Finding the "Early Medieval" in South Asian Archaeology

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JASON D. HAWKES

INTRODUCTION

THE "EARLY" OR "PREMEDIEVAL" PERIOD exists as a well-established, if poorly defined, period in the study of South Asia's past. Broadly accepted to have extended from around the seventh to thirteenth century C.E., the term "early medieval" has emerged in scholarship to define a particular phase of social and cultural development that mark it as being broadly distinguishable from the earlier ancient period that came before. Developments held to define the beginning of the early medieval include: the emergence of new political structures in both North and South India, a reorientation of exchange networks and urbanism across the subcontinent, and the crystallization of distinct regional cultures and identities manifest in the appearance of diverse litera- tures and arts. Yet, because these developments did not occur in the same way, or in- deed at the same time throughout the subcontinent, and with little consensus as to what marks the end of this early phase of the medieval and the start of the later medieval period that follows, the concept of the early medieval remains problematic (Singh 2011). Questions exist as to whether it should be deemed a definable period in its own right, central to which are wider questions about the meaning and validity of the term medieval in South Asian history.

These issues have been the subject of much discussion. Yet, what is even more important, albeit rarely discussed, are the ways in which the early medieval is studied almost exclusively through documentary sources (involving texts and inscriptions) and monumental remains. These are examined within the fields of documentary history, literary and religious studies, and architectural and art history. At best, archaeological evidence tends to be used to provide additional supportive evidence for interpreta- tions and conclusions defined on the basis of the study of documentary sources, or simply to provide the objects of study for art historical and architectural analyses. This practice is not limited to the study of the early medieval—it also characterizes the study of the later medieval and other historical periods. Nor is it a trend unique to South Asia; it is well attested elsewhere, with much having been written about the relationship between history and archaeology vis-à-vis the relative importance of

Jason D. Hawkes is a Project Curator in the Department of Asia, The British Museum, United Kingdom.

archaeological evidence (Andrén 1998; Chakrabarti 2003; Moreland 2001; Ray and Sinopoli 2004; Trautman and Sinopoli 2002). Yet, in the context of the study of the early medieval in South Asia, the situation has more serious implications. Here, the problem is not primarily that of archaeology being a handmaid to history; rather, it is the case that the early medieval tends not to be studied archaeologically at all, and is arguably the most poorly represented period archaeologically in the entire subcontinent. From the beginnings of archaeological research in South Asia, less than 200 sites dating to the early medieval period have been excavated. Notwithstanding concerns surrounding the definition of the early medieval, this paucity of archaeological research and the scarcity of archaeological evidence poses serious epistemological problems for knowledge and understanding of some six or seven hundred years of Indian history. It is possible to argue that the lack of archaeological research is an important contributing cause for the uncertainty and vagueness surrounding the definition and meaning of the period.

This article explores how and why this situation has come about, and reviews the current state and practical limitations of archaeological research on the early medieval. Attention then turns to the impact that a relative absence of archaeological research has on our knowledge and understanding of the period, before considering how this situation might be addressed.

FINDING THE EARLY MEDIEVAL: A HISTORIOGRAPHICAL SURVEY

Early Definitions of the Indian Medieval

The current paucity of archaeological research on the early medieval in scholarship on South Asia is rooted in the ways in which the idea of a medieval period originated and has been studied in India.[1] The concept of medieval India can be traced to the tripartite periodization established by the British historian James Mill (1817), who divided the history of India into three civilizational ages: Hindu, Muslim, and British. This division of the past was in keeping with what were then the emerging ways of thinking about history in European scholarship; and quickly became conflated with the (Eurocentric) terms ancient, medieval, and modern.[2] Yet, in defining these periods on the basis of the invasion and rule of foreign powers, first Muslim and then British, it is clear that an evolutionary perspective legitimizing colonial rule was being established—implicitly, a history thus defined demonstrated the inability of the Indian people to rule themselves. Throughout the nineteenth century, the notion that Indian society remained unchanged unless influenced by external forces continued and was rearticulated in European social and economic philosophies. Here, the works of Hegel (see Gans 1837), for whom Indian society revolved around immutable villages that were unconcerned with political relationships and thus prone to autocratic rule and foreign invasion, were particularly influential. Such assumptions nurtured existing ideas of Oriental Despotism,[3] itself influential in the formulation of later Marxist theories of the Asiatic Mode of Production (Marx 1859)—concepts that fed directly back into historical interpretations of the past. These ideas perpetuated the existing periodization, and ensured that both the medieval period and the centuries leading up to it were viewed in terms of decline, while the earlier (classical) ancient period was deemed more deserving of serious scholarly attention.

Given the limited scholarly interest in the medieval, it is perhaps unsurprising that it was not widely studied archaeologically. From the seventeenth and eighteenth

centuries, archaeology had emerged as a pursuit whose sole aim was to provide evidence for historical studies that were defined on the basis of textual sources: literary and religious texts, and inscriptions.[4] As such, the development of the discipline throughout the nineteenth century was subject to the same wider colonial agendas that directed the course of historical research. Archaeological activities focused on periods that were deemed "important." By and large, the monuments and remains of the ancient period dominated archaeological work. It was not simply a passive relationship, however: archaeological investigations of the ancient period also played an important role in formulating guiding ideological agendas. Thus, during the early nineteenth century a series of excavations and the decipherment of many hitherto un-translated inscriptions resulted in the archaeological discovery of ancient Buddhism (Hawkes and Shimada 2009; Singh 2004). The philosophical teachings of Buddhism, revealed textually, were already finding great appeal among contemporary European Romantics of the time.[5] Yet, with its archaeological discovery, Buddhism was identified as having existed during the centuries roughly contemporary with the European classical period.[6] This not only provided a connection, and thus academic justification, for the study of this particular period, but more importantly also provided proof of a non-Hindu ancient period, undermining notions of a noncolonial identity based on a Hindu past.[7] Together, this placed the study of ancient Buddhism firmly at the front of colonial historical and archaeological agendas, at the expense of the study of the medieval.

These scholarly trends continued throughout the later nineteenth and early twentieth centuries, albeit in a different way. Many attitudes were challenged during the early twentieth century, when a new wave of nationalist history changed the course of the writing of Indian history. A number of Indian scholars effectively questioned many of the notions inherent in colonial scholarship, rejecting the idea that civilizational changes could only be explained by invasion, and shifted the agenda to relocate the "Indian-ness" in India's past (Jayaswal 1918; Majumdar 1925; Mookerjee 1926; Ojha 1925–1941; Raychaudhuri 1923). Yet, despite this change in emphasis, Mill's tripartite periodization came to be further cemented as the defining framework on which history was constructed. In seeking a precolonial Indian past, the distinction between the Hindu ancient and Muslim medieval became ever more embedded. Put briefly, the medieval came to be identified with the foundation of the Delhi Sultanate in Northwest India in 1206 C.E., and the premedieval came to be located in the pre-Muslim past, identified as the "Golden Age" of India. As a result, the medieval and the centuries that led up to the Islamic conquests were further entrenched as a cultural equivalent to the European Dark Age, and the period continued to be ignored in archaeological research. Instead, archaeologists continued to focus on earlier ancient periods, the study of which had received additional impetus following the discovery of the Indus Valley civilization during the early decades of the twentieth century (Marshall 1923, 1931).

Post-Independence Developments and the Feudal Model

It was only after Independence in 1947 that the traditional periodization and perception of the medieval began to be questioned. Throughout the 1950s and 1960s, in the context of a postwar climate of revisionism that affected the study of history internationally, approaches to the study of India's past became less concerned with narratives

of past events, and focused instead on the nature of societal change.[8] These shifts in emphasis brought about calls for the concept of the Indian medieval to be clarified and understood, not just with reference to political history, but also with reference to social and economic changes (Ray 1967; Sharma 1974b; Thapar 1968). It was recognized that the earliest patterns of periodization (Hindu-Muslim-British) gave undue importance to ruling dynasties and foreign invasions (Kosambi 1956), and the use of different terms—ancient, medieval, and modern—did not clarify understanding, as the basis for the division remained unchanged (Thapar 1968; Thapar et al. 1969). An alternative model of transition from ancient to medieval was first suggested by Marxist historians, who identified important changes in state, society, and economics during the mid-first millennium C.E. on the basis of inscriptions recording grants of land by kings to *brahmanas* (members of the priestly caste) and their political subordinates (Kosambi 1956; Sharma 1965). This practice apparently commenced during the reign of the Guptas (c. fourth to sixth century C.E.), and gathered currency in the centuries that followed. Importantly, these inscriptions also recorded the conferral of revenue rights with the land grants. Drawing heavily from the works of European Marxist historians, such as Bloch (1939) and Duby (1952), these factors were interpreted as having contributed to a gradual decentralization of socioeconomic and political power, and to the creation of a more hierarchical feudal society.[9]

Important here, at least as far as the concept and study of the medieval is concerned, was that societal and economic systems during post-Gupta centuries were considered to be markedly different from those that came before. Seen in this light, the emergence of feudal society was deemed to be a more meaningful start to the medieval than Islamic conquests. This had two consequences: it pushed the start of the medieval back to long before the foundation of the Delhi Sultanate, giving rise to what later came to be known as the early medieval period; and with this, these centuries, which had always been relatively ignored, became a major focus of research.

In its inception, however, the study of what came to be defined as the early medieval period was the product of textual history; the reinvigoration of the study of the period was not accompanied by archaeological research. This is a situation in marked contrast to that in Europe. There, a similar socialization of history during the 1950s and 1960s, accompanied by a democratization of society after the Second World War (Judt 2005) and the increasing importance of the medieval period in the construction of European nationalist identities, inspired a drive to excavate more sites in order to find the subjugated people of feudal society—people that were missing from both the earlier orthodox histories and the objects of historical study: the documentary sources (Gilchrist and Reynolds 2009).[10] In part, the fact that this did not happen in India can be explained by the way in which archaeology developed as a discipline. Independence saw considerable resurgence of archaeological activity and formulation of the discipline as an empirical science, which led to an increased realization in both the archaeological community and historical scholarship of the value of archaeological evidence as something that could give "proof" of past realities. Yet, in the main, archaeological investigations continued to focus on periods that were deemed important due to other ideological factors. In post-Independence India, the construction of new national identities placed even greater importance on the study of the earlier Mauryan and Gupta periods.[11] At the same time prehistory began to assume considerable importance within archaeological research, as it was realized that the investigation

of prehistoric sites could fill the gaps of Indian history as part of a drive to write the total history of the newly independent nation (Chakrabarti 2003). In this context, the early medieval was not prioritized by archaeologists and remained largely outside the remit of their research, the exception being the conservation of temple monuments as part of the Archaeological Survey of India's mission to preserve the nation's cultural heritage.

From the 1960s to the 1980s, historical theories of feudalism continued to evolve and undergo refinement, not least in terms of comparisons with similar ideas of feudalism in medieval Europe.[12] Notwithstanding these caveats and refinements, the feudal model explained that the beneficiaries of land grants (usually members of the priestly caste and temple institutions), became landed intermediaries able to function outside royal control (e.g., Sharma 1965, 1972, 1982, 1987). On the one hand, this was deemed to have caused the increasing subjugation of the indentured peasantry through restrictions on their mobility and freedom as they were forced to perform labor to pay taxes in the form of revenue. On the other hand, the proliferation of these increasingly powerful intermediaries was argued to have gradually eroded centralized political control, leading to the fragmentation of the state. Crucially, archaeological evidence was cited that *seemed* to indicate a decline in foreign trade as well as economic activity in, and habitation of, urban centers throughout the subcontinent during the mid-first millennium C.E. (Sharma 1972, 1987). Excavated remains from a large number of urban sites appeared to show quantitatively less archaeological material from occupational layers dating to these centuries, which was interpreted as evidence for a decline in urbanism. Drawing on the works of Pirenne (1927, 1936), this urban decay was explained as a consequence of a cessation in international trade, and identified as having led to the growth of a self-sufficient economy, a factor further supported by the perception that metallic currency became scarce during this time (Sharma 1969). This construct, based on archaeological evidence (or rather its absence), became central to feudal models, as it seemed to explain *why* the practice of granting land and land revenue had started.[13] With a decline in urbanism, it seemed to follow that all payments by the state had to be made through the giving of land or its revenues. By these means, feudal society became the mechanism by which the means of production were distributed and surplus appropriated in what had become, by the mid-first millennium, a predominantly agrarian society. These (supposedly) universal economic theories were applied to other aspects of society.[14] Indeed, so all-encompassing were these theories that feudalism became the dominant model that both defined and explained the emerging early medieval period.

While it might appear that the construction of feudal models was accompanied by archaeological research, examination of this aspect of the scholarship demonstrates that this was not the case. Rather, the material being invoked was such as had come from the upper layers of excavations at sites with much earlier foundations. The guiding focus of archaeological investigation, which had supplied this evidence, was the earlier phases of occupation. This governed the choice of which sites, and which areas within those sites, to excavate—those deemed to have the deepest (i.e., earliest) deposits of cultural material—neither of which were necessarily representative of patterns of early medieval settlement in general, or the phases of early medieval occupation at these sites in specific. Yet, it was precisely this material that was assumed, somewhat uncritically, to be illustrative of the early medieval period, and used as supporting evidence to answer historical questions, which, given the interests and main focus of

Marxist scholarship were primarily concerned with socioeconomic dynamics. In this way, a text-based focus was maintained, inasmuch as Marxist models did not feed into the stimulation of new archaeological fieldwork, but simply digested the existing archaeological evidence.

This had serious consequences for the continued lack of development of archaeological research on the early medieval period. On the one hand, it effectively rendered it subservient to history. By granting it only a supporting role with a specific (limited) remit—to provide evidence for a very limited range of economic aspects (urbanism, trade, and craft production)—the notion was established that archaeology was only useful as a tool to answer questions defined in historical scholarship. This tendency was exacerbated by the fact that when archaeology emerged as an academic discipline in Indian universities in the post-Independence era, it tended to do so within departments of history, and was considered, primarily, a historical discipline (Chakrabarti 1999:17; 2003). On the other hand, with historians assuming the voice of authority in the study of the period, and using archaeological evidence to paint the picture of urban decay, a perception that there was nothing to study from this period seems to have become firmly entrenched amongst archaeologists. If there were no cities or towns and trade, the subjects that archaeologists *could* look at and comment on in the earlier historic period, then clearly there was nothing for archaeologists to find, and so there was little motivation to develop the archaeological study of the period.

The "Feudal Debate"

However dominant the idea of a feudal society might have been, it was not universally accepted; and throughout the later twentieth century both the emerging concept of the early medieval and the ways in which it was studied continued to develop and inform each other. While not disputing that major societal change had occurred, theories of feudalism were criticized for too easily conflating the Indian and European historical experiences, without sufficient consideration of the Indian sources (Mukhia 1981). It was noted that many inscriptions provided evidence of villagers having some control over the means of agricultural production, which brought into question the extent to which they were controlled by either donors or clients, and could thus be equated with serfs (Mukhia 1981). It was further pointed out that there was not necessarily a decline in international trade (Gopal 1965), and that fluctuations in trade did not necessarily explain a decline in urbanism (Chattopadhyaya 1974).[15] Other evidence suggested that an abandonment of some earlier centers was accompanied by the foundation and expansion of new, albeit different, towns and cities that appeared to have been more rooted in their regional contexts and local exchange networks (Chattopadhyaya 1986; Hall 1980). Indeed, place-names identified in inscriptions and texts seemed to demonstrate that the number of urban centers may have increased in some areas (Chattopadhyaya 1986), undermining the notion that the landscape (and economy) was dominated by self-sufficient villages. Furthermore, the presumed decline in monetary economy was questioned by alternative interpretations of the numismatic evidence, which demonstrated that quantities of coins were in circulation and monetary transactions may have remained comparable with those witnessed in other periods (Deyell 1990). Overall, it was generally accepted that the feudal model was too reductionist, and ignored much of the geographical variability that marked the subcontinent.[16]

Together these criticisms theoretically and textually undermined many of the premises of the feudal model. Yet they did not necessarily change the way in which the period was studied. Archaeological evidence, while still invoked as supporting evidence for some of these critiques, was used to a lesser extent than it had been in feudal scholarship. It did not really feature other than to highlight numismatic evidence for a monetary economy, or to demonstrate the existence of urban centers in certain places during particular centuries. The continued use of archaeology in this way confirmed the notion that its main value vis-à-vis the historical study of the period was as a means of providing supporting evidence to the primary means of enquiry: textual history. This served to further inhibit any motivation for archaeologists themselves to investigate the period. Reflexively, by not expanding the use of archaeological evidence in historical research, and because the period was not studied by archaeologists, the range of archaeological evidence and questions that could have been posed were not brought to bear. For instance, in questioning the decline in urbanism and the economic transformations that it was deemed to represent, no attempt was made to re-excavate those urban sites that appeared to have declined in order to uncover the causes of the apparent decline. Nor was any attempt made to excavate the new settlements considered to have emerged during this period. Arguably because of this neglect, the debate about the feudal nature of society lasted for over three decades without positions moving clearly forward. Studies based on texts alone could not provide convincing alternative explanations for the major societal transformations that were agreed to have taken place.

Indeed, it was not until historical scholarship turned to anthropological theories in the 1980s that alternative models were found. The first of these was the theory of a "Segmentary State" (Stein 1969, 1980), inspired by studies of acephelous societies in modern Africa.[17] Yet, the most dominant and convincing alternative theories proposed an integrative or processual model (Chattopadhyaya 1994; Kulke 1995*a*). They argued that the socioeconomic and political changes marking the early centuries C.E. were not regressive; rather, they could be considered as the positive result of continuous processes of development (Chattopadhyaya 1994). The most important of these was that of "integrative state formation" involving the interactive development of chiefdoms, early kingdoms, and imperial kingdoms, in which integration worked simultaneously at multiple levels (political, administrative, ritual, and cultural), with religious legitimization of power and tradition being the important link between them (Kulke 1993). In addition, other integrative processes, such as increased social complexity and the development of religious cults, were identified as taking place in different ways at different times across the country (Chattopadhyaya 1994). Interpreted in this way, sociopolitical, religious, and cultural domains were more effectively combined than they had been in the feudal models, and made greater allowance for regional variations. The transition from ancient to medieval—the early medieval—was thus characterized, not as a phase of social upheaval and political fragmentation, but as one marked by the development of new social phenomena and the proliferation of states.[18]

As a result of such critiques, and the widespread acceptance of alternative models to explain the emergence of the medieval period, the debate regarding the feudal nature of society is now largely played out in historical scholarship. Scholars are no longer primarily concerned with debating this aspect of state and society.[19] Rather, attention has turned to examining the developments that occurred during the period,

which has come to be defined according to a common set of criteria that distinguishes it from what came before.[20] Politically, it is seen as witnessing the reorientation of geopolitical orbits in North India, and the emergence of new states in the western Deccan and southeast (Kulke and Rothermund 1986), both of which are accompanied by the appearance of various new political structures in different regions. Closely related are significant socioeconomic developments, involving agrarian expansion and a reorientation of urban networks. Within these, craft production and trade are regarded as becoming more rooted in their regional contexts and local exchange networks. A vital component in all of these developments is the growth of religious institutions (Buddhist, Hindu, and Jain), all closely linked to the state and developments in agriculture, trade, and urbanism—their growth being manifest in large temple structures. Throughout the period, these dynamics are marked by a high degree of regional cultural variation, manifest in the proliferation of appearance and development of regional styles in literature, languages, and artistic traditions. Importantly, the concept of the early medieval is now regarded as having "emerged after sustained consideration of actual social, economic and political developments" (Ali 2012 : 10), rather than being simply transposed on to Indian history.

Yet, for all that there is now a generally accepted concept of the early medieval, the fact that the feudal debate dominated historical scholarship for so long has had a number of consequences on the way in which the early medieval is studied. First, it has resulted in a disjuncture in scholarship on the medieval between studies of the early medieval and studies of the later historical phase, still commonly referred to as "the medieval." So all-encompassing was the feudal debate that the early medieval is not only defined as a separate phase of the medieval, but it has come to be defined within a body of scholarship that has developed in a different trajectory from that which has continued to look at the later medieval. Both have drawn on different bodies of literature and have different agendas focusing on different subjects: studies of the early medieval focus on the transition from the ancient to the modern, while studies of the later medieval increasingly look toward the transition from the medieval to the modern. The fact that these two phases of the medieval have been identified is not in itself a problem; chronological labels are inescapable, but are also useful. Yet, the fact that these two periods have developed as, and remain separate branches of, scholarship *is* problematic because it means there is little discourse between them. By focusing on opposite ends of the chronological spectrum, we remain ignorant of how the features deemed to characterize early medieval society might appear and change during later centuries. Equally, the origins of many of the trends identified during the later medieval are understood only in general terms. This highlights a fundamental lack of understanding of certain aspects of both the early and later phases of the medieval, and means that many questions still remain regarding the basis for the chronological divide between the two—there is little or no consensus as to what should define it.[21]

Second, the dominance of the feudalist debate can be said to have caused historical scholarship of the period to stagnate, focusing only on state and society with an overriding interest in economics. Much as a focus on socio-economic developments by Marxist historians (for whom there was little room for the study of other dynamics and "elitist" cultural phenomena such as art and literature) was the lens through which the early medieval was first identified, the fact that these same dimensions became the subject of a debate that obsessed scholarship for so long has meant that the elucidation of the state, social hierarchies, and economics became entrenched as the main goal of

historical research. As a result, other aspects of the past, such as religious beliefs, have been comparatively neglected; when they are considered, it is in terms of their socio-economic dimensions, with topics such as patronage and legitimation being of most interest. Consequently, there is a myopic historical understanding of the early medieval with reference to socio-economic change.

Concomitantly so, and indeed the factor underpinning these lacunae, is that other topics peripheral to the main thrust of historical scholarship (religion, culture, the arts, and historical questions pertaining to more social aspects of the past) have been confirmed as the proper foci of research in other disciplines: literary and religious studies, architectural and art history. Within these contexts, such topics are examined in ways specific to these disciplines, and are rarely compared and integrated with findings from other disciplines. On the one hand, this means that all aspects of the early medieval are understood only one-dimensionally. More importantly, this has unequivocally affected archaeology. The study of the period having been defined in the way it has and its continued pursuit by the same means (with different aspects of the past being studied by different disciplines) mean that archaeology has become deeply entrenched as only being able to supply supporting evidence, which is only considered of any value within history—its use in other disciplines being limited to providing material remains for architectural, epigraphic, or sculptural analyses. As a result, the range of potential evidence and other questions that archaeology could bring to bear and the theoretical approaches that it can pursue continue to be ignored. With the limited avenue of enquiry open to it, there is no impetus for new archaeological research, and so the period has continued to be ignored by archaeological practitioners.

THE CURRENT STATE OF ARCHAEOLOGICAL RESEARCH
ON THE EARLY MEDIEVAL
An Absence of Archaeological Enquiry

Against this historiographical background, it is clear that the early medieval period does not tend to be studied archaeologically, and that wider archaeological research continues to be geared toward the investigation of earlier and (to a lesser extent) later periods. The examinations of Buddhism and urbanism in the earlier historic period, as well as the Iron Age, Indus Valley, and prehistory, constitute the main foci of archaeological research on the periods preceding the early medieval. Relatively isolated studies of the later medieval period in North India and Vijayanagara in South India define the archaeological study of later periods.[22] Archaeological evidence dating to the early medieval period does, of course, exist; a significant number of temples, as well as settlements with early medieval phases of occupation, have been located and excavated throughout the subcontinent. Yet, the mere existence of this material does not itself represent or reflect a coherent strategy of archaeological research on the period.

Approximately 85 religious sites dating to the early medieval period (including monasteries, rock-cut caves, stupas, and temples) have been excavated since archaeological scholarship emerged in the nineteenth century, and the existence of many more has been recorded through explorations and surveys (Table 1).[23] Yet analysis of the remains from these sites and the wider interpretations of the sites themselves tend to fall under the purview of art and architectural history and epigraphic studies. Thus,

TABLE 1. List of Excavated Religious Sites Dating to the Early Medieval Period in India

SITE	STATE	EARLY MEDIEVAL DATES (WHERE RECORDED)	INSTITUTION	REFERENCE
Alampur	Andhra Pradesh	6th to 13th centuries	Archaeological Survey of India	*IAR* 1986–1987, 11
Ambari	Assam	7th to 13th centuries	Gauhati University, Assam State Department	*IAR* 1987–1988, 8; 1988–1989, 6; 1989–1990, 8–9; 1995–1996, 4–5; 1997–1998, 10
Antichak	Bihar	9th to 13th centuries	Bihar State Department, Archaeological Survey of India	*IAR* 1960–1961, 3; 1961–1962, 3; 1962–1963, 3; 1963–1964, 5; 1964–1965, 5; 1965–1966, 6; 1966–1967, 6; 1968–1969, 4; 1972–1973, 3
Ayodhya	Uttar Pradesh	7th to 10th centuries	Banaras Hindu University	*IAR* 1969–1970, 41; 1979–1980, 77
Bakraur	Bihar	8th to 10th centuries	Archaeological Survey of India	*IAR* 1973–1974, 9–10
Balikeshwara	Madhya Pradesh	Precise dates not recorded	Archaeological Survey of India	*IAR* 1987–1988, 59–61
Ballal Dhipi	West Bengal	10th to 12th centuries	Archaeological Survey of India	*IAR* 1983–1984, 94
Bharat Mandir	Uttar Pradesh	8th to 12th centuries	Garhwal University	*IAR* 1982–1983, 96
Birchhabili Tila	Uttar Pradesh	9th to 12th centuries	Archaeological Survey of India	*IAR* 1999–2000, 157–169
Bisokhar	Uttar Pradesh	9th century	Archaeological Survey of India	*IAR* 1998–1999, 177–185
Burogaon	Uttar Pradesh	10th century	Archaeological Survey of India	*IAR* 1965–1966, 52–53
Chandimao	Bihar	8th to 10th centuries	Archaeological Survey of India	*IAR* 2000–2001, 8–18
Chandel	Madhya Pradesh	Precise dates not recorded	Archaeological Survey of India	*IAR* 1987–1988, 61–69
Chandor	Goa	7th to 11th centuries	Archaeological Survey of India	*IAR* 2000–2001, 19–29
Kilattali (Cranganore)	Kerala	10th century	Archaeological Survey of India, Kerala State Department	*IAR* 1969–1970, 15
Darasuram	Tamil Nadu	Precise dates not recorded	Archaeological Survey of India	*IAR* 1985–1986, 71; 1986–1987, 72
Devnimori	Gujarat	7th to 11th centuries	M.S. University Baroda	*IAR* 1960–1961, 9–11; Mehta et al. 1966
Dongalasani	Andhra Pradesh	10th century	Andhra Pradesh State Department	*IAR* 1977–1978, 1
Deopani Than	Assam	Precise dates not recorded	Assam State Department	*IAR* 1962–1963, 3
Duboroni	Assam	8th century	Assam State Department	*IAR* 1997–1998, 6–9
Dulmi	Bihar	Precise dates not recorded	Archaeological Survey of India	*IAR* 1991–1992, 7–8
Dwarka	Gujarat	7th century onward	Gujarat State Department, Deccan College	*IAR* 1962–1963, 7
Gangadhara	Andhra Pradesh	Precise dates not recorded	Andhra Pradesh State Department	*IAR* 1990–1991, 1
Gollathagudi	Andhra Pradesh	9th to 10th centuries	Andhra Pradesh State Department	*IAR* 1958–1959, 11; 1970–1971, 3; 1971–1972, 3; 1972–1973, 2–3

(Continued)

Site	State	Period	Institution	Reference
Goraj	Gujarat	Precise dates not recorded	Archaeological Survey of India	*IAR* 1982–1983, 133
Goswamikhanda	West Bengal	10th century	West Bengal State Department	*IAR* 1963–1964, 60–61
Gudisala	Andhra Pradesh	Precise dates not recorded	Andhra Pradesh State Department	*IAR* 1991–1992, 1
Gudnapur	Karnataka	10th to 11th centuries	Archaeological Survey of India	*IAR* 1988–1989, 36–39; 1989–1990, 43–45
Hoysalesware	Karnataka	11th to 12th centuries	Archaeological Survey of India	*IAR* 1984–1985, 31
Indragarh	Madhya Pradesh	8th century	Madhya Pradesh State Department	*IAR* 1958–1959, 28
Jagajibanpur	West Bengal	9th century	West Bengal State Department	*IAR* 1995–1996, 114–115; 1999–2000, 204–207
Kandhar	Maharashtra	10th century	Deccan College, Maharashtra State Department	*IAR* 1983–1984, 58–59
Kanheri	Maharashtra	8th to 10th centuries	Archaeological Survey of India	*IAR* 1969–1970, 21–22
Karvan	Gujarat	Precise dates not recorded	Archaeological Survey of India	*IAR* 1974–1975, 15–16
Keesaragutta	Andhra Pradesh	Precise dates not recorded	Andhra Pradesh State Department	*IAR* 1975–1976, 1–2; 1976–1977, 3; 1978–1979, 63–64
Kenduli	Orissa	8th century	Orissa State Department	*IAR* 1964–1965, 32–33
Kesariya	Bihar	7th century	Archaeological Survey of India	*IAR* 1997–1998, 13–14; 1999–2000, 9–11; 2000–2001, 8
Khajuraho	Madhya Pradesh	10th to 12th centuries	Archaeological Survey of India	*IAR* 1980–1981, 32; 1981–1982, 29–30; 1982–1983, 37; 1998–1999, 87; 1999–2000, 77–79
Kudavelli	Andhra Pradesh	6th to 7th centuries	Archaeological Survey of India	*IAR* 1982–1983, 9
Lalitgiri	Orissa	Up to 11th century	Archaeological Survey of India	*IAR* 1985–1986, 62–63; 1986–1987, 64–67; 1987–1988, 88–90; 1988–1989, 65–66
Lanjapatragandi Temple	Andhra Pradesh	Precise dates not recorded	Archaeological Survey of India	*IAR* 1997–1998, 1
Mahadeopura	Gujarat	Precise dates not recorded	Archaeological Survey of India, M.S. University Baroda	*IAR* 1983–1984, 23–24
Malinithan	Arunachal	Precise dates not recorded	Arunchal State Department	*IAR* 1972–1973, 3
Mamallapuram	Tamil Nadu	Precise dates not recorded	Archaeological Survey of India	*IAR* 1990–1991, 62–63; 1997–1998, 156–159; 1998–1999, 155–156; 1999–2000, 148–149
Mandhata	Madhya Pradesh	Precise dates not recorded	Archaeological Survey of India	*IAR* 1995–1961, 47
Manwan	Uttar Pradesh	Precise dates not recorded	Uttar Pradesh State Department	*IAR* 1968–1969, 39; 1969–1970, 44
Martand	Kashmir	Precise dates not recorded	Archaeological Survey of India	*IAR* 1969–1970, 13
Modhera	Gujarat	11th century	Archaeological Survey of India	*IAR* 1964–1965, 12
Nagaresvara	Karnataka	Precise dates of foundation not recorded	Archaeological Survey of India	*IAR* 1985–1986, 43; 1986–1987, 42

(*Continued*)

TABLE 1 (*Continued*)

SITE	STATE	EARLY MEDIEVAL DATES (WHERE RECORDED)	INSTITUTION	REFERENCE
Nalanda	Bihar	8th to 12th centuries	Archaeological Survey of India	*Annual Reports* 1915–1916; IAR 1975–1976, 3–4; 1976–1977, 13; 1977–1978, 16; 1978–1979, 67; 1979–1980, 14
Nanganallur	Tamil Nadu	9th to 10th centuries	Archaeological Survey of India	*IAR* 1973–1974, 24
Nrusimhanath	Orissa	8th to 9th centuries	Sambalpur University	*IAR* 1991–1992, 88–89
Padrauna	Uttar Pradesh	Precise dates not recorded	Archaeological Survey of India	*IAR* 1984–1985, 88
Paithan	Maharashtra	7th to 9th centuries	Maharashtra State Department, Archaeological Survey of India, Society for South Asian Studies	*IAR* 1965–1966, 28–29; 1995–1996, 56; 1996–1997, 70; 1997–1998, 125–129; 1998–1999, 107–112
Palampet	Andhra Pradesh	Precise dates not recorded	Andhra Pradesh State Department	*Memoirs*, 6; *IAR* 1973–1974, 8
Papanasi	Andhra Pradesh	9th to 12th centuries	Archaeological Survey of India	*IAR* 1982–1983, 9–13
Peddavegi	Andhra Pradesh	7th to 8th centuries	Archaeological Survey of India	*IAR* 1984–1985, 7–8
Periyar	Kerala	7th to 10th centuries	Archaeological Survey of India	*IAR* 1999–2000, 76
Poluvanpatti	Tamil Nadu	11th century	Tamil Nadu State Department	*IAR* 1979–1980, 68–69
Raibania	Orissa	Precise dates not recorded	Orissa State Department	*IAR* 1974–1975, 36
Rangapuram	Andhra Pradesh	8th century	Andhra Pradesh State Department	*IAR* 1979–1980, 9
Rani-ki-Vav	Gujarat	11th to 12th centuries	Archaeological Survey of India	*IAR* 1964–1965, 12
Ranipur Jharial	Orissa	9th to 10th centuries	Orissa State Department	*IAR* 1975–1976, 37
Ratnagiri	Orissa	7th to 10th centuries	Archaeological Survey of India	*Memoirs*, 80; *IAR* 1957–1958, 39; 1958–1969, 33; 1959–1960, 38; 1960–1961, 28; 1997–1998, 280; 1998–1999, 130
Rajbadidanga	West Bengal	8th to 12th centuries	Archaeological Survey of India, Calcutta University	*Annual Reports* 1929–1930, 143; *IAR* 1962–1963, 46; 1963–1964, 62–63; 1964–1965, 48–51; 1966–1967, 46–47; 1968–1969, 43; 1971–1972, 50–51
Saint Augustine's Church	Goa	8th to 12th centuries	Archaeological Survey of India, Calcutta University	*IAR* 1999–2000, 17–19
Salihundam	Andhra Pradesh	Up to the 8th century	Archaeological Survey of India	*IAR* 1953–1954, 11; Subrahmanyam 1964
Sanichara	Uttar Pradesh	Precise dates not recorded	Uttar Pradesh State Department	*IAR* 1990–1991, 79–80

(*Continued*)

Site	State	Date/period	Source	Reference
Saraswati	Uttar Pradesh	Precise dates not recorded	Archaeological Survey of India, Kansai University	*IAR* 1997–1998, 186–197
Sejakpur	Bihar	Precise dates not recorded	Archaeological Survey of India	*IAR* 1965–1966, 18
Shyamsundar Tila	Tripura	9th to 10th centuries	Archaeological Survey of India	*IAR* 1984–1985, 78–79; 1998–1999, 159–161; 1999–2000, 155; 2000–2001, 117–119
Sirpur	Chattisgarh	7th to 8th centuries	Sagar University	*IAR* 1954–1955, 24; 1955–1956, 26; 1999–2000, 96–99
Siyot	Gujarat	Up to the 12th–13th centuries	Gujarat State Department	*IAR* 1988–1989, 10
Saraswati Kunda	Madhya Pradesh	Precise dates not recorded	Archaeological Survey of India	*IAR* 1987–1988, 71–73
Sri Surjyapahar	Assam	10th to 11th centuries	Archaeological Survey of India	*IAR* 1995–1996, 3–4
Surya Pahar	Assam	8th to 9th centuries	Archaeological Survey of India	*IAR* 1997–1998, 5–6; 1999–2000, 1–9; 2000–2001, 3–7
Taradih (Bodh Gaya)	Bihar	Precise dates not recorded	Bihar State Department	*IAR* 1997–1998, 14–15
Thakurani Tila	Tripura	9th to 13th centuries	Archaeological Survey of India	*IAR* 1984–1985, 79–80; 1998–1999, 161; 1999–2000, 155–157; 2000–2001, 119
Tirukkulasekharapuram	Kerala	10th century	Archaeological Survey of India, Kerala State Department	*IAR* 1969–1970, 15
Tiruvanjikulam	Kerala	9th to 10th centuries	Archaeological Survey of India, Kerala State Department	*IAR* 1969–1970, 15
Tumain	Madhya Pradesh	Precise dates not recorded	Sagar University	*IAR* 1971–1972, 28
Udayagiri	Orissa	8th to 13th century	Archaeological Survey of India	*IAR* 1958–1959, 38–40; 1961–1962, 36–37; 1965–1966, 35; 1985–1986, 63–64; 1986–1987, 67; 1987–1988, 90–95; 1988–1989, 66–69; 1997–1998, 136–144; 1998–1999, 120–130; 1999–2000, 117–124; 2000–2001, 109
Udayasvara	Madhya Pradesh	11th century	Archaeological Survey of India	*IAR* 1981–1982, 48
Udayapur	Madhya Pradesh	11th century	Archaeological Survey of India	*IAR* 1983–1984, 56
Wasvi	Madhya Pradesh	8th to 9th century	Archaeological Survey of India	*IAR* 1991–1992, 53

Note: Unless cited otherwise, information in this table is drawn from the following publications of the Archaeological Survey of India (ASI): *Annual Reports*, *Indian Archaeology—A Review* (cited as *IAR*), and *Memoirs of the Archaeological Survey of India* (cited as *Memoirs*). These publications are listed on the ASI website at http://asi.nic.in/asi_publications.asp.

excavation has tended to be limited to uncovering carved remains, establishing the foundations of the monuments, and fixing their chronological parameters and spatial extents, primarily to document them, with the carved remains themselves then analyzed "art historically," either by archaeologists, or, more usually, within the disciplines of art and architectural history. Elements relating to the wider site are ignored. As at the famous temple complex of Khajuraho, for instance, excavated between 1980 and 1989 by the Archaeological Survey of India (see relevant volumes of *Indian Archaeology—A Review*), factors such as the nature of the activities that took place at the site, their place and role in the landscape, and contiguous areas of occupation were not to be considered archaeologically. Following excavation, religious sites then continue to be a focus of art historical and architectural interest, but do not tend to feature in archaeological literature, and are rarely revisited by archaeologists.[24]

Likewise, a large number of settlements dating to the early medieval period have also been identified through archaeological survey and excavation. However, most have only been identified as the result of general explorations geared toward identifying and locating all the sites and antiquities in a given area (either wide regions or small areas immediately surrounding other known sites), with little attempt made to investigate them. As far as excavated sites are concerned, excavations have tended, with one or two notable exceptions, to be aimed either at fixing the entire cultural sequence of a site, or investigating earlier phases of occupation—the early medieval layers being just one of a number of phases of occupation encountered on the way down to the earliest foundations of the site.[25] Initially, this might not seem problematic. If archaeological investigations have resulted in the discovery of material pertaining to the early medieval period, then what do the motives informing these enquiries matter? The problem is that, because the guiding foci have not been the examination of the early medieval, all such sites identified through field walking remain uninvestigated. Furthermore, the majority of the archaeological evidence that we have for early medieval settlements and secular activities (including the entire range of social, economic, and political dynamics) is the material that has been excavated from sites with premedieval foundations. In practice, this means there are only approximately 105 excavated settlements with early medieval remains that have been recorded in the whole of India (Table 2).[26] This situation is no better in other regions of South Asia.

Problems and Limitations

Over and above the limitations of having such a small data set for the period, there are also significant problems with the existing archaeological evidence. On a general level, it is by no means certain that the (few) excavated settlements represent the settlement history of the period in a meaningful way. All the sites excavated so far are towns and urban sites. No rural sites dating to the period have been excavated in the entire subcontinent. This makes the existing settlement data highly biased toward the urban sphere, which is somewhat ironic, given that it was villages, not urban centers that were (historically) supposed to have characterized the pattern of early medieval settlement. At the same time, the traditional focus on settlements with earlier foundations means that those sites argued to have been founded during the early medieval period have not been investigated. In some cases, this amounts to serious oversight

TABLE 2. LIST OF EXCAVATED SETTLEMENTS IN INDIA WITH EARLY MEDIEVAL PHASES OF OCCUPATION

SITE NAME	STATE	DATES OF EARLY MEDIEVAL LAYERS (WHERE RECORDED)	EXCAVATING INSTITUTION	REFERENCE(S)
Adiyamankottai	Tamil Nadu	Entire period represented	Madras University	*IAR* 1980–1981, 65
Agiabir	Uttar Pradesh	Precise dates not recorded	Banaras Hindu University	*IAR* 1998–1999, 193–197; 1999–2000, 183–186
Ahar	Rajasthan	Precise dates not recorded	Deccan College, Rajasthan State Department, University of Melbourne	*IAR* 1961–1962, 45–50
Ahicchatra	Uttar Pradesh	Up to 950 C.E.	Archaeological Survey of India	*Ancient India* 1, 39; 9, 39
Akota (Vadodara)	Gujarat	Precise dates not recorded	M.S. University Baroda	*IAR* 1953–1954, 10; Subbarao 1953
Alagarai	Tamil Nadu	Precise dates not recorded	Madras University	*IAR* 1963–1964, 21
Anahan	Uttar Pradesh	Entire period represented	Archaeological Survey of India	*IAR* 1995–1996, 98–103
Anand Bhawan	Uttar Pradesh	Precise dates not recorded	Allahabad University	*IAR* 1973–1974, 26
Anhilwad	Gujarat	From c. 1000 C.E. onward	M.S. University Baroda	*IAR* 1978–1979, 68
Antichak	Bihar	Precise dates not recorded	Patna University	*IAR* 1961–1962, 3–4
Arikamedu	Tamil Nadu	Entire period represented	University of Pennsylvania	*Ancient India* 2, 17; *IAR* 124; Begley 1996–2004
Atranjikhera	Uttar Pradesh	Up to 1100 C.E.	Aligarh Muslim University	Gaur 1983
Ayodhya	Uttar Pradesh	600–900 C.E.	Banaras Hindu University	*IAR* 1969–1970, 41; 1979–1980, 77
Bahiri	West Bengal	Precise dates not recorded	Asutosh Museum	*IAR* 1956–1957, 81
Balirajgarh	Bihar	Up to 1200 C.E.	Bihar State Department	*IAR* 1972–1973, 7
Ballal Dhipi	West Bengal	Precise dates not recorded	Archaeological Survey of India	*IAR* 1982–1983, 105
Banavasi	Karnataka	Precise dates not recorded	Mysore University, Karnataka State Department	*IAR* 1970–1971, 29
Besnagar	Madhya Pradesh	Up to 700 C.E.	Archaeological Survey of India	*IAR* 1963–1964, 16–17
Bhagatrav	Gujarat	Precise dates not recorded	M.S. University Baroda	*IAR* 1957–1958, 15
Bhinmal	Rajasthan	Precise dates not recorded	Rajasthan State Department	*IAR* 1953–1954, 12
Brahmagiri	Karnataka	Precise dates not recorded	Archaeological Survey of India	Morrison 2005; Wheeler 1947–1948
Brahmapuri	Maharashtra	Up to 1000 C.E.	Deccan College	Sankalia & Dikshit 1952
Broach / Baruch	Gujarat	500–750 C.E. & 1200 C.E. onward	Archaeological Survey of India	*IAR* 1959–1960, 19

(Continued)

TABLE 2 (*Continued*)

SITE NAME	STATE	DATES OF EARLY MEDIEVAL LAYERS (WHERE RECORDED)	EXCAVATING INSTITUTION	REFERENCE(S)
Champa	Bihar	Up to 800 C.E.	Patna University	*IAR* 1969–1970, 2; 1970–1971, 4–5; 1971–1972, 5; 1972–1973, 6–7; 1974–1975, 8–9; 1975–1976, 7–8; 1976–1977, 11–12
Chandraketugarh	West Bengal	Up to c. 750 C.E.	University of Calcutta	*IAR* 1957–1958, 51–53; 2000–2001, 156–160
Chaul	Maharashtra	Entire period represented	Deccan College	Gogte 2003; Gogte et al. 2006
Chirand	Bihar	Up to 600 C.E. & 800 C.E. onward	Bihar State Department	*IAR* 1970–1971, 7; 1962–1963, 6; 1964–1965, 7
Dwarka (Saurashtra)	Gujarat	Entire period represented	Deccan College	Ansari & Mate 1966
Ekalbara	Madhya Pradesh	Precise dates not recorded	Madhya Pradesh State Department	*IAR* 2000–2001, 86–90
Gangaikondacholapuram	Tamil Nadu	Precise dates not recorded	Tamil Nadu State Department	*IAR* 1983–1984, 80–81
Gilund	Rajasthan	Precise dates not recorded	Archaeological Survey of India	*IAR* 1959–1960, 41–46
Gomsi (Rani)	Arunchal	Precise dates not recorded	Arunchal State Department	*IAR* 1995–1996, 3; 1996–1997, 2
Hella Bazpur	Bihar	Precise dates not recorded	Patna University	*IAR* 1999–2000, 16
Jaderu	Madhya Pradesh	800–1200 C.E.	Archaeological Survey of India, Jiwaji University, Gwalior	*IAR* 1971–1972, 29–30
Jafar Chak	Jammu and Kashmir	Entire period represented	Archaeological Survey of India	*IAR* 1998–1999, 49
Jhusi	Uttar Pradesh	Precise dates not recorded	University of Allahabad	*IAR* 1997–1998, 164–170; 1998–1999, 161–167
Juna Khera	Rajasthan	Precise dates not recorded	Rajasthan State Department	*IAR* 1990–1991, 60–62
Junnar	Maharashtra	Precise dates not recorded	Deccan College	Recent excavations unpublished Gupta et al. 2004*b*
Kamrej	Gujarat	800–1000 C.E.	Indian Archaeological Society	*IAR* 1969–1970, 34–35; 1970–1971, 32–33; 1974–1975, 37–38
Kanchipuram	Tamil Nadu	Entire period represented	University of Madras	
Kansipur	Jammu and Kashmir	Precise dates not recorded	Archaeological Survey of India	*IAR* 1998–1999, 30–48
Karian	Bihar	900 C.E. onward	K. P. Jayaswal Research Institute	*IAR* 1954–1955, 26

(*Continued*)

Site	State	Period	Institution	Reference
Karna Chaura	Bihar	From c. 750 C.E. onward	Bihar State Department	*IAR* 1979–1980, 13
Karvan	Gujarat	Up to 800 C.E. & 1100 C.E. onward	Archaeological Survey of India	*IAR* 1974–1975, 15; 1975–1976, 15
Kaveripattinam (Puhar)	Tamil Nadu	Up to 1200 C.E.	Archaeological Survey of India	*IAR* 1962–1963, 13; 1963–1964, 20; 1964–1965, 25; Soundara Rajan 1994
Kayatha	Madhya Pradesh	Up to 700 C.E.	Vikram University	*IAR* 1964–1965, 18–19
Kelshi	Maharashtra	Unknown	Deccan College	Joglekar 2002
Kesarapalli	Andhra Pradesh	Precise dates not recorded	Unknown (part of Nagarjunakonda project)	*IAR* 1961–1962, 1–2
Khalkapatna	Orissa	From 1100 C.E. onward	Archaeological Survey of India	*IAR* 1984–1985, 59; 1994–1995, 61–62
Kothapatnam	Andhra Pradesh	Precise dates not recorded	Andhra Pradesh State Department	*IAR* 1996–1997, 1
Kottapurra	Kerala	From 700 C.E. onward	Kerala State Department	Unpublished
Kunnattur	Tamil Nadu	700–1200 C.E.	Archaeological Survey of India	*IAR* 1956–1957, 31–34; 1957–1958, 37–38
Lakhabaval	Gujarat	Precise dates not recorded	M.S. University Baroda	Unpublished
Lashkarshah	Gujarat	Precise dates not recorded	M.S. University Baroda	Bhan 2006
Lavet	Gujarat	Precise dates not recorded	M.S. University Baroda	Unpublished
Madilikam	Kerala	From c. 900 C.E. onward	Archaeological Survey of India, Kerala State Department	*IAR* 1969–1970, 15
Malhar	Madhya Pradesh	600–1200 C.E.	Sagar University	*IAR* 1974–1975, 21–23
Maligamedu	Tamil Nadu	Entire period represented	Tamil Nadu State Department	*IAR* 1999–2000, 147–148
Maner	Bihar	Precise dates not recorded	Patna University	*IAR* 1996–1997, 6–8; 1997–1998, 15–17
Mayo Hill	Arunachal	From c. 900–1200 C.E.	Arunachal State Department	*IAR* 1992–1993, 4
Moti Rayan / Nani Rayan (Mandvi)	Gujarat	Precise dates not recorded	Gujarat State Department, M.S. University Baroda	*IAR* 1961–1962, 10; Irani 2002
Nagara	Gujarat	Entire period represented	M.S. University Baroda	*IAR* 1963–1964, 9–10; 1964–1965, 12; Mehta 1968
Nagari	Madhya Pradesh	Precise dates not recorded	Archaeological Survey of India	*IAR* 1962–1963, 19–20
Nevasa	Maharashtra	Precise dates not recorded	Deccan College	*IAR* 1954–1955, 5–9
Pagara	Madhya Pradesh	800–1100 C.E.	Madhya Pradesh State Department	*IAR* 1980–1981, 32–33
Palayarai	Tamil Nadu	Precise dates not recorded	Tamil Nadu State Department	*IAR* 1983–1984, 79–80
Palir	Tamil Nadu	Entire period represented	University of Madras	*IAR* 2000–2001, 115
Pallavamedu	Tamil Nadu	Up to 900 C.E.	Archaeological Survey of India	*IAR* 1970–1971, 33
Palshet (Palaepatmai)	Maharashtra	Entire period represented	Deccan College	Gogte 2004

(*Continued*)

TABLE 2 (*Continued*)

SITE NAME	STATE	DATES OF EARLY MEDIEVAL LAYERS (WHERE RECORDED)	EXCAVATING INSTITUTION	REFERENCE(S)
Pakhanna	West Bengal	Entire period represented	Calcutta University	*IAR* 1999–2000, 197–198
Pandharpur	Maharashtra	From c. 1000 C.E.	Deccan College	*IAR* 1967–1968, 35–36
Pattadakal	Karnataka	Precise dates not recorded	Archaeological Survey of India	*IAR* 1954–1955, 62
Pattanam	Kerala	Up to 1000 C.E.	Kerala Council for Historical Research	Cherian et al. 2007
Paunar	Maharashtra	Precise dates not recorded	Nagpur University	Deo & Dhavalikar 1968
Perur	Tamil Nadu	Up to 800 C.E.	Archaeological Survey of India	*IAR* 1970–1971, 33–34
Prakash	Maharashtra	Up to 800 C.E.	Archaeological Survey of India	*IAR* 1954–1955, 13; *Ancient India* 20 & 21
Pumpuhar	Tamil Nadu	Up to 1200 C.E.	Tamil Nadu State Department	*IAR* 1997–1998, 159
Purana Qila	Delhi	Up to 800 C.E.	Archaeological Survey of India	*IAR* 1969–1970, 5; 1970–1971, 10
Rajahmundry	Andhra Pradesh	Entire period represented	Andhra Pradesh State Department	*IAR* 1979–1980, 1
Rajghat	Uttar Pradesh	Entire period represented	Banaras Hindu University	*IAR* 1957–1958, 50–51; 1960–1961, 35–39; Narain & Roy 1976–1978
Ramasare Purwa	Uttar Pradesh	Precise dates not recorded	University of Lucknow	*IAR* 1996–1997, 151–153
Sambhar	Rajasthan	Up to 1000 C.E.	Rajasthan State Department	Sahni 1999
Sanghol	Punjab	Up to 700 C.E. and from 1000 C.E. onward	Punjab State Department	*IAR* 1968–1969, 26; 1969–1970, 32; 1970–1971, 30–31; 1977–1978, 43–44
Sanjan	Gujarat	From 700 C.E. onward	Indian Archaeological Society	Gupta et al. 2004*a*; Gupta et al. 2002, 2003; Gupta et al. 2005
Saraswati	Uttar Pradesh	Precise dates not recorded	Archaeological Survey of India, Kansai University	*IAR* 1997–1998, 186–197; 1998–1999, 207–213
Shamlaji	Gujarat	Entire period represented	M.S. University Baroda	*IAR* 1961–1962, 13–14
Sirpur	Chattisgarh	600 to 1100 C.E.	Sagar University	*IAR* 1954–1955, 24; 1955–1956, 26
Somnath (Prabhasa Patan)	Gujarat	Up to c. 1000 C.E.	M.S. University Baroda	Nanavati et al. 1971
Sonkh	Uttar Pradesh	Entire period represented	Museum of Indian Art, Berlin	Härtel 1993
Sravasti	Uttar Pradesh	Precise dates not recorded	Archaeological Survey of India	*IAR* 1958–1959, 48–50; 2000–2001, 140–152

(*Continued*)

Site	State	Date	Institution	Reference
Sringaverapur	Uttar Pradesh	Up to 1000 C.E.	Indian Institute of Advanced Study, Archaeological Survey of India	IAR 1977–1978, 54–56; 1978–1979, 57–59; 1979–1980, 74; 1980–1981, 67–68; 1981–1982, 66–67
Sugh	Haryana	Up to c. 1200 C.E.	Haryana State Department	IAR 1998–1999, 23–24
Sunet	Punjab	Up to 800 C.E.	Punjab State Department	IAR 1983–1984, 69
Thaneswar	Punjab	Precise dates not recorded	Unknown	IAR 1957–1958, 68
Timbarva	Gujarat	Up to 1000 C.E.	M.S. University Baroda	Mehta 1955
Tirukkambuliyur	Tamil Nadu	Entire period represented	University of Madras	IAR 1961–1962, 28; Champakalakshmi 1976
Tiruverkadu	Tamil Nadu	600–1200 C.E.	University of Madras	IAR 1995–1996, 71; 1996–1997, 100–105; 1997–1998, 162–163; 1998–1999, 157–158; 1999–2000, 151–152
Tumain	Madhya Pradesh	Entire period represented	Sagar University	IAR 1971–1972, 28
Ujjain	Madhya Pradesh	Precise dates not recorded	Archaeological Survey of India	IAR 1956–1957, 20–28; 1957–1958, 32–36
Ukkirankottai	Tamil Nadu	Up to c. 1000 C.E.	Archaeological Survey of India	IAR 1970–1971, 35
Uraiyur	Tamil Nadu	Up to 700 C.E.	University of Madras	IAR 1964–1965, 25; 1965–1966, 26
Vadnagar	Gujarat	Up to 1000 C.E.	M.S. University Baroda	IAR 1953–1954, 10
Valabhi /Vallabipur	Gujarat	Up to 800 C.E.	M.S. University Baroda	IAR 1979–1980, 24
Vizhinjam	Kerala	800–1200 C.E.	Kerala University	Unpublished
Yeleswaram	Andhra Pradesh	Up to 1000 C.E.	Andhra Pradesh State Department	Khan 1963

Note: Unless cited otherwise, information in this table is drawn from the following publications of the Archaeological Survey of India (ASI): *Ancient India: Bulletin of the Archaeological Survey of India* (cited as *Ancient India*), *Annual Reports*, *Indian Archaeology—A Review* (cited as *IAR*), and *Memoirs of the Archaeological Survey of India* (cited as *Memoirs*). These publications are listed on the ASI's website at http://asi.nic.in/asi_publications.asp.

in the general archaeological endeavor. For instance, Kannauj, the much-contested capital of northern India, has never been excavated and is now almost completely destroyed through modern incursion (Heitzman 2008). Similarly, the Chavda city of Patan, which grew to be one of the largest cities in the world (Chandler 1987), has never been subjected to systematic excavation, other than the uncovering of monuments from the temple complex of Rani-ki-vav.[27] As a result, we have a large number of sites that exist only as place-names in the documentary sources; they remain unlocated on the ground, and the activities that took place within them have not been investigated.

Furthermore, examination of the details of the excavated sites reveals a number of methodological concerns that raise questions about the identification, dating, and interpretation of the early medieval remains.[28] Many sites were investigated using an excavation strategy introduced by Mortimer Wheeler in the 1940s (Wheeler 1954), which, as is well known, involves the vertical excavation of a limited number of trenches or test squares. Geared toward generating deep stratigraphic and pottery sequences, this strategy was initially intended to establish the broad chronological sequence of any given site, and thus develop an overview of the settlement history of the subcontinent, which could then be expanded with more extensive excavations. Yet, despite the changing needs of archaeology through the course of the twentieth century, the small-scale application of this vertical excavation strategy has remained the established method of archaeological investigation. In most cases, excavations have been limited to between one and five trenches across sites that usually cover many hectares, and comprise multiple occupation mounds. At the site of Nagara, for instance, four trenches were excavated in three of the five settlement mounds that defined the site (Mehta 1968). The total area covered by these trenches was 192 m^2. Yet, with the site itself extending over 330,000 m^2, this amounts to only 0.06 percent of the total area of the site. Indeed, at most of the excavated settlements with early medieval phases of occupation, only 0.05–2 percent of the total area of the sites has been excavated. This raises the question of the extent to which the objects discovered and activities identified archaeologically at many sites are representative of these sites as a whole.

An additional implication of following such methods is that in some instances earlier excavations have failed to record the movement of settlements over time. Areas of occupation can, and did, shift within the same locality. This is a process apparent in both the ancient and medieval past, as well as in the present day.[29] Clearly, the practice of only digging a limited number of trenches across the total area of a site does not, and cannot, account for such horizontal changes. This is further hindered by the fact that many archaeological sites are contiguous to modern settlements, which, if they cover archaeological deposits, are likely to cover the most recent phases of occupation history. Lacking an established methodology for excavation within areas of modern habitation, these areas, and by extension the more recent phases of occupation, tend not to be excavated. This has recently been demonstrated by Kennet (2013), who has shown that large and substantial areas of the sites at Bhokardan, Nevasa, and Beshnagar (all of which are supposed to have been abandoned during the early medieval period) were never excavated. Overall, as far as the early medieval is concerned, it seems likely that at least some of the excavations that have taken place may not have located the main phases of early medieval occupation.

Compounding this situation is the identification of stratigraphic layers (defined according to the nature of the archaeological deposits encountered), to which all artifacts can be assigned—the element central to Wheeler's method of vertical excavation. At many of the sites excavated from the mid- to late twentieth century, the definition of these layers is somewhat crude by modern standards (Kennet 2004*b*:2). Often, only a small number of layers per meter of excavation depth have been identified, each of which accounts for a considerable period of occupation history.[30] Modern excavation methods, regardless of the strategy being employed, define layers as single discrete units of deposition resulting from a wide variety of human activities or environmental changes.[31] These can account for much shorter spans of time, including: single events (such as the collapse of an individual structure); a number of months (such as deposits formed by monsoonal changes); or a number of years (such as the period during which a particular refuse pit was used). Through the identification and recording of these minute changes in deposits, together with an understanding of taphonomic processes, it is possible to reconstruct the cultural and natural processes that contributed to the formation of stratigraphic layers, and identify quite complex relationships between depositional matrices and the artifacts that occur within them. Yet, it is precisely this level of detail that often has not been recorded in earlier excavations. This not only makes it difficult to reconstruct a detailed picture of the occupation history of a site, but also introduces the possibility that artifacts of widely different dates may have been incorporated into the same broad layers with no means of disentangling them retrospectively. At some sites this problem is exacerbated by a tendency to group the stratigraphic sequences from various trenches together in order to construct a unified occupation history for the entire site, even if the excavated areas are widely separated (Kennet 2004*b*:3).[32] Different areas of any site, especially settlements, rarely experience the same occupation history, and attempts to construct unified "master stratigraphies" introduce a considerable degree of uncertainty concerning the accuracy of the identifications made.

To an extent, these issues affect the interpretation of all periods of a site's history. Yet, as far as the identification and interpretation of early medieval phases of occupation are concerned, they are further compounded by additional uncertainties surrounding the dating of these layers. Scientific dating methods, such as radiocarbon (^{14}C) dating, tend to be reserved for older pre- and protohistoric layers. Indeed, no remains from stratigraphic layers identified as dating to the early medieval period have ever been subjected to radiocarbon analysis. Nor, for that matter, have many of the layers immediately preceding them.[33] Instead, the chronologies of historical periods tend to be based on evidence provided by coins and key pottery types. Of these two types of evidence, the numismatic often underpins the dating of other associated remains, including pottery types. However, there is a relative paucity of coins dating from the fifth to tenth centuries in archaeological contexts.[34] This fact alone clearly makes the dating of early medieval layers more challenging. At the same time, the paucity of early medieval coinage brings into question the dating of many earlier phases of occupation that have been made on the basis of coin types. At many sites, coins dating to the early centuries C.E. have frequently been used to date stratigraphic layers to the centuries in which the coins were produced. However, such uncritical use of these coins as dating evidence ignores their potential residuality, and the rule of *terminus post quem*.[35] As it has recently been demonstrated, coins dating to the early

centuries C.E. often continued in use for some centuries after being minted (Shastri 1997:142–144). This, together with the paucity of early medieval coins in archaeological layers, makes it difficult to ascertain whether layers containing earlier coins date to these periods, or whether these coins were deposited later and the layers in question date to different centuries.[36]

It is fortunate that a few sites have yielded coins from the period, such as Akota (Subbarao 1953), but beyond these, early medieval layers have usually been dated on the basis of ceramic evidence. The use of well-known pottery wares, or "fossil types," is, of course, standard practice in the dating of archaeological deposits. Yet, as far as the dating of early medieval layers is concerned, this has not been achieved with reference to the ceramic types that occur in these layers. The majority of early medieval ceramic assemblages comprise a great many local and regional pottery types. Often subsumed under the broad category of local red or gray coarse wares, these appear to be the products of traditions of local ceramic production that did not change over the centuries (Sinopoli 1999). Due to the crude methods used in their manufacture and their uniform appearance, they offer little in the way of diagnostic features that can be used for dating. Instead, early medieval layers have tended to be dated (and thus identified) on the basis of key pottery types found in the layers immediately above and below them. The most common of these are Islamic (or Medieval) Glazed Wares, Red Polished Ware, and Roman Amphorae, all of which have a wide distribution across the subcontinent.

However, as has been noted elsewhere, significant problems surround (and are by no means limited to) the dating of these wares (Kennet 2004b:13–15; 2013:346–347). Glazed Wares are traditionally dated to the later medieval period. Recognized as a foreign ceramic tradition originating in the Middle East, their presence in India has usually (and somewhat crudely) been conflated with Islamic peoples, and dated according to when they were perceived to have been present in India. Thus, the standard view is that Glazed Wares in India cannot be dated to earlier than the tenth century. They are more commonly dated to the thirteenth or fourteenth century, when various parts of the subcontinent came under Islamic rule.[37] However, as recent excavations at a number of South Asian sites are beginning to show, a number of Islamic Glazed Wares were exported to the west coast of South Asia from the Persian Gulf from at least the early centuries C.E. (Kennet 2013:346).[38] At the other end of the chronological spectrum, Red Polished Ware is traditionally dated to between the first and sixth centuries C.E., often on the basis of its stratigraphic association with early coins.[39] Yet, recent excavations in East Africa and throughout the Persian Gulf have resulted in the discovery of exported examples of Indian Red Polished Ware in archaeological contexts that have been conclusively dated to at least the seventh and eighth centuries.[40] In addition, it has recently been demonstrated that many ceramic fragments that have traditionally been identified as Roman Amphorae are, in fact, fragments of Torpedo Jars, many of which date to as late as the eighth and ninth centuries (Tomber 2007).

This is not to suggest that all of the dates previously ascribed to these ceramics are erroneous. Yet, the simple fact that Glazed and Red Polished Wares have been found in earlier and later contexts, and that potsherds of Roman Amphorae may have been misidentified altogether, raises the distinct possibility that the dates for some of the stratigraphic layers made on the basis of these wares might need to be reassessed. Together with the uncertainties surrounding the phasing of sites on the basis of coins,

this has a number of implications. Most immediately, and in the absence of more se-cure scientific dates, it means that stratigraphic layers at many sites currently dated to both earlier and later periods might actually date to the early medieval period (Kennet 2013 : 347). Indeed, that this might be the case has been demonstrated by the recent reappraisal of the stratigraphic sequence at Brahmagiri, Tamil Nadu (Morrison 2005). Here, analysis of ceramic material first excavated by Mortimer Wheeler has resulted in the preliminary identification of a number of "Early Middle" period (early medieval) remains—an identification that is supported by a new AMS date of 1190 to 1280 C.E. (Morrison 2005 : 259–261).

Such uncertainty surrounding the dating of early medieval layers introduces a high degree of skepticism regarding the identification of early medieval artifacts, which, in turn, only serves to reinforce existing problems surrounding the understanding of local and regional pottery types. Indeed, there is currently only one established typol-ogy for early medieval pottery: the ceramic report from the recently excavated site of Sanjan in Gujarat (Nanji 2011). By necessity, this has relied heavily on comparisons with datable examples of glazed wares from the Persian Gulf and Arabia in order to make any sense of the local, indigenous Indian wares that also occur at the site. Nor is this problem limited to our understanding of early medieval ceramics. With the dating of stratigraphic layers being called into question, it is also difficult to identify other aspects of early medieval material culture, including: other ceramic material (bricks, terracottas, and tiles), animal and human bone assemblages, metal work, worked bone and worked stone assemblages, and the entire suite of other environ-mental remains. Ultimately, we are left with a situation where there is no real knowledge or typological understanding of artifacts—the basic building blocks of archaeological interpretation—from the early medieval period.

ASSESSMENT OF THE CURRENT SITUATION

The effects of this relative absence of archaeological research and the problems relating to the limited evidence available cannot be underestimated. Most immediately, they seriously bring into question current archaeological and historical interpretations. As noted, many of the historical debates over the socioeconomic changes defining the transition from the early historic to early medieval periods were underpinned by archaeological evidence from excavated settlements. Theories of a phase of de-urbanization and a decline in craft production, trade, and monetized exchange were "proved" by an apparent absence of archaeological material dating to the early medi-eval period from excavations. Yet, as a critical appraisal of this evidence shows, current archaeological evidence cannot support these theories. The possibility that strati-graphic layers previously identified as predating and postdating the early medieval may, in fact, date to the early medieval period itself means there is every chance that the perceived decline in activities or break in occupation at many sites is imaginary. This, together with the likelihood that the main areas of early medieval occupation at many sites may not have been excavated adds further questions to theories regard-ing a decline in the scale or nature of activities (craft production, trade, and the use of coins) that are based on a perceived absence of archaeological evidence. It is possible that many early medieval remains (including coins) exist in areas of sites that have not been excavated, not to mention the great number of sites that have yet to be excavated. While this clearly questions key arguments for the feudal models, it also

goes to the heart of many alternative theories, which in some respects have also ac-
cepted the perceived lack of archaeological evidence.

Not only do these difficulties undermine existing historical interpretations and
theories, they limit what *can* be said, archaeologically, about the early medieval period
as a whole. The limited ways in which both religious sites and settlements have been
excavated impedes a full and connected account of the sites themselves—the contexts
and spaces within which past activities and practices took place. At religious sites, an
exclusive focus on monuments has denied the investigation of the archaeological
realities of the wider sites of which they were a part. While as far as settlements are
concerned, there are clearly limits to what can be said about the horizontal pattern of
occupation at many sites. Due to the ways in which stratigraphic layers have been
identified, it is impossible to construct a detailed, let alone accurate, picture of a site's
history. These restrictions are further compounded by the poor level of understanding
of the material remains from the period. Without a sufficient grasp of the artifacts, it
is difficult to say much that is meaningful about the activities and practices that they
reflect on a site-by-site basis, let alone other processes and developments operating
on a wider scale, such as economic, political, and sociocultural networks and interac-
tions, beliefs, and identities. A limited understanding of early medieval artifacts does
not just impede a materially derived understanding of the period, it also causes serious
methodological constraints. It prevents recognition of early medieval sites during
field walking, and accurate identification of early medieval layers and deposits during
excavation. In short, present limitations make it almost impossible to pursue archaeo-
logical scholarship.

Thus, with the early medieval period not being studied archaeologically, it can be
considered to exist only as an historical entity within other disciplines that have sought
to define it. This raises a number of questions regarding the extent to which an ar-
chaeological picture of the developments that characterize the early medieval period
might differ from that constructed through historical and art historical research. It also
means that for the most part, textual sources and monumental remains currently pro-
vide the only means of studying the period. Given this, the limitations inherent in
those sources become even starker. As noted, the textual sources and monumental
remains bear heavy bias. On the one hand, clearly, both sets of evidence only record
specific types of information—that specific to its medium. Furthermore, that which
is recorded or represented is also shaped by its authorship. The subjects treated were
all determined by particular individuals or institutions for specific audiences. The
picture of society or economic structures that is either recorded in or inferred from
the texts can thus only ever be that side of the picture recorded by the person or
persons responsible for its production. Similarly, that which might, for example,
be gleaned about religious beliefs from temple carvings can only be that which was
selected for representation by those who caused them to be carved. Thus, at whatever
level these forms of evidence are interrogated—whether at the level of "big" questions
relating to societal developments, or at the level of "small" questions concerning indi-
vidual action and personal experience—it has to be accepted that only one side of the
picture is ever going to be visible.

This does not mean that these forms of evidence should be rejected out of hand.
From an archaeological perspective, documents, inscriptions, monuments, and carv-
ings can all be considered forms of material culture and should be investigated as such.
Indeed, it would be as much a failing of archaeological research to ignore these forms

of evidence as it is a failing of wider historical research to ignore the full extent of the value of archaeological evidence. Nor is it the intention here to undervalue the so-phistication of historical and art historical approaches and the importance of their findings to date. Problems of bias have long been recognized in these disciplines, and ways to recognize bias and surmount it are central to their methodological and theo-retical approaches.[41] Consideration of how they can be read and what can be inferred about the social and cultural contexts of their authorship has inspired a number of recent studies devoted to the textual and monumental data.[42]

Yet, although certain forms of bias can be ameliorated by shifting the perspective from which the evidence is examined, other biases also exist that are not so easily surmounted: those of geographical and temporal coverage. Geographically, different regions of India have yielded varying quantities of evidence, meaning that levels of historical knowledge and understanding are prejudiced toward those that have most extant sources. The history of the Chola Empire in South India, for instance, is known in greater detail than that of other areas, because more epigraphic sources pertaining to this particular region have survived.[43] Recognizing this, a number of scholars have begun to invest in the study of those less source-rich regions. Other parts of South India, such as Kerala and Orissa, that have been overshadowed by the study of the Chola Empire are coming under increasing scrutiny (Narayanan 2002; Talbot 2001; Veluthat 2009), as are rarely studied areas of the far north, such as Assam (Lahiri 1991). Chronologically, particular centuries are also more elucidated by texts and monu-ments than others. Returning to the example of the Chola Empire, despite its long history, the majority of epigraphic sources pertain to the later Chola dynasty (c. 1070–1279 C.E.). As far as temple architecture is concerned, certain areas of the subconti-nent witnessed more building activity during some centuries than others.[44] Relative absences in this respect mean some areas and particular centuries of the early medieval period will always be invisible. This further limits wider pan-Indian perspectives, and impedes a connected account of the period as a whole.

When these limitations with the textual and monumental sources are viewed in this light, and bearing in mind their foundational role in the construction of existing knowledge and understanding of the early medieval, it is perhaps not surprising that the period is so poorly understood in terms of its development and its relationship to the later medieval. Even allowing for certain issues, such as the fragmentation of scholarship into discrete academic disciplines, there will always be certain problems that limit what the texts and monuments can tell us about the early medieval. Indeed, these problems are widely acknowledged, prompting calls for more "comparative frameworks" that can be used to augment the limited understanding of certain aspects of the period (Singh 2011:34–36). Such awareness renders the absence of archaeo-logical research and the problematic nature of the existing data even more salient. If the early medieval continues to be interrogated as it has been, with one whole cate-gory of available evidence being effectively ignored by particular branches of scholar-ship and archaeologists themselves not studying the period, there will only ever be a limited understanding of what occurred, and the present limitations of definition and meaning will become further entrenched. What is needed over and above academic concerns relating to the meaning and use of the terms "medieval" and "early medi-eval" is a change in the way these centuries are studied. Central to this has to be a reinvigoration of archaeological research on the early medieval period. On a practical level, the current state of archaeological understanding alone warrants further study;

but more importantly, it would seem that only through incorporating archaeological research into the study of the period will it be possible to move beyond present limitations of what we know and how it is understood.

FUTURE DIRECTIONS

The first step in reinvigorating the archaeological approach to the study of the early medieval must involve a thorough reassessment of the early medieval material that already exists on a site-by-site basis: critically reappraising the stratigraphic layers that have been defined; assessing the bias inherent in original excavation methodologies; and thoroughly reanalyzing the material remains with reference to the new and better-dated ceramic material that is being generated from ongoing excavations in the subcontinent, as well as the Persian Gulf, Arabian Peninsula, and East African coast. Crucially important to any such assessment would be reconsideration of the dating of cultural layers at both extremes of the chronological spectrum of the early medieval period—layers that have previously been dated to the early historic on the basis of early coins and diagnostic pottery types, as well as layers that have been ascribed a later date on the basis of Islamic Glazed Wares. Only after such reassessment will it be possible to reconsider what needs to be done, at the levels of both individual sites as well as the wider archaeological approach to the study of the period in general.

At the same time, such a reassessment would need to be coupled with a comprehensive program of survey and excavation of both religious and secular sites. Religious sites need to be conceived, approached, and investigated in ways other than those that consider them repositories of monumental remains. It needs to be recognized, as is the case with the archaeological examination of religious sites in earlier historic periods, that the sites extend beyond the central monument(s), and that they were foci of any number of human activities.[45] It is the material traces of these activities that can be investigated through broadening the scope of investigation; excavating wider areas of sites to examine other activities that took place, in addition to looking at the wider landscape context to see how these activities were related to wider societal dynamics that were taking place around them.

Furthermore, it is critical that more excavations be carried out at settlements, both at sites that have already been excavated, in order to investigate areas of early medieval occupation that may have been missed, as well as sites that have never been investigated, looking at settlements that were founded during the early medieval period and broadening the focus to include the investigation of rural sites for the first time. It is only by means such as these that a more representative picture of settlement history and urbanism can be developed.

Given the limited (and limiting) nature of the results of earlier excavations, it is also crucial that excavation of both religious sites and settlements should be carried out with the more modern methods now generally available to archaeological practitioners. These include, but are by no means limited to: an in-depth understanding of and sensitivity toward archaeological contexts and features (as discrete depositional entities); geoarchaeological analyses; and scientific dating methods in the design and implementation of excavation strategies. This is not to suggest that there is no awareness of such theories and methods of investigation in South Asian archaeology. Nor is it the intention to dictate which methods should be used; numerous potential excavation strategies are, after all, available, all of which could be used to good effect. Rather, it

is to note that, bearing in mind the problems caused by continued small-scale and uncritical application of Wheeler's methods of vertical excavation, the crude identification of stratigraphic layers, and somewhat simplistic conflation of soil layers with occupation phases, it seems advisable that all new excavations of early medieval sites prioritize the implementation of at least the bare minimum standards now available.

By undertaking such approaches, archaeology will exponentially increase the amount of evidence available, and enable us to say far more about the period than is currently possible. It is not enough to simply undertake more archaeological excavations, however. While the resulting evidence would no doubt improve our understanding of various aspects of the early medieval period, excavating sites for the purpose of generating archaeological evidence is not sufficient in itself. To do so risks perpetuating the situation in which archaeological evidence is used solely to answer questions derived from documentary history, and supports interpretations and conclusions made on the basis of documentary sources. As has been noted elsewhere (Abraham 2009), archaeological investigations need to be targeted—designed and undertaken in response to research questions in order to avoid ending up with yet more (biased) data of limited value. Just as important as revitalizing the doing of archaeology, therefore, is a need to stimulate thinking about the sorts of questions that archaeological research can ask of the period. As it has been amply proven in the archaeological investigation of other periods (that have traditionally been just as beholden to text-based historical research), archaeology can have its own set of questions that are of equal value (Chakrabarti 1999; Ray and Sinopoli 2004). These include different ways of approaching the wider historical questions, and, given the nature of the evidence (the material traces left by almost every aspect of human activity), questions that pertain to the much more focused level of individual action and expression. It is not the intention here to set out these questions; that would not only be beyond the scope of this article, but also beyond the remit of any one scholar to dictate. Yet, with the current state of historical understanding in mind, it is certainly possible to suggest some potentially useful starting points.

First, and given the continued uncertainties regarding the perceived abandonment of urban centers and decline in economic activities during the mid-first millennium, a useful step toward the re-invigoration of an archaeology of the early medieval period might well be to focus on the transition from the ancient to the medieval, and try to answer the outstanding questions regarding this transition: Was there a decline in occupation and economic activities at urban sites? If not, what changes did occur? Or, even if, as seems to be the case, sites were not abandoned, but rather there was a process by which the main area of occupation moved to new areas, this is still a change that requires appropriate investigation. Thus, excavations might be carried out at any of the sites where this occupation migration appears to have occurred in order to ask what changes occurred and why? On the other hand, with regards to new settlements that were founded during this period, one might ask why they were founded. What took place at these settlements and who established them? Given the connection between political and religious institutions attested by the giving of land grants during this period, what role did religious institutions play in these developments? Accepting that the nature of these changes would have differed in different regions at different times, this too provides a useful question that archaeologists could ask while framing research strategies: Exactly how did the developments that can be identified in one region differ from those in other regions, and why? Of course, it would be beyond the

scope of any single program of investigation and research to answer all of these questions. Yet these, and other related questions, are some that could provide valuable guiding foci in the design of new strategies for archaeological investigation. Moreover, with more grassroots archaeological work, all of these questions could be answered within a relatively short space of time.

Just as potentially fruitful as investigating the transition from the ancient to the medieval would be to focus archaeological attention on the transition from the early to later medieval period. On the broadest scale, accepting that there is an early medieval period, defined according to a commonly accepted set of criteria that separate it from the earlier ancient period, it is important to shift attention away from what separates it from this earlier period and look at how it relates to the later medieval in order to ascertain whether there was a real divide or distinction between the two, or whether it is simply a taxonomic divide. Importantly, it also needs to be accepted that just as the developments and dynamics that define the early medieval period differed in various regions and happened at different times, so too any transition between the two phases of the medieval may have occurred variously at different times. Targeted investigation of the activities that took place at sites during this transition on a region-by-region basis will improve understanding, not just of each phase of the medieval, but also the medieval in general. In this regard, considerable work has already taken place, or is currently being undertaken, which could provide an extremely useful starting point for such investigations. A number of excavations have been carried out at later medieval sites in various parts of North India, from which it has been possible to reconstruct an archaeological picture of settlements dating to the thirteenth century and beyond (Mehta 1979). Any and all of these sites could provide useful foci for the investigation of the earlier patterns and phases of settlement that precede them, thus enabling focused questions on the types of activities that changed: Was there a change? Why did it occur? How were wider political and religious changes, identified textually, actually manifested in socioeconomic dynamics and the lives of inhabitants of settlements? By the same token, recent work at the fourteenth- to sixteenth-century city-state of Vijayanagara in South India (Sinopoli and Morrison 1995, 2007) provides an ideal opportunity for comparisons to be made with excavations at earlier sites in the region.

Another area for enquiry, to further contextualize the early medieval in India, might include the wider international contexts of South Asia. Thus far, all developments, in addition to being conceived of and understood only historically and art historically, tend to be somewhat insular, giving little consideration to the ways in which South Asia interacted with neighboring states and peoples and the effects such interactions may have had on developments within India. There are, of course, exceptions to this trend (Chakravarti 1999, 2000, 2001a, 2002; Hall 2001a; Jain 1990, 2001; Ray 2004). However, by and large the study of international trade and interaction during the early medieval period is only just beginning. Here again, the impacts of the feudal model are discernible. The overriding sense is that during the mid-first millennium, international trade largely ceased or diminished to such an extent that it was no longer a major factor in wider socioeconomic developments. Much as the archaeological bases for these assertions have been gradually undermined (e.g., by Nanji 2011 and Tomber 2007),[46] and despite the existence of a number of textual sources from Arabia that Islamic traders were trading with India from the seventh and eighth centuries onward (Jain 1990; Wink 1990), the perception remains that even if trade did

exist, it would not have affected developments within India; it was, literally, peripheral (Pearson 2007). Yet this is by no means certain. As far as archaeology is concerned, there has, since Carswell's survey of the Indian coast in 1976 (Carswell 1977–1978), been renewed investigation of Indian Ocean trade. Although, in keeping with the general trends of archaeological investigation in the region, this has tended to focus on earlier periods (with a fixation on Indo-Roman trade and the identification of sites in India with places mentioned in classical Greek and Roman literature). By contrast, the early medieval period has been grossly ignored. In short, we know that international trade existed, but the nature of this trade and its effects on developments within India remain largely unexplored. All of these topics warrant further study. In this connection, land-based networks of trade and interaction with neighboring states in Central Asia and Himalaya need also to be further pursued.[47]

By expanding the area of archaeological inquiry in ways such as this, the discipline, particularly in relation to the early medieval in India, would undoubtedly develop its own sense of what is important about the period, define its own questions, and move beyond the parameters established in historical scholarship. Through such processes, South Asian archaeology (defined in the broadest possible sense, as a discipline with practitioners in both South Asia and elsewhere in the world) might also develop new underlying theories and methods of practice for the investigation of the period. In this connection, it is worth pointing out that those implemented in relation to the survey and excavation of prehistoric sites are already markedly different from those used to investigate the Indus Valley civilization, the Iron Age, and early historic periods. There is no reason why the same should not be case with regards to the study of the early medieval.

CONCLUSION

What should, by now, be beyond doubt is that there is a need for archaeological research on the early medieval period. From its inception, its study has been largely devoid of archaeological investigation. Instead, the period emerged from and has been defined by the study of documentary sources and monumental remains, mainly within the fields of textual history and art history. Archaeology has tended to be used only as supporting evidence, either for historically derived and textually driven theories, or else to provide the objects of art historical analyses. This has undermined the role of archaeological inquiry, limiting an awareness of its full potential and stifling any impetus for research within the archaeological community itself. This absence of archaeological research has had a serious and negative impact on our understanding of the early medieval. Due to a number of other theoretical and methodological concerns, the evidence that does exist cannot necessarily be considered representative of the period. This undermines theories that have been based on this evidence—not least those pertaining to a perceived decline in trade and urbanism. Moreover, a lack of archaeological research has precluded an understanding of early medieval material culture, meaning that the study of the period can only continue with recourse to the documentary sources and monumental remains. Arguably, it is precisely because the study of the period has always been based on such a limited range of evidence that it continues to be so poorly understood and ill defined. When considered in this light, it would seem that improved understanding of the developments that took place within the early medieval period, and clarification of the meaning and use of the term

medieval, can only be achieved through a fundamental change in the way that the study of the period is approached. Central to this has to be reinvigoration of archaeological research.

Quite what form this archaeological approach could take, however, is another matter entirely. On a practical level, any such revitalization must surely involve both a reassessment of the existing evidence as well as renewed survey and excavation of sites (both religious and secular). Furthermore, and bearing in mind the limitations of earlier work, it is imperative that hands-on investigation should be accompanied by, and derived from, a framework of targeted research (and researchers) that attempts to consider this period from an archaeological perspective—framing archaeological questions that can challenge and complement existing historical models, and contributing to the development of a coherent body of archaeological theory. In this connection, a number of potential questions and avenues for future research have been suggested. The benefits of an archaeological approach to the study of the period would be great. Yet, in saying this, no attempt is being made to shy away from the fact that the problem of an absence of early medieval archaeology cannot be solved overnight. Reinvigorating the archaeology of the early medieval is impeded by limited funding as well as the existence of other priorities at both governmental and institutional levels in both India and internationally. At the same time, such factors should not dissuade archaeologists. While the current situation is certainly disheartening, the prospect of being able to say something new about the period is also exciting. Such is the scale of the problem that faces us that even single individual research projects on small, focused aspects of the period will make valuable and welcome contributions. It is, after all, only through such grassroots endeavor that the current situation will change.

ACKNOWLEDGMENTS

NOTES

1. The effect of the history of the scholarship on India's past on contemporary research is well recognized, and the historiography of this period has been well studied (Ali 2012; Ali and Sengupta 2011; Inden 1990; Singh 2004; Thapar 1978). It is not the intention here to explore this further. Rather, the focus of this discussion is the perceptions of the main developments that defined the past in general, and the medieval in particular.
2. The practice of periodization in European historical scholarship, and, indeed, the origin of many European modes of thought regarding the medieval, can be traced back to at least the fifteenth century C.E., with Renaissance scholars such as Leonardo Bruni (1442) labeling different periods of time in order to define them as separate and distinct from the "modern" Renaissance era. However, the precise terms ancient, medieval, and modern do not appear in historical writings until the beginning of the nineteenth century, with the first use of the term medieval in English appearing in 1817 (Fosbroke 1817 : vi).
3. Early incarnations of the idea of Oriental Despotism were already apparent in the works of European classical historians, such as Edward Gibbon (1776–1788, vol. 1, ch. 6), who posited the increasing use of Oriental-style despotic governance as one of the contributing factors that led to the decline of the Roman Empire. Arguably, this notion can be traced back even further to the works of Roman historians, such as Thucydides and Herodotus.
4. For histories of archaeological research in India, see Chakrabarti (1998, 2003), and Singh (2004).

5. Thanks to the growing popularity of translations of ancient religious texts by Orientalists such as Max Müller in Europe, Buddhist texts especially came to be very influential in the formulation of the writings and works of the German Romantic movement. See, for instance, the works of Schelling (1803), Schleiermacher (1831), and Schopenhauer (1819).
6. Early explorations and excavations of many Buddhist sites, such as Belar, Kabul, Manikiyala, and Rawalpindi, yielded Indo-Greek coins, as well as sculptural remains exhibiting striking stylistic similarities with Hellenistic examples. See Burnes (1833), Gerard (1834), Masson (1842).
7. Much has been written about the discovery of ancient Buddhism in India and the ways in which its study was influenced by wider colonial agendas. See, for instance, Almond (1988), Guha-Thakurta (1998), Leoshko (2003), Lopez (1995), and Singh (2004).
8. One notes, for example, the growth in popularity of studies of social organization (Karve 1961; Srinivas 1962), political structures (Drekmeier 1962), and economic systems (Habib 1963; Kosambi 1956). The factors underlying such a shift in emphases and intellectual concerns are themselves a large and important area of study, a full review of which is beyond the scope of the present study. They include: the gradual professionalization of academic history (which, in the twentieth century, invited social theory as a boundary marker to define its remit); the institution of literacy and education programs that called for histories that could be presented to the general public rather than to traditional elites; and the wider political climate following both the end of the Second World War, generally, and Indian independence specifically.
9. To a certain extent, the fact that both D. D. Kosambi and, later, R. S. Sharma drew so heavily on the works of European Marxist scholars should not be surprising. Both had been educated in the West (Kosambi having spent many of his formative years at Harvard, where his father taught, and Sharma having written his doctoral thesis at the School of Oriental and African Studies), where they had been exposed to and were undoubtedly influenced by what were then dominant trends in historical scholarship and social and economic theory. It is also important to note that as Marxist historians, recourse to such modes of thinking would not have been viewed in terms of a transposition of European ideas on to the Indian past. By the 1950s, Marxist theory was a fully developed orthodoxy, which saw feudalism as a universal stage of historical development. To a Marxist historian, the history of any country would therefore follow one of only two possible models: either it had a feudal period, or it had "no history" (except that of ancient empires and colonial subjugation to capitalist states).
10. For examples of the role of nationalism in the development of medieval archaeology in many European countries after the Second World War, see the various case studies in Díaz-Andreu and Champion (1996), and Graves-Brown et al. (1996).
11. The apparent unification of large parts of the subcontinent ruled by a single, centralized political power was identified as the natural precursor to the modern nation-state of India. That such associations were so consciously made is reflected in historical writings of the time, and is readily apparent in the iconography of the newly independent state. It is, after all, no coincidence that a Mauryan pillar capital was adopted as its official symbol.
12. Much as theories of feudalism in South Asia continued to develop in accord with those set out by European Marxist historians (who had identified the same process of decline, feudalism, de-urbanization, and monasticism as characteristic of the European medieval), scholars of Indian feudalism were at pains to stress that this apparent coincidence was simply the result of invoking a comparative framework, and that the Indian feudal system differed from its European counterpart in important respects, mainly in the precise formulation of feudal structures, such as the presence or absence of manorial systems. For an in-depth discussion, see Jha (2000b).
13. Ironically, the same European Marxist histories that inspired the concept of Indian feudalism had by this time constructed India as part of the Orient, which was argued to have drained Europe of bullion through trade surplus and hoarding money, thus explaining the emergence of feudal society in Europe. See Bloch (1933).
14. For instance, the emergence of a feudal economy was also identified as having been based on, and at the same time contributed to, a perceived breakdown of the preexisting social order evidenced by a proliferation of castes (Sharma 1978, 1982; Yadava 1975, 1979). Further, the feudalization of society was supposed to have led to the development of localism and the formation of culturally distinct regional units, visible in the appearance of regional literatures and styles of architecture and sculpture (Jha 2000b:25). The style and subjects of many of these sculptures were also interpreted as reflecting the new social order (Sharma 1996). Alongside these developments were also fundamental changes in religious practice and philosophy (Jaiswal 1967; Nandi 1973, 1986), marked by the formalization of the *puja* system of worship by making offerings (Einoo 1996) and the doctrine of *bhakti*, or personal devotion to deity (Jaiswal 1967), both of which were interpreted as manifestations of the wider society's subservience to and dependence on temple landowners. In addition, we see the emergence of Tantricism, which was interpreted as being one of the ways in which *brahmanas*, when moving into new areas of the subcontinent, could appropriate and incorporate tribal deities and consolidate their cultural hegemony (Jha 2000b:26; Sharma 1974a). For a useful review of the main trends in feudal scholarship since its inception, see Jha (2000b).

15. Indeed, such an interpretation seems to have been influenced mainly by Pirenne's (1936) thesis regarding the origins of European feudalism following the disruption of maritime trade with the rise of Islamic power in the Mediterranean Basin. To apply this idea to the context of India was also to imply the lack of any built-in potential for economic or societal change within India itself, and reflected Marxist theories of the Asiatic Mode of Production, which denied any capacity for self-determination within India.

16. The practice of land grants differed temporally and geographically (Singh 2011), and different political structures (chiefdoms, tribes, kingdoms, and states) co-existed across different regions (Kulke 1982, 1993).

17. For critical evaluations of Stein's model, see Chattopadhyaya (1994), Dirks (1979), Heitzman (1987), Jha (1993), Sharma (1990), Shrimali (1993), and Veluthat (1993).

18. Integral to this was the reinterpretation of the earlier "classical" empires of the Mauryans and Guptas as more loosely organized nodal states instead of centralized or bureaucratic ones (Fussman 1987; Thapar 1987).

19. This is despite a recent and somewhat worrying trend of publishing edited volumes of reprints and works, which summarize, and thus perpetuate, the various feudal and alternative arguments (e.g., Chattopadhyaya 1994; Jha 2000a; Kulke 1995b; Mukhia 1999).

20. It is not the intention to review all current scholarship on the early medieval period here. This amounts to a considerable body of literature. Yet, it is possible to highlight several thematic areas of research that dominate current scholarship. For general studies on the structure and formation of the state, see Champakalakshmi et al. 2002; Chattopadhyaya 1995; Hall 2001b; Kulke 1995b; Nandi 2000; Shrimali 1992; and Veluthat 1993. Within this general body of scholarship, other studies focus on specific dynamics, such as the relationship between religious and political structures (e.g., Heitzman 1997; Kulke 1993; Singh 1994). In terms of trade and urbanism, a number of studies seek to examine the relationships between political and religious institutions in the developments of trade and commercial networks, expanding agricultural hinterlands and urbanism (Chakravarti 2001b; Champakalakshmi 1996), as well as specific studies into the role of merchants and guilds (Abraham 1988; Chakravarti 2001a; Champakalakshmi 2001; Hall 1980; Jain 2001), the nature of trade and mercantile activity (Chakravarti 2002), and the relationship between international trade, changing social structures, and urban development (Chaudhuri 1985; Jain 1990; Liu 1996; Ray 2004). Moving away from traditional preoccupations with the socioeconomic, Daud Ali (2006) has invested the study of Sanskrit texts and inscriptions with new perspectives in the examination of the culture of political life in the royal court. Other literary studies, by far and away a minority area of research, examine the emerging rich vernacular traditions (see Pollock 2006), as well as inscriptions and religious texts, to reconstruct developments in religious identity (Alam 1989; Gilmartin and Lawrence 2002; Lorenzen 1999). In the fields of art and architectural history, a number of studies are devoted to the development of building practices and style (e.g., Dhaky 1996, 1998; Guy 2007; Hardy 2007; Meister 1999; Meister and Dhaky 1991, 1999; Meister et al. 1998; Michell 1977, 2001), as well as what they can tell us about changing religious beliefs, practices and ritual (Branfoot 2007; Willis 2009).

21. Indeed, the chronological divide between the early medieval and medieval periods continues to be commonly defined on the basis of the foundation of the Delhi sultanate in 1206 c.e. While it may very well be the case that new Islamic dynasties ushered in a variety of social, economic, and political changes, which may well warrant a terminological change from an "early" to a "later" phase of the medieval, this has not been adequately investigated. Furthermore, and as many historians working in South India have pointed out (e.g., Karashima 2009), defining the transition from the early medieval to medieval on the basis of Islamic rule does not account for the fact that other parts of the subcontinent did not come to be ruled by Islamic dynasties until much later—a fact that has led a number of scholars to experiment with different terminologies and chronological parameters, or else to come full circle and return to the question of the use and application of the term medieval in South Asian history altogether.

22. Archaeological research on the earlier periods each constitute significant bodies of research in their own right, and it is not the intention to offer a review of the substantial amount of literature pertaining to these bodies of scholarship here. Archaeological research on the later medieval period in North India has been due, and remains largely limited to, the works of Ramanlal N. Mehta (1979). For recent research on Vijayanagara, see Sinopoli and Morrison (1995).

23. This figure is based on a review of the *Annual Reports* of the Archaeological Survey of India, the *Memoirs of the Archaeological Survey of India*, and the annual *Indian Archaeology—A Review* series published since 1953. A distinction has been made between religious monuments that have been have been "cleaned" and reconstructed (which are not included in this tally), and those that have been excavated.

24. At this juncture, it must be stressed that the aim of referring to earlier works is not to engage in unwarranted negative criticism. Many archaeologists from both South Asia and the West were operating (and continue to operate) within the existing scholastic framework, and any perceived shortcomings

in either approach or interpretation should not be taken as a criticism of the efforts being made. The intention is rather to point out, with the benefit of hindsight, where works might be lacking.

25. Recent exceptions to this trend include excavations at the coastal sites of Chaul (Gogte 2003; Gogte et al. 2006), Sanjan (Gupta et al. 2004a; Gupta et al. 2002, 2003; Gupta et al. 2005), and Vizhinjan (unpublished), all of which were carried out specifically in order to investigate known early medieval sites.

26. As with figures previously cited for the number of excavated temple sites, this figure has been derived from a thorough review of the published data, including excavation reports in article and monograph form, as well as references to excavations that have taken place in the annual reviews of archaeological works published by the Archaeological Survey of India.

27. For the report on Subrahmanyam's excavations of the temple complex of Rani-ki-vav, see Ghosh (1969 : 12).

28. For an in-depth review and discussion of these methodological issues, see Kennet (2004b, 2013).

29. There are numerous archaeological and historical examples of large cities where core areas of urban occupation shifted over time, such as Delhi and Taxila. Still today, towns and villages will have expanding areas of new habitation or economic activity, while other areas are left unoccupied or unused for some time. For a thorough and useful review of the development and changing fortunes of cities in South Asia, see Heitzman (2008).

30. For instance, excavations on Mound 1 at Bhokardan (Deo and Gupte 1974 : 17–18), resulted in the definition of only six cultural layers for a period spanning some nine or ten centuries. Similarly, at Nagara (Mehta 1968), the excavation of Mound III (Trench IV) resulted in the definition of eight cultural layers accounting for nine centuries of occupation history; and at Somnath (Nanavati et al. 1971), the excavation of Trench IV resulted in eight layers for a period spanning ten centuries.

31. For a useful grounding in and review of the wide range of different excavation methods and strategies currently used in archaeology internationally, see Carver (2009).

32. At Nagara, for instance, Mehta (1968) presents a chronology of the site based on the findings from excavation trenches in three separate settlement mounds. Although contiguous to each other and dating roughly to the same chronological span, these mounds are physically distinct from one another and may thus represent very different patterns of occupation. At Baroda (Vadodara), Subbarao (1953) attempted to construct a master stratigraphy linking the cultural layers found in two areas despite being recorded as two separate sites: the town of Akota and the town of Vadapadraka. This is notwithstanding the fact the occupation histories of both sites are recorded as being very different from one another, which, together with the specifics of the environmental and geographic settings of each locale would have meant that dissimilar depositional processes were involved in the formation of archaeological deposits at each site.

33. Radiocarbon analyses of two charcoal samples from early historic layers at the site of Nagara, Gujarat (Mehta 1968 : 19) and three samples from Sonkh, Uttar Pradesh (Härtel 1993 : 85–87) constitute two of the only exceptions to this trend.

34. This fact, and its interpretation, is a matter of considerable discussion (e.g., Chattopadhyaya 1977; Deyell 1990; Sharma 1987). For a useful discussion of the problem of lack of archaeological visibility of coins dating to the early medieval period, see Kennet (2013).

35. This rule states that any archaeological deposit must be dated to the same date as or later than the oldest artifact found within it.

36. This situation is not helped by the almost complete absence of radiocarbon (or any other scientifically derived) dates for historical periods. One cannot help but imagine that if such analyses had been carried out, then a completely different approach toward the dating of coins would have emerged in archaeological scholarship—one that recognized their potential residuality through a variety of human actions, including continued use, hoarding, and reappropriation as "heirlooms."

37. See Mehta (1968) for one of the earliest (and most frequently cited) discussions of the dating of Glazed Wares in India.

38. Here, one can cite recent excavations at Anuradhapura in Sri Lanka (Coningham 2006), Chaul in Maharashtra (Gogte et al. 2006), Pattanam and Vizhinjam in Kerala (both still largely unpublished), and Sanjan in Gujarat (Gupta et al. 2004a; Gupta et al. 2002, 2003; Gupta et al. 2005; Nanji 2011).

39. Red Polished Ware was, of course, initially misidentified as Roman terra sigillata. It was recognized as an indigenous Indian tradition by Subbarao (1953), who also put forward the current chronological framework. This work has since been complemented by the results of subsequent excavations, most notably at Amreli (Rao 1966), which have provided further evidence for the ware having originated in Gujarat.

40. In East Africa, one can highlight the sites of Manda (Chittick 1984) in the Lamu archipelago and Unguja Ukuu (Juma 2004) in Zanzibar. In the Persian Gulf, Red Polished Ware is widely reported from a number of coastal sites, and has been found in contexts securely dated to the seventh and eighth centuries at sites including Hulaylah (Sasaki and Sasaki 1998) and Kush (Kennet 2004a) in the United Arab Emirates.

41. There is, of course, a vast literature on the theory and methods of history and art history in both South Asia and internationally. For a useful discussion of the nature of the sources in the writing of history in India, see Chakravarti (2009) and Chattopadhyaya (2006). For more general works on historical theory, see Fulbrook (2002) and Partner and Foot (2013). Standard critical works on the theories and methods of art history include: Cheetham et al. (1998), Fernie (1995), Pacht (1999), Peziosi (1998), Sears and Thomas (2002).

42. As part of a general shift in emphasis away from a traditional preoccupation with the social and the economic dimensions of the state, some historical studies have begun to explore the topic of religious identity through considering the ways in which different religious groups portrayed each other in the texts and inscriptions they produced (e.g., Chattopadhyaya 1998; Wink 1992). Others have begun to explore the construction and negotiation of gender identities in society and religion through examination of the role of women in religious practice and patronage (e.g., Orr 2000; Pintchman 2007). Similarly, in an attempt to move beyond traditional formalist assessment and iconographic interpretations, a number of art historical and architectural studies have begun to consider the wider contexts within which monuments were built and sculptures carved in order to investigate their meaning and importance at both individual and societal levels (e.g., Babb et al. 2008; Eck 1981; Willis 1997).

43. Although by no means all translated, the total number of inscriptions pertaining to the Chola Empire in Tamil Nadu amount to some 10,000 individual records (Heitzman 1987 : 37). This accounts for the majority of inscriptions dating to the early medieval period across the subcontinent.

44. In Gujarat, for instance, the construction of Shekhari temples under the Solanki dynasty between the tenth and thirteenth century outnumbers earlier building works.

45. See, for example, the ways in which the examination of early Buddhist sites and monuments, such as Amaravati (Fogelin 2006), Bharhut (Hawkes 2008, 2009), Karad (Rees 2010), and Sanchi (Shaw 2007) have been invested with new perspectives.

46. The increasing realization that many Islamic Glazed Wares found throughout India, especially at sites in littoral zones, actually date to the mid- to late first millennium c.e. and can be taken as indicative of active trade links between India and the Persian Gulf has already been discussed. Yet, in this connection, it is also worth highlighting Roberta Tomber's (2007) pioneering work, which demonstrates that a number of ceramics found at Indian sites that were previously identified as Roman amphorae dated to the early centuries c.e. are in fact torpedo jars that can be dated up to at least the seventh and eighth centuries.

47. This was, after all, the period when South Asia would have been connected with the Silk Route through the emerging states of Nepal and Tibet.

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ABSTRACT

The concept of an "early medieval" period (c. 600–1300 C.E.) in the study of South Asia's past is well established, yet remains ill defined and poorly understood. As a result, debates regarding grand explanative frameworks, not to mention the meaning and use of the term medieval, have dominated the study of the period. Important though these concerns are, what underpins them, and something that is rarely considered, is how sources and methodologies affect the study of the period. Historiographic review of scholarship on the early medieval reveals that from its inception, the period has been studied exclusively through the examination of documentary sources and monumental remains

within the fields of history, literary and religious studies, and art history. Archaeology has been used to support historical theories, largely in order to provide further empirical "proof" of a perceived decline in trade and urbanism. The continued use of archaeological evidence in this way has meant that the full potential of archaeological inquiry has not been fulfilled, and the impetus for new archaeological research has been stifled. As a result, the early medieval is arguably the most poorly represented period archaeologically in the entire subcontinent. Critical assessment of the limited amount of archaeological evidence that does exist reveals a number of methodological and theoretical concerns that bring into question its applicability and use. These shortcomings not only force one to question historical interpretations, but also limit what can be said, archaeologically, about the period. It is argued that many of the wider uncertainties surrounding the definition and meaning of the early medieval stem from this absence of archaeological research. What is urgently needed is a revitalization of the archaeological approach to the study of the period; some ways are suggested in which this might be achieved in terms of methodological approaches, and questions that could be asked. KEYWORDS: archaeology, early medieval, historiography, history, India, method, theory, South Asia.