethnology that occurred after the Civil War. These changes must be recognized as part of much broader, more general transformations that were taking place in American science and society at the same time. Both the Smithsonian Institution and the American Association for the Advancement of Science had been central to the changes in the social structure of scientific practice by promoting novel researches and by creating communities of scientists linked together through publications, meetings, or by providing facilities for research. American science had become a more communal, networked activity. By and large, however, these networks were anchored to focal points in the North, and were dominated by northern interests. The impact of the war, therefore, was most definitely felt throughout America's scientific community.¹⁵⁵ The issue of slavery, for one thing, ultimately led many members of the AAAS to choose sides in the conflict, and many served in the military.¹⁵⁶ Many research and publication agendas within the large scientific organizations were also disrupted (witness DeHass's comments above, as well as the fact that the Smithsonian Reports for 1863-71 did not get printed until 1872). It should go without saying that the damage wrought upon the South's infrastructure—as well as to its institutions of higher learning—further contributed to a long-term dominance of northern interests in matters of science, technology, and industry. The situation was helped by the fact that the nation's economy had shifted from the

¹⁵⁵ See Robert V. Bruce, The Launching of American Science 1846-1876 (Ithaca, NY: Cornell University Press, 1987), especially chapters 20-4.

¹⁵⁶ Kohlstedt, Formation,; Bruce, 271-6.

agricultural south to the increasingly manufacturing-based north, where machines were altering forever the shape of free-labor capitalism and industrial society.

Overall, the Civil War era seems to mark somewhat of a general turning point for American science. One recent commentator has said of the war that “at its end the scientific community was moving from central authority toward a federalism of specialized fields.”¹⁵⁷ Afterwards, no one or two cliques of elite organizations dominated the scene quite the same way they had before, but it would all take some time. A much broader process of diversification and specialization in the sciences had begun, propelled by a booming northern economy, the growth of industry, and the rapid integration of technology into many facets of American life, as well as the desire to more fully explore the great western expanses of the continent. All this is not necessarily to say that antebellum institutions suffered a dramatic decline of influence—only that in the years afterward, what gradually emerged was a pluralism of outlets for the promotion of science, as well as numerous new educational opportunities for those seeking specialized training. Of course, within each of these specialized fields, certain individuals and institutions would emerge as dominant authorities.

One important factor in all this was the increased role of the federal government in funding and sponsoring research, much of which involved a countless succession of surveys and expeditions in the west and elsewhere. The Smithsonian would benefit greatly these endeavors—between 1867 and 1877, the heyday of government expedition science, it received numerous specimens from no less than fifty-eight separate

¹⁵⁷ Bruce, Launching of American Science, 305.

government sponsored explorations.¹⁵⁸ The leader of one of the largest of these ventures, John Wesley Powell, would be appointed in 1879 to head both the U.S. Geological Survey and the Smithsonian's newly formed Bureau of Ethnology. The Smithsonian, however, was by no means the sole beneficiary of government expedition science—the Peabody Museum, to name one other, also participated in some of these undertakings, often in collaboration with the Smithsonian.

Given the right perspective, and as long as one did not probe too deeply into the increasingly deplorable conditions of its growing economic underclasses, American society in the 1860s was metamorphosing through technology. Technological marvels like the steamship, the telegraph, improved firearms, and the railroad played integral roles in the war itself, even if they had been developed earlier.¹⁵⁹ The war just seemed to showcase them. More than any other machine, the last of these, the railroad, symbolized the unstoppable momentum and transfigurative virtues of American technology in both geographic and economic terms. By the end of the war, railroad networks—again, centered predominantly in the north—had filled the distances westward to the Mississippi River with thirty-five thousand miles of rails.¹⁶⁰ After the War, railways rapidly expanded across the prairies with thousands more miles of rail, and lines in the South were rebuilt. The first fully transcontinental line went into service in 1869.¹⁶¹ People and raw materials could now be transported with relative ease over the greatest of distances, and a prolonged symbiotic relationship between railroad growth and economic growth had

¹⁵⁸ Smithsonian Annual Report for 1877, 110-4.

¹⁵⁹ Bruce, Launching of American Science, 306.

¹⁶⁰ Woods and Randall, 361.

¹⁶¹ Murrin, et al., Liberty, Equality, Power, 487.

begun. The frontier itself was gone, even if a frontier mentality continued to define the ethos of the Great American West.

The railroad would very quickly play a central role in the development of anthropology, though few could have foreseen the full magnitude of its impact. Of course, it facilitated the rapid transportation of artifacts and personnel. In the 1880s, Frederic Putnam could write letters from his Cambridge office and two days later be excavating a mound in the remote hinterlands of south-central Ohio. The railroad also contributed to the changing ecology of the west as a direct result of expansion. Although the impact was much broader, the Indians of the Great Plains, particularly, were devastated by the near eradication of bison herds within two short decades after the 1860s. The mechanization of the United States during this time had created a massive demand for machinery belts made of bison leather, and the creatures upon which much Indian economy rested were slaughtered by the millions. Railroads facilitated the shipment of hides in previously unimaginable numbers.¹⁶²

As for many Indians themselves in the decades after the Civil War, life only became more harsh. Land issues were dealt with by the federal government relatively swiftly—particularly for those tribes who sided with the South—and federal policy makers deemed it necessary to regulate more strictly the apportioning of tribal lands into administrative units overseen by government agents. All of this came fully to fruition under the “Peace Policy” of Republican war hero Ulysses Grant, who won a bitter 1868 presidential

¹⁶² Loretta Fowler, “The Great Plains from the arrival of the horse to 1885,” in Bruce Trigger and Wilcomb Washburn, ed., The Cambridge History of the Native Peoples of the Americas: Volume 1, North America, Part 2 (Cambridge: Cambridge University Press, 1996), 1-56, especially p. 34; Murrin, et al., Liberty, Equality, Power, 494.

election in which the Ku Klux Klan visibly sought to undermine Republican support in the south. Under Grant a new Bureau of Indian Commissioners was created in 1869, an agency which has been described recently as “one of the most extraordinary breakdowns of the separation of church and state in American history.”¹⁶³ Under this system, Indian groups were constrained onto designated and well-defined reservations administered by government agents; for the most part, Christian missionaries were appointed as federal agents (perhaps the only notable exception was the Seneca Indian Ely Parker). Although Grant’s “Peace Policy” in some respects represented a definitive “moment” in U.S.-Indian relations, in reality it was but a continuation of a long history of misunderstanding and mistreatment. The policy seems harsh in hindsight; nonetheless, it was the product of humanitarian ideologies, however ill-conceived. The alternative was total extinction—a near certain result in the face of the onslaught of western expansion.

Native Americans themselves were disappearing. And so too were the ancient earthworks of the Mississippi and Ohio valleys. The intensification of agriculture in America’s breadbasket had unimaginably destructive consequences for the mounds in a very short time. The data of American ethnology was definitely being lost.

¹⁶³ Fixico, “Federal and State Policies and American Indians,” 382.

Chapter 4.

Perspectives on Ohio valley prehistory in the 1880s: Peabody and Smithsonian approaches to the problem of the mounds.

4.1 Introduction.

By around 1880, just as in earlier parts of the century, the prehistoric mounds and earthwork complexes of the Mississippi and Ohio River valleys continued to present major challenges to thinking about the peopling of the Western Hemisphere. Within this context, the identity of the moundbuilders remained one of the central enigmas for American ethnologists. At the same time, the accelerating pace of development and the demographic boom in the American middle west had further largely detrimental consequences for the mounds, mainly due to the intensification of agriculture, the expansion of urban environments, and increasing numbers of hobbyist archaeologists, artifact collectors, and looters.

By far and away, however, the single most important development in American archaeology by around 1880 had to do with the increasing involvement of the Peabody Museum of American Archaeology and Ethnology and the Smithsonian Institution in the form of its newly formed Bureau of Ethnology. Both organizations sponsored extensive explorations of Ohio valley mounds at this time. Figureheads of these institutions were recognized as authorities in matters of archaeological theory and practice, and they sought to promote particular theoretical viewpoints regarding the place of moundbuilders in the overall scheme of North American prehistory. They also sought to standardize practice in the face of an unprecedented loss and dispersal of archaeological data. The resources provided by these institutional bases meant that data could now be centralized in equally

unprecedented ways, a step which was seen by the institutional heads as necessary for the progress of archaeology as a science. Centralization on a large scale could not be done easily, however. There were no pre-existing structures of professional practice to be drawn upon. Instead, the process required establishing and maintaining networks of associations connecting the institutional centers with those spaces where archaeological data existed. Correspondent field workers became essential links within these networks, but so, too, did many other individuals who could facilitate in some way the process of centralization.

My aim in both this chapter and the next is to convey a sense of the overall complexion of Peabody and Bureau of Ethnology mound archaeology in the Ohio valley during the 1880s. This chapter will mainly address the intellectual context for this work, whereas the next chapter will focus more on the social dimensions of centralization. Here, I will provide some background to the Peabody and Smithsonian mound initiatives of the decade and I will outline the philosophical perspectives that characterized these institutions' approaches to the study of Ohio valley prehistory during the decade. I will also consider some ways in which attitudes about the museum treatment of objects figured into contemporary perceptions of the different styles of archaeology practiced at the Peabody and Smithsonian.

4.2 Peabody and Smithsonian approaches to prehistory and the problem of the mounds.

During the decade of the 1880s, the mounds arguably received more archaeological attention than at any other time in history, mainly due to the involvement of the Peabody

Museum, in Cambridge, Massachusetts, and the Smithsonian's Bureau of Ethnology in Washington, DC. Frederic Ward Putnam, curator and director of the Peabody, and John Wesley Powell, director of the Bureau of Ethnology, both saw a need for more rigorous approaches to the anthropological problem of the moundbuilders. In 1880 Putnam began turning his attention more specifically to the Ohio mounds, and by 1882 the Bureau's Mound Exploring Division was up and running under the supervision of Cyrus Thomas, who was looking to gain access to the same region shortly thereafter.

As we know from previous chapters, southern Ohio was where especially dense concentrations of elaborate mound and earthwork complexes could be found. This region had been widely perceived as a sort of moundbuilder ground zero from much earlier in the century, and since then the idea of some sort of separate race of advanced moundbuilders had not lost currency in many quarters. Within ethnological and archaeological circles of a more or less scholarly bent, however, it would be safe to say that the more fanciful moundbuilder stories—such as that they were lost tribes of Israel or Vikings or some such thing—were no longer held in much regard. But the legacy of Ephraim Squier and Edwin Davis's 1848 Ancient Monuments of the Mississippi Valley nonetheless continued to provide most archaeological scholars with the framework for thinking about moundbuilders as a culturally and racially homogeneous population that was geographically extensive and was essentially dissimilar from historic or modern Indian groups. The idea that mounds might have been constructed by a number of different groups who could be culturally differentiated from one another according to other criteria was simply not part of the dominant way of thinking about them. An alien from outer space observing the earth might describe a pervasive race of automobile users,

but from our point of view this would be a strange way of defining a group of people who otherwise might have little in common. Of course, nineteenth century ethnologists and archaeologists were not outer space aliens, but they most certainly were looking at the native Americans from an outside perspective that was skewed by white Euro-American biases. Among them was a belief that technological prowess (such as that evinced by feats of moundbuilding) stood as an indicator of progress and advancement. But also, as the 1870s came to a close, the situation for the increasingly displaced living Indian tribes in the west had degenerated. White attitudes toward them had not improved overall, either, making the supposed gap between Indian tribes and the mysterious moundbuilders seem all the more manifest and unbridgeable.

Dissenting sensibilities did exist but were in the minority and did not have much influence. The Scots-Canadian Daniel Wilson had pointed out quite explicitly in the late 1850s that the ethnological record as a whole, both past and present, demonstrated a vast range of diversity among America's aboriginal inhabitants that could not be ignored.¹ Although Wilson was looking most specifically at cranial characteristics, his measured refutation of Samuel Morton's polygenist school of anatomical ethnology also implicated the entire breadth of American ethnology for its narrow typological thinking. The crux of his critique was methodological: he thought all ethnologists—not just the polygenists—had been too quick to subsume apparent variation between Native American groups into taxonomic categories that were either too broad to be of much use or were simply non-

¹ See especially Daniel Wilson, "On the Supposed Prevalence of One Cranial Type Throughout the American Aborigines," *Edinburgh New Philosophical Journal* (January 1858), 1-32.

existent.² The result was that a very general, simplistic picture of aboriginal North Americans emerged, one that downplayed very real, though often subtle, distinctions between innumerable sub-populations. Wilson's views had very little impact and narrow typological thinking prevailed, especially in the area of archaeology and the mounds. In many respects, the moundbuilders remained a race unto themselves, and until archaeologists more systematically integrated archaeological data of the mounds into the grand scheme of human history and development in the New World, a large black hole would remain in the ethnological record.

Putnam and Powell were the uppermost figureheads of a new generation of institutionally-situated authorities in matters of American ethnology. Once they set their sights on the Ohio valley mounds in the 1880s, they did so at least partly to combat the persistence of this kind of typological thinking about the moundbuilders. In their own ways, each man was interested in breaking down broad ethnological categories pertaining to American aboriginals, although their reasons for doing so (as well as their perceived

² Wilson's main target was the Morton school's insistence on a single taxonomic "American race" for *all* New World aborigines. His primary concern was that Morton and his adherents such as Josiah Nott, George Gliddon, and Louis Agassiz, had been, as he saw it, a bit too creative (or blind) in fitting a vast range of variation within the category by allowing for such adjustments as two quite distinct "families" (Barbarous and Toltecans) and in drawing sharp boundaries between the American type and others. Wilson was particularly vexed by the further implication that the Morton school's "American race" stand as a separate human species. Wilson presented his own craniometric data to show that the degree of physical difference between supposed American skulls and other supposed types was insignificant in comparison to the range of variation within the American type itself that was accepted even by the Mortonians themselves. For Wilson, humans comprised one species with a vast range of variation.

goals for so doing) contrasted sharply and their respective views were no less subject to bias and preconception than ethnological scholars of earlier generations. What truly set the Peabody and Smithsonian mound initiatives of the 1880s apart from those of earlier times was the institutional support base that lent longer term stability to the work and opened the possibility of large-scale centralization.

The Peabody Museum of American Archaeology and Ethnology was established in 1866 and quickly became a leading center for the promotion of archaeological research under curator Jeffries Wyman.³ Like the Smithsonian, it owed its existence to the generous financial bequest of a man interested in supporting such ventures and making a lasting legacy of his name. Unlike the Smithsonian, however, the Peabody Museum was not given to the general “increase and diffusion” of knowledge. Instead, it was formed with a much more specific focus and was in fact the first institution dedicated solely to the pursuit of archaeology and ethnology in the United States. Putnam had cultivated a deep interest in ethnology for some time prior to 1873, when he became permanent secretary of the American Association for the Advancement of Science and was largely responsible for redesigning an “Anthropology” subsection within Natural History.⁴ In 1875, he was named curator and director of the Peabody and by the end of that decade, the Peabody had become the Smithsonian’s chief institutional rival in these matters. Putnam was a gifted naturalist who had trained in Cambridge under the rigorous hand of

³ Hinsley, “Museum Origins of Harvard Anthropology,” in Clark Elliot and Margaret Rossiter, ed., Science at Harvard University: Historical Perspectives (Bethlehem PA: Lehigh University Press, 1992), 121-45.

⁴ Joan Mark, Four Anthropologists: An American Science in its Early Years (New York: Science History Publications, 1980), 15-21.

Louis Agassiz, the eminent Swiss-American naturalist whose Museum of Comparative Zoology sat next door to the newer Peabody building.

Putnam's archaeological interests initially may have been sparked by the shell heaps of the New England coast, but the Ohio valley increasingly drew his attention and he came to regard this region as centrally important to an overall understanding of North American prehistory. The Peabody itself underwent an architectural expansion in 1877, a development which was an important precursor for the archaeological work Putnam undertook through the 1880s.⁵ In 1881—when the AAAS met in Cincinnati—Putnam began collaborating closely with some members of the Madisonville Literary and Scientific Society who already had been conducting archaeological explorations within the Little Miami River valley just north of Cincinnati. One of these collaborators, Charles Metz, became not only Putnam's chief archaeological correspondent in Ohio but probably also his most reliable and productive correspondent overall from the period. Within a couple of years, Putnam and Metz began orchestrating fieldwork throughout southern Ohio.

The Bureau of Ethnology was formed in 1879, giving existing Smithsonian anthropological initiatives much needed direction and focus.⁶ Prior to this time, the

⁵ The Peabody existed in crowded temporary quarters for over a decade until 1877, when a substantial fire-proof building was completed. By the end of the 1880s, a second section was added to it. For details, see, F.W. Putnam, "The Peabody Museum of American Archaeology and Ethnology in Cambridge," Proceedings of the American Antiquarian Society for 1889, reprinted in The Archaeological Papers of Frederic Ward Putnam (New York: AMS Press, 1973), 161-2.

⁶ The fullest available account of the early history of the Bureau is found in Curtis Hinsley, The Smithsonian and the American Indian: Making a Moral Anthropology in Victorian America (Washington

Smithsonian had indeed served an important function as a publisher of anthropological researches and as a provisional repository of ethnographic and archaeological collections. The institution supported some level of in-house collections-based research as well even before the National Museum was officially completed in 1880. But these activities were for the most part piecemeal affairs, conducted by individuals who were external to the Smithsonian itself. The advent of the Bureau of Ethnology marked the start of something new: it provided the Smithsonian with its own in-house anthropological research program with a core staff dedicated to particular, long-term researches. The National Museum, though its own operational entity, provided increased storage and research space along with a number of staff dedicated to the care of ethnological collections. The Museum's expansion conveniently made much of the Bureau's collecting practices possible. Compared to the Peabody, however, archaeology at the Smithsonian was a more cumbersome affair

John Wesley Powell, Civil War and Interior Department western survey hero, lobbied hard to Congress in 1879 for the creation of the Bureau under the same bill that established the United States Geological Survey. Powell, whose relationship with the Smithsonian solidified as a result of the 1876 Centennial Exposition, was placed in charge of both of the new agencies, and thus the Bureau was also closely allied with the Geological Survey. The Bureau of Ethnology was intended to serve as the institutional base for a comprehensive North American research program with a focus on ethnographic and linguistic studies of western Indian tribes. This was Powell's vision for the organization and it meshed neatly with Smithsonian secretary Joseph Henry's previously

unfulfilled idea for a comprehensive multi-volume Contributions to North American Ethnology series.⁷ Given the deteriorating condition of western Indian tribes a dire sense of urgency underlay the Bureau's work, and it must also be pointed out that federal financial support for the study of Indian customs and languages on such a scale came largely for the work's perceived utility in facilitating treaty negotiations.⁸ As Curtis Hinsley has shown in his rich account of the Bureau's non-archaeological work, this was not value-free science.

Initially, Powell was concerned only with studies of western Indians and was not much interested in pursuing archaeological work. That changed very quickly, however, as pressure mounted for the Bureau to deal head-on with the problem of the mounds. By his own account, Powell came to realize that any designs on a comprehensive ethnology of North America had to address this troubling aspect of the prehistoric record.⁹ Congress then appropriated an additional five thousand dollars to the annual operating budget of the Bureau for the creation of a Mound Exploring Division. Wills De Haas, an active promoter of archaeology within the American Association for the Advancement of Science, was first given the head position of the Division but quit on short notice. Powell then put fellow military and western survey veteran Cyrus Thomas in charge.¹⁰ Thomas

⁷ Henry died in 1878, at which point assistant secretary Spencer F. Baird became secretary.

⁸ Curtis Hinsley, The Smithsonian and the American Indian, especially chapter 6.

⁹ J.W. Powell, "Introduction," Twelfth Annual Report of the Bureau of Ethnology for the Years 1890-1 (1894), xl-xli.

¹⁰ Powell, "Introduction," xli; Stephen Williams, Fantastic Archaeology: The Wild Side of North American Prehistory (Philadelphia: University of Pennsylvania Press, 1991), 64-5.

was not an experienced archaeologist even by the standards of the time, but he proved to be a skillful manager for the Mound Exploring Division, whose task was to conduct as complete a survey as possible of the ancient works of the Mississippi and Ohio valleys within a limited time frame. Powell did not initially specify a time frame for the project but it was clear that he thought the Mound Division would complete its mission by the end of the decade. Thomas's role as a strong central coordinator for the Division's complex field maneuverings was critical for its operations. His survey was more or less fully underway by 1882. Fieldwork trickled to an end by close of the decade by which time he had begun assembling what became a 700-plus page report, eventually appearing as the Twelfth Annual Report of the Bureau of Ethnology in 1893.

In sum, then, this was the situation in the earliest years of the 1880s. Authorities from the two leading American institutions for archaeological research were taking a proactive interest in the mounds and they were in a position to commit institutional resources to the task. The heads of these organizations came from significantly different backgrounds: Putnam was a trained museum naturalist and curator; Powell was steeped in the western survey tradition. Putnam's field investigations tended to be intensive, long-term studies of particular localities, whereas the Bureau's mound survey was designed to cover as geographically broad an area as possible. Nonetheless, both men were interested in the same big questions of American ethnology, and both were leading a newer generation of ethnologists who regarded—and promoted—their practice overall in increasingly scientific terms.

What made ethnology scientific for Peabody and Smithsonian officials? That depended. The most straightforward answer has to do with control and centralization.

This was definitely the case for Ohio valley archaeology, but it applied to other realms of ethnology as well. Whatever else could be said about its progress during the years and decades leading up to the early 1880s, without question Ohio valley archaeology remained a vastly undisciplined set of activities that happened at multiple levels. Some sort of across-the-board standardization was in order. Putnam on the one hand and Powell and Thomas on the other explicitly wanted to demarcate their institutionally-situated practices from what they saw as the ongoing proliferation of unorganized—and therefore non-scientific—approaches to the problem of the mounds. Centralization thus meant many things: it was the physical process of gathering archaeological data of every form to the institutional centers; it was the codification of methods for acquiring that data; it was the standardization of ways of thinking about what that data meant and how it should be interpreted. It also involved the often uneasy solidification of social categories for archaeological practice, in the sense that the institutional centers determined who could participate and in what capacity. In other words, centralization was discipline.

We will be dealing in great detail with many aspects of centralization as a social phenomenon in the following chapter. For now, it is important to identify a bit more clearly the particular theoretical commitments underlying Peabody and Smithsonian approaches to the mounds, for there were significant differences between the two. In fact, commentators during the decade identified the emergence of two “schools” of American anthropology on the basis of this distinction alone.

Putnam, for his part, believed that some—but not all—moundbuilding peoples were ancestral to particular modern Indian groups.¹¹ He thought of prehistoric North America

¹¹ Putnam expressed these views in a number of places, but perhaps earliest and most clearly in, Frederic

as a place where highly complex patterns of migrations throughout the continent occurred over an exceptionally long period of time, going at least as far back as the late Pleistocene, eight to ten thousand years ago.¹² Direct lineal ancestors of modern Indian tribes indeed lived there in the far distant past, but so too did other groups, some of whom came into present-day continental United States from the north and others from the south and subsequently moved on. Over the course of the prehistoric past, Putnam thought, some groups died out and various permutations of mixing and merging occurred between and among those who survived.¹³ All in all, pre-Columbian North America was characterized by immense diversity. As Putnam wrote:

To this heterogeneous people the name Indian was given, in misconception, nearly four hundred years ago, and now stands as a stumbling block in the way of anthropological research; for under the name resemblances are looked for and found, while differences of as great importance in the investigation are counted as mere variations from the type.¹⁴

Ward Putnam, "Archaeological Explorations in Ohio and Wisconsin," Seventeenth Annual Report of the Peabody Museum (1884), reprinted in The Archaeological Reports of Frederic Ward Putnam (New York: AMS Press, 1973), 209-21, esp. p. 216.

¹² F.W. Putnam, "The Peabody Museum of American Archaeology and Ethnology in Cambridge," Proceedings of the American Antiquarian Society for 1889, reprinted in The Archaeological Papers of Frederic Ward Putnam (New York: AMS Press, 1973), 159-69, esp. p. 166. Daniel Wilson, in an 1877 address to the newly-formed subsection of anthropology of the American Association for the Advancement of Science, sang the praises of the Peabody Museum particularly for its support of research into the antiquity of man in America. See, Daniel Wilson, "Address," Proceedings of the American Association for the Advancement of Science for 1877 (1878), 319-34, esp. pp. 332-3.

¹³ In the following chapters, I will explore how Putnam's theoretical views about prehistory were in many respects directly connected to his own personal field experience.

¹⁴ Putnam, "The Peabody Museum," 169.

As we shall see a bit in the next chapter and especially in Chapter 6, Putnam's views along these lines developed in close conjunction with his field investigations in the Ohio valley beginning early in the 1880s, when he collaborated with members of the Madisonville Literary and Historical Society on excavations along the Little Miami River. This work in particular seems to have been instrumental in leading him to read archaeological sites as complex multi-component entities which corresponded to a complicated record of human activity in the past. For Putnam, the mounds occupied a sort of archaeological middle ground—temporally speaking—between earlier stone age occupations and later Indian groups, though there was no necessary or easily identifiable chain of association linking all together.

As a method of analysis for understanding the complicated permutations of human activity and the spread of cultural traits, Putnam was amenable to cross-cultural comparative studies, often between North American and non-North American contexts. For example, in the published study of his Serpent Mound work, Putnam explored parallels of religious conventionalism between the Ohio serpent earthwork effigy and various manifestations of serpent symbolism in other parts of the world, including Asia.¹⁵ In so doing, Putnam was not merely pointing out interesting coincidences, such as that many people throughout the world over appeared to have some fascination with serpent imagery. He was raising deeper questions about the occurrence of certain cultural traits or conventions within human populations. Perhaps shared traits indicated their diffusion either through existing populations or via the migration of populations into new

¹⁵ Frederic Ward Putnam, "The Serpent Mound of Ohio," *Century Magazine* 39 (1889-90), 871-88.

geographic areas. Or maybe the occurrence of similar traits indicated instances of independent development. Only a continually refined program of comparative analysis could bring definitive answers to these kinds of inquiries and in due course shed light on broader questions pertaining to the ultimate origins of America's prehistoric inhabitants. In fact, it was Putnam's conviction that the essential purpose of the Peabody Museum was to facilitate a holistic "comparative study of man and his works" through its collections.¹⁶ Reasoning by analogy from one archaeological or ethnological context to another—even between New and Old World settings—was seen as a potent methodological tool, a fertile means of generating hypotheses about patterns of human development. In this regard, Putnam was keeping right in line with a well established tradition of American ethnological science.

Bureau archaeologists took a remarkably different position, which in all fairness emanated directly from Powell but was taken up in earnest by Thomas. Powell set a definite agenda for the Mound Exploring Division—and this was to establish conclusively that moundbuilders and Indians were not only *somehow* related, but were in fact one and the same people. Thomas was placed in charge of the mound survey specifically in order to gather massive amounts of evidence that could be used to support this view.¹⁷ The reasons for taking this clear-cut stance at least partly involved Powell's

¹⁶ Putnam, "The Peabody Museum," 161-2.

¹⁷ See, John Wesley Powell, "Introduction," Twelfth Annual Report of the Bureau of Ethnology for the Years 1890-1 (1894), xl-xli. Also, David J. Meltzer, "The Antiquity of Man and the Development of American Archaeology," in Michael B. Schiffer, ed., Advances in Archaeological Method and Theory, Volume 6 (New York: Academic Press, 1983), 13. The best recent account of the Bureau's non-

commitment to the evolutionist framework of Lewis Henry Morgan's work, which in some sense demanded a record of smooth, continuous human progress from ancient to modern times.¹⁸ As Hinsley documents throughout his account of the Bureau of Ethnology, Powell's major concern was not archaeology but the documentation of the languages and customs of western Indians. Hinsley repeatedly demonstrates that Powell felt a moral imperative to this work that had been intensified at a time when Native American populations in the west were diminishing under the weight of ill-conceived federal policies and military force (implemented by the same government that funded Powell's Geological Survey and the Bureau of Ethnology). The Bureau stance was also a response to the lingering polygenist theories of a generation earlier, in which Smithsonian archaeological studies served as important sources of evidence. By 1880, some significant errors in Squier and Davis's Smithsonian-published Ancient Monuments of the Mississippi Valley had become apparent.¹⁹ Since this study was still being used as a sort of bible of mound archaeology, and was frequently cited in support of moundbuilder separate race theories, there was a definite awareness that something needed to be done to repair the damage.

archaeological western work is, Curtis Hinsley, The Smithsonian and the American Indian: Making a Moral Anthropology in Victorian America (Washington, DC: Smithsonian Institution Press, 1994). For an early published articulation of the Bureau's position—and a strong indication of what the Mound Exploring Division was formed to accomplish—see Cyrus Thomas, "Who Were the Mound-Builders," American Antiquarian 6 (1884), 90-9.

¹⁸ Meltzer, "Antiquity of Man," 13.

¹⁹ Cyrus Thomas, The Circular, Square, and Octagonal Earthworks of Ohio (Washington, DC: Government Printing Office, 1889), 7.

Thomas single-mindedly directed this work with an eye toward producing a series of reports intended to dismiss any form of separate race theorizing by conflating the historical and cultural divide between moundbuilders and Indians. According to the Bureau position, the mounds were not much older than a few hundred years, and the moundbuilders' overall accomplishments were not nearly as sophisticated as generally supposed. Bureau archaeologists did not accept the possibility of such complex wanderings and mingling in prehistoric times that Putnam allowed—that is, they thought prehistoric peoples (including moundbuilders) were more or less stationary groups with relatively straightforward lineal connections to more recent Indian tribes. They also patently rejected the possibility of extreme human antiquity in North America.

So, unlike Putnam who adopted what might be called a more open-minded approach to the prehistoric past, Bureau archaeologists were much more conservative and considerably more restrictive in making claims about (and on the basis of) archaeological evidence. Thomas's final report of the Mound Exploring Division is a good example of the kind of sober empiricism that tempered Bureau archaeology. Starting with descriptions of known Indian customs and conventions, he worked back to the evidence from the mounds to find similarities: these were the same people. Thomas and other Bureau archaeologists took as a starting point what had been witnessed in operation among living natives and worked back in time. They were especially interested in eliminating what they saw as unwarranted speculations about archaeological data. The notion of moundbuilders as Lost Tribes of Israel certainly fit this bill, but so too did the use of any form of analogical reasoning between different cultural contexts such as North America and Asia, the kind of thinking Putnam regarded as productive. Putnam did not

much like the Bureau approach and thought it unnecessarily self-limiting. What if the human complexion of the prehistoric past was very different from what any Europeans had observed more recently? These are complex issues of method, and I will explore them much more precisely in Chapter 6 where I look at various approaches to the Serpent Mound site in particular.

The Bureau stance against Pleistocene man was tenable because in all fairness evidence was not terribly conclusive. In fact, two of Putnam's regular field correspondents—Charles Abbott of New Jersey and Charles Metz of Ohio—had reported on the recovery of worked paleolithic flints from glacial or pre-glacial deposits during the 1880s. Putnam fully supported these claims at the time. Abbott's finds proved especially controversial, and ultimately drew the attention of Bureau archaeologists such as William H. Holmes who regularly set out to discredit such claims of extreme antiquity in the 1880s and early 1890s.²⁰ Abbott was ultimately marginalized from archaeology, but this as well as the controversies over his finds seem to have been a function of his increasingly unstable personal relations with others (Putnam included). Metz did not face the same level of negative reactions in reporting on paleolithic stone tools found in southern Ohio.²¹ (Much more will be said about Metz in the two chapters to follow.) In either case, however, it had been possible for opponents of Pleistocene man to call into question both the context of individual finds as well as the tool-like status of what in

²⁰ See, Hinsley, "The Museum Origins of Harvard Anthropology;" and, Hinsley, "From Shell-heaps to Stelae: Early Anthropology at the Peabody Museum," in George W. Stocking, Jr., ed., Objects and Others: Essays on Museums and Material Culture (Madison, WI: University of Wisconsin Press, 1985), 49-74.

²¹ Charles L. Metz, "Interglacial Man in Ohio," Popular Science Monthly 34 (1888-9), 278.

reality were very crude—and potentially naturally produced—pieces of stone. Claims for the existence of glacial man were easily dismissed, and whether one believed in extreme antiquity or not really depended on other factors, such as one's methodological views or the extent to which one was actually involved in field excavations in the appropriate regions of the country.

As the Bureau official in charge of the mound project, Thomas was really only interested in the immediate problem of linking modern Indians and moundbuilders. Approaches such as Putnam's in his eyes unnecessarily complicated and problematized the prehistoric past, making the task of associating moundbuilders and extant native groups more difficult. One specific problem faced by Bureau archaeologists as their survey got underway needs to be mentioned. It had to do with facing up in some sense to the legacy of Squier and Davis. Over thirty years after the original Smithsonian-sponsored publication of Ancient Monuments of the Mississippi Valley, the monograph was still widely regarded as the most authoritative starting point for investigations of these ancient works, and particularly of those within the Ohio valley region. We already know that Ancient Monuments left the door wide open for separate race thinking about moundbuilders. One thing specifically that Squier and Davis emphasized was the geometry of earthworks. Their plan view maps generally depicted earthwork complexes which incorporated perfectly regular circular or polygonal earthen embankments. As some of Thomas's field workers re-examined many of these sites in the 1880s, they found that Squier and Davis's survey data was inaccurate and even appeared at times to have been fudged. Obviously, in keeping with the Bureau aims outlined above, these issues needed to be cleaned up—the moundbuilders (who were the same people as the Indians)

could not have possessed some more advanced knowledge of geometry. Thomas took the matter seriously enough to issue a special interim report on The Circular, Square and Octagonal Earthworks of Ohio in 1889 based on survey work that some of his correspondents had done over the previous couple years.²² As he concluded, “No valid reason can be presented why Indians, taught by necessity and practice, could not lay off by the eye and by means at hand figures with which they were familiar more correctly than the white man without instruments.”²³

At precisely the same time, Thomas issued a second report on The Problem of the Ohio Mound in which he made direct associations between the moundbuilders and known Indian tribes, most especially the Cherokees.²⁴ Thomas drew on historical evidence about the known recent migrations of tribes, and he made direct associations between the “arts and customs” of the moundbuilders and modern Indian tribes. The subject of burial practices took center stage, but he also pointed out similarities between archaeological materials recovered from mounds and materials in known use by Indians. Tying the evidence all together, Thomas made a case for the moundbuilders being ancestral at least to the Cherokees.

Given the benefit of hindsight, we can say that Peabody and Bureau archaeologists in the 1880s were indeed interested in giving credit in some fashion to ancestral native Americans for building the mounds, and that whatever other biases factored into their interpretations at the least this recognition was a step in the right direction. Even some

²² Thomas, The Circular, Square, and Octagonal Earthworks of Ohio.

²³ Thomas, The Circular, Square, and Octagonal Earthworks of Ohio, 33.

²⁴ Cyrus Thomas, The Problem of the Ohio Mounds (Washington, DC: Government Printing Office, 1889).

contemporaries recognized that this was the case. In 1884, for example, Otis Mason, a Smithsonian National Museum resident collaborator in ethnology, referred to recent papers by both Thomas and Putnam:

[O]ne could hardly fail to see that a great change of opinion has taken place among our American archaeologists concerning the Mound-Builders. Indeed, most investigators would advise us to drop the capital letters.... The Mound-Builders probably were not so numerous, so highly organized socially, nor so cultivated as many have held. Neither were their successors within the area of the United States so degraded as they have been made out. The chasm, therefore, so far as culture is concerned, between the two peoples, the Mound-Builders and the modern Indians, has been bridged over by Thomas, Putnam and others.²⁵

Not the “Mound-Builder” race or civilization, but merely “moundbuilders,” people who built mounds. Mason was commenting on the dissolution of a typological category that had long been ingrained into American archaeological thought.

Within a year, Mason was made curator of ethnology at the National Museum, one of the few permanent staff positions there, and he certainly was qualified to make such assessments. But notice how he put Thomas and Putnam on equal footing as though they had set out to make the same sorts of claims in order to “bridge over” the culture chasm between the prehistoric moundbuilders and more recent Indians.²⁶ What Mason did not

²⁵ Otis Mason, “The Mound-builders,” *American Naturalist* 18 (1884), 745-6. Mason had been closely affiliated with the Smithsonian National Museum as a resident collaborator for some time and was on his way to becoming its first official curator of ethnology. Mason had no formal connection with the Bureau of Ethnology.

²⁶ I would like to thank historian of archaeology David Browman for making me aware of the need to emphasize Thomas’s authoritative role in the work archaeological of the Bureau of Ethnology (personal communication, 2003). Thomas did not possess the nearly the same level of field experience nor breadth of scholarship as Powell or Putnam. As a result, Thomas has not been well-remembered to history. And yet, as

state here was that only in an extremely general sense could it have been said that these men were thinking along the same lines—even by 1884 it was abundantly clear that Thomas and Putnam had taken very different approaches to the problem of the mounds. Indeed, if one chasm had seemingly been bridged, another yet remained, and this had to do with questions of method for interpreting the data of prehistory, as I have pointed out above. For Putnam and Thomas, what was allowable as legitimate interpretation depended on markedly different visions of how the moundbuilders—whoever they were—fit within a broader framework of prehistory.

Actually, the gap became considerably wider than that, at least according to some contemporaries who saw in Peabody and Smithsonian museum displays representations of distinct archaeological philosophies. A good indicator came by way of an article written by respected British naturalist Alfred Russel Wallace in 1887 for the popular British periodical The Fortnightly Review.²⁷ Wallace, who had long-standing interest in the operation of public museums, had toured the museum, as well as Agassiz's Museum of Comparative Zoology, in 1886.²⁸ Here, Wallace distinguished between two “schools”

head of the Mound Exploring Division during the 1880s, he was in a position of archaeological authority second to none in the United States, and perhaps even the world. Even if he did not perform many field excavations himself, Thomas was a skillful administrator and coordinator for a complex project, and he was a successful spokesman for the Bureau of Ethnology.

²⁷ Alfred Russel Wallace, “American Museums: Museums of American Pre-historic Archaeology,” The Fortnightly Review 42 (1887, New Series), 665-675.

²⁸ Mary P. Winsor, “Louis Agassiz's notions of a museum: the vision and the myth,” in Michael T. Ghiselin and Alan E. Leviton, ed., Cultures and Institutions of Natural History (Memoirs of the California Academy of Sciences, no. 25) (San Francisco: California Academy of Sciences, 2000), 249-271; Mary P. Winsor,

of American anthropology (his term) that had formed over the moundbuilder problem.²⁹ The first school, he reported, focused on resemblances between extant Indians and the moundbuilders. Adherents of this position maintained a relatively uncomplicated view of human activity in the prehistoric past—that is, they thought all prehistoric peoples including moundbuilders did not move around much and there was a more or less unproblematic continuity between these stationary prehistoric groups and more recent or historic Indian groups. The second school emphasized the “differences between the remains left by the mound-builders and other prehistoric races and the works of modern Indians.”³⁰ Proponents of this view upheld a far more complicated picture of prehistoric America—it was a place where a highly heterogeneous mix of populations mixed, mingled, and displaced one another over an exceptionally long period of time. Although he did not make the link explicit, it was clear from the context of his discussion (and it should be clear from the context of my discussion above) that these schools indeed corresponded to the Bureau of Ethnology and Peabody approaches to the mounds.

The bulk of Wallace’s account, however, had to do more specifically with the museum aspects of Peabody and Smithsonian archaeology. In the first place, he identified the Peabody and Smithsonian National Museums as the two most sophisticated centers for the study of North American prehistory, and he cautioned against the perception still commonly held by many European anthropologists of North America as a pristine New World without much historical depth. On the contrary, Wallace urged, European scholars

“Louis Agassiz’s Ideas of Museum Arrangement,” unpublished ms., 4-5.

²⁹ Wallace, 675.

³⁰ Wallace, 675.

should take heed: thanks to the massive collections now housed at the Peabody and Smithsonian, it now appeared that the character of North American prehistory at least equaled that of Europe in terms of both its complex record of human activity and its great antiquity. Wallace then provided a general synopsis of archaeological materials found in both museums as support. He was particularly fond of the great variety of stone implements—a far greater variety, he thought, than what was found in Europe. He also devoted considerable attention to objects associated with the greater Mississippi valley mounds.

Wallace further distinguished between Peabody and Smithsonian methods of arrangement for archaeological collections. At the Peabody, Wallace reported, archaeological materials were arranged according to quite specific geographic localities. For instance, all objects found in a mound or group of mounds were kept together to demonstrate the “life history of the constructors.”³¹ Individual objects were presented as parts of a greater mosaic of objects to which they presumably belonged. The Smithsonian collections, on the other hand, were arranged by type “to illustrate the development of prehistoric industry and arts.”³² A visitor wandering through one long gallery in particular would be able to observe a succession of display cases filled with stone implements arranged into groups from crude forms through those deemed increasingly more elaborate or refined.³³ Wallace thought each method had its merits; each, he thought, provided a particular kind of window onto the prehistoric past, a way of visually making sense of the

³¹ Wallace, 666.

³² Wallace, 667.

³³ Wallace, 667.

data at hand.

We do get a sense from these remarks of the extent to which the actual arrangement and display of objects played a role within broader nineteenth century discourses about the function of ethnological museums. Historian Steve Conn has helpfully written about the “object-based epistemology” characteristic of this late nineteenth century era.³⁴ Conn rightly, I think, describes how museum arrangements served for both their audiences and their creators a heuristic function that is significantly different from what is experienced by museum-goers and museum administrators today. That is, displayed objects meant considerably more to people in the late nineteenth century than they do to people nowadays, at least according to Conn. At first glance, it may seem incongruous that a naturalist like Wallace placed such emphasis on how the Peabody and Smithsonian put archaeological collections on public display rather than on what sorts of behind-the-scenes provisions (or not) had been made for researchers. But this is a more modern way of thinking about museums—the transience of regularly changing exhibitions for public consumption on the one hand; on the other, the maintenance of research collections which are accessible only to those who are qualified to examine them. In the last quarter of the nineteenth century, the explicit distinction between public education and scientific research aspects of museum collections care was not so clearly defined.

For Wallace, arrangement meant everything for understanding the Peabody and Smithsonian as centers for scientific archaeology. “As in the case of zoological collections,” he wrote,

³⁴ Steve Conn, Museums and American Intellectual Life, 1876-1926 (Chicago: University of Chicago Press, 1998), chapter 1.

a great national museum [in general] should combine both methods of arrangement; and it is therefore fortunate that in the present progressive condition of the study the two great museums of American prehistoric archaeology should have adopted different systems.³⁵

What he meant here as the basis of this analogy was that zoological specimens could be studied in either some sort of holistic fashion—as complete, functional organisms or as functional components of some larger ecological context—or according to a more reductionist method of abstracting certain features (such as digestive systems) from different kinds of organisms into arrangements that facilitated various forms of comparative studies. Either way, the point was that the physical arrangement of specimens mattered to Wallace for more than aesthetic reasons. Arrangement had methodological consequences. It was the means through which objects constituted knowledge. Different systems of arrangement were thus indicative of particular philosophical outlooks on the natural world, of which human beings and the archaeological detritus they left behind were part. As Conn writes, “This faith in objects as the source of knowledge lay at the center of how Americans of the late Victorian period understood the world, and it lay at the heart of the whole museum enterprise.”³⁶

This is a strong statement. I am not sure it applies equally to all late nineteenth century Americans, but it was certainly true about museums. Exhibits could indeed transform institutions in ways nearly unimaginable today. Take, for instance, the Smithsonian’s contributions to the 1876 Philadelphia Centennial Exposition, which

³⁵ Wallace, 667.

³⁶ Conn, 9. An excellent bibliographic essay on the history of museums that focuses on the materiality of objects is, Samuel J.M.M. Alberti, “Objects and the Museum,” *Isis* 96 (2005): 559-571.

included massive archaeological and anthropological exhibits. This was a catalyzing event for the future of Smithsonian anthropology. It strengthened the Institution's ties to the federal government and provided the direct stimulus for government money to construct a dedicated National Museum building in the first place. Baird and his colleagues departed Washington with twenty one rail cars worth of objects and returned with forty two. The increase was due strictly to donations made by other organizations and individuals during the six month long event.³⁷ The closer relationship between the Smithsonian and the Interior Department that resulted from the Centennial exhibits also arguably gave Powell the leverage he needed to propose the creation of the Bureau of Ethnology. Without the Bureau, there would have been no Mound Exploring Division.

Whereas the Centennial gave Smithsonian archaeology a big push, the 1893 World's

³⁷ The long and convoluted story of the emergence of a functioning museum within the Smithsonian Institution is best discovered through two sources: Marc Rothenberg, ed., The Papers of Joseph Henry, Volume 7: January 1847-December 1849, The Smithsonian Years (Washington, D.C.: Smithsonian Institution Press, 1996); and, the various administratively-oriented reports opening the Smithsonian Annual Reports, especially for the years 1875 through 1880. It should be said that archaeology and anthropology formed only a component of overall Smithsonian exhibition contributions to the federal government space of the Exposition (contributions which also included geology, mineralogy and zoology), and that the Smithsonian anthropology efforts here were made in conjunction with the Interior Department, Indian Bureau, and War Department. When the six month long Centennial came to an end in November of 1876, the Smithsonian found itself in possession of considerably more total collections materials than it started with, as noted above. The increase was due largely to donations from other organizations or from individual collectors. There was simply nowhere to put all this stuff, a situation which gave Smithsonian secretary Joseph Henry and assistant secretary Spencer Baird long-awaited leverage to convince Congress to start forking over money for the construction of a dedicated museum edifice and its subsequent upkeep.

Columbian Exposition in Chicago pulled on Peabody archaeology for the few years leading up to it. Putnam was responsible for organizing the biggest and perhaps most ambitious anthropological exhibit to date in the United States for this event. The WCE importantly resulted in the permanent establishment of the Field Columbian Museum, but it also provided a focal point for much archaeological fieldwork conducted under Putnam's aegis. Some of Putnam's field correspondents during the late 1880s and early 1890s seem to have been working for the Peabody, though in the name of World's Columbian Expo preparations. Specifically in terms of Ohio valley work, the WCE marked an end to Putnam's period of intense interest in the area.

Wallace thought the coexistence of Peabody and Smithsonian styles of arrangement to be a healthy state of affairs for American archaeology. Not everyone saw it that way. Franz Boas, whose entrance into American ethnology had already been facilitated by the Bureau of Ethnology and who would shortly after be helped by Putnam, was also beginning to make some waves in 1887 by outspokenly criticizing the Smithsonian methods of display. His critique, first directed toward National Museum curator of ethnology Otis Mason, ultimately drew the response of others at the Smithsonian including Bureau of Ethnology director Powell. Much of the ensuing exchange was aired in the pages of Science.³⁸ This episode has been documented by a number of historians, some of whom have viewed it as a sort of launching pad for the transformations in

³⁸ See, for instance, the exchange of letters between William H. Dall, Franz Boas, and John Wesley Powell collected together and reprinted in Science. "Museums of Ethnology and Their Classification," Science, Volume 9, Issues 228 (June 17, 1887), 587-9; "Museums of Ethnology and Their Classification," Science, Volume 9, Issues 229 (June 24, 1887), 612-4.

American anthropology that would occur a bit later on, most notably the turn toward cultural relativism and the rise of university-based anthropological practice.³⁹

The main problem Boas had with Mason's ethnological classification was what he saw as its misleading emphasis on technology. According to Boas, the Smithsonian's evolutionary arrangements of artifact types were arbitrary and could not explain in a good

³⁹ "Cultural relativism" and "historical particularism" are some labels that have been associated with Boas's anthropology. Historians have generally viewed the formulation of these concepts by Boas and his students as a refutation of the "evolutionist" anthropologies of the mid to late nineteenth century. According to Cole's detailed and authoritative biography of Boas, however, Boas did not consider himself an anti-evolutionist. Cole's account situates the development of Boas's thought squarely in a methodological framework—that is, the problem Boas had with evolutionist anthropology was what he saw as its overly speculative attempt to reconstruct human developmental pathways. According to Cole, it was not for Boas inductive science. Boas had come to the United States after having conducted extensive fieldwork in the Canadian Arctic. He worked for Putnam for the World's Columbian Exposition in the early 1890s and then made his way to the American Museum of Natural History in New York and Columbia University. From there, he trained a large number of the subsequent generation of American anthropologists. See Conn, chapter 3; George W. Stocking, "Paradigmatic Traditions in the History of Anthropology," in R.C. Olby, G.N. Cantor, J.R.R. Christie, and M.J.S. Hodge, Companion to the History of Modern Science (London: Routledge, 1990); George W. Stocking, Jr., Race, Culture, and Evolution: Essays in the History of Anthropology (New York: The Free Press, 1968), chapter 7; Joan Mark, Four Anthropologists: An American Science in its Early Years (New York: Science History Publications, 1980), chapter 2, esp. pp 32-5; Ira Jacknis, "Franz Boas and Exhibits: On the Limitations of the Museum Method of Anthropology," in George Stocking, ed., Objects and Others: Essays on Museums and Material Culture (Madison, WI: University of Wisconsin Press, 1985); Hinsley, The Smithsonian and the American Indian, chapter 4 ; Douglas Cole, Franz Boas: The Early Years, 1858-1906 (Seattle, WA: University of Washington Press, 1999), esp. chapters 5-7.

inductive manner why similar technological traits appeared in different cultural contexts. People made rattles, for instance, for all sorts of reasons—from a religious need to drive off dark spirits to the more practical desire of giving children something to play with. Rattles were not products of some universal, progressive development of noise-making techniques—exactly what Boas thought the Smithsonian arrangements illustrated. In good Germanic fashion, Boas instead advocated for a complete “psychological museum” in which individual cultural traits were seen as products of the “physical and psychical character” of a particular group of people. Every group’s character was informed by its environment as well as by its own history, and not by some universal process of development.⁴⁰ For Boas, the ideal anthropological museum contained exhibits illustrating the complete physical and psychical character of all peoples. Within this scheme, Native American objects including archaeological materials should be grouped and displayed by tribal affiliation. Individual archaeological artifacts would thus be placed with all other objects determined to have been used by a particular people at a particular place and time.

Powell’s rejoinder to Boas went right to the heart of the museum issue.⁴¹ For Powell, the primary function of an ethnological museum was as a repository for researchers, plain and simple. Exhibition—either for instruction or public amusement—was a secondary function that interfered with the main purpose, since only a fraction of the total collection

⁴⁰ Boas was trained in physics before moving into ethnology, and this background directly influenced his highly materialistic form of environmental determinism.

⁴¹ J.W. Powell and Franz Boas, “Museums of Ethnology and Their Classification,” *Science*, Volume 9, Issues 229 (June 24, 1887), 612-4.

could ever be displayed at any one time. Public arrangements were best understood as provisional exemplifications of a particular viewpoint only. Powell furthermore thought Boas's tribal arrangements were just as idealized as were Mason's technological arrangements.⁴² How, for instance, should a tribal unit even be defined from the archaeological record, without corroborating linguistic and ethnographic evidence? Delineating a tribe at any given point in time involved a fair bit of speculation. At the very least, Powell thought, technology offered a concrete organizing principle.

In a subsequent response, Boas agreed that museum classifications by necessity had to be based on available evidence, but he nonetheless continued to advocate a comprehensive system of arrangement that would have been all but impossible to implement in any but the biggest and wealthiest of museums, if even then. A further interesting and important facet of Powell's side of the exchange here lay in his appeal to the mundane details of museum administration. In his mind, how things were displayed did not matter so much as long as the museum provided appropriate care and storage for its collections with good quality cases and other devices. Just as important, Powell thought, was the rigorous maintenance of records and descriptive catalogues, since proper documentation effectively tied different objects together and provided context. Boas's method was nice in theory, Powell intoned, but it had little basis in the practical necessities of day to day museum life. For researchers, it mattered little whether objects associated with a tribal unit were physically placed together or not.

Wallace's comments about Peabody and Smithsonian methods of arrangement and

⁴² Both Mark and Cole document that Boas's ideas about arrangement drew upon a German tradition associated with Adolf Bastian. See, Mark, *Four Anthropologists*, pp. 31-6, and Cole, *Franz Boas*, esp. ch. 7.

Boas's critique of Smithsonian museum practices seem to support Conn's thesis about the significance of objects as a source of knowledge in the late nineteenth century context. At the heart of these various museum discussions and debates was, at least at some level, the idea that arrangements themselves were indicative of particular kinds of knowledge about the past. But we should remember that when Conn talks about Victorian America's "faith in objects," it is in a fairly vague and general sense. Wallace and Boas provide us with evidence not only that there existed quite different methods of arrangement for constituting knowledge, but also that the merits of these methods were very much under debate. Subtle, layered distinctions characterized these attitudes. Wallace identified both a Peabody and a Smithsonian style of museum arrangement and thought the existence of both to be a good thing; Boas explicitly denounced the Smithsonian method and advocated one that was in its essence much more compatible with Putnam's ideas.⁴³ A third sort of option was represented by Powell's attitude that arrangements, however construed, were merely idealizations and that good museum administration and record-keeping mattered most.

Well, one thing certain about all of this was that no discussions or debates about museum arrangement could have occurred without objects to arrange. Building collections was a major part of the centralizing mission undertaken by Peabody and Smithsonian officials in general, and it was indeed an essential aspect of the mound

⁴³ As historian Ira Jacknis reports, by the turn of the century Boas had achieved a position of prominence at the American Museum of Natural History in New York, where his ideals of museum organization came head to head with logistical practicalities. Boas came to realize that in practice, his desired arrangement was difficult if not impossible to achieve. See, Jacknis, "Franz Boas and Exhibits."

projects in the 1880s. Make no mistake: as both Putnam and Thomas orchestrated fieldwork throughout the Ohio valley, they were keenly interested in accumulating specimens, as many as possible, especially in hopes of building up “complete” collections. In general, the desire was for exhibitions to be made up of duplicate specimens, with the “real stuff” being maintained behind the scenes for research.

An interesting dynamic emerged as a result. At one level, we find a significant amount of sharing and cooperation between the Peabody and Smithsonian, with temporary or even sometimes permanent exchanges of duplicate specimens occurring regularly. One has only to browse through the administrative and clerical reports pertaining to the National Museum that open any of the Smithsonian Annual Reports to get a sense of the extent to which the Smithsonian facilitated museum exchanges on an unprecedented scale. Mason and Putnam regularly corresponded on such matters. In one letter to Putnam, Mason wrote:

I have ordered a box to be made to transport your first invoice [of the year?] to Cambridge. I shall take especial pains to see that it is filled with such things as will be most useful to your archaeological studies. I think we agreed in the hasty conversation we had before you left that it would be better for us to send you lots tolerably exhaustive of certain types than to send a large heterogeneous mass without regard to connection. It takes longer to get a hundred feet of lumber to make a box than it does to make the collection fill it.⁴⁴

And yet, at another level such frank openness was not expressed—the two institutions were in sometimes fierce competition to get at the same sites and materials. This was especially true of the Ohio valley work. It is also true that some museum collections were kept more guarded than others. In one instance, Putnam would not allow a visiting

⁴⁴ Otis Mason to F.W. Putnam, 6 May 1887, Peabody Museum, Director Records, Frederic Ward Putnam, Box 8, Folder UAV 677.38.

Bureau correspondent full access to his southern Ohio collections for fear the information might subsequently be used to support the Bureau theory about the moundbuilders. We will get a much clearer sense of these kinds of issues in the next chapter.

Many specimens were recovered from directed excavations, but many too were gleaned through the purchase of private collections and through donations, the rates of which dramatically increased as these institutions made their presence increasingly felt throughout the Ohio valley and elsewhere and people took notice. Putnam had the benefit of being museum curator as well as field director, so he kept close tabs on the state of affairs at the museum and could use his own judgment in purchasing items or accepting donations. Thomas frequently had to defer to National Museum staff or even to Spencer Baird in such cases. Complicating the issue was the fact that correspondent fieldworkers often took it upon themselves to use their own judgment in purchasing items in the name of the respective institutions for which they worked. Collections building was so prominent a part of Peabody and Smithsonian archaeology in the 1880s that one knowledgeable commentator accused the Bureau of Ethnology of out and out relic hunting in the early phases of their survey; another individual, one of Putnam's own correspondents, worried about the detrimental effect of the "relic boom" excited by all this intense institutional interest in artifacts.⁴⁵

Nonetheless, both Putnam and Thomas insisted on certain standards when it came to the acquisition and documentation of objects. Peabody and Smithsonian circulars were

⁴⁵ Stephen D. Peet, "Relic Hunting Versus Archaeological Survey," *The American Antiquarian* 6 (1883-4), 204-8; Harlan T. Cresson to Putnam, 24 August 1891, Peabody Museum Director Records, Frederic Ward Putnam (PM/DR/FWP) 89-15, Box 11 "1891 A-F," Folder "UAV 677.38."

distributed which outlined methods of excavation or detailed what sorts of information to include in documenting the contexts of finds. Such circulars had routinely appeared in the Smithsonian Annual Reports for quite some time prior to the 1880s, but during this decade efforts increased in distributing such information. Putnam and Thomas each expected their correspondent fieldworkers to adhere to certain methods. Correspondents regularly submitted letter reports, and they were expected to perform excavations according to a sort of protocol and to document this activity on paper. Specimens were not to be sent willy-nilly back to the institutional centers; both Thomas and Putnam directed fieldworkers to carefully pack items and to take care that associated documentation clearly provided contextual information for those items. In the case of the Peabody work, I can say for certain that specimens were generally labeled in the field with numbers corresponding to logbooks which then contained considerable additional information associated with that specimen. Similar protocol was often followed in the Bureau mound survey.

It should be said, however, that by no means did Putnam and Thomas employ the same sorts of standards, nor that those in the field necessarily followed them to the letter. As I pointed out much earlier, Putnam preferred to spend long amounts of time investigating individual sites and Thomas wished to gather data from as geographically broad a range of sites as possible in a limited amount of time. These fundamental differences in field objective—excavation versus survey—necessitated different standards in terms of what kinds of information each man expected to emerge from investigations and to accompany specimens. And, as we shall see clearly in the following chapter, mid-western archaeological enthusiasts who wished to participate in Peabody or Smithsonian

archaeology at times strongly disagreed with methods insisted upon by these eastern institutional authorities. Frequently, the individual motives of fieldworkers overrode all other concerns.

The level of control that both Putnam and Thomas imposed onto archeological fieldwork from an institutional center represented something new in the overall scheme of nineteenth century American archaeology. Their overt insistence on precise documentation shared a common sensibility with those of Powell, mentioned earlier, about the primary importance of information accompanying specimens and collections. Indeed, there is a strong sense in which these archaeological authorities of the 1880s had begun thinking about quality supporting documentation as being at least as important as the objects themselves. Putnam voiced such sentiments in a piece about the Peabody Museum's mission published later in the decade, just after a second architectural expansion.⁴⁶ Putnam outlined his method of arrangement by "geographical sequence," and optimistically described that the added space would make a comprehensive arrangement along these lines possible. One point in the account that explicitly stood out was his insistence that the principal Peabody collections had been made by

trained explorers in the field, who have done their work in a thorough manner and have brought together masses of material of inestimable value for study, as each object is authenticated and the exact conditions under which it was obtained and its association with other objects fully recorded.⁴⁷

He then called attention to the "field notes, drawings, plans and photographs" which formed an essential component of the overall body of archaeological data to have been

⁴⁶ Putnam, "The Peabody Museum."

⁴⁷ Putnam, "The Peabody Museum," 164.

centralized at the Peabody.

Putnam was speaking the new rhetoric of archaeological authority that he was helping to create. Of course, the level of exactitude and overall quality of work and accompanying documentation could vary from one investigator's work to another, and highly idiosyncratic factors such as property access to sites could have a direct bearing on what sort of information was obtained. Also, to give the impression that each and every individual object was treated with the same level of care was definitely misleading. Carved steatite figurines and skeletal remains generally attracted more attention and energy than broken projectile points or pottery fragments. The point of Putnam's words was for readers to come away with a distinct sense that archaeology was something that should only be performed by experts. This kind of language also fully characterized Bureau pronouncements made by Thomas and others.

4.3 Conclusion.

My main purpose here has been to characterize the intellectual context of Peabody and Smithsonian archaeology such as it pertained directly to work done in the Ohio valley in the 1880s. As Putnam and Thomas both began orchestrating fieldwork in the early part of the decade, one central aspect of the moundbuilder question remained virtually unchanged from earlier in the century. This had to do with the nature of diversity among North America's aboriginal inhabitants. How significant was that diversity, and how far back in time should it be extended? No two questions could have been more central to the ways Putnam and Thomas each addressed the character of Ohio valley prehistory. At a fundamental level, both men wished to end the ongoing separate race theorizing and

storytelling about the moundbuilders and to situate the mounds more concretely within the continuum of North American prehistory and history. It was precisely in this sense that Otis Mason could claim that they had done much to bridge the chasm of culture between moundbuilders and Indians.⁴⁸ And yet, they did this in very different ways. Putnam wanted to acknowledge as much diversity and complexity to the prehistoric record as the evidence would allow—and, at times, perhaps even more than it allowed. Thomas wanted the exact opposite. For his purposes, the more moundbuilders and extant Indian tribes appeared to be one and the same people, the better. As I pointed out, the two approaches entailed very different methodologies when it came to reasoning about the distant past.

The discussion on museum arrangement was useful for identifying further distinctions between Peabody and Smithsonian styles of archaeology. Wallace's observations about arrangement were particularly informative along these lines. However, his added recognition of the formation of two "schools" of anthropology was probably far more important. Whether he intended it or not, the term was entirely appropriate. Putnam and Thomas, in extending their institutions' reach out into the field in order to centralize archaeological data to these places, were imposing discipline and demarcating hobbyist or popular archaeology from what they were putting forth as expert, authoritative archaeology.

⁴⁸ Mason, "The Mound-builders," 745-6.

Chapter 5.

Networking Ohio archaeology in the 1880s:

The social dimensions of Peabody and Smithsonian centralization.

5.1 Introduction.

The single most important development in Ohio valley archaeology in the 1880s had to do with the dramatically increased involvement of the Peabody Museum and the Smithsonian Bureau of Ethnology. Frederic Ward Putnam, director of the Peabody, and Cyrus Thomas, head of the Bureau of Ethnology's Mound Exploring Division, both were distressed by the accelerating loss of Ohio valley archaeological data to development, agriculture, and increasing numbers of hobbyist archaeologists and relic-hunters. At the same time, many important ethnological questions about the moundbuilders remained unanswered. The institutional authorities saw very clearly that centralization on an unprecedented scale was a necessary step. As a result of Peabody and Smithsonian Bureau of Ethnology efforts, the mounds received more archaeological attention during the 1880s than at any other time.

Whereas the previous chapter addressed the intellectual contexts for the Peabody and Smithsonian mound initiatives of the 1880s, this chapter will address the social dimensions of centralization. Centralization might be characterized as the process by which institutional authorities cultivated networks of associations linking the institutional centers back east with those spaces "out there" where valuable archaeological data existed.¹ In this chapter I will explore a number of factors that contributed to this process

¹ The development of my views about centralization in this chapter have been informed principally by two sources. The first comes from Bruno Latour's notion of "centres of calculation," an applicable descriptor

for both the Peabody and the Smithsonian. According to Latour, one feature of centers of calculation is that they are able to command the accumulation at the center of “things” (specimens, maps, excavation notes) brought from a distance. Latour here is interested specifically in the ways “unfamiliar events, places and people” are rendered in forms that are transportable. Although I am much more interested in the social structures that facilitate creation or acquisition and transmission of archaeological data to the center, Latour’s model has provided a useful way to conceptualize the process of centralization. See, Bruno Latour, Science in Action (Cambridge: Harvard University Press, 1987), chapter 6, especially pp. 219-223.

Secondly, as I began to confront a daunting archival record, Daniel Goldstein’s detailed analysis of the complex and highly heterogeneous nature of the Smithsonian “correspondent community” in the third quarter of the nineteenth century helped me to come to terms with the inherent messiness of the correspondence system. The Smithsonian correspondent community, Goldstein finds, cannot easily be defined in general terms. It was made up of individuals who approached their work from a variety of backgrounds and who conducted it for a variety of reasons. In reality, it did not represent a stable or clearly definable community in the standard sense of the term. Communication between parent institution and individual correspondents was a two-way process; institutional authorities issued methodological and other directives to correspondents in order to impose control over their work in the recovery of data, but at the same time, correspondents frequently approached those authorities with their own concerns and often acted in their own self-interest. See, Daniel Goldstein, “‘Yours for Science:’ The Smithsonian Institution’s Correspondents and the Shape of Scientific Community in Nineteenth-Century America,” Isis 85 (1994), 573-99. Although Latour and Goldstein have had the most direct bearing on my ability to envision a networked process of centralization, a number of other helpful sources that explore relationships between center and periphery in scientific practice, and that address issues of authority and participation in the production of scientific knowledge, have been useful. See: Ian Inkster and Jack Morrell, ed., Metropolis and Province: Science in British Culture, 1750-1850 (London: Hutchinson & Co., 1983); Andrea Rusnock, “Correspondence Networks and the Royal Society, 1700-1750,” British Journal for the History of Science 32 (1999), 155-169; Dorinda Outram, “New Spaces in Natural History,” in N. Jardine, J.A. Secord, and E.C. Spary, Cultures of Natural History (Cambridge: Cambridge University Press, 1996), 249-65; Other sources have emphasized the colonial contexts for many field sciences and natural history. See for,

of networking: the use of correspondent field workers; the impact of expositions in catalyzing archaeological activity; concerted efforts by institutional authorities themselves to spend time in the field as well as to publicize their work in hopes of drawing broader interest and involvement in their projects.

4.2 Salvage archaeology.

Even before mid-century, leading American ethnologists worried about the destruction of mound sites and the loss of archaeological information. By 1880, things in the Ohio valley had reached a crisis point. In the post-Civil War era, the states of the American middle west experienced an unprecedented demographic boom. The state of

instance, Susan Sheets-Pyenson, Cathedrals of Science: The Development of Colonial Natural History Museums During the Late 19th Century (Kingston: McGill-Queen's University Press, 1988); Lewis Peyenson and Susan Sheets-Peyenson, Servants of Nature: A History of Scientific Institutions, Enterprises, and Sensibilities (New York: Norton, 1999), especially chapters 5, 9, and 13; Michael Dettelbach, "Humboldtian Science," Janet Browne, "Biogeography and Empire," Gillian Beer, "Travelling the Other Way," and Michael T. Bravo, "Ethnological Encounters," in Jardine, et al., ed., Cultures of Natural History, pp. 287-304, 305-321, 322-337, 338-357, respectively. Other scholarship has emphasized the importance of the geographical situatedness of scientific knowledge-production, including: David L. Livingstone, Putting Science in Its Place: Geographies of Scientific Knowledge (Chicago: University of Chicago Press, 2003); Crosbie Smith and Jon Agar, ed., Making Space for Science: Territorial Themes in the Shaping of Knowledge (New York: St. Martin's Press, 1998); Robert Kohler, Landscapes and Labscapes: Exploring the Lab-Field Border in Biology (Chicago: University of Chicago Press, 2002); Jeremy Vetter, The Regional Development of Science: Knowledge, Environment, and Field Work in the U.S. Central Plains and Rocky Mountains, 1860-1920 (Ph.D. dissertation, University of Pennsylvania, 2005); Sam Alberti, "Amateurs and Professionals in One County: Biology and Natural History in Late Victorian Yorkshire," Journal of the History of Biology 34 (2001), 115-147.

Ohio itself had a population of close to four million by 1887, split into both large rural and urban segments. It had become an economic powerhouse in both agriculture and industry, and it served as an increasingly well-trodden transportation hub linking the mid-Atlantic and northeastern states to regions in the west.² All of this prosperous growth had an unimaginably detrimental impact on the survival of the archaeological monuments.

There are, of course, no concrete statistics on how many mound and earthwork sites were destroyed, but to say the number was considerable would be a gross understatement. One gauge of the damage was provided by Squier and Davis. Many of the most spectacular earthwork complexes made famous in their 1848 monograph had been completely destroyed or severely damaged in little over thirty years. One such site was the Portsmouth Works at the confluence of the Scioto and Ohio Rivers. On one of his many tireless reconnaissance trips through southern Ohio, Putnam himself stood in utter dismay at the near complete erasure of the site by the steady encroachment of the town of Portsmouth.³ Archaeological fieldworkers for both the Peabody and Bureau of Ethnology regularly found themselves in similar situations: literally with Squier and Davis's maps in

² Transcripts of the various speeches, orations, and addresses delivered at the Marietta Centennial Celebration of 1888 provide a intriguing source for historic and demographic information on Ohio up to the 1880s. The southeast Ohio town of Marietta was the former capital of the Northwest Territories according to the Ordinance of 1787. The Centennial Celebration, held a year after the actual centennial date, was indeed a glorification of Ohio progress in every sense of the term. A number of the speakers involved portrayed Ohio as a providential, chosen land in which economic prosperity meshed inextricably with the moral imperatives of a righteous Christian society that had stood firm against the evils of slavery during the Civil War. See, Ohio Archaeological and Historical Quarterly 2 (no.1, June, 1888), 1-242.

³ F.W. Putnam to Esther Orme Clarke Putnam, 30 May 1887, HUA, Unit HUG1717.2.1 Putnam Papers, General Correspondence 1851-1947 (1881-1890), Box 7 ("M-Z"), Folder "P."

hand, they searched in vain for what was no longer there.⁴

Forms of development such as town or railroad construction may seem the most obvious culprits for the destruction of archaeological sites, but agricultural practices played the most damaging role in obliterating mounds. Given the sheer size and volume of many of the ancient monuments, it is truly remarkable how even just two or three decades of plowing and erosion had the capacity to smudge them out of existence. Add to this the fact that a great proportion of mound sites were located on what had become prime agricultural real estate—the broad, fertile terraces of the Ohio basin—and it becomes easy to see why they fared so poorly, and why farmers often inevitably possessed impressive collections of moundbuilder artifacts.

Other concerns spurred work as well, such as archaeological fraud and looting. Among the most infamous case of fraud was the “Cardiff Giant,” which reportedly was a fossilized man of exceptional stature unearthed in central New York at the end of the 1860s. Despite its being revealed unequivocally as a hoax, it long remained an immensely popular—and profitable—traveling exhibit, especially once P.T. Barnum got involved.⁵ In an early 1883 volume of *Science*, Putnam urged extreme vigilance against at least one Cardiff Giant copycat fraud that had come to his attention.⁶ He summarily dismissed it, too, as a fake; nonetheless, the incident demonstrated the ongoing difficulties faced by the new museum professionals in demarcating scientific archaeology

⁴ Indeed, this fact alone makes Squier and Davis’s monograph a landmark of American archaeology: it is the only record we have of a vast number of Ohio valley sites.

⁵ See, for example, Stephen Williams, *Fantastic Archaeology: The Wild Side of North American Prehistory* (Philadelphia: University of Pennsylvania Press, 1991), 77-97.

⁶ Frederic Ward Putnam, “Archaeological Frauds,” *Science* 1883(1), 99.

from popular fancy. In the same piece, however, Putnam also warned against a far more dangerous situation—in the wake of a rising demand for American antiquities, more and more skilled craftsmen were creating large numbers of fake artifacts for sale. Especially popular was the production of false moundbuilder specimens—in the form of stone pipes, pottery, slate gorgets and hematite celts—many of which had been sold into museum collections both at home and abroad.

Demand for antiquities also fuelled site looting, though this is not something that can be quantified very easily if at all. Reckless digging into mounds for artifacts was not a new phenomenon to the late nineteenth century, but a rising demand for these items definitely was, and Peabody and Bureau of Ethnology authorities became especially vigilant against instances of mound looting throughout the 1880s. Putnam and Thomas regularly worked through local contacts, newspapers, or organizations such as the Ohio State Archaeological and Historical Society to find ways to ward off looting often by appealing to farmers and other landowners to be themselves more watchful or thoughtful in their actions. A common complaint among correspondent fieldworkers for both organizations had to do with the difficulty of finding undisturbed sites (if, as I pointed out above, sites could be located at all). As we shall see, however, there was a hitch. The increased visibility of the Peabody and Smithsonian in Ohio valley archaeology may actually have fuelled demand for antiquities and contributed to some of the very problems both Putnam and Thomas hoped to end.

Given the circumstances, it would be safe to say that the 1880s mound work of both the Peabody and Bureau was driven by an overall sense of crisis, a feeling that it was now or never for the mounds. As it turned out, those who maintained such sentiments were

correct, since so few of these structures have survived the nineteenth century. As bleak as this part of the story is, there was at least one bright spot—the beginnings of a preservation movement that was a long time in the coming. Putnam had become so concerned about the loss of monuments that he took it upon himself to arrange for the purchase, investigation, and restoration of Serpent Mound in 1887.⁷ Serpent Mound subsequently became the first prehistoric monument of any sort in the United States to be placed under protective legislation.

5.3 Centralization and correspondents.

As we saw in the previous chapter, from certain intellectual and methodological points of view, the Peabody and Smithsonian Bureau of Ethnology approaches to the problem of the mounds were poles apart. And yet, none of the discussion from that chapter told us very much about how the archaeological work actually got done. What sort of relationship was there between the institutional centers in Cambridge and Washington and the spaces where fieldwork occurred? What did the various theoretical commitments such as I have outlined them actually have to do with fieldwork and the ways archaeological data was actually generated? For that matter, what constituted fieldwork? In order to answer these questions, it will be necessary to understand something about the complex social structures for practice that facilitated Peabody and Smithsonian archaeology in the 1880s.

For all their intellectual differences, at a certain fundamental level the institutional figureheads of these institutions—Putnam on the one hand, and Powell and Thomas on the other—believed that centralization was the key to everything. Centralization was

⁷ Putnam's work at Serpent Mound is the focus of Chapter 6.

critical if any real progress were to be made on the ethnological problem of the moundbuilders, and it was the only way to counter the ongoing dispersal and loss of various forms of archaeological data. The Peabody and Smithsonian National Museums had both recently undergone significant architectural expansions, so for the time being the problem of storage space for collections had been minimized.⁸ Centralization, however, applied to more than the just gathering of specimens. The process also had to extend to all valuable archaeological data or information, such as maps, excavation notes, and photographs. This sort of information was seen by the institutional authorities as being at least as important as the objects themselves. Centralization also meant the standardization of practice insofar as those wishing to participate in either Peabody or Smithsonian archaeology were expected to adhere to certain rules and follow a certain

⁸ For over a decade, the Peabody existed in crowded temporary quarters until 1877, when a substantial fire-proof building was completed. By the end of the 1880s, a second section was added to it. The Smithsonian Institution received a major financial boost from Congress for its National Museum as a direct consequence of the Philadelphia Centennial. Up until this point, the Museum had always maintained a rather murky status within the Institution, existing mainly as an idea rather than as a physical entity, and it awkwardly shared space within the original Smithsonian building. This situation changed decisively with the completion in 1880 of a sizeable multi-purpose museum structure. For details, see, for example, F.W. Putnam, "The Peabody Museum of American Archaeology and Ethnology in Cambridge," 161-2; Smithsonian Annual Report for 1876, 43; Smithsonian Annual Report for 1880, 125. As historian Curtis Hinsley points out, the 1870s were crucial formative years for the Smithsonian in general, perhaps the most crucial of its history, and the question of how to accommodate anthropology became central to the entire National Museum ideal. See, Curtis Hinsley, The Smithsonian and the American Indian: Making A Moral Anthropology in Victorian America (Washington, DC: Smithsonian Institution Press, 1994), especially Chapter 3.

protocol. One of the major problems with archaeology up to this point in the century was that there had been little uniformity to methods of excavation and documentation employed in the field, a problem which made it increasingly difficult to compare results from different contexts. Earlier efforts to achieve standardization, such as the Smithsonian's distribution of circulars through its Annual Reports and elsewhere, met with limited success. However, after 1880 methodological standardization became a much more acute concern for both Putnam and Thomas.

Correspondents were an especially important element of Peabody and Smithsonian centralization in the 1880s. In general, individuals who became correspondent fieldworkers possessed valuable experience of some form, and they seemed reliable, and trustworthy. Most often they had proven track records in archaeology—Ohio in particular was known for its rather tightly-knit communities of avocational archaeologists, and many had published in local scholarly society periodicals or even the Smithsonian Annual Reports. Many correspondents had other useful experience, such as with topographic or geological surveying. Above all else, these were individuals who wanted to contribute to Peabody or Smithsonian archaeology, even if their personal reasons for doing so varied.

For the most part, correspondents had other careers or means of supporting themselves, which was important considering that fieldwork was done in a rather piecemeal fashion mainly during warmer months, and so was not necessarily a year-round, full-time affair. Populating the correspondent community were doctors, geologists, topographic engineers, teachers, lawyers and judges, clergymen, and retired military officers. Nonetheless, core correspondents generally received some form of financial

compensation for their work. At the least, Putnam and Thomas tried to ensure that their fieldworkers had their expenses reimbursed. In Putnam's case, given the limitations of the Peabody endowment, money for fieldwork came largely through a subscription campaign organized around public lectures that he regularly gave throughout New England. Available funds therefore tended to fluctuate accordingly.⁹ The Bureau's mound survey, on the other hand, received a Congressionally-appropriated annual operating budget. This resulted in one critical difference from the Peabody situation—Thomas could offer a modest salary to at least a small group of fieldworkers who were hired as “agents” of the Bureau.

Either way, the basic mechanics involved in the relationship between correspondent and institution were similar. Fieldworkers maneuvered about through mound territory, visiting sites and making contacts, and they were generally responsible for enlisting additional field laborers. Regular communications flowed from correspondent to institution and vice versa through the mail. If deemed necessary, correspondents received advice or instructions on where to go or how to do fieldwork. They submitted letter reports based on their work for a given period of time, and shipped crates of specimens by rail back to the respective museums. The letter reports were often subsequently incorporated into more synthetic Peabody or Smithsonian publications. Occasionally, and if funds permitted, a corresponding archaeologist would be brought in to help with museum work or to do some further writing.

In and of itself, the use of correspondents was nothing new. It had been a characteristic feature of virtually all Smithsonian field science for several decades, and

⁹ I talk about the Peabody subscription campaign further in the next chapter.

correspondents had been integral for Peabody operations from its inception in 1866.¹⁰ It was at least in part a way of taking advantage of extant regionally situated networks of practitioners. But there was a distinct shift in the tenor of correspondent-institution relationships after 1880. There is a definite sense in which these relationships became much more formal and official sorts of things. The possibility of receiving financial compensation in addition to gaining recognition for work was a major part of this. So, too, was the fact that the institutional authorities—Putnam and Thomas—were themselves continually involved in supervising and directing the fieldwork. Greater levels of control over field activities now emanated from the centers than in previous years.

Yet, even with the relative stability provided by the Peabody and Bureau institutional bases, these correspondence networks were not inherently stable things. Correspondents were not, for example, merely like contract employees temporarily hired to do a particular job, although in the case of the Bureau agents, this was somewhat the case. The correspondence system was composed of individuals who earned the trust of Putnam or Thomas and became involved for many different reasons, but who at times acted more for individual self-interest than for the greater good of systematic Peabody or

¹⁰ Spencer Baird, who was made Smithsonian assistant secretary in 1850, was largely responsible for cultivating and expanding vast networks of correspondents for all the Smithsonian's various geographically-based scientific endeavors. Baird's influence is discussed in detail in Goldstein 1994. On the early establishment of the correspondence system at the Peabody under Jeffries Wyman, see, Curtis Hinsley, "The Museum Origins of Harvard Anthropology, 1866-1915," in Clark Elliot and Margaret Rossiter, ed., Science at Harvard University: Historical Perspectives (Bethlehem, PA: Lehigh University Press, 1992), 123-45, especially p. 123.

Smithsonian archaeology.

Competition also factored heavily into the mix. Considering that Putnam and Thomas were looking to get at the same areas and the same information, it should come as no surprise to find concerns being raised about who had the rights to access sites and to work in particular regions of Ohio, or even to view artifact collections gathered from mound areas. And so we find, for instance, Bureau officials as early as mid-1884 seriously worried that Putnam had laid claim to so many of the important sites near Cincinnati; at the Peabody Museum a few years later, Henry Lee Reynolds, a Bureau field agent, visited the Peabody to do a little research, but found his access to Putnam's Ohio collections severely—and frustratingly—limited.¹¹ Such inter-institutional competition between the Peabody and the Smithsonian subsequently shaped dynamics within their networks of correspondents in very significant ways, as we shall see.

5.4 Individuals in the network.

One of the most successful correspondents for either institution was Charles Metz, who worked closely with Putnam for about a decade or so beginning in 1881.¹² Metz was a Cincinnati area physician and avocational archaeologist who had already undertaken his own explorations throughout the nearby Little Miami River valley. He published accounts of this work in the journal of the Cincinnati Society of Natural History, and he

¹¹ Baird to Thomas, 29 July and 4 August 1884, Smithsonian Institution National Anthropological Archives (NAA), Division of Mounds Exploration Records (DM/ER) 1881-1889, MS 2400 Box 8.; and, Henry Lee Reynolds to Cyrus Thomas, 29 March 1887, NAA/DM/ER 1881-1889, Box 10, Folder "H.L. Reynolds."

¹² Curtis Hinsley discusses Metz as part of Putnam's "correspondence school." See, Curtis Hinsley, "The Museum Origins of Harvard Anthropology 1866-1915," in Elliot and Rossiter, ed., Science at Harvard University: Historical Perspectives (Bethlehem, PA: Lehigh University Press, 1992), 121-45.

was an active member of the Madisonville Literary and Historical Society. In 1881, The American Association for the Advancement of Science held its annual meeting in Cincinnati and Putnam, as permanent secretary of the organization, was in attendance. Putnam then began field collaborations with the Madisonville society and out of this association, Metz emerged as the man upon whom Putnam would come to depend for much of the Peabody Ohio work of the 1880s. He became Putnam's chief correspondent in Ohio, and throughout the decade Metz would be responsible for an enviable register of Peabody fieldwork on some of Ohio's most famous archaeological sites—including the Madisonville Cemetery, Fort Ancient, the Turner Mound Group, and Serpent Mound.¹³

Although his field skills were exceptional and meshed well with Putnam's working methods, Metz was extremely valuable to the cause of Peabody archaeology for other reasons. For one thing, he could afford to spend considerable amounts of time in the field. The Madisonville, Turner, and Serpent Mound investigations, for example, were each done in field seasons over the course of several years if not longer. Metz was also already well-connected with his own group of friends and associates who helped him do fieldwork, people such as Baltimore and Ohio Railroad auditor Charles Low who worked with him before 1880 and accompanied Metz for much of the Peabody work afterward. In essence, by drawing Metz into the Peabody fold, Putnam was getting more than just the help of one individual. He was drawing upon existing locally-situated networks of archaeological practice that could now serve the interests Peabody centralization.

Metz had even more to offer, however. His social standing in southwestern Ohio put

¹³ Members of the Madisonville Literary and Historical Society had begun work on the Madisonville Cemetery, which was a massive prehistoric burial ground located within the Little Miami valley, prior to 1881. Putnam took especial interest in this site and directed further work there with Metz.

him on friendly terms with many landowning farmers in the Little Miami valley who were then willing to grant to the Peabody “exclusive right of exploration” upon their property.¹⁴ Nothing could have been more important. Without permission to access sites on privately owned lands—and to disrupt agricultural activities on those lands—far fewer sites realistically could have been investigated in much detail. The fertile southern Ohio lowlands consisted largely of farms, and this was precisely where a vast majority of mounds were found. Forging and maintaining relations with landowners required some degree of tact as well, such as when Metz sent a letter of condolence to Michael Turner on the death of his son. To Putnam, Metz then wrote in regard to the matter, “You know it is best to keep on the good side of the old gentlemen for next summers’ work & favors.”¹⁵ Metz and Putnam had already completed excavations on some of the Turner Group mounds, and they hoped to continue. Much yet remained to be explored.¹⁶

Living in Ohio meant that Metz was able to keep attuned to sudden developments or opportunities that might conceivably have benefited Putnam and the Peabody. For instance, he was in good position to monitor the availability of privately gathered artifact collections that were being offered for sale, and he could do so with discrimination knowing what sorts of objects Putnam was most likely interested in taking and how much the Peabody director might be willing to pay for them. Metz also kept abreast of land sale

¹⁴ F. W. Putnam, “Archaeological Explorations in Ohio and Wisconsin,” 212.

¹⁵ Metz to Putnam, 25 November 1888, Peabody Museum Archives, Accession File (PM/AF) 88-55-1, “Expedition to Ohio-Serpent Mound.”

¹⁶ Earlier in 1888, Metz expressed at least a bit of concern that Putnam was becoming too engrossed in the Serpent Mound project and momentum on the Turner work was being lost. Metz to Putnam, 16 March 1888, PM/AF 88-55, Folder 1.

possibilities, such as when Fort Ancient was looked for a time to be available for purchase in early 1888. Not much earlier, Putnam had secured the purchase of the tract of land containing Serpent Mound, and there was some hope that the same could be done for the spectacular Fort Ancient site, although nothing came of it in the end. Prices for such historically valuable properties, it seems, were starting to go up. Tracking weather conditions that might impact the course of field investigations was something else Metz could do. At one point in 1888, for example, we find him pressing Putnam to get moving on the plat and survey for Serpent Mound while drought conditions made it easier to do this kind of work.¹⁷

Finally, it should be added that the relationship between Putnam and Metz was an openly congenial one. They were genuine friends who worked well as a team. They periodically lived together in field camp settings for weeks on end—often with other members of their respective families in tow.¹⁸ And without question, Metz's reputation as an archaeologist benefited enormously from his association with Putnam. In any of Putnam's accounts of Ohio work in the Peabody reports from the period, one can hardly read a single page without encountering Metz's name at least once. Putnam was good about giving credit where it was due and about supporting the efforts of his correspondents to publish accounts of their work. Metz became so integral to Peabody archaeology in Ohio that he probably could have made a career for himself as an archaeologist had he wanted; and yet, he was content simply to receive credit in Putnam's

¹⁷ Metz to Putnam 15 March 1888 and 16 March 1888, PM/AF 88-55, Folder 1.

¹⁸ See, for instance, my account of the Peabody work at Serpent Mound on the John Lovett farm in south central Ohio in the following chapter.

reports, and by the early 1890s or so, he rather fades from the archaeological scene.¹⁹

After all, the man had a career as a medical doctor.²⁰

Contrast Metz's case with that of Charles Smith, who stands somewhat at the other end of the correspondent spectrum. Through a combination of serendipity, struggle, and craftiness, Smith carved out a lasting but at times uneasy working relationship with the Bureau of Ethnology. He grew up in western Ohio and had been eking out a poor living as a part-time schoolteacher and special student at the Ohio State University in Columbus in the early 1880s. Smith had developed an intense interest in archaeology and was spending much of his spare time traversing a twenty-square-mile area of the Licking County uplands east of Columbus better known as Flint Ridge. However, he had not published anything of note nor does he seem to have been active in any local archaeological societies. Smith had learned of Thomas's mound survey, and in early 1884 he began pleading with the Bureau for work, but, unlike Metz, he was a completely unknown quantity. His timing could not have been better, however, and he knew exactly what he needed to say in order to get the Bureau's attention.

Just a few months earlier, in the September of 1883, Putnam and Metz had begun

¹⁹ Metz did not entirely give up his archaeological interests. In 1897, he wrote to Putnam as one of the trustees of the Joseph Ferris Memorial Library in Madisonville regarding the care of Serpent Mound Park. Over a decade after that, in 1911, Metz presented a model of the Turner mounds to the Cincinnati Museum. See, Metz to Putnam, 8 May 1897, PM/AF 88-55, Folder 1; and also, Charles L. Metz, A Brief Description of the Turner Group of Prehistoric Earthworks in Anderson Township, Hamilton County, Ohio, Prepared by Dr. Charles L. Metz upon the occasion of his presenting a model of this group to the Cincinnati Museum (Cincinnati: Cincinnati Museum, 1911).

²⁰ Metz was also, apparently, at times heavily engaged in professorial duties, possibly at Miami (of Ohio) Medical College. See, Metz to Putnam, 16 March 1888, PM/AF 88-55, Folder 1.

looking beyond the Little Miami valley and were taking interest in sites further east in Ohio, some of which were famously described by Squier and Davis.²¹ On the surface, Putnam and the Bureau archaeologists maintained gentlemanly respect for each other; behind the scenes, however, was another matter. Thomas and Smithsonian secretary Spencer Baird were troubled about what they perceived as Putnam's growing control on southern Ohio archaeology.²² "Dear Professor," Baird responded to Thomas's concerns specifically along these lines in early August of 1884, "I suspect you are quite correct in your impressions in regard to Putnam. We cannot admit for a moment his right to monopolize any field of archaeological research, and I am glad you express yourself fully in the subject."²³ What the Bureau lacked at this point to counter the Peabody presence

²¹ Putnam reported on a fairly extended reconnaissance of archaeological sites in both Wisconsin and Ohio at the October 21, 1883 meeting of the American Antiquarian Society. A transcript of this account appears in the Proceedings of the American Antiquarian Society, New Series, vol. 3 (October 1883-April 1885), 4-20, especially pp. 10-4. Putnam shortly afterward published an account of the trip in the Peabody reports in which he explicitly expressed that "it has been my desire to examine the ancient works of the Scioto and Paint Creek valleys, many of which have become so well known from the oft-repeated descriptions and figures given by Atwater in 1820, and by Squier and Davis in their valuable work of 1848" (218). In this report, we also find Putnam beginning to take a very proactive stance toward the preservation of ancient works, particularly those of Ohio. See, F.W. Putnam, "Archaeological Explorations in Ohio and Wisconsin," Seventeenth Annual Report of the Peabody Museum (1884), reprinted in The Archaeological Reports of Frederic Ward Putnam (New York: AMS Press, 1973), 209-21.

²² See, for instance, Baird to Thomas, 29 July and 4 August 1884, Smithsonian Institution National Anthropological Archives (NAA), Division of Mounds Exploration Records 1881-1889, MS 2400 Box 8. Baird and Thomas also at least entertained the possibility of approaching Metz in order to obtain a collection from him, but this seems not to have transpired.

²³ Baird to Thomas, 4 August 1884, NAA/DM/ER 1881-1889, MS 2400, Box 8, Folder "Referred by S.F.

was an Ohio-based correspondent who could do for them precisely what Metz had done for Putnam.

Into this scenario stepped Smith, whose prospects for Bureau work brightened considerably by autumn of 1884. A recommendation from Robert McFarland (an Ohio State professor of mathematics and civil engineering) certainly helped his cause, but not nearly as much as his continual assurances to Thomas, Powell, and Baird that he could get more work done for the money than anyone else because he knew the terrain and he was friends with many farmers and others who could assist him.²⁴ Smith wanted to do a thorough study of Flint Ridge, so named for its unique outcrops of very fine, multi-hued, and eminently workable quartzes, high-quality material which had been heavily quarried in prehistoric times and distributed widely by ancient peoples. He got his chance.

The project resulted in quite a respectable paper for the Smithsonian Annual Reports.²⁵ Smith had been doing his research—the paper combined a relatively sophisticated geological understanding of the formation and distribution of silicate minerals with a lucid narrative interpretation of Flint Ridge’s use by ancient native Americans. Smith and his helpers examined a few of the numerous ancient quarry pits that dotted the ridge, some of which were quite massive. One of Smith’s best insights was his identification of different kinds of “workshop” areas that had been used by ancient

Baird to C. Thomas.”

²⁴ McFarland to Powell, September 21 1884; Smith to Baird, 22 August 1884; Smith to Powell, 15 September 1884; and, Smith to Thomas, 29 September 1884, all from NAA/DM/ER, MS 2400, Box 5, Charles M. Smith Correspondence, 1884-1887.

²⁵ Charles M. Smith, “A Sketch of Flint Ridge, Licking County, Ohio,” Smithsonian Annual Report for 1884, 851-73.

peoples to transform the quarried stone from large blocks to more finished and transportable forms. In his analysis, finished artifacts took a second seat to the various intermediate stages unfinished forms which revealed distinctive manufacturing processes.

All in all, it was a skillful piece of work, and Smith thereafter joined the ranks of the paid Bureau field agents, earning somewhere in the neighborhood of one hundred dollars per month. His search for a professional identity under Thomas over the next several years, however, frequently resulted in awkward situations. At one point, for example, we find Smith whole-heartedly toeing the Bureau line when reporting to Thomas that the members of a Maysville, Kentucky, historical society had nothing to say but “incoherent sentences” about moundbuilders and exceptional human antiquity.²⁶ Yet, some time later, Thomas felt it necessary to caution Smith not to fall prey himself to the subject of ancient Paleolithic implements.²⁷ Smith also once informed Thomas of the existence of the Columbus-based Ohio State Archaeological and Historical Society, adding, “but I doubt very much whether anything can be learned from them, as my acquaintance with most of its members convinces me that they know more of almost anything else than of archaeology.”²⁸ A strange commentary, considering that Thomas, Powell, Baird and even Putnam were all corresponding members of this actually rather important state-level archaeological organization. It is indeed somewhat perplexing that Smith was not. That said, given the circumstances, his comments are understandable in that he simply wanted to make himself look better.

²⁶ Smith to Thomas, 10 July 1885, NAA/DM/ER 1881-1889, MS 2400, Box 5, Smith Correspondence.

²⁷ Thomas to Fowke, 31 October 1887, NAA/DM/ER 1881-1889, MS 2400, Box 10, Folder “Gerard Fowke (‘Charles M. Smith’).”

²⁸ Smith to Thomas, 10 July 1885, NAA/DM/ER 1881-1889, MS 2400, Box 5, Smith Correspondence.

Smith was also incessantly worried about the prospects of further work. A vast number of his letters end with often rather desperate-sounding queries along these lines, so it is apparent that his employment status with the Bureau was by no means secure. Smith was, as he told Thomas, willing to go anywhere, even if he thought he would be unable to do efficient work in strange places. And so he found himself being directed rather briskly through many states bordering the Ohio River, as well as at least Michigan. He was also willing to undertake excavations in the frozen dead of winter. And yet he often expressed intense frustration at some of the mundane tedium of correspondent field life (such as dealing with railroad and shipping officials), and suffered episodes of severe writer's block (especially after confronting an unfamiliar piece of new technology called the typewriter).²⁹ Smith was happiest when the work directly involved the stone implements that he loved; anything else, such as collecting information about recent Indian tribal history or dealing with railroad freight offices, simply had to be suffered. He even worried about his abilities at making field sketches, admitting to Thomas that drawing was not one of his greatest skills.³⁰

As was the case with Metz for the Peabody, Smith maintained a ground-level awareness for the availability of artifact collections that might be of interest to the Bureau. In fact, Smith seems to have been particularly successful at ferreting out such collections, probably because wherever he went he was always looking to build his own personal collection. As result, we find him regularly reporting to Thomas on having acquired specimens from various locals, usually farmers. Generally, Thomas seems to

²⁹ See, for example, Smith to Thomas, 9 May 1885, 10 July 1885, and 29 November 1886, NAA/DM/ER 1881-1889, MS 2400, Box 5, Smith Correspondence.

³⁰ Smith to Thomas, 28 January 1885, NAA/DM/ER 1881-1889, MS 2400, Box 5, Smith Correspondence.

have been offered the leftovers after Smith picked out choice items for himself.³¹

Perhaps the most telling part of Smith's story has to do with his own name. Even that, it seems, became tedious to him in his quest for professional identity. In mid-1885, he adopted the moniker Kentucky Q. (sometimes just Ky. Q.) Smith in his letters to Thomas.³² Eventually in 1887, however, he changed his name altogether to Gerard Fowke.³³ Fowke continued working for Thomas until just about the end of the decade. He spent a considerable amount of time in the Washington offices working up a study on stone tools in the National Museum collections that was to be included as part of a planned final report of the Mound Exploring Division. Fowke and two other field agents, James D. Middleton and Henry Lee Reynolds, were also largely responsible for the effort to re-survey a number of Ohio earthworks depicted incorrectly in Squier and Davis's Ancient Monuments of the Mississippi Valley. As far as I have been able to tell from correspondence, Fowke's relationship with Thomas always retained the same degree of restless unease that characterized the earlier period of his work with the Bureau.³⁴

As much as Thomas wished to portray his corps of agents as a rigidly unified front promoting the Bureau views, nothing could have been further from the truth, and the

³¹ See, for example, Smith to Thomas, 6 November 1884 and 28 January 1885, NAA/DM/ER 1881-1889, MS 2400, Box 5, Smith Correspondence.

³² Smith to Thomas, 9 May 1885, NAA/DM/ER 1881-1889, MS 2400, Box 5, Smith Correspondence.

³³ According to one source, Gerard Fowke was the name of a maternal ancestor. See, William Coyle, ed., Ohio Authors and Their Books (Cleveland: World Publishing Company, 1962), 224.

³⁴ Fowke remained in archaeology, too, although not with the Bureau. Shortly after the turn of the century, he could be found working for the Missouri Historical Society where he conducted his own survey exploration of mound sites in the Missouri River valley. See, Michael O'Brien, Paradigms of the Past: The Story of Missouri Archaeology (Columbia, MO: University of Missouri Press, 1996), pp. 124-38.

roster of personalities operating as correspondents changed regularly and unpredictably. As a result, the presence of certain individuals within the ranks of Bureau personnel can seem downright perplexing, as in the case of Stephen Denison Peet. Peet was an Ohio-born Congregationalist minister and a keen student of both American archaeology and Biblical history. In the late 1870s, he founded and subsequently edited the long-running quarterly The American Antiquarian and Oriental Journal, which was quite popular among mid-western archaeology enthusiasts, but enjoyed a much wider distribution.³⁵ He was informed, well-connected to local collectors and hobbyist archaeologists, and a perfect candidate to become a Bureau correspondent for a time in 1886. And this is truly curious, since Peet, in his journal, did little to ingratiate himself with Thomas and others at the Bureau. He openly condemned the mound survey as flawed and he directly accused Bureau agents of being destructive, greedy relic hunters who cared little about archaeology as science.

Peet was a truly interesting character, and his role in the development of mid-western American archaeology deserves a much fuller examination than even the extended treatment I can give here. He saw himself as a real pioneer in the field of archaeology, and he promoted his journal as a first of its kind—an affordable open forum for exchange and publication to which anyone could subscribe and contribute.³⁶ The pages of The American Antiquarian are filled with a great range of subject matter. Mounds and the

³⁵ Peet's journal was not associated with the Worcester-Massachusetts based American Antiquarian Society. It first appeared under the name The American Antiquarian; "and Oriental Journal" appeared in the title of the third volume, beginning in October 1880, probably to avoid confusion with the older antiquarian society.

³⁶ S.D. Peet, "The Field We Occupy," American Antiquarian 1 (1978-9), 45-6.

antiquities of mid-western America figure most prominently, but each issue often included articles on such topics as American pioneer history, Biblical history, mythology, linguistics, and ethnography. Leading American ethnologists made scholarly contributions to the journal, and individuals who were not at all schooled in ethnology reported on finds that they had made or that were made in their neighborhoods. Peet fostered close ties with the Ohio State Archaeological and Historical Society and other more locally-based groups as well as individual collectors, and he was also remarkably well-connected to European anthropological circles and thus well aware of developments there.³⁷ Thus, one may also find in the journal such things as immensely useful catalogs of artifact collections and lists of collectors in Ohio, in addition to translated accounts originally by leading European anthropologists such as Pierre Broca. Although Peet billed The American Antiquarian as a “quarterly journal devoted to early American history, ethnology, and archaeology,” and it was published by Jameson and Morse in Chicago, it would probably be fairest to characterize the periodical not as a journal in the scholarly sense but more as something in between that and popular magazine.

To say that Peet himself wrote prolifically about the mounds in The American Antiquarian is a gross understatement. Most issues of the journal from its inception through the 1880s contain at least one lengthy study by him on mounds, in addition to many pages of related editorial material. Peet, it should be pointed out, often incorporated into his studies the sorts of Old World or Biblical analogies that Thomas and Powell did

³⁷ He was involved in some of the early proceedings of the French Congrès international des Américanistes, delivering a paper on “Les Mound-builders” to the organization’s second meeting in Luxembourg. See, Stephen D. Peet, “Les Mound-Builders,” in Congrès international des Américanistes, Compte-rendu de la seconde session, Luxembourg—1877, 1, 103-121.

not like.³⁸ And yet, despite his theological leanings, Peet generally remained quite open-minded about many of the pressing archaeological issues of the day—the nature of prehistoric human diversity and the question of human antiquity in particular.

Peet was quite influential through his editorial pieces, which served as something of a sounding board for methodological issues directly relating to the mound explorations of the Bureau of Ethnology and the Peabody Museum. In one piece from October, 1883, he generalized about the existing state of mound archaeology:

It would appear that mounds have heretofore been visited mainly with the idea of securing relics from them...as if the only way of studying mounds was to dig into them and destroy them.... No attempt is made to ascertain the reason for their erection in the localities where they are found, rather than in other localities; no study of the topographical surroundings, and no acquaintance with the system prevalent.³⁹

Too much contextual information was being lost in the rush to grab artifacts. “Private collectors,” he continued,

are not the only parties who are guilty of destroying mounds merely for the relics which they may contain.... The destruction of the monuments, we are sorry to say, has been carried on by the agents of the Smithsonian Institution. It is strange that such wantonness should be encouraged by an institution designed for the encouragement of science, but the passion for accumulating relics and enlarging the museum has overcome all consideration for the monuments.... Such is Science! It destroys more than it gathers.⁴⁰

Interesting timing for this commentary, too. In September, Putnam had made a

³⁸ To name just a few examples, see Stephen D. Peet, “A Comparison Between the Archaeology of Europe and America,” *American Antiquarian* 1 (1878-9), 212-24; “The Religious Character of the Emblematic Mounds,” *American Antiquarian* 6 (1883-4), 393-411; “The Serpent Symbol in America,” *American Antiquarian* 8 (1885-6), 197-221; “The Great Serpent and Other Effigies,” *American Antiquarian* 12 (1890), 211-28.

³⁹ Stephen D. Peet, “Explorations of Mounds,” *American Antiquarian* 5 (1883), 333.

⁴⁰ Peet, “Explorations of Mounds,” *American Antiquarian* 5 (1883), 333.

reconnaissance of mound sites throughout Wisconsin and Ohio. The Ohio leg of this trip—made with Metz—had gotten the attention of Thomas at the Bureau since it seemed to mark Putnam’s desire to expand the Peabody presence in Ohio beyond the Little Miami valley. As it turned out, Peet had accompanied Putnam in Wisconsin and had been influenced by the Peabody curator’s views on methodology. Putnam, by this time, was definitely thinking about mound sites as complex, multi-component entities that demanded extensive sub-surface investigation, a view not exactly shared by most contemporaries. The visible mounds themselves, Putnam thought, as well as collections of artifacts, told only small parts of a bigger story that could be understood only by integrating all the available archaeological information pertaining to a given site.⁴¹ While Peet did not quite understand the full archaeological significance of Putnam’s way of seeing things, and remained wedded to a view according to which different classes of archaeological data could be abstracted from each other and treated separately, he nonetheless preached an important message in his editorials.⁴² Greedy and careless relic-hunting in the interest of building collections was wholly counter-productive to the cause

⁴¹ Putnam’s work in the Little Miami valley arguably played a critically formative role in shaping these beliefs, a circumstance I will discuss this further in the next chapter in contrasting Putnam’s methodological views with those of Thomas. Putnam first gave some indication of the impact the Little Miami valley work—conducted with Metz—in his Peabody report for 1884. See, for example, F.W. Putnam, “Archaeological Explorations in Ohio and Wisconsin,” Seventeenth Annual Report of the Peabody Museum (1884), reprinted in The Archaeological Reports of Frederic Ward Putnam (New York: AMS Press, 1973), 209-21, esp. p. 209.

⁴² In one editorial, for example, Peet claimed that the monuments themselves, rather than artifact collections, revealed the real life of ancient people. See, S.D. Peet, “Relic Hunting Versus Archaeological Survey,” The American Antiquarian 6 (1883-4), 205.

of archaeology.

Thomas responded to Peet's accusations in early January, 1884. In a letter to Peet, he defended the actions of Bureau agents, stating that they routinely made drawings and took notes on topography.⁴³ Peet was not satisfied. Planning to reprint Thomas's letter in the next American Antiquarian, Peet wrote back: "My readers will see that your letter is no answer at all but rather an evasion."⁴⁴ In Peet's mind, not only had Thomas avoided addressing the relic-hunting issue, he had also failed to make other critical distinctions. As specimens were gathered and sent on to Washington, DC, Peet wondered, did the Bureau and Smithsonian National Museum differentiate between those unearthed from mounds as the result of directed excavations and others picked up from the surface? Did they discriminate clearly between materials actively acquired by agents of the Bureau and "all the mass of donated relics" that were constantly being passed on to the institutional center? Peet suspected not. "It makes the case worse instead of better," he wrote. "You throw the honor of your names over into the collective of those who have been destroying mounds and sending relics to the Smithsonian for these many years."

Thomas immediately sent another communication in which he reiterated the point that agents took copious notes in the field, and that exceptional care was taken in cataloging specimens both in the field and back at the museum.⁴⁵ Collections, Thomas

⁴³ Thomas to Peet, 1 January 1884, reprinted under the title "The Destruction of Mounds," in The American Antiquarian 6 (1883-4), 41; Thomas to Peet, 12 January 1884, reprinted under the title "Manner of Preserving Mound Builders' Relics," The American Antiquarian 6 (1883-4), 103-6.

⁴⁴ Peet to Thomas, 8 January 1884, NAA/DM/ER, 1881-1889, MS 2400, Box 10, Folder "Stephen D. Peet (Criticizing the Thomas Mound Surveys.)"

⁴⁵ Thomas to Peet, 12 January 1884, reprinted under the title, "Manner of Preserving Mound Builders'

emphasized, were not merely lumped all together; beginning in the field, Bureau catalog records were made according to standardized procedures in which a certain minimal level of provenance information was required. That information was preserved in the transition from field to museum catalogs so that it could be easily tracked down for any given specimen or group specimens. Thomas provided a sample of a catalog record made by Bureau agent P. W. Norris. “Every possible precaution,” Thomas wrote, was made “to insure the identity and preserve the correct history of the collections made by the Bureau.” Furthermore, Bureau agents were paid “for the time they are employed, and not according to the collections made.”

Baird wondered what Peet was up to, and suspected ulterior motives. Why shouldn’t the Smithsonian accept donated collections or contributions from miscellaneous parties? “I do not know what the venerable old gentleman is aiming at in his criticisms,” Baird wrote in an internal memo to Thomas, but he thought “at the bottom of it is the desire of Mr. Peet to be employed by the Bureau at a good salary, to survey and report on mounds in the northwest.”⁴⁶

Peet printed both of Thomas’s letters in his journal. Interestingly enough, in the same early 1884 issue appeared what was supposed to be a review by Thomas of a recently published study of mounds.⁴⁷ Given the disputatious context, it is not surprising that

Relics,” The American Antiquarian 6 (1883-4), 103-6.

⁴⁶ Baird to Thomas, 14 January 1884, NAA/DM/ER, 1881-1889, MS2400, Folder “Stephen D. Peet (Criticisms...)”

⁴⁷ The study under review was “The Mounds of the Mississippi Valley Historically Considered” by Lucien Carr, which had been published by in the Memoirs of the Geological Survey of Kentucky for 1883. See, Cyrus Thomas, “Who Were the Mound Builders?,” The American Antiquarian 6 (1883-4), 90-9.

Thomas sidelined the actual review, and instead used the occasion to further promote the work of the Mound Exploring Division. In rather officious terms, Thomas outlined the Bureau's position on the moundbuilder-modern Indian question, and he provided some results of the mound survey. In the process, Thomas proposed a classification of ancient works into geographic districts, each distinguished by the prevalence of particular structural or architectural characteristics, and he frequently alluded to the detailed field studies being performed under the auspices of the mound survey. By these appearances at least, progress was being made by the Bureau.

I mentioned earlier that Peet was intellectually open-minded. This certainly extended to his editorial policy of printing in The American Antiquarian just about anything on archaeology that came his way, even if he disagreed with it in principle. Thomas's review presented such a case. Before it went to print (and on the basis of a draft copy of the article), Peet claimed priority for the system of mound classification into geographic districts in a letter to Thomas.⁴⁸ Nonetheless, the review was printed with all of Thomas's claims intact.

Well, open-minded or not, Peet was just getting started in his criticism of the Bureau's mound project. After having aired much of the exchange with Thomas out in the open for his readers, he came right back with another editorial in May entitled "Relic Hunting Versus Archaeological Survey."⁴⁹ "It is evident," Peet opened, "that the work of relic hunting must go on. The authority of the government, the example of the museums,

⁴⁸ Peet to Thomas, 8 January 1884, NAA/DM/ER, 1881-1889, MS 2400, Box 10, Folder "Stephen D. Peet (Criticizing the Thomas Mound Surveys.)"

⁴⁹ Stephen D. Peet, "Relic Hunting Versus Archaeological Survey," American Antiquarian 6 (1883-4), 204-8.

besides the zeal of about five hundred private collectors, are all given to it and nothing can withstand it.”⁵⁰ By “authority of the government” Peet meant the Bureau of Ethnology, his principle target in this piece. Thomas’s mound survey, he thought, was focused on building collections. Not enough attention was being paid to the monuments themselves in their natural settings, to careful mapping and topography in the sense of a true survey. Peet thought the current Bureau efforts fell far short of the standard set much earlier by Squier and Davis, whose Ancient Monuments of the Mississippi Valley—published in 1848 by the Smithsonian Institution itself—remained in his view the model of a true archaeological survey and still the most widely cited work on American archaeology.⁵¹

Peet was unsparing in his critique of the Bureau. Thomas’s letters, he claimed, were unsatisfactory responses and did nothing to alleviate the charge of relic-hunting. Peet singled out Norris who passed unscrupulously “from one part of the country to another, wherever the most relics could be secured.”⁵² The upshot of such careless practices, he continued, was that monuments were being destroyed and an entire category of scientific data was disappearing as the inevitable consequence. Thomas needed to do much more to

⁵⁰ Peet, “Relic Hunting Versus Archaeological Survey,” 204.

⁵¹ Peet, “Relic Hunting Versus Archaeological Survey,” 204. Peet’s point about Squier and Davis is significant, for it demonstrates the extent to which Ancient Monuments remained the authoritative study for many archaeological scholars. Such attitudes were to change in short order. Within a few years, it would become evident that Ancient Monuments was full of survey errors and inaccuracies. Many of Squier and Davis’s maps depicted Ohio earthworks falsely consisting of perfect geometrical regularity. Some of Thomas’s field correspondents would resurvey some of these sites, the results of which were eventually presented in Thomas’s Circular, Square, and Octagonal Earthworks of Ohio, issued in 1889.

⁵² Peet, “Relic Hunting Versus Archaeological Survey,” 206.

protect and preserve mound sites first so that thorough surveys could be made prior to any collecting. But even this wasn't the end of it. Peet also wrote that Thomas's "grand discovery" that modern Indians were responsible for the mounds was hardly original, and had in fact been acknowledged by at least a few scholars for some time.⁵³ The Bureau's agenda to link modern Indians to the moundbuilders was somewhat of a red herring, Peet thought, essentially making them all out to be one people. It detracted from the more nuanced archaeological problem of distinguishing the various successive periods of moundbuilding.⁵⁴

Peet was nothing if not audacious, a quality perhaps stemming from experience as an independent Congregationalist minister. Whatever the case, he actually sent a preliminary draft of "Relic Hunting Versus Archaeological Survey" to Thomas, with a polite but rather threatening attached note:

If the treatment of the subject is unjust, I am prepared to modify it but unless I hear from you, I expect to publish it as Editorial. Some of us have been trying very hard to get appropriations from some source for this work. My own opinion is Mr. Norris is doing more damage than he is good. I see reports of his finds in all the newspapers, and [praise?] for him and for his work. Of course the tendency is for every collector to pitch in and get all he can. I protest against the whole thing and shall keep up the agitation until some change takes place.⁵⁵

And publish it he did, with few changes.

Peet's concerns about preservation and the need to seek a more complex understanding of the prehistoric past echoed the concerns of Putnam. Putnam had been expressing his views in accounts of his reconnaissance trip through Wisconsin and Ohio

⁵³ Peet, "Relic Hunting Versus Archaeological Survey," 206-7.

⁵⁴ Peet, "Relic Hunting Versus Archaeological Survey," 207.

⁵⁵ Peet to Thomas, n.d., but headed with "Editorial for May 1884," NAA/DM/ER, 1881-1889, MS2400, Box 8, Folder "Referred by S.F. Baird to C. Thomas."

and his influence seemed to be taking hold.⁵⁶ As I mentioned above, Peet was with Putnam for part of the excursion, and he certainly developed not only a deep respect for the man's work, but also something of an allegiance to his methodological views. Peet's disdain for relic-hunting was a relatively recent development, it seems—some of his earlier American Antiquarian writings placed a very strong emphasis on the heuristic value of individual specimens and collections over other forms of archaeological information.⁵⁷ Yet, in the “Relic Hunting” editorial, he had very clearly reversed his position, emphasizing field experience over artifacts. “The data of the science of archaeology,” he claimed,

are not found in the relics exclusively—in fact, a very small part of the information which may be gained concerning the prehistoric races come from the study of the relics. The real life of the pre-historic people is revealed to us by the monuments. We acknowledge that the cabinets have furnished many facts and that various theories have arisen from the study of relics from the cabinets, but a broad and comprehensive view of the science will come only from the study in the field; just as much so in Archaeology as in Geology.⁵⁸

If the change in attitude did not stem directly from Putnam, the association at least had a

⁵⁶ Putnam delivered one account of the trip to the October 21, 1883 meeting of the American Antiquarian Society. A transcript of this was later printed in the combined Proceedings of the American Antiquarian Society, New Series, volume 3 (October, 1883-April, 1885), 4-20. Putnam published another account of the trip in the Peabody Reports for the following year. See, F.W. Putnam, “Archaeological Explorations in Ohio and Wisconsin,” Seventeenth Annual Report of the Peabody Museum (1884), reprinted in The Archaeological Reports of Frederic Ward Putnam (New York: AMS Press, 1973), 209-21.

⁵⁷ See, for example, S.D. Peet, “The Sources of Information as to the Prehistoric Condition of America,” American Antiquarian 2 (1879-1880), 33-48, especially pp. 44-6. In this piece, Peet also emphasized the importance of studying the architectural characteristics of earthworks in their natural settings, but he presented this as secondary to the importance of studying relics.

⁵⁸ Peet, “Relic Hunting Versus Archaeological Survey,” 205.

lot to do with it.

In yet another American Antiquarian editorial notice from just a bit later in 1884, Peet lauded the Peabody efforts, contrasting Putnam's "thorough exploration of a locality for the sake of information" with the "superficial, haphazard search for relics" that had characterized in his mind the Bureau work.⁵⁹ Peet did not name the Bureau directly, but the implication would have been fairly clear. "Prof. Putnam," he continued,

does not believe that any one tribe or race built the earthworks, and he is therefore anxious to ascertain the tokens left by all the races, and to so explore and examine and collect and arrange so as to leave the characteristics of each separate layer or horizon.⁶⁰

The Peabody Museum and not the Smithsonian was setting a good example for the course of archaeology, and given the frenetic level of relic-hunting and unprincipled mound exploration going on, good examples were desperately needed. Peet thought Putnam was leading the charge in doing the kind of work demanded by the mounds—intensive field studies which included as much attention to topography and natural setting as to excavation practices. And so it was that, in 1884 at least, readers of The American Antiquarian would have come away with the sense that the Bureau of Ethnology was not helping matters. The Peabody was.

None of this made the Bureau officials very happy, needless to say. Thomas and Baird were well aware of the increasing Peabody presence in Ohio after late 1883 and into 1884, and they monitored the relationship that was developing between Peet and Putnam over the course of the following several months. In August, Baird, responding to

⁵⁹ S.D. Peet, "The Peabody Museum in the Field," American Antiquarian 6 (1883-4), 277.

⁶⁰ Peet, "Peabody Museum," 277. In the next chapter, I will explore in considerable detail how Putnam's beliefs along these lines informed his field practices, and vice-versa.

Thomas from Wood's Hole, Massachusetts, expressed unease about Putnam's growing monopoly on southern Ohio archaeology (see the discussion about Smith above), and added, "I should not be surprised if your impressions about his egging Peet on in his insinuations have a natural basis. I have torn up your letter, as desired."⁶¹

In every respect, Peet was a thorn in the Bureau's side at this point in time, contributing to what turned out to be a pivotal period for the Mound Exploring Division. In late 1884, two core field assistants dropped out of the picture, Edward Palmer—who was another conspicuously successful specimen collector—and the notorious Norris, who died at the end of the year.⁶² New agents were enlisted, including Charles Smith, as discussed above. Smith's Flint Ridge project, while certainly yielding a sizeable collection of artifacts and stone material, also went well beyond a mere collecting mission in its attention to fieldwork. By the end of 1884, even Peet eased up on the Smithsonian, having learned of a new circular to be distributed by the institution in which explicit emphasis was placed on the preservation and careful survey of mounds.⁶³ After mid-decade, the issue of topographical survey work—of "the monuments themselves"—

⁶¹ Baird to Thomas, 4 August 1884, NAA/DM/ER, 1881-1889, MS2400, Box 8, Folder "Referred by S.F. Baird to C. Thomas." Baird, apparently, was making good use of his influence at the Wood's Hole oceanographic laboratory in August. He managed to acquire a copy of a letter written by Putnam to botanist and Harvard trustee Asa Gray (dated 15 August 1884), in which Putnam expressed great concern that the "destructive presence" of Bureau agents in the Little Miami valley was threatening Peabody work there. Baird forwarded the copy to Thomas with the note, "Please keep it with your other files." Baird to Thomas, 21 August 1884, NAA/DM/ER, 1881-1889, MS2400, Box 8, Folder "Referred by S.F. Baird to C. Thomas."

⁶² Smithsonian Annual Report for 1884, 67-8.

⁶³ Peet, "Instructions on Mound Exploring," American Antiquarian 6 (1884), 350-1.

became a significant preoccupation within Thomas's Division, most particularly regarding a number of Ohio earthwork complexes inaccurately portrayed by Squier and Davis.

And so it was, that, for all his antagonism toward the Bureau's mound survey, Peet eventually found himself working for it in 1886. If this was truly what he desired, he was not very happy once there. At one point, Peet could be found complaining bitterly about not being paid as much as other field assistants, and about the fact that he was keeping the work going largely at his own expense.⁶⁴ Fifty dollars a month disappeared quickly when one needed to cover travel expenses and lodging, and to hire additional field hands for digging.

Why did Thomas want Peet working for the Bureau of Ethnology? The answer should be fairly obvious. In the context of centralization, the man was simply too valuable—and powerful—an asset to be marginalized from the process. Peet had already helped out Putnam and the Peabody Museum, and his journal was influential and reached many people. Mollifying Peet and utilizing his journal could—and did—serve crucial functions in the service of network-building and centralization. It was simply better in the long run to work with him rather than against him.

5.5 Remarks on correspondence networks.

My focus here has been on the archaeology of the Ohio valley mounds, and the cases of Charles Metz, Charles Smith, and Stephen Peet are but three of a number of examples that come up in this context. They are particularly instructive for illustrating a number of important points about the nature of the correspondence networks that were critical to the

⁶⁴ Peet to Thomas, 4 June 1886, NAA MS 2400, Box 9, Stephen D. Peet.

success of Peabody and Smithsonian archaeology of the period, and about the process of networking-building and centralization in general, as well as the role of authority in forging and maintaining these networks.

First of all, these networks must be seen not as stable entities but as collectives of individuals operating under their own motives. This fact must be taken into account if we wish to understand the processes by which Peabody and Smithsonian archaeology was accomplished. By any reckoning, the relationship between Metz and Putnam was just about ideal. Metz had an established record of archaeological work, and he willingly contributed to Peabody archaeology without visible ulterior motives. He was essential for Putnam's designs for working in the state of Ohio. Smith's case highlights the difficulties faced by someone who saw new career possibilities in archaeology thanks to the activities of the Bureau of Ethnology, even if those possibilities were in reality few and far between. Unlike Metz, who was a career doctor, Smith hoped to make archaeology a full-time vocation at a time when the glimmers of a professional discipline were only beginning to appear. For Smith, there was no model to emulate in choosing the path that he did. As a result, he faced certain difficulties and frustrations that influenced the sorts of choices he made in building a relationship with Thomas and the Bureau.

Peet's case speaks for itself here. Here was a man who had been deeply involved in mound archaeology on his own terms and whose motives involved challenging the very basis of the Bureau's survey on methodological grounds. I think it might be fair to say that jealousy of some form did underlie his attitudes toward the increasing involvement of the eastern institutions, even if he did agree with the Peabody methods over those of the Bureau. Peet certainly thought of his journal as an important and powerful new force

in archaeology, serving in its own way a vital centralizing function, and one that offered considerable resistance to the eastern institutions.⁶⁵ In a certain sense, for Peet, acknowledging Peabody and Smithsonian archaeological centralization meant deferring control and authority to those institutions. Nonetheless, The American Antiquarian continued to occupy a prominent watchdog role, bringing methodological debates out into the open for its many readers. For those who followed such deliberations, it was now apparent that there were consequences for doing archaeology one way rather than another.

The examples of Metz, Smith and Peet further serve to highlight a range of dynamics that characterized the manner in which institutional authority was imposed. The Peabody Museum and Smithsonian did not simply take control of archaeological practice in order to achieve centralization—there was considerable negotiation going on between institutional figureheads and individuals in the network almost continually. Putnam and Thomas had to respect the fact that fairly stable communities or organizations of avocational archaeologists existed, and that these needed to be either worked with or worked around. Groups such as the Madisonville Literary and Historical Society, with its strong emphasis on the archaeology of the Little Miami valley, and Peet's American Antiquarian, which tied together a large readership that seems to have consisted mostly of mid-western avocational archaeologists, both played significant roles along these lines. On top of all this, inter-institutional competition between the Peabody and the Bureau also greatly shaped those institutions' responses in dealing with correspondents and building networks.

⁶⁵ See, for example, S.D. Peet, "The Field We Occupy," American Antiquarian 1(1878-9), 45-6.

For example, things worked out nicely for Putnam insofar as Metz was concerned. Here, very little by way of accommodation was required on Putnam's part once the working relationship was established. This happened quite easily—in the early 1880s, Putnam took an active interest in the explorations being conducted by the Madisonville Literary and Historical Society, and out of that developed the relationship with Metz.

The situations with Smith and Peet were quite different. In each of these instances there was a considerable amount of adaptation on the part of the Bureau of Ethnology. Under different circumstances, I am not so certain Smith would have been given such an opportunity, as he had few to no archaeological credentials to his name. He certainly had not published anything nor showed any interest in associating with any archaeological or historical societies (at least early on), and there is plenty of room to question whether he had any substantial archaeological experience at all other than walking through farm fields and looking for artifacts. Even at this pre-professional stage of archaeological disciplinary identity, Smith stood out. As I have discussed, the timing of his approaching the Bureau for work was spot on. Thomas and Baird were concerned about Putnam's designs on Ohio, designs which from their perspective also involved Peet. I do not think it is at all out of the question that, given the context, Smith's relative un-connectedness with the archaeological scene may have played as an asset in his favor.

Metz, Smith, and Peet were not the only correspondents involved in Smithsonian and Peabody mound work in the 1880s, although they are the figures upon whom I have chosen to focus due to their relevance directly to Ohio work. To be fair, a couple of others deserve at least a passing mention. Henry Lee Reynolds, for one, did a considerable amount of work throughout the Ohio and Upper Mississippi valleys for the

Bureau of Ethnology for a couple years beginning in early 1887. Along with both James Middleton and Fowke, he was involved with resurveying a number of the spectacular geometric earthwork complexes in south central Ohio first documented (inaccurately) by Squier and Davis in the 1840s.⁶⁶ Reynolds suggested to Thomas a possible early French influence on the construction of these works, an hypothesis Thomas took into consideration in his Bureau report on the matter.⁶⁷ At one point, Reynolds was sent to

⁶⁶ Squier and Davis's depictions of these sites more often than not were used in support of moundbuilder separate race theories.

⁶⁷ Cyrus Thomas, The Circular, Square, and Octagonal Earthworks of Ohio (Washington, DC: Government Printing Office, 1889). In keeping with the overall Bureau aims for the mound survey, Thomas wanted to demonstrate that these particular works were not the ancient products of an advanced race with some sophisticated knowledge of geometry and engineering. The new survey data showed that Squier and Davis had been more than a bit liberal in rounding off measurements to depict perfectly regular embankment structures. In this 1889 publication, Thomas argued that the sites were of relatively recent origin, and that there was "nothing in [their] form or arrangement that is inconsistent with Indian ideas and usages" (p. 32). By denying Indians knowledge of basic principles of geometry and by repeatedly demonstrating how practical techniques alone could have been used to plan and build these sites, Thomas hoped to build a strong case for the exclusively Indian origin of the works. An important point should not be lost: the Bureau strategy in striking out against the "moundbuilder myth" was not to argue that Indians in fact possessed the creativity and mental capacity to have once lived as moundbuilders. Instead, the strategy involved downplaying the level of sophistication typically associated with moundbuilders. "No valid reason," Thomas concluded, "can be presented why Indians, taught by necessity and practice, could not lay off by the eye and by means at hand figures with which they were familiar more correctly than the white man without instruments" (p. 33). In this context, Reynolds's suggestion of French or European influences on the architecture of the geometric earthworks was worthy of consideration. For one, if true, it would support the idea that these sites were not that old; second, it would provide a further possible explanation

Cambridge to visit the Peabody Museum, meet with Putnam, and have a look at Putnam's Ohio collections, presumably because this would help get a better idea of what to expect when working in the state. Reynolds found Putnam cautiously guarding his Ohio materials—they were locked away and not on exhibit, and, although Putnam spent two full days showing them to Reynolds, he would not let the Bureau agent make sketches nor take notes on many of them. In confidence to Thomas, Reynolds surmised that Putnam did not want his data being used to support the Bureau's theory about the moundbuilders.⁶⁸ Reynolds also confirmed to Thomas one of the accusations made a few years earlier by Stephen Peet when he encountered first-hand evidence that field methods employed by P.W. Norris in the early phase of the survey were indeed not very good.⁶⁹

Hilborne T. Cresson worked as a Peabody correspondent in 1890 and 1891 in the rush leading up to the 1893 Chicago World's Columbian Exposition for which Putnam was overseeing the anthropological exhibits.⁷⁰ Cresson was a Philadelphia medical student

for how the ancestral Indians were able to build such complex works. Although Thomas gave due consideration to Reynolds's theory, he generally rejected the possibility of widespread European influence on aboriginal architecture.

⁶⁸ See, for example, Reynolds to Thomas, 28 February and 29 March 1887, NAA/DM/ER 1881-1889, Box 10, Folder "H.L. Reynolds."

⁶⁹ Reynolds to Thomas, 9 January 1888, NAA/DM/ER 1881-1889, Box 10, Folder "H.L. Reynolds."

⁷⁰ More on Cresson's background may be found in Stephen Williams, Fantastic Archaeology: The Wild Side of North American Prehistory (Philadelphia: University of Pennsylvania Press, 1991), 123-9.

According to Williams, Cresson was on the Peabody books as "Field Assistant" from 1887, but he provides no citations in support. In the 1860s Cresson claimed to have found in the Delaware River valley the "Holly Oak Gorget," a marine shell ornament engraved with a mammoth likeness purportedly recovered from Pleistocene gravels. This object, ultimately shown to be fraudulent, was the subject of intense controversy

who desperately wanted to drop his medical duties to get a foothold in archaeology and work “heart and soul” for Putnam.⁷¹ And work he did. In two short years, Cresson logged many hard miles traversing the continental United States, from the east coast to Ohio to Colorado and back to Ohio. In Philadelphia, Cresson directly experienced animosity toward the Peabody from the anthropological circle that included Daniel Brinton. These men, he wrote to Putnam, were nothing but rich merchants and lawyers who did no fieldwork and acquired their specimens from auctions. “They are the secret enemies of our museum,” Cresson continued, “and have tried in every case to throw dirt at and on our researches [into the American palaeolithic].”⁷² In the summer of 1890, Cresson worked with Metz at the Turner site. The following year was a busy one—he began field investigations in Ohio, then spent four rugged months doing “reconnaissance” work on Pueblo sites in southwestern Colorado, only to return by late August to the more hospitable environment of Ohio to continue investigations at Fort Ancient and the Foster’s group of mounds.⁷³

Warren King Moorehead was another intriguing character who floated through the correspondent community of Ohio archaeology during the 1880s. A young Ohio man of

for decades.

⁷¹ Cresson to Putnam, 2 January 1890, Peabody Museum Director Records of F.W. Putnam (PM/DR/FWP), 89-15, Folder “1890-H.T. Cresson.” According to this letter, Cresson’s family had earlier loaned a number of specimens to the Smithsonian National Museum, and he worried that Putnam would view him as an ally of that institution rather than of the Peabody. Cresson felt the need to explain to Putnam that his trips to Washington had been undertaken with the intent of reclaiming his specimens so that they might go to the Peabody instead.

⁷² Cresson to Putnam, 21 January 1890, PM/DR/FWP, 89-15, Folder “1890-H.T. Cresson.”

⁷³ Cresson to Putnam, 24 August 1891, PM/DR/FWP 89-15, Box 11 “1891 A-F,” Folder “UAV 677.38.”

apparently independent financial means, Moorehead first contacted Baird in early 1885, and then later Thomas in mid-1886, looking to see if he could be the Smithsonian's archaeological agent for Licking County. He was, it seems, entirely unaware that Charles Smith was already working in that county and had just finished his project on Flint Ridge. Moorehead had already been doing his own fieldwork (with the assistance of a number of field hands) and had built a large personal artifact collection. When a paid Bureau position looked out of the question, he simply stated that he would work at his own expense, and, as he was just then finishing college, would be able to devote much more time to archaeology.⁷⁴

I have not yet been able to follow Moorehead's path in great detail through the archival record, but I have seen enough to indicate that there is quite an interesting story to be uncovered. For one thing, by the very early 1890s, we find him working not for the Bureau of Ethnology, but for Putnam, either under the auspices specifically of the World's Columbian Exposition preparations or for general Peabody Museum explorations.⁷⁵ By this time, Moorehead had very carefully established a reputation for

⁷⁴ Moorehead to Baird, 3 March 1885; Moorehead to Thomas, 7 June and 13 November 1886, NAA/DM/ER 1881-1889, MS2400, Folder "Warren K. Moorehead, Licking Co. 1885-6."

⁷⁵ Moorehead was supervising investigations of the Hopewell mound group in south-central Ohio for Putnam. Williams, *Fantastic Archaeology*, 125; Browman, "Origins of Stratigraphic Excavation in North America," 251-2. For at least a short time prior to working for Putnam, however, Moorehead could be found in Washington doing some of work at the National Museum, most likely as an unpaid "resident collaborator," that was independent of any Bureau of Ethnology activities. In one reference to this situation, a letter from Thomas to Bureau linguist Henry Henshaw, Thomas indicates his feeling that Moorehead was not qualified enough for Bureau work. See, Thomas to Henshaw, 20 April 1890, NAA/DM/ER 1881-1889, MS 2400, Boz 9, Folder "1894-1896."

himself as an archaeologist, largely through his own investigations at Fort Ancient. The sprawling hilltop “fort” was one of the most famous of Squier and Davis’s documented sites; Moorehead lived near it and was able to spend a considerable amount of time working there between 1886 and 1889.⁷⁶ In fact, Moorehead seems to have been able to gain access to the site when others were not permitted to, and he used his experience there to great personal advantage.⁷⁷ He published numerous accounts of his work, at the same time heavily promoting himself and his field methods.⁷⁸ In an August, 1889 letter to

⁷⁶ Moorehead reported to Putnam that he had spent one week working at Fort Ancient in 1886, ten days in 1887, two weeks in 1888, and thirteen weeks in 1889. Moorehead to Putnam, 26 August 1889, Harvard University Archives (HUA), HUG1717.2.1 Frederic Ward Putnam Papers, General Correspondence 1851-1947; 1881-1890; M-Z, Box 7, Folder M.

⁷⁷ There is no question that both Thomas and Putnam wanted to get at Fort Ancient, but, it seems, could not. One explanation is that the property owners were not allowing free access. Just a couple years earlier Putnam had arranged the purchase of Serpent Mound in the name of the Peabody Museum; in 1888, Fort Ancient (or, at least tracts of land containing substantial portions of it) went up for public sale. Metz had been monitoring the situation for Putnam, but in the end it was too expensive. According to Moorehead himself, both the Smithsonian and the “Cleveland Historical Society” had bid on Fort Ancient. See, Metz to Putnam, 15 March 1888, PM/AF 88-55-1, Folder “Expedition to Ohio-Serpent Mound,” and Moorehead to Putnam, 9 May 1888, HUA, HUG1717.2.1, Putnam papers, Box 7, Folder M.

⁷⁸ For example, see, Warren King Moorehead, “The Fort near Granville, Ohio,” The American Antiquarian and Oriental Journal 8 (1886), 297; “Grave at Fort Ancient,” The American Antiquarian and Oriental Journal 9 (1887), 295-300; “A Detailed Account of a Mound Opening, Done during the Months of July and August, 1888, by Warren K. Moorehead and Clinton Cowen,” Ohio State Archaeological and Historical Quarterly 2 (1889), 534-40; “Fort Ancient,” Journal of the Cincinnati Society of Natural History 12 (1889-90), 83-92; Fort Ancient, the Great Prehistoric Earthwork of Warren County, Ohio, Compiled from a Careful Survey, with an Account of its Mounds and Graves (Cincinnati: Robert Clarke, 1890); “Fort

Putnam, Moorehead described the work had been overseeing at Fort Ancient.⁷⁹ He described the methods used, the number of people he had working for him, and the fact that he was sending a large collection to Washington for temporary loan. Moorehead also casually mentioned that to date he had spent nearly four thousand dollar of his own money on archaeological work in Ohio. “So you see I mean ‘business,’” he concluded, with an obvious desire to be recognized as a serious archaeologist. By the end of the decade, Moorehead was a fixture of the Ohio archaeological scene and seemed to be well-connected to everyone. In 1892, he published a book entitled Primitive Man in Ohio—to which both Fowke and Cresson contributed chapters—and later he found a career track position as curator for the Ohio State Archaeological and Historical Society.

Correspondents were indispensable for Peabody and Smithsonian archaeology in the 1880s, and through these examples—Metz, Smith, Peet, Reynolds, Cresson, and Moorehead—we get a clear window onto the archaeological correspondent community and the sorts of activities that went on behind the scenes of “official” Smithsonian and Peabody work. Whatever grand designs Powell, Thomas, and Putnam may have had for promoting certain theoretical viewpoints about the mounds, moundbuilders, native Americans, and, indeed, about the very character of prehistory itself, very little fieldwork and data collection would have occurred without the participation of correspondents. As we have seen, correspondents were valuable not only for their various skills and abilities for conducting field explorations, but also for gaining access to properties, for acquiring specimens, and for generally being connected to the existing archaeological scene in

Ancient,” Proceedings of the American Association for the Advancement of Science for 1890, 384-88.

⁷⁹ Moorehead to Putnam, 26 August 1889, Harvard University Archives (HUA), HUG1717.2.1 Frederic Ward Putnam Papers, General Correspondence 1851-1947; 1881-1890; M-Z, Box 7, Folder M.

particular regions. We also get a good sense from the above examples of the extent to which various individuals' personal motivations, as well as competition between the Smithsonian and the Peabody, shaped the dynamics of centralization and the process of networking.

Of course, all the individuals discussed here were the “visible” correspondents within this context of centralization and archaeological networks. I also want to recognize that there were a vast number of people who were invisible help in the process, who were not acknowledged in published reports or even within the archival record. Supplemental field help was routinely part of excavation work, but it was done in a very piecemeal sort of way. Laborers were hired for a day here, or a day there. Friends and family members often pitched in as well. In general, there exists a somewhat better sense of this level of involvement with respect to the Peabody work—partly because of the more extensive and long-term nature of those excavations, and also simply because Putnam was quite good about giving some credit even in published writings. Farmers were routinely credited by name in Putnam's reports, and on occasion he at least made reference to the numbers of additional laborers involved. Also, as we shall see below, Putnam himself spent much more time in the field than Thomas, and in his notebooks and correspondence he mentions numerous individuals who participated in the project of centralization who otherwise might not have been remembered. At least as far as I have seen, there does not exist nearly the same level of notice of the invisible field help within the Bureau records.

5.6 Expositions.

Correspondents were critical links in the process of institutional centralization that took place in the 1880s, but they were not the only means by which networking took

place. It is no coincidence that the Smithsonian and Peabody projects of the 1880s came sandwiched between two major public events—the 1876 Philadelphia Centennial and 1893 Chicago World’s Columbian Expositions—in which anthropology and archaeology were heavily promoted by these institutions. These were monumental events in and of themselves, and an extended analysis of the impact and cultural significance of each lies beyond the scope of things here. Both events were important within the broader context of the “museum movement” of the late nineteenth century, a movement which itself should be situated within a context national identity formation that occurred in the Progressive-era United States.⁸⁰ Both exhibitions roused popular and avocational interest in archaeology to unprecedented levels, and both gave direction in some form or fashion to the archaeological activities of the Smithsonian and Peabody.

Smaller scale celebrations also played a role. The Centennial of Marietta, Ohio—the once outpost capital of the Northwest Territories—took place in 1888. The state’s archaeology was heavily promoted here by the Ohio State Archaeological and Historical Society as well as by invited participants such as Frederic Ward Putnam.

In 1876, the Smithsonian played something of a starring role within the sizeable

⁸⁰ Steven Conn, Museums and American Intellectual Life, 1876-1926 (Chicago: University of Chicago Press, 1998); Robert Rydell, All the World’s a Fair: Visions of Empire at American International Expositions, 1876-1916 (Chicago: University of Chicago Press, 1984); Robert Rydell, ed., Fair Representations: World’s Fairs and the Modern World (Amsterdam: Vrei Univeriteit Press, 1994); Bruno Giberti, Designing the Centennial: A History of the 1876 International Exhibition in Philadelphia (Lexington: University Press of Kentucky, 2002); Sally Gregory Kohlstedt, “‘Thoughts in Things:’ Modernity, History, and North American Museums,” Isis 96 (2005), 586-601; Lewis Arnold, An Early Encounter With Tomorrow: Europeans, Chicago’s Loop, and the World’s Columbian Exposition (Urbana: University of Illinois Press, 1997).

federal government sponsored portion of the Philadelphia Centennial Exposition. The anthropology and archaeology exhibits, mounted in association with the Interior Department, proved in hindsight to mark a stunningly successful turning point for Smithsonian anthropological initiatives overall. The Centennial strengthened the Institution's relationship with the federal government and resulted in government funds for the construction of a dedicated National Museum building. It resulted directly in a dramatic increase in the size of collections, including anthropology. The Exposition gave momentum to John Wesley Powell's association with the Smithsonian, and provided direct impetus for the formation of the Bureau of Ethnology. It was an immensely popular public event which brought considerable attention to Smithsonian anthropology and archaeology.⁸¹

Not coincidentally, the Centennial also served as a launching pad for other groups and individuals who were interested in the changing fortunes of archaeology as a visible form of scientific practice.⁸² For one thing, the nucleus of the Ohio State Archaeological and Historical Society, not formally incorporated until the mid-1880s, coalesced for the Exposition and mounted its own exhibit of Ohio archaeology.⁸³ Reading an account of

⁸¹ All of this may be tracked in the Secretary's reports of the Smithsonian Annual Reports for the years 1875-1880.

⁸² Historian Thomas Tax points out that the Philadelphia Centennial spurred a great deal of archaeological activity in many western states by groups such as the Ohio State Archaeological and Historical Society. See, Thomas Tax, The Development of American Archaeology, 1800-1879, diss., University of Chicago, 1973 (Ann Arbor: UMI, 1973. T24741), 302-3.

⁸³ See, for example, F.C. Sessions, "The History and Prospects of the [Ohio State Archaeological and Historical Society]," Ohio State Archaeological and Historical Quarterly, 1 (1887-8), 325-32.

the group's activities for the Centennial by its secretary F.C. Sessions leaves one with the distinct impression that its members wanted to mount an exhibit to rival the Smithsonian's, one that would not only have great public appeal in promoting Ohio's archaeological and historical heritage, but would also show the "big boys" at the Smithsonian that careful archaeological work was already being done in the state. A second important by-product of the Centennial resulted from an American Archaeological Convention that took place there. Out of this was formed the short-lived American Anthropological Association (not to be confused with the more recent and durable organization of the same name), with Stephen Peet as secretary. Peet started an Archaeological Exchange Club within the group that evidently led to his establishing The American Antiquarian and Oriental Journal.⁸⁴

On the other side of the 1880s stood the Chicago World's Columbian Exposition in 1893. For this event, Putnam oversaw a massive anthropological section that included North American archaeology within a framework of global scope, a framework modeled on the German *Kulturgeschichte* ideal.⁸⁵ Putnam has been credited with attempting to mount an exhibit of unprecedented scale illustrating both diachronic and synchronic aspects of the developmental history of mankind. Indeed, Putnam's ideals for arrangement and display became a major promotional feature for the anthropological exhibit. Harper's Weekly, for instance, lauded Putnam for attempting a new style of display that featured greater than usual amounts of open space and that illustrated human progress through "complete life exhibits" of each people rather than through serial

⁸⁴ Tax, Development, 303.

⁸⁵ Conn, Museums and American Intellectual Life, 75-82.

arrangements of specimens.⁸⁶ Some historians even point to Putnam's impact on the Columbian Exposition as a defining moment for modern American anthropology, since Putnam's program advertised what would later become enshrined as the four sub-disciplines of the field—linguistics, anatomy, ethnography, and archaeology. The event also resulted in the permanent construction of the Field Museum, and it boosted the career of one of Putnam's chief assistants in the endeavor, a brash young German immigrant named Franz Boas who would himself go on to have no inconsiderable impact on the shape of American anthropology.⁸⁷

If the Centennial gave a big push to Smithsonian archaeology, the Columbian exerted a great deal of pull upon the Peabody in the few years leading up to the event. Between 1890-3, most of Putnam's projects in general were devoted to building collections for the Exposition, and this included the Ohio valley work. The Anthropological Exhibit of the Chicago Exposition was a major undertaking for Putnam; combined with his ongoing Peabody and Harvard commitments, the Exposition seems to have marked a convenient point toward which Putnam decided to wrap up some of the ongoing field projects in

⁸⁶ Clipping from Harper's Weekly, Saturday April 30, 1892, PM/DR, Folder "Putnam Papers P.R.O. 1 World Columbian Exposition Notes 1892-3, n.d."

⁸⁷ Boas has achieved near mythical status in the historiography of anthropology. He is almost universally described as the father of the modern American discipline, and generally portrayed as the man who made a radical and revolutionary break from the speculative museum-based "evolutionist" anthropology of the nineteenth century and who insisted that cultures be viewed as relative, historically conditioned and highly contingent entities rather than as products of some universal process of human development. His feuds with museum anthropologists from the 1890s on have become the stuff of legend, as has his role in bringing anthropology into the university. Only recently have historians begun to explore in depth the nature of Boas's impact on anthropology.

Ohio.⁸⁸ After 1893, he did not devote as much attention to Ohio work.

The Centennial and World's Columbian Expositions were important events, but so too in its own way was another centennial celebration: the year 1888 marked the one hundredth anniversary of the founding of Marietta, Ohio, the once-frontier capital of the Northwest Territories. The Ohio celebration became an immense source of regional pride, and participants came from far and wide, not just Ohio. The Ohio State Archaeological and Historical Society played a central role in its organization and proceedings.⁸⁹ Considering that the organization's mission statement was to integrate the study of "prehistoric man...[with] with history of the past century and of today," a synthesis in which "[h]istory and archaeology should go hand in hand," it is not surprising that grand designs were made for events and exhibits that extolled the virtues of Ohio as a crucial part of both recent and ancient history.⁹⁰ By this time, Ohio had

⁸⁸ At some point during the 1890s, disputes surrounding the Madisonville Cemetery site began to emerge, issues which eventually snowballed after the turn of the century into litigation lasting two decades. This situation, too, may have played some role in Putnam's lessening of field activity in Ohio. See, PM/DR/FWP, "Mound Groups—Excavations," Box 1, Folders 1.6 and 1.7.

⁸⁹ "The Centennial Celebration at Marietta," Ohio Archaeological and Historical Quarterly 2 (no. 1, June, 1888), 1. Recall, too, that Powell, Thomas, Baird, and Putnam were all corresponding members of the OSAHS.

⁹⁰ [Proceedings of the Society for 1885], Ohio Archaeological and Historical Quarterly 1 (no. 1, June, 1887), 92. An examination of the rhetoric employed for presentations for this Marietta Centennial would make for a fascinating study in its own right. Many of the speakers involved in the event portrayed Ohio as a providential, chosen land in which economic progress meshed inextricably with the moral imperatives of a righteous Christian society. Printed versions of many of the speeches, orations, and addresses given during the course of the Marietta Centennial may be found in Ohio Archaeological and Historical Quarterly

become one of the most populous and economically productive states in the country, and organizers of the Marietta Centennial wanted it to be known. The state's rich archaeological heritage and its more recent traditions of avocational archaeological work ensured that both would be on prominent display at the 1888 celebration.

Putnam himself did much to promote archaeology at Marietta. His established track record of work in the state (made much more visible by the Peabody's recent purchase of Serpent Mound) and his ongoing promotion of archaeological preservation in the state, for instance, certainly put him in good stead to be an invited participant. So, too, did his very name. He was a distant relative of Massachusetts Revolutionary War hero General Rufus Putnam, one of the founding fathers of the Northwest Territories from which Ohio was the first state to be carved in 1803.⁹¹ More precisely, Rufus Putnam was one of the architects of the Ordinance of 1787 and a founder of the original Marietta outpost.⁹² After one hundred years, the General had become something of a mythical figure in Ohio history. His direct descendents still lived in Marietta, at least one of whom, Douglas, was both active within the OSAHS and president of the Washington County Pioneer Association. The Putnam name, in other words, was itself woven into the tapestry of

2 (no. 1, June 1888), 1-242.

⁹¹ Stephen Williams, *Fantastic Archaeology*, 29.

⁹² Senator George Hoar's lengthy oration all but deified Rufus Putnam for his prowess as a military engineer in the Revolutionary War and for forging his way into the new territory. "Oration of Hon. George F. Hoar," *Ohio Archaeological and Historical Quarterly* 2 (no. 1, June 1888), 15-46. I cannot help but wonder what thoughts drifted through the mind of ethnologist Frederic Ward Putnam when hearing of his distant relative's exploits—after all, the founding of Marietta decisively marked the beginning of the end for native American tribes remaining in the region.

Ohio history.⁹³ And so, during a series of preliminary addresses for the Marietta Centennial festivities, Frederic Ward Putnam shared a podium with such dignitaries as Massachusetts Senator George Hoar and former President Rutherford Hayes.⁹⁴ While I have not encountered any concrete evidence that Putnam's name indeed gave him a decisive advantage in Ohio over other archaeologists (such as those from the Bureau of Ethnology), the possibility is too suggestive to leave unacknowledged.

5.7 Institutional authorities reaching out.

Bureau and Peabody officials took other, more direct measures to facilitate centralization. Earlier, I pointed out that Powell, Baird (until his death in 1887), Thomas and Putnam each were corresponding members of the Ohio State Archaeological and Historical Society. We have also seen in the correspondent accounts above how it was important for the institutional figureheads to facilitate and maintain ties to other archaeological entities such as the Madisonville Literary and Historical Society or to Stephen Peet and his journal. Of course, it makes perfect sense that Thomas and Putnam would seek information and assistance from those who were already doing archaeological

⁹³ The Putnam name, in fact, ran deep and wide throughout the mid-west. Not only did Frederic Ward Putnam have familial ties to Ohio, he also had distant relatives in Iowa as well, some of whom were involved in the Davenport Academy of Natural Sciences. See Stephen Williams, *Fantastic Archaeology*, p. 29, and David L. Browman, "Frederic Ward Putnam: Contributions to the Development of Archaeological Institutions and Encouragement of Women Practitioners," in David L. Browman and Stephen Williams, ed., *New Perspectives on the Origins of Americanist Archaeology* (Tuscaloosa: University of Alabama Press, 2002), pp. 240-1 [207-241].

⁹⁴ "The Centennial Celebration at Marietta," *Ohio Archaeological and Historical Quarterly* 2 (no. 1, June, 1888), 3.

work in some capacity in those regions where they themselves wished to conduct explorations. One thing I did not mention yet was the extent to which Smithsonian and Peabody authorities made further use of local newspapers not only to publicize their archaeological doings, but also to give advance notice of field operations so that locals might come forward and help out in any way possible.

It is worth quoting at length one such circular that was the result of an exchange between Cyrus Thomas and Matthew Read. Read was an Ohio geologist and archaeologist and one of the trustees of the nascent Ohio State Archaeological and Historical Society.⁹⁵ He was also at times a Bureau agent. By agreement with the Ohio Archaeological Society executive, the circular was distributed throughout many of the state's newspapers. Printed with the by-line "Mound Builders of Ohio: Important Notice," the piece then read:

The pre-historic remains of Ohio are becoming rapidly obliterated by the plow, and soon an accurate description of them will be impossible. Explorations are often made of sepulchral mounds and other works by persons only partially qualified, and objects of interest found in them are kept for a time as curiosities and finally lost. These relics are of little scientific value except when gathered into large collections, and when the precise manner and place of their deposit are accurately preserved.

Major Powell, in charge of the U.S. Bureau of Ethnology, has authorized a thorough and scientific exploration and survey of these works, without expense to the archaeologists of the State, only asking their cooperation and the aid of land owners in the way of permission to make thorough explorations.

All persons in the State having knowledge of pre-historic earthworks, fortifications, mounds or burial places in their neighborhood, are earnestly requested to send some one of the undersigned a statement of their character and location, and when the places are visited by the employees [sic] of the Ethnological Bureau to aid them in their work, and to secure the permission of land-owners to the thorough exploration of all works found on their premises. All articles found in the explorations will be deposited in the museum of the Smithsonian Institute,

⁹⁵ See, for instance, Read to Thomas, 19 May 1884 and 1 July 1884, NAA/DM/ER, MS 2400, Box 6, Folder "M.C. Read 1884."

becoming the property of the United States, and accessible without expense to all students of American ethnology. Facsimile casts of all specially interesting finds will be deposited in some central museum of the State of Ohio.

Editors of all newspapers in the State are especially requested to give this circular a place in their columns, and to call editorial notice to it. By doing so they will greatly aid an important work, and help secure a long desired exploration with the ultimate publication of results. Work will be commenced in July.⁹⁶

Printed beneath the piece were the names of twenty two Ohio residents and avocational archaeologists (almost all were associated with the OSAHS), along with their towns of abode.

The overall tone of the circular is certainly consistent with other such proclamations made by Thomas: We are the government professionals, we know what we are doing, and you have no choice but to assist us.⁹⁷ Needless to say, this adamant insistence on removing Ohio archaeological materials to Washington was not uniformly well-received within the Ohio archaeological community, but at the time no group there possessed the resources to do what, in principle, the Bureau of Ethnology was proposing to do with their survey. On top of that, the Bureau had a place to put all the recovered archaeological materials—the recently expanded Smithsonian National Museum. Also, as we can see

⁹⁶ Newspaper clipping from Cincinnati Commercial Gazette, n.d. but likely 1 July 1884, NAA/DM/ER, MS 2400, Box 6, Folder, “M.C. Read 1884.” Name of paper and date of publication verified through accompanying correspondence of M.C. (Matthew Canfield) Read to Thomas, 1 July 1884.

⁹⁷ See, for instance, George F. Wright, “The Importance of the Study of Archaeology in Ohio,” Ohio Archaeological and Historical Quarterly 1 (1887-8), 54-9. Wright was a prominent Ohio geologist and central member of the Ohio State Archaeological and Historical Society, and here addressed to some extent the status of Smithsonian and Peabody work being done in Ohio. In the article, Wright reprinted letters from both Putnam and Thomas. Thomas announced, “You can say to your society that the mystery of the mounds is being solved,” and may as well have added, “but not by you amateurs” given the prevailing tone of the piece.

from this particular circular, quite a lot was expected from the good people of Ohio in assisting the Bureau in their project. If the work was without expense to the archaeologists of the state, it was because nobody there had the money to finance such a large-scale, coordinated project themselves.

The Bureau use of the Ohio press was effective. There is no question that it successfully encouraged assistance and participation in the project. As Bureau agents worked their way through particular areas, the press coverage was at least partially responsible for securing field laborers for excavations, for enticing private collectors or those simply in possession of interesting specimens to come forth and perhaps offer them to the Smithsonian, and for opening channels with land owners in gaining access to properties.

For his part, Putnam made similarly effective use of the press, although he was perhaps a bit more diplomatic in going about it. As Putnam and Metz began expanding the range of Peabody fieldwork in the Ohio by the middle of the decade, the Putnam became increasingly vocal in his calls for the preservation of the state's mounds and earthworks, first through his Peabody Museum reports and then through letters that would were distributed and reprinted in numerous newspapers from Boston to Ohio.⁹⁸

⁹⁸ For instance, the Boston Globe regularly printed letters by Putnam on the state of his efforts at Serpent Mound in southern Ohio, as did the Cincinnati Post. Interestingly, the reason why I even know about many of these sorts of newspaper articles in the first place is because clippings of many of them have been preserved in the archives of both the Peabody and, particularly, the Smithsonian. But they were not preserved solely for posterity's sake: I found many of the clippings pertaining to Putnam's work at the Smithsonian archives, and there is a very distinct sense that Thomas and others at the Bureau were saving them as a means of keeping tabs on Putnam's doings. NAA/Department of Anthropology/Division of

These letters generally gave some account of the state of destruction, vandalism, and unscrupulous relic-hunting going on with the Ohio works. Putnam usually followed this with a detailed account of some recent Peabody explorations, taking pains to describe both the results of these explorations and how the work was done.

Putnam purchased Serpent Mound in 1887 and the complicated proceedings of this transaction are discussed at great length in the next chapter. Needless to say, it was widely regarded as a great archaeological victory. Here was an institutional authority from the east taking great strides to keep Ohio's ancient monuments intact and in place. This approach was very different that of the Bureau of Ethnology. For the next two years, Putnam used the press to keep the Ohio public well aware of his ongoing work at the unusual earthwork site in the south-central part of the state, and also of his plans to turn it right away into a public park. Nothing like this had been done in the United States before. Serpent Mound became something of a rallying point for Putnam in his appeals to save the ancient works of Ohio, a successful example to which he could point in saying preservation of this sort was at least possible.

Publicly accessible print sources such as newspapers were useful tools for both Thomas and Putnam in their respective campaigns to gather archaeological data, but there was at least one other important way for them to actively facilitate the process—to go into the field themselves. Sometimes, it was to monitor or participate in excavation work, or merely to witness a site in its natural setting. But keep in mind here, too, that in the context of the push toward centralization, fieldwork encompassed more than site surveys and excavations. Equally important were efforts to make physical contact with

landowners, collectors and others in order to arrange for site access or to inspect and possibly purchase specimens and collections. Granted, a considerable amount of this sort of work was done by correspondents who were often better situated through experience to make personal contacts, but the presence of institutional authorities in the field certainly made an impact. This was particularly so in Putnam's case.

I am not going to comment at length about Thomas's trips into the field. From what I have gathered from the archival record, Thomas's efforts along these lines were straightforward and actually rather unexceptional. Letters to him from archaeological correspondents are spotted with references to occasional field inspections by him. But, by all appearances, the mound survey director preferred not to get his hands and boots too muddy. We also know from the published record that Thomas simply did not directly supervise field excavations or survey work on any long-term basis. His publications are not first-hand accounts of archaeological research, but rather synthetic analyses of the work done by his field agents.⁹⁹ I have no doubt that Thomas did do some networking during his excursions. As often as not, I imagine it was rather inevitable that local enthusiasts who had found out about a Bureau archaeologist's presence might have

⁹⁹ Hinsley discusses the power structures of Smithsonian science that were certainly characteristic of the Bureau of Ethnology's linguistic and ethnographic work in the 1880s and 1890s. According to Hinsley, the attitude within the Bureau was that two classes of work existed: the "lower" empirical work of data collection and the "higher" work of synthesis. This model certainly fits the activities within the Mound Exploring Division, where Powell had bestowed unto Thomas the status of synthesizer. Whether Thomas actually had the requisite archaeological experience to be a good synthesizer—or whether he was even a good archaeologist—was beside the point. See, Hinsley, *The Smithsonian and the American Indian*, chapter 6.

sought him out. Nonetheless, Thomas did not spend nearly as much time in the field as Putnam did. Thomas's strengths as an archaeologist were purely administrative; he was a man much more content to direct from afar the complex movements of his field agents. Given the goals and the scale of the Bureau survey, such careful orchestration of field activities was essential to its success.

Putnam, on the other hand, thrived in the field, and during the 1880s he was never happier than when he was in southern Ohio. For a man of his administrative capacity, Putnam spent a lot of time there. He was often directly engaged in excavations with Metz and others, living on site for weeks on end in canvas tent camps. Sometimes his wife and daughter joined along. In the next chapter, I will examine in considerable detail Putnam's extensive work specifically at Serpent Mound, the site that came to be his greatest archaeological pride and joy, and we will get a better sense of what this field life was like.

Putnam was also quite successful when it came to the networking aspects of fieldwork. He undertook numerous excursions in which he would scout out sites or meet with collectors, or both. These reconnaissance trips—for lack of a better label—were sometimes physically draining affairs in which Putnam might rapidly cover a considerable amount of terrain and have dealings with numerous individuals along the way.

At the end of May, 1887, Putnam was recuperating in a Cincinnati guest-house after one such excursion made with his friends and archaeological associates Charles Metz and Charles Low. The three men had made their way by steamer some sixty or so miles up the Ohio River to the town of Maysville, on the Kentucky side, and from there nearly just

as far again upriver to Portsmouth, Ohio. In a long letter to his wife Esther, Putnam described the outing. It is worth quoting at length for the vivid picture it gives us of what he did when he went out into the field. “My darling little wife,” Putnam began,

...I had a chance to see the mouth of my dear Little Miami & the valley through which it flows from the “Sand Ridge” where we have been exploring. We also saw the mouth of Brush Creek, which is the stream that I have so largely invested in of late. There are earthworks & mounds on that stream, besides the Serpent & Fort Hill.¹⁰⁰

Putnam next recounted arriving at Maysville, whereupon he and his companions were shown “several collections of stone implements” the following morning in the town library. “During the eve,” he continued,

we called upon a Dr. who had a nice collection of things [artifacts] which he showed to us, but he was as drunk as a fiddler. His natural hospitality stood out however & we saw all he had. He turned out to be a [former medical school] classmate of Metz’s. Had he not been [high?] he would have been a very pleasant fellow. He has promised to send me a lot of things & those who know him best say he will do it.

The next day, Putnam, Metz and Low continued on up to Portsmouth aboard a second steamboat. Once there, they examined and sketched some surviving portions of what was known as the Portsmouth Works. This apparently interconnected series of earthworks and mounds had been famously documented by Squier and Davis, who described it as occupying the terraces on either side of an eight-mile stretch of the Ohio River. Little now remained. During the visit, a Kentucky-side landowner granted Putnam permission to explore one of the extant conical mounds. “If I can ever get the chance to explore it, it is at my service,” Putnam wrote, adding, “It has never been dug into.” He continued, “It is a strange condition of things—the works were surveyed many years ago & most of

¹⁰⁰ F.W. Putnam to Esther Orme Clarke Putnam, 30 May 1887, HUA, Unit HUG1717.2.1 Putnam Papers, General Correspondence 1851-1947 (1881-1890), Box 7 (“M-Z”), Folder “P.”

them on the Ohio side have been destroyed by the town of Portsmouth, but a good part on the Kentucky side are still preserved.” Not surprisingly, Putnam, Metz, and Low spent some time examining the situation there.

Well we walked & walked in the hot sun & I had about 20 lbs of rocks, specimens to carry all the way as well as my overcoat & bag. Finally, we reached old man King’s house on the Tygart River...& he was full of information about the works & what they were for. He is a hale old gentleman of 84 years & has lived all his life there. He had a lot of specimens he had collected & told me to help myself for the museum. So I picked out all we could carry with us & we started on at 7 o’clock [the next morning]. We reached the river bank, expecting to signal down a boat; after waiting awhile we found a native who said the boat was often as late as 11 or 12....Luckily, there was a man in a boat fishing & we hailed him. For a dollar he took us across & 3 miles down to Portsmouth. Then we got supper at the hotel, I packed up all my specimens & sent them on by express.

The following day, the three tired companions drifted back to Cincinnati.

Far from standing as an amusing anecdote, Putnam’s account provides a detailed picture of what he routinely did in Ohio. I cannot emphasize strongly enough that this sort of reconnaissance and information gathering fieldwork was every bit as essential to the process of institutional centralization as site excavations. This account also highlights some aspects of Putnam’s character that are worth mentioning. He enjoyed doing this kind of work. In fact, he did not see it as work at all, and, although he took it seriously, it was an escape for him from the pressing administrative life of Cambridge. Putnam was also a genuinely affable person who seemed to be able to get along with anyone. He especially valued the wisdom and memory of seniors such as the aforementioned “old man King,” people who often possessed a life-time’s worth of gathered specimens. My intent here is not to portray Putnam in glowing terms, but only to demonstrate that his interpersonal skills were valuable assets in the context of centralization.

Putnam was also careful to reciprocate for the help he received, either by a straight

exchange of money for collections, or just as often through other sorts of favors. His relationship with the Lovett family, who owned and farmed the land on which Serpent Mound was located, is a good case in point. From the time of Putnam's first visit to the remote Adams County site in the late summer of 1883 right up through the start of Peabody field explorations in 1887, he maintained a friendly relationship with them. Well before he had concocted any plans for purchasing the land, Putnam made sure to send copies of Peabody Reports to John Lovett by way of thanks for his hospitality, even though Lovett himself seems only to have had a lukewarm interest in archaeology. He did the same for one William Miller, a local who served as something of a guide for the area, and who may have arranged the initial visit with the Lovetts.¹⁰¹ In exchange for occasional lodgings and meals, Putnam would also bring gifts—a cream pitcher for Mrs. Lovett, books for her young daughters.¹⁰² These were as much matters of common courtesy as anything else, really, but such decency and tact served Putnam well throughout his Ohio work.

To be fair, Thomas probably did occasionally engage in similar sorts of interactions, but it should be said that I have seen no evidence to suggest that Thomas possessed the same vigor and enthusiasm for such dealings that Putnam did. If anything, Thomas by comparison was rigidly formal and rather terse in all his exchanges. Perhaps it was due to the military background and the pressures of working under Powell, but Thomas simply did not come across in his various correspondence as a very friendly—let alone

¹⁰¹ F.W. Putnam, "Notebook E," PM/AF 88-55, Folder 3.

¹⁰² F.W. Putnam to Esther Orme Clarke Putnam, 30 May 1887, HUA, Unit HUG1717.2.1 Putnam Papers, General Correspondence 1851-1947 (1881-1890), Box 7 ("M-Z"), Folder "P."

passionate—sort of man.¹⁰³ He and Putnam were very different from one another.

5.8 Relic boom.

By around 1891, the intense flurry of archaeological activity on the mounds I have been talking about here came to an end. Over the course of the preceding decade, the Peabody Museum and the Smithsonian Institution's Bureau of Ethnology had exerted an immense influence upon the practice and organization of Ohio valley archaeology, the likes of which had not been seen before. Between the two of them, Putnam and Thomas did much to dispel the simplistic moundbuilder separate race theories of an earlier era. As we know, however, they upheld very different theoretical perspectives on the character of New World prehistory. And yet, contemporaries such as soon-to-be Smithsonian National Museum Curator of Ethnology Otis Mason could point to a "great change of opinion" about moundbuilders. According to Mason, "The chasm, therefore, so far as culture is concerned, between the two peoples, the Mound-Builders and the modern Indians, has been bridged over by Thomas, Putnam and others."¹⁰⁴ The grand ethnological problem, it would seem, had at last been solved. Or had it? What kind of consensus had been reached? The Peabody and Bureau of Ethnology models of prehistory were not just different. They were essentially incompatible with one another. This is an important point since historians of American archaeology have uniformly positioned Thomas's 740-page

¹⁰³ Here is just one telling instance of Thomas's character. On the sudden death of long-standing Bureau field agent Henry Lee Reynolds, Thomas expressed severe disappointment—not because a colleague whom he knew well had died, but because he was expecting much more work from him. See, Thomas to H.W. Henshaw, 20 April 1890, NAA/DM/ER 1881-1889, MS 2400, Boz 9, Folder "1894-1896."

¹⁰⁴ Otis Mason, "The Mound-builders," *American Naturalist* 18 (1884), 745.

1894 “Report of the Mound Explorations of the Bureau of Ethnology” as the final nail in the moundbuilder-separate race theory coffin.¹⁰⁵ This report, more so than any of Putnam’s work of the period, has been characterized as a critical step in the ascent of North American anthropological theory. Obviously, there are significant problems with this view.

It is much more instructive to think about the history of Peabody and Smithsonian mound work also in terms of large-scale centralization rather than only as a narrative of theory development. Centralization helps make better sense of the broad range of events and activities that took place. It accounts for the amassing of collections and museum-building that took place at both centers. It makes sense of both Putnam’s and Thomas’s across-the-board efforts to standardize methods. Centralization explains the use of correspondents at a time when the institutions simply did not possess enough core staff to undertake work on such a scale. It also makes sense of seemingly anomalous elements in the process as well, such as the involvement of individuals whose interests at times ran counter to those of the institutional authorities. Putnam and Thomas each cultivated

¹⁰⁵ See, for example: Bruce Trigger, A History of Archaeological Thought (Cambridge: Cambridge University Press, 1995), pp. 125-6; Chris Gosden, Anthropology and Archaeology: A Changing Relationship (London: Routledge, 1999), pp. 54-5; William H. Stiebing, Jr., Uncovering the Past: A History of Archaeology (New York: Oxford University Press, 1995), p. 193; Stephen Williams, Fantastic Archaeology: The Wild Side of North American Prehistory (Philadelphia: University of Pennsylvania Press, 1991), pp. 67-8; Gordon Willey and Jeremy Sabloff, A History of American Archaeology (San Francisco: W.H. Freeman, 1980), p. 42; and Robert Silverburg, Moundbuilders of Ancient America (Greenwich: New York Graphic Society, 1968). The claim is also repeated in textbooks of archaeology and anthropology, such as Stuart J. Fiedel, Prehistory of the Americas (Cambridge: Cambridge University Press, 1992), p. 4.

networks of correspondents and others in order to access and acquire archaeological data, but these networks were made up of individuals often operating under their own motives. Thinking of centralization as a fundamental process of late nineteenth century archaeology also helps us understand the simultaneous cooperation and competition that occurred between the Peabody and Smithsonian. Centralization was not an inherently smooth process.

It was also not uniformly perceived in positive terms, and Stephen Peet's criticism of the Bureau mound survey was a good demonstration of this. Peet, who held considerable status within archaeological circles, saw the early Bureau work strictly as a rush to build collections. But it is simply not true that Thomas and others at the Smithsonian were exclusively interested in relic-hunting. From the start, Thomas wanted his field correspondents to supply accompanying documentary information. To what extent they did is a worthwhile question that can only be answered on a case by case basis, and the same applies to correspondents for the Peabody Museum as well.

None of this is to say, however, that Peet's critique was without good basis. For one thing, Bureau field agents such as Norris and Palmer certainly did not help appearances—by all indications, in fact, these two men in particular appear to have been voracious collectors. But it also true that the mass acquisition of specimens was an absolutely essential component of Peabody and Smithsonian centralization. It was important for building both research collections and, as a result, status as research centers. That said, authorities at both institutions were truly vexed by the wholesale dispersion and loss of data that was occurring. Cast in this light, it is perhaps somewhat understandable that those authorities did not need to think too long and hard when faced with the choice of

accepting specimens obtained under questionable circumstances, or not to accept them at all.

Obviously, there are aspects to the dynamics of centralization that need to be explored in more depth than I have done here. First, the tension between regional and national archaeological interests was substantial. Peet, for instance, saw himself as a figurehead (if not *the* figurehead) of midwestern archaeological circles, and while in principal he supported the move toward rigorous, tightly controlled institutional archaeology, he was nonetheless apprehensive about seeing Ohio valley archaeology being removed from the Ohio valley.

Secondly, the individual use of specimen collections as forms of capital was an immensely important part of the story and should be situated more definitively within the context of what historian Mark Barrow identifies as the post-Civil War craze for entrepreneurial natural history.¹⁰⁶ As the Peabody and Smithsonian became increasingly involved in not just Ohio valley archaeology but anthropology in general, desirable specimens and collections became more valuable in purely monetary terms. And so a whole spectrum of individuals emerged looking to capitalize on this—from farmers looking to make money by selling off choice relics to a few enterprising individuals who made good livings as specimen dealers. During the 1880s, Putnam, Powell, Thomas and even Baird were inundated with letters from such individuals looking to sell Ohio valley materials. They came out of the woodwork, knowing that these items were desirable and valuable. And they often knew the right words to say, too, such as that due to the amount

¹⁰⁶ Mark V. Barrow, Jr., “The Specimen Dealer: Entrepreneurial Natural History in America’s Gilded Age,” *Journal of the History of Biology* 33 (2000), pp. 493-534.

of work spent in collecting the items, they simply could not afford to donate them, or vaguely threatening that they would prefer to see the items go to an American institution rather than one abroad. Furthermore, specimen collections could serve as a different kind of capital for at least some individuals looking to gain entrance to the Peabody or Smithsonian correspondent network. Metz and Smith each maintained personal cabinets, and they did so at the same time that they were working for the Peabody or Bureau, respectively. I think that there was a real sense—especially for someone like Smith—that these personal collections served as a sort of c.v., an indication of experience and time spent in the field. They also may have served as a sort of job security, too: get rid of Smith, and you lose his collection, possibly to the Peabody. As I said, these issues deserve to be examined further. My main goal in this chapter has been mainly to unravel the inherent messiness of the centralization process within which correspondents played a such a prominent role.

Perhaps the best way to conclude this chapter is simply to point out the sadly ironic side of the Peabody and Smithsonian archaeological work in the 1880s. In promoting centralization, both Putnam and Thomas may indeed have rigorized practice among a certain set of individuals, but they also inadvertently promoted the continuation of unscrupulous relic-hunting and looting. Peabody correspondent Hilborne Cresson, for one, became deeply worried about the state of affairs. In regard to an available collection of large-sized copper implements, he wrote to Putnam from Philadelphia in December of 1890:

You must use every endeavor to get Jones his money before February the fifteenth or else, I am afraid we shall lose one of the finest collections of the kind in existence. That unfortunate price paid by the Smithsonian Institution, several months ago, for a whole lot of small coppers has made collectors open their

eyes—\$7000 for 500 specimens big and little. As you will know big coppers are few.¹⁰⁷

By late August of the very next year, Cresson was immensely relieved to be in Ohio where he was recuperating from a grueling reconnaissance trip out west through the Colorado region. Cresson limped back from the west both injured and disillusioned, having encountered numerous parties—both white men from the east and Ute Indians alike—who were engaged in what he described as astonishingly unethical archaeological practices. In a long letter to Putnam, Cresson detailed instances of falsified pedigrees for artifacts and of the careless and often violent “excavation” of sites simply to get hold of specimens to sell. He strongly suspected, in fact, that dynamite had been used in a number of cases to blow open sites. Cresson painted a rather shocking picture of archaeology in the west, going so far as to suggest that only a well-armed party would be capable of doing good archaeological work there. In sadness, he admitted that much of this behavior was being fueled by the World Columbian Exposition, and warned Putnam that western speculators were going to show up at the fair in hopes of financial gain. He implored Putnam to simply wait until the “relic boom” was over before dealing with these people. Cresson knew the potential impact his words could have.

Please do not think that I am making any insinuations against your section of the Fair...on the contrary, I am its most sincere well wisher but it breaks my heart to see the way that ruins have been “ripped” out and graveyards torn asunder.... Please do not quote me in any of these charges as it would only interfere with future work and make enemies.¹⁰⁸

Departing the west for Ohio must have seemed a complete return to civilization, in the

¹⁰⁷ Cresson to Putnam, 29 December 1890, PM/DR/FWP 89-15, Box 11 “1891 A-F,” Folder “UAV 677.38.”

¹⁰⁸ Cresson to Putnam, 24 August 1891, PM/DR/FWP 89-15, Box 11 “1891 A-F,” Folder “UAV 677.38.”

full nineteenth century sense of the term. But the effects of the “relic boom” were felt there, too, though not as violently.

Chapter 6.

“The trail of the Serpent is over them all:”¹

Archaeological investigations of Serpent Mound, Ohio.

6.1 Introduction.

This chapter is about the archaeological investigations and interpretations of one particular mound site—the “Great Serpent” of Adams County, Ohio—that occurred in the nineteenth century. As one of the most famous of the spectacular Ohio valley earthworks described by Squier and Davis in their 1848 Ancient Monuments of the Mississippi Valley, the Serpent became an object of interest for ethnologists who were particularly interested in the study of primitive religion and mythology. It was situated atop a bluff on an out of the way farm in the rural south-central Ohio hinterlands, which perhaps partly explains why, following Squier and Davis’s survey, no further archaeological fieldwork was done there until the early 1880s. At this time, a number of archaeologists became interested in examining and interpreting the site. This flurry of activity culminated in 1887, when Frederic Ward Putnam, the curator and director of the Peabody Museum of American Archaeology and Ethnology in Cambridge, Massachusetts, arranged purchase of Serpent Mound in order to preserve it as a park. Putnam, with much assistance, then spent several field seasons conducting detailed excavations, the sum total of which

¹ The quote that comes from an undated watercolor painting, made on a three by five inch card, found tucked into one of the folders of documents pertaining to Putnam’s work at Serpent Mound ca. 1887-1889. The image depicts a grinning serpent with its head raised, slithering toward the viewer. A jagged mountain range rises up in the distance, and the words “The trail of the Serpent is over them all” are inked onto the card. Artist/author unknown, n.d., Accession file 88-55-#2, “Expedition to Ohio—Serpent Mound,” (Peabody Museum Archives, Cambridge, Massachusetts).



Figure 6.1. Aerial view of Serpent Mound, circa 1930. Source: Robert C. Glotzhober and Bradley T. Leper, Serpent Mound: Ohio's Enigmatic Effigy Mound (Columbus, OH: Ohio Historical Society, 1982), 2-3.

yielded an exceptionally complex and rich understanding of the both the site itself and of “moundbuilders” in general that stood in stark contrast to other views. In order to demonstrate how Putnam reached these conclusions, I will provide a close examination of his Serpent Mound fieldwork, and will consider a number of methodological issues pertaining to that work.

6.2 Of serpents and sacred enclosures.

Because of its somewhat remote upland location, Serpent Mound went relatively unknown in the early part of the nineteenth century, unlike many other elaborate earthwork sites located along the lower terraces of large waterways.² In the 1820s, the site was located on farmland owned by William Hamilton.³ It was situated atop an unusual crescent-shaped spur of land some one hundred and fifty feet above a gentle bend in Brush Creek, a modest tributary of the Ohio River. The actual ridge-top immediately surrounding the earthwork was heavily forested in small timber at the time and not under cultivation. At least some people outside the Hamilton family knew about the Serpent—a young local man named William Miller, for instance, had spotted the mound on a hike

² The first attempt at a thorough survey of Ohio valley mound sites was commissioned by the American Antiquarian Society in 1820 and was conducted by Circleville, Ohio’s postmaster Caleb Atwater. There is no question that the Serpent would have appeared in this study had he known about the site. Caleb Atwater, “Description of the Antiquities of the State of Ohio and Other Western States,” Archaeologia Americana: Transactions and Collections of the American Antiquarian Society 1 (1820), 105-267.

³ Sarah DeWitt Dunlap to F. W. Putnam, 23 December 1889, Peabody Museum Accession Files (PM/AF), Unit 88-55, Folder 1. William Hamilton was Dunlap’s maternal grandfather and she knew that he owned the farm at the time of her parents’ marriage in 1820.

through the area, and was impressed enough to have made a sketch and composed an essay on it comparing it to the Biblical “serpent in the wilderness.”⁴ Nonetheless, the existence of Serpent Mound did not become a matter of common knowledge, even among archaeological hobbyists, until 1846, the year that Ephraim Squier and Edwin Davis learned about unusual site and made a survey of it as part of a much more extensive survey of Ohio and Mississippi Valley mounds and earthworks.

The resultant map and description of the work, dubbed the “Great Serpent of Adams County,” appeared in Squier and Davis’s 1848 Ancient Monuments of the Mississippi Valley, published as the flagship inaugural volume of the fledgling Smithsonian Institution’s Contributions to Knowledge series. As I discussed in Chapter 2, Squier and Davis presented in this work a picture of the “moundbuilders” as an ancient and sophisticated culturally homogeneous population whose productions were not being replicated in more recent times. The strongest impact of Ancient Monuments, I would argue, came from the work’s highly visual nature. The otherworldly plan view maps in particular—which were drawn by Squier well after the completion of fieldwork—depicted hyper-delineated earthwork structures sprawling over alien landscapes. Squier’s map of the Serpent (see Figure 6.2) is an especially good example, where the effigy is shown in exaggerated contrast to an unnaturally flat and mesa-like ridge-top.⁵

⁴ William Miller to F.W. Putnam, 21 October 1889, Peabody Museum, Director Records of F.W. Putnam (PM/DR/FWP) 89-15, Folder 2. Miller had saved his sketch and essay until late in life when he wrote to Putnam.

⁵ Squier drafted the maps based on field sketches and minimal survey data—possibly using only a pocket compass—some time after he and Davis visited the site in 1846. See Chapter 2.

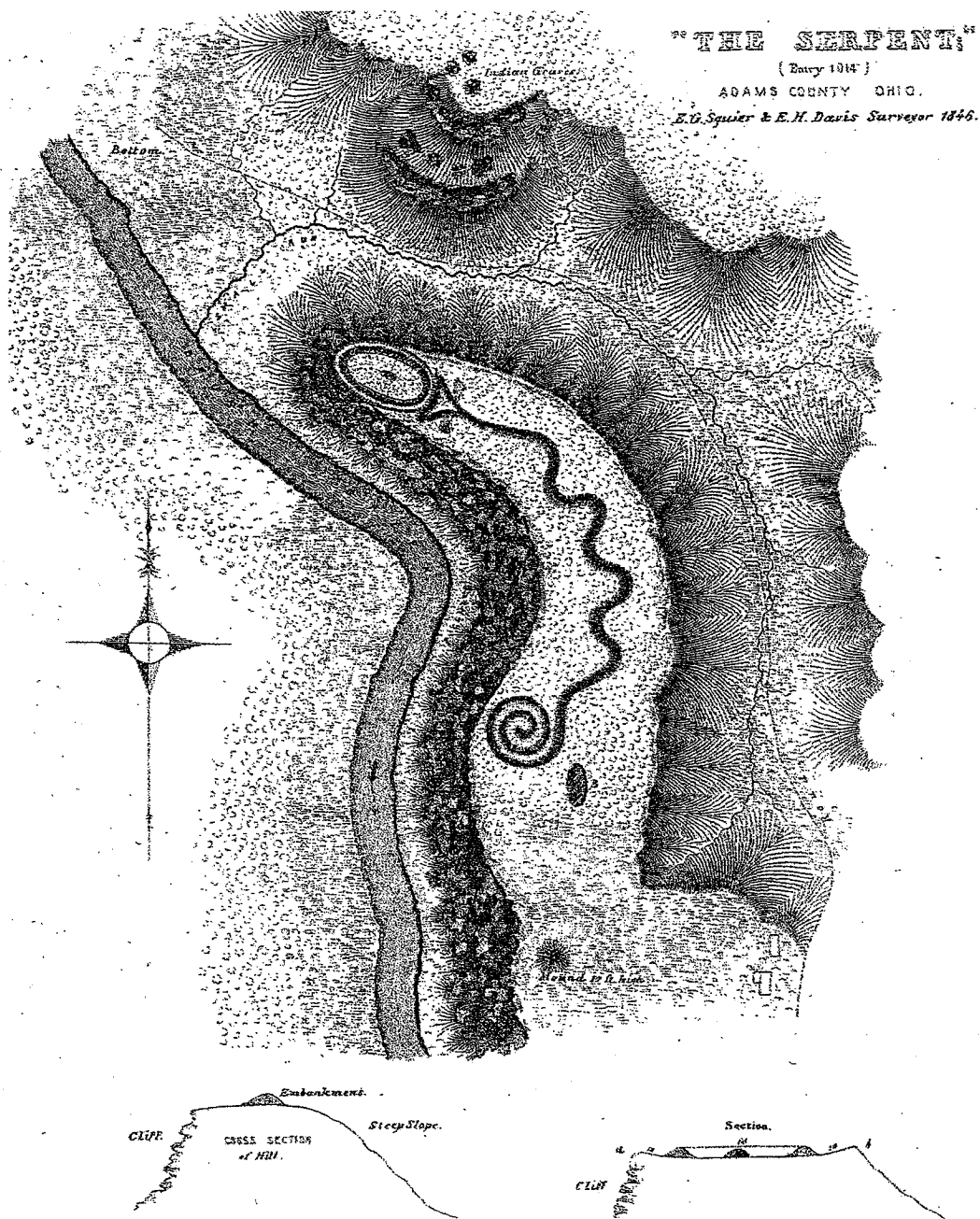


Figure 6.2. Plan view of "The Serpent" according to Squier and Davis. Source: Ephraim Squier and Edwin Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press, 1998 [1848]), plate between pp. 96-7.

What did Squier and Davis see in this site? First, Squier's map depicts a serpent "as if in the act of swallowing or ejecting an oval figure, which rests partially within distended jaws."⁶ It was the serpent and the egg—a compound symbol prominent "among many primitive nations" from Egypt to China to Mexico.⁷ The authors also documented small triangular elevations protruding outward from the base of the serpent's "neck," which were by their own admission hard to delineate accurately. A scattered pile of charred stones is shown at the dead center of the oval enclosure, taken to be the remnants of an "altar."⁸ Squier and Davis classified the work as a "sacred enclosure," and concluded their discussion with the following fateful words:

It is impossible [given the current state of ethnological knowledge] to trace the analogies which the Ohio structure exhibits to the serpent temples of England [among other examples], or to point out the extent to which the symbol was applied in America,—an investigation fraught with the greatest interest both in respect to the light which it reflects upon the primitive superstitions of remotely separated people, and especially upon the origin of the American race.⁹

The Ohio Serpent was for Squier and Davis more than an elaborate archaeological curiosity. It was an especially revealing manifestation of a primitive human impulse toward religious expression, and thus held immense heuristic potential for comparative ethnological studies along these lines. Why should serpent symbolism be found among the cultural productions of so many "remotely separated people"? Either the trait shared common origins and had spread via migration and diffusion, or it emerged independently of any other context. If the latter were true, one possible explanation was that human

⁶ Squier and Davis, *Ancient Monuments*, 97.

⁷ Squier and Davis, *Ancient Monuments*, 97.

⁸ Squier and Davis, *Ancient Monuments*, 101.

⁹ Squier and Davis, *Ancient Monuments*, 98.

groups—regardless of origins—possessed a tendency to develop belief systems along certain necessary and pre-ordained lines. Given the possibilities, the “American race” as a whole thus either brought the compound symbol with them into the New World, or else the symbol emerged as a stage of development among an autochthonous New World people. In this light, and in these antebellum years of American ethnology, the Ohio Serpent—viewed as an analyzable component of primitive religious systems—promised to contribute to debates over monogenism and polygenism.

Squier and Davis’s classification of the Serpent as a “sacred enclosure” was significant in and of itself, and reflected certain commonly maintained *a priori* beliefs about the moundbuilders. Broadly speaking, the many irregularly-shaped hilltop earthworks were thought to have military functions, since, it was thought, the moundbuilders frequently needed to defend themselves against onslaughts of less civilized peoples. Other works, the sacred enclosures, were more geometrically regular in appearance and were generally assumed to have ceremonial or religious functions. If not for war, then for worship, or so the thinking went. In Ancient Monuments we find the authors speculating on a “government of the priesthood” capable of evoking widespread “devotional fervor and superstitious zeal” for large scale constructions and for maintaining order in what seemed to be a relatively highly developed society.¹⁰ With a few notable exceptions, most of the spectacular sacred enclosures were in Ohio. Serpent Mound, with its commanding ridge-top setting, appeared to be the only one to incorporate the additional representational element of an effigy, lending to it that much more apparent significance.

¹⁰ Squier and Davis, Ancient Monuments, 47 and 49.

In terms of what they sought to document at the site, Squier and Davis were most interested in accurately delineating and describing the form of the serpent and oval earthwork, as accuracy would facilitate further comparative studies of serpent symbolism. Nonetheless, they did record several other features in close proximity, such as a “10 ft. high” conical mound to the south, and a strange “level oval terrace” also to the south. They further noted some “Indian Graves” across the ravine to the north. None of these features, however, merited additional investigation nor textual description. In comparison to the much larger burial tumuli often encountered in the Ohio valley, Squier and Davis likely thought this conical mound was too small to be worth opening. It is entirely unclear what the shaded oval on the map represents. The subtext for the “Indian Graves” is that these were automatically written off as crude remnants of recent “historic” tribes and deserved no further attention in a study about moundbuilders. In short, all else in the vicinity of the serpent work paled to insignificance by comparison.

Like a vast number of sites documented by Squier and Davis, the Great Serpent became famous among archaeologists and ethnologists after its appearance in Ancient Monuments. This circumstance was bolstered a few years later after Squier dove deeper into the waters of comparative ethnology by publishing The Serpent Symbol and the Reciprocal Principles of Nature in America. Squier here was very explicitly attempting to analyze primitive religious systems according to elementary binary conceptions, or “reciprocal principles of nature.”¹¹ Basic oppositional relations between ideas of active (male, the sun) and passive (female, the moon or earth) formed, Squier thought, at least

¹¹ Ephraim George Squier, The Serpent Symbol, and the Reciprocal Principles of Nature in America (New York: George P. Putnam, 1851), viii, 38.

some essential component of every religious system, if indeed not “the nucleus of every mythology.”¹² They were the vestiges of a primitive human state of existence.

The compound serpent and egg manifested these principles in a fairly complex manner, and so Squier situated serpent symbolism in the primitive American setting within a broader global context that especially included Egyptians, Hindus, and ancient Mexicans. Not coincidentally, only a decade earlier Catherwood’s spectacular images of Mayan ruins, replete with serpent imagery, had begun making a stir. Winged serpents in particular appeared to be associated with sun worship in Mayan and other mythologies; the serpent could also be associated with other phallic symbols. Tracing these sorts of connections and relations was what comparative ethnology was all about, and Squier’s Serpent Symbol was a remarkably detailed piece of research along these lines.

Nonetheless, he refrained from making any conclusive theoretical pronouncement on the critical question of whether this serpent business had migrated in to the New World or whether it was an independent invention.

After both Squier’s solo ethnological work and the more important archaeological collaboration with Davis, the “Great Serpent” became a well known object within the worlds of archaeology and ethnology. The fact that the elaborate and expensive Ancient Monuments of the Mississippi Valley was published and distributed by the Smithsonian Institution was a significant part of the story. Never before had an American archaeological monograph enjoyed such popularity, and over thirty years later it remained the most widely utilized source for information about the mounds.

¹² Squier, Serpent Symbol, 38.

6.3 Putnam comes to the Lovett farm.

The next time Serpent Mound was examined by an archaeologist in any meaningful way was when Frederic Ward Putnam came to the site in late 1883. One might point to a number of reasons for this long break, but essentially it boiled down to the lasting impact and authority of Ancient Monuments. Simply put, archaeological and ethnological scholars generally assumed the Squier and Davis got it right and all one had to do was cite their descriptions of sites. After all, the work had been sponsored by the Smithsonian under the editorial approval of Joseph Henry. The authors claimed to have personally overseen all fieldwork, and they insisted that surveys were done carefully and methodically with “the compass, line and rule.”¹³ Squier himself had at least some level of training in civil engineering.¹⁴ Squier and Davis additionally acknowledged the assistance of what amounted to a battalion’s worth of qualified and reputable individuals—including a number of topographic engineers—for the project.¹⁵ After Putnam’s visit, however, new perspectives about the site began to emerge.

By 1881, Putnam’s attention turned more fully toward Ohio archaeology, having begun field collaborations with members of the Madisonville Literary and Historical

¹³ Squier and Davis, Ancient Monuments, xxxiv.

¹⁴ David Meltzer, “Introduction: Ephraim Squier, Edwin Davis, and the Making of an Archaeological Classic,” introduction to reprint edition of Ephraim G. Squier and Edwin H. Davis, Ancient Monuments of the Mississippi Valley (Washington, DC: Smithsonian Institution Press, 1998[1848]), 4; Robert Bieder Science Encounters the Indian, 1820-1880: The Early Years of American Ethnology (Norman, OK: University of Oklahoma Press, 1986), 105.

¹⁵ Squier and Davis, Ancient Monuments, xxxiv-xxxviii.

Society on some sites in the Little Miami River valley near Cincinnati.¹⁶ One of that group's members, Charles Metz, who was a Cincinnati area physician and avocational archaeologist, quickly became Putnam's chief archaeological correspondent in Ohio. In the last chapter, I talked about Metz's important role in the Peabody correspondence system, but it would be good to reiterate just a few details from that discussion. Metz and some of his associates—including Baltimore and Ohio railroad auditor Charles Low—had already undertaken fairly extensive archaeological explorations throughout the lower Little Miami River valley near Cincinnati on a number of sites including the “Madisonville Cemetery,” a massive prehistoric burial ground.¹⁷ In conjunction with other members of the Literary and Scientific Society, Metz successfully negotiated with landowners to reserve the area for archaeological exploration. Under Putnam's further guidance and assistance, Metz continued to work intensively in this region, and his social standing and rapport with locals went a long way toward securing “exclusive right of exploration” for the Peabody from a number of landowning farmers throughout the Little Miami valley. Financial support for the work from the Peabody came via a modestly

¹⁶ Proceedings of the American Association for the Advancement of Science 30 (1882). As permanent secretary of the AAAS, Putnam certainly attended the meeting, and it is quite possible that his association with the nearby Madisonville Literary and Scientific Society began as a result of the meeting.

¹⁷ Curtis Hinsley, “The Museum Origins of Harvard Anthropology 1866-1915,” in Clark Elliot and Margaret Rossiter, ed., Science at Harvard University: Historical Perspectives (Bethlehem, PA: Lehigh University Press, 1992), 130. Also see, Charles L. Metz, “The Prehistoric Monuments of the Little Miami Valley,” Journal of the Cincinnati Society of Natural History 1 (1878-9), 119-128, and Charles F. Low and F. W. Langdon, “Archaeological Explorations Near Madisonville, Ohio,” Journal of the Cincinnati Society of Natural History 3 (1880), 40-68, 128-39, 203-20; 4 (1881), 237-57.

successful subscription campaign that at least partly revolved around popular lectures on archaeology regularly given by Putnam. Donors regularly contributed to an exploration fund that greatly facilitated the Peabody's working presence in the Little Miami valley.¹⁸

In September, 1883, Putnam made a general reconnaissance of sites throughout both Wisconsin and Ohio. This trip was significant in that it definitively marked a broadening of focus for the Peabody curator, especially in Ohio where his work had been pretty much confined to the Little Miami valley. For the Ohio leg of the trip, he was accompanied by Metz and Low, as well as Joseph Cox (a Cincinnati circuit court judge) and John Cone Kimball (Putnam's Cambridge museum assistant and photographer).¹⁹ The five men

¹⁸ F.W. Putnam, "Archaeological Explorations at Madisonville, Ohio," Fifteenth Annual Report of the Peabody Museum (1882), reprinted in The Archaeological Reports of Frederic Ward Putnam (New York: AMS Press, 1973), 185 ; F. W. Putnam, "Archaeological Explorations at Madisonville and Other Sites in the Little Miami River Valley, Ohio," Sixteenth Annual Report of the Peabody Museum (1883), reprinted in The Archaeological Reports of Frederic Ward Putnam, 202; F.W. Putnam, "Archaeological Explorations in Ohio and Wisconsin," Seventeenth Annual Report of the Peabody Museum (1884), reprinted in The Archaeological Reports of Frederic Ward Putnam, 212. For further details on Putnam's lectures (he had developed thirty eight separate archaeology-related topics), see David L. Browman, "Frederic Ward Putnam: Contributions to the Development of Archaeological Institutions and Encouragement of Women Practitioners," 217-9, and David L. Browman, "Origins of Stratigraphic Excavation in North America: The Peabody Museum Method and the Chicago Method," 246-7, both in David L. Browman and Stephen Williams, ed., New Perspectives on the Origins of Americanist Archaeology (Tuscaloosa, Alabama: University of Alabama Press, 2002).

¹⁹ Putnam gave an account of his trip through Ohio and Wisconsin at the October 21, 1883 meeting of the American Antiquarian Society. An transcript of this account was published in the combined Proceedings of the American Antiquarian Society, New Series, volume 3 (October, 1883-April 1885), 4-20. See esp. pp.

examined a considerable number of prehistoric works situated in the vicinity of the Paint Creek and the Scioto River confluence near the south-central Ohio town of Chillicothe. They also took a bumpy detour south from Hillsboro via mule wagon to the Lovett farm, where they had a good long look at the famous Serpent earthwork (Figure 6.3). Lovett kindly put the party up for the night, and must have taken an interest in their work, since by way of thanks, Putnam subsequently sent him copies of the Peabody Reports.²⁰

Returning to Cambridge, Putnam delivered an account of the trip to the American Antiquarian Society in Worcester, Massachusetts.²¹ One message came through loud and clear as he ran through a brief description of each of the sites visited—many of Ohio’s ancient structures described by Squier and Davis were being destroyed, usually as a result of repeated plowing. A population boom and the intensification of agriculture meant that more than a few of what were once massive and magnificent earthworks had been reduced to mere traces. Vandalism and looting were also problems.

In the address, Putnam devoted considerable attention to Serpent Mound, which not had only been plowed down, but also seemed to have been poorly represented in Ancient Monuments. Squier and Davis made a number of significant mistakes with their survey and description of the earthwork, especially near the northernmost end. Squier and Davis, Putnam reported, did not get the undulations in the supposed “body” of the serpent

10-4 for details of Serpent Mound visit. Biographical information on Cox may be found in William Coyle, ed., Ohio Authors and Their Books (Cleveland: World Publishing Company, 1962), 138.

²⁰ F.W. Putnam, “Notebook E,” PM/DR/FWP, 88-55.

²¹ Proceedings of the American Antiquarian Society, New Series, volume 3 (October, 1883-April 1885), 4-20.



Figure 6.3. Frederic Ward Putnam (right foreground) and company during his first visit to the Lovett farm to see the Serpent Mound in 1883. The men are standing near the “neck” region of the effigy. The photo was taken facing roughly southeast, toward the “tail,” and it certainly captures Putnam’s concern about the site’s condition. The contours of the serpent are indeed very faint, especially when compared with the aerial photograph in Figure 6.1, taken much later. Putnam’s investigations at the site, as I discuss below, involved “restoring” it as part of the creation of Serpent Mound Park. Source: Robert C. Glotzhober and Bradley T. Leper, Serpent Mound: Ohio’s Enigmatic Effigy Mound (Columbus, OH: Ohio Historical Society, 1982), 8-9.

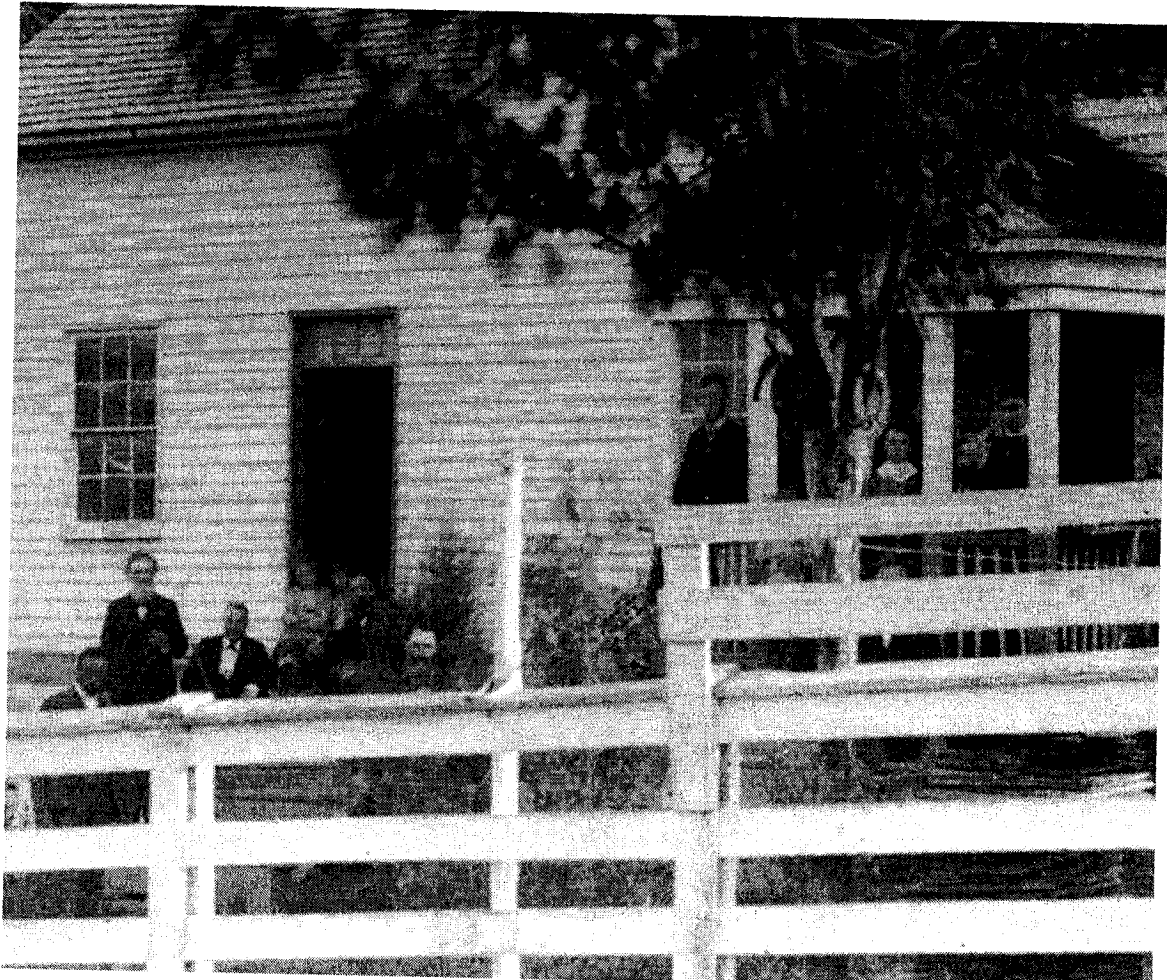


Figure 6.4. Putnam and company gathered in front of the Lovett farmhouse in 1883. Putnam's bearded visage appears just over the fence near the center of the photo. Charles Metz is seated in the doorway at left (in front of two unidentified girls). Two of the three men at the very left are Judge Cox and Charles Low. The Lovett family stands on the porch. Source: Peabody Museum Photographic Archives, Box 10-33:1.

correct, nor did they at all accurately depict the vague projections at the base of the “neck.” Exterior to the large oval embankment, Putnam noted some intriguing landscaping not recorded by Squier and Davis in that the ridge appeared to have been scooped outward toward the edges in a very symmetrical manner.²²

From the point of view of comparative ethnology, which at least in principle demanded descriptive exactitude as the foundation for making meaningful analogies across contexts, these were not minor, incidental details. Squier had already produced an entire monograph on serpent symbolism, a work that stood as an important contribution to the branch of ethnology dealing with systems of mythology and of religious beliefs. By the early 1880s, the influence of evolutionism (or at least the rhetoric of evolutionism) resulted in the emergence of sweeping theoretical frameworks for anthropology by the likes of Edward Burnett Tylor in Britain and Lewis Henry Morgan in the United States. For both of these theorists and others as well, the superstitious or religious aspects of human existence were every bit as tied to the laws of progressive development as anything else. Detailed comparative studies of primitive mythology thus continued to be a vital component of a broader science of man.

And so it was perfectly reasonable to ponder such details as whether the “neck” projections on the Ohio serpent were meant to represent wings or something else, or whether they were even original to the construction. Then there was the oval enclosure—did this really represent an egg? For that matter, was the entire work even meant to represent a serpent at all? (Look at Squier and Davis’s map again. Turn it upside-down.

²² [F.W. Putnam’s account of recent excursions in Wisconsin and Ohio], Proceedings of the American Antiquarian Society, New Series, volume 3 (October, 1883-April 1885), 11-2.

Does it not look like a germinating seed?) Putnam did not doubt that the main component of the mound was meant to represent a snake, but he called into question Squier and Davis's narrative interpretation of the oval as an egg being either swallowed or ejected by the serpent. He did not necessarily think the oval was an egg; the embankment simply may have delineated a functional social space, perhaps for worship.²³

Putnam documented other interesting features at the Serpent site. A plowed area some two hundred feet southeast of the monument consisted of a much darker soil thick with artifacts, which could have been the location of a former village or burial area. And lastly, the Peabody curator mentioned that the conical tumulus (the same one documented by Squier and Davis) had not been excavated, though it had been diminished by the plow. Hinting that no certain conclusions could be reached "until an extended exploration" was made, Putnam in so many words had put his own stamp on an overall interpretation of the Great Serpent. The effigy work itself, while important, was but one component of a much larger archaeological site that had considerably more going on: locations of ceremonial activity, living areas, burial spaces, and probably much more.

Putnam's particular ability to read archaeological sites as complex, integrated entities was unusual. It is important to bear in mind that most archaeological work on mounds up to this point in time usually involved surveying and sketching of the above ground structures, collecting surface artifacts, and excavating the conical mounds only (since these were generally known to contain the "treasure troves" of moundbuilder specimens). The work Putnam and Metz had been doing in the Little Miami valley—and especially at

²³ [F.W. Putnam's account of recent excursions in Wisconsin and Ohio], Proceedings of the American Antiquarian Society, New Series, volume 3 (October, 1883-April 1885), 13-4.

the Madisonville Cemetery—involved subsurface investigations over spatial expanses far more extensive than were common for most other archaeological investigations of the day.²⁴ Putnam—and, it should be remembered, Metz as well—were accustomed to spending a great deal of time working intensively in one area. The same cannot be said, for instance, about the fieldwork being done at the same time by archaeologists working for the Smithsonian Institution’s Bureau of Ethnology Mound Exploring Division. The Bureau archaeologists were working under an entirely different methodology, more interested in completing a geographically broad survey of as many sites throughout the greater Mississippi valley as possible within a relatively short period of time.

Of course, Putnam’s initial observations at Serpent Mound owed a great deal to the contingencies of changing land use patterns, a fact which needs to be pointed out so as not to be taken for granted. When Squier and Davis surveyed the site, the ridge was in timber and not being cultivated very intensively. Sometime in 1859 or 1860, a severe storm—either a hurricane or a tornado—leveled virtually all of the trees on the ridge, clearing the way for more extensive plowing and grazing, as well as erosion.²⁵ Thus, when

²⁴ The Madisonville Cemetery was a massive site, determined to be over fifteen acres in area and was in close proximity to a village site and small conical burial mound. As of early summer 1881, approximately three acres only of the cemetery had been excavated, which yielded in excess of six hundred separate burials and thousands of artifacts. Putnam and Metz’s work at this site would continue at intervals through the end of the decade. See, F.W. Putnam, “Archaeological Explorations at Madisonville, Ohio,” Harvard University Bulletin 2 (no. 6, June, 1881), 216-8; and, F.W. Putnam, “Archaeological Explorations at Madisonville, Ohio,” Fifteenth Annual Report of the Peabody Museum (1882), reprinted in The Archaeological Reports of Frederic Ward Putnam (New York: AMS Press, 1973), 185-90.

²⁵ John P. MacLean, “The Great Serpent Mound,” The American Antiquarian and Oriental Journal 7

Putnam and his four companions arrived in 1883, more details of the site had been exposed—and more were being destroyed.

In his Peabody Museum report for 1884, Putnam included a condensed written version of his American Antiquarian Society presentation. Although he again emphasized the errors of Squier and Davis regarding Serpent Mound, the general message here was about the dreadful state of preservation of many monuments compared with when they were first documented. For Putnam, this was not merely a sad situation, but a matter of national shame, and demanded action:

A generation of men has not yet passed away since most of these earthworks were in a good state of preservation; our children's children will look for them in vain unless something is done at once to preserve them.... Every year that passes without action is one more year allowed for ploughing over and destroying these wonderful works. A few thousand dollars expended *now* for the purchase of those which are best preserved will save monuments that future generations will surely value beyond all price.²⁶

Calling specifically upon the states of Ohio, Illinois and Wisconsin to do their parts, Putnam also urged cities, counties, and local archaeological and historical societies to take on the responsibility of archaeological preservation. Indeed, it was a matter of patriotic duty to do so. “Shall such vandalism, such shame,” he asked, “be laid to Americans of this century?”²⁷

6.4 Entangled rivalries and rival interpretations: Creating Serpent narratives.

In the previous chapter, I talked about the dynamics of the correspondent system and

(1885), 44-7; “The Serpent Mound Saved,” Ohio Archaeological and Historical Quarterly 1 (1887-8), 187-90; Otis Mason, “The Great Serpent Mound,” American Naturalist 21 (1887), 786-7.

²⁶ Putnam, “Archaeological Explorations in Ohio and Wisconsin,” 220.

²⁷ Putnam, “Archaeological Explorations in Ohio and Wisconsin,” 220.

how institutional competition between the Peabody Museum and the Smithsonian's Bureau of Ethnology played a major role in shaping the direction and nature of fieldwork. This certainly was the case with Serpent Mound.

Just to reiterate, the Bureau of Ethnology was founded in 1879 under the directorship of John Wesley Powell. It was a federal agency embedded within the Smithsonian, and it gave the Institution's overall program of anthropological research much needed focus and direction. Powell set up a Mound Exploring Division that was fully operating by 1882 under Cyrus Thomas, who had been charged with nothing less than overseeing a comprehensive survey and catalogue of moundbuilder remains, a task which involved revisiting sites documented in earlier reports. This project was at least partly motivated by Powell's desire to end moundbuilder separate race theories and establish conclusive genealogical links between modern Indian groups and moundbuilders. But as the mound survey got under way, there was also an increasing awareness of problems specifically associated with Squier and Davis's work. Errors such as those pointed out above by Putnam in regard to Serpent Mound often shadowed the Bureau efforts because so many people utilized data from Ancient Monuments as though it were entirely accurate and reliable. Thus, the Mound Exploring Division was also partly motivated by a concern to address the integrity of Squier and Davis's earlier Smithsonian-sponsored research and to correct errors especially in regard to the supposed geometrical precision employed within Ohio's "Sacred Enclosures."²⁸

²⁸ An effort which culminated in a special publication in 1889 in which a number of works from Ancient Monuments were resurveyed and shown to be not nearly so "perfect" as Squier and Davis had presented. See, Cyrus Thomas, The Circular, Square, and Octagonal Earthworks of Ohio (Washington, DC:

With all this getting under way at the Bureau, here was Putnam extending well beyond the Little Miami valley, becoming particularly interested in Serpent Mound, and taking a very pro-active stance toward preservation in general. While the Peabody and the Smithsonian participated in institutional exchanges of specimens and printed material—and Putnam, Powell, and Thomas all got along amicably enough in their professional dealings—on the ground there existed considerable competition to get at archaeological sites and artifact collections. By late summer 1884, Thomas and Smithsonian Secretary Spencer Baird had become concerned about what they perceived as Putnam’s growing monopoly on Ohio archaeology, especially in the Little Miami valley.²⁹ And Putnam, for his part, had become frustrated that his plans for a systematic exploration of the Little Miami valley was being thwarted by the activities of Bureau field agents in that region, hinting in confidence to Harvard naturalist and Peabody trustee Asa Gray that the Bureau’s destructive presence had disrupted his own “careful comparative work” there.³⁰ Putnam’s increasing interest in broader levels of Ohio valley archaeology at this time must be seen at least partly as a response to the Bureau of Ethnology’s activities. And when the Peabody curator appeared to be announcing his intentions to pursue further explorations at Serpent Mound—and others—made famous in Ancient Monuments of the

Government Printing Office, 1889).

²⁹ Baird to Thomas, 29 July and 4 August 1884, Smithsonian Institution National Anthropological Archives (NAA), Division of Mounds Exploration Records (DM/ER) 1881-1889, MS 2400 Box 8.

³⁰ Interestingly, Baird had somehow obtained a copy of this letter that summer while at Wood’s Hole, Massachusetts, where he ran a fisheries research facility. He then forwarded the letter on to Thomas at the Bureau of Ethnology. Baird to Thomas, 21 August 1884, with attached copy of Putnam to Gray, 15 August 1884, NAA, Division of Mounds Exploration Records 1881-1889, MS 2400 Box 8.

Mississippi Valley, Smithsonian authorities in turn took action.

In the early autumn of 1884, Thomas sent one of his corresponding agents, John Patterson MacLean, to the Lovett farm in order to make an official survey the Serpent.³¹ However, far from putting some sort of conclusive Bureau stamp on the site, MacLean, who was an Ohio Universalist minister and avocational archaeologist, produced some remarkably unexpected results that only stoked the fires of debate over what exactly the earthwork was supposed to represent. MacLean's resulting map (Figure 6.5) depicted, as he saw it, an uncoiling serpent striking at a frog (!), the alarmed batrachian leaping to safety while simultaneously emitting an egg into the serpent's mouth.³² MacLean speculated that this represented to the work's creators the creative (frog), productive (egg), and destructive (serpent) powers of nature. No mention whatsoever was made of any of the accompanying details of the site to which Putnam had drawn attention.

Even before MacLean got his results published, there was a response by Stephen Denison Peet. Peet was an Ohio-born Congregationalist minister, a keen student of both archaeology and biblical history, and, as I pointed out in the previous chapter, somewhat of a gadfly in the affairs of institutional archaeology. He was the founder and editor of the popular American Antiquarian and Oriental Journal (not associated with the American Antiquarian Society), which served as a melting pot for his interests in New and Old

³¹ MacLean had already visited the site twice, once the previous year as a "sight-seer," and once in May of 1884 in order to make a survey of his own. Apparently, this survey was not acceptable under the Bureau's standards who then sent him back in the autumn accompanied by a certified engineer. J.P. MacLean, "The Great Serpent Mound," The American Antiquarian and Oriental Journal 7 (1885), 44-7.

³² MacLean, "The Great Serpent Mound," 46.

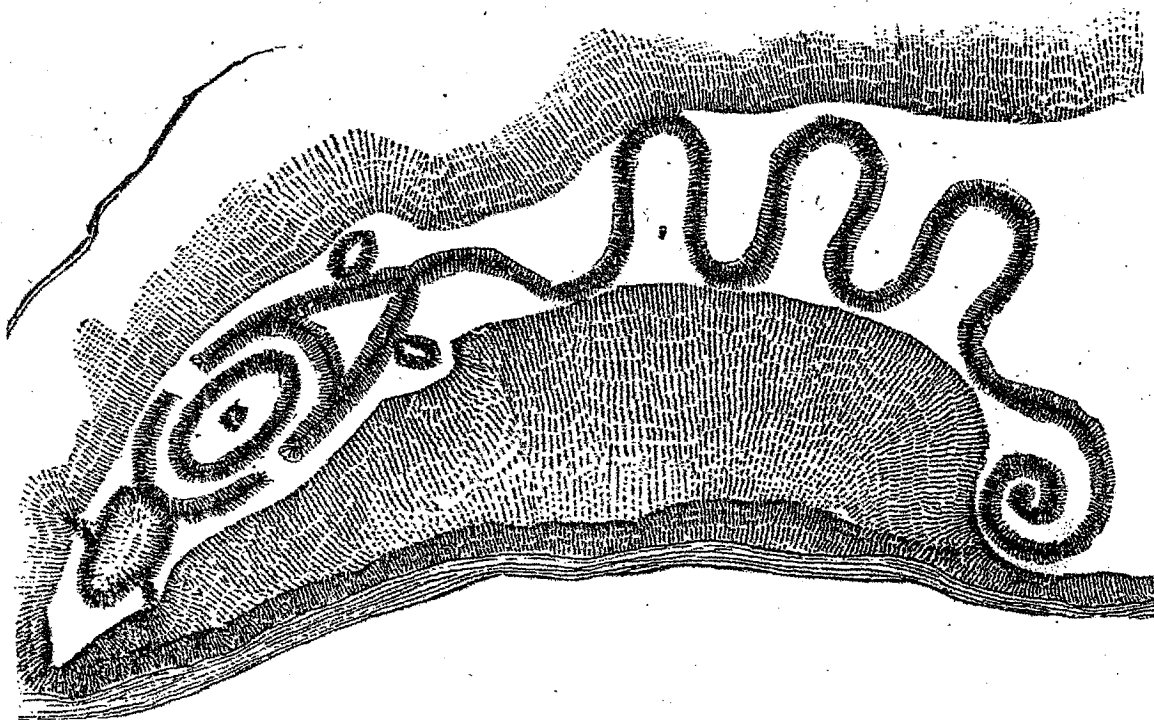


Figure 6.5. Plan view of Serpent Mound according to John P. MacLean. Source: John P. MacLean, "The Great Serpent Mound," American Antiquarian and Oriental Journal 7 (1885), 46.

World archaeology and history. The journal also served as a platform from which he monitored the progress of mound archaeology and editorialized about the methods employed in the Bureau and Peabody projects in the early 1880s, usually to the detriment of the former. Along these lines, it is worth mentioning that Peet accompanied Putnam for the Wisconsin portion of his big reconnaissance trip, and so was personally familiar with Putnam's views.

In a comparative study entitled "The Religious Character of the Emblematic Mounds" published in late 1884, Peet considered the nature of effigy mounds in both Wisconsin and Ohio.³³ Incorporating contemporary anthropological ideas about the progressive development of religion, he reasoned that sites such as Serpent Mound represented a sophisticated form of expression that involved not only the worship of animals, but also the further identification of animal spirits to particular landforms, a sort of fetishistic specificity. Such commingling of animal and nature worship was proof that the moundbuilders were a people in a state of ascendant developmental transition from a lowly stage of "primitive animism" toward a more sophisticated one of "primitive idolatry."³⁴ Topographic relationships between effigy and surrounding terrain were thus for Peet incredibly important details—as far as he was concerned enough alone to conclude that "emblematic mounds" in general were inherently sacred sites. Further evidence, however, came from the fact that most were associated with what appeared to be some sort of "altar" area (which usually amounted to an adjacent pile of charred

³³ Stephen D. Peet, "The Religious Character of the Emblematic Mounds," American Antiquarian and Oriental Journal 6 (1884), 393-411.

³⁴ Peet, "Religious Character of the Emblematic Mounds," 407.

stones).

From his point of view, the ethnological significance of Serpent Mound lay exclusively in the earthwork's setting atop a very serpentine sort of landform. The moundbuilders had recognized in the shape of the secluded ridge a resident serpent spirit and constructed the effigy as a place for worshipping this spirit. Peet had learned of MacLean's interesting interpretation of the site and presented it simply as a "new view," the details of which were largely unimportant in the face of the grander ethnological dimension of topography.³⁵ Rather interestingly, the only illustration accompanying Peet's discussion of the Serpent was a plan view closely copying the one from Squier and Davis's Ancient Monuments.³⁶ Since he did not doubt that "the old Brahmanic tradition of the serpent and the egg" had been at play on the Lovett farm, this map best indicated the important relationship between landform and effigy. Peet further speculated that the oval enclosure, situated as it was in such a wild, isolated locale with a commanding view of the valley below, must have been used as a place of sacrifice to honor the resident serpent deity.

To Powell and Thomas at the Bureau of Ethnology, these were unacceptable statements. They were just the sort of unwarranted ethnological speculations that they had decided were causing so much trouble and making it difficult for many people to accept that the ancestors of modern Indian tribes were in some measure responsible for building the mounds. Even MacLean's analysis of the Serpent—which, it should be pointed out, was published in Peet's journal—left things open for similarly speculative interpretations.

³⁵ Peet, "Religious Character of the Emblematic Mounds," footnote on pp. 403-4.

³⁶ Peet, "Religious Character of the Emblematic Mounds," 403.

As discussed last chapter, the Bureau mound survey was initiated according to the very specific goal of linking historic and modern Indian tribes to the moundbuilders. (And yet, also as I pointed out in that chapter, both MacLean for time and even Peet briefly worked for Thomas's mound survey because they were valuable resources for such an ambitious project.)

Thomas had already sent off one pre-emptive statement of the Bureau position a bit earlier in 1884 in an article published in Peet's American Antiquarian.³⁷ In what was supposed to be a review of a recently published book on the mounds, Thomas, probably under direction of Powell, instead used the platform to enunciate clearly the Bureau's position on the moundbuilder-modern Indian question: all moundbuilders were ancestral to modern or historic Indians. They were not some advanced, separate race. This was the guiding philosophy for all of the Mound Exploring Division's work. Of all the factors contributing to the separate race view, Thomas specifically singled out the ascription of a religious function to many earthworks, stating:

The unfortunate term "sacred enclosures," adopted by Squier and Davis, has been so generally accepted as indicative of the purpose for which a certain class of these works were erected, that it is looked upon as heresy to doubt its propriety. Yet there is not a particle of proof to justify it....³⁸

But the label would not go away.

Considering the magnitude of the Bureau's mound survey, it would take a bit of time but eventually Thomas himself visited Serpent Mound in late 1886, accompanied by

³⁷ Cyrus Thomas, "Who Were the Mound Builders?" American Antiquarian and Oriental Journal 6 (1884), 90-9.

³⁸ Thomas, "Who Were the Mound Builders?" 93.

William Henry Holmes, a rising star of Smithsonian ethnology.³⁹ Holmes, himself a trained and gifted artist, had been an illustrator for the U.S. Geological Survey for several years, and had made his own inroads into ethnological research in the American southwest.⁴⁰ In 1882, Holmes was made Honorary Curator of Aboriginal American Ceramics at the National Museum, and he quickly busied himself in analyzing the evolution of native decorative conventions in both pottery and textiles in the museum's collections. Although Holmes did not officially become part of the Bureau of Ethnology until 1889, his work was largely done for the Bureau. His research had already established a reputation as the paragon of rigor, and he kept his conclusions about the emergence of art forms tightly tied to known practices of historic or modern Indians.

Holmes, too, was also eager to view the Serpent with his own eyes. As if to emphasize that the Bureau was through messing around with interpretations of Serpent Mound, Holmes's account of the site appeared in the popular periodical Science late that winter.⁴¹ It was brief, very much to the point, and Holmes's sketch map of the work—very different in character from all preceding ones—spoke volumes (Figure 6.6). Both the earthwork itself and the surrounding landscape were portrayed in a much more

³⁹ On Holmes's career, see: Joan Mark, Four Anthropologists: An American Science in its Early Years (New York: Science History Publications, 1980), chapter 5; David Meltzer and Robert Dunnell, ed., The Archaeology of William Henry Holmes (Washington, DC: Smithsonian Institution Press, 1992), vii-xlix; and, Curtis Hinsley, The Smithsonian and the American Indian: Making a Moral Anthropology in Victorian America (Washington, DC: Smithsonian Institution Press, 1994), 100-9.

⁴⁰ Recall that John Wesley Powell was the director of both the Geological Survey and the Bureau of Ethnology.

⁴¹ William H. Holmes, "A Sketch of the Great Serpent Mound," Science 8 (1886), 624-8.

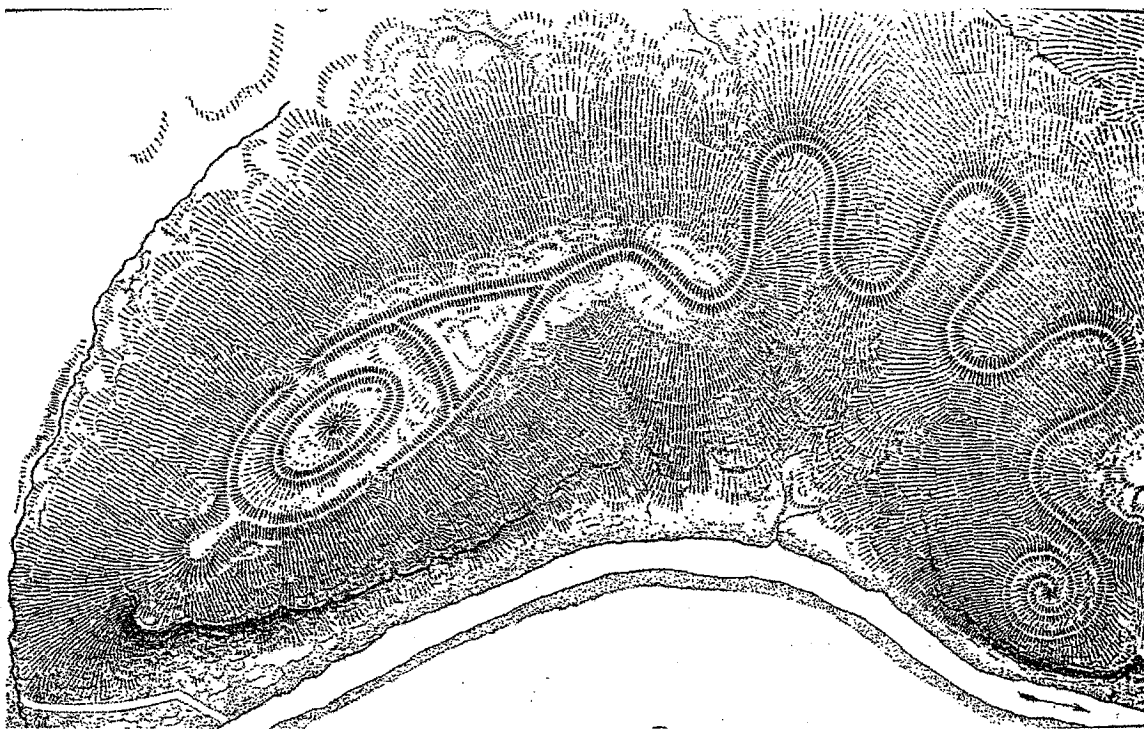


Figure 6.6. Plan view of Serpent Mound according to William H. Holmes of the Smithsonian Bureau of Ethnology. Source: William H. Holmes, "A Sketch of the Great Serpent Mound," *Science* 8 (1886), 626.

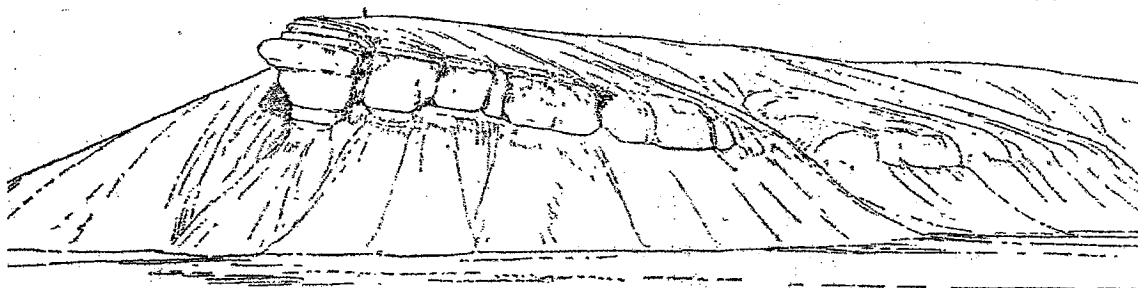


Figure 6.7. Profile view of "Serpent headland" according to William H. Holmes. Note the very small human figure on top of the bluff for scale. Source: William H. Holmes, "A Sketch of the Great Serpent Mound," *Science* 8 (1886), 626.

naturalistic fashion. Gone were the flattened ridge-top and unnatural contrast setting off mound from landform. Holmes remarked that:

Most of the attempts to throw light upon the more extraordinary features of the work have been made through the medium of oriental philosophy; but it is manifestly wrong to go thus out of our way to seek a symbolism for the oval enclosure, as do Squier and Davis, who liken it to the symbolic egg of old-world philosophy; nor need we make a serious effort to combat the idea that the terminal portion is a frog, as suggested by McLean [sic].⁴²

In Holmes's view, the oval actually represented the heart of the serpent. There was no terrifying act of devouring going on at all; instead, the serpent reposed rather benignly along the hilltop, its head a mere nub of a projection at the terminus of the work just over the steepest point in the bluff. This interpretation, according to Holmes, was warranted upon the basis of analogies to known native ceremonial art, in which the heart, representing life, figured as a recurrent motif. At least in partial agreement with Peet, Holmes proposed that the shape of the bluff certainly suggested a reptilian appearance (especially in profile, Figure 6.7), and thus may have been seen by prehistoric peoples as containing a serpent spirit. But the most important aspect of Holmes's reasoning was that it emphasized a continuity between known Indian artistic traditions and those of the moundbuilders. According to the Bureau archaeologists, this kind of thinking was not supported by the forms of interpretation being articulated by the scholars such as Peet.

MacLean, Peet, Holmes—the truly remarkable thing about each of these various interpretations of Serpent Mound is the extent to which individuals seemed to see in the earthwork and its surrounding landscape what they wanted to see. In terms of reconstructing some sort of meaningful narrative account of prehistoric life at Serpent

⁴² Holmes, "A Sketch of the Great Serpent Mound," 627.

Mound, however, it is somewhat surprising that other known details about the site did not get considered. Putnam had called attention in 1883 to the potential presence of an adjoining village site and burial area, but nobody else was all that interested. For that matter, no one else even cared to speculate on how the nearby conical mound—originally documented by Squier and Davis—might have figured into the overall complexion of the site.

6.5 Putnam buys the farm.

If anyone was becoming well-connected to some of the leading archaeologists of the mid-1880s, it was the Lovett family, whose secluded farm was increasingly subject to archaeological comings and goings. In early October, 1886, around the same time that Thomas and Holmes visited the farm, Putnam returned for a second time.⁴³

To the Peabody curator's eyes, the site was continuing to weather poorly and action was demanded immediately in order to stop its destruction. On the spot, he negotiated with the Lovett's to purchase some of the land, making an initial offer of \$3,000 for about 40-50 acres. Lovett insisted on the much more exorbitant amount of \$13,000 for everything—land, farm and all—since his sons were moving out and presumably the farm could not be kept going. Lovett agreed to \$6,000 down, with the balance to be paid over

⁴³ An important note here: Putnam's quite extensive final report on Serpent Mound appeared in the 1889-90 volume of the Century Magazine. Near the beginning of this article (p 872), he gave an account of the purchase of the site that contains some errors in chronology. He wrote that his second visit occurred in 1885, and that the subsequent purchase occurred in the summer of 1886. These dates simply do not hold up to the wealth of archival evidence showing the events in both cases to have occurred a year later. See, F.W. Putnam, "The Serpent Mound of Ohio," Century Magazine 39 (1889-90), 871-88.

two years at six percent interest. He may have been an unschooled farmer, but Lovett was sharp as a tack when it came to the practical matters of life. A draft agreement, binding for thirty days from that day of October 5, was written up and signed by Putnam and both John Lovett and a woman named Kate who, it seems, was help for an infirmed Mrs. Lovett. The transaction was witnessed by Putnam's photographer Kimball, who had come along, and another man named William Miller, a local contact for Putnam in this part of Ohio. That same day, Putnam got a letter off to the Boston Herald with an impassioned plea to save the work. Four days later Putnam sent another letter to Martin Brimmer, a Massachusetts attorney for the Corporation of Harvard University, outlining a plan that had coalesced in his mind about how this might be successfully accomplished in a profitable way. The Peabody would in fact purchase Lovett's entire lot, then resell all but around fifty acres encompassing the Serpent once a proposed new road was completed and the property value went up.⁴⁴

The tedious details of the transaction, which would take some time to sort out, are nonetheless very important to this story. In the first place, Putnam was taking remarkable initiative here. This was a precedent-setting example that attracted much attention, as nothing like it had been done before in the United States. Secondly, the successful purchase of land and subsequent working out of property boundaries directly shaped the scope of Putnam's field investigations there. This may seem obvious, but it is one of

⁴⁴ F.W. Putnam, handwritten agreement, 5 October 1886; F.W. Putnam to Martin Brimmer, 9 October 1886, PM Folder UAV677.45 "1883-1886 Putnam." Kate (and her role in the Lovett household) is mentioned in F.W. Putnam to wife Esther Orme Clarke Putnam, 30 May 1887, Harvard University Archives (HUA) Unit HUG1717.2.1, Box M-Z, Folder "P."

those determinative contingencies easily taken for granted when speaking of Putnam's investigations at Serpent Mound. And lastly, I wish to make it clear that the negotiations involved in the purchase of Serpent Mound were as much a part of what constituted Putnam's fieldwork as was the time he spent on site directing excavations.

Returning to Cambridge, Putnam soon found the preservation effort taking on a momentum of its own. The Sunday Boston Herald for November 21, 1886, ran a piece with the headline, "The 'Serpent Mound' One of the Greatest Ancient Wonders of America—A Serpent's Effigy over a thousand feet long—Immanent Danger of Destruction."⁴⁵ The article to follow reproduced Putnam's letter of October 5, which was then widely copied in the press.⁴⁶ People were taking notice, not least of whom was the remarkable Alice Fletcher, an ethnologist who had been working for some time in conjunction with both the Peabody Museum and the Bureau of Ethnology.⁴⁷ Although her research and fieldwork primarily focused on western Indian tribal life, she was quite supportive of archaeology and lauded Putnam's efforts to promote preservation. But she was also a woman of some means and influence. At a lunch in Boston, Fletcher roused the interest of a small coterie of ladies who then began corresponding with Putnam about how they could help.⁴⁸ A plan coalesced over the next few months by which the Boston ladies, under supervision of Mary Ware, would solicit contributions specifically for a Serpent Mound preservation fund to be sent to Peabody treasurer Francis Lowell. Ware

⁴⁵ Boston Herald Clipping, 21 November 1886, PM/DR/FWP Box 1, Folder 20.

⁴⁶ "The Serpent Mound Saved," Ohio Archaeological and Historical Quarterly 1 (1887-8), 187.

⁴⁷ Mark, Four Anthropologists, 62-95; Browman, "Frederic Ward Putnam," 225-7.

⁴⁸ "The Serpent Mound Saved," Ohio Archaeological and Historical Quarterly 1 (1887-8), 187-8.

made one of the largest single contributions at five hundred dollars.⁴⁹ Putnam himself was not directly involved in any of this.

The sale itself took considerably longer than the binding thirty day period to which Putnam and Lovett initially agreed. On January 31, an anxious Lovett contacted Putnam in an attempt to speed things along. “I would like to know against the 15th of February what you are going to do,” Lovett wrote, “there are parties who wish to open and investigate the Serpent Mound; they are so anxious that they would give something to have an investigation.” Just who these parties were remains unclear—possibly agents from the Bureau of Ethnology, but plenty of others would have been interested. Members of the nascent Ohio State Archaeological and Historical Society certainly would have been keen to explore Serpent Mound as part of their preparations for an upcoming centennial celebration at Marietta, Ohio, the once gateway and capital of the Northwest Territories.⁵⁰ Putnam, however, was a corresponding member of this group and in close

⁴⁹ F.W. Putnam (?), notes dated 28 March 1887, PM/AF 88-55, Folder 2; Various documents from 1887, PM/DR/FWP, Box 1, Folder 8 “Ohio Mounds-Serpent Mound, OH (funding drive) 1887.”

⁵⁰ The roots of the OSAHS go back at least to 1875 when the nucleus of the organization came together to mount an archaeological and historical exhibit for the 1876 Philadelphia Centennial Exposition. Lack of funds prevented the formation of a permanent society until 1885, at which point state monies became available, a move prompted largely by proposals to organize a large Marietta Centennial celebration in order to showcase the state’s important role in the history of the nation (and particularly in the process of western expansion). As always, archaeology was to be central to these proceedings. Ohio by this time was perhaps the fastest growing state with well over three million residents and its numerous large cities had become economic powerhouses, and this was in addition to its already strong agricultural economy. Interestingly, one of the semi-mythical founders of Marietta (founded, 1787) was Rufus Putnam, a distant

contact with several of its most prominent members; likely they would not have interfered in Putnam's plans. As I pointed out in the last chapter, there were at least a few financially well-off avocational archaeologists who were hoping to make a name for themselves in the field.

In any case, Lovett remained more interested in selling off the entire farm than in excavator's rights to the Serpent only. And he must have felt some allegiance to Putnam by this point—Putnam, for instance, had been sending copies of the Peabody reports to the farmer by way of thanks after his first visit in 1883.⁵¹ On February 16, Lovett was still awaiting Putnam's decision, and tried to sweeten the deal by offering another 15-20 acres at sixty dollars each. Lovett at this point really wanted to know what Putnam was going to do, writing, "I don't want to speculate [sic] off of you[;] by waiting on you 30 (?) days last fall throwed [sic] me out of a good sale at that time."⁵²

By mid-spring, as the Boston fundraising campaign was catching on, complications emerged regarding the deed to the land, but these were soon enough smoothed out.⁵³ At

relative of Frederic Ward Putnam. The Putnam name ran deep in the mid-west. See, F.C. Sessions, "The History and Prospects of the Society," Ohio Archaeological and Historical Quarterly 1 (1887-8), 325-32; "The Centennial Celebration at Marietta," Ohio Archaeological and Historical Quarterly 2 (1888-9), 1 (pp. 1-242 provides a full account of the Marietta Centennial proceedings); "Brief History of the Ohio State Archaeological and Historical Society," Ohio Archaeological and Historical Publications 35 (1926), 546; Williams, Fantastic Archaeology, 29; Browman, "Frederic Ward Putnam: Contributions," 240-1.

⁵¹ F.W. Putnam, "Notebook E," PM/AF 88-55, Folder 3.

⁵² Lovett to Putnam, 16 February 1887, PM Folder UAV677.45 "1883-1886 Putnam."

⁵³ John Wilson, not Lovett, possessed the deed to the land. This only temporarily held up the sale while appropriate arrangements and a full title search were made. See, F.W. Putnam to F. Lowell, 30 May 1887,

the end of May, after spending a grueling several days with Metz and Low exploring a long stretch of the lower Ohio River by boat and following up on a number of archaeological contacts, Putnam found himself recuperating in a Cincinnati hotel. Everything appeared to be in order for the Serpent Mound sale. He had been to see the Lovetts again, the title had been cleared, and the money was there in town in possession of a gentleman acting as Putnam's banker. Getting the appropriate signatures was all that was necessary.⁵⁴

One of the practical implications of the sale was that the Lovetts were to continue living at the farmhouse for the time being. Putnam felt he had already taken advantage of their hospitality enough already, and would certainly need to do so again. In a letter from Cincinnati to his wife Esther, he wrote:

Have bought a cream pitcher to take to Mrs. Lovett & three books for her daughters & for Kate, the girl who has to help her. I do this as I have not paid anything there for my meals & lodging & I want them to be ready to help us when we go out this fall....⁵⁵

This letter also reveals the extent to which Serpent Mound had become somewhat of an obsession for him. Even more than that, establishing the park had become an intensely personal act as well. It is worth quoting at length:

The papers have had all sorts of notices about me & the Serpent.... I shall devote this eve & tomorrow to writing out a full account for publication & giving the Boston ladies their full credit for the grand work done. It is a splendid thing darling—This presentation of this wonderful old monument & I feel it to be the grandest act of my life that I have been instrumental in bringing it about. I am so

PM/DR/FWP Box 1, Folder 8.

⁵⁴ F.W. Putnam to Esther Orme Clarke Putnam, 30 May 1887, HUA, Unit HUG1717.2.1, Box M-Z, Folder "P."

⁵⁵ F.W. Putnam to E.O.C. Putnam, 30 May 1887, HUA, Unit HUG1717.2.1, Box M-Z, Folder "P."

elated over it dear I wish you could be here to enjoy the pleasure it gives me, with me, for I know that when you have seen the place & taken in all I have accomplished you will be proud of your old man for what he has brought about. It will result in a beautiful park for the people for all time, as well as saving the monument. You will be charmed with the place. By the way, we have got a [running?] mineral spring on the place—The water comes out of the rock and contains plenty of sulphur odor. I have a bottle full here to take home to be analyzed. I look forward to its being just the thing for you. It certainly acts upon the digestive system & is cold and sparkling like some of the Saratoga waters. I tell you darling the fresh air & mineral water, fresh milk & eggs will make a new woman of you. You see if I don't have you strong on your legs yet. I'll [make] you acknowledge, my beauty, that I've cured you yet. You just bet your bottom dollar on that.—Yes we shall have some 1500 or more to spend in fixing the place up & exploring this fall. There are two stone mounds near the Serpent but not in the purchase which Lovett [agreed] I may [explore] also. So we shall have plenty to do this fall. Won't we have a jolly time my dear? Now I must say good bye as I have much to do & write. Send my love to the children and take good care of your own dear self. Don't get tired out trying to do too much.⁵⁶

Putnam's first wife had died in 1879, and Esther was ill. She and at least two of the three children (Alice and Eben) from his first marriage had accompanied him on fieldwork before.⁵⁷ The creation of Serpent Mound Park, as Putnam was beginning to envision it, was not just for the sake of archaeological preservation. Deeply personal feelings were motivating him in addition to his more academic views such as that the scientific study of ancient monuments demanded they be seen in their natural setting.⁵⁸

⁵⁶ F.W. Putnam to E.O.C. Putnam, 30 May 1887, HUA, Unit HUG1717.2.1, Box M-Z, Folder "P."

⁵⁷ Putnam's first wife, Adelaide Martha Edmands, died in 1879. They had three children together, Eben, Alice Edmands, and Ethel Appleton Fiske. Putnam married Esther in 1882. See Browman, "Frederic Ward Putnam," 238.

⁵⁸ Putnam's insistence that the scientific archaeological study of mounds must include seeing the ancient works with one's own eyes was made explicit in a letter he wrote to the members of the Ohio State Archaeological and Historical Society dated 10 February 1885. This was reprinted in the first number of that group's journal, the Ohio Archaeological and Historical Quarterly for June 1887. See, "Importance of

6.6 Putnam's Serpent Mound fieldwork: chronology, personnel, and field life.

Later in the summer of 1887, with Serpent Mound officially in the possession of the Peabody Museum, field explorations began and Putnam would be able to examine the site at a far greater level of detail than any previous investigator. Most of the more intensive fieldwork involving excavations and restoration was conducted in the late summer or early autumn of 1887 and 1888, with follow-up work being done during the subsequent two or three summers. I will be focusing primarily on the 1887-8 work, what I will call the first phase. Serpent Mound Park officially opened in spring or early summer of 1888, and drew over three thousand visitors over the summer. At about the same time, the Ohio State legislature passed a bill granting tax exempt status to any land held for the purpose of preserving prehistoric earthworks. Importantly, this first such protective legislation of its kind in the United States was prompted directly by Putnam's efforts at Serpent Mound. Later in 1888, Putnam installed a resident superintendent for the park to oversee general upkeep.⁵⁹ The second phase of work (from 1889 to 1891) generally involved such activities as additional surveying and the compilation of geological and botanical data relevant to the site, though some additional piecemeal exploratory excavation and restoration work was likely done as well. Much of the data generated from the second phase was used to prepare an elaborate scale model of the Serpent Mound and the surrounding terrain for exhibition at the 1893 World's Columbian Exposition in Chicago (for which Putnam was in charge of the anthropological exhibits).

the Study of Archaeology in Ohio," *Ohio Archaeological and Historical Quarterly* 1 (1887-8), 54-9.

⁵⁹ The Ohio bill and some details of early park visitation may be found in F.W. Putnam, "The Serpent Mound of Ohio," 873, especially fn. 1 and 4.

As for fieldwork, Metz generally got things started, setting up camp and laying in supplies, before Putnam's arrival. Such was the case in August of 1887, when the first round of fieldwork began. After tidying up assorted business details in Cambridge, Putnam reported arriving at the site on Saturday, August 27, spending a restless, bed-bug ridden first night at the Lovetts, and joining camp on Sunday. A canvas tent complex had been up and occupied for a couple days by a host of folks who had already joined Metz. Kimball and Low were there, as were a number of additional field hands—George Whitzel, John Jackson, Martin Noon, and a relative of Low's (whose name might have been Martin). It was not all men, however—Putnam's wife, Esther, and youngest daughter, Ethel, were also there, as was Annie Metz (probably Metz's daughter).⁶⁰ To what extent these ladies helped out is vague. As mentioned above, Esther was not well. Nonetheless, this core group seems to have been present for much of the work over the next couple years, but inevitably there was a fair bit of coming and going. In the later stages (1889 and afterward), certified engineer Clinton Cowen frequently appeared on site for topographic survey work during summer or autumn "field seasons," and Charles Abbott, one of Putnam's archaeological correspondents on the east coast, helped with a botanical survey of the park. One must also not forget the Lovetts, whose hospitality throughout the process greatly facilitated the end product, even if (as far as I have been able to determine) they never were directly involved in excavations.

Additional transient volunteer or temporary paid laborers very likely took also part in field proceedings as circumstances dictated. As was the case with other archaeological excavations of the time, few if any records were ever kept pertaining to these "invisible"

⁶⁰ F.W. Putnam, Unlabeled notebook, PM/AF 88-55, Folder 3.

fieldworkers. Even with Putnam—who generally seemed to be better than others at crediting helpers—we simply must accept the fact that his field notes and other writings are certainly incomplete in naming everyone who helped in some way or another. Given the high-profile nature of Serpent Mound, especially once it opened to the public, there is no telling what sorts of spur-of-the-moment arrangements were made.

Putnam's scattered field notes and the numerous photographs taken by Kimball during the period provide glimpses into field life at the site. One of the most striking facts to emerge from examination of Putnam's general notes is the efficiency of nineteenth century transportation systems. That the railroad played a central role in shaping the networks of field sciences in general is a given, but when we consider that Putnam could leave Boston on a Thursday and be at the Lovetts on Saturday, it rather puts things in perspective.⁶¹ (For comparison, consider that today it is about a twelve hour drive by freeway from Boston to Pittsburgh, and probably another four or five from there to Peebles, the nearest town to Serpent Mound.) For Putnam and his family, field life at Serpent Mound was never that terribly far away from the comforts of Cambridge. Figure 6.8 shows Putnam relaxing in what appears to be the mess tent, beverage in hand. From other images we know, for instance, that Ethyl had her own private tent, that a milk cow was maintained on site, and that the archaeologists' tents were generally provisioned with a bed, a desk, and chair.⁶² Putnam had hoped to have about fifteen hundred dollars available for expenses; the fundraising campaign apparently was successful enough that

⁶¹ F.W. Putnam, Unlabeled notebook, PM/AF 88-55, Folder 3.

⁶² Relevant images can be found in Peabody Museum Photographic Archives (PM/PA), Boxes 10-33:1 and 10-33:2.



Figure 6.8. Putnam relaxing in a tent at Serpent Mound. Source: Robert C. Glotzhober and Bradley T. Leper, Serpent Mound: Ohio's Enigmatic Effigy Mound (Columbus, OH: Ohio Historical Society, 1982), 5.

well in excess of five thousand was spent in the fall of 1887 alone.⁶³ Expenses covered travel, pay to workers, fencing posts, excavation supplies and equipment, and general provisions. General provisions included food items from chickens to fruit to aforementioned milk cow, as well as tobacco, soap, alcohol, and beer. All in all, it seems, Putnam, his family, and crew all had a good time of it.

In order to facilitate the discussion here and in the following section, it would be good look at the map that accompanied Putnam's published report of work at Serpent Mound Park (see Figure 6.17). It should immediately be apparent that this map of the site is of a very different character than all the previous depictions. We can see that Putnam documented significantly more information than what he had reported during his initial visits. These details could only have resulted from an extended investigation and interpretation of the site. Subsurface discoveries comprised only part of the new information. Several of the new details were in fact visible from the surface: numbers 3 and 4, adjacent to the "head" of the serpent correspond to two very small and faint artificial mounds; numbers 5, 6, 7 correspond to very large circular depressions in the ground; and number 32, near the cherry tree, corresponds to another small mound noticed by Metz, several feet high and considerably larger than either 3 or 4. None of these particular features had been documented before; each of them should have been more or less clearly visible from either the access road leading up to the farm house or from the serpent effigy itself. What this tells us about this particular case is true for just about all archaeological fieldwork—one's ability to decipher, make sense, or even register the existence of certain details as relevant archaeological features is continually in flux. It

⁶³ F.W. Putnam, Unlabeled notebook, PM/AF 88-55, Folder 3.

may be a function of how other site details as they are encountered point the way toward seeing the surrounding area in a new light, or it may be a case of suddenly seeing something entirely anew.⁶⁴ A small mound or a depression covered with growth might go entirely unnoticed as being natural features of the landscape until someone comes along who is able to interpret these differently, as parts of a larger archaeological site.

Putnam's 1887 season involved the most labor-intensive work.⁶⁵ Much of the site was cleared off, and a considerable amount of sod removal was undertaken so that soils could be examined near the effigy and around the habitation area (the cluster of features labeled 8 through 24). An exploratory trench was cut through the oval embankment in one location to examine its construction. The conical tumulus (26 on the map) was fully excavated, the contents documented and removed, and adjacent soils (25) examined. This mound was then "restored" to its pre-excavation form. Restoration work was also performed on damaged portions of the Serpent, measures which Putnam quite intriguingly projected would be done "in strict accordance with the survey made by Squier and Davis in 1849."⁶⁶ Given Putnam's earlier criticism of some details of that survey, I find this a very curious plan and can only take it as a reference to the general bulk and height of the embankments as Putnam did not restore the "neck" projections. General grounds keeping in preparation for the park was also begun, as were efforts at

⁶⁴ For an excellent account of how archaeologists' reasoning undergoes continual revision during site investigations, see Ian Hodder, *The Archaeological Process* (Oxford: Blackwell, 1999).

⁶⁵ These details come from F.W. Putnam, "The Serpent Mound of Ohio," and his unlabeled field notebook.

⁶⁶ F.W. Putnam, quoted in "The Serpent Mound Saved," *Ohio Archaeological and Historical Quarterly*, 1 (1887-8), 189.

creating a detailed map of the park area.

The 1888 field season picked up where the previous one left off, continuing work within the park bounds. The “very ancient graves” adjacent to numbers 27 through 31 were examined, and, I believe, the mound at 32 was excavated at this stage. Putnam had also received permission to examine the stone burials (not documented on this map, but noted on the earlier Squier and Davis map) north of the effigy across the ravine. While their absence from the map is somewhat intriguing, Putnam nonetheless discussed these at length in the final report, as we shall see.

The 1887 and 1888 sessions constituted most of the fieldwork insofar as excavations were concerned. As noted above, 1889-1891 saw additional activity on the site, but this seems to have been confined mainly to various survey and cataloging work, as well, I am sure, to park maintenance.

6.7 Putnam’s Serpent Mound fieldwork: considerations regarding methodology.

One of the things that set Putnam’s fieldwork apart from many of his contemporaries was his emphasis on exceptionally meticulous field methods.⁶⁷ Stephen Peet, in some of his American Antiquarian editorials from the mid-1880s, in fact went so far so as to explicitly contrast Putnam’s exemplary and “proper” field methods with those of the Bureau of Ethnology, as we saw in the previous chapter. As Peet saw it, a critical

⁶⁷ See, for example, David L. Browman, “Origins of Stratigraphic Excavation in North America: The Peabody Museum Method and the Chicago Method,” in Browman and Williams (2002); Stephen Denison Peet, “Relic Hunting Versus Archaeological Survey,” American Antiquarian and Oriental Journal 6 (1883-4), 204-8; Peet, “The Peabody Museum in the Field,” American Antiquarian and Oriental Journal 6 (1883-4), 277; Peet, “Mound Explorations,” American Antiquarian and Oriental Journal 9 (1886-7), 390-1.

difference was that Putnam spent much more time investigating a site, and he paid much more attention to ancient works in their natural settings.

Where did Putnam get his ideas about field methods? This is a good question, and one that probably cannot be answered very easily. Meticulous attention to detail in matters of natural history certainly may have been instilled into a younger Putnam when he studied under Louis Agassiz. It has been suggested that Putnam bore with him throughout his career certain insecurities about his early formal education (he never actually received a degree from his studies with Agassiz), and that these insecurities continually drove him in his work.⁶⁸ We can really only surmise as to how this background may have shaped his notions of archaeological fieldwork. What is clear, however, is that a definitive shift in thinking occurred as a result of Putnam's initial collaborative work with Metz in the Little Miami River valley from around 1882. Putnam had begun to realize that the mounds were just the visible, above-ground parts of sites that were in fact far more extensive underground, and therefore demanded larger-scale excavations. I will return to this very important feature of Putnam's thinking at the end of this chapter.

In any case, we know for certain that by the mid-1880s, Putnam was publicly advocating careful, specific methods for excavation. At least one of his thirty-eight public lecture topics was devoted specifically to the subject, but he also instructed his correspondent archaeologists to follow certain guidelines, and he went out of his way to circulate information and to inform would-be correspondents to adhere to rigorous

⁶⁸ Curtis Hinsley, "Frederic Ward Putnam," in Tim Murray, ed., Encyclopedia of Archaeology: The Great Archaeologists, Volume 1 (Santa Barbara: ABC-CLIO, 1999), 141-54, especially pp. 142-3.

practices.⁶⁹ Helpfully, an abstract of his lecture entitled “On Methods of Archaeological Research in America,” was published in the Johns Hopkins University Circulars for 1885.⁷⁰ Here, Putnam said it was necessary to “know the exact conditions under which every object placed in our museums of archaeology was obtained and its associations with other things.”⁷¹ Those “other things” to be carefully noted within the context of field explorations included everything from finely crafted finished artifact specimens (such as carvings, figurines, and worked ornaments) to stone chips, fragments of various materials, burned fibers, seeds, nuts and animals bones. All human skeletal remains, no matter how small the pieces, should be saved. Excavations of sites must proceed methodically and with care in order to maintain control over the contexts from which specimens were recovered and also to trace soils or structural variations in both mounds and general sub-surface settings.

⁶⁹ Browman, “Frederic Ward Putnam,” 218-9. Putnam could be quite blunt in responding to avocational archaeologists who sent in results of their work in hopes of working for the Peabody, but who did not use care in their excavations. Such was the case when Putnam responded to one William Thompson, whose methods were obviously not up to par. Putnam sent the man two of his pamphlets on method and referred him to two of Putnam’s published papers. “Should you wish to do work for this Museum,” he further wrote, “of course, I should write you in detail. All I can do now is urge you to do the work in such a thorough manner that it will be of scientific importance. This digging into mounds and graves just for the sake of obtaining a few relics is perfect vandalism.” Putnam then urged the Thompson to visit his camp in the Little Miami valley and to contact Metz to learn how excavations should be done. F.W. Putnam to William Thompson, 10 June 1890 PM/AF 90-37A, Folder 1.

⁷⁰ Anonymous, “On the Methods of Archaeological Research in America, by Professor F.W. Putnam,” Johns Hopkins University Circulars 5 no. 49 (1886), 89-92, reprinted in Williams, ed., 1973., pp. 1-4.

⁷¹ Anonymous, “Methods of Archaeological Research,” in Williams, ed., p. 2.

More specifically, Putnam's directions for excavation called for judicious trenching as a controlled exploratory technique. In mounds, he outlined a "trench and slice" method by which one began by digging a linear trench tangential to and just intersecting the edge of the mound in order to determine the horizontal and vertical limits of the mound construction (the base of a mound, he said, was frequently several feet below the modern surrounding surface). The excavator then should make a drawing of or photograph the vertical profile; measurements, Putnam recommended, should also be taken from two reference strings set up over the mound, one oriented north-south, the other east-west. (Presumably, the trench wall should be oriented in one of the cardinal directions.) After this has been completed, the first "slice" is to be made—approximately one foot cut away from the established vertical wall, so that a new vertical profile is created one foot further into the mound. Putnam emphasized the need to document all changes in structure, soils composition, locations of finds, and so forth. The slicing technique should be repeated until the entire mound has been excavated, and each stage documented accordingly.⁷² Considering the size and contents of many mounds, this was a far more labor-intensive method than was typically used.

Exploratory trenching was also to be used in examining other sub-surface features, such as village sites, habitation areas, refuse piles, or cemeteries. Putnam first recommended removal of surface leaf mold and other surface soil (presumably, plowed and churned soils would qualify here). As in the case of mounds, judiciously placed trenches would determine both horizontal and vertical limits of these areas; once this was done, trenches should be expanded by slices in a methodical fashion, and details

⁷² Anonymous, "Methods of Archaeological Research," in Williams, ed., p. 3.

documented in the process. Putnam explicitly stated that under no circumstances “should an excavation be made from the surface of a mound, site of habitation, burnt space, or refuse pile. From the moment this is done all is confusion, and much is destroyed by being broken with pick or spade.”⁷³ It is important to understand that, unlike modern methods where a site is often excavated by complete minute layers from the surface, Putnam emphasized working vertical sections through features as the method of control. An anachronistic analogy might be that Putnam’s method was akin to running a scanning beam through portions of a site. As long as all the details in profile were carefully documented along the way, one should be able to reconstruct some semblance of the various horizontal dimensions.

Looking at Putnam’s excavations at Serpent Mound Park, we can see that his methods were perhaps even more refined than this. Figure 6.9 was adopted from one of many photographs taken during excavation of the large conical burial mound and used in the final published report.⁷⁴ At the time that the original photograph was taken, the bulk of the slicing had progressed about halfway through the mound. But we do not see a fully intact vertical profile—instead, important features had been left in place for further examination and documentation. In the far left and center of the image, we can see that skeletons had been left fully *in situ*. These “intrusive burials” were made some time after the single central burial over which the mound was initially erected. Putnam treated burials with care, and as far as it was possible, they were left intact to be sketched and

⁷³ Anonymous, “Methods of Archaeological Research,” in Williams, ed., p. 3.

⁷⁴ F.W. Putnam, “The Serpent Mound of Ohio,” 881. I have seen the original photograph from which this plate was made. PM/PA Box 10-102:2.



SECTION OF CONICAL MOUND, SHOWING INTRUSIVE BURIALS.

Figure 6.9. Excavation of large conical mound in progress. Note skeletons left *in situ* at left and center. Source: Frederic W. Putnam, "The Serpent Mound of Ohio," Century Magazine 39 (1889-90), 881.

photographed first. Similarly, a large stone had been left in place at the right foreground. Across the lower level of the excavation is an interesting clay platform that had been left more intact than the upper soils (the left-most intrusive burial happens to be resting upon this layer). We can also see here along the base of the vertical wall at the right foreground a sort of talus slope of loose dirt that had been sliced away from the wall and not yet cleared. This is an important detail, for it shows that the mound was not just shoveled away in large chunks. And, if one squints, one can see that finer tools than shovels were used—atop the earthen pillar immediately behind the central skeleton there is a trowel.

Of course, all this is not to say that Putnam's methods here stand up to modern standards. His insistence on vertical slicing automatically entails that any artifacts recovered from the loose talus, for example, could not have been given a reliable provenance. Nonetheless, this was remarkable fieldwork for the time. Comparing Putnam's explicit statements on excavation method with images of the work done at Serpent Mound reveals a fundamentally interesting aspect of his method as a whole. For Putnam, excavation was a process of give-and-take according to what evidence or information was encountered.

Actual techniques employed for excavation were one thing. Techniques for documenting the results of field activities were another. Without question, photography was the single most important documentary technique employed at Serpent Mound, at least to our modern eyes. Numerous photographs were taken throughout the course of fieldwork, and these reveal details that are not mentioned or captured anywhere else. As noted above, John Cone Kimball's photographs reveal to us aspects of general field life at the site, and they served as the basis for all of the plates depicting excavations in progress

used in the final published report (and quite likely many of the artifact illustrations as well). In other images, one may see the tools of the archaeologist's trade—shovels and picks for the heavier work, and trowels and whisk brooms for more refined picking and cleaning around skeletons.⁷⁵ Considerable care was taken in recording photographs, too. A number of field maps developed over the course of the work plot locations of camera positions for various shots. Kimball's work was essential for Putnam to be able to reconstruct an account of excavations for the report.

The same can hardly be said about Putnam's own field notes and sketches, at least at first glance. These seem to be a rather haphazard mess, with piecemeal entries not always appearing in any discernable order. Entries pertaining to completed stages of fieldwork did not often yield much information other than dates. One typical sort of entry reads:

Oct 15 1887 Finished exploring conical mound & began to restore it. Oct 19, finished exploring the graves under stones near path & small mounds nearby. Oct 20 Thursday made survey of Serpent tail. Packed & started for Hillsboro at 3:20....⁷⁶

Figures 6.10, 6.11, 6.12, and 6.13 are some characteristic examples of the kinds of sketches that may be found in two of Putnam's field notebooks.⁷⁷ Pretty crude, to be sure. Nonetheless, there was method to it all, and I would argue that these rough images and accompanying scrawled notes contain immense amounts of information—even if it would have been of benefit only to Putnam himself. Many measurements appear in these sketches, and the brief notes provide many additional descriptive details, often tying the

⁷⁵ Photographs of Serpent Mound excavations in progress may be found in PM/PA Box 10-102:2.

⁷⁶ Unlabeled notebook, PM/AF 88-55, Folder 3.

⁷⁷ "Notebook E," and unlabeled notebook, PM/AF 88-55, Folder 3; Unlabeled notebook, PM/AF 87-60, Folder 1.

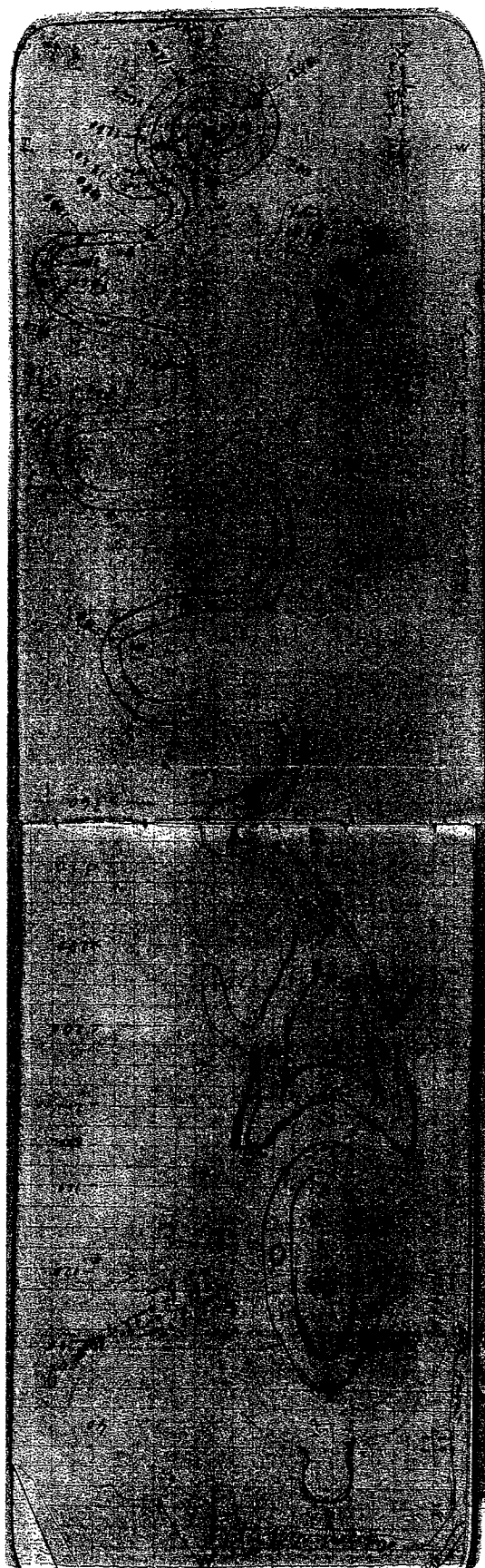


Figure 6.10. One of Putnam's many field sketches of the serpent effigy. Source: "Notebook E," PM/AF 88-55, Folder 3.

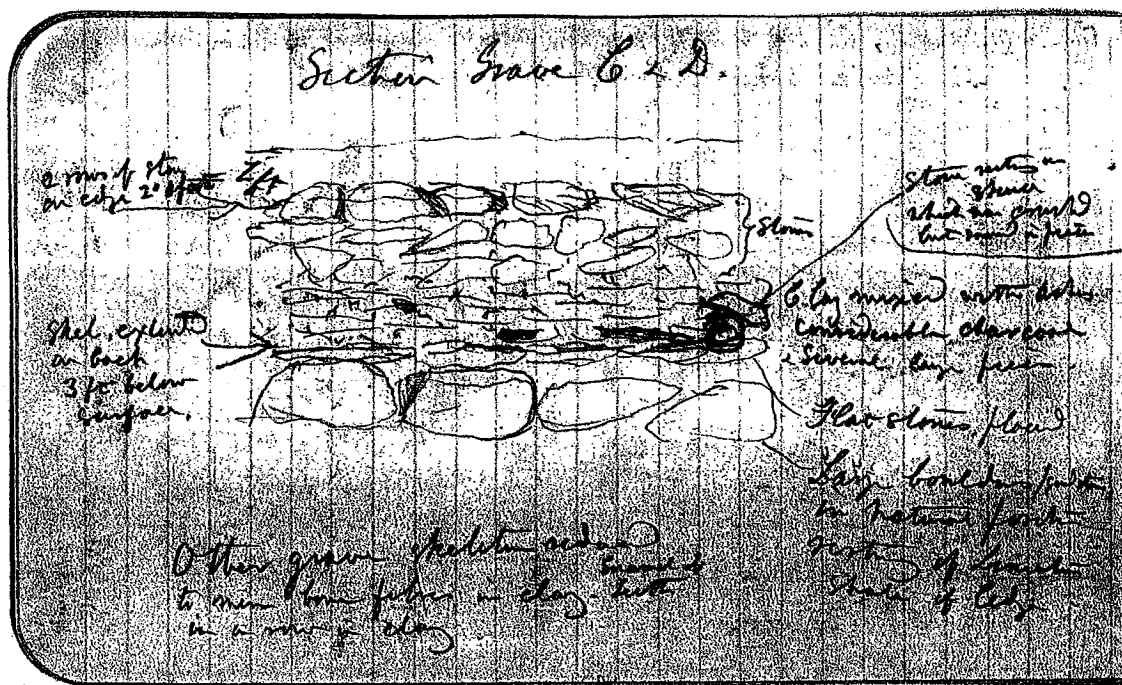


Figure 6.11. Sketch of burial profile, showing vertical position of skeleton within grave stratigraphy. Source: F.W. Putnam, unlabeled field notebook, PM/AF 88-55, Folder 3.

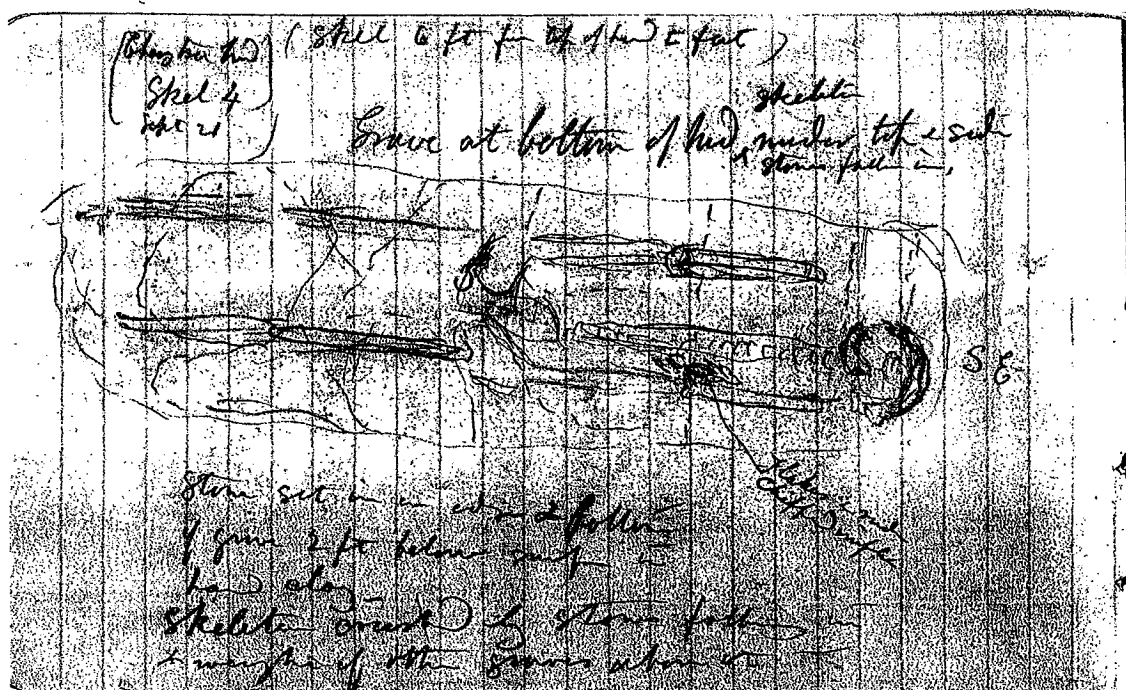


Figure 6.12. Sketch of central burial in large conical mound, showing general orientation of skeleton. Source: F.W. Putnam, unlabeled field notebook, PM/AF 88-55, Folder 3.

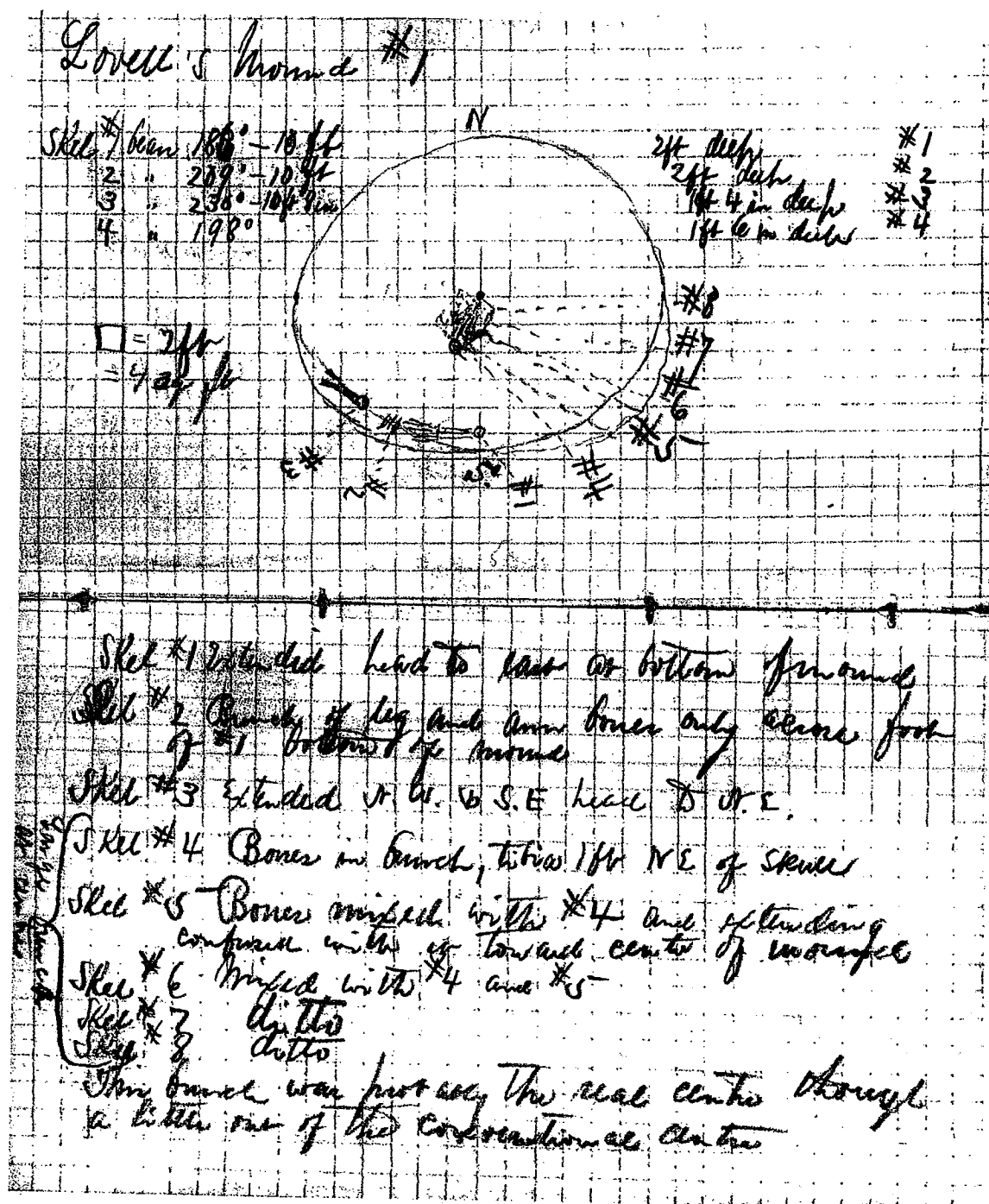
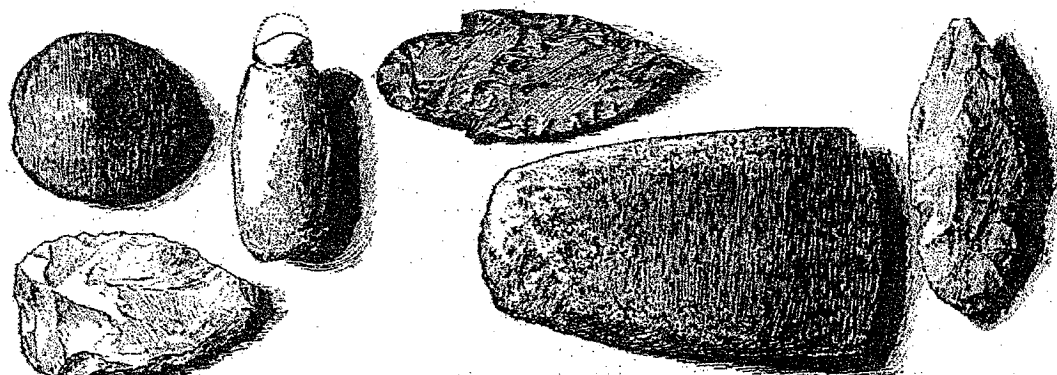


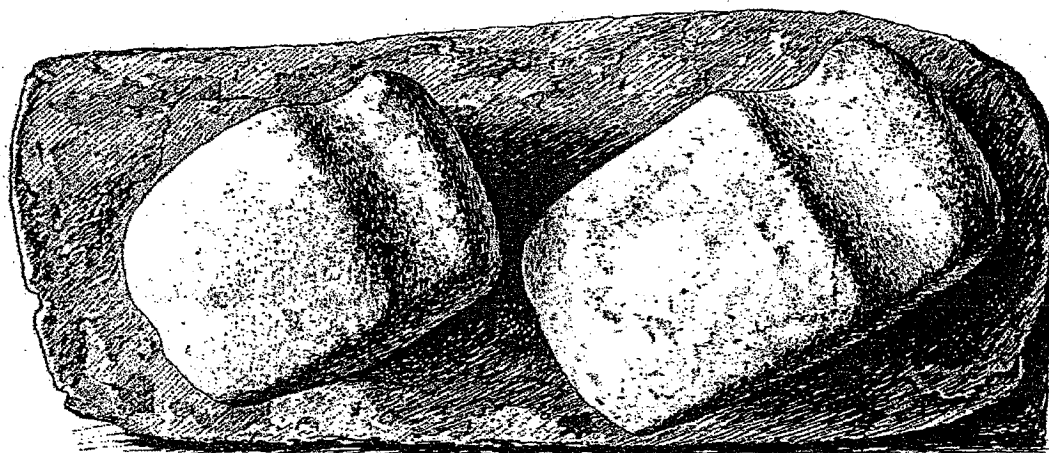
Figure 6.13. Sketch describing locations of "intrusive burials" in large conical mound. Source: F.W. Putnam, unlabeled field notebook, PM/AF 87-60, Folder 1.



STONE IMPLEMENTS (HALF SIZE), FROM DIFFERENT PARTS OF CONICAL MOUND,
(NOW IN THE PEABODY MUSEUM.)



HEMATITE HEMISPHERE, GROOVED STONE IMPLEMENT, HATCHET, AND FLINT KNIVES (HALF SIZE), FOUND ON
OR NEAR ASH BED IN CONICAL MOUND. (NOW IN THE PEABODY MUSEUM.)



COPPERPLATE AND STONE AXES (HALF SIZE), FROM BOTTOM OF CONICAL MOUND. (NOW IN THE PEABODY MUSEUM.)

Figure 6.14. A sample of stone artifacts and a copper plate recovered from large conical burial mound. Images not to original scale. Source: F.W. Putnam, "The Serpent Mound of Ohio," *Century Magazine* 39 (1889-90), 882-3.

sketched features to other parts of the site. In conjunction with photographs, survey maps, and even the recovered artifacts and specimens, Putnam's field notes were thus quite valuable. The thing to remember here is that it was not intended for anyone else to read them: they functioned more like mnemonic triggers for his use only in writing up an authoritative account of the work. What made the report authoritative, of course, was the fact that he had written it.

As for artifacts and specimens, I should mention briefly how they were handled once removed from the ground during the Serpent Mound excavations. At least some minimal level of processing took place in the field in order to maintain control over the provenance of individual specimens or groups of related specimens. In other words, they were not simply lumped as a whole into a general "Serpent Mound" category and left at that, although groups of associated materials that were recovered from single contexts were kept together (as in the case of clusters of bone fragments or flint chips). Specimens were packed into crates, with packing material often coming in the form of Boston Herald newspapers that Putnam had sent out to his field locations, and presumably accompanied by some form of documentation. Back at the Peabody Museum laboratory, the various objects were labeled directly with a number (in bright orange ink or paint) as far as was possible depending on size and condition. The numbers were then entered into a master logbook along with other relevant information pertaining to the entry.⁷⁸

⁷⁸ It is entirely possible that logbook-keeping began in the field, though I have not encountered direct evidence to this effect. Putnam maintained a single series of numbers for all specimens coming into the museum; that is, each specimen simply received the next number in sequence. This system became far too

The Serpent Mound specimens remained in storage at the Peabody for quite some time and were not put out on public display. In fact, this was the case with many of the Ohio materials, and it was not merely a function of museum inefficiency. Putnam kept his Ohio specimens closely guarded at this point in time, and some of them not just boxed but locked away out of sight. One Bureau of Ethnology archaeological correspondent, Henry Lee Reynolds, made his way to Cambridge in 1887 to get information from Putnam on Ohio materials. As with other Bureau-Peabody dealings, beneath the surface veneer of gentlemanly courtesy lay more cutthroat attitudes. Putnam kindly spent two entire days with Reynolds in showing him some of the Ohio collections; however, the visitor was not permitted to sketch or take notes about many of the materials. Reynolds strongly suspected that Putnam was worried any information he might gather on these particular collections would be taken to Washington and used in support of “Major Powell’s theory.”⁷⁹

On that note, I would like to conclude this section with one final comment. It is imperative to understand the extent to which Putnam’s field experience was intertwined

cumbersome as numbers approached 100,000, and in the 1920s, the system was changed. Various “Accession Books” were begun with different letter designations for different geographic regions, and the older materials were incorporated into them along with the older numbers. Subsequent systemic updates over the years have maintained the links to the original system. Thus, today, one may be led from active accession logs with Serpent Mound information to Putnam’s original books, as well as to the original specimens themselves. Personal interview with Susan Haskell, Peabody Museum Ethnographic Collections Curator and acting document archivist, 10 April 2002.

⁷⁹ Henry Lee Reynolds to Cyrus Thomas, 29 March 1887, NAA/DM/ER 1881-1889, Box 10, Folder “H.L. Reynolds.”

with his views about the complex character and prolonged duration of North American prehistory. This point is missed by many commentators on Putnam's work. As I mentioned much earlier, Putnam's views on prehistory contrasted starkly with those of the Smithsonian Bureau of Ethnology archaeologists. Putnam thought that human presence in the New World pre-dated the "glacial epoch" and that the ancient Americans were far from a single, homogeneous group.⁸⁰ From the Bureau's point of view, there simply wasn't acceptable evidence of extreme antiquity, and admitting too much prehistoric human diversity complicated the task of associating moundbuilders with extant tribal groups. For Putnam, archaeological fieldwork simply had to proceed in a cautious, intensive manner in order to make sense of the deep time and intricate processes of mixing and migration that he thought occurred during that time. However, I do not at all wish to suggest that Putnam's ideas about prehistory uniformly predetermined the nature of his fieldwork; on the contrary, I would argue that these emerged together and informed each other. Serpent Mound, it turned out, provided excellent evidence for Putnam's views.

6.8 New Serpent Mound narratives.

We are now in position to summarize Putnam's conclusions about Serpent Mound, conclusions derived largely from what I have called the more intensive first phase of field work in 1887-8. The dense eighteen page report, long and very detailed by standards of the time for single-site accounts, appeared in the popular Century Magazine. The fact that it was not published in a more scholarly periodical—including the Peabody Reports—was

⁸⁰ Anonymous, "On Methods of Archaeological Research in America, by Professor F.W. Putnam," 1.

an important strategic move. Putnam wanted it to be accessible and widely read as part of his ongoing efforts to educate a broad audience in his new archaeology.⁸¹ Nonetheless, Putnam did not skimp on the details nor dumb things down—this was as technical an archaeological account as any to be found in the 1880s.

After a general introduction and account of the purchase of the park, Putnam addressed the construction of the serpent and oval works.⁸² The geological formation of the hill-top consisted of bedrock overlain by successive layers of gray marl (decomposed rock with clay), yellow clay, and the most recent dark soils at the top (which varied from five inches to two feet in thickness across the site). Exploratory trenching into the effigy embankments showed that, beneath the sod, they were made of yellow clay and stones resting directly upon the decomposed clay layer (Figures 6.15 and 6.16). Any existing surface soils had been stripped from the ridge at the time of construction. Most of the clay used in the serpent and oval, Putnam reasoned, had come from the three large borrow pits (locations 5, 6, and 7 on the site map, Figure 6.17). Considering the length of the effigy—1,348 feet from the tip of the oval to the tip of the tail following the curves—it was a sizeable undertaking.

The “village area” indicated by the dense cluster of features at points 8 through 24 just northeast of the borrow pits yielded an immense amount of information.⁸³ After

⁸¹ For example, Putnam advised William Thompson, who was interested in working for the Peabody as a correspondent, to consult the Century Magazine article as an example of proper archaeological methodology. F.W. Putnam to William Thompson, 10 June 1890 PM/AF 90-37A, Folder 1.

⁸² Putnam, “The Serpent Mound of Ohio,” 874-5.

⁸³ Putnam, “The Serpent Mound of Ohio,” 878-80.

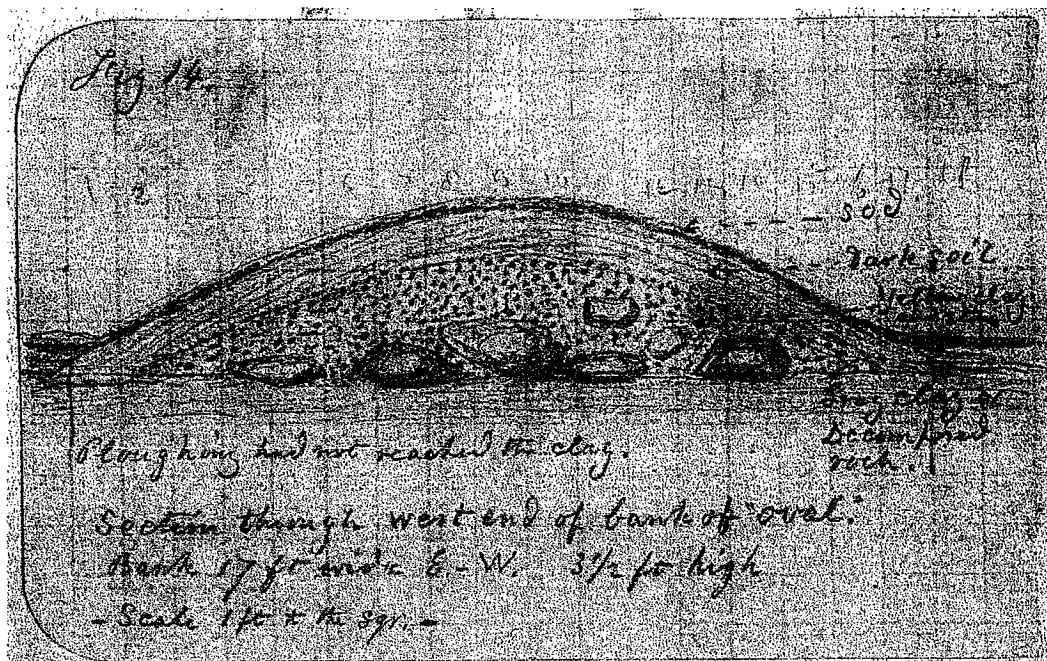


Figure 6.15. Putnam's field drawing of a cross-section through the oval embankment. Source: F.W. Putnam, PM/AF 88-55, Folder 1.

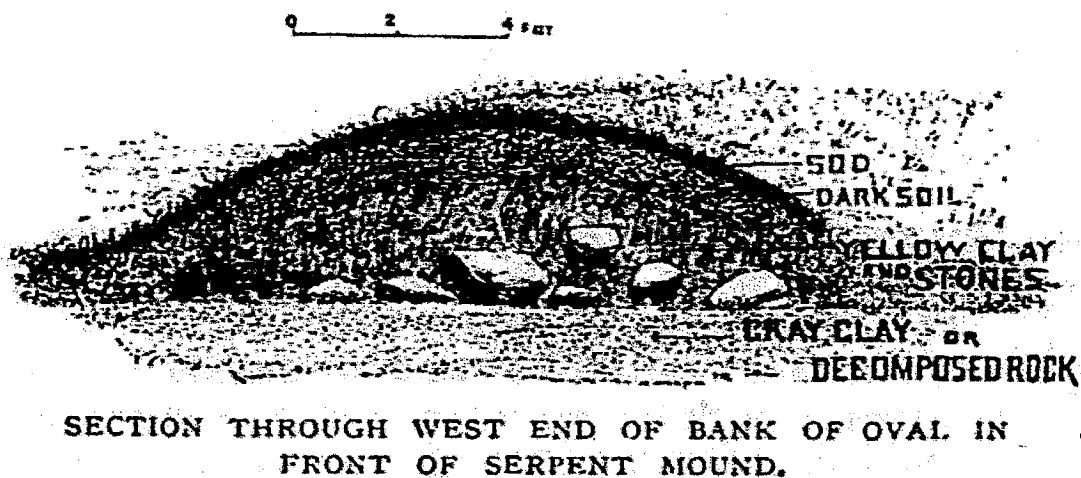
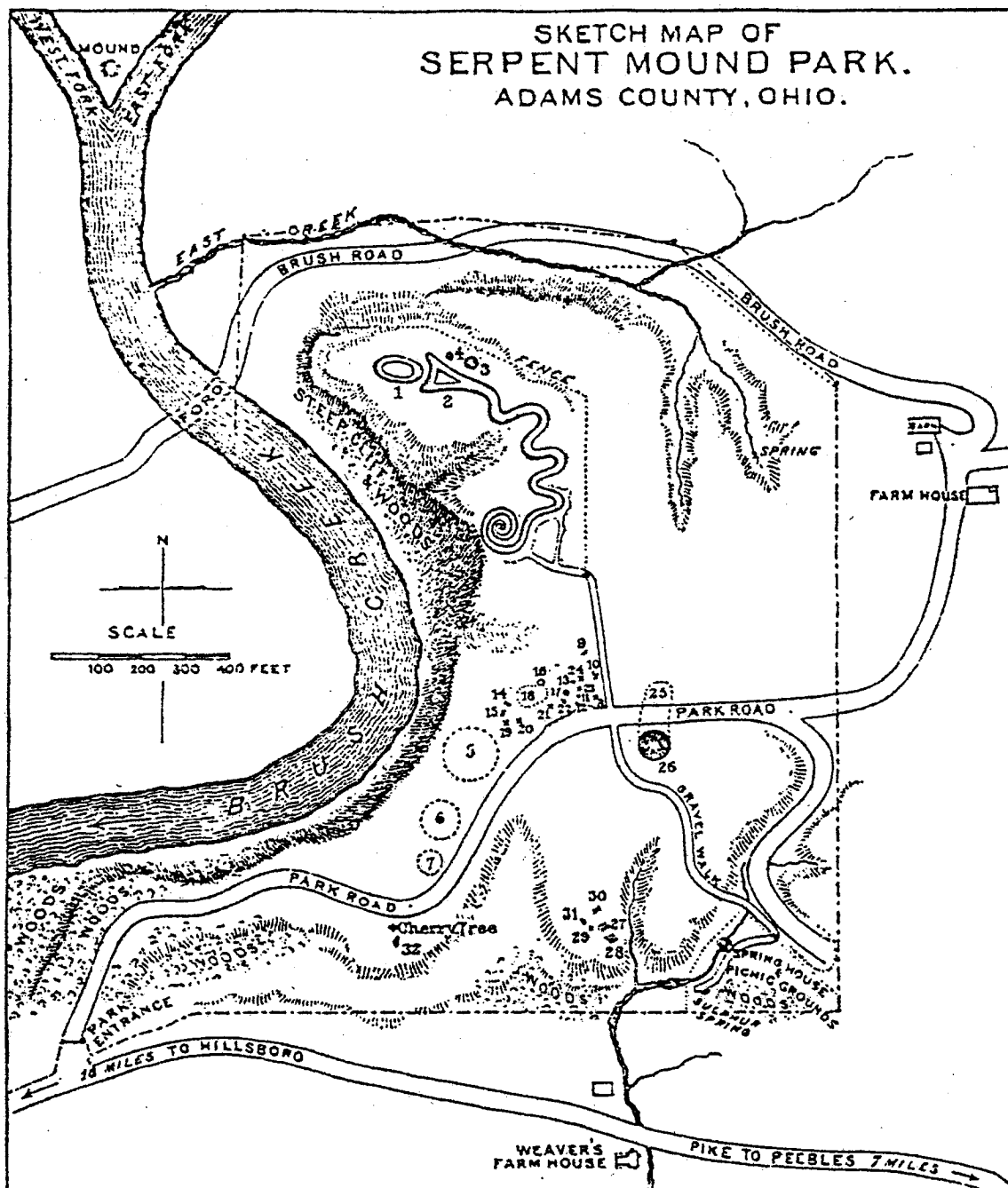


Figure 6.16. Profile of oval embankment, adopted from field drawing above and used in final report. Not to original scale. Source: F.W. Putnam, "The Serpent Mound of Ohio," Century Magazine 39 (1889-90), 875.



1, The Oval Embankment in front of the serpent's mouth. In this inclosure is a small mound of stones. 2, The Serpent. 3, A low Artificial Mound near the head of the serpent. 4, A very small Artificial Mound just west of 3. 5, 6, 7, Ancient Excavations, appearing like sink-holes. 8, 19, 20, 21, 22, 23, 24, and in space bordered by 18, 15, 20, 21, are Sites of Ancient Habitations. 9, Burnt Stones on the clay. 10, A recent Indian Grave over two graves. 11, Portions of Three Skeletons in a pile. 12, 13, Skeletons in the clay. 14, Grave with Two Skeletons. 15, Grave with Skeleton, over which was an ash bed. 16, Pieces of a large Clay Pot. 17, Small Burial Mound. 18, Several small Excavations in the clay, filled with dark earth. 19, 24, See above. This Village Site was afterwards found to extend 200 feet east and south. 25, Burnt space under the dark soil extending to the edge of large conical mound. 26, The Conical Mound, a monument over a single body. 27, 28, Cremation Places in the clay under the dark soil. 29, 30, 31, Very Ancient Graves deep in the clay. 32, Small Mound over four ancient graves in the clay.

Figure 6.17. Putnam's map of Serpent Mound Park. Source: F.W. Putnam, "The Serpent Mound of Ohio," *Century Magazine* 39 (1889-90), 874.

clearing the disturbed surface soils, many features were discovered at various depths within the yellow clay, from hearths and various kinds of pits to a number of graves. Thousands of stone fragments were found throughout the area—flint flakes, as well as rough pieces of jasper and quartz in various stages of manufacture. Hundreds of finished stone implements were found largely in proximity to the hearths. These included hammers, chisels, knives, drills and perforators, various types of projectile points, and various stone ornaments. Numerous pottery fragments and bones of many different kinds of animals were also recovered. Approximately twelve burials were discovered in this region (some were mixed together). These graves were found at various depths with little uniformity evident in their construction from one to another. In one case, a recent burial (similar in character to stone graves on the nearby hill to the north) overlaid a much older one set deep into the clay within a lining of flat stones and creek cobbles (Figure 6.19). Over another burial excavations revealed evidence of the base of a small mound that could no longer be seen from the surface. In yet another grave, portions of three skeletons had been laid prior to placement of three skulls in a bundle. Putnam further found significant anatomical variability among the recovered skeletal remains here—the skull from the stone-lined burial, for instance had much thicker bones than the others. Taken together, the sum of evidence from the village area alone demonstrated that the site had been occupied by successive groups of people over a long period of time.

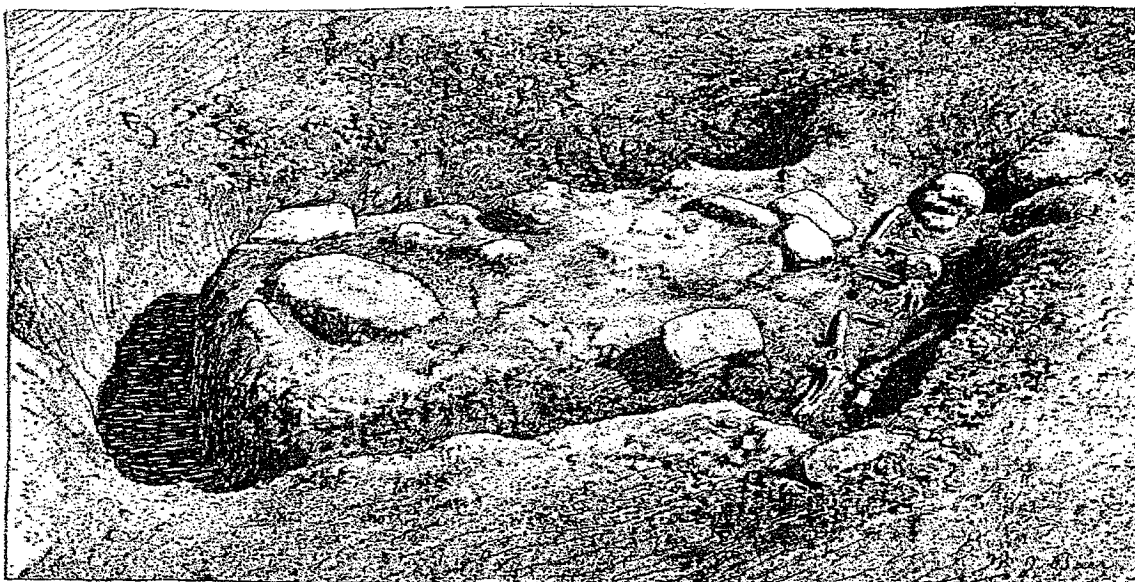
Excavations about the large conical mound (26 on the map) yielded similarly intriguing results.⁸⁴ I have already mentioned some of these in the section on methodology above, such as that the mound had been constructed over a single central grave and over

⁸⁴ Putnam, “The Serpent Mound of Ohio,” 880-3.



BURIAL-PLACE UNDER VILLAGE SITE, SERPENT MOUND PARK; LARGE CONICAL BURIAL-MOUND ON RIGHT IN MIDDLE DISTANCE. THE STAKE SHOWS POSITION OF SKELETON IN CLAY.

Figure 6.18. Excavations at village site. Source: F.W. Putnam, "The Serpent Mound of Ohio," *Century Magazine* 39 (1889-90), 878.

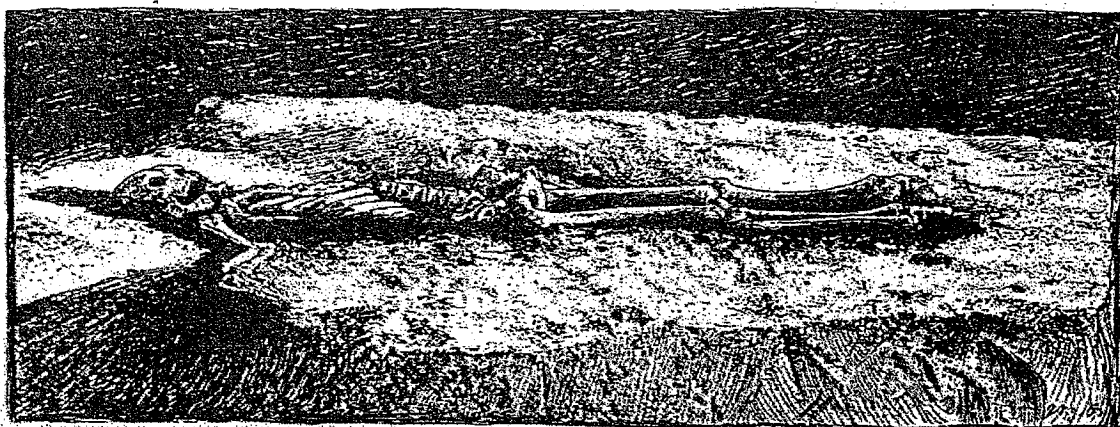


RECENT INDIAN GRAVE, SHOWING POSITION OF BURIAL OVER AN ANCIENT GRAVE MARKED BY THE STONES.

Figure 6.19. Excavation of recent grave overlying older one, in the vicinity of village site. Source: F.W. Putnam, "The Serpent Mound of Ohio," *Century Magazine* 39 (1889-90), 879.

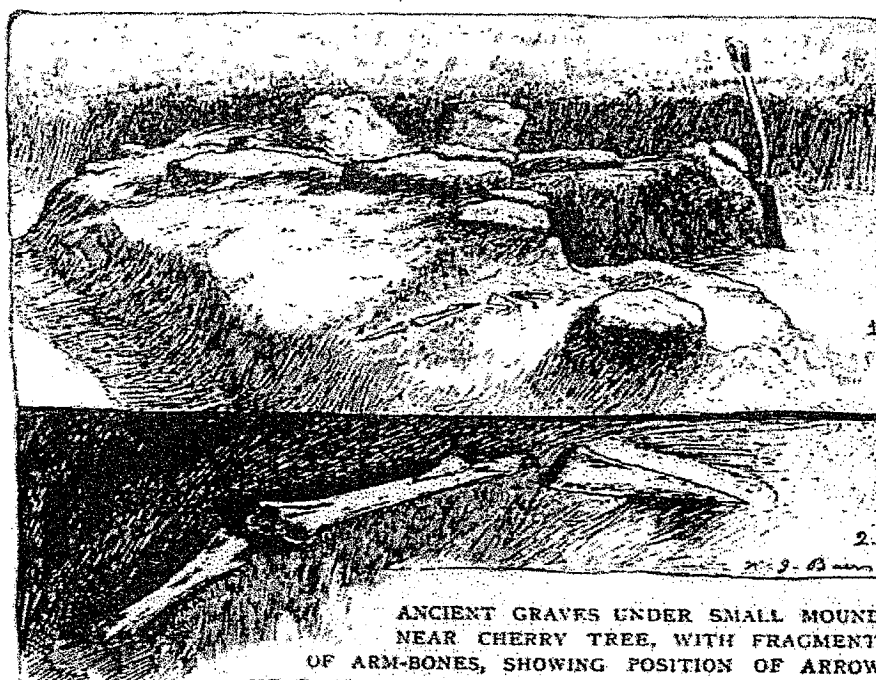
time a number of additional burials were added by later peoples. In conjunction with the trenching and slicing through the mound itself, excavations had been made throughout the immediate surrounding area, revealing a large burned zone (25 on the map) extending northward from the base of the mound containing numerous pottery shards, burned bone, and stone fragments. Putting this together with details of the mound's construction yielded a highly complex interpretation of the ceremony involved in the principle grave. Beneath the mound were outlines of quite a number of holes of various sizes that Putnam reasoned were intentionally dug and filled with specific objects such as stones or animal bones. A clay platform had next been raised over these ceremonial pits. Upon the platform, a large fire was constructed in order to form a thick ash bed which was heavily supplemented with ashes gathered from the burned zone extending north from the mound. The full body of an adult male, nearly six feet tall and twenty-four to thirty years old (Figure 6.20), was placed upon the hot ash bed with few material accoutrements and covered with another layer of clay and subsequently the remainder of the mound. The brachycephalic (or "short-headed") skull had been somewhat crushed by the weight of the earth above, but not so severely that Putnam couldn't glean a few interesting anatomical details from it. This individual never had any wisdom teeth, for one thing, and the cranium exhibited unusual suturing in the frontal bones, a feature generally more characteristic, Putnam claimed, of the longer dolichocephalic skulls of white people. (Putnam did not then argue that white people were present at the site at some remote date; on the contrary, this evidence merely reinforced his conviction of great diversity among the various prehistoric peoples of ancient America.)

Excavations also revealed four poorly preserved graves beneath the small oblong



SKELETON ON ASH BED AT BOTTOM OF CONICAL MOUND.

Figure 6.20. Central burial in large conical mound. Source: F.W. Putnam, "The Serpent Mound of Ohio," *Century Magazine* 39 (1889-90), 880.



ANCIENT GRAVES UNDER SMALL MOUND, NEAR CHERRY TREE, WITH FRAGMENTS OF ARM-BONES, SHOWING POSITION OF ARROW-HEAD AND DAGGER-POINT.

1, Position of stones covering ash bed and graves in mound. 2, Enlarged drawing of arrow-point on upper part of right arm and dagger-point on right chest; original objects now in Peabody Museum.

Figure 6.21. Details of graves from small mound near cherry tree. Putnam speculated that the projectile point had been embedded in the arm prior to death. Source: F.W. Putnam, "The Serpent Mound of Ohio," *Century Magazine* 39 (1889-90), 884.

mound (32) near the cherry tree to the southwest of the borrow pits (Figure 6.21). Three of these were situated closer to the surface, all of them overlying an older central stone-lined grave sunk deep into the clay beneath the mound. A variety of artifacts were recovered throughout.

Putnam was particularly intrigued by the features located on an outcrop of the plateau east of the cherry tree knoll.⁸⁵ Upon removing the surface soils, five groups of stones were revealed. Two were associated with hearths, three covered graves (numbers 27-31 on the map). One grave contained a single badly decomposed skeleton, another contained three skeletons, all likewise poorly preserved. The third grave (at 29) contained two deteriorated side by side skeletons underlying a thick layer of mixed clay, ash, and charcoal. This layer also contained numerous animal bones, and finished bone and stone implements. Two characteristics set this grave apart from others. The first was that the skulls were missing, and Putnam thought that the bodies had in fact been decapitated prior to burial (Figure 6.22). The second anomalous feature of the burial was that it contained, in Putnam's own words, "the largest number of objects I have ever seen in a single grave."⁸⁶ These included numerous finely finished stone projectile points and knives, stone hatchets, masses of red ochre, bone tools, a rare "ornament cut from a crystal of galena," as well as masses of burned material containing fragmentary artifacts and numerous bits of bone—many positively identified as human. Many of the finely crafted objects appeared to have been placed in ritualistic fashion about the skeletons. The burned material matched the character of that found in one of the nearby hearths,

⁸⁵ Putnam, "The Serpent Mound of Ohio," 885-7.

⁸⁶ Putnam, "The Serpent Mound of Ohio," 885.

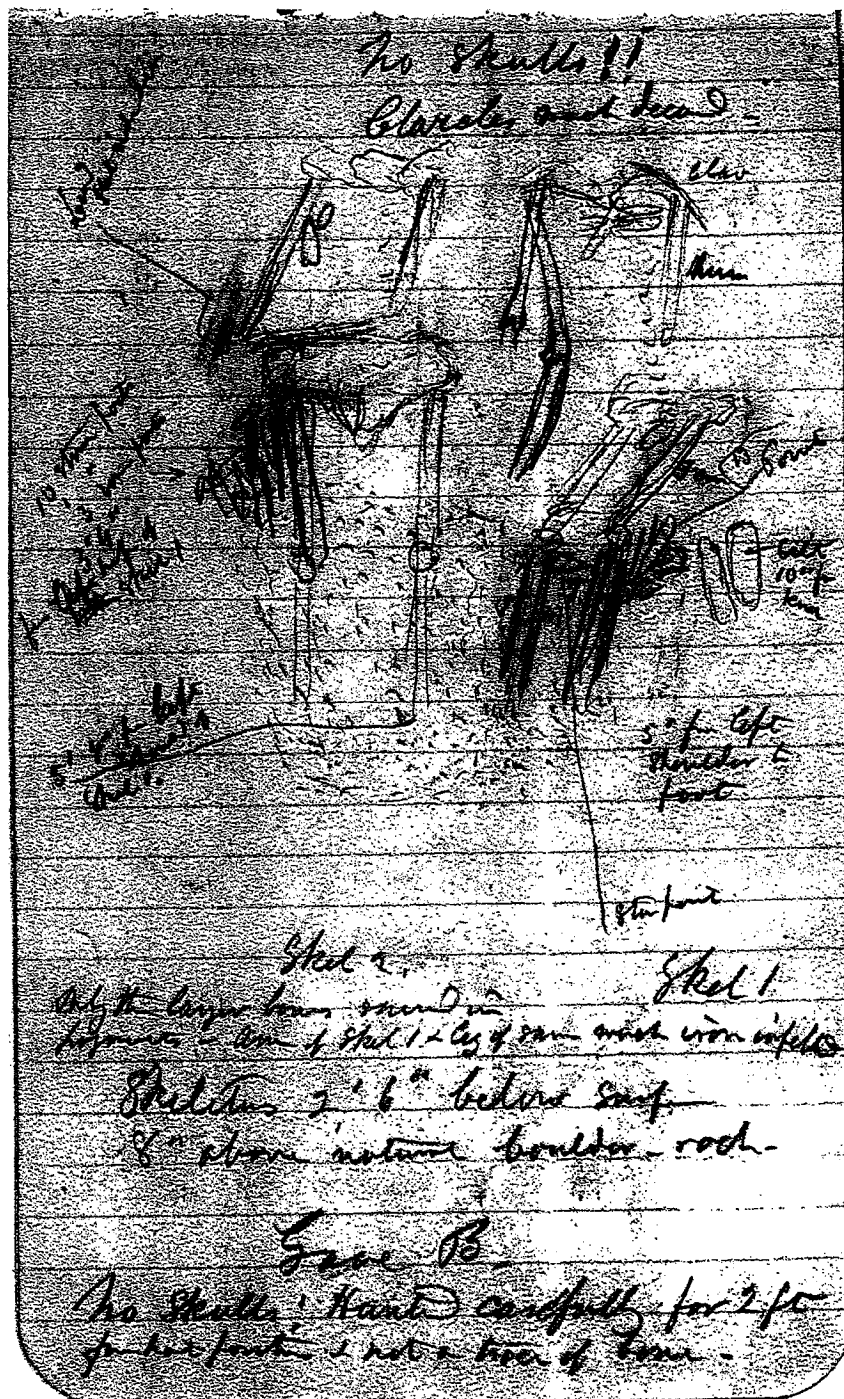


Figure 6.22. “No Skulls!! Clavicles much decayed.” Sketch from one of Putnam’s field notebooks showing the side-by-side decapitated skeletons. Source: F.W. Putnam, Unlabeled notebook, PM/AF 88-55, Folder 3.

leading Putnam to conjecture an elaborate yet somewhat gruesome ceremony having taken place here. After the two headless individuals had been interred, the burned remains of a third individual were placed over their legs and feet.

Of course, there was no means for obtaining absolute dates for any of the site components, so Putnam could provide only educated estimates of relative ages and draw comparisons to other situations he had encountered elsewhere. This was all any archaeologist in the nineteenth century could do, really, so the more experience one had the better. Putnam made sure to let readers of his Serpent Mound report know that he had explored literally “thousands of graves, under many and varied conditions or burial in various parts of the country, during nearly a quarter of a century of active field-work,” and was thus able to make a determination as to the exceptional antiquity of a number of the graves at Serpent Mound.⁸⁷ Those near the cherry tree and, especially, those on the hill to the east were the oldest on the site. “I have seldom found more conclusive comparative evidence of antiquity,” Putnam wrote of these burials, which was no inconsiderable statement in light of his experience and his views on prehistory.⁸⁸ Certainly, Putnam reckoned the age of these particular graves in the thousands of years, perhaps even as much as ten thousand years for the oldest. He was not so forthcoming, however, about the exact comparative evidence. The crude nature of the burials as well as conditions of skeletal preservation and comparative anatomy were likely important clues; so, too,

⁸⁷ Putnam, “The Serpent Mound of Ohio,” 884.

⁸⁸ Putnam, “The Serpent Mound of Ohio,” 884.

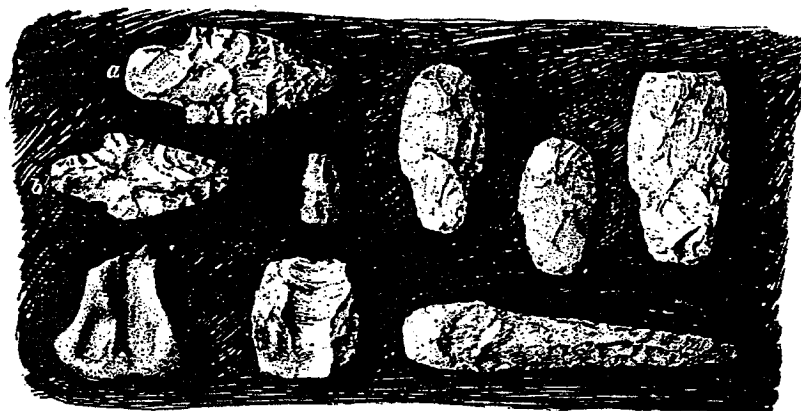
would have been the forms of stone implements found in these graves (Figure 6.23).⁸⁹

Whatever the case, his readers simply had to take his word for it that these were old burials, and when Putnam said old, he was talking about considerable antiquity. Quite interestingly, he further associated the cherry tree graves directly with “the people who worshiped at the serpent shrine,” but, again, did not provide any further details as to exactly what evidence suggested the link.

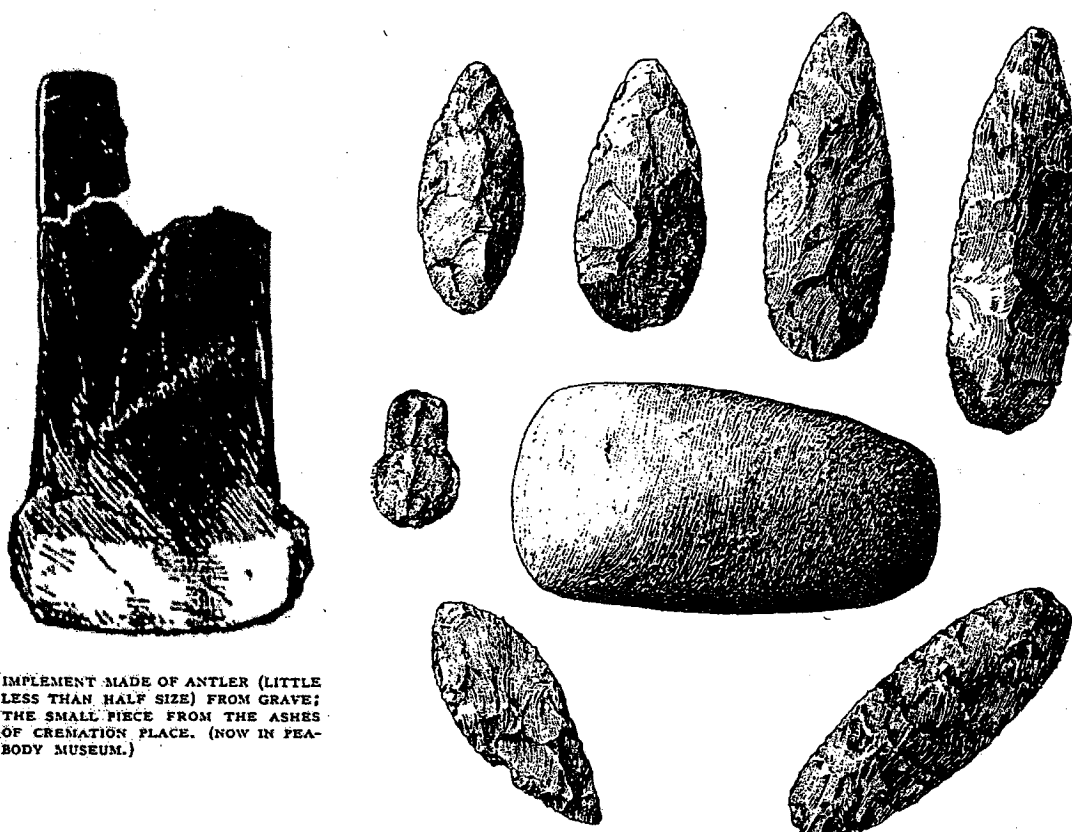
If there was a single overarching theme or point to Putnam’s Serpent Mound report it was that the site had attracted many different peoples over a long time span. The range of artifact styles, burial practices, and anatomical evidence clearly pointed to such diversity, and, in Putnam’s words, to the “great drama of life” that had occurred over the ages. He also unambiguously interpreted the effigy serpent and oval as religious structures, and even if he did not use Squier and Davis’s term “Sacred Enclosure,” Putnam certainly thought of the earthwork that way. In fact, he asserted it was the overall religious character of the site that had drawn people to it over the eons. The awe-inspiring topography of the place had as much to do with it as the mound itself. And so it is with little surprise that Putnam’s preparation of Serpent Mound Park involved restoring the effigy roughly according to the data provided by Squier and Davis.

Putnam went considerably further with his discussion of religious symbolism and comparative ethnology, explicitly hypothesizing Asiatic origins for the ancient American

⁸⁹ Putnam was, in fact, a gifted practicing anatomist (he trained, remember, under Louis Agassiz at the Museum of Comparative Zoology), and not only after the fact in the lab after fieldwork was completed. His Serpent Mound notebooks contain numerous on-the-spot analyses and it is clear that was aware in the field of anatomical diversity at the site.



CHIPPED FLINT IMPLEMENTS AND SHARPENING STONE (REDUCED TO ONE-THIRD SIZE) FOUND NEAR LEFT SHOULDER OF SKELETON. *a*, FLINT KNIFE FROM MASS OF RED OCHER WITH SKELETON; *b*, FLINT KNIFE FROM BETWEEN THE KNEES OF SKELETON. (NOW IN THE PEABODY MUSEUM.)



IMPLEMENT MADE OF ANTLER (LITTLE LESS THAN HALF SIZE) FROM GRAVE; THE SMALL PIECE FROM THE ASHES OF CREMATION PLACE. (NOW IN PEABODY MUSEUM.)

STONE HATCHET AND KNIVES OF FLINT (LESS THAN HALF SIZE) FOUND NEAR LEFT KNEE OF SKELETON. (NOW IN THE PEABODY MUSEUM.)

Figure 6.23. Some of the artifacts recovered in association with the side-by-side decapitated skeletons. The antler implement at lower left provided conclusive proof that burned material from the nearby hearth had been deposited over the legs of the interred bodies—the large piece was found mixed within the material re-deposited in the grave, whereas the smaller—exactly matching piece was recovered from the hearth (or, “cremation place”). Not to original scale. Source: F.W. Putnam, “The Serpent Mound of Ohio,” *Century Magazine* 39 (1889-90), 884 and 886.

serpent imagery.⁹⁰ In his own words, he posed the question:

Will it be forcing the facts to argue—unless all religious symbolism is merely coincident, vague, and meaningless, which seems an absurd position to hold—that in the oval embankment, with its central pile of burnt stones, in combination with the serpent, we have the three symbols everywhere regarded in the Old World as emblems of those primitive faiths? Here we find the *linga-in-yoni* of India, or the reciprocal principles of nature guarded by the serpent; or life, power, knowledge, and eternity.⁹¹

The “emblems” of primitive faith in the New World shared too many similarities with those of the Old to be a mere chance occurrence, and pointed strongly to common origins not just for religion, but, by extension, for at least some human populations as well.

6.9 Reactions and forms of reasoning.

At precisely the same time Putnam was writing up his Serpent Mound report, Cyrus Thomas of the Smithsonian Bureau of Ethnology was assembling his “Report on the Mound Explorations of the Bureau of Ethnology,” which appeared in the Twelfth Annual Report of the Bureau for 1890-1 but was not actually printed until 1894. This substantial 700-plus page production incorporated data from surveys and explorations of hundreds of mound and earthwork sites throughout the greater Mississippi valley, and it contained numerous definitive statements about moundbuilders, very few of which accorded with Putnam’s views. In a lengthy introduction, Powell himself summarized the “ultimate conclusions” of the Bureau’s mound report. Number one on the list was that the mounds were not of great antiquity. Number four was that “none of the mounds were built for religious or sacred purposes.” The final and most essential overall point from the

⁹⁰ Putnam, “The Serpent Mound of Ohio,” 870-1.

⁹¹ Putnam, “The Serpent Mound of Ohio,” 870.

Bureau's point of view was that "the links of evidence connecting the Indians and mound-builders are so numerous and well-established as to justify archaeologists in assuming that they were one and the same people."⁹² As I have discussed earlier in this chapter and in the previous chapter, Powell and Thomas had organized the Bureau mound survey in order to show that the moundbuilders were immediate ancestors to extant Indian populations. The methodological core of the project was modeled on the linguistics surveys which represented the principle bulk of Bureau work: begin with a wide sampling of known facts about modern or historic Indian tribes and then work backward to find points of agreement. Questions of ultimate origins were simply barred from Bureau discussions. Bureau archaeologists, furthermore, did not conduct intensive, long-term excavations on individual sites. Instead, they sought to gather data from as geographically broad a range of sites as possible and to relate that data to the historically known locations of various tribes. This kind of approach contrasted sharply with that taken by Putnam.

The Bureau aims were no secret, either, and had been extensively promoted in a number of interim reports published in the lead up to the final one, as well as by the archaeologists themselves in their dealings with other archaeologists. I have already mentioned that Putnam guarded his Ohio data out of concern it be used to support "Major Powell's theory." In the summer of 1889, Peabody correspondent Charles Abbott contacted Putnam, who was in the field in Ohio, and wrote, "I assume you are in camp [at Serpent Mound] and still solving the problem of the 'Serpent,' which is no mystery now

⁹² J.W. Powell, "Introduction," Twelfth Annual Report of the Bureau of Ethnology for 1890-1 (1894), xlvii-xlviii.

Cyrus has had his say.”⁹³ The sarcasm was intentional. Thomas had just released two special Bureau reports, and one was entitled The Problem of the Ohio Mounds. Here, the Cherokees were credited for many of the Ohio valley works and monuments.⁹⁴ The second report, The Circular, Square, and Octagonal Earthworks of Ohio, presented re-survey data of a number of Ohio works and concluded that the moundbuilders did not possess advanced knowledge of geometry.

Putnam did not like making such conclusive pronouncements, and he strongly disagreed with what he saw as the Bureau’s unnecessarily self-limiting methods of archaeological interpretation. Although he did not mention the organization by name, the final paragraphs of his Serpent Mound report were clearly intended as a criticism of the Bureau’s insistence on starting with known history and working backward. Historical knowledge, Putnam thought, can have its place in archaeological reasoning, but one must be very careful—particularly considering the major dislocations and displacements that had occurred among Indian groups since the arrival of Europeans. Such historical knowledge must be “critical and trustworthy” and “not the simple patchwork of vague generalities.”⁹⁵ To look in the archaeological record for data relevant to present conditions, therefore, could blind one to the possibility that the distant past was very different from the present. Putnam thus urged archaeologists to approach the “earliest traces of man” with an open mind and work forward from the prehistoric past literally to

⁹³ Charles C. Abbott to F.W. Putnam, 7 July 1889, Harvard University Archives, Pussey Library, Unit HUG1717.2.1 “Putnam Papers, General Correspondence,” Folder “A.”

⁹⁴ Cyrus Thomas, The Problem of the Ohio Mounds (Washington, DC: Government Printing Office, 1889).

⁹⁵ F.W. Putnam, “The Serpent Mound of Ohio,” 887-8.

the “dawn of history, when language, myths, and legends open the door to the historian.”

He continued:

To work in the other direction, with the idea that language and history can tell us of the relations of mankind in early times, is starting with the assumption that man has everywhere, and in all past times, been as now; that there has been no development within a race, no diversity of races, no migrations of peoples, except in recent times, and no change in the innumerable languages now spoken.... Archaeology leads us to believe that man existed so long ago that the thousands of years since man in America hunted at the foot of the glaciers are but as yesterday compared with his probable but still unknown origin.⁹⁶

These views on archaeological reasoning represented the polar opposite to the Bureau’s approach to archaeology, and in many ways were much more closely allied to the thinking of individuals like Ephraim Squier and Stephen Peet.

6.10 Concluding remarks.

My goal in this chapter was to provide a narrative of nineteenth century archaeological accounts of Serpent Mound, paying particular attention to the work done at the site by Frederic Ward Putnam (and his colleagues). I have presented Putnam’s working field methods in detail for a number of reasons.

First, his work represented the first thoroughly systematic investigation of the site, and his resulting interpretations were vastly more nuanced and sophisticated than others. In and of itself, this is hardly surprising or indeed all that interesting. What makes it so is the fact that Putnam saw the site in fundamentally different terms from other archaeologists who examined and wrote about it, and he saw it in such a way long before a single shovelful of earth was ever turned. For Putnam, any account of Serpent Mound had to take into consideration details of the site other than the effigy itself, which is to say

⁹⁶ F.W. Putnam, “The Serpent Mound of Ohio,” 887.

that there was more to Serpent Mound as an archaeological site than just the serpent. Although Squier and Davis had documented the nearby conical mound in the 1840s, any attempt at integrating its presence into an overall understanding of the area was simply left aside to “grander” ethnological speculation about serpent symbolism and the development of primitive religion. With some qualification, the same might be said of the accounts provided by Peet, MacLean, and even Holmes, each of which were made after Putnam had further alerted the archaeological world to the presence of a village area in close proximity to the serpent. Why did nobody else wish to mention this detail, which would seem essential for understanding what might actually have occurred at the site in prehistoric times? The answer, quite simply, is that these other archaeologists were not attempting to work out what happened.

Wondering why serpent motifs appeared in both America and Asia, on the one hand, and why a village site or burial area was located so closely to the serpent earthwork, on the other, were two very different kinds of questions, and required different kinds of evidence to answer them. Squier and Davis, Peet, and MacLean were interested in the first question, and from each of their perspectives it was thus perfectly reasonable to abstract the serpent effigy from all other local context and to bicker about what the effigy actually represented and how it was topographically situated. Holmes and the Bureau of Ethnology archaeologists wanted to limit what they saw as over-speculative and unsubstantiated comparisons between New and Old World contexts by tying the serpent symbol to known conventions in native American art. This was a perfectly reasonable move; nonetheless it, too, devolved into a similar method of abstraction. Putnam, for his part, was certainly interested in the first question, but he was more interested in the

second and others like that one. For him, archaeological interpretation of Serpent Mound went hand-in-hand with detailed knowledge of the site's many constituent components. Serpent Mound had not one, but many stories to tell.

I would like to suggest that in Putnam's developing views over the 1880s we do see some sort of individual gestalt shift taking place in his thinking about the very nature of archaeological evidence and about the constitution of archaeological sites. This shift, I would argue, was directly tied to the contingencies of his field experience. It originated with his work with Metz in the Little Miami valley and it impacted his initial view of Serpent Mound shortly thereafter. I have pointed out that Putnam and Metz performed intensive, long-term sub-surface excavations along the Little Miami. However, these did not start out with such intentions. A change occurred in the early stages, sometime during the 1882 field season. In Putnam's words, "we have been led to subterranean explorations where at first we supposed that all of importance was to be found in the tumuli above the level of the surrounding land."⁹⁷ Metz and Putnam had started off examining only the above ground mounds according to the careful methods Putnam had at least informally worked out some time earlier.⁹⁸ But as they excavated the mounds away, other features revealed themselves and needed to be followed out—layers of burned clay, mysterious pits, an even more mysterious subterranean stone wall. Just as it became apparent that a lot of important stuff was not only not in the mounds, but everywhere under the ground, they ran out of money. Luckily, Metz secured "rights of exploration" from the requisite landowners, and the Peabody generated additional funds for expenses. The mounds could

⁹⁷ Putnam, "Archaeological Explorations in Ohio and Wisconsin," 209.

⁹⁸ Browman, "Origins of Stratigraphic Excavation," 248.

be blindingly seductive in their magnificence, but given the right opportunities to broaden investigation in the field, Putnam did.

Putnam's work at Serpent Mound was also conditioned by his theory of prehistoric America as a place where numerous peoples migrated about, sometimes mixing together, sometimes splitting apart, over a very long period of time that extended at least as far back as the last glacial period. These views ran counter to those of the Bureau of Ethnology archaeologists, whose working theory of North American prehistory was built upon the notion that ancient populations were much more static and stable entities that did not extend so far back in time. In and of itself, the contrast is an interesting and important part of the story. But it is also worth pointing out that Putnam's theory of prehistory and his intensive field methods emerged together—systematic and methodical excavations revealed the subtle distinctions between the traces of different peoples who had moved through an area over time, which in turn demanded careful field methods so as not to blur these distinctions. Admittedly, there is a bit of a chicken-and-egg element to this. I believe Putnam had begun thinking about prehistory in such terms prior to the early Ohio fieldwork, but I also think that the contingencies of this work itself reinforced and informed his theoretical commitments so that by the time he began excavating at Serpent Mound, the relationship between field method and theory was pretty clear in his mind.

Of course, one might counter that it really wasn't until Putnam had finished exploring the Serpent Mound location and had processed his discoveries into a coherent account that all the various site details became important archaeological evidence for a complex history of human presence at the site. It should not be surprising, then, to find other archaeologists ignoring these details and focusing solely upon the serpent effigy. To a

large extent, this is true. But take into account the additional depiction of Serpent Mound (Figure 6.24) to appear in Thomas's 1890-1 final report of the Mound Exploring Division, well after Putnam's Century Magazine article had appeared. The image, again abstracted from all other local context, was presented merely "for the purpose of comparison with other published figures."⁹⁹ Holmes had made this drawing on the basis of yet another visit to the site, this time in 1888 (after Putnam had completed the first heavy round of fieldwork—obviously, the Serpent was not entirely off-limits to Bureau archaeologists). All traces of topography had been excised from Holmes's new drawing, a move I am certain was intentionally made to discourage talk about the effigy's supposed relationship to the landform. In the accompanying terse five line entry, Thomas did not refer to any of Putnam's findings—no mention of the village site, the burial mounds, the borrow pits, nor any of the graves. Not the slightest mention that there was anything else to the site at all. This is a clear indication that Thomas and the Bureau archaeologists were not thinking about archaeological sites in the same terms as Putnam. One may dispute this claim, but it is inarguable that the Bureau archaeologists at the very least had chosen a different path.

On that note, I would not suggest that we think about Putnam's views as a necessary advance over others as part of a steady progression in understanding about moundbuilders. Instead, it is more instructive to recognize that there coexisted drastically different ways of reasoning about moundbuilders and about mounds as archaeological

⁹⁹ Thomas, "Report on the Mound Explorations of the Bureau of Ethnology," 493.

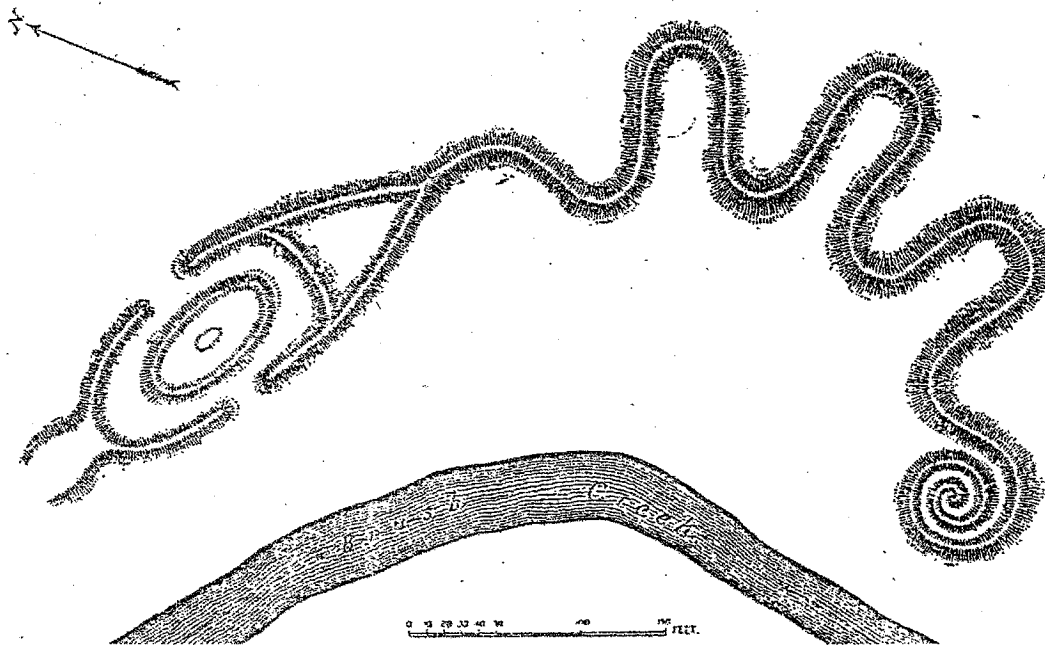


Figure 6.24. William H. Holmes made this plan view of Serpent Mound after visiting the site again in 1888. It is markedly different from his own earlier map (see Figure 5.6) of 1886, for which great pains were taken to depict the topography naturalistically using fine shading and hatching. This image shows the Serpent as though resting upon a flood plain level with Brush Creek. Source: Cyrus Thomas, "Report on the Mound Explorations of the Bureau of Ethnology," Twelfth Annual Report of the Bureau of Ethnology for 1890-1 (1894), 493.

sites.¹⁰⁰ One must keep this fact in mind when dealing with the subject of the mounds in the context of nineteenth century American archaeology, and I have hoped to show how this played out in the case of Serpent Mound. When, for example, Putnam and Thomas each spoke of “investigations at the Serpent,” or merely of the “Serpent mound of Ohio,” they were really talking about two different things, and from each respective purview, what counted as valuable archaeological evidence for making claims about the past differed greatly.

I have also covered Putnam’s Serpent Mound work in detail in this chapter in order to highlight the numerous contingencies that shaped the direction and nature of that work. Chief among these was certainly the negotiations for land purchase that went on between Putnam and the Lovetts. In conjunction with the Peabody fund raising campaign, this process resulted in the museum’s ownership of a substantial tract of land to be explored. Had Lovett not been ready or willing to sell, or had he agreed only to sell off a small portion of the ridge immediately around the effigy, the results in Putnam’s final report would have looked substantially different. I also pointed out that Metz’s cooperation and the cooperation of many others was essential to the course of fieldwork and that Putnam’s motives for creating Serpent Mound Park involved deeply personal feelings that had little

¹⁰⁰ Ian Hacking’s notion of “styles of reasoning” might be applied here. In Hacking’s estimation, comparing the language used by different groups of scientists in large measure reveals different styles. As I have addressed in Chapters 4 and 5, and in this chapter as well, Putnam and the Bureau archaeologists may all have used the term “prehistory,” but the theoretical frameworks underlying the use of the term were essentially incompatible with one another. See, Ian Hacking, *Historical Ontology* (Cambridge, MA: Harvard University Press, 2002), especially Chapter 12, “‘Style’ for Historians and Philosophers,” pp. 178-99.

to do with archaeology *per se* and more to do with his family and his desire to do something for the public good. Furthermore, Putnam's work at Serpent Mound must be seen at least in some measure as a product of inter-institutional competition between the Peabody and the Smithsonian, and within the context of the "networking" of archaeology that I discussed in the previous chapter. Thus, whatever changes may have occurred in Putnam's own mind about methodology over the course of the 1880s, his fieldwork, like that of any archaeologist, was subject to a multitude of influences. Accordingly, anytime we hear about Archaeologist X's investigation of Site Y, we must be aware that regardless of any employed standards of method, idiosyncratic factors often if not always shape the investigation in ways that are unique to that individual case.

Putnam's field notes themselves also provide us with some revealing insights. He was the foremost proponent and public spokesperson of his day for careful methods in the field. Yet, we have seen that his field records were often quite messy, and at times bordered on the indecipherable to probably anyone but himself. As is the case with any examples of scientific practice, it is always good for historians to maintain an awareness that the process of discovery is seldom if ever reflected with accuracy in the final product of research.

That said, Putnam himself was actually quite aware of the highly contingent and provisional nature of archaeology as a form of scientific practice. In February, 1885, he wrote encouragingly to geologist George Frederic Wright, one of the core members of the newly incorporated Ohio State Archaeological and Historical Society. After a long period of fits and starts, the OSAHS was at last up and running, a sign which Putnam thought to bode well for the future of archaeology in Ohio. Putnam implored the organization to take

action to preserve the state's rapidly disappearing monuments, and he urged that documenting the sites alone was insufficient. The conscientious investigator, he wrote, "must see the works for himself and study them with that minuteness of discrimination which modern science demands in all departments."¹⁰¹ And a "modern science" of archaeology for Putnam certainly did include methodological rigor, but it also meant "the constant re-examination of facts."¹⁰² By "facts," however, Putnam meant many things here—not just a sense of the works in their natural settings, but also knowledge of all other forms of data associated with a site. Archaeological interpretation as a whole was an ongoing process and continually subject to revision. It was not necessarily about formulating definitive statements and conclusions, since even supposedly established "facts" could change in light of later discoveries or interpretations.

In an article on "The Importance of the Study of Archaeology in Ohio" written for the inaugural issue of the OSAHS's quarterly, Wright quoted Putnam's full letter. In the same piece, Wright also included an excerpt from a letter he had recently received from Cyrus Thomas. Thomas's statement opened with more than a hint of institutional arrogance: "You can say to your society that the mystery of the mounds is being solved. We have now proof that the Cherokees were the authors...."¹⁰³ The difference in attitude could not

¹⁰¹ Letter reprinted in G.F. Wright, "Importance of the Study of Archaeology in Ohio," Ohio Archaeological and Historical Quarterly 1 (1887-8), 55.

¹⁰² Letter reprinted in G.F. Wright, "Importance of the Study of Archaeology in Ohio," Ohio Archaeological and Historical Quarterly 1 (1887-8), 55.

¹⁰³ Letter reprinted in G.F. Wright, "Importance of the Study of Archaeology in Ohio," Ohio Archaeological and Historical Publications 1 (1887-8), 59. Thomas was referring to evidence that would eventually make its way into his report on The Problem of the Ohio Mounds (Washington, DC: Government Printing Office,

have been more pronounced. For Thomas, there was no room for ongoing process.

Archaeology from the Bureau's point of view meant not just making but authoritatively issuing definitive statements. The mystery was being solved, alright—by the Bureau and not the members of the OSAHS.

Finally, the preservation of Serpent Mound represents, I think, the first true success story of nineteenth century American archaeology. In the face of the wholesale erasure of a vast majority of the “ancient monuments of the Mississippi Valley,” here, at last, efforts had been made to save just one. It could not have happened without Putnam's individual efforts, the cooperation of many others, and, ultimately, the institutional base provided by the Peabody Museum of American Archaeology and Ethnology. By any reckoning, however, it was only a qualified success. Although an important precedent had been set, no widespread preservation movement immediately followed, and the destruction of mounds continued unabated right through the first half of the twentieth century. It is a sad fact that relatively few people today know much about America's pre-Columbian moundbuilding civilizations.

We must not forget, too, that archaeological excavation is an inherently destructive activity, and no matter how carefully one documents each step of the process or keeps

1889). Here, Thomas drew on historical evidence about the known recent migrations of tribes, and he made direct associations between the “arts and customs” of the moundbuilders and modern Indian tribes, especially the Cherokees. The subject of burial practices took center stage, but he also pointed out similarities between archaeological materials recovered from mounds and materials in known use by Indians. Tying the evidence all together, Thomas made a case for the moundbuilders being ancestral at least to the Cherokees.

track of specimens and their contexts of recovery, in the end these records are all that is left. The original site is more or less gone depending on the scale of excavation. Putnam's "restoration" work at Serpent Mound Park meant that visitors would see only a facsimile of the original, an interpretation of the site as Putnam thought it once looked.

In 1900, the Park was turned over to the state of Ohio.¹⁰⁴ Since that time, archaeologists have continued to debate the representational elements of the effigy.¹⁰⁵ Newer techniques such as radiocarbon dating have shown the serpent effigy to have been built around AD 1070, around the time of a spectacular appearance of Halley's Comet.¹⁰⁶ Other areas like the village site appear to date to a much earlier period, though not nearly so old as Putnam would have had it.¹⁰⁷ Archaeoastronomers have suggested that the effigy might have served as an astronomical calendar. Geologists have identified that the site sits within a bizarre geological disturbance, some four miles in diameter, that they have named the "Serpent Mound Cryptoexplosion Area."¹⁰⁸ At the same time, the Great Serpent of Adams County continues to elicit a considerable range of popular

¹⁰⁴ Robert C. Glotzhober and Bradley T. Lepper, Serpent Mound: Ohio's Enigmatic Effigy Mound (Columbus, OH: Ohio Historical Society, 1994), 13.

¹⁰⁵ See, for example, Charles C. Willoughby, "The Serpent Mound of Adams County, Ohio," American Anthropologist 21 (New Series, 1919), 153-63.

¹⁰⁶ Glotzhober and Lepper, Serpent Mound, 9.

¹⁰⁷ The earlier occupation is attributed to the "Adena" culture, whereas the later "Fort Ancient" culture is thought to have been responsible for the effigy construction. See, Glotzhober and Lepper, Serpent Mound, 5-6.

¹⁰⁸ Glotzhober and Lepper, Serpent Mound, 15-6.

speculation.¹⁰⁹ In many ways, the site remains as much a mystery now as it did when Squier and Davis first documented it.

¹⁰⁹ See, for instance, www.greatserpentmound.org.

Chapter 7.

Conclusion.

By the end of the 1880s, it would have been virtually impossible for anyone interested in Ohio valley archaeology to have remained completely unaware of the work done there over the previous decade by Smithsonian Bureau of Ethnology and the Peabody Museum of American Archaeology and Ethnology. Cyrus Thomas and Frederic Ward Putnam had made their presences strongly felt throughout southern Ohio. Hobbyist archaeologists who lived there continued doing what they had always done throughout the century, but this time with a much stronger awareness of the importance of institutional centralization—as well as to the implications and possibilities associated with the process. Individuals could now more easily participate in archaeology by becoming part of the networks linking farm fields and collectors' cabinets with the museum shelves and document storage spaces of the Smithsonian or the Peabody. Individual movement through these fluid networks, however, ultimately depended on the support—or not—of those higher up in the chain. Thus, by 1890 it had become much clearer that archaeological authority and expertise was a matter of institutional identity: the more one could identify with either the Smithsonian or the Peabody, the more seriously they were likely to be taken as archaeologists.

The mounds and earthworks of the Ohio valley became true objects of scientific inquiry within a developing science of ethnology in the first half of the nineteenth century. Ethnologists wanted to know how to situate the “Moundbuilders” within broader accounts of human history and development in North America. But, as we have seen, there were always nearly as many ways of doing ethnology as there were individuals

professing to be ethnologists. At the same time, a growing number of hobbyist archaeologists living in southern Ohio simply went about their business without being too much influenced by theoretical debates raging in far away places. After all, they lived among these spectacular sites, they were proud of their state's archaeological history, and they were responsible for generating a vast amount of the data that ultimately became important for later archaeologists. At the end of the 1880s, despite the Smithsonian and Peabody efforts at centralization, the long-standing tradition of avocational archaeology in southern Ohio remained as vital as ever. Likewise, farmers continued churning artifacts out of the ground and gathering them into personal collections. In this sense the history of archaeology told through the people "on the ground" looks rather different from a history that focuses on the theorists.

Whether or not, as Barbara Alice Mann has suggested, the "moundbuilder myth" was a consciously adopted fiction born in racism that eased the guilt of Indian removal in the early nineteenth century, is a deeply important question for responsible American citizens of today to ponder.¹ That said, I do think it is possible to talk about the conceptual difficulties faced by ethnologists and archaeologists throughout the nineteenth century as they struggled to define what they did as a form of scientific practice. In hindsight, we might say that at least one central issue lay at the heart of these difficulties: the nature of diversity, in both the archaeological record and among living Native American societies. Monogenism and polygenism, moundbuilders and Indians, extreme antiquity or more recent occupation, Asian or other origins, diffusion or independent invention, all of these perspectives have been tied in some sense to the ways archaeologists and ethnologists

¹ Barbara Alice Mann, Native Americans, Archaeologists and the Mounds (New York: Peter Lang, 2003).

chose to classify their evidence. What we have seen is that by and large until late in the century most chose to fit new evidence within existing typologies rather than allow the evidence to break those categories down. Thus, despite mounting evidence that considerable variability existed across the range of greater Mississippi valley data, the concept of a singular “race of the mounds” had become so deeply entrenched into the ways most archaeologists thought about the past as to stand as a given as unassailable as the law of gravity. We have seen how this archaeological concept was formalized in Squier and Davis’s 1848 Ancient Monuments of the Mississippi Valley.

Along the way, there were dissenters such as Daniel Wilson who just after mid-century criticized the American ethnological community as a whole for its failure to meaningfully acknowledge differences. Wilson’s was a minority position, and it was not until much later in the 1880s through the work of the Bureau of Ethnology and the Peabody that archaeological typologies began breaking down in ways that gained wider acceptance. The various ancient aboriginal peoples who built the mounds came to be regarded as part of the same continuum of North American history as were living Native Americans. Of course, we saw that the end result was not the emergence of a consensus. Bureau and Peabody archaeologists developed models of prehistoric North America that were as radically different from one another as were those archaeologists’ methods of reasoning about the prehistoric past.

Of course, we must explicitly acknowledge the extent to which archaeological knowledge about the moundbuilders was shaped in a variety of ways by the local or regional contingencies of fieldwork. Taking a cue from David Livingstone, I would like to emphasize very strongly the importance of geography and locality in the history of

Ohio valley archaeology.² By this, I do not simply mean that many mounds were located in southern Ohio, and for that reason alone, it became a region of archaeological interest. Livingstone, who does not explicitly discuss archaeology, calls into question the assumption that scientists—whether working in labs, museums, or the field—extract universal truths about nature. Instead, the knowledge they create is contingent on the physical setting of their work. This geographic contingency works on multiple levels for Livingstone, from the “macro” levels of national styles of science or the fact that field sciences will vary according to the contents of the region under scrutiny to the “micro” levels of behavioral norms among particular groups of scientists in particular places.

Obviously, there is much here that can be applied to the case of Ohio valley archaeology. At the macro level, we can indeed point to the fact that ancient people found in the Ohio River valley an eminently livable environment, and so they established long-term settlements there. We can also point to the fact that this was agriculturally rich land that was developed rapidly by Euro-American settlers after the War of 1812, and so there was a certain inevitability to the emergence of archaeological interest in the ancient works. Euro-Americans settling into southern Ohio in the early nineteenth century encountered these sites whether they wanted to or not.

Now, there is a bit of a problem with the way I have characterized things throughout this dissertation by equating “Ohio valley archaeology” with “southern Ohio archaeology,” because they are really not the same. After all, there was a Kentucky side of the Ohio valley that was also archaeologically rich, but this region did not become part

² David Livingstone, *Putting Science In Its Place: Geographies of Scientific Knowledge* (Chicago: University of Chicago Press, 2003).

of the discourse of mound archaeology in the nineteenth century in the same way that Ohio did (which is not to say that archaeological work did not take place there). While I have not looked into Kentucky's archaeological scene in great detail, I can at least suggest good reasons for the skewed emphasis on Ohio, reasons independent of whether or not Ohio was actually more interesting archaeologically. From a purely statistical point of view, Ohio's population was much denser along the Ohio River than was Kentucky's. I think that there were many more people involved in archaeology in some capacity in southern Ohio, and that the avocational networks to support or promote archaeology were better established there from early in the century. Without question, however, Squier and Davis's Ancient Monuments made Ohio—particularly the region surrounding Chillicothe—the seat of moundbuilder civilization, and as a result many subsequent archaeologists thought of the region in these terms. These sensibilities figured strongly into the growing sense of historical identity that characterized many Ohioan's views about their state, and that peaked with the Marietta Centennial in the late 1880s (discussed in Chapter 5), where discussions and presentations about archaeology comprised a large part of the proceedings. There is more to consider here, but without question, modern political boundaries of American states have shaped our geographical sense of the prehistoric past.

I have shown in Chapters 5-6 that contingencies at a far more localized scale need to be considered if we want to understand the operation of archaeological networks and the shaping of archaeological knowledge. As I have suggested, we need to broaden our conception of fieldwork in the context of institutional centralization, since only then can we begin fully to grasp the sorts of behavioral norms that characterized it and that

resulted in the generation of archaeological data. Individuals who worked as correspondent archaeologists maneuvered about in ways that were not always very predictable. They performed excavations, thus fulfilling our traditional ideas of fieldwork, but they also—perhaps more importantly—negotiated with farmers, with specimen collectors, and, as was often the case, with institutional authorities. For their part, institutional authorities who went into the field themselves behaved in similar ways. Even in the case of excavations, we need to expand our established notions of fieldwork. Archaeological sites themselves are not generally self-evident things, and site boundaries emerge as a result of many sorts of negotiations. In the case of Frederic Ward Putnam's work at Serpent Mound, we saw how all sorts of negotiations shaped the boundaries of the piece of land he was able to purchase and subsequently investigate. The end result was that Putnam was able to direct excavations that covered an extensive area of land, and he was then able to make some striking claims about the prehistoric people who once lived there.

I have focused at great length on the Smithsonian Bureau of Ethnology and Peabody Museum work of the 1880s because this was also when the explicit desire to centralize archaeology on a large scale drove the work, and the results of centralization significantly changed the organization and dynamics of archaeological practice. However, we have seen that the networking aspects of centralization have deeper roots going back to much earlier in the century. Networks of some form or fashion had been important for Ohio valley archaeology since well before the time of Squier and Davis, but it was through their association with Joseph Henry and the early Smithsonian that things began to change. The various associations Davis had established through years of avocational

archaeological work in southern Ohio made it possible to undertake such a large scale project with Squier, but so, too, did the fact that Henry agreed to support the publication of results. The subsequent promotion and distribution of Ancient Monuments by the Smithsonian made Davis's wealth of experience accessible to whole new audiences. The data of Ancient Monuments in a sense had been purified in the process of its becoming a Smithsonian publication: scholars did not doubt that Squier and Davis's representations were truthful and accurate, and as such those representations were cited widely for decades afterward by individuals who had never actually seen one of the monuments in its natural setting.³ Thus it was a matter of considerable concern to Smithsonian Bureau of Ethnology archaeologists in the 1880s when the accuracy of Squier and Davis's representations was called into question.

By the end of the 1880s, the Smithsonian and the Peabody were the two leading centers for archaeological research in the United States. They had become the sources for archaeological expertise. Soon enough, however, it became evident that the maintenance of disciplinary control and authority required a more efficient means of training and indoctrinating new archaeologists. In the early decades of the twentieth century, archaeology, along with the other sub-fields of anthropology, shifted location from

³ In Chapter 5, I noted that my views about centralization had been partly informed by Bruno Latour's notion of centers of calculation. Here is another sense in which Latour's work provides a further useful model. One of the things Latour says about fieldwork is that it often results in the creation of "immutable mobiles," by which he means that large-scale nature is somehow translated into a portable, stable form (such as a map) that can travel around and be processed at the centers of calculation. Bruno Latour, Science in Action: How to Follow Scientists and Engineers Through Society (Cambridge, MA: Harvard University Press, 1987), chapter 6.

museums to universities. The Peabody benefited from its close association with Harvard, whereas the Bureau of Ethnology's influence declined after the turn of the century.⁴

Either way, the nineteenth century histories of these institutions are best understood as a function of centralization. In 1887, Alfred Russel Wallace wrote about the existence of two distinct schools of American anthropology corresponding to Peabody and Bureau approaches to the mounds, as I discussed in Chapter 4.⁵ Centralization in an obvious sense was about bringing things to the centers. But it was also about discipline, about the drawing of ranks, the tightening of rules, the creation of boundaries. The long-term legacy of Peabody and Smithsonian influence on archaeology was disciplinary identity, but this did not come cleanly nor easily. As we have seen, archeological authorities from each of these institutions held differing views about the kind of discipline to be imposed, since methodological commitments went hand-in-hand with models of prehistory. Wallace's dichotomy, then, actually referred to the co-existence of differing Peabody and Bureau "disciplines."

Numerous historians have addressed issues of scientific identity and discipline formation, as well as processes of professionalization.⁶ Much of this extensive literature

⁴ Curtis Hinsley, The Smithsonian and the American Indian: Making a Moral Anthropology in Victorian America (Washington, DC: Smithsonian Institution Press, 1994), chapter 9; Curtis Hinsley, "The Museum Origins of Harvard Anthropology, 1866-1915," in Clarke A. Elliot and Margaret Rossiter, ed., Science at Harvard University: Historical Perspectives (Bethlehem, PA: Lehigh University Press, 1992), 121-145.

⁵ Alfred Russel Wallace, "American Museums: Museums of American Pre-Historic Archaeology," The Fortnightly Review 42 (1887), 675.

⁶ An excellent recent review of much of this literature is, Jan Golinski, Making Natural Knowledge: Constructivism and the History of Science (Chicago: University of Chicago Press, 2005), chapter 2. To the

has approached the issues from the setting of laboratory sciences, where matters of supervision, technique, and instrumentation have played key roles in the gradual creation of boundaries between experts and non-experts, between those who belong and those who do not. More relevant to my case here is the subset of this scholarship that has addressed the role of scientific societies or institutions in mediating between categories of amateur and professional, especially in relation to realms of natural history.⁷ One thing

literature covered by Golinski, I might also add: George H. Daniels, "The process of professionalization in American science: The emergent period, 1820-1860," in Ronald Numbers and Charles Rosenberg, ed., The Scientific Enterprise in America: Readings from "Isis" (Chicago: University of Chicago Press, 1999); Toby Appel, "Organizing biology: The American Society of Naturalists and its 'affiliated societies,' 1883-1923," and Philip J. Pauly, "Summer resort and scientific discipline: Wood's Hole and the structure of American biology, 1882-1925," in Ronald Rainger, Keith Benson, and Jane Maienschein, ed., The American Development of Biology (Philadelphia: University of Pennsylvania, 1988); Mary Jo Nye, From Chemical Philosophy to Theoretical Chemistry : Dynamics of Matter and Dynamics of Disciplines, 1800-1950 (Berkeley: University of California Press, 1993); Sally Gregory Kohlstedt, The Formation of the American Scientific Community: The American Association for the Advancement of Science, 1848-1860 (Urbana: University of Illinois Press, 1976); Alison Kraft and Samuel J.M.M. Alberti, "'Equal Though Different': Laboratories, Museums and the Institutional Development of Biology in Late-Victorian Northern England," Studies in History and Philosophy of Science Part C: Studies in History and Philosophy of Biological and Biomedical Sciences 34 (2003), 203-36; Adrian Desmond, "Redefining the X Axis: 'Professionals', 'Amateurs' and the Making of Mid-Victorian Biology," Journal of the History of Biology 34 (2001), 3-50; Andrea Rusnock, "Correspondence Networks and the Royal Society, 1700-1750," British Journal for the History of Science 32 (1999), 155-69.

⁷ For example, Sally Gregory Kohlstedt, "The Nineteenth Century Amateur Tradition: The Case of the Boston Society of Natural History," in Gerald Holton and William Blanpied, ed., Science and Its Public: The Changing Image (Dordrecht: Riedel, 1976); Sally Gregory Kohlstedt, "International Exchange and

that is clear from the vast diversity of approaches to the issue of identity is that it can be very difficult if not impossible to generalize processes of professionalization or discipline-formation.⁸ It can be very easy to assume a sort of teleological perspective in which categories of expert and non-expert, or professional and amateur are imposed into historical contexts where they do not properly belong. Daniel Goldstein, for one, has pointed out how frustratingly difficult it can be to use terms like “professional” and “amateur” in relation to American science prior to the twentieth century.⁹ I can concur. Throughout this dissertation I have tried (with much difficulty) to avoid using these terms precisely because they have virtually no applicability to the entire period I have considered. Instead, I have used awkward terms like “hobbyist” and “avocational” archaeology. The difficulty is that these terms seem to relegate these forms of

National Style: A View of Natural History Museums in the United States, 1850-1900,” in Nathan Reingold and Marc Rothenberg, ed., Scientific Colonialism: A Cross-Cultural Comparison (Washington, DC: Smithsonian Institution Press, 1987); Sally Gregory Kohlstedt, “Curiosities and Cabinets: Natural History Museums and Education on the Antebellum Campus,” Isis 79 (1988), 405-26; Samuel J.M.M. Alberti, “Amateurs and Professionals in One County: Biology and Natural History in Late Victorian Yorkshire,” Journal of the History of Biology 34 (2001), 115-47; A. Bowdoin Van Riper, Men Among the Mammoths: Victorian Science and the Discovery of Human Prehistory (Chicago: University of Chicago Press, 1993); Daniel Goldstein, “‘Yours for Science’: The Smithsonian Institution’s Correspondents and the Shape of Scientific Community in Nineteenth-Century America,” Isis 85 (1994), 573-99.

⁸ In noting this circumstance, Jack Morrell nonetheless states that generalized models of processes of professionalization can be useful tools for historians. See, Jack Morrell, “Professionalization,” in R.C. Olby, G.N. Cantor, J.R.R. Christie, and M.J.S. Hodge, ed., Companion to the History of Modern Science (London: Routledge, 1996), 980-9.

⁹ Goldstein, “‘Yours for Science,’” 591.

archaeological practice to a lower status, which was not at all the case. For the most part, “avocational” archaeology was, quite simply, archaeology. Even in the 1880s, there were so few full-time positions related to archaeology that one cannot meaningfully talk about a profession. However, as I have shown, this began to change slowly and in a piecemeal fashion with the Peabody and Smithsonian initiatives of the decade. The prospects of paid work enticed individuals to become participants in the archaeological networks associated with these institutions, sometimes in hopes of working their way upward and finding some way to build a career out of it.

Alfred Russel Wallace also referred to “anthropology” rather than “ethnology.” I have drawn attention to the shifting meanings of these terms throughout this dissertation, especially in Chapters 1 and 3. The term anthropology had been in wide usage in Europe since the early 1860s. Although it gained currency in the United States not long afterward, it never replaced ethnology, at least through the end of the nineteenth century. Like ethnology, anthropology was a term denoting an all-inclusive science of mankind, but its disciplinary origins lay with physical anatomists such as Paul Broca and others who loved to measure skulls.¹⁰ In the United States, the connotations of race and anatomy left a somewhat sour taste after the Civil War, and as a testament to the fact, we have only to look at institutional names: the Peabody Museum of American Archaeology and Ethnology, and the Bureau of Ethnology (which changed name to the Bureau of American Ethnology in 1897). Terminological distinctions aside, however, Wallace’s comments highlight something else very important about the archaeological problem of

¹⁰ Paul Broca, “History of the Transactions of the Anthropological Society of Paris from 1865 to 1867,” translated by C.A. Alexander, Smithsonian Annual Report for 1868, 376-91. Also, see Roger Smith, The Norton History of the Human Sciences (New York: Norton, 1997), 398.

the mounds—in the nineteenth century, it was perceived as being of central importance to the very constitution of a broader science of man in North America. Sadly, this is no longer the case. Very few of the ancient monuments of the Ohio and greater Mississippi valleys have survived the nineteenth century, and today, surprisingly few North Americans know anything about the extensive ancient civilizations that once thrived here.

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