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Chapter 15
Paithan Excavation: Historical, Archaeological, Geographical and Epigraphical Contexts

The importance of the Paithan excavation goes beyond the site itself and touches issues central to the history of India and the profound changes in politics, society and economic relations during the early centuries AD. That the transition from the Early Historic period to the Medieval was a crucial moment for India has long been noted (e.g. Kosambi 1956; Sharma 1965; Thapar 1968). Impacts were felt not only in established urban centres such as Paithan, but in marginal zones that appear to have been transformed by land grants and the foundation of Brahmanical temples (e.g. Kulke 1995b; Willis 2009: 159, following Bakker 1992). Yet while historians have done much to develop and modernise the study of India's transition to the Medieval, archaeologists have made only modest contributions to the subject (a problem pointed out in, for eexample, Singh 2011, Hawkes 2014b). Unfortunately, the ways archaeology has been practiced in South Asia have not always been conducive to resolving the questions that historians have raised. Archaeology is essential, however, because many changes are outside the concern of Indic literatures and epigraphic texts and thus not recorded in them. The evidence from Paithan is accordingly crucial. It furnishes not only information about changes at the site itself, but provides a starting point for new programmes of research that can tackle salient issues at the regional and pan-regional level.

History and Archaeology

Looking back, there are a number of reasons for a lack of exchange between history and archaeology in the study of South Asia's ancient and Medieval pasts. Primary among them has been inherent differences between the disciplines and how they are practiced in the Indian context. Historians tend to focus on political, social, religious and economic change, while archaeologists of the Early Historic period—up to now at least—have focused on tackling sites, establishing chronologies, developing artefact typologies, identifying coins and documenting phases through stratigraphy. For the Medieval period, the principal datasets tend to be different, primarily temple architecture, inscriptions and sculpture. There is little communication between history and archaeology and the disciplines rank their priorities in different ways. Institutional venues also differ, with archaeology led primarily by the Archaeological Survey of India and state departments of archaeology, while history rests in university departments. This is further complicated by the fact that where archaeology is studied and practiced in universities, it often tends to be within departments of history where it is deemed a sub-discipline.

We begin with the historical side. Over the last fifty years, historians have generated a variety of narratives embedded in an historiography that is particular to India. The literature has been reviewed by Daud Ali (2012, 2014). These articles are essential reading and necessary points of departure for anyone seeking to understand how Indian history—and the understanding of the Medieval in particular—has developed. As Ali (2014) noted, the study of the Medieval "has been vexed by issues of chronological uncertainty, obscurantism, communal distortion and heavy model building." The levels of obscurantism are usefully introduced by one example: Vishwa Mohan Jha's (1996) review of B. D. Chattopadhyaya's (1990) *Aspects of Rural Settlements and Rural Society in Early Medieval India*. This review, chosen from an extensive discourse, gives insight into the parochial nature of much of the historical scholarship on the Medieval. Jha does not cite the book he is reviewing because the readers of the *Indian Historical Review* are expected to know, which of course they do. But aside from this indicative silence, the patience with which Jha has read Chattopadhyaya's complex arguments, understood them and seen links across the script that normal readers would miss and Chattopadhyaya does not bother to point out and, moreover, the rigorous attention to which Jha has subjected Chattopadhyaya's interpretations and inserted his own carefully worded and poignant observations, are as commendable as they are remarkable. Outsiders will be baffled. The performance is readily explained, however, by the fact that B. D. Chattopadhyaya was the professor of history at Jawahar-

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lal Nehru University until his retirement in 2004 and, following R. S. Sharma's retirement from Delhi University in 1985, the father of the 'Delhi school' and the leading voice for historical studies in India. From his position of eminence and power, Chattopadhyaya (1998) meticulously explored what he termed the 'twin burdens' of history: historiography and the written sources. He was not burdened by fieldwork. Throughout his life, Chattopadhyaya described himself to his students as a desk-based scholar with no pretensions to field study (Masahiko Mita, personal communication, Nagoya, June 2019).

This is understandable, up to a point. Chattopadhyaya, like Sharma and other luminaries of historical scholarship, were historians and not archaeologists. Indeed, many were at pains to point this out—explicitly stating the value of archaeological fieldwork and articulating what archaeological findings might be useful in their research (see, for example, Sharma 1983, 1987). However, for reasons that we will turn to later, this archaeological work was not forthcoming and in its absence no attempt was made by historians to venture into the landscape to see what materials might be found to support their investigations. The consequent limitations of this are apparent in the slow pace of development in Chattopadhyaya's ideas, although whether a dose of fieldwork would have clarified his thinking (and that of others) is moot. The literature is dense and complicated, made more so by the vigorous counter-arguments written by Chattopadhyaya's academic opponents at each step. From our own side, we would observe only that for every theory or proposition Chattopadhyaya has put forward, a qualification or variant theory seems to appear somewhere in his extensive writing. To be fair, Chattopadhyaya did not hold a single view, but rather refined his interpretative position over a long career. With his book *The Making of Early Medieval India* (Chattopadhyaya 1994) his arguments were articulated in mature form. There he accords Early Medieval society, from the time of the Gupta dynasty, with several key socio-economic and political features: an increased clearing and settlement of uncultivated lands (often through the deployment of Brahmins and land grants), the growth of networks of rural settlements, the growth of new political lineages, the transformation of hinterland and forest societies into 'state-societies', the peasantisation of cultivators and hunter-gatherers as a part of this process and the concomitant incorporation of non-caste peoples into the caste hierarchy. Ali (2014) may be consulted for a guide to the development of these ideas.

Chattopadhyaya's thinking emerged from a primary focus on the nature and constitution of the state as understood by historians of the colonial and immediate post-colonial period. During this time, historical scholarship in India borrowed much from the economic, social and anthropological theories that were emerging in Europe and America. Ideas of feudalism and associated socio-economic changes (after Bloch 1933, 1939; Pirenne 1936; Duby 1952) and social and cultural evolution (Childe 1951) all found their way to the writings of historians in India. Yet over time, as that scholarship became weighed down by arguments and counter-arguments, it also became more circumscribed. The degree to which the historiography of the Delhi school (and Calcutta at the formative stage) was inward looking and self-referential is shown by the ways in which new methodologies were ignored or rejected summarily. Scholars like Ronald Inden, Burton Stein and Nicholas Dirks, influenced by powerful new trends in anthropology and sociology (which had continued to evolve on the international stage), developed innovative forms of historical analysis and wrote pieces that are now regarded as classics. Inden (1985), inspired by A. M. Hocart (1950), proposed that the state was an 'imperial formation', constituted as an evolving hierarchy of human and divine lordships (see also Inden 2006). Stein (1980) famously drew on the study of acephalous societies in Africa to propose a 'segmentary' model of the Cola state, while Dirks (1988) created what he termed 'ethnohistory' by mapping the changing roles of 'big kings' and 'little kings' in Tamil Nadu. Daud Ali (2014: 390) paused to reflect on these contributions, saying: "Interestingly, this literature and its categories, whether as segmentary polity, ethnohistory or imperial formation, generally did not articulate clearly with the trends and camps of Medieval historiography well established in India and were largely ignored or refuted, though their contributions have arguably been just as formative for later developments in the field."

Ali's detached analysis does him much credit. The inability of the Delhi school to take on ideas from the outside deserves, however, a harsh rebuke. The problem is highlighted by James Heitzman's (1997) *Gifts of Power: Lordship in an Early Indian State*, a work that involved a reappraisal of temple institutions in their economic, historical and political landscapes. This is a book of such innovation and creative scholarship that its lessons are being assimilated but slowly. Heitzman surpassed much in the historiography of loosely Marxian affiliation, but his cross-cultural comparisons were greeted with a facetious dismissal by R. Champakalakshmi (1998). The impact of Heitzman, Inden, Stein and Dirks on the Delhi school has been minimal, in part because their works were from outside the school, but also because most of the publica-

tions deal with the Cola state in Tamil Nadu—a foreign land and cultural anomaly as far as many inhabitants of Delhi are concerned. The circumscribed geographical vision shows also in the impact of Upinder Singh's (1994) *Kings, Brāhmaṇas and Temples in Orissa*, an excellent work informed by a careful analysis of inscriptions and an understanding of the theoretical positions then prevailing, especially evident in her cogent critique of Sharma's theories about Medieval feudalism. If David Lowenthal (1985) is right in believing that *The Past Is a Foreign Country* then, for the Delhi school at least, Orissa and Tamil Nadu are removed both spatially and temporally—so doubly foreign.

The impact of anthropology and sociology on historical writing has already been noted. More influential has been the *Annales* school with its emphasis on long-term social history and, in more recent manifestations, its focus on the *histoire des mentalités*. Subaltern studies—an examination of the colonial populations who were (and are) socially, politically and geographically outside the hierarchies of power—has a genuine base in Indian society and an intellectual reality beyond the abstract debates about the nature of the state. Ironically, however, the approach was championed by academics in Europe and America. The leading figures—Homi K. Bhabha, Gayatri Chakravorty Spivak and Ranajit Guha to name the main players—work (or worked) at Harvard, Columbia, Sussex and the Australian National University. Nor is their style of analysis indigenous, showing as it does an ultimate debt to Antonio Gramsci to whom we owe not only term 'subaltern' but a general theory of 'cultural hegemony' based (yet again) on Marxist principles (see, for example, Gramsci 1995, 1996).

Readers of this chapter may think us somewhat dated in rehearsing the intellectual fashions set by grandees of social and literary theory in the 1990s for a book completed in 2020, especially since authors like Chatterjee (2012) have reflected on how subaltern studies has given way to a more nuanced cultural history. There is, however, remarkable deference to authority on the Indian academic scene and a reticence to engage in robust critiques. We will not here explore the networks of patronage and influence that support the status quo and that are, to varying degrees, characteristic of elite academia everywhere. Rather, we will content ourselves with a few words on approaches to the study of subaltern history as exemplified by the works of Gayatri Chakravorty Spivak. Her (1988) essay "Can The Subaltern Speak?" is regarded by some as a seminal text in subaltern studies. The ramifications in the literature is extensive, but a start can be found in Maggio (2007). As is well known, Spivak was concerned with *sati*, the self-immolation of wives on the funeral pyres of their husbands. This is a perennially debated topic, especially in the on-going contest between liberal and conservative thinkers. Spivak explored the race and power dynamics involved in the banning of *sati* under British rule, noting that what we know of the matter comes only from the accounts written by British and Hindu reformers. We never hear from *sati*-performing women, a problem that prompted Spivak to reflect on whether the subaltern can speak. Looking at the documentation of *sati* in colonial archives, Spivak attempted to show that western scholars have assumed that their sources are objective and assumed also that the 'oriental other' is anonymous and mute. However interesting this seems, we will turn the pages of Spivak in vain looking for a quotation from a *sati* pillar inscription, and this despite the fact that these inscriptions survive in great numbers. To put the matter in plain language: subalterns are certainly able to speak, but if we want to hear their voices we will have to listen, and listen in their language. More precisely, we will have to read what was written on the spot, and look at how the *sati* was represented in life and death. This subject is not without relevance to the present book and its concern with the transition to the Early Medieval. Although inscribed memorial stones of this kind belong mainly to Medieval times, the oldest *sati* in India is at Eran, on the ancient route to and from the Deccan—and so also Paithan (Fig. 15.1). It is dated AD 510–11 (Fleet 1888: 91–93; Bhandarkar *et al*. 1981: 353–54). The text of the inscription has been known for more than a century, but the pillar was only located and photographed recently by Peter Bisschop as part of the *Beyond Boundaries* project. The pillar and its documentation are available online (Siddham OB00045).

Fig. 15.1: Eran (Dist. Vidisha, Madhya Pradesh). Inscribed memorial stone of Goparāja, dated AD 510–11.

The relative lack of intersection between history, theory and field archaeology as practiced in India—thrown into high relief by the Eran pillar—characterises subaltern studies and the degree to which those engaged in this mode of historical research have not engaged with the archaeology of social memory, even when *sati* itself is the subject. In a landmark study, S. Settar and Günther-Dietz Sontheimer (1982) focussed on the Deccan and worked in a way that showed it is not for historians and ethnographers to decide if the practice documented by *sati* memorials is good or bad, but rather to record that it happened and to understand what these memorials meant and mean to the people in whose places they are found. Certainly in our own travels in Central India we found that the stones are often preserved and respected, the focus of prayers and offerings (Fig. 15.2). They are taken as signals that women long past were heroic and that they, even now, stand as moral exemplars. These memorial stones fill the countryside and survive in their thousands, making the landscape a place populated by events and people. Historians often lament the lack of sources in the periods they study and rightly note that written texts do not tells us much about everyday people. But such observations ignore this vast archive in stone. It is one of the largest archives in the world and it is, in fact, an archive without parallel anywhere. If we reflect on the scale of the corpus, we soon realize that an entire dimension of Indian history and social life is absent from the ways historians normally depict the past: it is as if individual people—and the actions they deemed worthy of record—have been banished from the scene. Once we take notice of these memorials and the memories they embody, we find that far from lacking local and subaltern histories, India is filled with them. The reservoir is so deep, wide and varied—and so densely filled with undigested data—that it presents an impossibly huge and daunting task. No single person could cope with it, least of all a desk-bound scholar constrained by liberal ideologies that render the very subject repugnant.

This excursus brings us back to the central problem raised in this chapter, namely a lack of connection between history and data collection in the field. What is particularly evident is that history is based on re-cycled data and derivative methods. As a result, the analyses produced are little more than re-configurations of existing information and the historical propositions put forward mere hypotheses. Kennet (2013), and again in the introduction here, has shown that R. S. Sharma's influential theory of urban decline, proposed first in 1972 and a dominant theme in historical discussion since, has no substantive basis but has enjoyed influence because

Fig. 15.2: Salkanpur (Dist. Sehore, Madhya Pradesh). Hero-stone memorial with a pinnacle in the shape of a temple spire, with a relief sculpture of warriors in battle below, circa 12th century.

it was based on an archaeology that was deemed "unassailable" (Sharma 1972, 1987). Thanks to this assumption, the urban decline that took place at the end of Early Historic times was, in the words of Chattopadhyaya, "not a matter for speculation" (Kennet 2013: 334, citing Chattopadhyaya 1974, 1986). However, a close reading of the archaeological reports shows that the theory of urban decline is to some degree an historiographical illusion, generated by the excavation strategies of the archaeologists and a selective reading of their publications on the part of R. S. Sharma (Kennet 2013: 334; Hawkes 2014b). The critique is essentially Foucauldian in that the historical picture created by R. S. Sharma simply rehearses the perceived exclusions of the archaeological, numismatic and epigraphic archives. So what is the upshot? It is simply this: *contra* Chattopadhyaya, urban decline is very much a matter for speculation. And while Chattopadhaya is correct to say that archaeology can resolve the questions he has raised, the way archaeology is practiced currently precludes any such a resolution. With this we

come to the discipline of archaeology, the second theme of this chapter.

Archaeological Methodology in India

Jürgen Neuß (2012) has provided a severe critique of Indian archaeology and how the nature of its organisation at the state and national level impedes significant progress and innovation. The wider context of this and the way that the development of archaeology is also constrained by its position as an historic discipline in universities as noted by Chakrabarti (1988, 2003). Dass and Willis (2002) raised similar questions based on their preliminary work at Udayagiri. At that time they suggested—naïvely as it turns out—that "future research might involve micro-documentation of the visible traces on the site and a close study of these features in partnership with historians of traditional Indian astronomy. As theories and possible lines of investigation developed, this could lead, in the longer term, to some careful excavation work. The degree of interdisciplinary co-ordination and attention to minutiae required would necessitate a type of archaeology that is not practiced in India today. As there are no signs of such an archaeology developing in the present circumstances, we can only hope that this essay does not trigger off some ill-informed escapade of random digging similar to that seen at Satdhara and Sanchi over the last few years" (Dass and Willis 2002: 41). This same article further cited R. G. Collingwood (1889–1943) who, writing in the 1930s, was one of the early critics of archaeological practice. He wrote: "Once digging has been decided on excavation becomes a sport for human terriers, tempered by the possibility that scientific results—who knows?—may be forthcoming. Today, matters have reached a point when it can be said clearly and publicly that no archaeologist ought even to be allowed to excavate at all unless, when he is deciding to work on a certain site, they can answer the question 'what archaeological or historical problems lead you to that site, why do you think you can solve them there, and how exactly do you mean to go about it?'; unless, every time he or she orders a new trench to be opened or even a single shovelful of earth to be moved, he is prepared to explain, in terms of historical questions and their possible answers, exactly why he is doing it; unless his record shows that, instead of nibbling away at this site like a small boy with a cake until nothing is left, he is capable of saying 'now I have answered the question I came here to answer, and we are going home'" (Dass and Willis 2002, citing Collingwood 1999: 65). Coming forward in time and back again to India, we note that the issues have not gone away. For example, Hawkes (2020) and Hawkes *et al.* (2020) have tried to point to the evidence available, and come to some evidence-led conclusions, without avoiding the fact that much is defective in the way the archaeology has been done. The problem today is as much methodological as it is conceptual. While it is important for archaeology to be question-driven, the nature of those questions is also important. For the last seventy years or so, the vast majority of archaeological activity and research in India has been on pre- and proto-historic periods—as archaeology has developed to fill the gaps where textual history cannot reach (Chakrabarti 1988). Yet as Hawkes (2014a, 2014b) makes clear, for historical periods such as the first millennium AD, both the questions archaeologists have asked and the interpretations they have made have been defined by traditional historical research.

While we may hope that the publication of excavation reports (as a bare minimum) may trigger improvements, past experience suggests that they will have little immediate impact. It is worth reflecting on why this is so. The matter turns, in our view, on the question of deference noted before and, more importantly, on the way organisations (governmental, academic and non-profit) perpetuate and defend themselves. As civil servants know, it is essential to be discreet and impartial, and not to sacrifice one's career to a cause—be it political, intellectual or academic—especially if that cause has the potential to undermine one's department and those within it. Criticism is mute. Perhaps "Can The Civil Servant Speak?" would have been a more relevant question for Spivak to have posed. Certainly it is better not to speak—and not to publish—and collect a pay cheque and pension in the normal way. The alternative is to publish and be damned. The fraught nature of publishing in the official setting and a reasonable fear of unintended consequences naturally encourages deferral. Moreover, all organisations develop systems of practice and will perpetuate those systems unless there are cogent reasons to change. In Indian archaeology there have been numerous campaigns of reform and re-organisation, the first undertaken by Alexander Cunningham who established the Archaeological Survey of India and became its first Director General in 1871. At the beginning of the twentieth century vigorous reforms were led by George Curzon who was Viceroy from 1899 to 1905. He took an active interest in cultural heritage and appointed John Marshall as the Director General of the Archaeological Survey in 1902. From this point on, Indian nationals were allowed to par-

ticipate in (and direct) excavations in their own country. There followed a series of well-known excavations and restorations at major sites such as Charsadda (1902–3), Harappa (1920, 1925–34), Mohenjo-daro (1922–27), Sanchi (1912–19) and Taxila (1913–33). The reports of these excavations continue to book-end much of our archaeological knowledge (see Marshall 1924, 1931, 1951; Marshall *et al.* 1939). Guha-Thakurta (2013) has reflected on the restoration of Sanchi and its publication, *The Monuments of Sanchi*, that the books themselves are a sort of monument in three elephant folio volumes.

By the time *The Monuments of Sanchi* appeared, the need to modernise, and move away both academically and professionally from an Edwardian footing, was felt clearly. This led to the appointment of Mortimer Wheeler in 1944. In his day, Wheeler was at the forefront of archaeology and museum practice. Influenced by Pitt-Rivers, he saw that excavation required a more scientific approach central to which was the careful recording of stratigraphy. He was highly critical of the large scale horizontal digs that revealed a single period of a site's history (as had characterized excavations carried out under Marshall). In response, he developed a system of excavation that has come to be known as the "Wheeler method", wherein an excavation grid (of any size) is divided into squares or 'box trenches', which are then excavated so as to leave a baulk of earth between them to facilitate recording of stratigraphy (Wheeler 1954). This method was applied in India as an expedient means of quickly establishing the chronology of the many hundreds of sites that had been discovered (Wheeler 1955; see further Ray 2008). In doing this, Wheeler not only transformed our understanding of the archaeological history of India, but also trained what later became the next generation of archaeologists in India.

As is well known, world archaeology has moved ahead in scientific, technical and theoretical ways that would have been inconceivable seventy years ago. We now know that the Wheeler-method is not the best (or indeed the only) method of excavation, and 'area excavation' has now come to be the norm, certainly in the west (e.g. Barker 1982). But it has also become clear how fundamentally important it is to take a holistic view of an archaeological site, before beginning to dig a trench to establish the chronological sequence. The full extent of the site must be understood and the 'horizontal stratigraphy' clearly grasped. If this is not done, the excavation is likely to provide an unbalanced view of the site's development (Kennet 2013). Institutionally speaking, however, the Archaeological Survey of India and the education system that feeds it has perpetuated and retained a variant of Wheeler's deep-sounding system and the excavation system has remained largely unchanged. There is also a strong resistance to deviation from this method, due to the notion that preserving a single system ensures a common standard of practice. The level of ownership is indicated by R. S. Bisht, Deputy Director of the Archaeological Survey at the time of the Paithan excavation, who was in regular touch with departmental staff on the site to "make sure they [i.e. Kennet *et al.*] follow our methods [i.e. those of Wheeler]".

What is often seen is an absence of a clear research framework for many of the excavations that take place, and a failure to examine the extent of the site from the surface using simple techniques that are available and regarded as standard practice in many countries. These include controlled surface artefact collection, geophysical examination, aerial photographic examination, detailed mapping and the excavation of a network of test-pits. Once excavation has begun, the focus on Wheeler-style 'box trenches' makes it difficult for the archaeologist to detect the more subtle large-scale evidence that is easily obscured by the excavation baulks that are a part of the Wheeler box system. A tendency to phase whilst excavating rather than at the post-excavation stage and to record objects by phase only, (making later reinterpretation of the phasing and chronology impossible) is still a problem at many sites. But probably the most serious issues are the lack of systematic study of the artefacts using robust methodologies and detailed descriptions, a failure to sample systematically for environmental evidence, and the almost complete lack of synthetic, regional, comparative analysis of the data from excavations.

Archaeological sites that are obscured by modern settlement are generally excluded from excavation. Indeed, the concept of 'urban archaeology' which became such an important element of European and Scandinavian archaeology from the 1950s onwards, is practically unknown in India. There are two major problems with this. The first is that in only looking at sites that are presently unoccupied we are unable to investigate the movement of human settlement to locations that are still occupied today. The second, and real heart of the matter, is that while uninhabited sites allow archaeologists to work in the ways they prefer, given the space and lack of overburden, an exclusive focus on such sites results an archaeological profile only of settlements that were abandoned at some time—and not those that have continued to be inhabited, or even flourished. This is enforced by the archaeological reserve areas that have been set aside by the Government of India to protect the archaeological record from modern development. This is laudable,

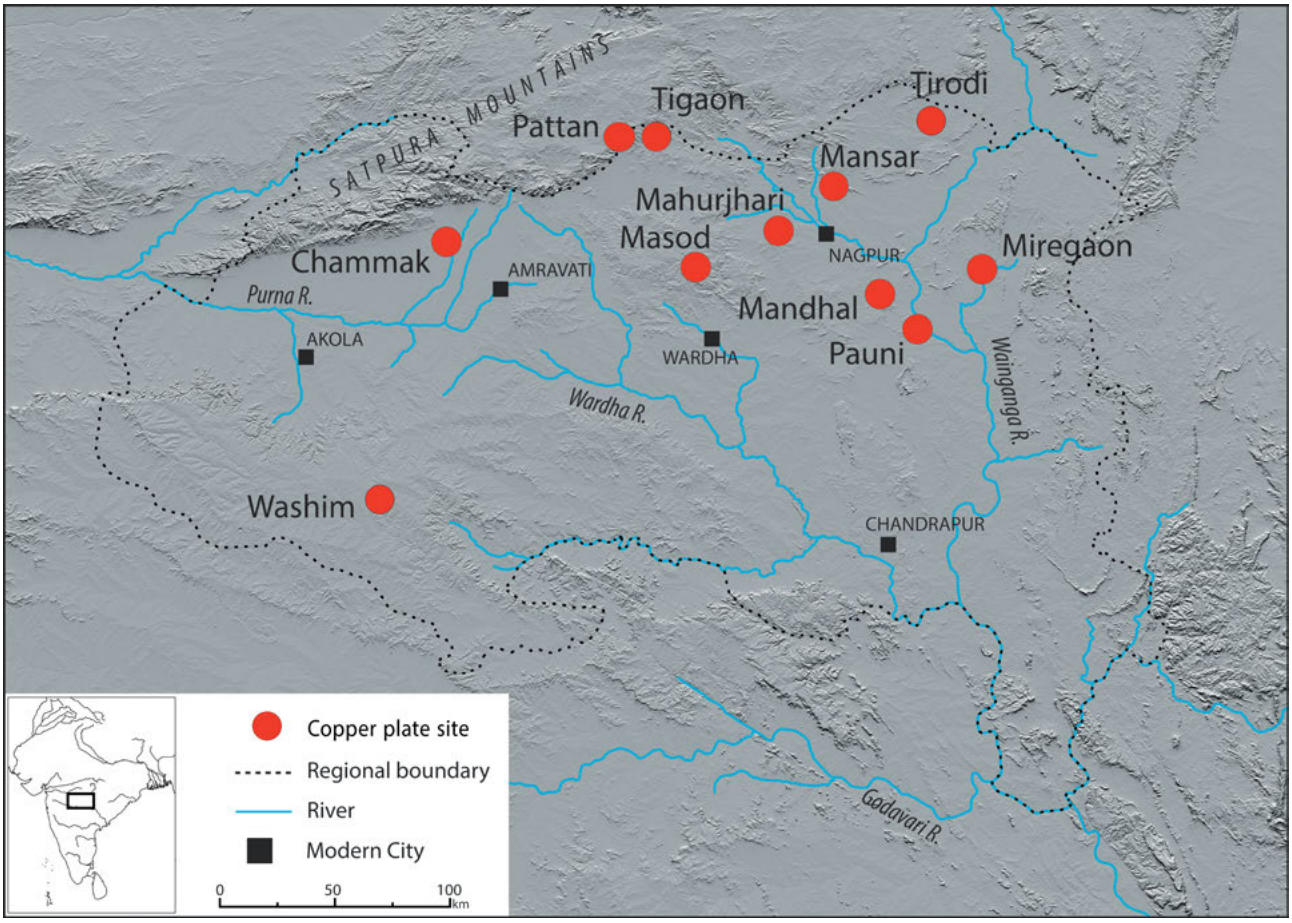

Fig. 15.3: Maharashtra, region of ancient Vidarbha, showing the distribution of key copper-plate charters of the Vakataka period.

of course, but the reserve areas are often located on the peripheries of sites, in areas that had no modern habitation when they were set aside—thus compounding the problem set out above. The combination of Wheeler's methods with the system of reserves and a reluctance to excavate ancient settlements that are still occupied introduces important biases into our data collection. We end up, in essence, with an archaeology of sites that have failed over time, not those that endured and prospered. No wonder, then, that India's greatness is so frequently portrayed as being in the distant past—the wonder that *was* India—in A. L. Basham's immortal words (1954). If we are to believe this notion that India is a place that has 'had its day' it is because archaeologists and historians have made it so.

New Directions

The possible solutions to this problem suggested by Kennet (Chapter 14 this volume) are modest and in no way prescriptive. The remedies posed certainly have the potential to address the issues raised by historians and thus establish common cause between archaeologists and historians on matters relating to the transition from the Early Historic period to the Medieval. The recommendations are as follows: (1) a shift to the excavation of Medieval rather than only ancient sites, even if this involves excavation in modern urban centres; (2) a change of focus in the procedure of excavation and exploration with a more careful and holistic surface evaluation of any site before excavation commences; (3) the routine, quantified study of archaeobotanical, zooarchaeological, ceramic and find sequences; and (4) the documentation of rural settlement patterns.

The present volume sets an example on the first three points and is, as a result, a milestone marked by innovation in practice, the collection of new empirical data and the articulation of interpretative theories to explain that data. At the same time, the authors of this chapter have recognised that there is an urgent need to look at wider settlement patterns around key sites. This strategy allows

the exploration of changes that were taking place on a regional scale, traces of which can be observed at specific locations like Paithan.

To situate Paithan in its regional setting, Hawkes, Lefranq, Abbas and Willis studied land-granting activities as documented by copper-plate inscriptions. This line of analysis was based on the long-established historical proposition that the appearance of land records marked innovations in land use, agrarian practice, property relations and social formation in many parts of India. The find spots of the copper-plate charters were traced, from Uttar Pradesh to Karnataka, to test what might be concluded from their spatial distribution (Hawkes and Abbas 2016). The focus was on the earliest charters—from the fourth, fifth and sixth centuries—and the stone inscriptions of the same period. The number of surviving charters is substantial. These data have been compiled and published online (see Siddham as well as the aggregated data available at www.siddham.network). Having plotted the distribution of the copper plates, research then concentrated on an area of eastern Maharashtra—a region known in ancient times as Vidarbha. Not only does this area neighbor Paithan (thus facilitating the investigation of its regional context), it also yielded a number of copper plates issued by one one dynasty—the Vakatakas—making it possible to test the effects that these grants had. The results of the findings are shown here in Fig. 15.3.

Investigations in Vidarbha made a conscious attempt to implement the recommendations noted above, but adapted them to suit local circumstances. There were three main aspects to this: (1) in terms of focussing on Medieval sites rather than ancient ones, field surveys showed that it was difficult to distinguish reliably between Medieval and ancient sites and that most sites had multi-period occupation; consistent abandonment of earlier settlements or movement to new ones seem to have occurred only later in the early second millennium AD; (2) in terms of changing procedure, work focused on the systematic reconnaissance and recording of archaeological sites in areas selected on the basis of the presence of known epigraphic and monumental remains, coupled with the collection of surface material so as to characterize those sites and establish patterns in the regional distribution of archaeological material; (3) in order to supplement and improve on this regional picture, a programme of limited test-pitting was implemented at Adam and Mandhal, sites known from excavation and for their inscriptions (Nath 2016; Sali 1998). This was carried out to generate data necessary to improve pottery typologies, as well as to collect environmental samples as a step towards understanding the supposed agrarian innovations that took place during the early centuries AD, and the apparent settlement shift from Adam to Mandhal during the mid-first millennium. The results of these surveys and key-hole excavations are being published separately. Here we will restrict ourselves to some comments on settlement patterns and the distribution of copper-plate charters, the third and final theme of this chapter.

Vidarbha

Mapping the find spots of the Vidarbha copper-plates has shown that of the twenty-nine charters from the region, 15 (52%) were found during digging or ploughing in locations that may relate to their original contexts, at least until evidence emerges to the contrary (for discussion, see Hawkes and Abbas 2016). Others were found in the possession of people and museums, having been dug up long ago—notably for the present purpose the copper plates in Pune (Siddham OB00143), and in Patna (Siddham OB00159). It is for this reason that some of the Vakataka charters do not appear in the map (Fig. 15.3). There are also some plates that are outliers, i.e., they were found geographically outside the main corpus and possibly not in locations that are historically reliable. The several charters found in the Satpura hills to the north (beyond the area covered by our map) are good examples, with the so-called Siwani copper-plate charter a prime case (Siddham OB00150). This was found in or shortly before 1836 in the possession of a *zamindar* in the then Seoni District. Before 1888, the plates were still in the possession of a *zamindar* of Pindarai village (Siwani Tahsil, Madhya Pradesh). This location is 95 km north of Balaghat, itself the source of a copper plate of uncertain provenance. The original find spot of the Siwani plates is not known but, like the other inscriptions recovered in the Satpuras, they seem to have been carried into the hills and, as a consequence, preserved there. This distribution of evidence is not unique to Vidarbha or the Vakatakas. Cultural artefacts deemed valuable or talismanic are often preserved outside core areas, especially if those areas have experienced a sequence of upheavals and adjacent regions have provided a safe haven.

Looking, then, at the distribution of those copper plates that were found in or near their original contexts, a picture beings to emerge. Once we discount the 'noise' in the data produced by the highland evidence, we see that all the Vakataka grants are from low-lying geographical zones, i.e. at elevations below 500 m. The Masod charter

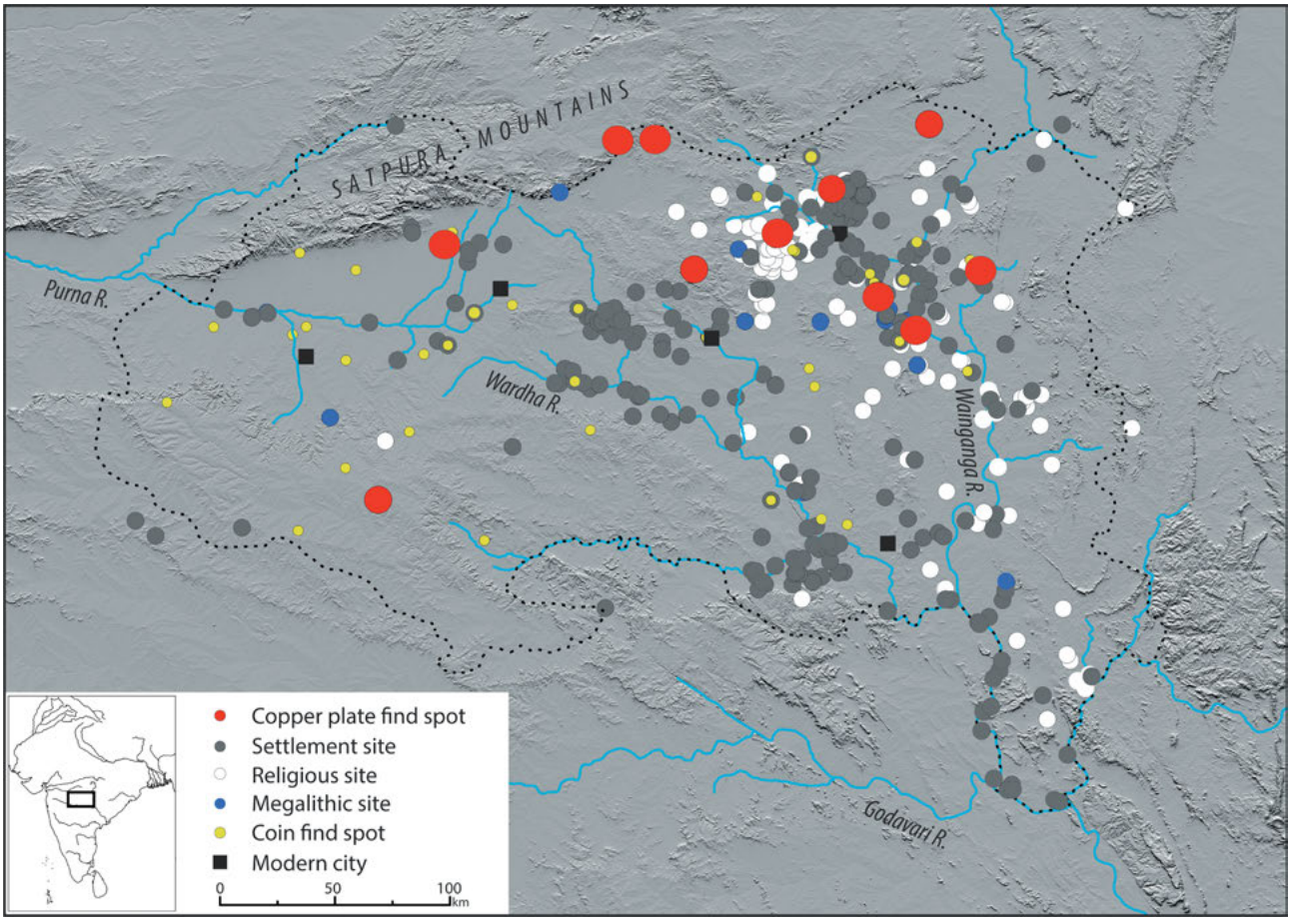

Fig. 15.4: Maharashtra, region of ancient Vidarbha, showing the distribution of relevant archaeological sites and coin finds.

seems to be an exception (Fig. 15.3, Siddham OB00171), but this anomaly is explained by the fact that this grant is not an original Vakataka inscription but a forgery of Medieval times. With this example set aside, we can see that the Vakataka charters—and the administrative, economic and social practices they reflect—pertain to lowland areas, almost all of which were broad, alluvial riverine plains.

When we then relate the charters with the distribution of settlements, further patterns of importance emerge. As noted above, the results of new surveys and excavations are currently in the process of being finalised for publication. Still, it is still possible to identify certain broad-scale patterns in the distribution of known archaeological remains when they are mapped in space and time (see Fig. 15.4). The data in this map were gathered through a comprehensive survey of the existing archaeological literature (published and unpublished). This resulted in the compilation of a census of all archaeological sites and remains that have been documented in the region over the last one-hundred and thirty years (Hawkes and Casile, forthcoming). As would be expected, the consistency, dependability and detail of these data varied. For instance, the dating of many remains and sites is uncertain, with only three sites across the area having been subject to radiocarbon dating; in addition, there is no regional pottery typology and thus little consensus as to the date of the remains found at any one site (Lefrancq *et al.* 2019; Lefrancq and Hawkes, forthcoming). Additionally, as we know from the accounts, many surveys and campaigns of exploration were not systematic. Sites were found through local informants or simply turned up along the paths walked by archaeologists, while large areas of the region have not been surveyed at all. As we might expect, areas that are difficult to reach have been surveyed far less intensively than those close to Nagpur and other towns.

Notwithstanding its obvious idiosyncrasies, the dataset as a whole is not lacking historical weight and substantive implications. The first of these, and most

important of all, is that the existing settlement data shows that the entire region was well-settled from at least the 'Megalithic' or early Iron Age (from *circa* 800 BC in this region). Further, when we look at the distribution of these sites, we can see that settlements tended to cluster around water bodies—small tanks developed and managed by local residents—and along rivers and streams. In addition, most settlements—like the land grant inscriptions—are located at elevations below 500 m. The main historical implication of the census data for the present chapter is this: if we compare land grants and settlements, we can see that the grants have a settlement context indicative of the agrarian and geographical environment in which the grants operated. It is perhaps better to rephrase this observation in terms of human agency: if settlements are proxies for the people and communities living on the land and managing its environmental and agricultural resources, and land grant inscriptions are proxies for the presence of elites receiving land from the king, then the find spots of the grants juxtaposed to settlements shows the social and economic matrix in which beneficiaries of the grants lived and derived their livelihoods. The network of settlements—and their economic surpluses—also gave rise to monumental remains, such as temples and monasteries, and the inscriptions that sometimes survive to document the construction and endowment of these institutions.

Agriculture and property relations

The pairing of epigraphic find spots and settlement distribution has significant implications for the historical theories put forward about the Early Medieval period. Far from documenting the increased clearing and settlement of uncultivated lands through the deployment of Brahmins and land grants, and the growth of networks of rural settlements—as Chattopadhaya would have us believe—matters are exactly the opposite. The charters document gifts of land to Brahmins in well developed zones where settlements had been established for at least 1000 years. In one case, at Mahurjhari, the plates were found at a site where there was a well-developed industry in carnelian-bead production (Mohanty 1999, 2005; Mohanty *et al.* 2019) (Figs 15.5 and 15.6; for the plates see Siddham OB00178). Some of these stones were engraved and their inscriptions show they were produced for Buddhist clients and had a wide distribution (Fig. 15.7). These findings indicate that wealth at Mahurjhari came from a vibrant trade in semi-precious stones as well as agricultural activity.

Fig. 15.5: Mahurjhari (Dist. Nagpur, Maharashtra). Raw material for carnelian from ancient quarry.

Fig. 15.6: Mahurjhari (Dist. Nagpur, Maharashtra). Carnelian bead fragments in fields at village.

Fig. 15.7: Mahurjhari (Dist. Nagpur, Maharashtra) (?). Engraved carnelian with a Buddhist inscription in Vakataka-style script reading *apramāda* with corresponding impression. Probably made at Mahurjhari. British Museum, 1892, 1103. 126, see SIDDHAM IN01102.

Fig. 15.8: Chammak (Maharashtra). Copper-plate charter of Pravarasena II with seal of the king and a paper note by J. F. Fleet. British Library Ind Ch 16, see SIDDHAM OB00149.

Lest we be accused of selecting examples that suit our interpretation, we can take the Chammak plates as a final example (Fig. 15.8, Siddham OB00149). These plates were found in the ground in the nineteenth century and the inscription on them registers the donation of the village of Carmanka by Pravarasena II at the request of ruler named Kondaraja (Mirashi 1963). The village of Chammak perpetuates the ancient name. The donation was made to a large number Brahmins of various Vedic schools and the charter lists 49 of these donees by name. An exploration of the area under the project led to the discovery of a large settlement immediately south of Chammak village. This dates from at least the Iron Age to the late Medieval period (Hawkes *et al.* 2017, 2020). There can be little doubt that this location was the seat of the king named Kondaraja in the inscription. Now the settlement of a large number of Brahmins in this location at the behest of a local grandee—and its sanction by king Pravarasena—would necessitate there being adequate resources to support the people in question. The developed nature of the wider area is shown by the *stupa* and associated settlements at Bhon, slightly to the south on the Purna river (Deotare *et al.* 2007; Naik and Deotare 2016). The Chammak charter does not represent, therefore, some sort of pioneering effort in a remote forest area. It is the settlement of a community of learned individuals, their families and attendants in what was already a prosperous zone. The fact that this site was discovered through simple reconnaissance, and that more than a century stands between the publication of the Sanskrit text on the plates in 1888 and the publication of the archaeological context in 2016, underlines the need for the integration of fieldwork and historical studies.

Summary

Returning to the excavations reported in this volume, we have, it is hoped, demonstrated that some of the observations made at the regional level correspond with interpretations that emerge from the site of Paithan. This provides a methodological basis for placing sites in a broader context as well as providing insight into the contemporary development of two different but key parts of the region: the Godavari and Vidarbha. As Kennet makes clear in Chapter 14, the Paithan excavations document the range and depth of agrarian reform that took place in a long-settled core area during the Early Medieval. Kennet suggests these changes may have been stimulated by the settlement of Brahmins from outside the region, and by the building of temples whose endowment lands probably came under new management regimes. These changes reflect the developments evidenced by the two land charters discussed above—Mahurjhari and Chammak—and demonstrate the value of linking landscape surveys with in-depth excavations at selected locations. Moreover, this chapter has shown that a coupling of landscape studies and excavations can indeed shed light on the the transition from the Early Historic to the Medieval, providing a level of insight into processes of change and their regional variations that are not available from the epigraphic sources alone.