ON THE STUDY OF HEALTH IN PREHISTORY

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By

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The superfluous, a very necessary thing.

-Voltaire

ABSTRACT

This thesis examines the approaches used in the study of health in prehistory, focusing in particular on the potential of archaeology to contribute to this field. Archaeological data have been under-used in studies of prehistoric health; obstacles to their utilization include the popular conception of archaeological information as less direct than osteological data, the idea that scientific research must produce testable hypotheses to be valid, and the challenge of integrating large quantities of information of different temporal scales into synthetic interpretations. Alternative conceptions of scientific research, and the use of the Braudelian model of historical time as an organizing structure, circumvent these obstacles and permit a fuller use of archaeological data in reconstructing patterns of past health. In particular, archaeological information about social behaviour, as well as subsistence and factors pertinent to disease ecology, can form a basis for inferences about health in prehistoric communities. A demonstration of this approach, using recent archaeological data and interpretations for the Mississippian site of Cahokia, suggests that prevailing negative opinions of the health of Mississippian groups and early agriculturalists may not be supportable with regard to the people of Cahokia.

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CHAPTER I

INTRODUCTION

> *A man writes a book not so much because he has an idea to express but to excuse himself for having had it.*
>
> André Gide, *Prometheus Misbound.*

The idea behind this thesis was quite simply that archaeological information could be used to better advantage in the study of prehistoric health. Hopefully, the pages which follow will provide both the full expression of this idea, and justification for its having occurred to me.

I had originally contemplated titling this thesis 'Diseases of the American Bottom in Prehistory'; however, although it does devote two chapters to the subject of prehistoric health at the site of Cahokia and in the surrounding American Bottom region of Illinois, that is not what this work is primarily about. Rather, it is about the way we study prehistoric health, and specifically about the exploration of health in prehistory from a primarily archaeological perspective. It is also about the backdrop against which inquiries into prehistoric health are carried out – a collage of Western ideas of progress, the history of modern North American society in relation to the original inhabitants of this continent, conceptions of science, and the histories of the individual disciplines of archaeology and

physical anthropology, each with their own motion and imperatives. By way of introduction, this chapter will offer some observations regarding the motivations and context of research into past health, some comments on the place of archaeology in this endeavour, and a brief discussion of the intrinsic interest the site of Cahokia holds for prehistorians.

The answer to the question, 'why study prehistoric health?' is both multifaceted and of central importance, though rarely considered. In fact, the question of *why* is as crucial to contemplate as the question of *how*, because of the necessity of understanding the context of the research. It has been repeatedly shown that in scientific inquiry, researchers' motivations and personal beliefs can affect their conclusions – often unbeknownst to the researcher – in ways ranging from subtly embarrassing to shockingly harmful (Gould 1981). Moreover, the field of anthropological archaeology in particular is somewhat renowned for periodic radical shifts in interpretations about prehistoric peoples, which can often be linked to changes in the prevailing political and cultural climate (Hodder 1991:173). After all, the ideas at stake are about ourselves, a subject very near and dear to us; their importance is emphatically illustrated by the number of anthropological hoaxes perpetrated to substantiate theories about human history or social and biological evolution, including, for example, Piltdown Man (Reader 1981), the Vinland Map (Feder 1990), and the Tasaday (Berreman 1991). Thus, not so much because of concern that any conscious duplicity is at work, but because the context of research cannot be ignored, it is necessary to consider why we study the health of past human populations.

The first motivating factor is probably the same one that prompts people, at least according to aphorism, to climb mountains – that is, 'because it's there.' The very fact that we have skeletal remains from past eras is somehow so intrinsically remarkable – and so appealing to the human dual fascination with the morbid and exotic – that it is difficult to resist the attempt to wrest every last bit of knowledge possible from them.

A second reason for studying past health is its potential for contributing to the understanding of modern health. Pfeiffer has suggested that while there are severe constraints upon applications of palaeopathological knowledge to modern situations, the dead can indeed teach us something about present and future health; in particular, she submits that palaeopathology, as well as having some minor predictive utility, "may help us to understand the commonality of biological response which in ancient times led to one outcome, but in modern times may lead to quite another" (1991:12). Manchester (1987:164) expressed a similar sentiment, arguing that palaeoepidemiology "must surely contribute to an understanding of modern medical problems."

A third reason for studying past health is to better understand the changing relationship of human beings with their environment throughout the millennia. For example, as William McNeill's *Plagues and Peoples* (1977) has shown, disease has played a large role in the course of history, helping to make our world what it is today. Further, however, studies of past health also impact upon theory about the processes of cultural and technological change. For example, Cohen, in his synthesis *Health and the Rise of Civilization* (1989), aligns himself with the Boserupian view that changes in subsistence

technology are driven by increasing population pressure, but do not necessarily result in a net benefit to the people in question. Part of the assessment of the effects of changing subsistence adaptations on well-being has come from osteoarchaeological studies of prehistoric health, such as those compiled in *Paleopathology at the Origins of Agriculture* (Cohen and Armelagos 1984). These assessments have in turn been used to evaluate theories of culture change like Boserup's (Roosevelt 1984).

Finally, the political dimensions of studies of past health cannot be ignored. Just as uniformitarian assumptions about past social relations can make present situations appear "inevitable and legitimate" (Hodder 1991:169), information about past health can (whether intended to or not) serve to legitimize views of human social evolution, and in turn, political stances regarding modern relationships between groups of people. For example, shifting views on whether the Neolithic Revolution helped or harmed human health can be linked to changing impressions of 'progress,' and this century's sequential enchantment and disenchantment with technology and the contemporary world. Cohen (1989:1-6) notes that in practice, the Western conceptions of 'the primitive' and 'progress' translate into governments' foreign policy decisions regarding the modernisation of non-industrial peoples and internal decisions about aboriginal groups. In light of this, Cohen suggests, a close re-examination of the effects that cultural evolution has actually wrought upon health is in order; he ultimately concludes that in terms of health, "our progress has been bought at higher cost than we like to believe" (1989:6).

Cohen (1989:1-6) explicitly situates himself as a critic of modernity.[1] Similarly, Goodman, Martin and Armelagos (1992) locate themselves emphatically in the present, in their discussion of health in communities of prehistoric Sudanese Nubia, and the Dickson Mounds populations of Illinois. It is their contention that the model of core-periphery interaction,[2] which has been used to explain poor health in modern countries which were or are subject to various forms of colonialism, is applicable to societies in prehistoric times as well. This application of modern critiques of capitalism to the subject of prehistoric health is another illustration of the political nature of the subject. A final example of this political aspect is provided by the debate over the extent and timing of contact-period depopulation in the Americas (*e.g.*, in Verano and Ubelaker, eds. 1992). It can be argued that much of the research in precontact health and demography in the Americas has ultimately been driven by the sociopolitically important questions of 'just how many Native American deaths did Europeans cause at contact, and were those deaths deliberate or accidental?' rather than by the more anthropologically motivated question of 'how did Native American people live prior to contact?' (Denning in press). This is

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[1] Cohen reveals his passionate belief in the importance of this critique through his response to Wood *et al.*'s (1992) suggestion that the skeletal data upon which he based his (1989) conclusions about worsening health are also consistent with improving health: "I am concerned with the clear pro-state or procivilization bias which dominates both popular and scientific interpretations of history (and of current events). Much of the work that is at stake in this discussion (Cohen 1989) has challenged this bias, and it disturbs me that the questions raised by Wood et al. emerge now, just as uncivilized lifestyles and the meaning of our history are being re-evaluated... I hope that we will remember to be equally skeptical of *all* conclusions from skeletal data... not just discard some of the recent revisions to standard history" (Cohen 1992:359).

[2] The concept of core-periphery interaction stems from 'dependency theory' – wherein it is postulated that modern capitalist nations (core areas) have achieved prosperity by exploiting peripheral areas – and was also used in Wallerstein's 'world systems theory' (Giddens 1985:162).

rarely mentioned by those conducting the research (Henige 1990), although the subject of contact is intrinsically intensely political in nature (Chomsky 1993).

In answer to the question 'why study prehistoric health?' it can be seen, then, that the motivations are far from straightforward, and at the very least, include factors other than the simple quest for knowledge. The next question which should perhaps be addressed is 'why focus on archaeology in the study of prehistoric health?'

Some may find this choice of emphasis ironic, for osteoarchaeologists have long struggled for what has been called "data parity" (Powell, Bridges and Mires 1991:4) with archaeological information about past behaviour (Roosevelt 1984). The lot of the osteoarchaeologist has been a frustrating one in many ways; archaeologists have often placed retrieval of human remains low on their list of research priorities, and have excavated bones carelessly, and stored them with little regard for their preservation (Wells 1964). Further, archaeologists have often had little knowledge of, or regard for, the utility of osteoarchaeological data (Powell, Bridges and Mires 1991, Cohen 1989; Buikstra 1991). A number of efforts have been made to bridge the gaps between these disciplines (*e.g.*, Blakely, ed., 1977; Cohen and Armelagos, eds., 1984; Powell, Bridges and Mires, eds., 1991), but they have not always met with unqualified success. It is possible that an emphasis on archaeological data in the study of prehistoric health might be viewed by some as ironic at best and ill-advised or unfair at worst.

However, while collaborative efforts obviously have much to recommend them, as do osteoarchaeological studies – this is not subject to debate here – there is also something to be gained by the examination of archaeology's potential as a separate entity

to investigate prehistoric health. To begin with, it is clear that osteoarchaeology is facing some interpretive dilemmas which only archaeology can help resolve (Wood *et al.* 1992). In addition, an exploration of the ability of archaeological data to shed light on past health in isolation from skeletal data may contribute to our understanding of life in those archaeological communities for which we have no human skeletal remains, or only remains which are few in number, poorly preserved, or to be repatriated. Finally, archaeology can contribute information about social behaviour, pertinent to health, which osteological analysis cannot provide. These are all good reasons for closely examining the potential of archaeological data as bases for inferences about past health.

Obviously, the idea that archaeological information can be useful in studies of past health is not revolutionary; many authors have emphasized this, either through words or example (Buikstra and Cook 1981; Goodman and Armelagos 1985; McGrath 1988a; Milner 1982; Saunders, Ramsden and Herring 1992; Storey 1992; Wood *et al.* 1992). However, it seems that it is not being used to full advantage. Usually, archaeological data are used in studies of past health primarily as context for the findings of osteoarchaeological analyses. Occasionally, archaeological data are independently used to shed light on ecological variables relevant to health. For example, patterns of garbage disposal can allow inferences about the likelihood of exposure to scavenger-borne diseases, or problems with water contamination (Saunders, Ramsden and Herring 1992; Larsen *et al.* 1992); similarly, community size and housing density can give an idea for how quickly infections would spread, and whether or not they could be sustained (McGrath 1988a; Milner 1982; Saunders, Ramsden and Herring 1992). Even less

frequently, archaeological data pertaining to social behaviour are used in interpretations of past health. However, the data used have usually been limited to indications of political structure and social ranking (Goodman and Armelagos 1985; Goodman, Martin and Armelagos 1992; Milner 1982; Powell 1988). Rarely have more basic levels of social behaviour – for example, interactions within households, sharing of economic tasks and food, and active social responses to ill health – been considered in such analyses.

As discussed in Chapter 2, the prevalent 'commonsense' conception of archaeological data as a less direct kind of information about past health than osteological data has perhaps minimized its exploration. In addition, the emphasis on the formulation and rigorous testing of hypotheses as the only appropriate methodology for investigating past health has likely impeded the open recognition of non-testable hypotheses – such as inferences drawn about social behaviour from archaeological data – as worthy scientific alternatives. Such inferences are sometimes made in studies of past health, but any explicit discussion of methodology seems to consist of advocations of testable hypotheses as the only reliable vehicle for advancement of understanding (*e.g.*, Rothschild 1992). Other impediments to the full utilization of archaeological data in research on prehistoric health include the tendency of broad-scale histories of human health (*e.g.*, Cohen 1989) to employ an evolutionary framework which entails an emphasis on archaeological data pertaining to ecology at the expense of those pertaining to social life.

Addressing these conceptual barriers to the full use of archaeological data in studies of prehistoric health is essential, and so Chapter 3 is devoted to suggesting some reorientations in thought, however slight, which can help to facilitate contributions from

archaeology in this endeavour. In particular, a freer methodology, which demands coherence of interpretation, and correspondence with the data, but not necessarily testability, can allow archaeologically-based exploration of prehistoric health with a special emphasis on social behaviour. The Braudelian model of layered historical time can be used as an organizing/integrating principle in place of an evolutionary scheme.

In working through these problems of epistemological and methodological assumptions, I have drawn heavily from archaeological theory; ultimately, it brings as much to the subject of prehistoric health as do archaeological data. However, it also brings with it some baggage. Thus, as is necessarily the case in any discussion of modern archaeological theory, another backdrop to this thesis is the battlefield of what have been called the processual and post-processual paradigms in archaeology. The theoretical skirmishes on this field have furnished a basis for some of the critiques of conventional studies of past health which I have included herein. This seems only appropriate, for whether the data from the past are pot sherds or human bones, the problems of their interpretation are often much the same.

This particular emphasis on theory is perhaps unusual in writings about past health, but truly is necessary not only in considerations of the study of past health – like this thesis – but to those actually doing the studying. Those who argue that "[p]hilosophy just does not matter to science" (Watson 1991:280) may be right if they are describing the current reality of scientific research; however, this is hardly a situation to aspire to. Wylie (1989:19) has persuasively argued that, while philosophical inquiry does not "delineate static, trans-contextual foundations for knowledge," it is relevant to

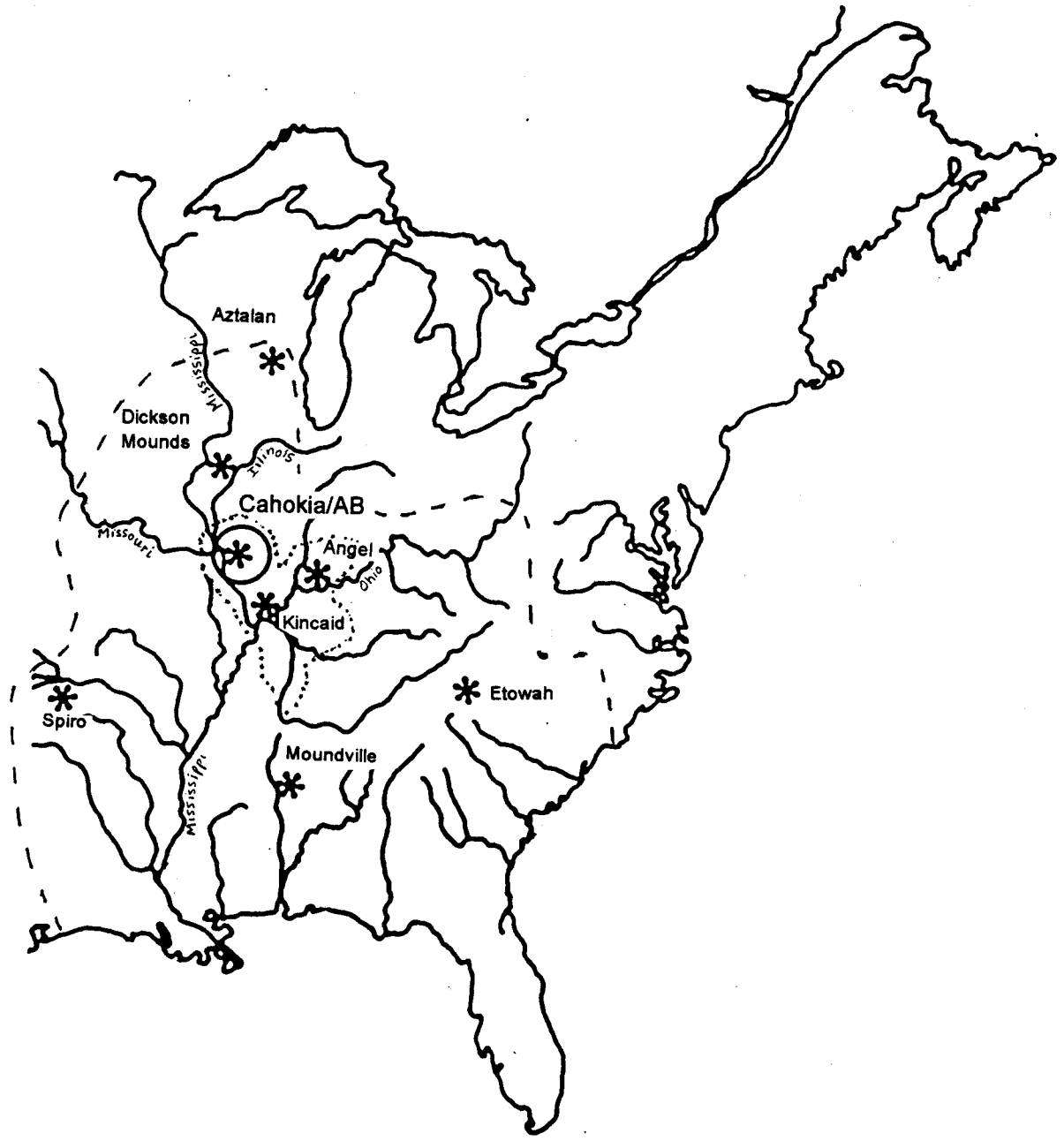

Figure 1.1 Eastern United States, showing major Mississippian sites.
Dashed line denotes edge of Mississippian culture area ca. 1400 A.D.
Dotted line denotes Empty Quarter, ca. 1540 A.D.
(Derived from Smith 1978, Armelagos and Hill 1992, O'Brien 1991, Smith 1985.)

strategies, including maize agriculture, and by the construction of earthen mounds, an elaborate ritual complex, social stratification, and towns of comparatively high population size and density. At the time of contact, when the de Soto entrada and their successors entered the region, some Mississippian societies were still flourishing, but there was an abandoned region now called 'the Empty Quarter' (Williams 1990), including the site of the greatest Mississippian community known, Cahokia. Located in west-central Illinois, Cahokia dates from about 900 to 1350 A.D., and is widely renowned for its huge size – the site covers 13 km^2 and includes well over 100 mounds (Fowler 1989:10) – and for its giant landmark, Monks Mound, 17 acres in area and 30 metres tall (Fowler 1989:7) (Figure 1.2). Cahokia in particular, and the Mississippian in general, have thus been of great interest to archaeologists because they represent, arguably, the most complex and elaborate prehistoric culture of North America (O'Brien 1989). There has been much debate, as discussed in Chapters 4 and 5, over just how complex this culture system was. It is perhaps a mark of the ideological value of these sites, as well as the ambiguity of the data, that decades of speculation have failed to produce a resolution on the subject.

The reasons for an interest in Cahokia, the surrounding American Bottom settlements, and the Mississippian culture system in general, also include their position as complex early agricultural groups. This trait makes them particularly intriguing, because they offer an opportunity to study the effects of intensification of subsistence strategies upon health (Cohen and Armelagos 1984; Milner 1982). Furthermore, there is a relative abundance of substantial Mississippian skeletal samples, including the series from Moundville, Alabama (Powell 1988) and Dickson Mounds, Illinois. Analyses of the

Figure 1.2 Monks Mound, Cahokia, Illinois, viewed from the southeast. Reproduced from Mink, Corley and Iseminger 1992, frontispiece.

latter have been extremely influential in shaping current interpretations about the health effects of the transition to agriculture. This is partly because of an abundance of research, due to the excellent preservation and enormous size of the sample (over 500 individuals are routinely used in Dickson Mounds studies, although more are available), and the presence of several different Dickson Mounds series from different time periods, which permits diachronic analysis of changes in health (Buikstra and Milner 1989). As discussed in Chapter 4, the results of the many Dickson Mounds analyses have indicated a substantial worsening in health over time, and it is this view which has gained prominence in the broad-scale work of researchers like Cohen (1984, 1989, 1992) and in general works on Mississippian groups (Mink, Corley, and Iseminger 1992). However, analyses of other Mississippian populations have shown no such dramatic changes (Powell 1988, Milner 1982), and the negative interpretation of the Dickson results has been demonstrated to be only one of several possible explanations of the data (Wood *et al.* 1992). In short, despite the influence of the Dickson Mounds analyses upon images of health in early agricultural groups, there was certainly considerable variation in Mississippian health, and as Buikstra and Milner (1989:61) suggest, tight control over interobserver differences in future osteological studies is needed to clarify the picture.

It is unfortunate that there are few skeletal samples of any size from Cahokia that are available for study (Milner 1983a, 1991a); however, this in itself would seem to qualify Cahokia as a test case for an archaeological approach to studying past health. Cahokia's position as a centre of debate in American archaeology, and current heated disputes over general patterns of Mississippian health, add to the interest. Contemplation

of Cahokian social structure and its effects on health is particularly relevant because of assertions that the apparent ill-health of the Dickson Mounds people was due to exploitation by Cahokian elites (Goodman and Armelagos 1985). A final factor which makes Cahokia a suitable test case is the comparative abundance of settlement data for the site and its American Bottom hinterlands, which provides ample material for speculation about past health.

On the most general level, then, this thesis may be considered in two parts. Chapters 2 and 3 consist of an exploration of how past health is studied, and ideas about how it might be done otherwise. Chapters 4 and 5 consist of an attempt to put some of these ideas into action by applying them to the case study of Cahokia.

More specifically, Chapter 2 offers some background to the study of prehistoric health, a description of the usual uses of archaeological data in this endeavour, and the identification of some problems with these approaches, especially ways in which the use of archaeological data has been unnecessarily limited. Chapter 3 outlines some routes by which these limitations can be avoided; in particular, it considers alternative conceptions of scientific method, different ways of using archaeological data in making inferences about past health, and the utility of Braudel's model of historical time as a device which can be used in structuring diachronic accounts of prehistoric health.

Any archaeologically-based assessment of health in prehistory must, of course, be preceded by a thorough understanding of the archaeology of the culture in question, and an understanding of the pre-existing studies of past health already carried out upon the

population. Cahokian archaeology has a long and multifarious history, and so Chapter 4 is devoted first to an overview of the archaeology of the region, and second to an overview of osteoarchaeological studies of Mississippian populations in Illinois. Chapter 5 discusses in more detail the archaeological debates of import, pertaining to Cahokia's level of complexity, provides a description of one reasonable vision of Cahokian society, and explores the implications for health of that vision.

CHAPTER II

THE STUDY OF HEALTH IN PREHISTORY: THE STATUS QUO

"I wish that I could have half an hour with the patient."
- Calvin Wells

My primary concern in this thesis is the use of archaeological data as bases for inferences about past health. Archaeological data, however, are rarely – if ever – used alone in this regard. A more normal approach is to use osteological and ecological information, sometimes in combination with archaeological and other data. It seems appropriate, then, to give an overview first of the history, and second of the current state, of the study of past health. The third section of this chapter is devoted to an explicit examination of the role of archaeological data in explanations about past health, while the final section provides a critique of some of the methodologies and conceptions involved.

TRENDS IN THE STUDY OF PAST HEALTH

The discipline of palaeopathology is said to have inauspiciously come into being in 1774 with Esper's description of a bone anomaly in a prehistoric French cave bear's femur, and to have been christened a century later (Grmek 1989:47). Initially, interest was

primarily in the diseases of Quaternary fauna, but the attention of now-illustrious names such as Virchow and Broca turned to human bones late in the nineteenth century (Grmek 1989:48). In general, the period from 1870 to 1900 was characterized by an emphasis on evidence for trauma and syphilis in ancient human remains (Ubelaker 1982:337). [1]

The early twentieth century was marked by Ruffer's work on mummies[2], as well as the research of Moodie, Hrdlička, and Williams on skeletal remains (Ubelaker 1982: Buikstra and Cook 1980). New interests had emerged, particularly the identification of infectious diseases, the documentation of culturally-caused variations like tooth mutilation and cranial deformation, and evidence for prehistoric medical treatments such as trephination (Ubelaker 1982). Grmek (1989:48) referred to the first three decades of this century as a "Golden Age of paleopathology," and observed that, with the benefit of advances in the understanding of disease processes and causation, and new technology in radiography and histology, the "anthropologist-doctors" of this period had considerable success in the identification of diseases affecting ancient remains. This was, as Ortner (1991:5) put it, the phase of palaeopathology devoted to the question of "what is it?"

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[1] This review, and the subsequent discussion about osteoarchaeological methods and theory, is heavily slanted towards the Western (primarily North American) experience; the generalizations made about osteoarchaeology are not necessarily applicable to research traditions elsewhere. See Sigmon, ed. (1993) and Baker and Eveleth (1982) for comments on the influence of political ideology and imperatives on the direction of physical anthropological research in the former Eastern Bloc and the U.S.A.

[2] Bodies, preserved through freezing, mummification (accidental or deliberate), or submersion in bogs have yielded considerable information about past health, not only from their preserved tissues, but from parasites and stomach contents (Cockburn 1971:52; Ruffer 1967[1910]:177).

Ubelaker noted that each new period one may demarcate in the history of palaeopathology "marks the beginning of new interests rather than the cessation of the old" (1982:337). Thus, an interest in specific diseases in antiquity, such as syphilis, has persisted to the present day (*e.g.*, Baker and Armelagos 1988); similarly, as Ortner commented in disappointment, the "emphasis on description continues to predominate in publications on palaeopathology today" (1991:5). Lovejoy, Mensforth, and Armelagos (1982:335) concurred with Ortner, stating that in their review of palaeopathological research papers published in the American Journal of Physical Anthropology, the proportion of descriptive papers in relation to analytical papers "seems excessive."

However, as new interests have been added to the list, so have new approaches been added to the arsenal. Palaeopathology moved onwards from the description of isolated oddities to a population biology approach with the work of Hooton, whose 1930 report on the human remains of Pecos Pueblo "brought a new dimension to paleopathological research by presenting detailed frequency data on disease in a large, well-documented archaeological sample" (Ubelaker 1982:342). Hooton's work effectively ushered in a new era with a fundamental shift in methods and goals; Ubelaker has described the focus of the period from 1930 onwards as the rise of "the paleoepidemiological approach," wherein disease was viewed from an ecological perspective (1982:337).[3] Although today it is still often the case that skeletal samples

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[3] The period from 1930 onwards has seen also great changes in the people conducting research into past health. Spencer (1982:1) indicated that in 1930, most members of the American Association of Physical Anthropologists were originally trained in anatomy or medicine, worked in museums or anatomy departments, and "regarded physical anthropology as an ancillary occupation." Of course, this is no longer the case; however, it is still interesting to look at the

(especially those unearthed by contract archaeology projects) are only given cursory examination and discussed briefly in site report appendices which only highlight atypical or peculiar specimens, population-based studies which "emphasize the evolution of disease patterns or general measures of the quality of life" are becoming more prevalent (Buikstra and Cook 1980:435-6). Similarly, the focus on diagnosing specific diseases has, with the notable exceptions of syphilis and tuberculosis, largely given way to the identification of types of disease, and the use of nonspecific indicators of stress in assessments of population health (Buikstra and Cook 1980; Buikstra 1991; Cohen 1984).

Ubelaker (1982:345) observed wryly that one reason for this trend – away from disease in the individual and towards the health of a population – was the lack of success in achieving definitive, specific diagnoses; however, it was also enhanced by the increased availability of large, accurately provenienced skeletal samples, improvements in archaeological dating techniques and research design, the accessibility of computers, and more interdisciplinary connections with fields such as nutrition studies, demography, and epidemiology. The newer emphasis on "the necropolis instead of the individual skull" may be a part of a larger trend in the study of humanity; for example, it parallels the modern interest of historians in everyday life in the past, instead of 'great men' and exceptional events (Grmek 1989:51). Many see it as essential for this trend to continue, and also for palaeopathology to "move beyond the diagnostic phase of research, and ask

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institutional affiliations of those doing research in palaeopathology/osteoarchaeology. For example, in the compilation *Prehistoric Tuberculosis in the Americas* (Buikstra 1981), the list of contributors reveals that fully one-third of the authors were primarily affiliated with a medical institution. In contrast, the contributors to *Paleopathology at the Origins of Agriculture* (Cohen and Armelagos 1984) were overwhelmingly from with anthropology departments.

questions about the biological and evolutionary significance of our findings" (Ortner 1991:10). Cohen (1984) echoed Ortner's sentiment with respect to questions of cultural significance, and underlined the ability of the newer approach, with its emphasis on populations and on quantifiable indicators of stress, to contribute to the resolution of problems such as the effects of the transition to agriculture on human well-being.

Thus, the questions being asked about prehistoric pathology now include not only "what is it?", but "what does it mean?" (Ortner 1991:5), and the questions asked about prehistoric health are not only "what is the history of a particular disease?", but "what are the frequencies of the disease categories in a population, and how do they relate to other biocultural data?" (Ubelaker 1982:345). To find answers to these wider questions, researchers have availed themselves of other kinds of osteological data, including, for example, growth studies, palaeodemographic profiles and biomechanical analyses (Saunders and Katzenberg 1992, Ortner and Aufderheide 1991, İşcan and Kennedy 1989). Since the health-related information drawn from skeletal material now includes more than data regarding pathology and trauma, many researchers now prefer to avoid the narrow term "palaeopathology," and write instead of "osteoarchaeology" or "bioarchaeology."

Of course, throughout this time, human remains have not been the only source of information about past health. Palaeopathologists and historians of medicine have long shown interest in works of art – from Mochica pots to Renaissance paintings to ancient Greek sculpture to Egyptian stelae – which sometimes depict individuals with unusual congenital or acquired conditions, or scenes of medical procedures (Brothwell and Sandison 1967; Janssens 1970). Diagnosing specific conditions with reasonable confidence

has been possible where the artistic tradition is known to be highly realistic, rather than stylized; however, one often cannot be sure whether the unusual feature is part of an accurate depiction of a diseased person, or the product of artistic distortion or convention. Nonetheless, the exploration of the topic has prompted much vigorous debate, and most palaeopathology books from the 1960s and 1970s are filled with intriguing photographs of exotic artifacts or art, as well as the requisite close-ups of mummies with gruesome expressions, and skulls with traumatic or infectious conditions almost too severe to comprehend (*e.g.*, Brothwell and Sandison 1967).

The same sort of difficulty that applies to the interpretation of art also applies to the interpretation of literary evidence regarding disease, which has also been used extensively by medical historians and some palaeopathologists. It takes expert analysis to discern the literal from the metaphorical, and to precisely translate descriptions into modern medical terms. Nonetheless, written materials have been a popular source of information about ancient health; Brothwell and Sandison's 1967 compilation, for instance, includes analyses of a wide range of written sources, including Egyptian medical papyri, the Bible and Talmud, and the ancient histories and medical writings of China, Greece, and Rome. Grmek (1989) provides a remarkable recent example of detailed analysis of ancient Greek medical literature.

Alongside this wider tradition of palaeopathology, which drew from diverse sources to demonstrate the existence of various diseases in the past, emerged the wider tradition of palaeoepidemiology. As noted earlier, osteoarchaeology in general underwent a shift in the middle of this century, and began to place a greater emphasis on the health

of populations than on the pathologies of individuals. This shift was paralleled by a growing interest in the evolution of human diseases, as related to changing human adaptations and ecology, as well as the recognition that the human environment consists of physical, biological, and cultural factors. Early writers on subjects related to this included Burnet, who wrote in the 1940s about viral ecology (Burnet and White 1972), as well as those concerned with the way disease affected the course of recorded history through its action in populations rather than individuals (*e.g.*, Zinsser 1935, McNeill 1977). Others who contributed early to these subjects included Dunn (1968) and his contemporaries, who focused on the examination of disease patterns in contemporary hunter-gatherer bands, and, employing uniformitarian assumptions about the nature of such communities throughout history, extrapolated their findings into the past.[4] The subject began to solidify as an area of inquiry with the writings of Polgar (1964) and Cockburn (1963, 1967, 1971). Cockburn (1967:50) declared that palaeoepidemiology differs from palaeopathology "in being not merely descriptive, but also attempt[ing] to show how and why the diseases arose, spread, and evolved."

This palaeoepidemiological tradition – still very much alive today, as will be discussed shortly – has concerned itself primarily with the coevolution of infectious disease and human culture, specifically the changes in patterns of diseases which would

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[4] As has often been noted since (*e.g.*, Boyden 1987:60), one must be cautious when using data derived from recent hunting-gathering groups as a basis for inferences about groups in the past, or about the 'primeval human condition'; the groups studied by ethnographers all have their own distinct history, which precludes generalization to a certain extent. Feit (1994:422) has stated that in fact, "a universal concept of socially distinctive hunter-gatherer societies may not be a credible anthropological category."

have affected human populations as they moved from hunter-gatherer adaptations to settled farming to pre-industrial, and finally industrial, city life. The types of evidence drawn upon in these evolutionary/ecological reconstructions have included the palaeopathological sources discussed above, but also comparative disease ecology in other animals (especially modern primate groups), health in contemporary populations studied by anthropologists (Dunn 1968), and general archaeologically-based information about cultural adaptation and evolution (Cockburn 1967, 1971; Polgar 1964).

Thus, since the early days, when those who did palaeopathology almost invariably researched primarily in forensics, evolution, or medicine (Spencer 1982), the study of past health has become so specialized and diversified that no one person can master it all. Indeed, the field is characterized by increasing eclecticism; novel approaches to the study of ancient human remains are now appearing at an exponential rate in response to newly available technologies, driving the publication of compilations about new osteological research on a nearly annual basis (Saunders and Katzenberg 1992, Ortner and Aufderheide 1991, İşcan and Kennedy 1989). The use of sources other than human remains has continued in efforts to understand the health of early populations. In particular, ethnographic analogy, mathematical modelling, archaeological evidence, environmental evidence, and ethnohistorical information have all been used to shed light upon the health of prehistoric communities, as research discussed in this chapter will show.

There are many different ways of characterizing or categorizing the research currently being done on prehistoric health, but for the purposes of my analysis later in this chapter, the distinction between local-level and broad-scale studies is most relevant. Local

level studies focus on understanding the health of a particular community or small region during a specific period in prehistory. In contrast, broad-scale studies focus on finding regularities in patterns of past health, and making generalizations which are applicable to all human societies with similar ecological relationships and modes of adaptation.

Local-level studies of prehistoric health

Book-length examples of the local-level approach include Rebecca Storey's (1992) study of demography and health for the Tlajinga-33 population within the larger community of Teotihuacan, and Mary Lucas Powell's (1988) analysis of health at the Mississippian centre of Moundville, Alabama. Both are derived from the authors' doctoral dissertations, and indeed, it seems that a detailed examination of a particular population's health, based primarily on osteological analysis but considering other factors as well, is a popular approach often used in advanced graduate study (*e.g.*, Milner 1982). Local-level studies of prehistoric health are not limited to primarily osteological analyses or lengthy publications, however. McGrath (1988a), for example, has used mathematical modelling to explore the possibility of tuberculosis epidemics existing in prehistoric Illinois, and short analyses of health, or a specific aspect thereof, in particular archaeological communities are frequently seen in physical anthropology journals or compilation volumes (*e.g.*, in Cohen and Armelagos 1984, and Verano and Ubelaker 1992). Two examples of these types of research follow.

As mentioned, McGrath (1988a) added a new dimension to the long-lived discussion about the presence of tuberculosis in the Americas prior to contact with the use

of a computer simulation of a TB epidemic's behaviour. Although mathematical models of disease spread have long been used in epidemiology (Bailey 1975), their use in archaeological contexts has been very recent, and has primarily been driven by questions of the timing and extent of post-contact depopulation in the New World (*e.g.*, Milner 1980, Ramenofsky 1987, Snow 1992). McGrath's work is something of an exception, as she attempted to determine, based on archaeological settlement evidence of social networks and population sizes, the probability that tuberculosis could have existed in the Middle Woodland, Late Woodland, and Mississippian communities of the Lower Illinois River Valley. This approach offered an independent assessment of the likelihood that the tuberculosis-like pathology observed in skeletal remains from these populations was in fact caused by tuberculosis, as has been claimed (*e.g.*, Buikstra and Cook 1981).

Another important area of innovation in local-level health analyses has been palaeodietary reconstruction, which has seen remarkable technological advancement within the last few years, in the areas of trace element and stable isotope analysis of bone. Trace element analysis of human bone – particularly of strontium and calcium – has been used quite extensively, particularly with respect to establishing the relative quantities of animal and plant foods consumed, and variations in this ratio within stratified populations, and over time (Sandford 1992). Carbon isotope ratios in bone collagen have been used to determine the degree of dependence on C_4 plants (in particular, maize) in the subsistence of a number of prehistoric New World horticultural populations, including the Hopewell (Bender *et al.* 1981) and the Late Woodland-Mississippian peoples of Illinois (Buikstra 1992). Both carbon and nitrogen isotope ratios have been used to trace the reliance upon

marine resources in various populations, while nitrogen isotope ratios can, through distinctions between trophic levels, determine the ratios of animal to plant food (Schwarcz and Schoeninger 1991). The analysis of nitrogen isotope ratios has also been used to reveal information about infant feeding practices in both prehistoric and historic populations (Katzenberg, Saunders and Herring 1993). As well as examining differences between populations in reliance on various food resources, stable isotope ratios have been used to examine variations within burial populations; at times, they have revealed marked differences in diet which have been attributed variously to sex, status, or migration from another community with different dietary habits (Buikstra 1992; Katzenberg 1992).

These are only two areas where local-level analysis of prehistoric health has been expanding; there are others, and there are also studies which combine, to varying extents, the results of several specific methods of investigation – like mathematical simulations and palaeodietary analysis, but also archaeological settlement data and palaeodemographic profiles – into syntheses about health in a particular prehistoric place and time. These will be discussed in greater detail later in this chapter.

Broad-scale studies of prehistoric health

As mentioned, broad-scale studies of past health stand in contrast to local-level studies, for the focus is not on determining the situation for a particular place and time, but on finding regularities in patterns of health, based on differing ecological conditions and adaptations. Such studies are marked, in general, by a heavy evolutionary emphasis

and a concern with larger theory about population growth, technological advancement, and their effects on humanity.

The primary tradition of broad-scale research into prehistoric health is a continuation of the legacy of Polgar, Cockburn, and their associates; Cohen's 1989 book, *Health and the Rise of Civilization*, is probably the best-known and most substantial recent example of a broad-scale history of human disease patterns, but it is part of a wider tradition which has included reviews of varying length from Diamond (1992), Fenner (1970, 1980), Kelley (1989), and Armelagos *et al.* (1990), as well as the earlier writings of Polgar (1964), Cockburn (1963, 1967, 1971), Fiennes (1978), Black (1978[1975]), Armelagos and Dewey (1978[1970]), and Armelagos and McArdle (1975).[5]

These researchers have drawn upon a wide variety of evidence, including disease ecology, ethnographic analogy, evolutionary theory, and archaeological evidence of changing subsistence adaptations and settlement patterns, in order to establish general models of human disease ecology from an evolutionary perspective. As a scholar well abreast of the trends of the recent years in archaeology and biological anthropology,

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[5] Cohen's (1989) book is complex to characterize, because it uses a number of different, intertwined approaches to the material. There are chapters devoted to "Behaviour and Health," "The Evolution of Human Society," "The History of Infectious Disease," "Changes in the Human Diet," "Health among Contemporary Hunter-Gatherers," and "The Evidence of Prehistoric Skeletons," and he purports to examine all of these different sources of information separately, and then integrate their implications into a single account of the history of disease in the final chapter. Of course, it is impossible to truly separate the chapters' subject matter, and so in "The History of Infectious Disease," one sees many elements from the other chapters – after all, it is studies of contemporary hunter-gatherers, human social evolution, and prehistoric skeletons, in addition to disease ecology, that have combined to give us this "History." My point here, and in the critique at the end of this chapter, is that it is very interesting to see *which* elements crop up in "The History of Infectious Disease" and which do not.

towards answering quite specific questions, Cohen (1989) included an explicit reminder that this 'genre' of academic research is of a very different nature than much current archaeological and anthropological work, in that it provides answers to different questions. The emphasis here lies not on particularistic precision, or information applicable to specific places or times, but on the understanding of the general trends of health in human history. A summary of the current understanding of those general trends follows.

Hunter-gatherer bands, with small and mobile populations, would have had a low macroparasite burden, a possible problem with chronic infectious diseases (such as the treponematoses, herpesviruses and mycobacteria) which could sustain themselves in small populations, occasional soil-borne diseases, zoonoses and arthropod-borne infections, but no acute crowd diseases (Armelagos and Dewey 1978:102; Armelagos, Ryan and Leatherman 1990:355; Armelagos and McArdle 1975:3-4; Cockburn 1971:47-48; Cohen 1989:32-38; Fenner 1980:14-5; Polgar 1964; Swedlund and Armelagos 1990:6).

Agricultural villages would have suffered from, in addition to the diseases of hunter-gatherers, correlates of sedentism, close contact, and waste accumulation, including: a higher macroparasite burden, more vermin-borne diseases, more water-borne diseases, and more respiratory and enteric infections. Tilling the soil would result in a heightened exposure to soil-borne diseases, and domesticated animals would have been another source of infection. Landscape modification associated with farming is also a health risk because of its encouragement of insect vectors like mosquitoes *etc.*, as well as water-living vectors of disease (Armelagos and Dewey 1978:103; Armelagos, Ryan and

Leatherman 1990:356; Cockburn 1971: 48-50; Cohen 1989:42-3; Fenner 1980:16; Polgar 1964; Swedlund and Armelagos 1990:6-7).

Finally, the development of cities and wide trade routes increased effective population sizes, thus opening the door for the crowd infections such as measles, diphtheria, cholera, and smallpox. Cities would also have suffered from an amplification of the problems seen in settled agricultural villages; sanitation would have been more and more a problem, as would increased close contact between members of the population (Armelagos and Dewey 1978:104; Armelagos, Ryan and Leatherman 1990: 356-7; Black 1978:123; Cockburn 1971:50-51; Cohen 1989:47-53; Fenner 1980:16-7; Polgar 1964).

In short, it is argued that there has been an increase in the infectious disease load over time. This model of changing disease patterns in human history has varied surprisingly little over the last 25 years. Virtually without exception, the accounts consider two factors to be the key determinants of disease patterns in a population: population size and density, and exposure to pathogens. Inferences regarding pathogens which may have been present are drawn from information about subsistence technology (especially animal domestication, and landscape alterations through cultivation), degree of sedentism (which affects food storage, interpersonal contact, and human waste accumulation) and trade networks. Similarly, there is little variation in the classification of human societies which is used. The typology to which most accounts conform is: hunter-gatherer bands, settled agricultural villages, preindustrial cities, and industrial cities.

Thus, these authors have been working within a particular configuration of joined frameworks: overall, they are working primarily in a biological approach concerned with

"the coevolution of human culture and infectious disease" (Inhorn and Brown 1990:93). Within this approach, they are using the host-pathogen-environment triadic model of disease causation, wherein the environment is considered in comparatively narrow ecological terms (in contrast to inclusion of social, political, and economic variables under the category of 'environment') (Armelagos, Ryan and Leatherman 1990:353; Inhorn and Brown 1990). This is in turn set against a backdrop of sociocultural change where societies are classified according to their size and subsistence technology (Cohen 1989:17; Fenner 1980:9).

Having provided a brief overview of different approaches currently used to the study of prehistoric health, both local-level and broad-scale, I will now turn to a consideration of the ways in which archaeological data are used in these enterprises.

The current place of archaeological data in the study of prehistoric health

Archaeological information – by which I here mean the evidence of sites, as opposed to isolated artifacts with artistic depictions of possible disease conditions – has had an ill-defined and, at times, uneasy relationship with other kinds of information in the study of prehistoric health. Archaeological data are often simply not used. Where they are, they seem to be in one of the three following contexts with relation to other kinds of data:

a) differential diagnosis: osteoarchaeology as hypothesis-generating, archaeology as hypothesis-testing

b) population health: archaeology as hypothesis-generating, skeletal biology as hypothesis-testing

c) integration of all different lines of evidence.

i) with respect to health in a specific place and time

ii) with respect to general histories of human health

Of course, since actual research is rarely straightforward or formulaic, these different relationships of archaeological to other data are not mutually exclusive or separate; they intertwine, and many works combine more than one of these variations. In order to concretize this discussion somewhat, examples of these variants follow.

a) Differential diagnosis: osteoarchaeology as hypothesis-generating, archaeology as hypothesis-testing.

Although research methodology – in the non-technical sense – is not often written about in osteoarchaeology, the mode of hypothesis-testing research seems to be a dominant ideal in the field today. For example, Klepinger (1992:121) wrote: "several hypotheses will be presented about the effects of a high meat diet on the skeleton, and ways will be suggested of inferring such a diet from analysis of bone – or, to take the Popperian twist, ways in which the hypotheses can be falsified," and included a number of explicit references to the testing of hypotheses, as did Buikstra and Cook (1980:439,440,457), Rose, Marks and Tiezen (1991:9), and Bridges (1991:89). Rothschild made an even stronger recommendation about research methodology in palaeopathology:

> A major avenue for advancement of paleopathology lies in coordinated efforts to form testable hypotheses and then to identify valid techniques for testing them. If a testable hypothesis cannot be generated, perhaps the

> problem should be placed on hold, or an interdisciplinary council formed for determining new technologies appropriate to its assessment.... Testable hypotheses are the key. (Rothschild 1992:131)

One of the ways in which hypothesis-testing has been manifest is the use of archaeological evidence to test palaeopathological and palaeoepidemiological hypotheses, especially in the area of differential diagnosis.

The project of differential diagnosis, the oldest in the tradition of palaeopathology, has been dismissed by some as an unproductive enterprise when disease is being considered from an anthropological perspective with a focus on adaptation and evolution (Clark *et al.* 1987:58), and as noted previously, attention has generally shifted away from the identification of specific pathogens to the identification of classes of disease. However, Buikstra and Williams (1991:168) have taken issue with Clark *et al.*'s contention, suggesting that differential diagnosis is an important and worthwhile enterprise: "[t]he study of human adaptation in the past requires scientific methodology, including both theory and data-based tests.... differential diagnosis plays a crucial role in this process." Whichever view one subscribes to, it seems clear that differential diagnosis is not about to relinquish its place near the core of the palaeopathological tradition, and so an examination of the place of archaeological data in its approach is warranted.

The example of tuberculosis in the precontact New World, already mentioned, provides a good illustration. Evidence of tuberculosis-like pathology in precontact skeletal remains is substantial, though not abundant; the question is whether this pathology is produced by *Mycobacterium tuberculosis* behaving as it does today, by *M. tuberculosis* behaving otherwise due to a different host-pathogen relationship, or by another organism,

possibly an atypical environmental mycobacteria (Buikstra and Williams 1991). In order to resolve this question, researchers have turned to historic information about tuberculosis as seen in New World groups after contact, information about the geographical distributions of soil-based organisms capable of causing similar pathologies, and to archaeological evidence (Buikstra 1981, Buikstra and Williams 1991). In 1963, Cockburn concluded that pre-Columbian tuberculosis was highly unlikely because the New World's population density was not high enough to support the disease; Buikstra and Cook (1981) argued that, on the contrary, consideration of the biocultural factors in west-central Illinois, especially the size of the population aggregated at Cahokia, clearly supported the tuberculosis model.[6] McGrath (1988a) reached different conclusions through her archaeologically-based mathematical models of TB spread in the Lower Illinois Valley, contending that the disease as it is known today could not have existed in the communities for which Buikstra and Cook suggested it could. No definite answers have been found, but, nonetheless, archaeological information on population size and structure has been considered a valuable adjudicator or arbiter in this instance.

b) Population health: archaeology as hypothesis-generating, osteoarchaeology as hypothesis-testing.

It is now more common in palaeoepidemiological and palaeopathological enterprises for archaeological data to be used in the generation of hypotheses than in the

[6] Buikstra and Cook (1981:120) used the estimates for Cahokia, supplied by Gregg (1975), of a population between 25 500 and 43 000 people; as discussed in detail in Chapter 5, a population of this size for Cahokia is considered unlikely by many American Bottom researchers.

testing of hypotheses. For example, Cohen and Armelagos wrote that the compilation, *Paleopathology at the Origins of Agriculture*,

> developed out of our perception that many widely debated theories about the origins of agriculture had testable but untested implications concerning human health and nutrition and our belief that recent advances in techniques of skeletal analysis, and the recent explosive increase in data available in this field, permitted valid tests of many of these propositions. (Cohen and Armelagos 1984:xix)

Cohen elaborated further, commenting that a number of recent trends in palaeopathology – the integration of palaeopathology with palaeoecology, the emphasis on the population instead of the individual specimen, the use of general skeletal indicators of stress, and the development of isotopic and trace element analysis – provided the ability to objectively test archaeological theories regarding the effects of economic transitions in prehistory (1984:3-4). In particular, Cohen (1984:1-2) felt it was becoming possible to determine which of two theories about the results of subsistence intensification in human history was correct: the traditional evolutionary view of 'technological advance = improvement in human life,' or the view, inspired by Boserup, studies of the !Kung San, and writings like those of Cockburn and Polgar, that 'technological advance = diminishing returns on labour and increased risks to health.'

This was, of course, a broad question, and remains a focus for investigation. However, many more specific examples of testing archaeologically-derived hypotheses with osteological data exist. A good example is provided by Powell's (1988) research on the skeletal population of the Mississippian site of Moundville, Alabama. The presence of a distinct system of ranked status at Moundville prompted the hypothesis that rank

would have affected health (*i.e.*, the subordinate group would have had worse health). From this hypothesis, Powell derived a number of predictions, or "modeled expectations," for differences in health between supraordinate and subordinate individuals (1988:80). Powell then tested her predictions through examination of a large sample of skeletons, and found that in general, the differences that she had expected were not to be found. She therefore concluded that "[t]he hypothesis that Mississippian nonelite individuals suffered major systemic stresses from resource deprivation because of their inferior ranked status is not substantiated by the analysis reported here" (Powell 1988:197).

In situations such as that explored in Cohen and Armelagos (1984) and Powell (1988), then, skeletal data are seen as appropriate evaluators of archaeological theories.

c) Integration of all different lines of evidence.

The integration of multiple lines of evidence, including information derived from archaeology, osteology, ethnography, and disease ecology, has become recognized as a desirable course of action by many researchers in palaeoepidemiology and palaeodemography, although overall, comparatively few efforts have been made in this direction (Milner 1982; Storey 1992:xv-xvi). The 'integrative' publications which do exist vary considerably in their nature and in their use of archaeological data.

Intensive efforts to integrate different sorts of evidence for a population in a specific time and place have been particularly scarce. Many local-level, primarily osteoarchaeological articles refer to some extent to archaeological evidence in their reconstructions of past patterns of health in a particular population, but in most cases, it

seems to be provided primarily for background or context regarding the society or site being studied (*e.g.*, Perzigian, Tench and Braun 1984). Quite often, subsistence data gleaned from archaeological study are used to frame chemical analyses of nutrition (*e.g.*, Norr 1984). Occasionally, an archaeological detail is used to explain apparent anomalies in the osteological data; for example, Larsen *et al.* (1992:33) suggest that an increase in porotic hyperostosis in La Florida populations, unaccompanied by a change in diet, was related to infection caused by the contamination of wells by nearby middens. In fewer cases, like Saunders, Ramsden and Herring (1992) and Milner (1982), settlement data are used as a basis for substantive inferences about health which are regarded as equal in status to inferences based on other kinds of data.

Obviously, the exact nature of the integration of different types of evidence depends both upon the population being researched, and the available research that has already been done. Regardless, it is rarely the case that archaeological data are central to local-level studies of health in a particular time and place. On the other hand, the broad-scale models of disease in human history use archaeological information about changing settlement sizes and subsistence adaptations as a central organizing principle. However, many kinds of archaeological information are notably absent. In particular, minimal attention is paid to evidence of the complexities of social structure, or political and economic aspects of life in prehistory. [7]

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[7] Of course, there are exceptions; Armelagos and McArdle (1975:5-6) consider band structure and fluid social organization in hunter-gatherers as a important factor in models of disease transmission, although they offer no discussion of more complex social forms. Also, Boyden (1987) is a refreshing counterexample, but is noticeably absent from citations in the anthropological literature because of its emphasis on Western civilization.

In sum, then, I have found three primary relationships of archaeological to other data in studies of past health: (1) archaeological data as hypothesis-testing, (2) archaeological data as hypothesis-generating, and (3) archaeological data integrated equally with other varieties of information. While these three different relationships have, to a certain extent, been a product of different research traditions and goals, all are evident in the current body of research into prehistoric health. The first is to some extent a remnant of the early days of palaeopathology which were concerned primarily with the specific diagnosis of pathology, while the second has become a predominant pattern in local-level studies as an interest in population health has developed, and as newer, more powerful osteoarchaeological analytical techniques have emerged. The rising popularity of the third kind of relationship is likely due to the convergence of various influences; in the case of the integration of different avenues of investigation in specific site cases, these influences may include an increasing trend towards historical particularism, the accumulation of substantial quantities of data which can be used, and a broadening of the concepts of health stressors to include cultural factors (Goodman *et al.* 1988).

There are, of course, practical and theoretical limitations to all three approaches to the use of archaeological data in the study of past health. A discussion of some of these limitations should lead to a better understanding of how archaeological data may be most productively used in this endeavour.

This discussion will first focus on the quality and character of osteological data, and on the process of hypothesis-testing, and will then turn to the shortcomings of certain types of integrative research.

SOME LIMITATIONS OF CURRENT APPROACHES TO HEALTH IN PREHISTORY

My interest in this thesis is principally in the ill-defined relationship between osteological and archaeological evidence regarding past health, and so only some limitations, relating to this relationship, will be covered in this necessarily circumscribed discussion.

Osteoarchaeological data and interpretation

There are, as with any field of investigation, both practical and theoretical problems with the study of skeletal material. The first difficulties that must be considered are those of skeletal age estimation, sex determination, and differential diagnosis of pathological lesions. The importance of the latter to interpretations of past health is obvious, but sex and age determination can be equally important, as they permit palaeodemographic analysis, which can also shed substantial light upon the health of a prehistoric population. For example, an unusually high infant mortality rate is in itself an indication of poor population health. Similarly, age-at-death assessments can help to compensate for one of the most restrictive features of bone – that it rarely shows evidence of acute infectious disease – by revealing unusual patterns of mortality.

Unfortunately, procedures of sex determination and age estimation, though fundamental to even the most basic skeletal analysis, are still fraught with difficulty. For example, it is well known among osteologists that intact skeletons, let alone fragmentary or damaged ones (like many archaeological specimens), do not always permit the accurate

evaluation of sex and age. Although some researchers are more enthusiastic than others, there are enormous obstacles to palaeodemographic analyses (Bocquet-Appel and Masset 1982, 1985; Buikstra and Konigsberg 1985; Jackes 1986, 1992; Roth 1992; Van Gerven and Armelagos 1983).

On the palaeopathology side, the first restriction upon the utility of skeletons is that many severe physical stresses leave no trace in bone; for example, the vast majority of acute infectious diseases do not (Kelley 1989). Similarly, it is a problem that while bone is a dynamic, responsive tissue which can act as a record of many events in the life of an individual, there are only a few ways in which bone can respond to a stress, and so, lesions or abnormalities are not always attributable to a specific cause (Ortner 1992). Accurate diagnosis can be especially difficult with fragmentary skeletons where the lesion distribution cannot be determined, and with damaged bones. And in the case of some pathologies, such as osteoarthritis, even if a condition is confidently diagnosed, its precise cause can remain a mystery (Miller and Ragsdale 1993).

With respect to the analysis of diet, it should be noted that the problems of diagenesis in trace element and isotopic analysis cannot be completely controlled for, any more than they can in research using ancient DNA (Katzenberg 1992; Sandford 1992), and that factors which can confound isotopic analysis are many (Buikstra 1992:89).

The problems of interpreting osteological data are even more complex. It has been said that an academic discipline is "a group of scholars who ha(ve) agreed not to ask certain embarrassing questions about key assumptions" (Cohen 1989:viii). Such agreements are almost never actually articulated, and it appears that the participants often

forget what the embarrassing questions and key assumptions even are. Part of the challenge, then, is to rediscover these assumptions, and part of the process of growth, surely, is to ask the embarrassing questions at least once in a while. This is being done by a number of scholars in osteoarchaeology, as will be discussed below.

There is a belief prevalent in osteoarchaeology which I call 'the myth of epistemological superiority' (Denning 1993). By this, I mean the perception that bones provide direct evidence about past health, and that other kinds of data (and inferences based on them) are less immediate, or one step further removed from reality. However, it is critical to recognize that bones are not *direct* evidence, but merely evidence; as is obvious from the disputes mentioned above, bones, like any other empirically observed entity, must be transformed by theory before meaning can be extracted. It is certain that most osteoarchaeologists recognize this to be true; nonetheless, it is not difficult to find instances of researchers referring to bones as "direct evidence" (*e.g.*, Roosevelt 1984:569), or stating that prehistoric health can be "directly assessed" only through skeletal remains (Stodder and Martin 1992:55). Such comments must be seen as something of a Freudian slip, revealing perhaps an occasional disregard for theory, but also a sort of 'commonsense' bias against non-osteological data as a source of information about health.

If it is granted that skeletal remains do not actually provide "direct" evidence about past health – and one has little choice but to grant this, since the record they provide has been transformed by the selective response of bone to stress, by archaeological processes, and by interpretation – then at least two questions must be

asked. First, how are these remains interpreted, and is there ever disagreement? Second, is it appropriate to use osteological data to test archaeological theories?

Interpretation – what does it all mean?

If accurate information on sex, age, and pathology is available, how can it be translated into hypotheses about population health? At present, we do not know.

The problems go well beyond the vagaries of preservation and questions about representativity of samples, for what bone lesions mean, in terms of individual and population health, is far from clear. For example, Hutchinson (1993:113) has emphasized that infectious lesions on bone are not necessarily indicative of the prevalence of a condition in a population, since "diagnostic lesions often occur in only the most severe or advanced cases of a disease." Even knowing what those lesions mean in the individual is difficult; just because one successfully diagnoses the condition that caused the pathology, one does not know precisely how the individual's life and overall health were affected (Rogers 1993:167). Ortner (1991:9) further complicated the picture with his suggestion that evidence of skeletal infection may in fact be evidence of a healthy immune system responding well to an insult, rather than radically compromised health. The corollary to this is Ortner's questioning of the "easy assumption... that a relatively high prevalence of cases of infectious skeletal disease is, indeed, indicative of decreased population health" (1991:9). Stuart-Macadam (1991:37) has added the suggestion that porotic hyperostosis, long regarded as evidence of an "inability to adapt," may in fact be evidence of a beneficial escalation of the body's response to infection.

In their recent and dramatic article, Wood *et al.* (1992) summed up the deepest problems which osteoarchaeological analyses must face. In a nutshell, even ignoring the difficulties of accurate age and sex determination, fragmentary, jumbled, and poorly preserved remains, differential diagnosis of pathologies, *etc.*, there are three unavoidable, devastating problems: "*demographic nonstationarity, selective mortality, and hidden heterogeneity in risks*" (Wood *et al.* 1992:344).

Wood *et al.* further stated that: every skeletal sample is so intrinsically biased that "we doubt that it will ever be possible to estimate population prevalences reliably from skeletal lesion frequencies" (1992:344); that the meaning, with respect to individual and population health, of many pathologies is inherently ambiguous; and that paradoxically, it may often be that "[b]etter health makes for worse skeletons" (1992:356).

Wood and colleagues illustrated what happened when they asked embarrassing questions about some key assumptions in osteoarchaeology, by referring to some well-known Illinois skeletal series, including an Oneota sample, and the Dickson Mounds Late Woodland and Mississippian samples. Their explorations of the mortality pattern in Oneota children up to five years of age illustrate that "a given aggregate mortality pattern can result from an infinite number of possible combinations of subpopulation mortalities," which is a problem in archaeological situations, where the number of subpopulations is not known (Wood *et al.* 1992:348).

Another example given is derived from the Dickson Mounds skeletal series, where increases over time in enamel hypoplasia prevalences, concomitant with a lower mean age

at death, were postulated by Goodman and Armelagos (1988) to mean one of three things: first, that the enamel defects were an expression of general frailty; second, that the stress resulting in the defect caused susceptibility to stress later in life; or third (and most likely, they felt), that general stresses arising from social conditions produced both enamel defects and premature death in a disadvantaged group (Wood *et al.* 1992:354-5). Wood and coworkers showed that there is a fourth, albeit initially counterintuitive, hypothesis which could account for the pattern seen in the data – that the higher rate of hypoplasia was in fact an indication of survival of stress in an *advantaged* group, and that the lower mean age at death was a result of increased fertility, not higher mortality (1992:355).

The implications of this kind of alternative interpretation for population health are, of course, profound. In the case of Dickson Mounds, it is implied that over time, the group as a whole became more healthy, not less; with respect to the transition to agriculture, it is implied that osteological evidence which is usually interpreted as a deterioration in health in early farmers (*e.g.*, Cohen 1989 and Roosevelt 1984) could just as easily represent the opposite (Wood *et al.* 1992:356). Wood and colleagues finally conclude that "choosing among competing interpretations of the osteological evidence requires tight control over cultural context as well as a deeper understanding of the biology of frailty and death" (1992:358).

Given these problems in interpreting skeletal evidence, it may be that skeletal evidence is insufficiently reliable to arbitrate in archaeological disputes regarding the effects of a given factor on a population's well-being. The next question to consider is

whether or not the hypothesis-testing approach is in fact the only alternative, and whether or not it is always an appropriate mode of inquiry into past health.

Hypothesis-testing

The observations described above severely undermine the assumptions, prevalent on varying levels and to varying extents in osteoarchaeology, that bones provide direct evidence about past health, and that testing a hypothesis with osteological data provides a basis for evaluating its veracity. However, the reasons for the adoption of the hypothesis-testing mode of research in osteoarchaeology are undoubtedly complex, and many; like most trends in science (Kuhn 1970), this surely has as much to do with history as any inherent superiority in the methodology now favoured.

Salmon wrote of the hypothetico-deductive method that:

> According to the standard accounts, the method works as follows: (a) formulate the hypothesis H; (b) deduce .... some prediction, P, which is amenable to observation; and then perform the observation to see whether the stated prediction is true or false. If the prediction is true, the hypothesis is confirmed; if not, it is disconfirmed. (Salmon 1982:34)

However, as Salmon next noted, there is actually an important asymmetry between instances of confirmation and instances of disconfirmation. A true prediction offers inductive support for the hypothesis, whereas a false prediction deductively requires the rejection of the hypothesis. This is the basis for Popper's distinction between corroboration and definitive confirmation of hypotheses; only the former is possible, although definitive falsification is possible (Salmon 1982:35). Popper's method therefore

focuses on the falsification of hypotheses, rather than their corroboration. However, even falsification is not as straightforward as it might seem, because of the problem of auxiliary hypotheses. In archaeology, Salmon noted,

> hypotheses can rarely be isolated for testing. Auxiliary hypotheses regarding the likelihood of preservation of artifacts, the correctness of dating techniques, the care with which fieldwork is undertaken, etc. are almost always intertwined with the hypothesis which is under consideration. Because of this, a false implication statement [a prediction] shows only that at least one of the set of hypotheses being tested jointly is false. The test gives us no clue as to which of the joint hypotheses is false. (Salmon 1976:378)

For an example relevant to osteoarchaeology, let us again consider Powell's work on the Moundville skeletal series, wherein her prediction that supraordinate male individuals would "display more trauma associated with warfare than do subordinate males" was not borne out by her observation that supraordinate males "display less trauma of all types than subordinate individuals" (1988:179). This disconfirmation of her hypothesis could quite easily be a disconfirmation of one of the auxiliary hypotheses embedded within it – for example, that the samples were not biased by involving subgroups within the categories of supraordinate and subordinate males, or that there were not differential burial practices for some victims of violence. Other embedded auxiliary hypotheses frequently seen in osteoarchaeological research include some of the fundamental assumptions questioned by Wood *et al.* (1992) and discussed above.

Thus, both when a hypothesis is supported by an observation, and when it is not supported by an observation, the process of testing offers inductive rather than deductive support for the truth or falsity of the hypothesis (Salmon 1982:35). And thus, although

the framing of explicit hypotheses, and their testing, can be an important research device, the process does not operate under a more compelling or stronger logic than other forms of inductive reasoning. When researchers emphasize that an understanding of past health can be arrived at only through deduction and testing of hypotheses, they underestimate the complexity of the research process.

An even more fundamental, and perhaps even less tractable, concern is that of epistemology. An important part of the post-processual critique of the New Archaeology's hypothetico-deductive method has been the rejection of a positivist/empiricist conception of knowledge (Miller and Tilley 1984b:151). This rejection of the objectivity and neutrality of data has important extensions, of course, in terms of the way scientists approach their research, and with respect to criteria for judging a theory's merits. To put it succinctly, if data are invariably theory-laden, and do not in fact 'speak for themselves,' it makes little sense to think that one can use them as an objective test of a hypothesis. This very fundamental objection to "the definition of scientific method as based on procedures of quantification, testing, and falsifications of hypotheses" (Shanks and Tilley 1989:2) has been raised in archaeology a number of times in the last decade (see also Shanks and Tilley 1987a, 1987b).

Here, I have only scratched the surface of the body of archaeology-based critiques of positivist epistemology and methodology; this ground has been well-trodden by archaeologists interested in theory and before them, by philosophers of science. My intention has been simply to indicate that such critiques exist, and that although I have been able to find no previous application of these or similar ideas to the enterprises of

osteoarchaeology, they are indeed applicable. Why such critiques have been ignored in osteoarchaeology is an interesting question in itself; it may be because, as Ortner and Aufderheide (1991:1) and Lovejoy *et al.* (1982:335) claimed, there has been insufficient attention given to theory in palaeopathology. It may also be that such critiques have been perceived as irrelevant or belonging to another domain, because the study of past health has always had a primary affiliation with the medical and biological sciences, rather than with historical disciplines such as archaeology (Wienker and Bennett 1992).

Regardless of the reasons for the present state of affairs, however, it would seem that the hypothesis-testing approach, as advocated by Cohen (1984), Klepinger (1992), Buikstra and Cook (1980) and Rothschild (1992) above, is, at the very least, not the only answer. In fact, it may be, as Ramsden wrote, that "the scientific method, in the sense of hypothesis formulation and testing, is an altogether inappropriate vehicle for investigating any but the simplest of past phenomena" (1990:178).

The problem of integration

As discussed above, there are a number of approaches to research about past health which involve the integration of multiple lines of evidence. Some authors have used all available relevant information – such as osteoarchaeology, archaeological settlement data, environmental data, ethnohistoric data – in local-level analyses, to shed light upon health at a particular prehistoric site or region (*e.g.*, Milner 1982; Storey 1992; Saunders, Ramsden and Herring 1992), while others have used various kinds of information to

construct broad-scale histories of human health. These approaches are much more difficult to describe in methodological terms than the hypothesis-testing modes of research already discussed.

What the local-level and broad-scale approaches share is the use of different lines of evidence in a non-testing relationship, and the challenge in using them together to create a coherent account of past health. They have used different solutions to this challenge. The small-scale reconstructions, in general, simply present small quantities of information in discrete sections; this solution affords the author the advantage of not having to prioritize the information, but it would seem that it is self-limiting in terms of size. In local-level studies which are greater than a few pages in length, integration is a daunting task. When multiple sources of data are used in book-length descriptions of past health in a certain place, the evidence usually ends up compartmentalized into different chapters, and the actual integration – usually confined to a few pages at the end – is minimal (*e.g.*, Storey 1992; Milner 1982). This may be justified in the way that it was by Cohen (1992), who, in his response to Wood *et al.* (1992), argued that his (1989) conclusions about trends in health through history are the product of independent analyses of several different lines of evidence (skeletal data, epidemiological theory, and ethnographic comparisons), which all indicate similar processes. It is difficult to imagine, however, that these analyses could truly be independent of one another, and one is compelled to wonder what is to be gained by keeping them separate, apart from convenience in consideration and presentation.

The broad-scale accounts of disease in human history, on the other hand, have achieved true integration of a number of different types of information into coherent and elegant descriptions of past health, but at a price; to do this requires an organizing principle or backbone, and the one chosen – an ecological-evolutionary framework – has necessitated the omission of certain types of data, particularly sociopolitical information. Thus, although these broad-scale reconstructions of health in history have more coherence than many local-level, but long-length, integrative analyses, their choice of principles for organization have not been entirely satisfactory.

Shortcomings of the broad-scale accounts of disease in human history: a lack of consideration of sociopolitical variables.

The resemblance of the usual broad-scale reconstruction of disease in human history to older unilineal models of social evolution should not go unnoticed. It can be seen that this reconstruction, in its 'hunter-gatherer band, agricultural village, city' scheme, draws from the worst part of unilineal evolutionary models – that is, the part which is ethnocentric, typological, essentialist, and reductionist (Shanks and Tilley 1987a) – but ignores the better part, which is the detailed attention which has been paid to different forms of sociopolitical organization. Models of social evolution in anthropology have moved on from community size and subsistence technology as prime considerations to a focus on sociopolitical structure (*e.g.*, Johnson and Earle 1987). Similarly, as discussed in more detail in Chapter 3, modern anthropological epidemiology recognizes that sociopolitical variables are critical factors affecting health.

Inhorn and Brown sum up the current understanding of infectious disease causation:

> It is important to note that infection with a specific agent does not necessarily result in disease. This progression depends on a number of intervening variables, including the pathogenicity of the agent, the route of transmission of the agent to the host, and the nature and strength of the host's response. All of these factors, in turn, are affected by the natural and social environments in which the agent and host are juxtaposed.... Critical characteristics of the environment result largely from sociopolitical influences; thus, many infectious diseases ... are rightly considered 'social diseases.' (Inhorn and Brown 1990:90)

Thus, in modern anthropological disease studies, following the lead of such scholars as May (1958), Alland (1970) and Audy and Dunn (1974), the traditional triadic model of disease causation of 'host, pathogen, and environment' has been expanded to include – under the rubric of environment – economic, social, and political factors (Inhorn and Brown 1990:96). The consideration of these factors, and the problem of culture acting as a health stressor, has also been a central aspect of much local-level research. Modern research programs in osteoarchaeology often focus on the effects of social stratification and other aspects of culture upon health in prehistoric communities, and authors such as Goodman and colleagues (1984b:15) have commented specifically upon the ability of cultural systems to cause or amplify health stresses as well as ameliorate them. Goodman *et al.* (1988:169) took this statement a step further with the suggestion that "[b]iological anthropologists are in a unique position to elucidate the human condition if, via concepts such as stress, attention is paid to both human adaptive and political economic processes," and argued that the negative influences of political centralization are the probable causes of the high levels of stress seen in populations of prehistoric Nubia and Dickson Mounds.

It seems well-recognized by most osteoarchaeologists engaged in local-level reconstructions of past health that it is not adequate "simply to demonstrate the presence of infectious agents or population sizes adequate for their survival and propagation" (Saunders *et al.* 1992:118). Yet, it is apparent that this kind of demonstration serves as the basis of broad-scale models of disease patterns in human history; throughout, the focus has been on population size and subsistence technology as the ultimate determinants of disease patterns. This is not to say that such models have ignored other factors completely, but at present, it remains the case in broad schemes that where a sociopolitical factor is considered at all, it is as an ecological variable. For example, where 'sociocultural change' or 'social organization' are discussed, it is principally in terms of population size and density, not in terms of social stratification or political inequality (*e.g.*, Fenner 1970, 1980). Subsistence technology, nutrition and similar issues are thus viewed more as ecological entities than sociopolitical considerations.

Curiously, some authors simultaneously acknowledge their allegiance to the inclusion of sociopolitical factors in studies of disease today, and ignore them in their broad-scale models of changing disease in the past. For example, in Armelagos, Ryan and Leatherman (1990), an extended perspective, including considerations of sociopolitical factors, was employed only with respect to the modern situation of AIDS, and not to the past, where an "ecological-evolutionary" perspective was presumed to suffice. The irony here is amplified by a quote from Goodman, Martin, and Armelagos:

> While political and economic factors are widely regarded to be the primary determinants of health for contemporary populations, they have infrequently been considered to affect the health of prehistoric groups. This

> incongruity appears to stem from the notion that it is analytically difficult to link political-economic processes to health in populations long dead, and that "simple" prehistoric societies were relatively more affected by ecological than political-economic constraints. We contend that both notions are at least partly incorrect. (Goodman *et al.* 1992:51)

Goodman *et al.* then proceeded to use the core-periphery concept[8] to explain the indications of poor health seen at Dickson Mounds.

The situation, then, seems to be this: authors who are well-versed in modern theories of health stress write about the primacy of political, economic, and social factors in disease causation; they apply this idea to modern case studies, such as AIDS; they apply it to a specific prehistoric case studies, such as Dickson Mounds; but in their broad-scale history of human disease, an ecological-evolutionary perspective is retained.

This pattern is very revealing of a problem in the study of prehistoric health, a problem fundamentally of scale. Studies at different scales seek to answer different questions, and therefore should not be expected to be interchangeably applicable to a given situation. Equally, it should be understood that the different scales of endeavour should not be completely unrelated, and that significant understandings from one can often be worked into the other. The two should, ideally, interact in order to refine understandings and shift emphases as required.

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[8] Many different theories have been put forward regarding the relationships of the industrialized world with economically subordinate nations. 'Dependency theory' postulates that capitalist countries ('core areas') have achieved economic prosperity through the exploitation of other parts of the world ('dependent,' 'peripheral' areas), and that because of this relationship, the peripheries develop under different influences than the core areas. Wallerstein's 'world systems theory' counters that the core and periphery distinction is valid, but both sectors develop according to the imperatives of the world economy of which all are a part; Wallerstein further contends that there are important discontinuities between the modern capitalist world order and previous systems (Giddens 1985:162-7).

It is true that when examining subjects of larger and larger scale, it is necessary to zone in on fewer and fewer variables for simplicity's sake; a complementary fact is that fewer variables are needed in a model in order to give a reasonable approximation of a large-scale system's behaviour than are needed to model a small-scale system. Scale theory tells us that as the phenomenon you wish to model becomes larger and larger, the number of key variables grows smaller and smaller (Meentemeyer and Box 1987).

One of the results of the above effect of scale is that variables which are derivative from those under direct consideration are often ignored; for example, sociopolitical structure could be regarded as a minor variable which is a derivative of the major variables of population size and subsistence. However, what the 'main' variables of concern should be is subject to debate – and in this case, because of the tremendous amount of research demonstrating the relevance of sociopolitical structure to disease in the small scale, a good argument can be made for its explicit inclusion in large-scale reconstructions.

Approaches to past health using multiple lines of evidence are, of necessity, varied and complex. So are their shortcomings. In the case of long local-level analyses of health, true integration is sacrificed in order to be all-inclusive; in the case of short broad-scale analyses of health in history, integration is achieved at the cost of eliminating crucial evidence. Neither is an entirely satisfactory model for emulation.

Summary

Prehistoric health has been studied from a number of perspectives, using different methods and sources of information. Archaeological data have been used in these different methods in a number of ways. All of these variations are valuable in certain contexts; all have shortcomings.

Osteoarchaeological investigation has progressed from the early days of palaeopathology, when the primary focus was on the diagnosis of infection in the individual specimen, to an interest in the general health of populations. This change has considerably broadened the horizons of osteoarchaeology; however, significant problems in data interpretation remain. Furthermore, it can be argued that the use of osteological data to test archaeological theories is, in certain situations, rendered questionable both by methodological concerns, and by the often-compromised quality of the data themselves.

Archaeological evidence has played a different role in some studies of past health, as one of a number of equal varieties of data. In cases where multiple lines of evidence are used in local-level analyses to shed light on a particular place and time, the primary challenge is in truly integrating the evidence into a coherent theory. Where multiple lines of evidence are used to create broad-scale histories of human health, the attempts have been compromised by the ecological-evolutionary framework used, which has neglected a facet of archaeological evidence which has been used in local-level osteoarchaeological inquiries, namely evidence regarding sociopolitical factors affecting health.

My main points in this chapter have been these:

First, archaeological data are no less 'direct' a source of information about past health than osteological data; the interpretation of the latter is also theory-laden. Second, the relationship between osteological and archaeological data has been varied – and appropriately so – but a hypothesis-testing approach wherein osteological data are used to test archaeological theories is not superior to other approaches. Third, a consideration of social and political factors in any study of prehistoric health, no matter what the scale, is critical. Fourth, the integration of multiple lines of evidence into theories of prehistoric health, while desirable, is difficult to truly accomplish without paying an unacceptable price, and some explicit guidelines and methodology would be helpful.

There are a number of different directions which could be taken, based on these points. The one that I have chosen is the exploration of prehistoric health through archaeological data alone, with emphases both on disease ecology and on social and political factors. In the next chapter, I will discuss the possible parameters of such an approach, including conceivable ways of managing large quantities of data in a manner that is meaningful but not restrictive.

The reasons I have chosen to follow this path are several. First, I contend that the utility of archaeological data as bases for inferences about past health has not been explored very fully, and that an independent investigation of their potential is in order.

Second, the solutions to many of the problems faced by osteoarchaeology today may lie in a better understanding of archaeological data. Wood *et al.* (1992:357-8) concluded their rather gloomy review of osteological difficulties with a list of theoretical contributions that will help to resolve the problems; notably, the contribution that they

feel anthropology in general is best able to make is the "development of a better understanding of the role played by cultural context in determining heterogeneous frailty and the level of selective mortality." Jankauskas and Česnys (1992:360) concurred, stating that "[t]he problem can be solved only through close collaboration with archaeologists."

Finally, as Ortner (1992) commented, all the new osteoarchaeological methods and tools in the world will do nothing to further the study of past health if skeletal samples are not available for study. In the Americas, concern for the repatriation and reburial of native remains is becoming more and more common (Ubelaker and Grant 1989); it would seem our responsibility to explore the alternatives.

CHAPTER III

SOME RESPONSES TO THE CHALLENGES OF STUDYING HEALTH IN PREHISTORY

Untested theories cannot compete for serious attention in the arena of modern science ... an unverified hypothesis has the epistemological status of a daydream. D.H. Thomas 1972:674

... there are no coherent grounds for the belief that we can test against an independent non-subjectively defined reality. The testing process provides no more certainty than if we had not tested a proposition. M. Shanks and C. Tilley 1987a:42

The conflict over how and what we can know about the past, illustrated in one of its more recent incarnations by the above quotes from Shanks and Tilley (1987a) and Thomas (1972), is a complex, recurring problem which bears further exploration here, as do some of the other difficulties described in the last chapter. The present chapter is devoted to a discussion of some responses to those challenges, while later parts of this thesis attempt to put some of these ideas into action.

In chapter 2, I outlined several problems with the customary approaches to the study of past health. The first of these is the belief among some researchers that hypothesis-testing is the only appropriate methodology for the study of past health. As demonstrated, the hypothesis-testing approach has its own flaws, and has no claim to logical superiority over other reasoning processes. However, the positivist conception of

knowledge and the hypothesis-testing mode of research do provide straightforward rules for scientific practice. Their rejection has profound implications – though not as negative as might initially be assumed – which I briefly address in the first part of this chapter.

Two problems described in Chapter 2 have contributed to the under-utilization of archaeological data in the reconstruction of past health. The first of these is a tendency, primarily among some osteoarchaeological researchers working at the local level, to regard archaeological data as inferior sources of information compared to data derived from skeletal analysis. The second is the use, in broad-scale studies of past health, of an ecological-evolutionary framework which systematically favours the consideration of variables such as population size and subsistence technology over information regarding social interactions. As discussed earlier, however, archaeological data are no more subjective in nature than osteological data, and can offer insight into areas of social life, which, in turn, are of prime importance in determining disease patterns. The second section of this chapter, therefore, elaborates slightly on the influence of social phenomena upon community health, and summarizes the different ways in which archaeological evidence regarding social factors (as well as more mundane concerns) has been used, and can be used, in studies of disease at the local level.

A final challenge defined in Chapter 2 is that of integrating different kinds of evidence, including archaeological data, into synthetic interpretations of prehistoric health. Broad-scale studies circumvent this difficulty neatly, but their organizational framework is probably not a good choice for emulation, and local-level studies usually do not achieve much integration at all. The *Annaliste* model of historical time is presented in this chapter

as an alternative overarching construct which may be used to integrate different types and scales of information in local level studies.

TOWARD ALTERNATE CONCEPTIONS OF RESEARCH IN PAST HEALTH

It has been suggested that, by and large, the philosophy of science is irrelevant to the doing of science itself, since it describes the scientific process more than providing guidelines for use (Chalmers 1982; Feyerabend 1975; Watson 1991). This may explain the comparative lack of concern with such philosophy in the literature on prehistoric health, upon which I commented in Chapter 2. However, as Gibbon (1989:6) observed, "philosophical perspectives and substantive research are inextricably interwoven; to view them as separate spheres of discourse is but one result of our having adopted a positivist philosophy of science in archaeology." Philosophical questions – of how to evaluate theories, choose between them, distinguish legitimate archaeology from propaganda and tall tales, and legitimate archaeologists from charlatans and the misguided – are important to consider, not only for purely theoretical reasons, but also to demonstrate that in practice, the rejection of positivist conceptions of data and falsificationist rationalism does not inevitably lead to a relativist quagmire where belief is impossible and all answers are equal. In turn, this should encourage discussions of methodology in the study of past health which are not limited to advocations of hypothesis-testing strategies, any more than the research itself is limited to the use of such strategies.

Jettisoning the notion of data as entirely objective, and rejecting falsificationism as a guiding principle, does not lead researchers inevitably into a methodological limbo, stripped of their status as scientists, although this is what some imply and others fear (Trigger 1989a, 1989b; Renfrew 1989). The reaction against falsificationism should not be read as a lack of concern with data, or merely a comment on the formal logic involved. It should be considered an advocation of not only those explanations for a body of data which can be tested against a second body of data, but also those explanations for data which do not lend themselves to testing.

This is obviously understood by many of those conducting research into past health; as mentioned in Chapter 2, there are studies which do not conform to a hypothesis-testing methodology, and statements unconcernedly made about past health which are not testable. However, discussions of the methods which *are* used in these situations to make interpretations about past health are conspicuously absent. Whether or not scholars researching past health think about such matters, they rarely write about them, perhaps feeling that it is best to leave this to professional philosophers. The result has been an almost complete absence of philosophical discussion in the health in prehistory literature, punctuated only by the occasional inflexible statement about the way scientific investigations are to be carried out (*e.g.*, Rothschild 1992; Klepinger 1992). I consider it is a disservice to the field to limit consideration of problems in metaphysics and epistemology in this manner, and so, a few brief observations on the subject follow.

First, the achievement of correspondence between data and theory need not involve formal testing procedures; there is no "neutral algorithm" for finding truth (Kuhn

1970:200). Thus, a theory need not be testable to be considered scientifically valid, as long as the appropriate 'tacking back and forth' – successive comparisons and refinements – between our own explanatory constructs and the data from the subject culture has taken place (Wylie 1993). Second, while all data, archaeological and osteological alike, are theory-laden (Chalmers 1982; Wylie 1993), it seems clear that the extreme relativism which is one logical outgrowth of this realization is a position of critique, not of agency (Yoffee and Sherratt 1993:8). It is, after all, agreed by working archaeologists of all ilks that the data will constrain which interpretations it is possible to make (Shanks and Tilley 1987b; Trigger 1989a; Hodder 1989; Wylie 1993). Yet, this rejection of extreme relativism is not to say that the theory-ladenness of data isn't cause for concern and humility; but neither is it to suggest that it is always important to decide which explanation for a given phenomenon is best.

The conflation of two levels of analysis – that of the individual theory (theory appraisal), and that of multiple theories regarding the same phenomenon (theory choice) (Chalmers 1982)– is to be avoided. A separation of the two questions of 'is theory A a good theory?' and 'is theory A better than theory B?' provides a choice of where the emphasis is to be laid. In many aspects of the study of past health, it may be more productive to focus on theory appraisal than theory choice. The need to choose one explanation is felt most strongly when one is looking for 'the truth' about what happened in a particular time and place. For example, the debates regarding the existence of tuberculosis in the Americas prior to contact (Buikstra and Williams 1991), and the origins of venereal syphilis (Baker and Armelagos 1988), or even which theory of

subsistence intensification is correct (Cohen 1984), have meant that a great deal of research time has been devoted to providing single answers to highly specific questions. I would argue, however, that the results have been mixed.

On the down side, while it may be interesting to know that tuberculosis was present in the precontact Americas, this knowledge alone is not of much use. Tuberculosis infection can produce a wide range of end results, varying from a complete absence of symptoms to rapid death; the outcome is highly dependent upon the circumstances of the individual and the community (see next section for elaboration). Thus, this is another case where, as Saunders, Ramsden and Herring (1992:118) commented, demonstrating the pathogen's presence, and a population large enough to sustain it, is not enough in itself to offer much understanding. Similarly, answering the question of whether the unitarian, Columbian, or Precolumbian theory of the origin of venereal syphilis is accurate will, in itself, not contribute much to our understanding of past health. Saunders (1988:727) questions this emphasis, asking, "why the persistent fascination with the *origin* of syphilis?" and adds that worthy research problems lie in "the fact that infectious diseases are complex biological puzzles reflecting a range of interactions," and "not in origins alone." If the goal of research into past health is to explore the possibilities for all peoples in the past, rather than finding out 'what really happened' to People X at Time Y, or if Disease Z originated in one place or another, the urgency of problem of theory choice is alleviated a little.

It would be overly cavalier, obviously, to completely dismiss the tradition in prehistoric health research of seeking to answer highly specific questions about particular

diseases, even if only because the search – through testing of falsifiable hypotheses – for single accurate answers to these questions has successfully introduced many new research tools into palaeopathology. For example, the tuberculosis controversy contributed to the use of mathematical modelling for prehistoric epidemics (McGrath 1988a), and both the TB and syphilis debates have prompted the use of immunochemical techniques of analysis on prehistoric bone (Rothschild 1992:137). I would argue, however, that many of the most interesting and important questions to be considered regarding prehistoric health cannot be answered by looking through a better microscope.

Finally, there is a positive side to relativism – what Shanks and Tilley (1987b:12) call "the positive value of subjectivity" – which is only infrequently discussed. As Kohl (1993:14) puts it, it liberates us from insisting upon mathematical rigor "for its own sake" and allows us to form and use "impressionistic, qualitative judgments," to exercise our creativity which so often can lead to great understandings. Bradley (1993:131), too, advocates a rediscovery of creativity in archaeology, and an escape from the "emphasis on the minutely physical" which is such a trademark of much modern archaeological research. The same may also serve students of prehistoric health well. For example, although Rothschild (1992:131) suggests that "[i]f a testable hypothesis cannot be generated, perhaps the problem should be placed on hold, or an interdisciplinary council formed for determining new technologies appropriate to its assessment," there is much to be gained by thinking about questions which are not necessarily answerable. The wondering itself can lead to great insights. For example, although we do not, and arguably cannot, know for certain whether the Dickson people became healthier or sicker over

time, the speculation has certainly furthered the study of past health. Even the invention of a number of different scenarios which could account for a given pattern in the data – as Wood and colleagues (1992) did – is not an empty enterprise just because a single 'right' answer has not been found.

These brief comments do not provide a well-defined methodology for how to proceed in studies of past health, but rather, suggest some slight but important redirections in terms of how our task is to be represented, and perhaps, in terms of how we conceive of it. In fact, this absence of a definite prescription for progress is the point. Replacing a dogmatic advocacy of an explicitly hypothesis-testing mode of research with advocacy of another would be pointless; as Feyerabend (1964, 1975) soundly argued, a plurality of methodologies is necessary for progress to be made, and the primary criterion of legitimacy should simply be a true concern with the problems and challenges of understanding, rather than conformation to arbitrary procedures. Gibbon (1989:180) wrote that "archaeology is more uncertain, open, challenging and perhaps anxiety-ridden enterprise than our positivist heritage has indicated"; the same, I would say, is true of the study of health in prehistory, for contrary to the programmatic suggestions sometimes made in this literature (*e.g.*, Rothschild 1992; Buikstra 1991), there is no one definitely right way to proceed.

SOCIAL FACTORS AND DISEASE: POSSIBILITIES FOR ARCHAEOLOGICALLY-BASED INFERENCES ABOUT PAST HEALTH

As discussed in Chapter 2, archaeological data are used in a variety of ways in studies of past health at the local level. Most of the time, they are used to provide context for osteological studies; sometimes they are themselves used as bases for substantive inferences about health. It is the latter use which is of concern here.

Archaeological data have been used to shed light on ecological variables, such as contamination from refuse accumulation, subsistence, population density, habitual activity patterns, and settlement organization, which are all important to disease causation (*e.g.*, Milner 1982:103-123; Powell 1988:62-65; Storey 1992:50-70; Saunders, Ramsden and Herring 1992; Larsen *et al.* 1992) In these cases, the inferences drawn from the archaeological data are usually fairly basic, and relatively unassailable. For example, there is little to argue with in Larsen *et al.*'s contention that midden contamination of wells in La Florida "could likely lead to diarrheal infections" (1992:33), in Saunders and colleagues' suggestion that the dogs and rodents found at Iroquoian villages would have heightened the risks of contracting zoonoses (1992:121), or in Milner's observation that increasing size and density in Mississippian settlements would have enhanced opportunities for transmission of disease by the faecal-oral route, or from person to person (1982:118). However, archaeological data have also been used as bases for making higher-level inferences regarding the effects of social organization and behaviour on health. Before discussing ways in which this has been done and can be done, however, a brief overview of select aspects of epidemiological theory seems appropriate.

Some notes on epidemiological theory

In the last century, major leaps have been made in our understanding of disease. From a simple cause and effect model of 'one pathogen, one disease' (which was itself a breakthrough), medical anthropology and epidemiology moved on to regard disease as a product of the interaction of a triad of variables: the host, the pathogen, and the environment (Swedlund and Armelagos 1990). As epidemiological theory has changed, the interpretation of 'environment' has broadened past the initial focus on climate, geography, topography, and biotic factors, and an appreciation of the specific importance of the social environment and human behaviour to disease processes has increased. For example, in the 1950s and 1960s, Bates and May argued that cultural systems (including technology, social organisation, and ideology) must be considered with environmental concerns (Swedlund and Armelagos 1990). Later, Audy and Dunn (1974) developed the idea of disease as a result of a range of 'insults,' including psychological and social as well as physical factors, and noted that cultural practices can ameliorate or exacerbate the effects of such insults, or even *be* an insult to health. Thus, the original 'doctrine of specific etiology' was replaced by a comprehensive model incorporating biotic, abiotic, and cultural factors. This 'biocultural model' has been the basis of much anthropological study of disease (*e.g.*, Moore *et al.* 1980, Janes *et al.* 1986, Mascie-Taylor 1993, McElroy and Townsend 1989) but already, a new vision has begun to emerge in the subfields of medical anthropology and applied anthropology, especially where the focus is on countries with post-colonial economies.

An example of this new perspective is provided by Turshen, who argues that the epidemiological model which focuses on the triad of agent, host, and environment is woefully inadequate, as it falls short of explaining the ultimate causes of disease. Turshen contends that the inclusion of social considerations into the triadic model, while a slight improvement, has merely been an adjustment to a fundamentally flawed conception which inappropriately "locates sickness in the individual's body (as opposed to the body politic)" (Turshen 1984:11). In Turshen's Marxist framework, the pathogen itself is of comparatively minor concern in relation to social, political and economic influences on health: "[i]f only human action has causal efficacy, then germs cannot be said to cause disease, and the empiricist model of infectious disease etiology, which is central to epidemiology, is inaccurate" (1984:16). Turshen's perspective regarding the position of social, political, and economic variables in disease causation is perhaps more strongly stated than most, but their importance is gaining increasing recognition.

Social factors may thus be said to affect human disease experience in many ways. Three particular but intertwined aspects of social behaviour as related to disease which are relevant here are: the culturally based definition of illness; the effects of social networks upon exposure to infectious disease; and the influence of sociopolitical factors on the outcome of infection.

First, what is defined as health or illness varies considerably between cultures (Lewis 1993). Similarly, health and disease are social phenomena in that their significance to the individual is often socially determined (McElroy 1990). Further, sick roles vary considerably within societies, according to the type of disease experienced, as well as

between societies. These aspects of the social construction of illness have been widely discussed in medical anthropology, partly because of attempts to improve health in societies which do not share the Western biomedical conception of disease (Lewis 1993:94). [1]

Second, one of the central axioms of epidemiology is that "disease does not distribute itself randomly in human populations" (King and Solskone 1988:950), and it is accepted that social networks are the key to this distribution (McGrath 1988b). This has long been appreciated by medical workers observing different frequencies of infectious disease in different social groups (Trostle 1986). However, this fact is a difficult one to cope with mathematically (Bailey 1975), and so the influence of population structure on disease spread has only been explored, and fully accepted, as a consequence of attempts to model the development of the current AIDS pandemic (Sattenspiel *et al.* 1990).

Third, sociopolitical factors exert a strong effect on the expression of infectious disease, both in individuals and in population. Even in the case of severe virgin soil epidemics, it has been shown that the microorganism itself is not necessarily the key factor in determining the extent of illness or loss of life; rather, "high mortality is firmly embedded in the disintegration of daily life which accompanies community-wide sickness"

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[1] Social constructions of disease come in many forms: for example, measles was traditionally regarded in Hong Kong "not as illness but as a stage of development," but other groups may consider an attack of infectious disease to be due to sorcery (Lewis 1993:96-98). Examples a little closer to Western experience include the social construction of homosexuality as a medical illness (Jones and Moon 1992:7), and the metaphors which affect our understanding of tuberculosis, cancer, and AIDS (Sontag 1989).

(Herring 1992:158).[2] It is a well-known and understood reality that social structure and nutrition can both be severely disrupted by general social/political upheaval, often resulting in a change in the population's health status. McGrath, for example, wrote that "a single pathogenic organism can manifest infection in more than one way in any given population, depending upon the conditions at the time" (1988b:329). In her mathematical model, severe disturbances in a population can be enough to push the existing interaction of disease and community through a "breakpoint" into a new state (*e.g.*, from an endemic state to an epidemic state).[3] And, she notes, "a primary category of biological event that severely disrupts the system is nutritional stress" (McGrath 1988b:329).

The synergy in the modern world between hunger and disease, and famine and epidemic, is all too well known. Figure 3.1 shows the range of possible responses to infection for three diseases, tuberculosis, measles, and rabies; for tuberculosis and measles, there is a considerable range in the outcome of infection with the pathogen. In some cases, such as tuberculosis, infection does not necessarily cause disease, while in

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[2]A particularly vivid example of this is provided by the measles outbreak among the Yanomamo in 1968. The vast majority of the Yanomamo had not previously been exposed to the measles virus; it hit their communities very hard, resulting ultimately in a death rate of 9%, which is very high even compared to other virgin-soil measles epidemics. This unusually severe impact was attributed to the collapse of community organization, resulting in shortages of food and water, and to respiratory complications brought on by the Yanomamo habit of lying immobile in their hammocks, which was in turn caused partly by a resignation to the wishes of the evil spirits which brought the disease (Neel 1982:49-50).

[3] The tuberculosis outbreaks experienced by many aboriginal groups in the Americas during the 1800s were of a magnitude and severity that would normally indicate a virgin soil epidemic, yet it seems probable that tuberculosis was present in these populations prior to contact. A possible explanation, as put forth by McGrath (1988b), is that tuberculosis was indeed present before contact in these populations, but its manifestation was changed as a result of relocation and other changes which had sociopolitical upheaval as their basis.

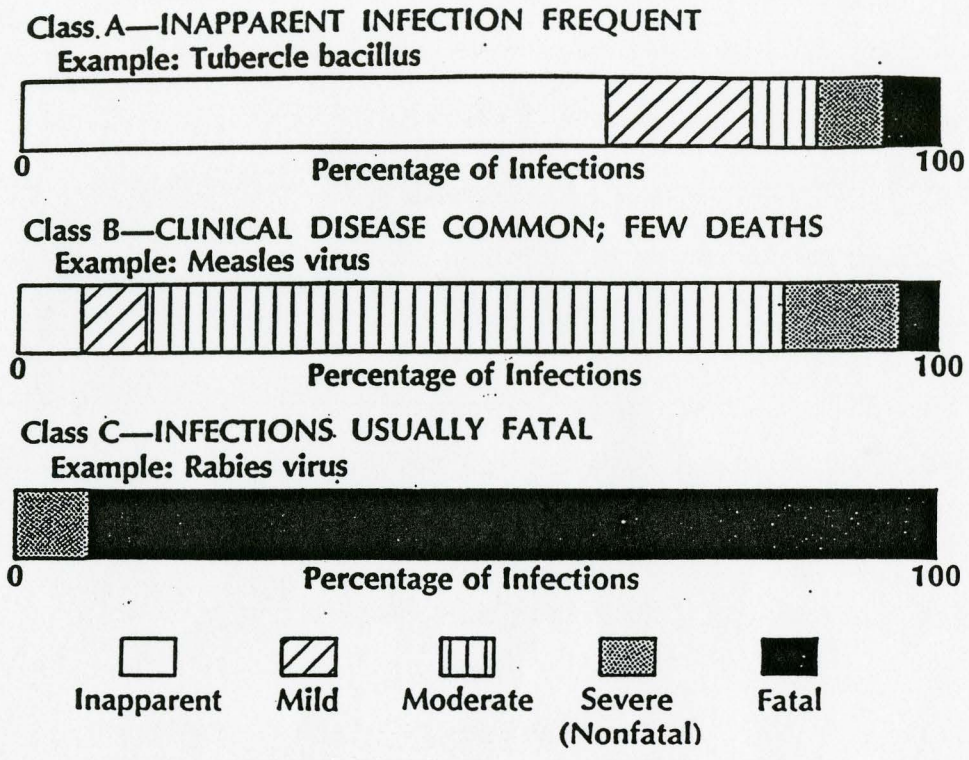

Figure 3.1 Varying clinical severity in three classes of infection. (Mausner and Bahn 1974:23)

Nutritional Influence on Outcomes[a] of Infections

DEFINITE	EQUIVOCAL OR VARIABLE	MINIMAL
Measles	Typhus	Smallpox
Diarrheas	Diphtheria	Malaria
Tuberculosis	Staphylococcus	Plague
Most Respiratory Infections	Streptococcus	Typhoid
Pertussis	Influenza	Tetanus
Most Intestinal Parasites	Syphilis	Yellow Fever
Cholera	Systemic Worm Infections	Encephalitis
Leprosy		Poliomyelitis
Herpes		

a Morbidity or mortality

Figure 3.2 Nutritional Influence on Outcomes of Infections. (Bellagio Conference 1983:308)

other cases, such as measles, the severity of the disease can be quite variable. Figure 3.2 shows that nutritional status is a contributor to this variation in expression, as it is known to affect definitely the outcome of measles and tuberculosis infections, amongst others; hence the synergy between hunger and disease in individuals, and famine and epidemic in populations. In turn, it is well-recognized that the ultimate cause of such chronic pressures is usually sociopolitical rather than environmental (Turshen 1984).

Taylor (1983:285) notes that famines and epidemics share the same ultimate causes – "social forces such as wars, migrations, and political disruptions." Thus, as folk wisdom has said for generations, the two tend to coincide. However, Taylor expresses concern that famines and epidemics, as dramatic events, receive an undue proportion of the attention accorded to disease and hunger. He observes with dismay that in the modern world, "[t]he impact of the synergism between malnutrition and infections, however, continues almost unrestrained because epidemics and famines were only the tip of a very large iceberg" (Taylor 1983:288). In reality, Taylor notes, conditions like long-term hyperendemic malaria and chronic mild hunger have a more significant impact on the health of a population than such dramatic, short-term events as epidemics and famines.

These are only a few examples from the substantial epidemiological and anthropological literature of ways in which social behaviour can affect community health, chosen because of their relevance to archaeologically-based inferences about health, discussed below.

Using archaeological data regarding social behaviour to make inferences about past health

As mentioned above, archaeological settlement data have often been used to make inferences about disease ecology in prehistory. However, archaeological data have also been used as bases for inferences about the effects of different kinds of social behaviour on disease. In particular, there has been an emphasis on politically-caused differential access to food, both within and between communities. There has also been some consideration of the effects of population structure on disease spread. As will be suggested below, however, there are opportunities for further exploration into socially-affected aspects of health in archaeological communities.

Considerations of population structure in archaeological groups have included comments on the relative risk of disease exposure in small and large centres (*e.g.*, Milner 1982:117-8), and mathematical modelling analyses of epidemic dynamics in a regional archaeological system. In her simulations of disease spread in the later prehistoric populations of the Lower Illinois Valley, mentioned in Chapter 2, McGrath found that social structure was actually more important than population size in determining the impact of epidemics. She concluded that knowledge of social organization both within and between communities is critical to understanding the effects of infectious disease on prehistoric groups (McGrath 1988a:495). Thankfully, archaeology is uniquely equipped to provide this kind of information; data and theory regarding intra- and inter-group interactions, including family composition, day-to-day interactions within residential units, vertical and horizontal social divisions (for example, classes and clans), occupational

specialization, trade, and warfare, are available for many of the more complex and well-preserved archaeological sites (*e.g.*, Storey 1992, Pauketat 1991).

Archaeology is also able to provide some information about past political behaviour, through indications of trade relationships between communities, differentiation in mortuary rituals and housing, and aspects of settlements such as fortifications. This information has been widely used in studies of past health. In particular, the effects of social stratification, within and between communities, on nutritional status have been a major focus of research, since the effects of nutrition on health described above are well-appreciated, and because indicators of poor nutrition are visible osteologically. For example, many analyses of Mississippian health specifically test archaeologically-derived predictions about the health of elite and nonelite individuals – these analyses also often include considerations of other sources of stress not related to nutrition, such as trauma (*e.g.*, Powell 1988, 1991; Milner 1982). On the other hand, archaeological data have also been used to explain patterns of health previously observed in osteological data; the investigations of the Dickson Mounds skeletal series, discussed in greater detail in Chapter 4, provide an excellent example. In this case, the location of the Dickson Mounds community on the proposed periphery of a powerful Cahokian political system has been cited as the cause of the Dickson people's apparent poor nutrition and health (Goodman and Armelagos 1985; Goodman, Martin and Armelagos 1992). Goodman and colleagues (1992:58) affiliate themselves with modern critics of capitalism (such as Turshen, above) by drawing direct comparisons between Dickson and modern-day communities suffering from the effects of acculturation and colonial exploitation.

The theories of Goodman and colleagues regarding core-periphery relationships within precapitalist systems in prehistory are based on uniformitarian assumptions about political systems which may or may not be supportable, as discussed in Chapter 5. Regardless, however, their effort to use archaeological data in more than an ecological sense is an important one which has pushed the boundaries of theory about past health. Yet, I would argue that these boundaries could be pushed a little further still, through the consideration of some of the basic social factors relating to disease, described above.

For example, attempts to step outside our own social constructions of disease may be worthwhile. As an illustration, it is possible that our emphasis on treponemal diseases and tuberculosis in precontact Native American groups (Powell 1992; Buikstra and Williams 1991; Baker and Armelagos 1985) relates not only to the observability of these conditions in bone, but also to Western culture's historically well-developed negative responses to these diseases (Sontag 1989; Nikiforuk 1991). At any rate, it should not be assumed that prehistoric Native American responses to these or other diseases would have been the same as our own. Given differing conceptions of the causes of disease, for example, it is possible that among some groups, a very minor ailment (say, a mild skin condition) could have caused grave worries if its etiology was suspected to be supernatural, or if it was unfamiliar; in contrast, a disease which we, in the Western biocultural concept of disease, would consider serious, might not have been a cause for much worry. Of course, exact responses to different conditions are aspects of the sociality of disease which are difficult, if not impossible, to access archaeologically. However, this does not mean that they should not be mused about occasionally, or that we should not

wonder about what past people's health meant to them. Furthermore, in many cases, it may be possible to gain some idea about a group's world-view, or concepts of disease causation, or native pharmacology, from historically known groups of a similar tradition.

Related to the issue of disease definition is the question of responses to disease. Generally in studies of prehistoric health, there is little or no concern for cultural responses to disease; although people are not passive recipients of infection (or negative social influences), prehistoric people are treated primarily as biological entities in studies of past health. The emphasis has been upon exposure to pathogens and the biological responses to that exposure. In addition, there has generally been little consideration of heterogeneity in disease experience within communities except for differences resulting from the obvious elite-nonelite dichotomy; this is a serious shortcoming resulting from the difficulty of addressing the issue using skeletal data (Milner, letter to author, Sept. 20, 1993). However, archaeological settlement data can sometimes offer insights into these matters. We can see something of family structure, larger corporate units, community stability, household economics, and changes in these over time in the archaeological evidence for well-studied sites (Storey 1992, Collins n.d., Hargrave 1991). Such evidence can offer clues regarding care for people in their time of need, as well as very small-scale differences in disease experience. For example, a cluster of four nuclear-family dwellings around communal storage and cooking facilities might suggest that the families in question would be likely to share infections, but also to share in caring for the sick.

Thus, there are a number of advantages to using archaeological data in the study of health in prehistory. As well as offering contextual information for the interpretation

of osteological data, and offering knowledge of mundane but relevant considerations such as garbage disposal patterns and subsistence, archaeological data can provide a basis for inferences about aspects of social behaviour which impact upon disease. In particular, archaeology can supply knowledge about population structure, social stratification, and political relationships between communities. In addition, it may be possible, and productive in some cases, to examine aspects of even more basic social behaviour. The strong emphasis on social stratification in past health has probably come about partly because the elite vs. nonelite dichotomy is the only social difference which is consistently visible in burial contexts for most North American prehistoric populations; however, if one is willing to consider archaeological data in isolation from osteological data, it becomes possible to make basic inferences about localized distributions of, and responses to, ill health. It could be argued that inferences of this nature are highly speculative. This is certainly true; however, the degree of speculation involved is surely no more than that seen in the interpretations of Goodman and colleagues, above, and their non-testability should not in itself be cause for concern.

Unfortunately, any attempt to use archaeological data in a local-level, diachronic study of prehistoric health is complicated by the need to integrate a vast quantity of information of different temporal and spatial scales. Awareness of these relative magnitudes is crucial because of the effects of scale upon modelling and interpretation (Meentemeyer and Box 1987). For example, investigations of disease on a regional level require the consideration of fewer factors than the same investigation on a household scale. The corollary is that analysis of fewer factors on a coarser scale will result in

decreased detail, and inapplicability on a finer scale; however, it can reveal emergent properties which are due to synergisms occurring at a high level of system integration. With a system as complex as a human society, this variability in results from analyses of different scales is obviously an important consideration. One method of organization which explicitly acknowledges the role of information of different scales has been borrowed by archaeologists from history, and will be discussed next.

ANNALES AND ARCHAEOLOGY

The "*Annales* school," a multifaceted movement in French historical thought, has been characterized as a reaction to the writing of histories of political events and individuals, organized in a narrative stream (Fogelson 1989; Clark 1990). Now into the fourth generation of its existence, the *Annales* school has been distinguished at various stages by emphases on the quantifiable aspects of history (*e.g.*, prices, demography), interdisciplinary work, *mentalité*, and structures of time (Clark 1990; Bintliff 1991; Knapp 1992). '*Annalisme*,' however, is not a monolithic or clearly defined entity; as Bulliet (1992:131) put it, the *Annales* school "does not have sufficient coherence and self-understanding to make appropriating ideas from it an easy or straightforward task."

Nonetheless, there are a number of scholars in archaeology and ethnohistory who have adopted *Annaliste* approaches to the past (Fogelson 1989; volumes edited by Knapp 1992 and Bintliff 1991), although their interpretations of the *Annaliste* approach are often divergent and sometimes contradictory. There are, however, several key aspects of

Annaliste history which are agreed to be generally relevant to archaeology, which also happen to be specifically relevant to the study of disease in prehistory. These aspects include the recognition of archaeology as a form of history, rather than its inferior cousin (Bintliff 1991:19; Duke 1992:99; Fletcher 1992), a strong emphasis on breaking down interdisciplinary barriers (Clark 1990; Knapp 1992), and a focus on *mentalité*, or the sum of ideology, beliefs, symbolism, and cultural patterns (Duby 1985; Le Goff 1985; Lucas 1985). However, perhaps the most famous of the *Annales* school's contributions to historical theory and method – and the most relevant to the problem at hand – are Braudel's meditations on the complex nature of historical time. To the dilemma of how to gain some control over time, a central concern of historical disciplines, Braudel provided one potential solution. His epic work, *The Mediterranean and the Mediterranean World in the Age of Philip II*, is explicitly structured around the premise that there are three layers of historical time, the *longue durée*, the *conjoncture*, and the *événement* – concerned, respectively, with "the history of man in relation to his surroundings," "the history of economies and states, societies and civilizations," and the "traditional history" of political events and individuals (Braudel 1980:3).

Braudel's notion of historical processes operating on different time scales, and his trademark emphasis on the *longue durée*, are among his most important contributions to historical theory (Smith 1992; Knapp 1992; Bintliff 1991). However, though there is agreement on the importance of Braudel's formulation of different historical time spans, and on the explanatory power of the *longue durée*, there is no consensus on exactly how Braudel's apparently straightforward scheme is to be interpreted.

First, among archaeologists who use *Annaliste* approaches to history, there is no concordance on the exact number of different time spans.[4] Some, for example, argue that there are in fact four scales of time in the *Annaliste* perspective (Fletcher 1992:37; Lucas 1985:6; Smith 1992:25). In general, however, the standard interpretation of Braudel's scheme seems to be that there are three scales of time, pertaining to different kinds of processes. Bintliff's depiction of Braudel's formulation (Figure 3.3) is a representative example of this 'standard' interpretation.

There is a related debate over the use of Braudel's scheme. Some view the different time spans of Braudel as fixed in length, while others see them as relative. Bulliet (1992), for example, is convinced that Braudel's *longue durée* must be only a few centuries in length, and that the concept, therefore, has little utility for archaeologists. Conversely, others, like Duke, consider the time spans involved to be relative: "the event is defined by its relationship to structure, not just by its length of time. It constitutes a marker of transition, and serves as a point of analysis of changing structural configuration" (Duke 1992:101). The difficulty with this standpoint is that one is then vulnerable to accusations of arbitrariness. Smith felt compelled to add a longer term to Braudel's scheme rather than interpreting the *longue durée* flexibly because "Braudel's association of each temporal level with a suite of relevant sociocultural processes and constraints... represents an empirical finding that arose out of his research" (Smith 1992:25-6). Thus, in Smith's view, the temporal divisions used are not arbitrary, and

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[4] This disagreement is not restricted to archaeologists, but appears in the work of *Annalistes* themselves. For example, while Braudel used the *longue durée* to mean "almost motionless history," La Roy Ladurie uses it for social systems of only a few centuries' span (Lucas 1985:6).

HISTORY OF EVENTS	SHORT TERM—EVENEMENTS	Narrative, Political History; Events; Individuals.
STRUCTURAL HISTORY	MEDIUM TERM—CONJONCTURES	Social, Economic History; Economic, Agrarian, Demographic Cycles; History of eras, regions, societies; Worldviews, ideologies, (*Mentalités*).
	LONG TERM—STRUCTURES OF THE 'LONGUE DUREE'	Geohistory: 'enabling and constraining'; History of civilizations, peoples; Stable technologies, world views (*Mentalitiés*).

Figure 3.3 Braudel's model of historical time. Reproduced from Bintliff 1991:6

therefore they should not be used loosely. Yet others, such as Barker (1991), use Braudel's paradigm almost metaphorically or analogically.

This last interpretation of Braudel's paradigm seems the most appropriate to the research at hand, especially given Braudel's own statement that "these levels are intended only as means of exposition" (1980:4). Thus, although I will use the 'standard' tripartite time scale in my examination of American Bottom prehistory, I will be using it flexibly (or analogically, if one prefers). I would suggest that in general, this is a useful approach to the problem of gaining control over data of different time scales in diachronic studies of prehistoric societies, and furthermore, may be a useful organizational and integrative framework in local-level studies of prehistoric health.

Summary

My intention in this chapter has been to suggest that studies of past health may indeed be able to confront some of the major difficulties described in Chapter 2. The Braudelian model of historical time, for example, can provide a local-level framework for the integration of archaeological evidence in a human and meaningful way that does not require the exclusion of important considerations to past health. Since it uses time, and time alone, as its organizing principle, it permits a scale-sensitive incorporation of all the information which archaeology can give us about past health, including knowledge of social networks and conditions.

Ideas about the ways in which these social factors may have exerted an influence on past health may not always be testable; however, this should not be cause for concern, because the strict rationalist methodology of falsificationism is only one answer to the problems of scientific study. On the other hand, the recognitions that all data are theory-laden, and that the hypothetico-deductive method is seriously flawed, need not result in a relativist tailspin in which nothing can be known about the past, and all theories are equally good, or can be distinguished between only on political grounds. An absence of a single universal criterion for theory appraisal and theory choice does not mean that there are no grounds for these assessments at all.

I argue with Feyerabend for a methodological plurality; the study of health in prehistory should not be limited to testable hypotheses, definitively answerable questions, and the search for singular truths, and indeed it is not. Much existing research on past health does not conform to the canons of falsificationist methodology, and explores areas for which we have little hope of ascertaining exactly what happened in the past. However, to our detriment, this reality is not reflected in the comments on scientific method which are seen in this literature, though perhaps someday it will be.

Finally, then, I would wish that the ideas about health at Cahokia expressed later in this thesis not be dismissed on the grounds that they are not falsifiable, but evaluated according to their correspondence with the data and their internal coherence, and whether or not they illuminate the complexity of the problems and the challenges they present. They should be considered as one of a realm of possibilities, which may have been true there, or in another time and place in the past.

CHAPTER IV

AN INTRODUCTION TO THE ARCHAEOLOGY AND PREHISTORIC HEALTH OF CAHOKIA AND THE AMERICAN BOTTOM

"What a stupendous pile of earth!" (Henry Brackenridge 1814:187)

The considerable height of Monks Mound affords the climber an impressive vista, revealing the enormity of the place now called Cahokia. The effort to understand the people who lived in this extraordinary place has led to well over a century of archaeological research, and while archaeologists have yet to achieve consensus about many of the details, much has been learned. This chapter provides, in turn, a brief characterization of the known archaeological remains, an overview of the history of investigation of Cahokia, and a summary of recent research on the health of Mississippian populations, including those of Cahokia and the American Bottom. The next chapter is devoted to a more detailed examination of some areas of contention in Cahokian archaeology, as well as an exploration of the implications for health of a particular vision of Cahokia.

THE ARCHAEOLOGY OF CAHOKIA AND THE AMERICAN BOTTOM

The site of Cahokia is located in west central Illinois, just south of the confluence of the Mississippi, Missouri, and Illinois Rivers. The joining of these three rivers has created, through millennia of flooding, an exceptionally fertile area of the Mississippi River Valley known as the American Bottom, because of its low elevation (Figure 4.1). Our attention here will be on the northern part of the American Bottom, at its widest, which is the usual focus of archaeological interest (Figures 4.2, 4.3).

Like most major archaeological sites in the region, Cahokia is located at the conjunction of a number of different ecological zones (Figure 4.4). Perhaps because of its abundant and varied resources, this part of the continent has a very long occupation history – just a little to the north, in the Lower Illinois River Valley, the remarkable Koster site has at least 26 distinct occupation levels ranging in date from 7000 B.C. to 1000 A.D. (Streuver and Holton 1979:204). The general area has seen, as well as relatively continuous use through the ages, two major periods of cultural efflorescence, known as the Hopewell and Mississippian cultures or interaction spheres.

Cahokia itself also has a long history of occupation, as does the American Bottom area in general. (See Figure 4.5 for a chronology, based primarily on ceramic seriation.) There were people in the American Bottom region by at least 12 000 BP (Mink, Corley and Iseminger 1992). The first substantial occupations appeared during the Archaic period (8000 - 600 B.C.), while the earliest burials and structures known in the area occurred in the Middle Archaic, as did the earliest domesticated plants (Woods 1986).

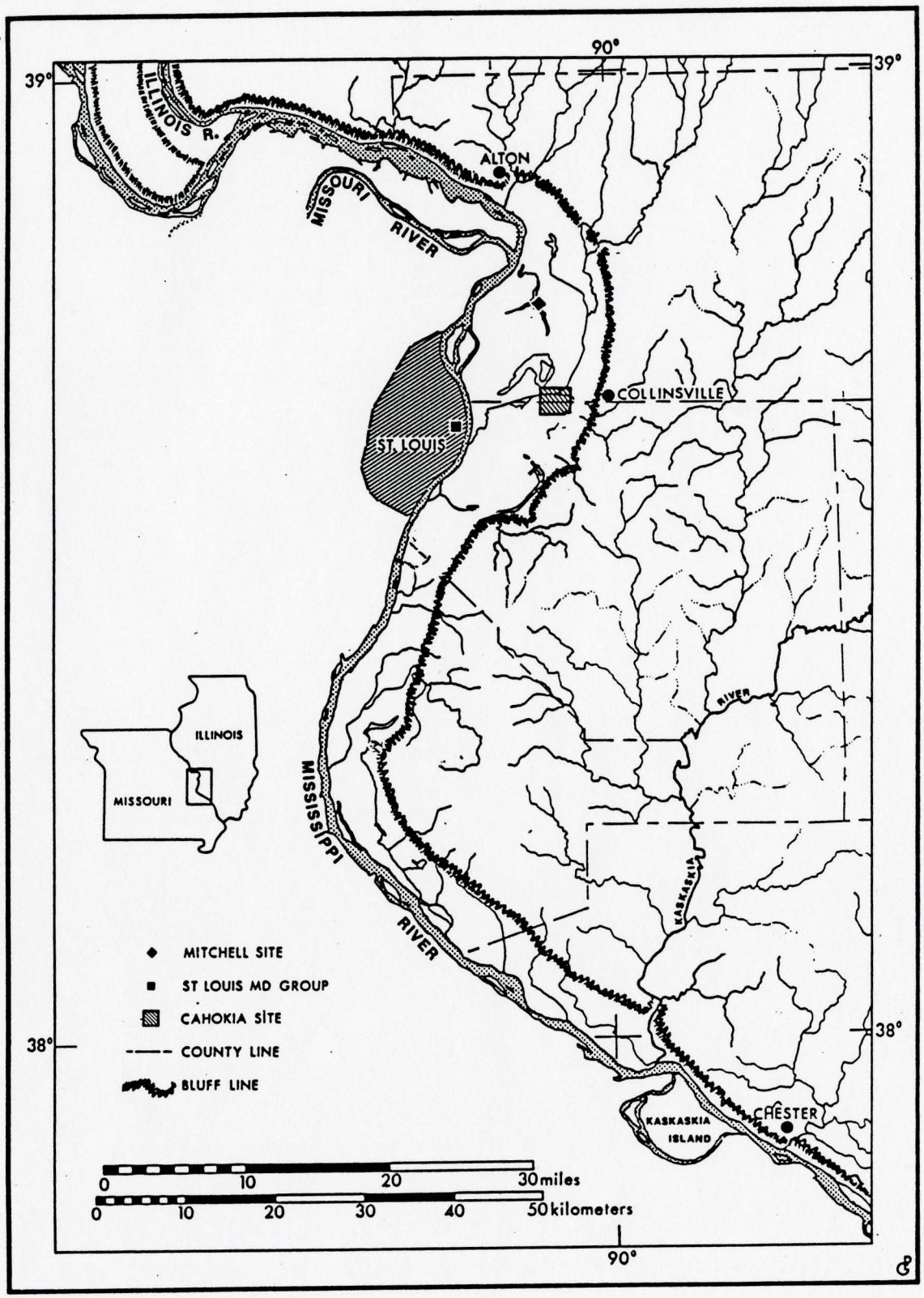


Figure 4.1 The American Bottom stretches from Alton to Chester, Illinois, lying between the Mississippi River and the bluff line. Adapted from Bareis and Porter 1984:5.

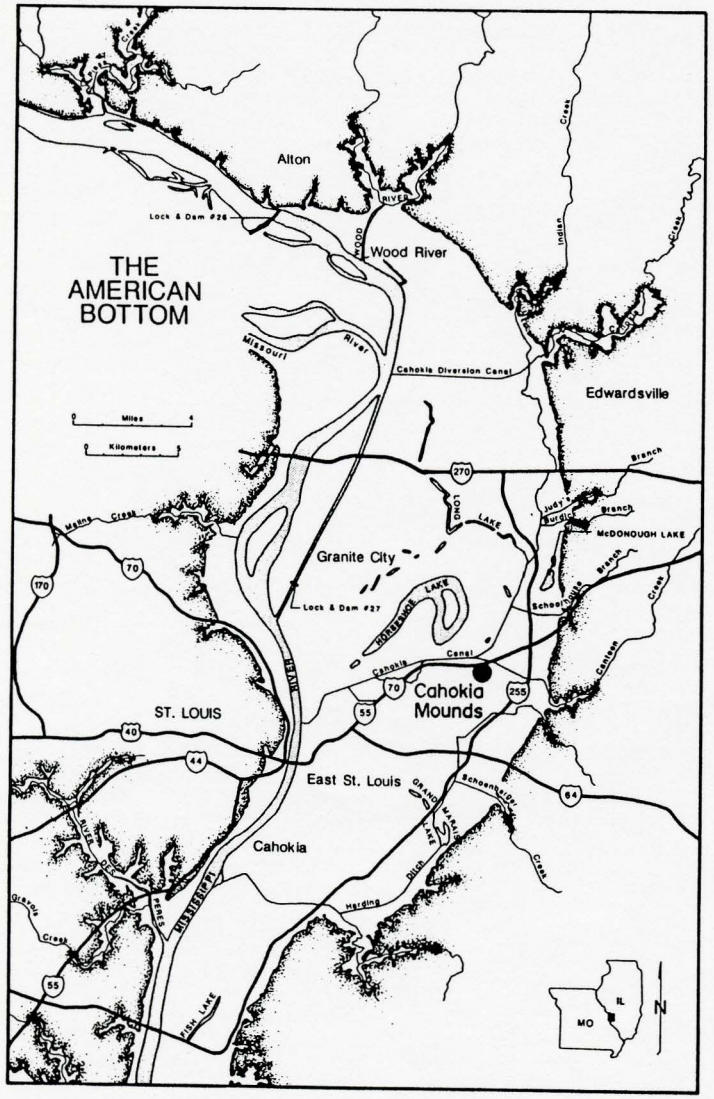


Figure 4.2 The northern American Bottom at present. Note positions of highways 55/70 and 255. (Reproduced from Fowler 1989:2)

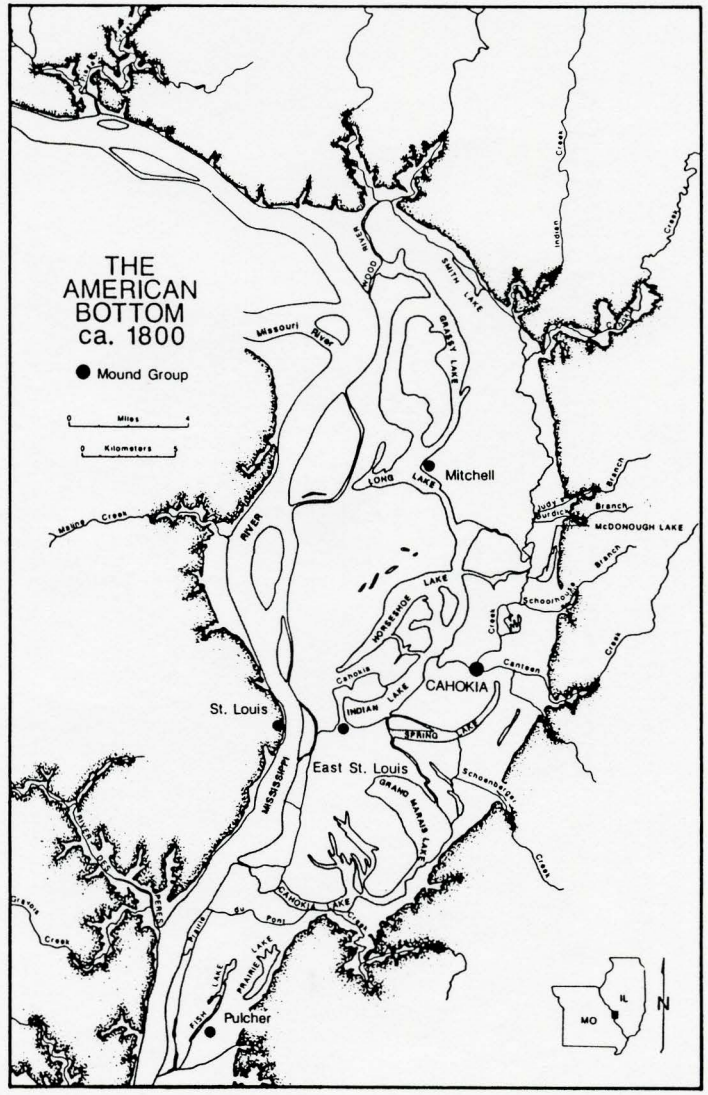


Figure 4.3 The northern American Bottom ca. 1800 A.D., showing major Mississippian mound groups and bodies of water. (from Fowler 1989:3)

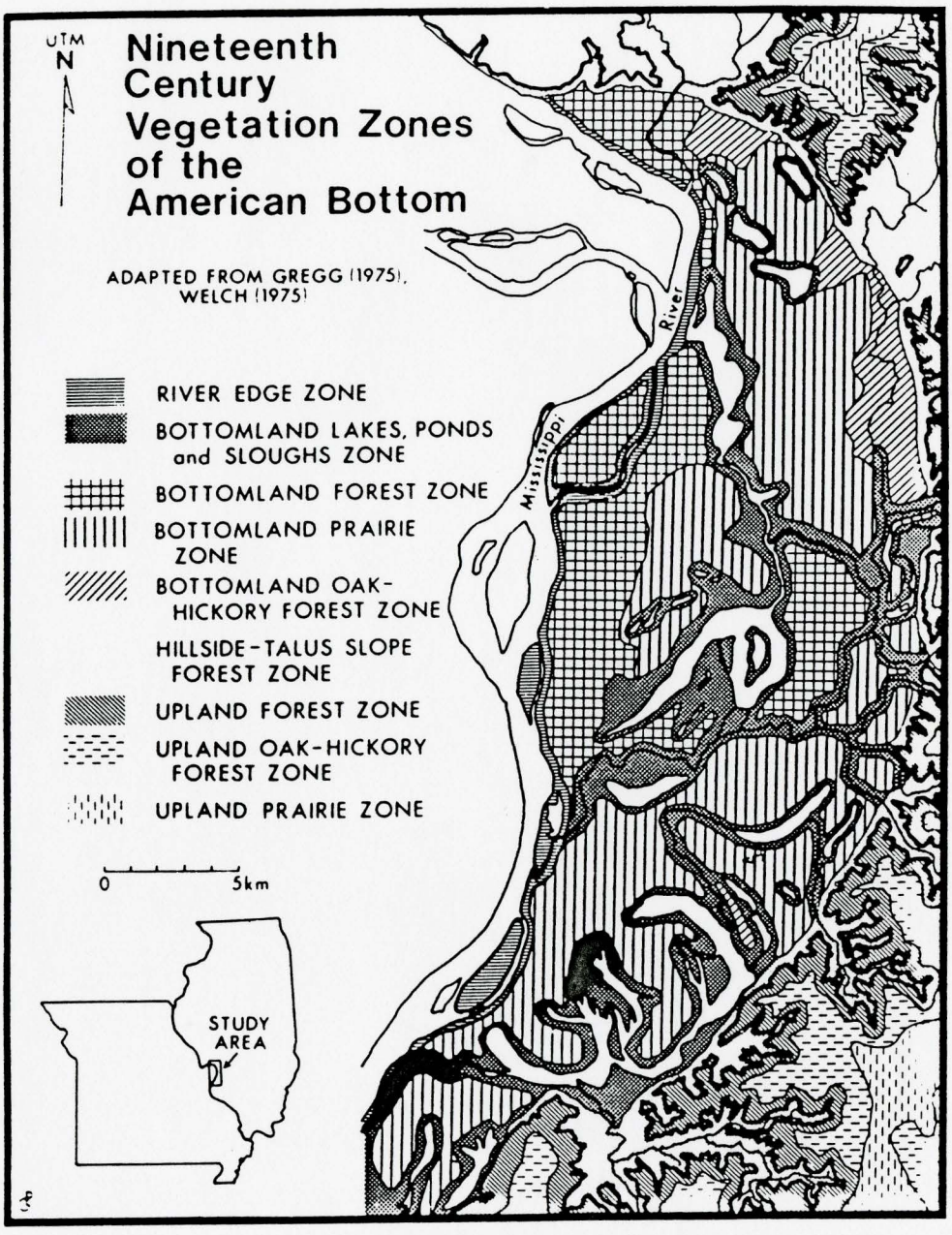


Figure 4.4 The biotic communities of the American Bottom. (Reproduced from Kelly 1990:115)

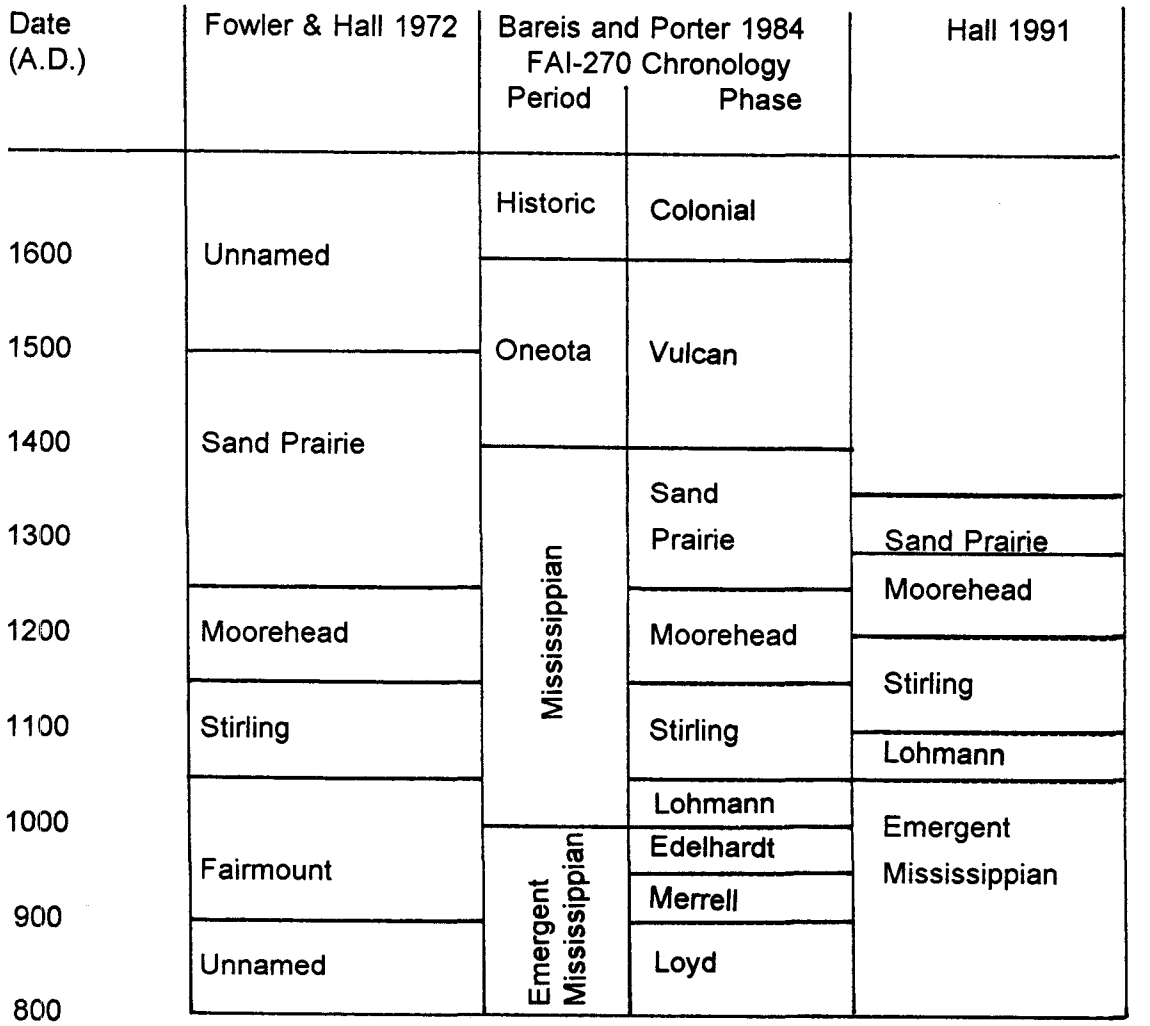


Figure 4.5 – Chronologies of the American Bottom
(derived from Fowler 1989:14, Bareis and Porter 1984:12, and Hall 1991:10)

Note: The basis for the current division of the Emergent Mississippian and Mississippian periods into phases was created at the 1971 Cahokia Ceramic Conference (see Fowler and Hall (1972) chronology above), and revised as new data became available in the FAI-270 investigations (see Bareis and Porter (1984) chronology above). Revisions are ongoing, however; for example, Holley (1989) and Pauketat (1991) have both suggested adjustments and subdivisions in the existing phases. Recently, there has also been concern about the exact timing of the FAI-270 phases; based on analysis of the phases' characteristics and radiocarbon calibration curves, Hall (1991:9) showed that the timescale is "more consistent with calibrated dates if the time assignments are moved forward variously by fifty to a hundred years or more" (see Hall (1991) chronology above). This is significant; however, this thesis will use the standard FAI-270 American Bottom chronology, in order to maximize consistency with the literature. This should not significantly compromise any findings herein, because (a) inter-regional comparisons do not figure prominently in my analysis, and (b) Hall's revised chronology places the closing of the Sand Prairie phase at 1350 A.D., which is even more comfortably earlier than the first European contact in this area.

The Woodland period (600 B.C. - 800 A.D.) in the area was the time of the "Hopewell Interaction Sphere" which was centred in the lower Illinois River Valley and the central Ohio River valley. Nucleated settlement patterns appeared, as did complex mortuary rituals, ceremonial centres with substantial earthworks and mounds, and widespread trade networks (Tainter 1988:15). The Hopewell complex declined by around 400 A.D. (Tainter 1988:15), but complexity emerged again during the later part of the Late Woodland, with an increase in population, some indications of formal community planning, and the introduction of the bow and arrow (Woods 1986). The Emergent Mississippian period (800 - 1000 A.D.) is essentially the transition between the Woodland and Mississippian periods. Permanent agrarian communities appeared, and maize achieved some prominence in the diet, although it did not replace earlier subsistence resources (Hudson 1976:94). It was at this stage that Cahokia emerged as a regional centre, and some of the classic Mississippian characteristics started to appear.

Cahokia was a part of what has been termed the Mississippian culture system, which lasted approximately from 800 to 1650 A.D., and covered most of the deciduous woodlands of the southeastern United States. This highly sophisticated system shared certain characteristics with the great Mesoamerican civilizations, but there is no distinct evidence for significant contact between the two (Mink *et al.* 1992). Some of the chief characteristics of Mississippian cultures include "a ranked form of social organization," and "a specific complex adaptation to linear, environmentally circumscribed floodplain habitat zones," including a reliance on maize cultivation (Smith 1978:486). Substantial complexity and diversity is evident at Mississippian sites both in terms of material culture

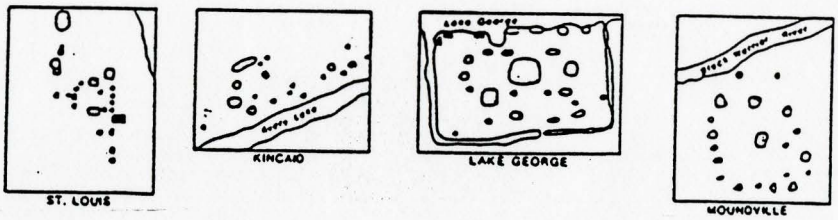
and social organization, as displayed through technology, status differentiation, settlement patterns, and burial rituals (Goldstein 1980:13). Our knowledge about Mississippian cultures comes not only from archaeological evidence, but also from ethnohistoric information. Records have been left by observers, including French explorers and the de Soto entrada, of the Natchez and other Mississippian groups that survived into historic times (Williams 1990; Mink *et al.* 1992). This ethnohistoric information has been used to flesh out the list of specific Mississippian characteristics which follows.

The Mississippians lived in settlements of various sizes, including major town centres, subsidiary villages, and isolated farmsteads. These sites were usually located near or on major floodplains, with access to a variety of natural resources, such as fish and fowl from oxbow lakes, as well as fertile soil. Their homes were single-family houses of pole and thatch or wattle-and-daub construction. Earthen mounds, sometimes of a size which clearly required large-scale coordination of labour, were constructed both to hold burials and to provide a base for structures. Many settlements were palisaded, and also show evidence of planning in the form of central plazas, and houses in regular rows. Most major Mississippian centres are characterized as chiefdoms, since there was significant social stratification, as well as populations in the thousands, and widespread trade (*e.g.*, shells from the Gulf of Mexico, copper from the upper Great Lakes). Craftsmanship reached a high level of artistry. Ritual and religion were important, judging from various burial rites, and evidence of seasonal observances; apparently, certain games achieved a very high profile, and an almost ritual status. Warfare was frequent, as attested to archaeologically by the palisades, violent scenes depicted on shells and ceramics, and by

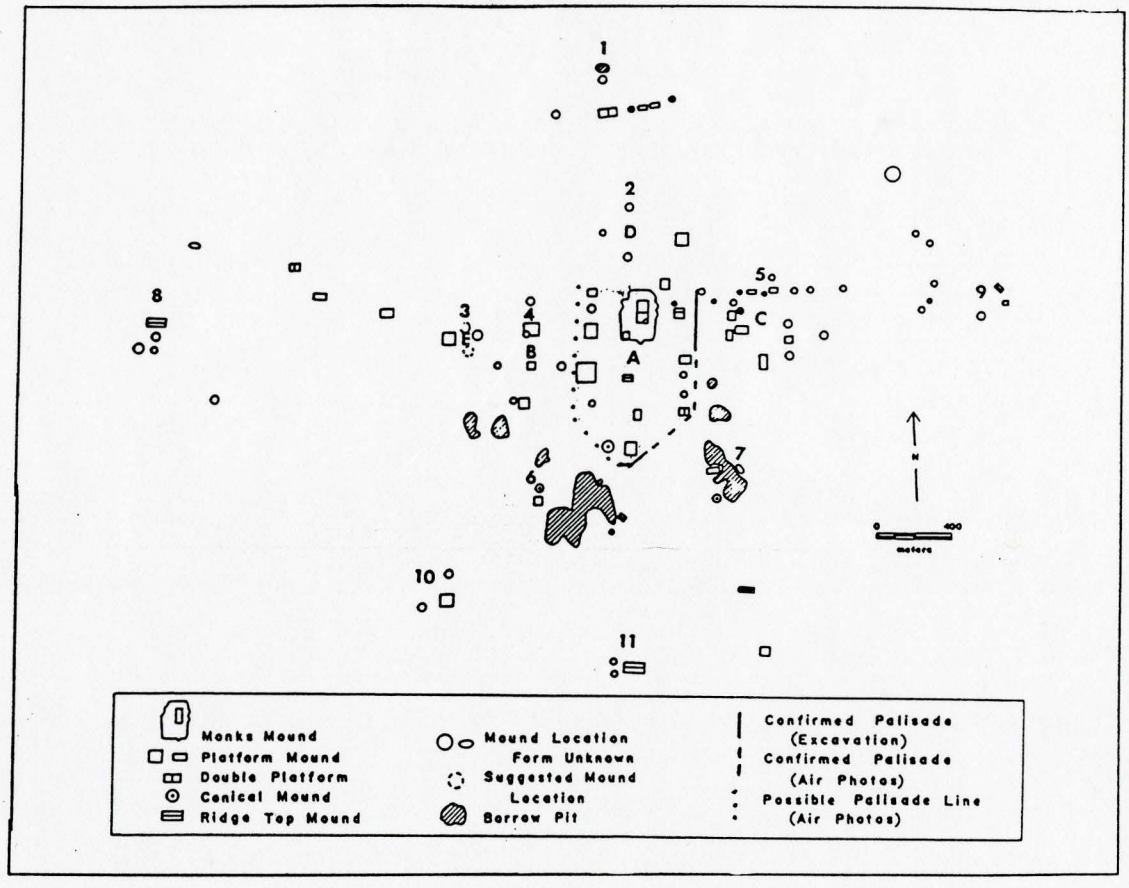
traumatic injuries evident in skeletal remains (Mink *et al.* 1992; Griffin 1990:7-12; Peebles and Kus 1977:435-441; Goldstein 1980).

Cahokia was clearly part of a regional interaction sphere including other Mississippian sites like Etowah, Moundville, Spiro, outliers like Aztalan, and closer neighbours in the Lower Illinois River Valley, just to the north (see Figure 1.1). Nonetheless, it is clear that the Mississippian system was not monolithic or homogeneous; although the site of Cahokia, considered together with its hinterlands, constituted the largest, and possibly the most organizationally complex, pre-Columbian settlement system north of Mexico, it cannot be considered to be somehow representative of the Mississippian period, in and of itself. Just as the site of Cahokia cannot be considered in isolation from other Mississippian centres, it cannot be considered in isolation from other sites in the American Bottom, since "in a sense the entire American Bottom is one large Mississippian site" (Fowler 1973:5).

We know that Cahokia was the central focus of a local polity of some kind. The exact nature of the political/economic structure is debatable, but regardless, Cahokia and its contemporary outliers in the American Bottom can be divided into several scales of settlement sites, including major town and mound centres, smaller one-mound villages, and isolated hamlets and farmsteads. The exact nature of the site hierarchy is the subject of considerable discussion, as discussed in Chapter 5. It seems that the only thing that is not disputed – and indeed, is indisputable – is that Cahokia is the biggest site in the American Bottom region, and bigger than any other Mississippian centre. (Figure 4.6 illustrates the size of Cahokia relative to other major mound centres.)



Other major Mississippian centres (same scale)



A schematic map of the Cahokia site showing the interpretation of the community organization between 1050 and 1250 A.D. (Stirling and Moorehead Phases). The arabic numerals represent mound clusters or groupings and the letters label suggested plaza areas as follows:

Mound Groupings

1. Kunneman Group
2. North Group
3. Mound 44 Group
4. Merrell Group
5. Ramey Group
6. Twin Mound Group
7. Listerman Field Group
8. Powell Mound Group (West Group)
9. East Group
10. Roach Group
11. Rattlesnake Group

Plaza

A. Main Plaza
B. Merrell Plaza
C. Ramey Plaza
D. North Plaza
E. Mound 44 Plaza

Figure 4.6 Plan of Cahokia site. Reproduced from Fowler 1974:24, adaptations from Fowler 1989:27 and Holley et al. 1989:342.

Cahokia is about 13 km^2 in area (or 1400 hectares, 5 square miles, or 3300 acres, if you prefer), and includes over 100 earthen mounds, and at least 19 large borrow pits, which are often ponds filled with wildlife (Fowler 1989:10,185). Figure 4.6 shows a plan of the site, indicating possible mound groupings. Among Cahokia's claims to fame is Monks Mound, named after a short-lived Trappist occupation nearby in the nineteenth century; a 4-tiered platform mound with a height of 100 feet and with a base covering over 17 acres, it is both the largest pre-Columbian structure north of Mexico and the third largest pre-Columbian structure of any kind in the Western hemisphere (see Figure 1.2) (Fowler 1989:7). It truly is, as Brackenridge called it, "a stupendous pile of earth."

Other than the mounds, the main features of the Cahokia site include a substantial residential occupation, which fluctuated in size over time, a large palisade, a main plaza, and borrow pits which, as mentioned, were likely ponds much of the time. The chronological sequence of development of Cahokia is summarized in sketch form in Figure 4.7, and the details are discussed in the following pages.

Estimates of Cahokia's population vary substantially, as will be discussed in Chapter 5, but it is certain that there were thousands of people in Cahokia and the American Bottom for most of the Mississippian period. Social stratification within the populace is certain, although the nature and extent of status differentiation is also a subject of much dispute. The infamous Mound 72, dated to around 950 A.D. – quite early in Cahokia's development, centuries before it reached its peak population – provides clear evidence of marked stratification. The excavations of this outwardly unprepossessing mound, carried out in 1967-71 under the direction of Fowler, yielded something of a

Period	Date A.D.	PHASE NAME	ACTIVITY IN CAHOKIA AREA
Oneota		VULCAN	Oneota villages in area
	1400		Site of Cahokia abandoned
			Population relocated to uplands
		SAND PRAIRIE	
	1300		Climate cools
			Marked decline of activity
Mississippian	1200	MOOREHEAD	Last palisade built Population decreasing Construction of Monks Mound ends
	1100	STIRLING	First palisade built Peak population at site
		LOHMANN	Woodhenges built
	1000		Reorganization of site plan
		EDELHARDT	
			Mound 72 burials
		MERRELL	Construction of Monks Mound begins
Emergent Mississippian	900		First mounds built
		LOYD	Occupation expands Agriculture intensifies, population grows
	800		
Late Woodland		PATRICK	First settlements at Cahokia site

Figure 4.7 Chronological Development at the Site of Cahokia
(derived from Bareis and Porter 1984:12; Mink, Corley and Iseminger 1992:14; Collins and Chalfant 1993; Fowler 1989)

surprise; among its nearly 300 burials are 53 young women buried in a single pit, as well as four young men without heads and hands, evidently sacrificed to accompany an elite individual into the next world (Fowler 1989:148,191). Other manifestations of considerable cultural elaboration include evidence of widespread trade networks, and indications of at least part-time craft specialization (Milner 1990). Knowledge of solar cycles is evident from Woodhenges, circles of very large posts which were likely used to observe the solstices, which were also built fairly early in Cahokia's history.

The palisade's existence was first indicated by soil irregularities in aerial photographs taken in 1922; later excavation indicated that a substantial palisade was indeed present, probably enclosing Cahokia's central precinct – Monks Mound and 17 other mounds (see Figure 4.6). Although it has not been completely excavated, it has been established that the palisade was first built in the later part of the Stirling phase, around A.D. 1100, and was rebuilt several times in the next hundred years, with each episode probably requiring at least 20 000 large logs (Fowler 1989:195-198; Pauketat 1991:95). It was undoubtedly designed to fulfil a defensive function, judging by the presence of bastions and screened entrances, and the possible existence of catwalks on the inside of the wall (Iseminger in Pauketat 1991:95). However, it is generally agreed that there is no substantial evidence of invasion at Cahokia. Like the palisades of Iroquoian villages, it is likely that the Cahokian central palisade served a secondary, symbolic function, whether or not this was intentional on the part of the builders (Ramsden 1990).

The kind of conversion of the use of space illustrated by the "rezoning" of houses for the building of the palisade during the Stirling and Moorehead phases is not

uncommon at Cahokia. It seems clear that the use of areas varied over time, with some areas changing from residential to public in nature, and back again (Pauketat and Barker 1992). In contrast to rural sites in the American Bottom, there is also abundant evidence for houses being rebuilt on the same site over and over (Pauketat 1991). (An example of this from the Mitchell site, another major American Bottom Mississippian mound centre, is shown in Figure 4.8.) Further, the alignments of mounds at the site suggest some degree of formal site planning, and the configuration of the mounds outside Cahokia's palisaded central precinct suggests that there were residential subcommunities of some description within the site (Fowler 1989).

Other markers of time in Cahokian archaeology, beyond public constructions of monumental proportion, include shifts in aspects of housing. Storage practices change from external to internal, while the actual structures' size, shape, and orientation were also modified. Building practices also change; throughout the Mississippian at Cahokia, houses were pole and thatch constructions, but later houses used wall-trenches to secure the vertical poles, while earlier houses had individual post-holes for each (see Figure 4.8).

At around 1200 A.D., Cahokia began to exist in a less complex form than before, and around 1300 A.D., the settlement's day was done. As discussed in Chapter 5, the cause is something of a mystery; it seems that although the site of Cahokia was largely abandoned, the population did not disappear, but dispersed to the surrounding highlands and adjacent river valleys (Hall 1991). Moreover, the Cahokian collapse was localized. At the time of its completion, around 1300 A.D., the other great Mississippian centres of Spiro, Etowah, and Moundville were just reaching their height, and Etowah remained at

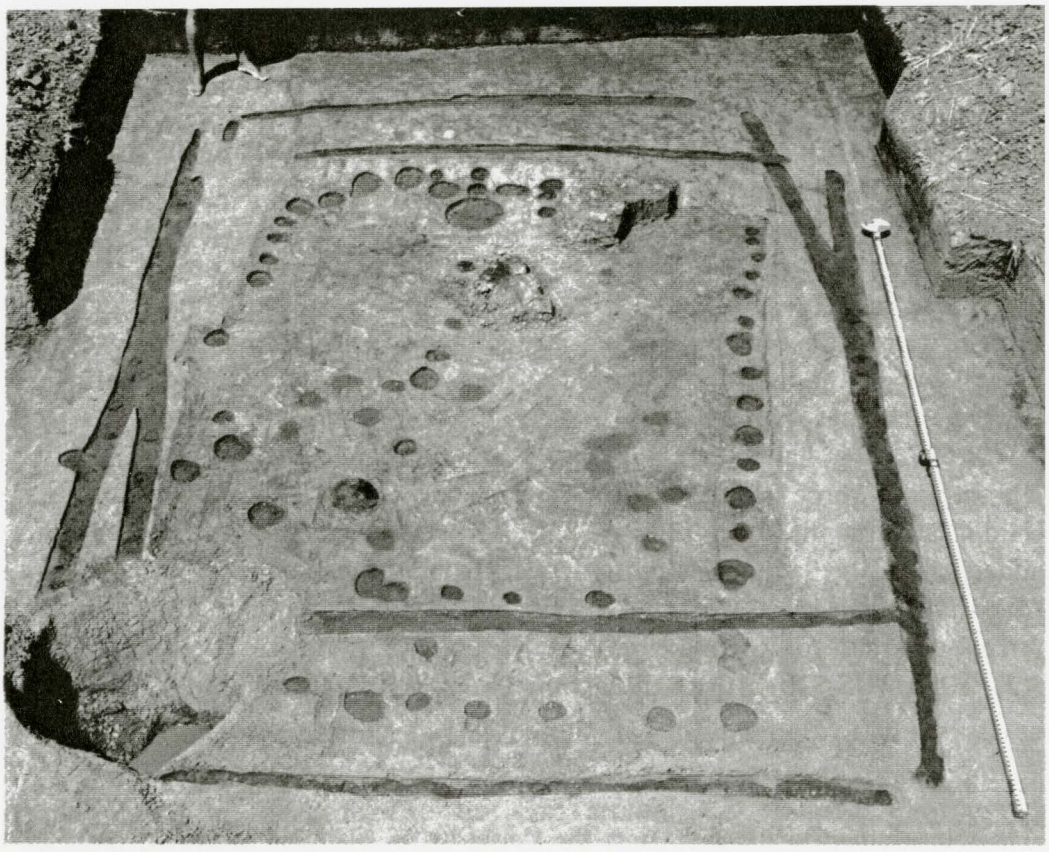


Figure 4.8 Superimposition of house structures at the Mitchell site. The pole-type structure is Emergent Mississippian, while the two wall-trench structures are typical of the Mississippian period in the American Bottom. Reproduced from Porter 1973:149.

the height of its activity and population until the late 1400s (Peebles and Kus 1977:434). The only contemporary occupations in the Cahokia area were modest settlements of a people who had recently immigrated into the area, the Oneota (Bareis and Porter 1984).

Mississippian culture elsewhere survived up to the time of first European contact, and beyond – for example, as mentioned, the de Soto expedition and later French explorers had substantial contact with the Natchez people living in the lower Mississippi Valley, who were clearly part of a continuing Mississippian tradition (Fiedel 1987:250). This has been useful in terms of ethnohistoric information which seems at least partly applicable to the Cahokian situation.

The history of the Cahokia site, of course, does not end there, but since this point is nigh to the beginning of the history of Cahokian archaeology, the next phase, of colonial interest and occupation, will be discussed in the next section.

In sum, Cahokia is agreed by almost all archaeologists to have been a complex society of substantial size, with the central site having a degree of influence on surrounding settlements, and a history with distinct phases of existence. However, if the goal is to gain a reasonably refined picture of Cahokian society, and to understand the health of its inhabitants, there are many further questions to be resolved. And, as seems inevitable in archaeological endeavours, there are many points of contention regarding Cahokia, and the dust has yet to settle. An understanding of the areas of dispute – to be discussed in detail in Chapter 5 – will certainly be facilitated by an understanding of the history of Cahokian research.

A brief history of Cahokian archaeology

Henry Brackenridge was the first to write about Cahokia to the outside world, after visiting the site in 1811. Other Europeans had known about the site for some time, however; the French had constructed a chapel on top of Monks Mound in the early 1700s, Decades after they left, a group of Trappist monks established themselves at the site, gardening on top of the great mound that later became their namesake (Fowler 1989:6).

Not surprisingly, given their spectacular nature, Cahokia and other sites like it generated incredible interest from European-Americans. In the nineteenth century, there were the Moundbuilder Myths, in which the great constructions of the Mississippi and Ohio River Valleys were imaginatively attributed to almost everyone other than their real builders. The mounds were seen as links with the history of the Old World, and so they were attributed variously to lost Tribes of Israel, Phoenicians, ancient Greeks, Hindus, Picts, Vikings, Romans, Tartars, and Welshmen with a bad case of wanderlust – "anyone, in short, who had ever built a mound in the Old World" (Silverberg 1968:6).[1] This was to be expected, not just because of the low opinion many colonists held of aboriginal people's intelligence and character, or the colonists' longing for ties with 'home,' but also because of the political climate of the 1800s in the frontier areas. As Fiedel put it, "[w]hite Americans, so acutely aware of their recent arrival from overseas, derived a peculiar psychological satisfaction from imagining the ancient landscape populated with

[1] Although early European explorers had substantial contact with moundbuilding peoples, descriptions of these peoples and their mounds in accounts by members of the de Soto expedition "failed to influence the course of future theories; within 250 years, some highly learned Americans [including Benjamin Franklin and Noah Webster] were suggesting quite seriously that the mounds of the Southeast had been built by de Soto's own men!" (Silverberg 1968:7)

heroic white men" (1987:3). Even more to the point, the displacement and annihilation of aboriginal groups then in the area was more easily justified if "the Indians themselves had violently wrested the land from its original inhabitants, the more civilized, and presumably white-skinned, moundbuilders" (Fiedel 1987:3).

The identity of the builders of the great mounds of the Ohio and Mississippi valleys was not established to the satisfaction of the authorities until Cyrus Thomas' exhaustive report to the Bureau of Ethnology in 1894 (Fiedel 1987:4), in spite of an already decades-old history of investigation and excavation at the Cahokia site, amongst others, and the long-held belief of many researchers that the builders were obviously Native Americans. Initially, this may seem remarkable, but as McGuire (1992:233) observed, the date of this report is noteworthy, for the slaughter at Wounded Knee, which ended the Indian Wars in the West, took place in 1890. The aboriginal origin for the mounds could not be accepted officially until there was no more need "to justify the taking of Indian land and the breaking of tribal military power"; by the time of Thomas' report, the Moundbuilder Myths had served their purpose and could finally be discarded by the government (McGuire 1992:233).[2]

The early investigations at Cahokia in particular included a series of informal expeditions, following Brackenridge's publications about the site, which produced sketches of Cahokia and collections of material (now mostly long-lost). Around 1860, the

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[2] Some people still believe in a form of Moundbuilder Myth. As Silverberg (1968:89-93) discussed in detail, the Book of Mormon, although regarded by adherents of the Church of Jesus Christ of Latter-day Saints as being divinely inspired, has been called an "inflated plagiarism" of an early 19th-century Moundbuilder romance novel, *Manuscript Found* by Solomon Spaulding.

investigations of Cahokia became more serious, and by the late 1870s, the first detailed site map had been drawn by J. Patrick (Fowler 1989:20). A series of individual investigators, including McAdams and Snyder, excavated and published on the site in the late 1800s and early 1900s; the dawn of the twentieth century coincided roughly with the 'take-over' of research at the Cahokia site by professional archaeologists affiliated with major museums, such as David Bushnell Jr. (Fowler 1989:22-24).

Next, according to Fowler, came the first of two periods of intensive archaeological work at Cahokia. In the 1920s, Warren King Moorehead, of the Illinois State Museum and later of the University of Illinois, excavated extensively at the site, and published descriptions of most (though unfortunately not all) of those digs.[3] Moorehead also lobbied for protection for the site, combating a new variation on the theme of the Moundbuilder Myths – he had to prove that the mounds were indeed man-made. Those influenced by the racist ideology of the day had failed in their attempts to prove that mounds throughout the Mississippi region were built by white men in ancient times; the next best thing was to suggest that they had not been built by people at all, but were geological curiosities (Fowler 1989:101-2). Both theories served the purpose of deflecting credit away from those to whom it belonged. Eventually, in 1925, after decades of

[3] Recent excavators have found evidence of Moorehead excavations for which no records exist. It was Moorehead's habit to leave tobacco tins with notes inside at the bottom of his test trenches; several of these tins have been found in places where he was not previously known to have excavated (Fowler 1989:26).

pressure, the State of Illinois created the Cahokia Mounds State Park, protecting the central mound group, approximately 144 acres of the site (Mink *et al.* 1992:68).[4]

Excavation has continued at Cahokia and other Mississippian sites, and abundant publications and scores of doctoral theses have been produced. A survey of the more recent historical developments in Cahokian archaeology can, as well as explaining what has been done and why, explain why current conflicts over interpretation exist. This history can be divided up into 'phases' of investigation or 'generations' of investigators, which may help to shed light on the nature of certain controversies, to be discussed in the next section.

If Patrick, McAdams, and Snyder can be characterized as 'first-generation' Cahokia researchers, then Bushnell and Moorehead are the beginning of the second generation. This second wave of activity, as most research since at Cahokia, continued to be driven by adversity; by the turn of the century, some of the most important sites of the American Bottom had already been obliterated by the growing cities of St. Louis and East St. Louis (Pauketat 1991:79), and the destruction at Cahokia also continued. Despite state protection for part of the site, much remained vulnerable to development. The Powell Mound, one of the largest mounds on the west side of the site, was unceremoniously levelled in 1930-31, with archaeological observation that was remedial at best (Fowler 1989:28). Cahokia's Murdock Mound, too, was levelled; unfortunately, Harriet Smith's

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[4] The Cahokia Mounds State Historic Site is now 2200 acres in size; it was designated a World Heritage Site by UNESCO in 1982, and a new interpretive centre was opened in 1989 (Mink *et al.* 1992).

rescue excavations at Murdock ended with the bombing of Pearl Harbour in 1941, and no more projects were undertaken at Cahokia until the mid-1950s.

Fowler (1989:207) comments that the 1950s problem-oriented research of James B. Griffin and Albert Spaulding at several Cahokia mounds, as well as at the Pulcher site, constituted a major turning point in archaeological research at Cahokia. It is perhaps only fair, then, to call this the beginning of the third generation of Cahokian researchers. More salvage work at and near Cahokia was also undertaken by Preston Holder, who removed the Wilson Mound elite burials from the present-day location of the Indian Mounds Motel, and by Joseph Caldwell, who performed emergency test excavations at Cahokia, in order to make way for a new discount store (Fowler 1989:30-32).

The impetus provided by impending destruction of unprotected areas of the site continued to impel archaeological research at Cahokia. The second major period of activity at Cahokia came in the 1960s (Fowler 1989); since the proposed Interstate 55/70 passed along Cahokia's edge, just north of Monks Mound (see Figure 4.2), and federal legislation had just been passed permitting a small portion of highway construction budgets to be used for archaeological salvage work, archaeologists in the area suddenly found themselves very busy (Griffin 1984a:xv). Ultimately, the Interstate 55/70 projects produced more information "from a larger area with more competent personnel than all of the previous excavations" (Griffin 1984a:xv). These competent personnel included those who might be called the fourth generation of Cahokia researchers: Warren Wittry and Robert Hall excavated the famous Woodhenges; Charles Bareis and Patricia O'Brien worked on the Dunham Tract and the Powell Tract; James Porter, Bareis, and Elizabeth

Benchley all excavated at Monks Mound; Melvin Fowler supervised an extensive project to prepare a detailed map of Cahokia, and supervised the excavation of Mound 72 (Fowler 1989:32-42). A vast amount of information about the site was produced during this period by these archaeologists and numerous others, many of whom have continued to be active in Cahokian research.

Now, the considerable gaps in the knowledge of Cahokia and the American Bottom began to close enough to permit the formulation of theories regarding population size, ceramic chronology, and social structure. However, the data were still insufficient to offer much support for these theories. Griffin has termed these early theory-building endeavours "entertaining," and wryly noted that "[a]n archaeological axiom would seem to be that a minuscule amount of sound excavation data allows one to propose an entrancing model of Cahokian development" (1984a:xvii). A particular problem was that at this stage, knowledge of Emergent Mississippian settlement patterns at Cahokia was particularly scant, "partly because the ancient Cahokians did not realize they should construct a nucleated settlement entirely within the FAI-55/70 right-of-way" (Griffin 1984a:xviii). Herein lies the problem of salvage archaeology – digging takes place where and how it must, not necessarily where and how it would provide the most useful information. There is a counterbalancing benefit, however, for when a site is soon to be obliterated by development, there is nothing to be lost by completely excavating it. The site plans and other large scale information yielded by such an approach can be very valuable, as demonstrated by both the FAI-55/70 efforts and the subsequent FAI-270 project (Griffin 1984b:253).

With the initiation of the Federal Aid Interstate-270 Archaeological Mitigation Project[5] in 1977 came the next major era of investigation in the American Bottom. The FAI-270 right-of-way ran down the east side of the American Bottom, narrowly missing the site of Cahokia, but impacting upon many other smaller sites. Under the direction of Charles Bareis and James Porter, almost 100 sites from the right-of-way and the surrounding uplands were salvaged by archaeological teams between 1977 and 1983, and dozens of FAI-270 site reports have since been published (Bareis and Porter 1984:1-14).

According to Fowler, the ultimate contribution of the FAI-270 investigations to Cahokian archaeology has been "in demonstrating the complexity of the smaller communities that supported Cahokia; it has also refined our understanding of the evolution of Cahokia, and contributed to fundamental and refined changes in the chronology of the area" (1989:43). Another important contribution of the FAI-270 investigations has been the production of the fifth and most recent generation of Cahokia/American Bottom scholars, which may be said to include Thomas Emerson, George Milner, John Kelly, William Woods, Neal Lopinot, Timothy Pauketat, George Holley, Michael Hargrave, Mark Mehrer, and James Collins, amongst many others who have begun to publish on the subject since about 1980.

Research at the site of Cahokia with excavations at the location of the new Interpretive Centre in 1984 and 1985 (Fowler 1989:43), as well as limited test excavations on Monks Mound in 1985-86 (Collins and Chalfant 1993), and surface collection in 1985

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[5] Bareis and Porter (1984:xiii) noted that the FAI-270 highway was later redesignated as the FAI-255. (See Figure 4.2 for the road's location.) In keeping with the archaeological literature, however, the project will herein be referred to as the FAI-270 Project.

in the Kunneman Tract (Woods and Holley 1989). The most recent research at Cahokia has included electromagnetic surveys providing evidence of landscape modification in the area of the southern palisade wall, and the Grand Plaza (Dalan 1991, 1993), and field school excavations of the palisade under William Iseminger (Pauketat 1991).

I have divided Cahokian research up into 'generations' because it can help to make clear the lines of dissension; popular opinions and focuses within Cahokian archaeology have followed certain discernable trends. Of particular interest here are the differences between the fourth and fifth generations. The fourth generation contributed modern chronologies and a great deal of new data through the FAI-55/70 salvage excavations, as well as the first comprehensive theories about the nature of Cahokia's social system. In their writings, the fourth generation's particular focus was on the complexity and size of Cahokia. The fifth generation added even more information, from the FAI-270 excavations, to the database, but has concerned itself as much or more with the hinterlands than Cahokia in particular, and has established a trend towards research on fine-grained analyses of households, and diachronic changes in household structures and settlement patterns, as well as a contrasting post-processual interest in ideology.

The actual differences in interpretation regarding Cahokia are many. There are the usual archaeological squabbles about fine-tuning chronologies, and classifications of pots; however, there have also been more significant disputes over Cahokia's status as a 'town' or 'city,' and the designation of the Cahokian cultural system as a 'chiefdom' or a 'state.' It is the latter debates that are of primary concern here, for through them all runs a common question: just how complex was Cahokia?

The view most characteristic of the fourth-generation scholars is that Cahokia was a full-fledged city, and the centre of a polity that was highly organized and scored well on the 'cultural evolution scale.' Given a range of possibilities, they frequently chose the complex extreme (O'Brien 1972, 1989, 1991; Fowler 1974, 1975, 1978; Hall 1993). In contrast, fifth-generation Cahokian scholars have argued for a smaller population organized in a less rigid and formal hierarchy, and have consistently leaned towards a vision of Cahokian society that is a little less grandiose (Milner 1990, 1991b; Pauketat 1991, 1992; Emerson 1991, 1993). The origins of these views, and their implications, are important to bear in mind; as Patricia O'Brien pointed out, "[s]uch diversity of opinion about a single archaeological site and or about a single cultural tradition, is reflective not simply of disagreements on data interpretation, but of deeper theoretical issues and perspectives" (1989:276).

Cahokia has a special place in North American archaeology; partly, of course, this is due to the site's sheer enormity. But there are other reasons for its importance:

> [s]ize is but a reflection of the complexity of the social order that must have built and governed the community. It is this social complexity that intrigues archaeologists and motivates much of the research directed at understanding what Cahokia was and the processes which caused it to evolve... a major transformation in the long course of human social evolution was the transition from village-oriented folk cultures to city-centered political states... Detailed research on the actual nature of Cahokia's social, political, and economic organization and the development of ideas... should offer a case history for comparison with other studies. (Fowler 1989:12)

Fowler later added that "Cahokia is anthropologically significant because it presents a rare opportunity to understand how human societies developed complex civilizations in a part

of the world where such complex societies were not thought to have developed" (1989:207). Correspondingly, O'Brien noted that Cahokia "has a special role in relation to theories concerning the cultural development of native North Americans" (1991:143).

Clearly, interest in Cahokia has come about partly through a concern with cultural evolution; indeed, the rise to prominence of theories about Cahokia's complexity in the 1970s coincided somewhat with a renewed archaeological interest in cultural evolution (Sanderson 1990). This influence had just begun to be widely felt when the first formal theories of Cahokian complexity appeared on the scene. The idea of Cahokia being part of a complex and populous state may also relate to other movements in archaeology. A roughly contemporary development was the reassessment of the Mayan civilization, after excavations at Tikal in the 1960s showed that the site had a substantial residential population, and that the society was more complex than was previously believed (Tainter 1988:155-6).

The emphasis on Cahokian complexity also relates to other developments in anthropology, for example to the high-end estimates of population size in the pre-contact Americas from authors including Cook, Borah, and Dobyns, which began to gain favour in the late 1960s and early 1970s (Henige 1990). Apart from the simple idea of a large population, these two schools of thought share something important. Henige (1990:187) writes off high estimates of pre-contact native population as a "fatal combination of the lack of evidence and the apparent need for expiation"; he contends that the estimates of the "High Counters" allow them to make moral judgments, for by "inflating the number of victims that resulted [from European contact].... the specter of genocide appears." The

idea of Cahokia being a 'civilized state' can provide the same kind of expiation that Henige wrote about – in this case, it does not provide grounds for a moral judgement against European invaders who, one way or another, 'removed' much of the native population, but grounds for a judgement against those who sought to obliterate the natives' memory as well as their existence by denying their agency in the building of the great mounds of the Mississippi River Valley. This extension has not gone unnoticed by Cahokia researchers. O'Brien, perhaps the most insistent advocate of Cahokia's cityhood and statehood, notes the poetic justice of her theory: "[b]y a strange twist, the myth of the 'civilized moundbuilders' is basically true, except that the mythical builders were not a lost race, they were the American Indians of the Eastern United States" (1972:197).

O'Brien later proclaimed that her argument for Cahokia's statehood proves "that we must reassess the cultural contributions and achievements of the late prehistoric Native Americans of the Eastern United States" (1989:286). Fowler wrote that "[t]here is nothing else like it, either in size or complexity, representing Native American achievements within the boundaries of the present United States" (1989:207). Clearly, to some archaeologists, Cahokia is far more than a fascinating and complex prehistoric site – it has come to represent the pinnacle of Native North American achievement, and the exact height of that pinnacle is important for all kinds of reasons, archaeological and not.

The discussion in Chapter 5 of the most relevant major areas of contention may now be more intelligible, with these generational trends, and wider implications, kept in mind. These disputes are also relevant to studies of Mississippian health, discussed below.

ANALYSES OF THE HEALTH OF MISSISSIPPIAN POPULATIONS

In any overview of American osteoarchaeology, as in any overview of American archaeology, it is impossible to underestimate the influence of the Moundbuilder Myths. One is brought back to the Myths time and time again as the driving force behind early studies of the original inhabitants of the continent. In particular, the first studies of Mississippian human remains were a part of attempts to establish the racial identity of the Moundbuilders, to resolve the controversy over who they were, where they came from, and determine their relationship or lack thereof to modern Native North Americans. Notable landmarks in these debates include Samuel Morton's *Crania Americana*,[6] published in 1839, and Aleš Hrdlička's *Skeletal Remains Suggesting or Attributed to Early Man in North America*, published in 1907 (Silverberg 1968:107-9, 160). Scholars of this period frequently asserted that modern Native North Americans' inferiority and savage nature was clearly visible in their skull shape, whereas the Moundbuilder peoples were slightly more intelligent and civilized (Silverberg 1968:159). This politically convenient opinion was popular until Hrdlička exhaustively debunked these claims for inferiority in his 1907 work (Silverberg 1968:229).

Skipping ahead through several generations of physical anthropological research, we find that the study of prehistoric Mississippian skeletal remains is still highly political, although in a somewhat different way. Now, part of the importance of the skeletons lies

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[6] See Gould (1981) for a detailed re-evaluation of Morton's measurements of cranial capacity.

in their ability to shape or confirm opinions about the changes which have occurred in human health throughout the stages of our history on earth. Since Mississippian peoples were early agricultural groups involved in a large-scale cultural system, they are particularly interesting to those concerned with past health (Armelagos and Hill 1990). Indications of improving health support optimistic theories about human capabilities; indications of worsening health support negative ideas, about humanity being out of balance with nature, and our tendency towards the development of power imbalances, wherein one group exploits another, causing suffering and oppression. This is the backdrop against which studies of Mississippian health occur.

My focus here is primarily on analyses of Illinois Mississippian populations, although of course there are skeletal remains from many other Mississippian areas which have also been studied, including the Ohio River Valley (Perzigian *et al.* 1984; Cassidy 1984), Alabama (Powell 1988, 1991), Georgia (Larsen and Ruff 1991), and the Central and Lower Mississippi River Valley – including the states of Missouri, Tennessee, Arkansas, Mississippi, and Louisiana (Rose *et al.* 1984; Rose, Marks and Tiezen 1991). Mississippian populations have been a target for analysis because sites are abundant, with material remains which were quite elaborate in comparison to many other North American prehistoric groups, and they tended to inhabit areas which were previously occupied by less intensive agriculturalists, and hunters and gatherers. This has made them especially useful subjects for studies of the effects of subsistence changes on health (see Cohen and Armelagos 1984). The emphasis has been largely on diachronic trends, both because of

questions about the transition to agriculture, and because of the unexplained disappearance of the Mississippians in the Illinois area long before European contact.

Modern investigations into Illinois Mississippian remains gained momentum with studies in the Lower Illinois River Valley, where there are also abundant remains from the Woodland period. Of all the Illinois Mississippian skeletal samples, the Dickson Mounds series have been the most prominent in the osteoarchaeological literature. The history of investigation at Dickson Mounds is a long one, as was the original period of use (the site includes burials from the Woodland period as well as the Mississippian). Don Dickson, a young chiropractor, began excavations of these mounds in the 1920s, and found himself caught up in the excitement of studying the pathologies of the skeletons interred within; eventually, his attention turned to attempts to understand the life and death of the population as a whole (Goodman and Armelagos 1985:12). Ultimately, there were two further major episodes of excavation, the first in the 1930s, and the second in the late 1960s under Alan Harn (Buikstra and Milner 1989:2).

The result of these three periods of excavation is "one of the largest and best-documented collections of human skeletal remains available for study," consisting of 1,050 individuals, or about one-third of the total number of people estimated to be buried in the cemetery (Buikstra and Milner 1989:1,5). The excellent preservation of the skeletons has made them a popular choice for study for graduate students and scholars from medical sciences and dentistry, as well as physical anthropology, with the result that publications about the Dickson Mounds series involve topics ranging from skeletal pathology and form to dental health and diet, and appear in a wide variety of journals

(Buikstra and Milner 1989:1). Of special interest here, however, are the many publications on the Dickson people (see Buikstra and Milner's (1989) annotated bibliography) which have been devoted to the evaluation and explanation of changes in health which accompanied the adoption of agriculture and associated lifestyle changes.

The interpretations made regarding health for Dickson Mounds have already been alluded to in examples given in Chapter 2; however, it seems appropriate to elaborate here. In general, it is concluded, on the basis of a number of indicators – including "measures of growth disruption, growth retardation, disease, and mortality" (Goodman *et al.* 1984a:272) – that the Dickson Mounds people suffered from steadily worsening health from A.D. 950 to A.D. 1300[7]. Goodman and colleagues wrote:

> The traditional interpretation of this [worsening] is that it is due to local, ecological changes such as increased population density and intensification of agriculture. We argue that the broad patterns of increasing stress evidenced at Dickson may be equally due to Dickson's increasing involvement in Mississippian-based exchange systems. (Goodman *et al.* 1984a:272).

This latter hypothesis, of cultural and economic change causing the problem, has been more and more strongly favoured over the former ecological explanation. In particular, Goodman *et al.* (1984a:300) proposed that the area inhabited by the Dickson Mounds people was more than productive enough to sustain the population at its height, and that the increased stress shown in the skeletons was due not to ecological types of changes resulting from agriculture, such as increased population density, sedentism, and

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[7] Some of the main analyses of the Dickson series, pertaining to population health, include: Blakely (1971), Lallo (1973), Rose (1977), Lallo *et al.* (1977), Rose *et al.* (1978), Lallo *et al.* (1978), Lallo and Rose (1979), Goodman *et al.* (1980), Blakey and Armelagos (1985).

changes in diet, but to the settlement's "exploitable position in a regional system of ideological and economic exchange." They conclude that:

> Just as it would be ill advised to explain patterns of disease in contemporary societies without reference to their involvement in the modern world system, so also would it be ill advised to explain health at this prehistoric site without reference to the precapitalist systems of which the Mississippian culture is an example. (Goodman *et al.* 1984a:300)

This argument is reiterated, and made yet more specific in Goodman and Armelagos (1985); therein, the polity of Cahokia is blamed for the Dickson people's poor state. The "disparity between what was available and what was eaten" is accounted for by their hypothesis that the Dickson people were trading their foodstuffs for Cahokian luxury items (Goodman and Armelagos 1985:18). It is further suggested that in general, communities located in political hinterlands, such as those which used the Dickson mounds as burial grounds, are likely to be at greater risk for decreasing health with the advent of agriculture than communities at the centre of political systems (Goodman and Armelagos 1985:18). Goodman *et al.* (1988:181) later repeated the contention that comparison to modern disadvantaged groups is important and valid, suggesting that "the situation at Dickson may parallel many of the most persistent problems we see today." And finally, in their 1992 paper, Goodman, Martin, and Armelagos explicitly situate themselves theoretically with references to the 'core and periphery interaction' concept, used to describe the origins and development of European capitalism, and to critiques of capitalism's effects on health in developing countries. Here, they clearly identify the origin of the Dickson community's health problem as not merely analogous to those of health problems in modern 'peripheral' societies, but as essentially the same.

While the efforts of Goodman and colleagues to relate theory about modern political influences on health to archaeological case studies are commendable in principle, such interpretations of the evidence are not problem-free. One problem is that they rest on uniformitarian assumptions about the nature of political systems, which may be unwarranted; the political relationship between the Cahokians and hinterland communities is unclear archaeologically, as will be discussed in Chapter 5. Nor is it certain precisely who was buried at Dickson Mounds. There are other cemeteries in the immediate area, and so it is possible that only members of a certain social class, rather than an entire community, were buried there (Buikstra and Milner 1989:6); by extension, it is also possible that the social status of the subset of the population buried there changed over time. In addition, as discussed in Chapter 2, Wood and coworkers (1992) recently postulated alternate explanations for the Dickson Mounds observations of increasing enamel hypoplasia and decreasing mean age at death over time; it is conceivable that these apparently negative trends are in fact indications of improving population health.[8]

Discussions of these problems have yet to catch up in prominence to the interpretations of health for Dickson Mounds given in the various articles by Goodman, Armelagos, and colleagues. Jackes (1993:437) commented that, despite many problems and probable sample bias, the Dickson studies have been "accepted as definitive of the transition to agriculture" in numerous works, such as Cohen (1989). Milner (1992:103)

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[8] Jackes has also commented on the Dickson pathology rates; given problems in precise age determination, an increased frequency of pathology in the later Mississippian sample could be interpreted to mean that there are more older adults present (Jackes, letter to author, June 19/94). Jackes (1993) has also pointed out several serious problems with the calculations used in palaeodemographic analyses of the Dickson samples.

similarly noted that the results of the Dickson studies "serve as a baseline guide against which other eastern North American skeletal series are evaluated, and they are a critical part of the commonly cited conclusion that prehistoric agriculturalists were generally not so healthy as their hunting and gathering predecessors." Wood *et al.* (1992:354) wrote that they singled out the Dickson analyses for re-examination specifically because of the influence the analyses have had in the reevaluation of the transition to agriculture's effects on health. However, it is important to be cognizant and wary of the conclusions drawn under this influence, because in addition to the problems with the Dickson series studies mentioned above, there is abundant evidence from studies of other Mississippian samples which indicates that there was great variation in health in late prehistoric communities of this region (Buikstra and Milner 1989:61).

For example, Rose and colleagues emphasized heterogeneity in health between culture areas during the transition to maize agriculture. They wrote of the Caddoan, Central Mississippi Valley, and Lower Mississippi Valley populations that "[j]ust as the reasons for the adoption of maize varied among the three areas, the health consequences of maize consumption differed" (Rose *et al.* 1984:417). Cook's (1984) analysis of skeletal and dental indicators in skeletal series from the Lower Illinois River Valley, indicated that intensification of food production in the Late Woodland period resulted in worsening health at that time, as compared to both earlier and later populations. Cook (1984:261) argued that after this transition period, the inclusion of maize into the existing collection and horticulture economy was ultimately beneficial; the one negative consequence was

increased population size, which resulted in the presence in Mississippian times of density-dependent infectious diseases which were previously absent.

Powell's exhaustive analysis of the skeletal remains from Moundville also indicated good Mississippian health. She concluded that while "[p]revious assessments of Mississippian health in diachronic perspective have stressed apparent declines from pre-Mississippian levels," this impression of "dismal and attenuated" Mississippian health "is not substantiated by the evidence observed at Moundville" (1988:197).

Given these comments on the variability of Mississippian health, it seems prudent to question the assertions, made both in displays at the new Cahokia Mounds Interpretive Centre, and in the accompanying book (Mink *et al.* 1992:46), that Mississippian health in general was very poor. Their comments on Cahokian health were these:

> Analyses of human skeletal remains show that, even at the height of Cahokia's agricultural productivity, its people were malnourished by the excessively high carbohydrate diet and seasonal scarcity of food. Such poor nutrition caused serious medical problems including iron deficiency anemia; arrested growth of the longbones; a high infant and juvenile mortality rate; and dental disease, twice as prevalent in the Mississippian as in the preceding Woodland period. Other ailments – arthritis, endemic syphilis, and tuberculosis caused by a fungus borne in soil[9] – all were common at Cahokia.... At best, the average life expectancy was only 35 to 40 years. (Mink *et al.* 1992:66)

Since no source is given, it is difficult to say where this grim picture of Cahokian health originated. However, it bears considerably more resemblance to the usual health

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[9] The disease referred to here is presumably blastomycosis, which was prevalent in some prehistoric North American populations. Blastomycosis is caused by a soil fungus (*Blastomyces* sp.), and results in bone lesions similar to those seen in skeletal tuberculosis, but is not actually a kind of tuberculosis, the latter being caused by *Mycobacterium tuberculosis* (see Kelley 1989).

profile of the Dickson Mounds population than it does to any research done on American Bottom groups.[10] The studies on Cahokian and American Bottom skeletal remains are certainly fewer than those on Dickson Mounds, but as described below, they clearly support a rosier vision of the population's health than that given by Mink and colleagues.

George Milner has been the primary researcher working on skeletal samples from the American Bottom region. Milner's 1982 and 1984b publications list the known Mississippian burial populations from Cahokia and the American Bottom; there are many, but only a few collections of substantial size are extant and well-preserved enough for osteological study.[11] Those examined in his dissertation research (Milner 1982) included samples from the nonelite, 'rural' American Bottom cemeteries of Kane Mounds, Signal Hill, De Frenne, and Krueger, and from Cahokia proper, the nonelite cemetery from Tract 15B. Milner (1982) also examined the remains from the elite cemetery of Wilson Mound.[12] Important later additions to the list of American Bottom skeletal series include those from the East St. Louis Stone Quarry cemetery (Milner 1983a), and the Florence

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[10] Notes from the development of the Cahokia interpretive centre, given to me by Bill Iseminger of the centre, include no references but seem quite clearly to be drawing primarily from Dickson Mounds studies, judging by the figures given and the occasional references to the site.

[11] Skeletal remains have been found in great abundance at the site of Cahokia itself, for example, but most have fallen victim either to the razing of mounds, or to excavation before the importance of keeping skeletal series together and curating them was appreciated (Milner 1982:257-264). Mass burials of over one hundred individuals were found in Rattlesnake Mound in 1927, and in Powell Mound when it was razed in the 1930s (Milner 1982:260). Many skeletons have also been uncovered from the vicinity of Monks Mound, and there have been isolated graves found throughout the site.

[12] See Milner (1982:262-301) for detailed descriptions of the cemeteries and skeletal populations in question.

Street site cemetery (Milner 1983b), excavated as part of the FAI-270 rescue operations. Both are nonelite farmstead cemeteries from the Sand Prairie phase; the former series consists of 120 skeletons (Milner 1983a:18), whereas the latter included the poorly preserved remains of at least 48 individuals (Milner 1983b:262). Other skeletal series from Cahokia proper include the collection from Fingerhut, a small cemetery containing approximately 46 poorly preserved individuals (Milner 1982:262), and the Mound 72 collection (Fowler 1989). Neither has been published on extensively as yet.

Milner's 1982 conclusions about prehistoric health in the American Bottom were based primarily on the Moorehead phase Kane Mounds collection, as it was by far the largest studied. The results of his osteological analysis indicated that adolescents and adults in this period were quite healthy, but that infants and young children suffered from higher mortality, as would be expected due to poor quality weaning foods and constant exposure to the area immediately around the home, which was contaminated with occupational refuse (Milner 1982:234). Young women also suffered from elevated mortality, which Milner (1982, 1984a) suggested was due to childbearing stress. In general, the pattern of health observed for the Kane people "is consistent with a nutritionally adequate diet and a low or moderate disease load" (Milner 1982:235).

Comparing his results to those for other Mississippian populations, Milner (1982:242-3) noted that "the level of health displayed by the American Bottom Mississippian Indians seems to be somewhat higher than that reported for many other later prehistoric populations" including Dickson Mounds. He attributed this discrepancy to regional variations in subsistence strategies, settlement longevity and organization, and

emphasized that "[e]xisting concepts of the late prehistoric period as being a time of deteriorating community health may be in part a result of the many historic accidents that determine which archaeological sites are the subject of concerted study" (Milner 1982:250). Unfortunately, as Milner noted, the lack of a diachronic perspective in his 1982 study compromised comparisons to other studies of Mississippian populations which traced changes in health patterns over time. However, this was partially rectified shortly thereafter with the studies on the Sand Prairie phase skeletal series from the East St. Louis Stone Quarry (ESLSQ) and the Florence Street sites.

Milner (1983a) presents a detailed comparison of his ESLSQ and Kane Mounds results. In general, health seems to have improved somewhat over time,[13] but the general pattern of good adolescent and adult health, and poorer infant and young juvenile health, is preserved. Concerning other Mississippian studies, Milner later concluded that

> it appears that levels of community health varied among roughly contemporaneous late prehistoric populations. Overall, it seems as if the people of the American Bottom were more or less as healthy as the Mississippians of Moundville, Alabama and several later prehistoric populations from the Arkansas region. They were not as highly stressed as the Dickson Mounds "Middle Mississippian" and Norris Farms #36 Oneota populations of the central Illinois River Valley, some central Mississippi

[13] The specific results were these: young adult females from ESLSQ were apparently less stressed than those from Kane; life expectancy at birth for ESLSQ was 27.7 years, compared to 24.1 years for Kane; life expectancy at age 20 for ESLSQ was 21.4 years, compared to 17.3 years for Kane; the percentage of the population dead before age 20 was 35.6% for ESLSQ, 43.2% for Kane, and 32.3% for the Florence Street sample (Milner 1983a:86-89, 125). Enamel hypoplasia and infection frequencies were comparable for all populations, but a much higher frequency of cribra orbitalia/porotic hyperostosis was seen in ESLSQ than in Kane (17.1% versus 6.1%). Milner (1983a:126) attributes this difference to increased survival in the later period, since it correlates with a lower mortality rate in early childhood for the ESLSQ group.

> River Valley late Mississippian groups, and the Mississippian Averbuch population from central Tennessee. (Milner 1991a:67)

Diachronic information about health for the American Bottom is still lacking, however, since there are no extant, comparable collections for the Emergent Mississippian period or Lohmann and Stirling phases. Similarly, there is little opportunity for comparison between elite and nonelite burial populations (Milner 1991a). However, the existing information about health in the Moorehead and Sand Prairie phases does at least seem to reduce the possibility that the decline of the Cahokian polity was due to an increased disease load (Milner 1991a:67).

The omission of the Mound 72 collection from consideration above may be puzzling; however, while it is large, and from the otherwise unrepresented Emergent Mississippian period, the unknown origin of the individuals interred there as sacrifices poses a problem for interpretations of population health. In addition, the preservation of the bones was extremely poor, so much so that age and sex determinations were all but impossible for many of the 272 individuals represented (Rose n.d.). However, some research has been carried out on these remains. Janice Cohen (1974), seeking to determine the biological relationship between the people at Cahokia and those buried at Dickson Mounds, compared the dental morphology of a sample of individuals from Mound 72 and from each period at Dickson Mounds. Cohen's results indicated genetic continuity between the Dickson Mounds groups, and disparity between the Cahokian and Dickson Mounds samples; she concluded that a model of migration from Cahokia to the Dickson Mounds area was not supported. The same analysis was used as the basis for the

suggestion made by Rose and Cohen in a 1973 conference paper, that the Cahokian sample was "not genetically homogeneous" (Buikstra and Milner 1989:25). According to Rose and Cohen, this heterogeneity in the Cahokian sample also apparently extended to diet and living conditions, judging by higher rates of infection and dental caries in the sacrificial females as compared to non-sacrificial females buried in the mound (Buikstra and Milner 1989:25).

This suggestion has implications for the other work, namely stable-isotope analysis of diet, which has been published on Mound 72. Because "the variability of dependence on maize through time and region represents an important factor in interpreting variability in nutrition and morbidity," Buikstra obtained carbon-isotope values for a number of bone samples from the midcontinental United States, from the Early Woodland period through to Mississippian times (Buikstra 1992:87).[14] Similarly, in their effort to establish the relative importance of maize to the Hopewell diet through carbon isotope testing, Bender and coworkers (1981) also tested samples from Mound 72 and from the Fingerhut cemetery. However, the results given in these reports conflict.

The Fingerhut samples used by Bender *et al.* (1981) were from two traditional Cahokia burials, circa A.D. 900, and the Mound 72 samples, from approximately the same period, were drawn from 4 individuals of apparently different rank, according to burial context. The analysis indicated that there was significant variation between individuals in

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[14] Higher (less negative) $\delta^{13}C$ values in bone collagen indicate higher corn consumption, since the ratio of ^{13}C to ^{12}C is higher in plants with a C_4 photosynthetic pathway than in the more common C_3 plants. C_4 plants in the New World include maize and *Amaranthus* species (Bender *et al.* 1981; Buikstra 1992).

the amount of corn consumed. Bender *et al.* note also, that unlike samples tested from Aztalan, the Cahokian apparently of highest rank was one of those consuming *more* corn (1981:350). Buikstra's samples also included some from Cahokia's Mound 72, but her results stand in contrast to those obtained by Bender *et al.* (1981). Buikstra (1992:93) writes that the apparently *lower* corn consumption of the highest ranking individual tested suggests that "the economic and ritual importance of maize did not extend to daily cuisine for persons of high rank."[15]

The entire enterprise of carbon-isotope analysis is confusing at best. For example, it seems worth noting that the $\delta^{13}C$ value of -20‰ obtained by Bender *et al.* for one of the low-status Mound 72 burials is as low as that which drew them to conclude that the Hopewell "did not make use of corn as an important item in their diet" (1981:351). It seems that the variation *within* populations is greater than that *between* populations for which disparate conclusions about dependence on corn are being drawn. Also, the data mentioned above regarding the origin of the females in Mound 72 are obviously highly relevant. As Spence put it, discrepancies in isotope ratios could reflect either "status

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[15] The actual $\delta^{13}C$ values obtained for the bone collagen were as follows:

Bender *et al.* (1981:350): Mound 72 burials: highest status (male): -16.5 ‰
second in status (male): -18.7 ‰
lowest status (females): -20.0‰ and -15.0‰
Fingerhut burials: -15.9‰, -15.2‰

Buikstra (1992:93): Mound 72 burials: highest status (male): -17.2‰
lowest status (females, averaged): -14.1‰

These can be compared to the Hopewell values obtained of -20‰ to -24‰, and the Aztalan values of -12‰ to -19.2‰ (Bender *et al.* 1981:350-1).

differentiation in access to particular foods, or origins in different environments where different subsistence systems were in effect" (Spence personal communication). Thus, the variation in the females' $\delta^{13}C$ values could indicate either that some of them, at least, were not from Cahokia, or that they were of different statuses – or even both.[16] Finally, given the possibly confounding problem of elevation of $\delta^{13}C$ values through consumption of corn-eating animals (Buikstra 1992:88-9), or consumption of *Amaranthus* species, which are known in archaeological contexts in the American Bottom (Johannessen 1984), the results of Bender and colleagues and Buikstra become very difficult to interpret. Their meaning with respect to the diet and health of Cahokians is unclear.

In sum, then, Mississippian health is recognized by osteoarchaeologists as having been highly variable, despite the popularity of the view, derived from the Dickson Mounds analyses, that these late prehistoric populations suffered from poor health. Certainly, uniformitarian assumptions, generalizing to all Illinois Mississippians on the basis of one well-studied community, are not well-founded. Studies of American Bottom remains – while comparatively few, and hampered by small samples, poor preservation, and a lack of diachronic perspective – indicate that the health of these populations was quite good.

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[16] It seems worth noting here that among the Calusas of Florida, known by the Spanish to practice retainer burial, the people sacrificed were usually war captives (Hudson 1976:77).

Summary

The history of research in Cahokia and the American Bottom is long and complex, as is the history of research into prehistoric Mississippian health. The two have many points of connection, including the importance of Cahokia and the Mississippians in evolutionary schemes, the influence of the Moundbuilder myths on early investigations, and the fact that much remains unresolved. Interpretations of Cahokian society, and of health in Mississippian-era Illinois, are bound up in larger issues and concepts to which different generations of researchers have varying degrees of allegiance. For example, an image of Cahokia as the centre of a system of state-like proportions and character permits the interpretation that exploitation caused the poor health seen in hinterland communities, just as happens today in societies on the periphery of capitalist economies. On the other hand, the rejection of this model of Cahokian society by fifth-generation scholars would seem to dovetail with more positive pictures of Mississippian health.

Thus, it would seem that the subject of Cahokian health and society is a very fertile and theoretically important area for study. It is also practical to focus on archaeologically-derived speculations about health for the American Bottom, given the comparative shortage of skeletal remains available for study. Thus, the next chapter will include a more detailed discussion of some debates regarding the nature of Cahokian society, a statement of the position which I think is most reasonable, and an exploration of the implications for health.

CHAPTER V

CAHOKIA: HOW COMPLEX, HOW HEALTHY?

...considering the size of the Cahokia site proper, approaching six square miles, and considering the length of the Mississippian occupation, at least six centuries, let's say 25 generations, and considering the small area explored archaeologically, less than a quarter of one percent of the site, just the ambition to make a definitive statement at this time about Cahokia presumes the confidence a scholar might assume who seeks to understand the operations and inner workings of the United States Congress by monitoring the archaeologically visible activity of one legislative wastebasket. R.L. Hall 1975:30.

Hall's observation is, of course, cause for humility. There has been a great deal of work done at Cahokia, and in the American Bottom, since Hall's comment was written, but the order of magnitude of the extrapolation probably has not changed. And, as Wylie (1993:21) has written, it has become progressively harder over the years "to sustain the (millenarian) belief that we are approaching a time of 'greater fullness of data' in which explanatory questions will be resolved," since the primary result of the exponential increase in the available data in archaeology has been a parallel increase in confusion. This is perhaps nowhere more true than in the study of Cahokia and the American Bottom. The vast gains in relevant data from the investigations of the last decades have helped to clarify some details of Mississippian life in this area – for example, the chronology has been fine-tuned considerably – but many important questions remain emphatically unresolved. Accordingly, this chapter first provides a brief overview of some of these debates; second, it describes a picture of Cahokian society and development

which I consider reasonable, and briefly reviews the relevant settlement data; and finally, it discusses the implications of these for health in the American Bottom in prehistory, using an *Annaliste* framework of organization.

CAHOKIA: SOME CENTRAL DEBATES

For the reasons described in the previous chapter, the designation of Cahokia as a town or city, and the central community in a chiefdom or state, has been important to some scholars. As supporters of high-end population estimates for Cahokia, O'Brien (1972), Hall (1993) and Fowler (1974, 1989) have predictably been proponents of Cahokia's 'city-hood,' and it is their view that has had the greatest impact in the more popular literature about Cahokia. Like Pfeiffer (1974), *Cahokia: City of the Sun* – the companion volume to the displays in the new Cahokia Mounds Interpretive Centre – frequently and enthusiastically refers to Cahokia as "a prehistoric metropolis," and a "great urban center" (Mink *et al.* 1992). A display in the award-winning Interpretive Centre itself, which opened in 1989, provides a point-by-point comparison of Cahokia to the modern city of St. Louis, concluding that the two are much alike. In contrast, fifth-generation scholars tend to avoid the town *vs.* city issue entirely, labelling the site a "centre" (*e.g.*, Milner 1990). No doubt this is because many of the issues have been effectively subsumed by the argument regarding the putative statehood of the Cahokian culture system, which has received a great deal of attention. For example, O'Brien's (1972) article, arguing for Cahokia's city-hood, uses criteria from Childe's definition of

a city – dense population, specialization of labour, monumental public works, social stratification, long-distance trade, art and science, writing (the only thing which everyone agrees Cahokia lacked), and state organization – which are very similar in nature to the criteria she used in 1989 to argue for the existence of the "Ramey" State, with Cahokia as its capital, an idea introduced by Gibbon (1974).

The state *vs.* chiefdom argument is, of course, partly a "semantic quibble" (Fiedel 1987:251), but simply labelling it thus does not accurately convey the damage that such emphases have done to the enterprise of studying past societies. Wolf (1982:3-6) contends that our historical and anthropological disassembly of the human world into labelled abstractions, which eventually become reified, creates "false models of reality." McGuire (1992:250) adds that archaeological theory habitually "transforms real human experience such as the San family or the Inka state into abstract categories, the "family" or the "state," and treats these abstractions as what should be explained; what is a household mode of production, or what caused the rise of the state?" Emerson (1993:2) expresses distress both that the endeavour of labelling Cahokia has become an end in itself, and that the typological approach in question "obscures critical synchronic and diachronic variability within societies."

Feinman and Neitzel (1984) offer a very concrete objection to the typing of societies. Their study of ethnographically known Native American cultures indicates that the supposition that the variables used to distinguish different social types co-vary is simply unfounded, and that the categories used are not discrete. Thus, recognition of the range of variation in prehistoric societies is precluded by archaeologists' tendency to "use

one or two key attributes to infer the presence of all characteristics traditionally associated with particular types" (Feinman and Neitzel 1984:72). Emerson (1993) notes that this tendency has been pronounced in Cahokian archaeology; examples will be seen below.

In short, then, I join Emerson (1993:3) in rejecting the identification of Cahokia as a chiefdom, state, or any other type as a useful research goal. However, although Cahokia's designation is not a concern here, some of the issues which are components of the labelling debate are clearly important to the understanding of health. In particular, some key areas which crop up repeatedly in the Cahokia literature as part of the debate about its classification include population size, the inter-site organization of the system, the nature of elite power, and occupational specialization. These are all complex topics which are worthy of lengthy discussion; however, only brief overviews will be provided here. The subject of Cahokia's decline will also be discussed here, for it too is linked to questions of complexity, and is certainly important to considerations of health.

How many people lived at Cahokia?

Fowler gives a brief discussion of the history of population estimates for Cahokia, pointing out that the considerable variability in numbers has been a result of "attitudes regarding Indians, their hypothetical mound-builder ancestors, and both popular and anthropological prejudices" (1989:191). Brackenridge guessed that Cahokia probably had a population as big as that of Philadelphia in his time, around 40 000 people, while Cyrus Thomas suggested a figure of ten to twelve thousand in 1907. Interestingly enough, projected figures have oscillated between these two extremes ever since.

Population estimation is, unfortunately, an uncommonly difficult and frustrating archaeological enterprise, as attested to by the vigorous debates on the subject (*e.g.*, Deboer 1985). Early attempts were made to estimate Cahokia's population using what Fowler (1989:191) called "the labor-force approach," estimating the number of man-hours required to build Cahokia's substantial public edifices, and then extrapolating the population size from that figure. In 1969, using this method, Reed estimated a minimum population size of 10 000 (Fowler 1989:191). Muller, demonstrating that "large-scale public works do not need to imply large populations," calculated that Monks Mound could have been put up by 2000 persons in a total of 200 days (1987:12). However, most estimation efforts have followed the recent archaeological fashion of calculating population size from the number of residential units in the area.[1]

Gregg (1975) was the first to apply a systematic population estimation technique to the site of Cahokia. Using Naroll's (1962) method, Gregg arrived at a figure of 25 500 people for Cahokia's Stirling phase, the height of the site's occupation (1975:134). An editor's note in the same article indicated that the alternative estimation method created

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[1] Several formulae for estimating population on the basis of structures have been developed. Naroll (1962) used the relationship between roofed floor area and number of people to estimate a population's order of magnitude. Naroll's formula has since been both elaborated upon (LeBlanc 1971), and criticized (Shea 1985), but his method of converting floor space to population, through the use of a 'universal constant' based on ethnographic data, is still the most popular method of estimation for most kinds of sites. While Casselberry's (1974) formula operates on the same principle, Cook and Heizer (1968) elaborated on Naroll's realization that – despite his provision of a $10m^2$/person rule of thumb – the relationship between space and people is in fact non-linear, and created a new formula to reflect this reality.

Naroll's (1962) formula: Dwelling population = floor area/$10m^2$

Cook and Heizer's (1968) logarithmic formula: Dwelling pop. = $0.54994(\text{floor area in } m^2)^{0.62284}$

Casselberry's (1974) formula: Dwelling pop. = floor area(in m^2) /6

by Casselberry produced a figure of 42 780. These figures have served as the basis for most references to population size in the Cahokia literature since.

Fowler's opinion has oscillated over the years: in 1975 (p.100), he wrote that "the Moorehead population of Cahokia may have approached 40 000"; in 1978 (p.467), he had downscaled his estimate somewhat, but still concurred with the high end of the range, quoting a figure of 30 000 for the size of Cahokia's population at its peak. In his 1989 book, he wrote that while some, such as Ford and Griffin, registered objections that this figure may be too high by a factor of four, he feels that this division by four would result in an estimate that is probably more removed from the truth than the high estimate (Fowler 1989:192). Fowler's final position on the matter is this:

> It seems to me that the best order-of-magnitude population estimate is more than 10 000 rather than in the thousands. Even if not as great as 30 000, the population at Cahokia was the largest in prehistoric times for any community within the present boundaries of the United States of America. To put it another way, Cahokia was not only physically the largest community, it was also the most densely populated. (Fowler 1989:192)

There are those who would at present disagree. While the fourth generation of Cahokia researchers, including Gibbon (1974), Gregg (1975), Fowler (1974, 1975, 1978, 1989), and O'Brien (1972, 1989, 1991), has consistently favoured high-end estimates, the fifth generation has equally consistently favoured lower estimates (*e.g.*, Pauketat 1991, 1992; Milner 1986, 1990, 1991b; Emerson 1991, 1993). Milner's often-quoted 1986 work on Mississippian period population density in the American Bottom area made several major criticisms of the previous estimates of Cahokian population: first, that Gregg's work relies on the assumption that the structures in question were each occupied for the

duration of the Stirling phase, 100 years; second, that the estimate was based on a comparatively small excavated area (2.2 hectares), which is likely not representative of all areas of the site; third, that both of the estimating procedures used (Naroll's and Casselberry's) are inappropriate.

Establishing contemporaneity of structures is a major problem in population estimates (Schacht 1984), but one that is ameliorated somewhat when structures could not conceivably survive for the whole period being examined. Milner (1986:231) estimated that for American Bottom pole-and-thatch houses, a structure longevity of 10 years errs on the side of generosity. He further noted that Naroll's (1962) procedure underestimates population size when the dwelling area of the structure is small, while Casselberry's method is specifically designed for multi-family dwellings. Since Stirling-phase dwellings in the American Bottom were small single-family houses, Milner opted instead for the use of Cook's estimating procedure, which corrects for these biases by allowing 25 square feet of living space for the first six occupants, and 100 square feet for each additional person (Cook and Heizer 1968).

Milner finally concludes that, given a structure longevity of five years, population density for the American Bottom sites during the Stirling phase was about 60 persons per square kilometre of habitable land, which is "on the low end of the range for swidden agriculturalists" (1986:234), given by Hassan (1981:41) as 3 to 288 people per square kilometre. Milner later concluded that "the occupants of Cahokia numbered in the thousands, not the tens of thousands" (1990:11).

Leaving aside for now the question of absolute numbers – thousands *vs.* tens of thousands – it seems worthwhile to add a note on the subject of population density. If Milner's population density figures for American Bottom sites are in any way applicable to the site of Cahokia, as is probably the case, then Fowler's (1989:191) assertion that Cahokia was "the most densely populated" community in what is now the U.S.A. (as also claimed in Hudson 1976:77) is questionable. For example, late proto-historic Huron villages in Southern Ontario are estimated to have had population densities of 86 people per hectare (or 8600/km^2) (Saunders, Ramsden and Herring 1992:120), although their overall population size was probably smaller by five to ten times.

Many archaeologists feel that the prospects for accurate population estimation for archaeological sites are not good (*e.g.*, see Fletcher 1981). Yet, the subject is not likely to be ignored, for population size is a subject upon which a great deal depends; for example, not only is it important for purposes of political designation, but it is also a major determinant of health patterns. It would seem that, barring marvellous breakthroughs in methodology, it is safest to adopt a moderate position on the question of Cahokia's population, a figure of perhaps seven to twelve thousand. If population figures are used not as a surrogate measure for complexity, but for their own sake, then they need not have so much significance attached to them.

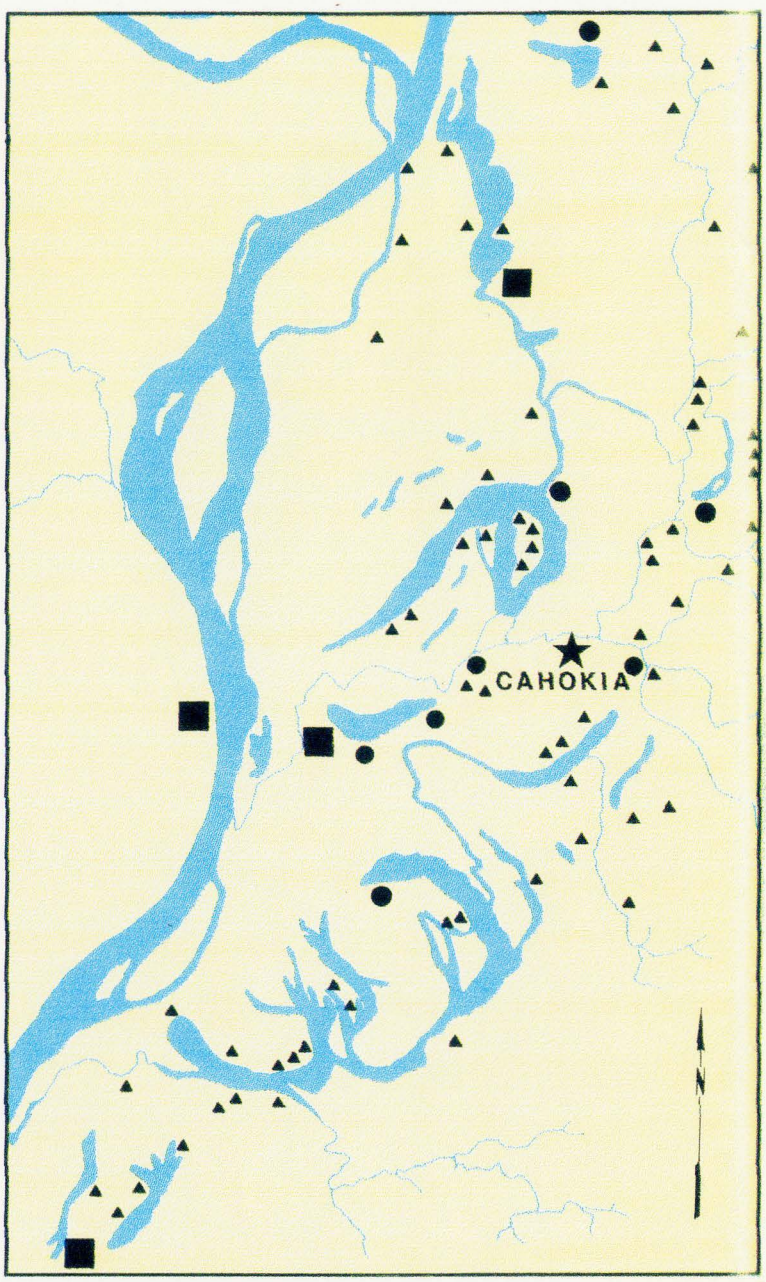
intersite organization of the Cahokian polity

The argument over the classification of the Cahokian polity effectively begins with Melvin Fowler's (1974, 1975, 1978) characterization of inter-site relationships in the

American Bottom, for to a large extent, it is on this characterization, and Gregg's (1975) high population estimate, that the case for statehood rests (Milner 1990). Regardless of whether the site is to be labelled on this basis or not, the issue of inter-site relationships is certainly important to explore.

It is clear to even the casual observer that the American Bottom has sites of different sizes and types. They differ in their area and in the number of mounds. Fowler's settlement model (1974, 1975, 1978) offered a formal division of these sites into a four-tiered hierarchy (see Figure 5.1). According to this hierarchy, Cahokia is the only first-line community. Second-line communities include the Mitchell, East St. Louis, St. Louis, and Pulcher sites, all relatively large sites with several mounds and an area of over 50 hectares. Third-line communities were single mound sites with habitations. Fourth-line communities were small villages, hamlets, and farmsteads without mounds.

Fowler's (1974) explanation suggested that the settlement system was centralized and carefully planned in order to efficiently exploit resources and control the population. According to Fowler, Cahokia was the seat of power, and controlled all the other sites through a formalized network with local elites. Second-line sites, he hypothesized, were "collection and redistribution points for regional resources as well as controlling major networks of communication [*i.e.*, waterways]" (1974:27). Fowler also suggested that communities as far away as Aztalan (in Wisconsin) were essentially second-line Cahokian outposts. Third-line communities were designed to exploit localized resources, and fourth-line communities were simply satellites.



Mississippian communities in the American Bottom were situated near the many lakes and streams: ***Star****—Cahokia, the first-line community;* ***Squares****—second-line;* ***Circles****—third-line;* ***Triangles****—fourth-line.*

Figure 5.1 Fowler's hierarchy of American Bottom settlement sites.
Reproduced from Mink, Corley and Iseminger 1992:18

This model of American Bottom society is often quoted, and has held great sway in the literature. Once again, it is this view of great complexity which was adopted by the Interpretive Center at the Cahokia site. This is not surprising, as its elegance is appealing. But, as should be predictable by now, fifth-generation scholars beg to differ with this portrait; most consider Cahokia to have been the seat of a less-structured chiefdom, albeit an extraordinary one (Pauketat 1991, 1992; Emerson 1991, 1993; Milner 1990, 1991b; Brown *et al.* 1989). The criticisms of Fowler's model are twofold.

The first difficulty is that the model rests upon the assumption that all the sites were contemporaneous, an assumption which Fowler himself recognized as problematic (1974, 1978), but which O'Brien (1989:284-5) does not even mention. Harn suggested at the time that the third and fourth line communities were not occupied when Cahokia was at its height (Fowler 1974), and that instead they existed before the population's nucleation into Cahokia and the second-line centres (Fowler 1978). As research has progressed, it seems that neither assumption is particularly supportable, and that site occupation histories vary considerably (Milner 1990). Certainly, as Milner puts it, "the conflation of sites with different histories – a result of our current temporal controls – produces a composite settlement pattern that is more dense and complex than the arrangement of sites at any single point in the past" (1991b:31).

The second problem with Fowler's model is that it infers attributes of the political hierarchy directly from physical attributes of the sites. Pauketat (1991:100) argues that "the number of mounds at sites is not a reliable means of inferring hierarchical relationships"; Milner agrees that it is "premature to equate the number of mounds and

the extent of surface debris at the second and third-ranked sites with distinct positions in a tightly integrated system" (1990:21). Milner further contends that:

> The number and size of mounds at the intermediate level American Bottom centres probably serve as better barometers of site longevity and the volatile nature of social relations featuring competing chiefly lineages with varied histories of ascendancy than of separate positions maintained for long periods in a highly developed site hierarchy. (Milner 1990:21)

Few have offered a well-constructed alternative to Fowler's four-tiered model, however, despite the major increases in knowledge since 1974, when the model was first developed. However, recently, Pauketat (1991) Emerson (1991, 1993) and Milner (1990, 1991b) have contributed to the definition of an alternative model. Milner, for example, wrote that "[t]he Cahokian system was arguably the most organizationally complex of the Mississippian societies, but it is doubtful that it was as highly structured as the existing literature would suggest," and suggested that the Cahokian culture system was "comparatively decentralized, geographically and socially segmented, less populous, and more dynamic" (Milner 1990:2).

Features of Milner's model, which I find more supportable than Fowler's, include self-sufficient mound centres which were headed by quasiautonomous elites, directly controlling the surrounding territory for variable periods of time. Cahokia also had a territory of its own, and had linkages, varying with distance and over time, with elite groups in other mound centres. Relationships were somewhat less formalized than Fowler postulated, communities were not planned outposts, and elite politics resulted in shifting and volatile power relations (Milner 1990:21-23; 1991b:32). Emerson (1993:1) reiterated the argument for a "much less cohesive sociopolitical organization," and suggested that

instead of a state-like scenario, the American Bottom of Mississippian times consisted of "moderate populations organized into a number of competing chiefdoms that display a diachronic political flux of coalescence and dissolution."

The nature of elite power

Intertwined with the question of inter-site organization is the question of the nature of elite power in the Cahokian polity, both within and between sites. Power, social stratification, and the nature of governments have long been popular topics of discussion in the social sciences and their precursors (Lopreato and Lewis, eds. 1974); however, I will only make brief comments here, without delving too deeply into the formidable body of general theory on these subjects. Throughout, it should be kept in mind that the social stratification visible in the archaeological record for Mississippian societies, primarily in burial contexts, could be of a number of different kinds; it could involve material inequity in daily life, extending to such basic necessities as food, or rest primarily upon inequity in religious privileges or decision-making power. There are many different possibilities, each with its own implications for the nature of inter-community relationships, and for heterogeneity in health. It should also be borne in mind that power, in and of itself, is not "inherently negative or repressive" (Miller and Tilley 1984a:5). As Foucault emphasized in *Discipline and Punish* (1979), power is multifaceted, involving not only constraint and denial, but enablement.

As mentioned, it has been postulated by some that Cahokia was a rigidly organized polity, with settlements located to permit maximum resource extraction within

the American Bottom (Fowler 1974, 1978), and by others that Cahokia also exploited its hinterlands (O'Brien 1991), even to an extent which adversely affected the health of the Dickson Mounds people living in the Lower Illinois River Valley (Goodman *et al.* 1984a; Goodman and Armelagos 1985; Goodman and Armelagos 1988; Goodman *et al.* 1992).

However, Milner (1990:26) has countered with the argument that such relationships are "simply implausible"; he contends that the level of organization, degree of efficiency in transportation, and extent of coercive power necessary to systematically exploit communities almost 200 kilometres away probably did not exist at Cahokia. Furthermore, the chronologies for the two regions do not match up, in that the exploitation postulated by Goodman and Armelagos would have been taking place *after* Cahokia's peak of development (Milner 1991b:39). Milner concluded it to be unlikely "that Cahokia was an expansionist, exploitive entity that possessed the means to coordinate far-flung economic activities, and to alter dramatically the ways of life of people located hundreds of kilometers away from the American Bottom" (1990:27).

It is certainly possible that this is an example of interpretations regarding Cahokia being structured to fit theoretical preconceptions about societal types. It also seems to trace back to the question of Cahokia's population. The extraction of food tribute from the hinterlands has been postulated as necessary because Cahokia's population was so high that it could not have supported itself on the resources of the bottomlands alone (O'Brien 1991); however, this same fact, the bottomlands' inability to sustain a high population, has been used to argue that there simply wasn't a large population (Milner 1990; Muller 1987). The latter argument seems more cogent than the former, which is

circular in the sense that it begins with the supposition that Cahokia was a state, proceeds to the assumption that therefore, Cahokia had a large population, and comes to rest on the assertion that therefore, Cahokia must have displayed another characteristic of statehood, the extraction of tribute from subordinate communities (O'Brien 1991). This layering of assumption upon assumption has created a position which is shaky at best. Finally, it is also possible that this notion of Cahokia exploiting communities far away is the outcome of trying to fit the Cahokian polity into theories of power in modern states, and core-periphery interactions. More will be said about this in a moment; first, however, on to the question of elite power within communities.

In much of the literature about Cahokia, it is supposed that the elite class wielded extraordinary power over their subordinates. The three principal examples which appear repeatedly are the sacrifices excavated from Mound 72, the presence of large monuments such as Monks Mound, and the existence of a 'site plan.'

The sacrifices of Mound 72 are almost universally viewed as evidence of the great power of the elite. Milner (1990:12), in a comparatively moderate statement, refers to "the capacity to command the sacrifice of numerous people" as a "particularly graphic indication of the power wielded by the elite social stratum" at Cahokia. Brown and colleagues (1989:5) imply that the Cahokia elites were despots through references to the sacrificial burials as "an intimidating show of force," and arguments that the burials attest to "the absolute use of power." Others take it further, explicitly tying these sacrifices into the larger theoretical framework which is at stake. O'Brien (1989:279) argues that the Mound 72 burials prove the Cahokian elite had a "monopoly over the use of legitimate

force," thus allowing Cahokia to meet yet another criteria for statehood; Yerkes concurs, suggesting that the burials "demonstrate the life-and-death power of one Mississippian class over another, a power that is found only in primitive states" (1991:51).

Classifications aside, I would argue that the sacrifices at Mound 72 have been regarded from a vantage point which fails to adequately consider the different possible ways of achieving the outcome seen archaeologically. It has been tacitly assumed, for example, that the people involved were unwilling to die, and that force was required to compel them to do so. This is not necessarily true. For example, examination of our own history shows that coercive force is not needed to compel self-sacrifice where religion or political ideology teaches that the subjugation of the good of the individual to the good of the many is noble, and brings its own rewards. It is possible that such a religion or ideology may be cultivated by an elite, and manipulated to further their own ends, but it is also possible that it may develop according to other imperatives. Furthermore, death can be viewed in many lights. It is certainly conceivable that those sacrificed and buried in Cahokia's Mound 72 considered the afterworld to be their true home, and eagerly sought the honour of sacrificing their bodies to go there with a revered leader.[2] In short, ritual human sacrifice is a highly complicated subject, and attributing the sacrifices to only the despotic exercise of coercive force is surely simplistic.

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[2] Ethnographic observations of the Natchez would seem to support this possibility; records of mass Natchez sacrifices indicate that those who died did so voluntarily, with a sense of higher purpose, and confidence that in the land of the spirits, many of their earthly burdens would be lifted (Hudson 1976:328-9).

The issue of Cahokia's site plan requires further consideration as well. First of all, it must be emphasized that different processes can result in an apparently orderly settlement plan; not all of them necessarily involve intensive planning or elite coercion.[3] Second, the level of site organization at Cahokia has sometimes been exaggerated. It is true that in the Lohmann phases, houses shared a common north-south orientation, and that the central precinct and grand plaza were not used for general occupation during the Lohmann and Stirling phases (Woods and Holley 1989); however, this hardly constitutes a pattern which could not possibly have arisen without central enforcement of a global site plan. The sudden appearance of these features of site organization at the beginning of the Lohmann period has also been postulated to be evidence for the existence of a coercive elite (Pauketat 1991). However, what appears to be 'sudden' or 'instantaneous' archaeologically can easily take years or decades, and in a society where individual house structures have longevities of only a few years (Milner 1986), the process of reorientation need not have been a jarring or enforced change.

The conversion of areas of Cahokia from residential to public use, and back again, has also been interpreted as evidence of waxing and waning elite influence; for example, Fowler (1978:467) wrote that the location of the palisade on previously residential ground indicates "the power of a coercive central authority directing the destiny of the Cahokia

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[3] In an intriguing paper, Banning (1994) has shown that orderly settlements in the ancient Near East which have been used as a basis for asserting the existence of "town planning" by specialists, and coercion by powerful elites, may be accounted for by other processes. Banning (1994:11) contends that in the case of prehistoric Near East sites, "global patterning in these settlements could well have resulted... not from overall design, but through the cumulative effects of many individual decision-makers, each locating, orienting and shaping houses and other structures by reference to pre-existing ones as well as topography."

community," whereas the later reversion of the central precinct to residential use in the Moorehead phase has been interpreted as the result of decreasing political organization (Mehrer and Collins in press). The latter seems probable; however, changes in function of parts of a site, from general to ritual use space – as in the case of the Woodhenge sun circles being constructed in a formerly residential neighbourhood (Mink *et al.* 1992) – and from private to public space, as in the case of the palisade, need not be attributed to a carefully designed site plan enforced by a coercive power (Milner 1991b:32).

Finally, the existence of Monks Mound and other mounds at Cahokia has also been used as evidence of the coercive power and extensive administrative structure of the Cahokian elite (Mink *et al.* 1992; Denny *et al.* 1992; O'Brien 1991). (See figure 5.2 for a vivid illustration of this conception of mound-building labourers subjected to the will of elites.) Once again, however, such extreme interpretations are not necessary to account for the available data. As Mendelssohn (1974:399) pointed out in reference to Egyptian pyramids, intensive supervision of labourers by a large number of knowledgeable and forceful overseers is not necessary for the construction of such monumental edifices, nor is it reasonable to suppose that an enormous work force could be compelled to labour against its will. The same is likely true of Cahokia. Giddens (1985:47) has argued that this conception of highly developed administrative power relates to Wittfogel's "wildly exaggerat[ed]" characterization of the degree of centralized coordination required for the building of irrigation projects.

It should be clear by now that interpretations of coercive, centralized authority and despotic elites are emphasized because they permit Cahokia to conform to theoretical

Figure 5.2 Popular image of Cahokian life. (Denny et al. 1992:14)

definitions of an evolutionary 'type.' However, two other influences must be mentioned here, the ethnographic accounts of the Natchez, and theory about modern states.

Often-quoted accounts of the Natchez emphasize the absolute power of the 'Great Sun,' the paramount leader, and the abundant human sacrifices which took place at his death or that of his brother, the 'Tattooed Serpent' (*e.g.*, Mink *et al.* 1992; Silverberg 1968). It is usually asserted that the Natchez data lend support to the interpretation of Cahokian elites as very powerful politically. However, although the Natchez would seem to provide a fairly strong ethnographic analogy, in that there is a relationship between the groups being compared (Duke 1991), other factors significantly compromise the understandings which can be drawn from historical accounts of these people. First of all, there is the likelihood that these last survivors of the Mississippian tradition were already much transformed by the events of European contact by the time observations of their culture were made (Silverberg 1968; Muller 1987). Second, the Frenchmen to whom we owe our accounts of the Natchez were not exactly neutral observers, but wrote their reports with various agendas which have been obscured by time. In addition, the French conceptions of their own society and monarchy surely influenced their understandings of the Natchez Sun's role and influence; for example, they interpreted the people's deference as a clear indication that he was an absolute ruler (Hudson 1976:210). Observations of the Sun's responsibilities and actions suggest that he "reigned more than he governed"; his council of elders and warriors contributed greatly to the decision-making process, individual Natchez villages could act independently of his wishes in time of war, and the deference shown him was largely ceremonial (Hudson 1976:210). Finally, other post-

Mississippian groups like the Creek, Chickasaw, and Choctaw confederacies showed a real lack of, and opposition to, coercive authority (Hudson 1976), and may be equally suitable models for Cahokian society; a comparative paucity of documentation on these groups has perhaps contributed to an overemphasis on the Natchez (Muller 1987:11).

It also seems that interpretations of Cahokian society have been influenced by models of modern statehood and core-periphery interactions, as well as modern theory regarding health in recently acculturated or colonized peoples. Unwarranted assumptions may be involved. For example, it has been assumed by some that the power wielded by elites in traditional societies is much like that exerted by modern states, only of a lesser magnitude; this is simply wrong (Giddens 1985). A coarse illustration is that modern weapons make it possible for a few people to physically subdue many, whereas such tools would not have existed in prehistory. Similarly, uniformitarian assumptions about the nature of regional political systems are involved in the assertions of Goodman and colleagues, and others, that Cahokia systematically economically exploited its outliers. However, core-periphery interaction theory is based on recent European history, and modern states operate according to economic imperatives in a way that cannot be assumed to have been true of past societies (Giddens 1985). In addition, the adverse health effects observed in modern 'peripheral' societies (Wirsing 1985; Turshen 1984) probably have as much to do with their acculturation into a rapidly changing economy with a different basis (and the confluence of disease pools) as with their actual position relative to the centre of power. Even if Cahokia extracted tribute from its outliers, it is unlikely to have caused radical economic re-organization of the kind or magnitude seen in modern

peripheral societies, and the disease pools of the communities would not have differed much. Pauketat (1991) has suggested that it is possible that the core-periphery model of interaction may be applicable to Cahokian society if it is used to conceptualize the locus and spread not of economic influence, but of religious customs; this seems a more fair use of the model in this instance.

In sum, then, it is not necessary to postulate an elite class with absolute power, or the frequent use of coercive force upon the people of Cahokia or the hinterlands. Such suggestions often stretch interpretations of available data, and seem to have been driven by modern theories regarding statehood, relationships between nations, and the control of economic resources including labour.

Occupational specialization

The subject of occupational specialization in Mississippian societies has been given a good deal of attention in recent years. Evidence is sparse, which seems only to enhance speculation about the subject. The major sorts of specialization proposed have been a bureaucratic class, a religious order, professional trading, salt production, production of chert blanks and hoes, and shell bead manufacture (Milner 1990; O'Brien 1991; Muller 1984, 1987; Yerkes 1986, 1991). Muller (1984, 1986) observes that much of this speculation derives from the prior assumption that Cahokia was a state or close to it – and specifically, from attempts to make Cahokia conform to Service's model of production systems – and indeed, this would seem to be a prime example of Emerson's (1993) complaint, that archaeologists take one attribute of Cahokian society, and assume

other attributes to be present on the basis of a presumed association between them. Muller (1984:493) provides an illustration, quoting Porter from 1969: "With the necessary population at hand, the evidence of Mesoamerican contact, and the appearance of 'town squares,' I would suggest that a market system actually developed in and around Cahokia by A.D. 1100." Yerkes (1991) falls into a similar pattern of inferring occupational specialization from the presence of a large population, monumental earthworks, and exotic materials. Muller (1984:493) comments further on the circularity of using such interpretations to support claims for Cahokia's statehood.

But what of the evidence? Holley and colleagues write that

> At present, the only indication for craft specialization at the subcommunity level during the Mississippian period derives from the concentration of microlithic technology (cores, blades, drills) at the Kunneman Tract [at the northern end of the Cahokia site].... This singular example, and its presumed relation to the shell-bead industry, may suggest that the Kunneman Tract was a subcommunity involved in a commissioned mode of production attached to the service of the elite and producing items for the prestige economy. (Holley *et al.* 1989:343)

Certainly, evidence for specialization of activity at particular sites, such as salt pans, exists (Muller 1984, 1986, 1987; Yerkes 1986, 1991); however, as Muller points out, the existence of specialized activity *sites* cannot be equated with the existence of *specialists*. Evidence for a specialized class of full-time bureaucrats, as discussed above, is limited to the existence of large public monuments and a site plan which are supposed to have required extensive administration; although the top leadership and religious roles may have been fulfilled by 'full-time' specialists, it is not necessary to postulate the existence of other full-time administrators, nor is this supported by the ethnographic

evidence (Muller 1984:493). O'Brien (1991:148-9) argues that the trade goods found at Cahokia indicate a class of traders similar to the Aztec *pochteca*; however, the existence of a wide variety of trade goods from faraway places does not necessarily imply the existence of a mercantile sector (Stoltman 1991).

The intractability of the debate (see Muller 1984, 1986, 1987; Yerkes 1986, 1991 for an ongoing dispute) seems to have two origins. First of all, "specialization" has been used to mean many different things; its definition has perhaps been stretched on occasion, again, to allow conformation to theoretical models of social types. For example, O'Brien (1991) classifies hinterland subsistence farmers as "agricultural specialists," which is clearly pushing the case, as is classifying gender-based division of labour as specialization (Muller 1987). Furthermore, for an economy to be classified as one involving occupational specialization, more than one industry must be represented (Holley *et al.* 1989:343). Second, it is hard to say what constitutes support for either position. Muller (1986:406) advocates the application of Occam's Razor, and thus, the postulation of only the minimal level of specialization required to account for the observed pattern. Yerkes (1991:52) attempts to shift the burden of proof to those postulating minimal specialization, demanding that they provide evidence for this position. Ultimately, as Milner puts it, "available evidence is insufficient for discriminating among different levels in the incidental to full-time work continuum" (1990:14). Given this impediment, I agree with Muller that currently, the best suggestion is that there was minimal specialization.

Why did Cahokia decline?

A morbid curiosity regarding the decline of earlier societies seems to be a hallmark of archaeological enquiry and modern thought (Tainter 1988). Cahokia provides an excellent example of numerous competing theories. The Mississippian in general ended in historic times, with the last populations decimated by disease and violence that began with the devastating invasion of the de Soto expedition in 1539-42. The small descendant populations that remained were little more than refugees (Williams 1990). However, at the time of de Soto's arrival, when some Mississippian centres were just reaching what was to be their peak, Cahokia and a large area around it were already long-empty of major centres, and constituted what Williams (1990) has called "The Vacant Quarter" (See Figure 1.1). The people had not actually vanished, but trickled out from the floodplain first into the uplands surrounding the American Bottom, and then farther afield (Milner 1991b).

But why? Cahokia's decline has been the subject of much speculation – as has the decline of the Hopewell interaction sphere in the same area a millennium earlier – especially, perhaps, because it was not an apocalyptic event, but a slow decline that took two hundred years to reach completion.

Early Europeans on the scene were told of this long decline, predating their own arrival, by the Natchez and other nations then living along the Mississippi; some attributed it to the intense fighting between these nations (in Silverberg 1968:325). However, while it is true that the skeletal remains of certain populations which lived in the Mississippi River Valley in late prehistoric times, such as the Oneota of west-central

Illinois, show markedly elevated levels of trauma (Milner, Anderson and Smith 1991), those from the American Bottom, and other Mississippian groups, do not (Milner 1982, 1983a; Powell 1988). On the other hand, the last stockade reconstruction at the site of Cahokia took place at around 1200 A.D., when the population size was declining from its early Stirling Phase peak, and so it is possible that this coincidence of population decline and stockade repair may have something to do with inter-group conflict.

An obvious and popular suggestion has been that the depletion of resources such as timber, game, and fertile soil finally became too severe for the population to sustain itself (Fowler 1975). Hargrave (1991), for example, found that architectural changes in the Mississippian houses of the American Bottom were partly consistent with attempts to improve thermal efficiency, possibly to lessen consumption of scarce fuelwood. A contrary opinion has been put forth by Hall (1991), who tied the Cahokian and Hopewellian collapses together in his theory of "the Shmoo Effect." Hall argued that "agricultural success as well as crop failure could have had equally disastrous results for the Cahokia system" (1991:23); the adoption of more productive crops could have caused a reversal of the environmental circumscription which had caused the consolidation of the polity. The collapse may have thus been due to "the breaking down of social organization in the face of abundance and diminished need for interdependence" (Hall 1991:26).

Other environmental causes have been suggested. For example, Griffin postulated in the 1960s that the collapses of Hopewell and Cahokia were both due to a cooling climate that necessitated subsistence shifts. He later made the interesting suggestion that the movement of rivers over time may have limited the life cycles of Mississippian

communities; as rivers moved away from centres, they declined (Griffin 1984b). Alternatively, flooding might have been a problem; given that the American Bottom is one big floodplain, and that the modern communities there have had to resort to building levees as protection, it seems conceivable that bad flooding could have been a problem at prehistoric Cahokia. This idea is supported by Brown *et al.*'s observation that changes in the local hydrology, *i.e.*, a raised water level in the Mississippi, likely resulted from erosion caused by wood removal and field clearing; this in turn would account for the observed trend towards habitations on higher ground in the American Bottom in the later Mississippian era (Brown *et al.* 1989:10).

Others have suggested that the demands of an increasing population finally exceeded the ability of the existing subsistence technology to provide food and health (in Tainter 1988). Nabokov and Snow (1991:140) suggested that "Mississippian society was seduced by the blessings of its labor-and-cost effective riverine environment," and that "unabated population growth" could have caused acute problems of sanitation with concomitant epidemics of dysentery and tuberculosis. Mink *et al.* paint a similarly gloomy picture of steadily worsening health conditions:

> Unfortunately, Cahokia's dense population eventually experienced what we now call urban stress. Despite unprecedented prosperity... residents suffered from periodic malnutrition and disease. They must have been plagued, as well, with depletion of natural resources, pollution from wood smoke and human waste, and increased competition for space. As these conditions worsened, it is likely that the Cahokians also experienced tightening political control, stricter social distinctions, and increasingly limited access to goods and services. (Mink *et al.* 1992:26)

Mink *et al.* thus propose that political sequelae to worsening conditions would have ultimately contributed to Cahokia's fall. Other ideological explanations for Cahokia's collapse have also been catching up to materialist explanations in prominence. For example, while not denying the likelihood of decreased abundance of resources like wood, Pauketat (1992) has argued, contrary to Mink and colleagues, that there are no reliable indications of *substantial* shortages, environmental degradation, or worsening health, at the time of Cahokia's downslide. He explains that the answer may lie partly in the natural life-cycle of complex societies: "[t]he process of pre-state centralization contains within it a tendency towards decentralization. The former begets the latter, and the consequence is a complete cycle of a chiefly polity" (Pauketat 1992:43).

A novel about Cahokia, *People of the River* (Gear and Gear 1992) – which initially appears to be a prehistoric 'bodice-ripper,' but is not without merit – has proposed an intriguing possible scenario with a level of detail possible only in fiction. This scenario combines two classic elements of decline theories (Tainter 1988), suggesting that resource exhaustion caused by overfarming and climate change caused strife in Cahokia, and led to tribute not being paid by outlying communities; this, plus elite moral turpitude (the Sun Chief was characterized as an irresponsible, sexually depraved drug-abusing sociopath) led to crisis at Cahokia, and ultimately to population dispersal.

Depraved despots notwithstanding, perhaps the final word on the subject of collapse should go to Tainter, who concludes that it is important to realize that

> under a situation of declining marginal returns collapse may be the most appropriate response. Such societies have not failed to adapt. In an economic sense they have adapted well – perhaps not as those who value

> civilizations would wish, but appropriately under the circumstances ... Collapse then is not intrinsically a catastrophe. It is a rational, economizing process that may well benefit much of the population. (Tainter 1988:198)

One vision of Cahokia and the American Bottom

The discussions above will have revealed that I consider it best to keep interpretations regarding Cahokian society as simple as possible, and at as low a level of extrapolation as possible. This is primarily because extrapolations which go too far inevitably reflect more about the community of archaeologists than the archaeological community in question. My position, then, is that Cahokia was not necessarily highly complex (in the sense of having attributes of 'statehood' such as multiple levels of bureaucratic decision-making or elites with absolute power), although it was surely enormously complex in other ways, such as ritual and religious life, that we have had little chance to appreciate archaeologically. In this sense, my views are most closely affiliated with the fifth-generation interpretations of Cahokian society from authors such as Milner, Muller, Emerson, and Pauketat.

To reiterate: I consider it probable that at its height, Cahokia proper had a population of no more than about 12 000 souls, and possibly fewer. The population of the American Bottom hinterlands probably numbered in the low thousands (Milner 1986). Trade between communities of the American Bottom, and well beyond, was lively, although long-distance trade was carried out by down-the-line passing along of items rather than by specialists (Muller 1987). Full-time occupational specialization was probably rare, with exceptions including paramount leaders and ritual specialists (Muller

1987). The extraction of tribute from outlying communities is doubtful, although there may have been some redistribution of resources within communities (Hudson 1976). Relationships between communities of the American Bottom were amorphous and shifting, and were not confined within a formal hierarchy of power (Emerson 1991). Elite power was less absolute than is often suggested, and may have been primarily religious in nature (Pauketat 1991). Social inequality probably was expressed in predominantly non-material ways, for even low-status American Bottom farmsteads had access to exotic goods (Milner 1991b). As to Cahokia's decline, a population using diverse subsistence strategies, and of a size well within the region's carrying capacity (Milner 1991b), suggests that catastrophic shortages of food probably were not a contributing factor; however, some shortages of critical resources such as wood are certainly possible (Hargrave 1991), as are problems in cultivation due to changes in climate, and anthropogenic changes in the local hydrology (Griffin 1984b; Brown *et al.* 1989). A general worsening of the quality of life may have occurred, but it is not necessary to assume that this was the cause of Cahokia's demise; its roots may have lain in extraordinary success in cultivation (Hall 1991), or in the political processes which once permitted its consolidation (Pauketat 1991).

It should be borne in mind, of course, that the sketch given above is only one possibility, and I do not contend that it is the 'right' one. But it is past time to turn to the actual settlement data from Cahokia and the American Bottom, to see what further inferences can be drawn from them regarding social life and disease ecology, for interpretations about health there in the Mississippian era.

Changes in settlements over time:

Emerson (1991:222) noted balefully that "[i]t is very frustrating when one examines the data that are actually available from the Cahokia site," because many large-scale excavations which were conducted there have not yet been fully reported; it would seem that the trend has been towards "theorizing at the expense of more mundane data description, analysis, and publication." Certainly, there are fewer settlement data available for Cahokia than would be expected given the amount of theoretical discussion about it; however, recent excavations at Cahokia's Interpretive Centre Tract II and Kunneman Tract, as well as the FAI-270 excavations of American Bottom settlement sites, have contributed some information about diachronic trends in settlement at and around Cahokia, summarized below. These trends include changes in site plan, house construction and design, house location, and storage practices.

The Lohmann phase (1000-1050 A.D.) at Cahokia is demarcated from the previous Emergent Mississippian period by major changes in the site plan and house construction. With respect to the latter, there was an (archaeologically) abrupt, universal shift at the beginning of the Lohmann phase from single-post structures to wall-trench structures (see Figure 4.8). Contemporary with this shift was the engineering of major transformations to the Cahokian landscape, including the conversion of residential areas to public space, monument construction and the levelling of plazas (Holley *et al.* 1992; Collins n.d.). Houses were carefully oriented along a north-south axis, and did not intrude into public areas, evidence of adherence to an overall site plan (Mehrer and Collins in press; Collins n.d.). At this time, the smallest unit of settlement is apparently the supra-family cluster,

a group of four or five structures arranged around a central courtyard (Collins n.d.). Storage was external and communal, as were cooking facilities, presumably indicating significant cooperation and cohesiveness between households (Holley *et al.* 1989). During the Lohmann and Stirling phases, residential subcommunities, each with their own mounds and small plazas, are evident at Cahokia (Holley *et al.* 1992).

As is demonstrated by Figure 5.3, house structures changed in both shape and size over time, becoming progressively more square and larger in floor area. This was not as pronounced in the Stirling phase (1050-1150 A.D.) as in later phases; however, the Stirling phase did have other definite differences from the preceding Lohmann phase. In particular, storage became internal and private, far less attention was paid to the cardinal orientation of houses, and architectural diversity in house structures increased (Mehrer and Collins in press; Collins n.d.; Holley *et al.* 1989). In addition, increased residential stability (*i.e.*, the rebuilding of structures on the same spot over and over) is observed, as is a trend towards fewer structures per cluster – the unit of residence probably being the nuclear family – and more inward-looking sub-communities (Collins n.d.; Holley *et al.* 1989; Mehrer and Collins in press). The change to internal, private storage and cooking has been postulated to reflect increasing insularity of the household from its neighbours, and possibly the hoarding of food out of sight (Holley *et al.* 1989). The observed residential stability may also reflect insularity or possessiveness, in the sense of familial ownership of, or entitlement to, land.

Although the early Stirling phase probably represented Cahokia's zenith, with a proliferation of mound construction, and the achievement of its maximum population, the

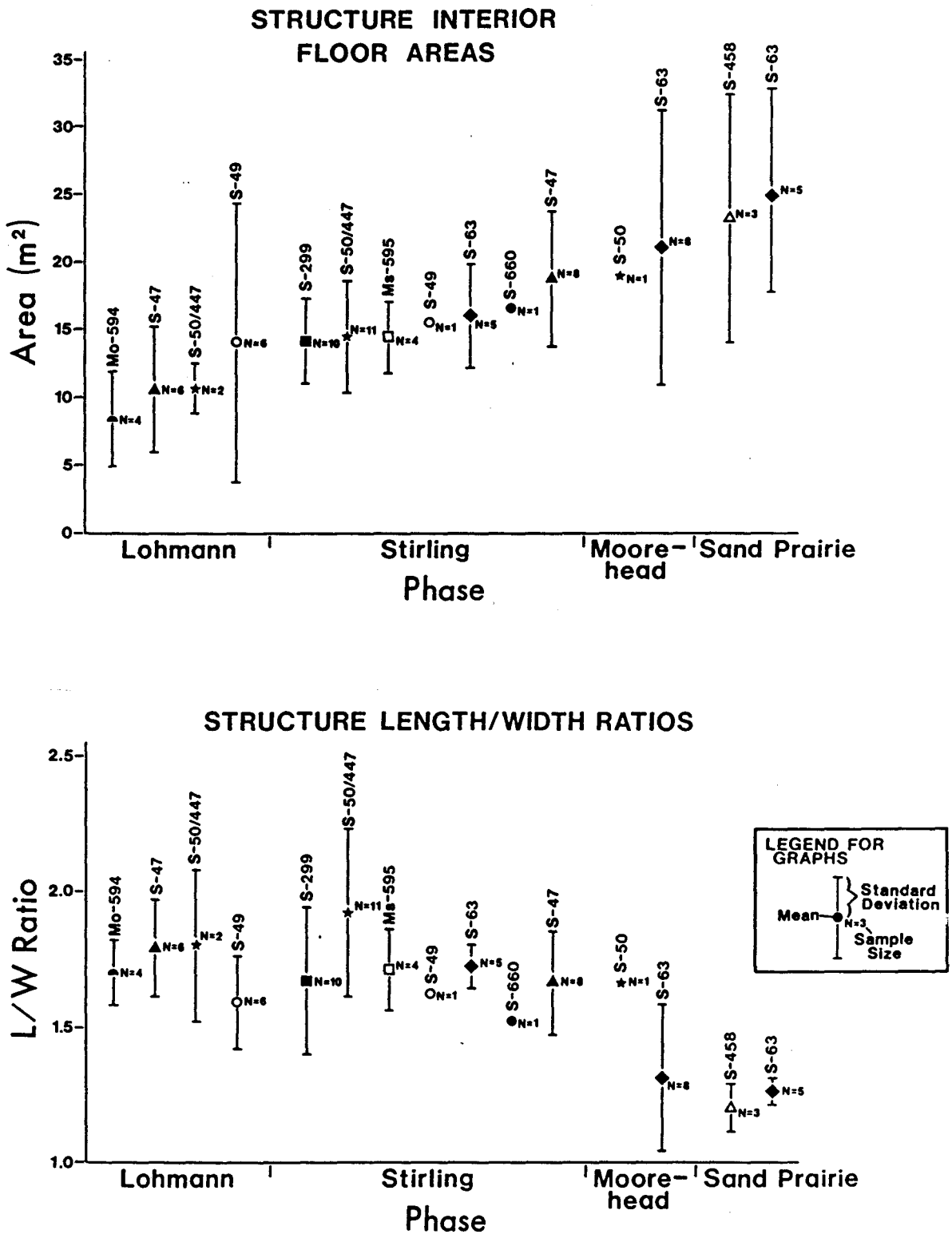

Figure 5.3 Changes in Mississippian house size and shape in the American Bottom. Reproduced from Milner et al. 1984:167.

beginning of the end was nigh. During the late Stirling phase, the slow conversion of public areas, such as the central precinct, back to residential use, began (Holley 1992). Contemporary with this development, the stockade was built for the first time, attesting to conflict of some kind (Brown *et al.* 1989).

The Moorehead phase (1150-1250 A.D.) is marked by the maintenance of the stockade, a pronounced return to external storage practices (Holley 1989), and a slight increase in the number of structures per residential unit, and possibly the number of people per unit (Collins n.d.). In addition, there is a movement to the higher land within the Cahokia site, possibly as a result of heightened water tables (Brown *et al.* 1989), and no obvious adherence to an overall, or even local, site plan (Collins n.d.). Houses were built in less formal arrangements and in former public zones, and there is no evidence for construction of new public monuments (Holley *et al.* 1989; Holley 1992; Mehrer and Collins in press). The Moorehead phase is also characterized by smaller residential subcommunities and a dramatic population decrease at Cahokia itself, the latter probably due to people moving back to the uplands (Brown *et al.* 1989; Holley *et al.* 1989).

In the Sand Prairie phase (1250-1400 A.D.), structures are usually single, rather than part of a cluster, but are of a more square shape and a much larger size. The changes in house structure shape and size have been suggested to relate to shortages in wood for building and heating (Hargrave 1991), but may also relate to changing household composition. Hargrave (1991) and Collins (n.d.) have postulated that a larger household unit – as seen both in the Lohmann phase and in the Sand Prairie phase – may have permitted a diversification of economic adaptations, through greater flexibility in the

exploitation of subsistence resources. The Sand Prairie phase represents the culmination of the reversion of 'special-purpose' areas back to general use, with occupation of the previously ceremonial area immediately adjacent to Monks Mound (Mehrer and Collins, in press). The stockade was not rebuilt, and there were intrusive non-elite burials in elite mounds at Cahokia, both of which indicate a declining elite presence (Milner 1991b).

And so, in general, the Lohmann phase represents the consolidation of Cahokia as a regional power, and the Stirling phase is regarded as the zenith of the Cahokian polity. The Moorehead and Sand Prairie phases are clearly the periods of Cahokia's decline and final disintegration, which was so complete that native groups who lived in the area at the time of the first European contact knew nothing of Cahokia or its inhabitants (Fowler 1989). Changes in the surrounding American Bottom region correspond to these changes at Cahokia proper to some extent; some of the more prominent diachronic shifts are in the frequency and type of outlying sites, and aspects of mortuary behaviour.

During Cahokia's growth and ascendancy in the Lohmann and early Stirling phases, outlying populations are scarce (Brown *et al.* 1989); people were presumably drawn in from the uplands by Cahokia's "city lights" (Holley personal communication). Later, as part of the Cahokian diaspora in the Moorehead phase, more little settlements in the American Bottom and surrounding uplands appear, and the new mound centres at Mitchell, East St. Louis, and St. Louis achieved prominence (Brown *et al.* 1989). In general, the complexity of bottomland sites (in terms of their possession of features such as elite dwellings and specialized ritual buildings) paralleled the development at the site

of Cahokia itself, in that the highest complexity is seen in the Lohmann and Stirling phases (Emerson 1993). Multicommunity cemeteries, prevalent at this time, were eventually completely replaced by single community cemeteries by the Sand Prairie phase, possibly reflecting increasing autonomy of local groups (Milner 1984b, 1991b). However, nonlocal materials continued to be prevalent in this final period, even in outlying farmsteads, indicating the maintenance of trade networks despite the dissolution or weakening of the overarching regional political structure (Milner 1991b).

These changes over time in Cahokian settlement patterns, when added to the interpretations about the nature of Cahokian society discussed previously, provide a basis for some inferences about the health of the people who lived there.

HEALTH AT CAHOKIA

The Braudelian model of time as it applies to Cahokia

Here, I use "the *longue durée*" to refer to phenomena which transcend the duration of Mississippian society. These factors would include environmental and geographic constraints, climate, Cahokia's location at the conjunction of various biotic zones, the available abiotic resources, potential disease vectors in the local fauna, and *mentalité*. The *Annaliste* concept of *mentalité*, or the sum of ideology, beliefs, symbolism, and cultural patterns (Duby 1985[1974]), is clearly relevant to any study of disease – but just as clear is the difficulty of achieving a deep understanding of these aspects of a society through archaeological remains. However, without knowing the details, one can suggest that

aspects of *mentalité*, such as gender roles and the significance of disease to the individual, were probably deeply rooted in pre-Mississippian culture, as were some aspects of ritual and symbolism (Hall 1986).

On the level of the *conjoncture*, or "the history of economies and states, societies and civilizations" (Braudel 1980:3), I am considering forces which are operating within the 400 year duration of the Mississippian period. This includes changes in sociopolitical organization – on the most general level, the rise and fall of Cahokia – and changes in population size and density, changing settlement configurations, house construction, waste disposal patterns, and storage patterns. There is also the process of environmental degradation through erosion, resource depletion (especially wood), and pollution, changes in regional relations, and alterations in the landscape, including the building of woodhenges, mounds and stockades. And here again is the aspect of *mentalité* – at this level, we can perceive shifts in social ideology to some extent. Specifically with respect to health, this is the timespan upon which epidemic cycles of some infectious diseases, such as tuberculosis, operate; concomitant with this would be the waxing and waning of herd immunity to these diseases (Jones and Moon 1992).

Finally, there is the level of the *événement*, or history of the short term, of events and individuals (Braudel 1980:3). Factors on this order of time which would affect health at Cahokia include disasters such as resource shortages, floods, crop failure, or war. Also included would be the introduction of outside infections, which would probably occur especially at times of regional gatherings. The actual disease processes operating at this scale would include episodes of illness in the individual, and short-term epidemic cycles.

As well as different temporal scales, there are also different spatial scales to consider, since the Cahokian polity contained several types of settlements, including isolated farmsteads, town-and-mound centres, and Cahokia itself. Although the whole American Bottom region would have been one disease pool in the sense that disease would be spread from one community to another through regular contact, there would likely have been different prevalence rates, just as suburban communities in the United States have lower tuberculosis rates than inner city areas (Jones and Moon 1992).

The value of using different scales of analysis is easily demonstrated. If, for example, insight is sought into the overall picture of Mississippian health as a whole, then the main variables of concern would probably be maize-based subsistence, social stratification, and sedentism. At first glance, these factors suggest potential nutritional imbalances, an underprivileged class, high risk of contagion, and thus quite poor health for some. But this picture is too generalized to be very useful – as demonstrated through skeletal analyses, it wouldn't necessarily be accurate for subpopulations or particular time periods within Mississippian culture. If more specific insights are sought into the health of particular populations, more and more variables must be considered, simply because of the complexity of the phenomena being studied. Moreover, this 400 year span was highly dynamic culturally, encompassing the rise and fall of the Cahokian polity. Health may not have radically worsened, but disease patterns would hardly have been static.

A Diachronic Portrait of Health at Cahokia

Milner (1982) devoted considerable space to the consideration of archaeological indicators of health, as well as skeletal analysis, for populations of the American Bottom; however, his discussion is limited to subsistence, and social organization in the sense of its relevance to disease ecology (*i.e.*, population size and distribution). His primary concerns were opportunities for exposure to pathogens, and for their transmission. His analysis is added to here, in terms of inferences about responses to disease, based largely on data regarding Cahokian settlement patterns which were unavailable in 1982.

I will first consider factors which would have consistently affected health at Cahokia and the American Bottom throughout the Mississippian period, and then will discuss aspects of health which may have changed over the four centuries in question. In this context, the levels of the *longue durée* and *événement* can be considered together, because the *longue durée* includes **constant** influences common to each age, and *événements* would have been **occasional** influences also common to each age.

Throughout Cahokia's history, the landscape of the American Bottom would have exerted an effect on health. Simply put, much of it is marshy and wet, which means that insects are abundant; this in turn can translate into occasional exposure to vector-borne zoonoses, such as the mosquito-borne equine encephalitis viruses which have been known to affect, and sometimes kill, modern humans in the region (Horsfall 1962:214-227).[4] Also

[4] The various viruses which cause 'equine encephalitis,' it should be noted, have a natural reservoir in birds indigenous to the Mississippi River Valley; thus, they would have been present in the area before the introduction of horses (Horsfall 1962:216-218).

with respect to insects, contact with wild mammals and living in comparatively close contact in small dwellings allow for infestation with ectoparasites such as lice, fleas, and ticks, which can transmit infectious diseases, including a number of serious rickettsial diseases known in the central United States, and tularemia (Horsfall 1962:263-295).

Living in small dwellings also permits the effective spread of skin and respiratory infections. However, though the population density may have been quite high within the dwellings, the population density and size for the overall site was quite low, which means that acute infectious diseases, like smallpox, measles, or influenza, could not have regularly existed there. These diseases need a large pool of susceptibles to sustain themselves in a population (Fenner 1980), and so while they may have afflicted Cahokians on occasion, they would have had to be introduced from the outside. Such introductions almost certainly took place, given Cahokia's wide trade networks (Stoltman 1991). One should perhaps note here an aspect of *mentalité* – that diseases which have always been around are often regarded differently from those which are suddenly introduced from the outside. This can be seen in differing cultural responses to measles; as mentioned earlier, in Hong Kong, measles was traditionally regarded as a childhood stage of development, and was greeted with little concern (Lewis 1993:95), whereas the Yanomamo were literally scared to death by this unfamiliar disease (Neel 1982). Thus, the cultural response to unfamiliar infections in the American Bottom was probably consistently different from the response to endemic infections.

Although it is technically outside the sphere of archaeology, some mention here of ethnohistorically known responses of post-Mississippian groups to ill-health seem

appropriate. Although cultural responses to illness would surely not have been static, it seems reasonable to suppose that there would have been substantial continuity in this aspect of *mentalité*, and important to emphasize the non-passive nature of humanity's interactions with disease. In many post-Mississippian groups, disease was thought to arise from some failing in ritual separations between male and female, or different cosmic spheres, or from some failure to observe other necessary customs. For example, among the Southeastern Indians, disease was widely thought to derive from the improper killing of animals; for example, a vengeful deer spirit, angered at a hunter which did not ask its forgiveness for killing it, would cause arthritic pain in the trangressor's limbs (Hudson 1976:340). The well-developed Southeastern pharmacopoeia would have both mitigated risks of illness, through such practices as the use of bear oil on the skin as protection against insect bites, and treated illness, as in the consumption of a willow bark concoction as a treatment for arthritis. The latter is only one example of a Native remedy which involved an active ingredient used in modern Western medicines; in this case, the ingredient is salicin, which aspirin users recognize as an effective pain reliever (Hudson 1976:349). Many other herbal remedies were known to the historic Southeastern Indians, including treatments for worms and other intestinal parasites, and fever; at times, quarantine was even employed, although its stated purpose was to protect the patient rather than the well (Hudson 1976:340-351). Reactions to death in archaeological communities would of course have been complex to an extent that confounds our understanding; however, it is possible that compassionate euthanasia of the old and infirm,

and infanticide of neonates, were permitted, as they were among some post-Mississippian groups (Hudson 1976:231).

Trade networks, and thus inter-regional contact, appear to have been relatively constant throughout the Mississippian period in the American Bottom – they did not decline in concert with Cahokia's breakdown (Milner 1991b:38). Also, there was fairly consistent access to exotic materials for both elite and nonelite, suggesting that the material effects of social stratification at Cahokia may not have been very extreme.

Subsistence appears to have changed little at Cahokia over the centuries. The economy was based on hunting, fishing, collecting, and maize cultivation, activities which all have their own health implications. From cultivation as well as moundbuilding and plaza-levelling, there would be exposure to soil-borne pathogens like tetanus and blastomycosis, and from hunting there would be exposure to zoonotic diseases. It is likely that members of the elite who didn't participate in subsistence would have less likelihood of contracting such infections; and also, there may have been some divisions along gender lines according to division of labour. The reliance on maize allows for the possibility of food poisoning; Matossian (1989) notes that there are moulds which grow on corn in the United States which can produce serious episodes of mycotoxic poisoning, or more subtle effects such as immunosuppression. These effects would, notably, be felt more strongly among the young, who would consume more corn per unit of body weight than adults (Matossian 1989:12). This brings up the question of differential health conditions according to age and gender; in this regard, it seems probable that young children would have been relatively susceptible to ill health, both because of their tendency to stay in the

more contaminated environment immediately around the home and because of poor weaning diets (Milner 1982). It is also possible that child-bearing stresses exacted a disproportionate toll on young women, although this may have been matched in times of conflict by the loss of young men.

Although there were some shifts in house construction through the phases at Cahokia, they were all basically pole and thatch construction. Houses of this kind would be susceptible to insect and vermin infestation, which could also enhance exposure to infections, such as the hantaviruses now known to be transmitted by mice (Henig 1993). Insect infestation of structures and of fields could have been severe; it is known to have been a contributor to village relocation among Northern Iroquoian groups (Starna *et al.* 1984). Throughout the phases of Cahokia's existence, there would have been occasional catastrophes such as resource shortages, floods, crop failure, or as mentioned, the introduction of infection from the outside. The probability of the occurrence of the first three may have increased slightly over time. In particular, increases in water levels which probably occurred in the later phases at Cahokia may have contributed to crop failure.

This sums up the basic influences on disease on the scales of the *longue durée* and the *événement.* The next subject is the scale of the *conjoncture*, or factors affecting health which would have changed during the Mississippian period.

Large-scale labour projects in the Lohmann and Stirling phases, such as the building of mounds, and woodhenges, and land levelling to create plazas would have ensured close contact between many people, providing ample opportunity for spread of infectious diseases; this would have decreased over time as these large-scale projects

ceased. In the early phases, population movement to the bottomlands from the uplands would have resulted in increased exposure to various insects which can act as disease vectors, as well as a confluence of local disease pools, especially intestinal bacteria which can be regionally specific. During the Lohmann phase, indications of communal storage and cooking, and comparatively large domestic units suggest that there would have been regular close contact between fairly large numbers of people – this, added to the small house size seen in this phase, creates an ideal situation for disease spread, especially things like faecal-oral contamination, and droplet infection contagion. On the other hand, it suggests that there was a big support network, which would be a major advantage in times of illness. During the Stirling phase, a move to greater privacy and smaller domestic units would cut down on the number of people between whom there were intimate interactions, and thus would cut down on the opportunities for disease spread, but conversely, it may have meant a less effective support network during periods of ill health. The building of a stockade during this period, and its subsequent rebuilding, indicates social strife, but the implications of this for health are unclear.

During the Moorehead phase, from 1150 to 1250 A.D., the return to smaller subcommunities, and apparently decreasing political control, as well as the population movement away from Cahokia, suggest an overall decrease in social stress and opportunities for contagion. As the population size and community size dropped, in all likelihood, so would have the number of healing 'specialists'; although the relevant knowledge regarding herbal remedies would probably be preserved in the general population, there may have been a lessening of the beneficial ritual element in health care.

However, this elaborate social dimension may have been less needed in a context with fewer people and less structured relationships. It seems possible that this period of decline for the site of Cahokia may have been a period of better health for its occupants.

Finally, the people of the Sand Prairie phase – characterized as a squatter occupation in the shadow of Monks Mound (Holley *et al.* 1989) – would likely have faced few of the problems that come with great social complexity, as the population of this period was dispersed, small, and apparently enjoyed significant autonomy. By the end of the Sand Prairie phase, the dissolution of Cahokia was complete.

So, then, this preliminary analysis of health at Cahokia indicates that the overall trend may have been major risks for contagion in the Lohmann phase, declining somewhat in the Stirling and Moorehead phases; on the other hand, increasing insularity and decreasing resource sharing in the Stirling phase may signal a less extensive social support network, which could have had negative consequences in times of illness or epidemic. The Moorehead phase's decreasing population and political control may have resulted in improved health for the local inhabitants. However, it seems that these periods would likely have had more in common than not with regard to health, given the strong continuity in disease influences at the scale of the *longue durée*.

Probably as important here as variation through time is variation across space. In Cahokia proper, structures were rebuilt in same areas over and over unlike rural areas where the duration of occupation rarely exceeded a few years. This shorter duration of occupation at farmsteads, and low local population, would mean patterns of refuse

deposition which would pose less risk of contamination of soil, water, and food storage as compared to villages, and thus, in general, less exposure to infectious disease from these sources as well as from other people (Milner 1982:248; 1983c). But this has a temporal dimension too, in that during the Lohmann and Stirling phases, at Cahokia's height, there were fewer isolated farmsteads than there were either earlier or later.

Summary

No-one alive knows what life was really like at Cahokia, but as Hall observed in the quotation at the beginning of this chapter, this does not prevent us from having the audacity to discuss it. Lively debates continue regarding many aspects of Cahokian society, such as population size, models of inter-site organization, the nature of elite power, the possibility of occupational specialization, and the cause of its decline. Threads of modern concerns are inevitably interwoven throughout these discussions; I have argued that avoidance of these complications is simplest if minimalist interpretations of data are adopted. A comparatively simple view of Cahokian society, in conjunction with a consideration of specific archaeological settlement data, yielded an interpretation of Cahokian health which is basically consistent with existing interpretations of health in the American Bottom, based on disease ecology and osteology (Milner 1982). However, an extra dimension was added through the consideration of household-scale social behaviour

and active responses to disease. With regard to the larger issue of Mississippian health, this contributes to the view, growing in popularity (Buikstra and Milner 1989), that Mississippian health was diverse. However, it should also be noted that the theories of Goodman and colleagues – that the apparent health problems seen in the Dickson Mounds populations were caused by Cahokian exploitation – have been called into question by examination of different theories of Cahokian society; the idea of Cahokia as exploiting the remote hinterlands does not stand up well to close scrutiny. It is supported primarily by dubious assumptions regarding Cahokian population size, and superimposes theory about modern international political and economic relationships onto prehistory.

In conclusion, then, the use of archaeological settlement data provides access to social information which is both unavailable through skeletal analysis and crucial to understanding infectious disease patterning in prehistory. The challenge of organizing the data was met quite effectively in this brief, local-level analysis of health by a framework based on aspects of scale theory and the Braudelian paradigm of different scales of historical time. Preliminary explorations of Cahokian health using this framework support the idea that the sun set quietly upon Cahokia, rather than the notion of an apocalyptic decline due in part to drastically worsening health.

CHAPTER VI

CONCLUSIONS

This thesis has consisted of an exploration of the study of prehistoric health, as much as of prehistoric health itself. My particular focus has been on the place of archaeology in this endeavour.

Chapter 2 served to outline some of the shortcomings, both practical and theoretical, of usual approaches to the study of past health. Practical shortcomings include problems of osteological analysis, such as age and sex determination, and the identification of pathology; as with any field, there are practical limitations to what can be done. There are also more significant problems in interpreting osteoarchaeological data, with respect to the meaning of observed conditions and mortality profiles for individual and population health. These have been ably reviewed in recent years by authors such as Wood *et al.* (1992), Jackes (1992) and Ortner (1991); however, a subject which has received little attention in the recent spate of osteoarchaeological introspection is the use of archaeological data in the study of prehistoric health.

Archaeological data have been used in a number of capacities in studies of prehistoric health, usually in close relation to osteological data – as background information for skeletal analyses, as generators of hypotheses to be tested by osteological

data, or as testers of hypotheses generated by osteological data. More occasionally, archaeological data are used independently, as bases for substantive inferences about aspects of past health (these are later integrated with other interpretations, usually from osteology). My primary interest has been in the latter category of use; it is especially interesting to note the boundaries of the archaeological data used in this way. In local-level analyses of health, they have been used to supply basic ecological information (*e.g.*, garbage disposal, and population size and density as related to infection transmission) and information regarding social stratification within communities, as well as political inequality between communities. These approaches have been useful in rounding out multi-faceted studies of past health, but have often been problematic where the archaeological evidence is ambiguous about the extent and exact nature of stratification (as is the case for Mississippian societies) and unclear when it comes to the nature of the regional political system (*e.g.*, theories about the exploitation of the Dickson Mounds people). In broad-scale analyses of health, archaeological data have been used almost exclusively to provide information pertinent to disease ecology. In both small-scale and broad-scale analyses, inferences about social responses to disease and relevant household-level behaviour such as the sharing of food and subsistence duties have been conspicuously absent. This is a problem, given that our current understanding of the web of causation of disease gives great weight to social behaviour as an etiological and mitigating factor.

Thus, archaeological data could be used more fully in studies of past health. However, there are epistemological and methodological barriers which could obstruct the

realization of its potential. For example, the conception of archaeological data as providing less direct information about past health than osteological data is prevalent among osteoarchaeologists, though perhaps on a less than fully conscious level. This is clearly misleading, since neither kind of data can provide 'direct' information of any kind, but must be interpreted to have meaning – and, as has been shown, interpretation of osteological indicators is not as straightforward as was once supposed (Stuart-Macadam 1991; Wood *et al.* 1992; Ortner 1991). A second potential barrier is the fact that whenever methodology is explicitly discussed in publications on prehistoric health, hypothesis-testing is advocated as the only key to understanding (*e.g.*, Rothschild 1992; Buikstra 1991). However, the hypothetico-deductive method is merely one way of conducting scientific inquiry, and is as subject to criticism as any other method (Salmon 1982). A final barrier to the use of archaeological data in studies of past health is the problem of integration of different scales of data. Local-level studies rarely achieve much integration, instead employing a patch-work technique of presenting their interpretations, whereas broad-scale analyses achieve integration through the use of an evolutionary framework, which is unsatisfactory because of its typological tendencies and because it entails the omission of many kinds of relevant social information.

Once these potential impediments to the use of archaeological data in the study of prehistoric health were identified, I looked to archaeological theory and the broader philosophy of science for alternative ways of conceiving of the task at hand. My first concern was with the methodology advocated by some researchers as the only way to study past health. The shortcomings of hypothesis-testing methodology have been much-

discussed, but furthermore, it is clear that not all research into prehistoric health aspires to this format; quite often, untestable speculations are unconcernedly offered about past health conditions. Perhaps even more to the point, much of the time, the data available simply cannot support a highly rigorous analysis.[1] Clearly, then, theory about method in studies of past health does not match up to practice in this regard. My goal in the first section of Chapter 3 was to show that there are alternatives to hypothesis-testing, and that although the consequences of the rejection of a purely positivist and falsificationist orientation can be intimidating, they needn't be. The methodological plurality which exists in the study of past health is positive and necessary, and discussion of the philosophical bases of different scientific methods can only help in their development.

My second concern in Chapter 3 was to provide an overview of some bases in epidemiological theory for making interpretations about past health from archaeological data. The importance of social factors in disease causation and expression has frequently been written about in anthropological and osteoarchaeological literature on health, although as mentioned above, little has been said about the effects of household-level social behaviour on disease in works on past health. Archaeological settlement data can provide some basis for such inferences, however. The final section of Chapter 3 dealt with the problem of replacing the cultural evolutionary scheme as the main integrative structure

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[1] For example, Buikstra (1991:188) writes that bioarchaeology should aspire to being a discipline where "sophisticated models are developed, expectations are formally derived, and tests are statistically rigorous"; yet, in the very next sentence, she notes that "the major limiting factor would seem to be sample size and the quality of contextual data." This is a major factor indeed, and would seem to indicate that there is a fundamental incompatibility much of the time between the data available and the method advocated.

in studies of past health; the Braudelian model of historical time holds promise in this regard, for it provides a strong structure based on historical observation which can be used in local-level analyses, which have hitherto lacked substantial integration.

The second part of the thesis, Chapters 4 and 5, consisted of an effort to implement some of these strategies in an examination of health in a test case, that of the Mississippian society of Cahokia. The first important realization demonstrated through this example is that in any attempt to use archaeological data as a basis for interpretations of past health, a thorough familiarity with the interpretive debates, and their larger contexts, is necessary. For example, a reliance on the fourth-generation researchers' view of Cahokia, as a very populous, highly complex, rigidly stratified, expansionist entity whose leaders had absolute power over life and death, would produce a very different view of Cahokian health than the one given in Chapter 5, based on a less complex model of Cahokian society. In terms of context, the concatenation of the legacy of the Moundbuilder theorists, a revival of cultural evolutionism, and a growing interest in the rights of surviving Aboriginal groups probably influenced the fourth-generation theorists' views, whereas the fifth-generation scholars operate more within the post-processual ideal of particularistic research, while simultaneously reacting to the work of those before them. In Mississippian osteoarchaeological research, an explicit relationship of the osteological results to theories about culture change, the connection of humanity to the environment, and the effects of political relationships upon health, has surely affected the interpretations given. Similarly, Cahokia represents more than just an archaeological site to the people who work on it; to some, it is not only an awe-inspiring remnant of a little-understood

people, but the pinnacle of achievement of Native North American culture (O'Brien 1991). Views about the health of Cahokia and other Mississippian populations have an impact far beyond the spheres of Mississippian research or North American archaeology.

The archaeological picture I chose regarding Cahokian society is fairly minimalist. The evidence does not yet compel belief in despotic elite power, a rigidly structured political hierarchy, a rapidly degenerating environment, or a population in the tens of thousands; thus, it seems best at present to allow for those possibilities, but not assume their existence. The implications for health are several, and again, far-reaching. For Cahokia itself, the interpretation of relatively good and stable population health corresponds with the results of osteoarchaeological analyses of American Bottom populations, and with the theory that Cahokia's decline was not due to a drastic worsening of health conditions. The picture chosen of Cahokian society also implies, however, that the apparent poor health seen in the Dickson Mounds population cannot be explained away as the result of exploitation by the Cahokian polity, and suggests that either the interpretations of the osteological data should be reconsidered, or that other factors are responsible for the patterns observed.

A strictly archaeology-based approach to health at Cahokia met with some success. The inferences drawn about health in Cahokian society are largely non-testable; however, as discussed in Chapter 3, this does not mean that they have no basis, or cannot be evaluated. They conform to modern epidemiological knowledge about disease processes, they correspond with the data, and they make sense within the larger body of theory about Cahokia which I have chosen as most intelligible. However, they make no claim to being

correct, but only to being a possibility which is worthy of consideration in the debates about Mississippian health, and the health of populations in early complex agricultural societies. The Braudelian model of layered time proved to be quite useful in structuring the interpretations of Cahokian health; existing knowledge about the Cahokian polity fell quite naturally into the time frames specified by the model.

Clearly, an archaeology-based approach to the study of past health can be used only in areas where there is substantial settlement data, and ideally, a reasonably fine-grained chronology. Further, it is enhanced when there is ethnographic information available to flesh out interpretations of attitudes towards disease, and cultural responses to it. These are significant restrictions, but not impossible to meet.

An archaeological approach to the study of past health can contribute valuable insights into the lives of our forebears. However, as important as the study of past health is, no less important is the study of that study, for it brings its own insights.

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