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Negotiating Evidence: History, Archaeology and the Indus Civilisation

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I

Following the destruction of the Babri Masjid in Ayodhya in December 1992, the discipline of archaeology has been increasingly exploited for meeting the demands of religious nationalism in India, for offering material proof for the primordially of Hindu *dharma*, and for substantiating claims that the ‘Vedic Hindu’ had an indigenous origin within the subcontinent. Over the last decade, statements such as ‘new astrological and archaeological evidence has come to light which suggests that the people who composed the Vedas called themselves Aryans and were indigenous to India’ (Prinja 1996: 10), have not only propped up the doctrinaire of *Hindutva*, but have also acquired an official sanctioning from many within the professional community of Indian archaeologists (e.g. Lal 1998), who are actively involved in a programme of promoting the premise that it is possible to unearth true histories objectively through archaeological means (Gupta 1996: 142).

The decision taken by the Allahabad High Court in March 2003, to examine the Vishwa Hindu Parishad’s claim for the existence of an ancient Rama temple at Ayodhya through an excavation, has added to the claims of these archaeologists, as this is the first instance in the history of Indian archaeology where the discipline’s principal method (i.e. excavations) has been legally endowed with the potentials for unearthing the ‘truth’. The verdict of the State judiciary of Uttar Pradesh, which requested the Archaeological Survey of India¹ to

I thank Simon Schaffer, Eivind Kahrs and Michael Dodson for their comments on an earlier draft.

¹ First established as the Archaeological Survey in 1861 to promote historical investigations in British India, the Survey was and continues to be organized under the Central Government. Its performance before and after Indian independence has been largely dependant on the political will of the ruling party, as although archaeology has

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dig under the foundations of the Babri Masjid, was based on the assumption that the physical remains of a temple would present viable evidence of Muslim iconoclasm. However, typically, it did not accommodate the nature of an excavated record, which can only be 'read' through lengthy processes of analogy, be they as obvious to commonsense as morphological, ethnographic and stratigraphical. By choosing to keep the public ignorant of the methodologies through which corroborative analogies are commonly derived to interpret material finds, the officers of the Survey seem to have willingly complied with the myth implicit in the judiciary's decision, that historical truth exists 'somewhere out there' waiting to be unearthed.²

Even a cursory overview of worldwide archaeological practice in the twentieth century (when archaeology was professionalised and institutionalised as a discipline) shows innumerable examples of situations where historical evidence established through archaeology has been used to ratify partisan ideologies. In situations where archaeological enquiry has been conducted through state agencies, one finds instances of fieldwork organised to create a positivist approach towards the excavated material. In such cases, material evidence is usually presented as having been objectively extracted through the employment of precise excavation techniques (cf. Ucko 1995).³ Where archaeological work has been directly organised to meet political ends, as it was in the case of Ayodhya (excavated between March and September 2003), excavation even when undertaken in controlled

developed as an academic subject in many Indian Universities from the 1950s, large scale and intensive excavations are possible only through financial assistance from Central and State Governments, making political patronage more or less mandatory for institutional fieldwork.

² B.B. Lal, a former Director-General of the Archaeological Survey of India (1968–72), implied this as early as in February 1991. He was asked at Vijayawada whether a temple existed under the Babri Masjid and answered—'if you do want to know the reality, the only way is to dig underneath the mosque' (1998). Lal's rhetoric is foundational to archaeological practice, although he effectively side-stepped issues related to how evidence is established from excavated artefacts, and meant his remark to substantiate the notion which, as the historian B.D. Chattopadhaya notes, is 'held dear by many, that archaeology is more scientific than other disciplines' (2002: 114).

³ In his overview Peter Ucko has remarked that 'the vesting of archaeological enquiry in the 1990s within a national state agency has led to two consequences,' and one of them has been 'the fostering (whether it be in Germany, India, Japan, the former Soviet Union or the United Kingdom) of an approach to archaeological fieldwork which assumes (in a good old-fashioned Pitt-Rivers-type way, and often using Pitt-Rivers methodology) that archaeological facts are out there to be recorded objectively according to a series of always improving strategies and technical skills' (1995: 8–9).

environment with sophisticated implements has not necessarily altered the premise of enquiry. *The Politics of the Past* (Gathercole and Lowenthal 1990), *Nationalism, Politics, and the Practice of Archaeology* (Kohl and Fawcett 1995), and *Nationalism and Archaeology in Europe* (Diaz-Andreu and Champion 1996) are just three edited volumes among the vast collection of literature published since the 1990s that offer comprehensive surveys of instances where archaeology has been systematically used to situate ethnic identities for prehistoric groups, define racial types from excavated artefacts, promote research on ethnogenesis, and impose legendary places on maps of modern states.

Therefore, the selection of archaeology to legitimize the politics of *Hindutva* has not come as a surprise. For, compared to other disciplines in the humanities, and history in particular, archaeology's investigative techniques, of which many are science-based, can effectively be evoked for cultivating the notion that archaeological methodology is truth-making. However, a growing trend within the community of professional archaeologists in India to present their evidence as being of a non-negotiable nature, is a new development.⁴ For example, although expressing his criticism for the events that led to the excavations at Ayodhya in 2003, Dilip Chakrabarti (2003a: 580) has offered the view that all those who are suspicious of the Survey's claims are affiliates of the Congress, the political party which was then in opposition to the ruling government.

Chakrabarti's support for undeniable archaeological proof is remarkable as he seems to ignore the polysemic nature of truth, or indeed how facts get privileged as facts.⁵ His implicit assertion that archaeological proof is non-negotiable, is at odds from the way material evidence has often been presented in the past by practitioners themselves. It is worth our while to recall that while offering his

⁴ An example related to the Indus Civilisation can be found in the Correspondence section of *Man and Environment* (2000: 105–18). The rejoinders to Shereen Ratnagar's article (1998) highlighted her lesser knowledge on matters related to 'present day practices and research goals of modern biological anthropologists' (Kennedy *et al.* 2000: 105). Yet Ratnagar's own reply shows that in its making, evidence derived through quantitative methods is also influenced by ideological presumptions about the past (2000: 119).

⁵ Chakrabarti perceives the 'Ayodhya issue' as problematic in terms of 'commitment to archaeology'. He does not feel the need to question the nature of evidence presented by the Archaeological Survey, which he assumes as being given. He denounces the Survey on moral grounds, for the historic role it has played in establishing the 'dichotomy between the ideal and the reality of heritage preservation in India' (2003a:580; also 2003: 201–5).

proof for a 'pre-Aryan' religion in the Indus Civilisation, even an English Director General of the Archaeological Survey of India, Sir John Marshall, who was firmly rooted in the British empiricist tradition had noted that 'to assume' that this evidence represents the 'sum total of the religion of the jungle tribes [is as] irrational as to suppose that the rude grass and mud hovels of these... tribes are representative of the massive edifices of Mohenjodaro and Harappa' (1931: 78). Chakrabarti has also geared his move towards epistemic fundamentalism to establish national consciousness among the 'grass roots' through archaeology, although the best examples of sustained efforts in this direction are from those who present archaeological proof of a primordial Hindu (often traced within a presumed interchangeable category, Vedic or aryan) culture. The 'revised' school text books on Social Sciences and History (for classes six and eleven), which have now been re-published by the National Council for Educational Research and Training (Prakash 2002 and Lal 2002), reveal the extent to which these archaeologists have willingly gone to establish an unholy alliance between data and religion,⁶ by offering archaeologically derived historical evidence as being non-contingent, non-conjectural and purely empirical.

Through a historiographical sketch of the early archaeological work on the Indus (or the Harappan) Civilisation,⁷ which is being increasingly appropriated to contextualise a sub-continental ethos for Indian history and expose the 'racist' and 'elitist' histories of India written during the colonial and post-colonial pasts (e.g. Chakrabarti

⁶ Example, chapter 9, on 'The Vedic Civilization', in *Ancient India* (2002: 89–92). For an overview of the sustained protests against the re-writing of Indian history, see *Communalisation of Education: The History Textbooks Controversy* (published in 2001 by the Delhi Historians Group, Jawahar Lal Nehru University, New Delhi). Lal, who is the authority behind this chapter and the preceding one related to the Indus Civilisation, has also presented his evidence to the Vishwa Hindu Parishad's website (http://www.vhp.org/englishsite/e.Special_Movements/dRanjanambhumi%20Muti/the_ramayana.htm), and in the journal of the Rashtriya Swayamsevak Sangh, *Manthan*.

⁷ This chalcolithic (i.e. bronze-age, pre-iron) and urban (in the mature Harappan phase) civilisation, dated between the middle of the third millennium B.C. to the middle of the second millennium B.C., is now provided with a new name, Indus Valley Tradition (Shaffer 1991), which has mainly acquired currency among North American archaeologists (e.g. Kenoyer 1991, Kennedy 2000). Jim Shaffer coined the phrase to include 'all human adaptations in the Greater Indus region from around 6500 B.C. until 1500 B.C.' (Kenoyer *ibid*: 342), which would allow a wider frame of reference for the spatial and temporal patterns of site formation within the regions of Greater Indus Valley, Baluchistan and the Helmand. The excavation and exploration of sites related to the Indus Civilisation spans over eighty years, beginning in 1921 with the excavations at Harappa. Mohenjodaro was excavated the following year. For a summary see Possehl (1999, 2002).

2003; Lal 1997), I shall in this paper draw attention to something obvious, namely the contingency of archaeological evidence. My aim is to highlight how different meanings have been, and can be, attributed to the same set of excavated artifacts, and stress that archaeological representations need to be cautiously used as an instrument of rationalization in the creation of histories. The very definition of an artifact rests on its associations with other objects, as well as contemporary perceptions of what its functional characteristics may be. Therefore, facts can only be established through reasoning, which makes the archaeological method analogical, and not empirical. I also hope to demonstrate that dramatic discoveries through archaeology usually follow earlier scholarly efforts (one cannot dismiss the fact that even Harappa and Mohenjodaro were ‘found’ and known before they were excavated),⁸ and artefacts unearthed during such feats acquire the legitimacy of proof only through subsequent interpretations. The veneer of unexpected finds may retain the romance in excavations for those who are students of archaeology and professionals in the subject, but a history created through claims of unexpected finds can only lie about its own genealogy, as magical discoveries seldom establish phenomena that are self-evident. Therefore, even if we accept, as the recent excavators of Ayodhya wished us to, that a Hindu temple did exist under the Babri Masjid and was destroyed in 1528 by Mir Baqi, the Mughal emperor Babur’s official, such a ‘discovery’, contrary to what they and the Hindu organisations deemed to impress us with, does not by itself become proof of Muslim bigotry.⁹

⁸ Alexander Cunningham, the first Director-General of the Archaeological Survey (1871–85) described ‘the ruins of Harappa’ as the ‘most extensive of all the old sites along the banks of the Ravi . . . the whole circuit’ being ‘about 12,500 feet, or nearly two and a half miles’ (1875: 105). See Vats (1940: 11) for the history of the discovery of Harappa, and Possehl (1999: 43–63) for a narrative of the 19th century explorations, and Lord Curzon’s archaeological programme in northern India. The latter led to location of other sites, such as Mohenjodaro, Kalibangan, Dabarkot, Amri and Suktagendor, and through their excavations in the twentieth century, archaeologists have been able to reveal the nature of inter-site linkages.

⁹ The evidence for a Hindu temple destroyed by Muslims is not unique. Investigating the Sangh Parivar’s claims that the Muslims had destroyed more than 30,000, Richard Eaton (Professor of History, Tuscon, Arizona) had found approximately 80 examples (see ‘Temple Desecration in pre-modern India-part I’, *Frontline*, 9–22 December 2000, and ‘Temple Desecration and Indo-Muslim States- part II’, *Frontline*, 26 December–5 January 2001). However, Eaton has been accused by Koenraad Elst (a preacher of the *Hindutva* ideology) for selectively quantifying his data (Elst 2002: *The details about ‘Hindu iconoclasm’* <http://www.bharatvani.org/reviews/Eaton.html>).

II

The interpretations that are today aggressively offered as evidence for a 'Vedic'¹⁰ presence within the Indus Civilization, demonstrate the extent to which ideologies infiltrate definitions for artefacts. The new genre of archaeological evidence challenges their creators' slogans that 'facts of history cannot be altered' (Lal 1998). By providing a known phenomenon with a new name (the Sindhu Saraswati Sabhyata), by choosing a set of excavated and explored objects to argue for the presence of a cultural tradition that can supposedly be traced in the *Rgveda*, and by liberally translating this Veda to demonstrate that archaeological and literary sources reveal an unbroken genealogy for the Hindu *arya*, they are establishing a foundational myth.¹¹ That these are precisely the techniques commonly employed for altering perspectives in history, they deliberately ignore. In recent years, Indian excavators of Harappan sites have increasingly flaunted their expertise of the Sanskrit language and scoured references from the Vedas to identify their finds. For example, the excavator of Dholavira, Ravindra Singh Bisht, proposes that the three major architectural forms he ostensibly found there, the citadel, and the middle and lower towns, could correspond to references in the *Rgveda* for units of a tripartite settlement system, the *parama* (highest), *madhyama* (middle) and *avama* (base), that according to him alludes to the functional hierarchy of habitations within the Vedic *grama* or village (1999: 420). The skeleton of a horse (Sharma 1992–3), and terracotta objects understood to represent spoked wheels, chariots and armours (Lal 1997), and presumably found over the last fifteen years from sites such as Surkotada, Kalibangan, Banawali and Harappa, are being offered to substantiate the argument that the Aryans peopled this civilisation (Lal 1997),¹² although it is quite clear from sources linguistic and

¹⁰ The association of the Vedas with a cultural 'age' forged on the basis of textual sources, and before the presence of the Indian chalcolithic was archaeologically unearthed, is itself problematic. Romila Thapar (2001) has shown one way of deconstructing the association.

¹¹ 'That the Vedas are foundational to the sub-continental culture of South Asia' (Thapar 2001a:1124). Crucially, Thapar lays bare some of the motivations behind this mythmaking.

¹² Quoting palaeo-anthropologists who claim to have discovered two discontinuities in the biological continuity of people within the Indus Valley, one which occurred between 6000 and 4500 B.C. and the other after 800 B.C. but before 200 B.C., Lal speculates on the 'basic biological continuity... from ca. 4500 B.C. to ca. 800 B.C.' and is of the opinion that the chalcolithic people of Mehrgarh 'who in course of time

anthropological, that the latter are biologically non-existent (e.g. Thapar 2001; Kennedy 2000). Just one example of how wrong a biological classification for the Aryan can be is the retort for the claims of the Brahmanas, that they were the best varna, in the Pali canon's *Majjhima-nikaya*—

What do you think about this, Assalayana? Have you heard that among the Greeks (i.e. foreigners in Western Kingdoms) and the Kambojas and other bordering communities there are only two varnas, arya and dasa; [and] having been arya one becomes dasa, and having been dasa one becomes arya? (MII 149).

Considering the manner in which the *Rgveda* is being currently translated by Indian archaeologists, it is rather ironic to recall that the first excavations in Harappa and Mohenjodaro allowed the excavators precisely the opposite option: to neglect literary sources and establish evidence for an urban and hierarchical society through assemblages of artefacts and explored landscapes alone. For by 1925, it was widely assumed that the sites belonged to an era before the period described in the *Rgveda*, as the material remains did not seem to reflect the pastoral nomadic lifestyle, then accorded as being referred to in the text. The comments of Marshall,¹³ under whose leadership the excavations at Mohenjodaro and Harappa were conducted during the 1920s, that the sites produced 'an entirely new class of objects which have nothing in common with those previously known to us, and which are unaccompanied by any data that might have helped to establish their origin and date' (1924: 529), set the stage for drawing inferences from outside the Indian subcontinent to explain this bronze-age phenomenon.

Rakhal Das Banerji, the first excavator of Mohenjodaro in 1922, chose to compare the painted pottery and the inscribed seals with the Minoan antiquities, of which every excavator of his generation would have had detailed knowledge (see Marshall 1924: 548). The palace-based, peaceful civilization of the Minoans at Knossos (as its

evolved into Harappans' may have been the Indo-Aryans (1997: 287). His arguments (*ibid*) are a classic example of 19th century evolutionism, where culture, technology and biology were used as interchangeable categories.

¹³ John Marshall was the Director-General of the Archaeological Survey of India from 1902 until 1928. The excavations at Mohenjodaro and Harappa, and the preparation of the three volume report were carried out under his authority. He personally led only one season's work at Mohenjodaro in 1925–26, and none at Harappa.

excavator, Sir Arthur Evans, had suggested), with no statues and art to identify the gods and kings, would have logically presented to him obvious sources for comparison with the finds from Mohenjodaro. Both sites yielded inscribed tablets, knowledge of metal technology and a complex social organization, and presented no apparent clues on the nature of the government. Although Marshall categorically rejected the resemblances Banerji proposed, as 'being at best problematic, and in any case too slight and intangible to warrant any inferences being drawn as to a cultural connection between the two areas' (*ibid*), the latter's choice of a comparable subject to understand his finds is a useful hint of how inferences have since been drawn for the archaeological remains of this civilization. Banerji's intuitive choice of relating the unknown to known phenomena was a logical strategy, one which influences most definitions initially imposed on excavated objects, and through which preliminary inferences on their identities are derived.¹⁴

Harappa and Mohenjodaro were excavated during the decade when other sensational 'discoveries' related to ancient civilisations in Egypt and West Asia were being made through archaeology; the opening of Tutankhamen's tomb in the Valley of Kings in November 1922, finds of a library of cuneiform tablets and a palace from Kish in February 1924, and the royal cemeteries in the Ur of Chaldees, the city of the Biblical hero Abraham, in 1926.¹⁵ Theories of culture-contact between the excavated sites in Egypt, Sumer and Indus were proposed by archaeologists to explain their finds. Noticing similarities in the types of seals between Harappa and Mohenjodaro, and Susa, the Assyriologist at Oxford University, A.H. Sayce, saw the possibilities for 'an intercourse' between the Indus and Sumer 'during the second millennium B.C.' (1924: 566).¹⁶ G.C. Gadd and

¹⁴ It is interesting to note that Marshall, who had attended Sir Arthur Evans' excavations at Knossos in the 1898, wished to emulate a comparative excavation methodology for unearthing a comparable civilization, and noted that cities such as Mohenjodaro and Harappa required 'a well organised and comprehensive campaign of excavation conducted on a scale comparable to that attained at Knossos' (1926a: 50-1).

¹⁵ The discoveries were sensationalized through *The Illustrated London News* with titles such as 'A Great Discovery in the oldest city of which human records exist; Kish—relics 4000 years old' (1 March 1924: 348-9), and 'The Moongoddess and her Kitchen: New discoveries at Ur of the Chaldees, the city of Abraham' (10 July 1926: 56-8).

¹⁶ Sayce's letter to *The Illustrated London News* appeared in the very next issue that followed Marshall's. He began by suggesting that the discoveries in Punjab and Sind

Sydney Smith (from the Egyptian and Assyrian department of the British Museum) substantiated his observations more fully (1924: 614). What is interesting to note is that the 'new archaeological finds' which they reported, 'namely that there were in India a people who had been in close contact with the Sumerians' (*ibid*), was at this stage a speculation, and as such a succinct example of how intuitive inferences assume evidential status of data. The photographic display which accompanied Gadd and Smith's article in *The Illustrated London News*, of stone pestles, pottery, ring stones, mace heads, terracotta models, shell ornaments, seals with pictographic inscriptions, and brick work from structures at Mohenjodaro and Harappa, and from Ur, Al Ubaid and related sites in southern Mesopotamia (mentioned interchangeably as Babylonian and Sumerian in the text) visually forged the 'Early Indian and Babylonian Kinship' (*ibid*: 615–6).

From 1925 through the 1930s, finds of Mesopotamian artifacts in Harappa and Mohenjodaro, and vice versa (see Marshall 1931: 104), and the references to Dilmun, Magan and Meluha in the Mesopotamian clay tablets, established the archaeological evidence for theorising on situations of culture-contact between the two civilizations (e.g. Mackay 1938: 639–47). Inferences for external trade (cf. Asthana 1976, Chakrabarti 1978, Ratnagar 1981), chronologies and origins were sustained through this evidence, which was so deeply embedded within the scholarship of the period that although Marshall dropped the terminology of Indo-Sumerian before the excavation reports on Mohenjodaro were published,¹⁷ the change in nomenclature

'were even more remarkable and startling than' what Marshall had supposed, as they were 'likely to revolutionise our ideas of the age and origin of Indian civilization', and remarked that the seals 'or plaques' from Harappa and Mohenjodaro were 'practically identical with the proto-Elamite 'tablettes de compatabilite' discovered by De Morgan at Susa'. Sayce suggested that 'the identity is such that' they 'might have come from the same hand' (27 September 1924: 566).

¹⁷ Marshall 'provisionally adopted' the terminology to indicate 'cultural connections between this prehistoric civilization of the Indus and that of Sumer' and 'not as implying that the peoples of these two regions were of the same stock or spoke the same language' (1926: 346), but soon dropped it as he felt that it 'implied a closer connection with Sumer' than 'now seems justified' (1928: 75). Ernst Mackay who excavated both Mohenjodaro and Chanhudaro, used the terminology of Harappa Culture based on the first type-site excavated to define the civilisation (1938a: 39). His term was later used by Stuart Piggott in *Prehistoric India* (1950). The extent of this civilisation is now beyond the regions served by the Indus, the Ghaggar-Hakra, and their tributaries, as sites have been found in Baluchistan and Afganistan, and 'Harappan' artifacts excavated from settlements originating from the third millennium B.C. along the coast on the Gulf of Oman.

did not deter Sir Mortimer Wheeler, approximately fifteen years later, from locating the stimulant for civilization in the Indus within the flood plains of the Euphrates and the Tigris (Wheeler 1953).¹⁸ In this respect, Wheeler did not deliberately impose a 'secondary civilization built by the foreigners' on the Indian subcontinent, as the Indian archaeologist Swarajya Prakash Gupta would have us believe (1995: 181), but essentially followed and modified the opinions on a comparable and archaeologically excavated 'civilisation' of one of his peers, the excavator of Ur, Leonard Woolley. Explaining the civilization at Ur, the latter had suggested that the Sumerians 'were immigrants, and brought with them from abroad the germs of culture', although 'by 3500 B.C. the immigration was a thing of the dim and distant past, and the culture had long been acclimatized and developed on lines of its own' (1928: 28).

Diffusion of cultures and culture-traits, migration of people and invasion of foreign lands were heuristic tools for understanding the 'rise and fall' of civilizations during much of the twentieth century. As the anthropologist Alan Barnard has shown, diffusionism is not dead, and debates which are still waged in biological anthropology and archaeology between those who favour the 'out of Africa' or 'replacement model' and those who subscribe to the 'regional continuity model' of human expansion, present different variations of this theoretical model. But Barnard has also noted that 'diffusionism lives on through ideas such as that of culture area, world systems and globalization, and in the intensive study of regions' (2000: 54–9). In this sense all theories of transition between the phases or eras of the

¹⁸ Mortimer Wheeler was the Director-General of the Archaeological Survey of India from 1944 to 1948, and excavated Harappa in 1946 and Mohenjodaro in 1950. Although Wheeler had speculated upon the general similarities between Sumer, Egypt and the Indus after he had excavated in Harappa (Wheeler 1947), it was only after his excavations at Mohenjodaro that he explicitly mentioned that the 'idea of civilization came to the Indus from the Euphrates and the Tigris, and gave the Harappans their initial direction or at least informed their purpose' (1953: 93). However, he also held that 'a citizen of Ur in Mesopotamia would have found himself in a substantially alien world, had he been transported to a street of Mohenjodaro' (1952: 183), and suggested that the points of resemblance between Egypt, Sumer and Harappa could be 'ascribed . . . to the inherent cousinship of a social phase than to literal, local exchange' (1953: 94). During his presentation at the Marett Memorial Lecture in Exeter College, Oxford (on 7th June 1952), Wheeler first highlighted the importance of the transmission of ideas, which he felt played a 'dominant' role in intellectual discussions on past civilizations, but had been theoretically neglected as environmental factors received more attention. He also admitted that the 'absence of material transition from old idea to new form is a little baffling' (1952: 187).

Indus Civilisation which have been proposed by archaeologists working on the Harappan sites until now (e.g. Possehl 2002; Shaffer 1991), are variants of this frame of reference.

III

Mohenjodaro and Harappa offered the ‘authentic’ evidence for certifying the historicity of the Indian subcontinent before Alexander’s invasion. For, compared to India’s literary tradition which was classified by western, and mainly British, scholars to be ‘of the religious kind’ and from which ‘historical facts’ had ‘to be collected laboriously, bit by bit’ (Smith 1923: xviii), archaeological finds were deemed infinitely more reliable as historical sources.¹⁹ Although by the late nineteenth century ‘the labours of the Sanskrit scholars’ for promoting progress in Indian archeology was usually acknowledged (e.g. Cunningham 1871: xliii), the skepticism with which indigenous texts were received as being of historical value is best revealed through the observations of the Governor of Bombay, Mountstuart Elphinstone, on the ‘state of the Hindus at the time of Menu’s code.’ He wrote:

As the rudest nations are seldom destitute of some account of the transactions of their ancestors, it is a natural subject of surprise that the Hindus should have attained to a high pitch of civilization without any work that approaches to the character of a history . . . the fragments which remain of the records of their transactions are so mixed with fable, and so distorted by a fictitious and extravagant system of chronology, as to render it hopeless to deduce from them any continued thread of authentic narrative (1843: 19).

Elphinstone’s sentiments were fully echoed in the early twentieth century by Vincent Smith, the self-styled historian of India, and was foundational to the classificatory scheme he adopted for organising his sources, or ‘the original authorities for Indian history’ (1923: xvi) to write *The Oxford History of India*. But paradoxically, the confirmation

¹⁹ The date 326 B.C., derived from Greek sources, was regarded as ‘the earliest absolutely certain precise date’ (Smith 1923: xiv) for a historical event in India. The extent to which indigenous literary traditions were deemed unfit as sources for historical information during the mid-twentieth century is explicit in the remark, made by Professor Cowell in his introduction to Elphinstone’s *History of India*, that ‘it is only at those points where other nations came into contact with the Hindus, that we are able to settle any details accurately’ (in Smith 1924: 1). See also Bhandarkar (1895: iii).

of a prehistoric civilization through archaeology endowed the subject with a seminal role in substantiating evidence for the aryanisation of India.²⁰ As in his last article Edmund Leach commented, ‘rather than scrap all their historical reconstructions’ and start from scratch after archaeologists started to turn up evidence of the Indus civilization, ‘scholars in question managed to persuade themselves that despite appearances the theories of the philologists and the hard evidence of archaeology could be made to fit together’ (1990: 237). By the early twentieth century, when Harappa and Mohenjodaro were being excavated, the Aryans had been fashioned through empirical sciences as being culturally superior, with fair skin and caste hierarchy. They were systematically compared to the non-Aryan indigenous people, the *dasas* or *dasyus* of the *Rgveda*, and credited with the authorship of the Vedas. Despite the material finds, the three-fold periodisation of Indian history, first offered by James Mill, as Hindu, Muslim and British, remained and this new evidence was fitted into a ‘prehistoric’ past.²¹ Grand theories (e.g. Max Müller 1861) that envisaged a common homeland for the Indo-Europeans from which the Indo-Aryans migrated and supposedly invaded the Indian subcontinent also remained, as did the climate of colonial politics which manipulated them, and in which the scientific study of races allowed overlaps in typifying the *arya* within linguistic and physical genres. The explanations offered for the racial make-up of the population of Harappa and Mohenjodaro show very clearly that the material ‘evidence’ from the Indus valley was expected to sustain the existing narrative for historical India, retaining the central theme of an Aryan invasion to explain the beginnings of ‘Hindu’ history.

²⁰ At the core of the Aryan theory, which until the 1920s was largely developed through text-based comparative linguistics and field ethnology, was the understanding that the Indo-Aryans had migrated into the Indian subcontinent, were culturally superior to the indigenous tribes of India whom they had colonized, were a biological entity, and could be physically identified. See Trautman (1997), Bhatt (2001) and Kennedy (2000) for the first use of the term, the manner in which the migration and homeland theories were developed, and the establishment of the Aryan ‘race’ as a cultural marker through ethnographic, linguistic and historical surveys of South Asia.

²¹ Amalananda Ghosh, the Director General of the Archaeological Survey of India (1953–68), questioned the use of the word ‘prehistoric’ to define ancient civilisations in his presidential address to the Prehistoric Society in 1975, where he stated that ‘periodisation is in no remote way connected with terminology, for periods must be expressed in nominal terms... I now submit that the matter be reconsidered irrespective of any prestige attached to our having a long tradition and that we de-link the use of archaeological terms from national prestige’ (1975–6: 125).

Speculations related to the authors of the Indus Civilisation began to circulate even before any skeletal material was unearthed, and in this respect they were similar in character to the nature of claims made by archaeologists, such as Ajay Mitra Shastri, throughout the 1990s for the presence of a temple under the Babri Masjid at Ayodhya.²² In both cases, the evidence was forged prior to significant discoveries of material remains. The ‘positive evidence of intercourse between India and Western Asia before the first millennium’ inspired all discussions related to the authorship of the civilization, and Gordon Childe, by then a renowned archaeologist of prehistoric Europe, in his preface to *The Aryans* made it clear that ‘to await . . . the excavation of every mound in the Indus Valley would be cowardice’ (1926: xii). Childe suggested three possible ways of relating the meager archaeological data available to him in 1924 with the aryanisation of northern India. He proposed that ‘either the whole civilisation of the Punjab’ (where the site of Harappa was located) ‘is Aryan, or the Aryan elements enter at some date within the long ages represented by the accumulated debris . . . or [that] the Aryans were just the destroyers of the newly discovered culture’ (*ibid*: 34). Theories about physical resemblances between the Dravidians of peninsular India and the Sumerians, that were first aired by H.R. Hall (of the British Museum and the first excavator of Ur in 1919) in his *Ancient History of the Near East* (published in 1913), resurfaced, and the anthropometry of a Dravidian racial type played a significant role in analyzing the measurements which were taken of the excavated skeletal assemblages from 1926 onwards. Marshall, however, provided a circumspect review of these speculations (cf. 1931: 109), and held his belief that ‘any attempt to equate the Sumerians with ancient Dravidians is complicated at

²² The observing archaeologists who were present when the Babri Masjid was destroyed on 6 December 1990, claimed to have found an inscribed stone slab in one of its walls. The evidence for the physical presence of a temple was created through this accidental discovery. Ajay Mitra Shastri, then Professor of Ancient Indian History, Culture and Archaeology at the University of Nagpur, who acknowledging that the inscription was fragmented and could not be read in full, managed to ‘translate’ it, and claim that it ‘clearly tells us that a beautiful temple of Vishnu-Hari, built with heaps of stone, and beautified with a golden spire unparalleled by any other temple built by earlier kings was constructed’ (1992–3: 37). Although Shastri did not publish the full text of the inscription anywhere, he actively publicised his discovery of an eleventh-twelfth century temple with patrons throughout his lifetime (making it internationally known at the World Archaeological Conference held in Croatia in 1998), and linked it to the worship of Rama by establishing ‘evidence’ through this inscription, and through others found previously, from different regions and of an earlier chronology (*ibid*).

the outset by the difficulty of defining either the Sumerian or the Dravidian type' (1931: 110). But what he also discarded from the very outset, were the claims that the people of this civilisation were Aryans. In this respect, Marshall's footnote with an exclamation mark at the end to highlight the absurdity of the hypothesis of 'one Indian writer' who 'sought to find in this civilisation confirmation of [the] theory that the Aryans emanated from India itself' (*ibid*: 107, see also 111–2), expresses his sense of exasperation for those who ignored the historical narratives of the Aryan migration.

Six groups of skeletons from Mohenjodaro and the 'intrusive culture' of Cemetery H, represented for Wheeler the destruction of the city by Aryan invaders (1947: 81–2), and with this interpretation he offered the evidence to substantiate Childe's third proposition.²³ As Kenneth Kennedy (palaeo-anthropologist of South Asia) has pointed out, 'speculations about the archaeological traces of the Aryans which were feeble before excavations began in Mohenjodaro became firmly entrenched with continued excavations... and especially after the unearthing of the Cemetery H at Harappa' (2000: 366). What also becomes clear is that the practice of verifying a theory derived from a particular reading of a specific text, through material finds which were reckoned to predate the text in question by approximately a thousand years, posed no methodological problems.

The invasion theory, which accounted for the decline of the civilization in the Indus valley has served some of the major twentieth-century intellectual perspectives on political change and social progress (see also Kennedy *ibid*: 377). Wheeler's 'finds' effectively mirrored diffusionism, which has remained in vogue until the 1980s, and which has anchored all archaeological evidence for the origins of the 'chalcolithic cultures' that have been exposed in India since the 1950s.

It is interesting to note, and especially now that some archaeologists denounce the Aryan invasion theory as a 'plain racist myth' (Chakrabarti 2003: 205), that although scholarship on the

²³ Wheeler's evidence was discredited within twenty years. George Dales, from the University of Berkley, re-examined the contexts in which the bones from Mohenjodaro were found and realised that the skeletons belonged to different chronological periods. He could thus refute Wheeler's case for a single tragedy (1964). Thirty years since Dales' work, Kennedy has explained their disarray due to conditions generated during an epidemic, and stated that the Harappans, 'mindful of hygiene' may have interred them in 'those parts of the city which were abandoned during the later periods of cultural decline' (1994: 249).

aryanisation of the Indian subcontinent developed within the milieu of colonial politics, the quest for seeking the Indo-Aryans materially was fully developed through excavations of chalcolithic and early historic sites by Indian archaeologists only after the end of Raj.²⁴ Even the effective demolition of Wheeler's evidence (cf. footnote 23), failed to disturb their deeply rooted belief in the historicity of waves of migrating Aryans, and therefore the Aryan participation in the decline of the Indus Civilisation was never fully rejected even by those who have made insightful comments on the contingent nature of archaeological evidence (e.g. Malik 1968: 143).²⁵ India's once leading archaeologist, Hashmukh Sankalia, who from the 1950s tried to co-relate his discoveries of chalcolithic sites in Maharashtra with the tribes and dynasties mentioned in the Puranas, added another dimension to the Aryan involvement through his suggestion that 'if we regard the Harappan culture as Aryan, and the various Baluchi and pre-Harappan as pre-Aryan, then we can explain very well the subsequent spread of the Harappan Culture in Punjab, U.P., N.W.F.P., Gujarat, and Saurashtra, and its subsequent decay or disappearance by about 1700 or 1500 B.C.' (1975-6: 84). His speculations are now being revived by Lal (1997, 2002) and Gupta (1996) by interchanging the *arya* with the Hindu, and imposing a questionable methodology on groups of 'finds' from sites situated mainly within India, in Gujarat, Haryana, Rajasthan and Uttar Pradesh, to expose features of modern Hindu rituals.

IV

The same set of artifacts that Marshall selected to base his evidence for 'the religion of the Indus people' to be a 'linear progenitor of Hinduism', has been offered by Gupta to demonstrate that the religion was indeed 'Vedic-Hindu' (1996: 147). The distinction between Marshall's hypothesis and Gupta's terminology may seem trivial at first, as both trace elements of Hindu religious traditions back to

²⁴ Three of the earliest excavations which were conducted after Indian independence and which sought to correlate textual references for the Aryans with the excavated material, were Lal's at Hastinapura (1950-2), and H.D. Sankalia's at Nasik (1954) and Navdatoli (1957-9).

²⁵ Only Chakrabarti (1968) had questioned the usefulness of the migration theory, four years after Dales had demolished Wheeler's evidence of a massacre at Mohenjodaro.

this civilisation. However, there is a clear ideological divide between the two, which even Gupta (*ibid*: 148) has failed to notice while pointing out similarities between his views and those of Marshall. Marshall regarded the seal depicting the ‘prototype of the historic Siva’ (1931: 52–6), the ‘Great Bath’ (*ibid*: 24–6), and the finds of aniconic objects, terracotta figurines of women, and representations of objects of worship and ritual on seals, as ‘the only authentic and contemporary evidence . . . of the religious beliefs of the pre-Aryans’ which he suggested was external to the main tenets of the Indo-Aryan religion (*ibid*: 48). Unlike Gupta, Marshall saw in the Indus religion aspects of the ‘primitive’ religion of India’s indigenous ‘jungle tribes’ and suggested that some of the features were absorbed into Hinduism in later times.

Compared with Marshall’s informed guess, Gupta’s terminology of the Vedic-Hindu appears not only vacuous, but is also misleading. His choice, prompted by his thesis that everything about the Indus valley was indigenous to the Indian subcontinent (1996: 193), reveals his unwillingness to recognise a well-documented phenomenon, that Hinduism is an organic religion, which has changed (and is still changing) in its nature and form through time. Gupta’s Vedic-Hindu is simply another projection of the many colonial constructions of ancient India, where the Hindu, and particularly the Brahmanic, religion was identified as a singular faith with a concrete form, perceived as having remained unchanged and static through time, and understood as having dominated the cultural traditions of the majority living in the northern part of the subcontinent before they were invaded by the ‘Mohamedans’.

The continuities in religious practices from the Harappan to the early historical period that are traced by Gupta and also those who stay away from his brand of *Hindutva* history, such as D.P. Agrawal (1982: 452), include the entire corpus of the Vedic, Brahmanic and Saivite religious traditions. Are we then to infer, on the basis of the suggested religious roles for the artifacts, that all these traditions were germane within the Harappan religion and have ‘evolved’ from the third millennium B.C.? Lal writes of Harappan women wearing *sindura* in their *maanga* (2002: 83),²⁶ finds the *Pancatantra* stories of ‘the thirsty crow’ and ‘the cunning fox’ in painted pots from Lothal (1997: 175), and informs us that people in Kalibangan cooked on clay *tandurs*

²⁶ Lal’s reference here is to a Hindu practice, where married women apply vermilion to the hair parted on their foreheads.

(2002: 95). He gathers his evidence from regions as distant as Baluchistan and Gujarat to justify his belief that ‘there is no walk of life’ (presumably in India) ‘where we cannot discern the grass-root features of this ancient civilisation’ (*ibid*: ix). Lal however makes it difficult for us to ‘assess’ his evidence ‘with an open mind’ as he forces his objects, selected from different geographical distances and chronological time scales, to narrate evolutionary sequences of cultures and traditions of the ‘grass roots’ (*ibid*). And yet, it is his evidence that is being enshrined in *Ancient India* (Lal 2002: 91), the book currently subscribed for schools in India.

A shining example of the manner in which archaeological facts are established through conditions of knowledge is the so-called evidence that was established for the occurrence of granaries at Harappa and Mohenjodaro. The term is still retained in popular literature although it is at present being dismissed by contemporary excavators (e.g. Kenoyer 1998; Jansen 1993). Daya Ram Sahni, the excavator of Harappa in 1921, reported a structure with parallel walls ‘whose purpose and character’ he hoped ‘further explorations’ would determine (1927: 76). During his furlough in England in 1930, Marshall realised this to be a granary, and saw physical similarities between this structure and the *horrea* attached to many Roman forts in Britain (Vats 1940: 21). In his letter to Madho Sarup Vats, who subsequently excavated Harappa from 1926 to 1934 and authored the excavation reports, Marshall wrote that ‘in some Roman forts in England and Germany there are structures remarkably like the Great Granary at Harappa’, and mentioned his visit to the ‘Roman wall this summer’ where the ‘resemblance of one of these . . . to the Harappa Granary was striking’ (*ibid*: 16). In Mohenjodaro, the building south west of the Great Bath, which Marshall speculated in 1926 to be a ‘hot-air bath’ despite finding no clues for its use (1931: 143), was uncovered as the ‘great granary’ by Wheeler in 1950, through comparisons with similar structures in Crete and Rome (cf. Kenoyer 1998: 64), and not through any relevant finds within the premises, such as that of plant material. Wheeler’s vivid description of the manner in which he ‘hit upon the idea of a civic granary at Mohenjodaro’ in his semi-autobiographical work *Still Digging* (1955: 225) is a rare self-confession of how inferences for excavated objects have at times been negotiated.

It is obvious from the above, that definitions for granaries at Mohenjodaro and Harappa were partly based on comparisons with excavated structures elsewhere in the world, although Marshall

and Wheeler's functional interpretations for a structural complex, seemingly non-domestic, non-religious and comparable to storage buildings from other civilisations, can also be perceived as a logical extension of their enquiries into the urban-ness of the sites. Through their discoveries of a granary they could substantiate their judgment that Harappa and Mohenjodaro were urban cities, and could complement a situation where no other structure seemed to qualify as an obvious candidate for the public storage of grains. For, as Woolley's interpretations of the economy of prehistoric Ur clearly reveals (1925: 393), the importance of food storage and re-distribution for sustaining powerful ancient polities and urban economies was an accepted 'fact' by the time Mohenjodaro was excavated. By 1936, Childe had eloquently argued through his 'urban revolution,' the necessity for a surplus in food production to sustain the 'priests, princes, scribes and officials, and an army of specialized craftsmen, professional soldiers, and miscellaneous labourers' in Egypt, Mesopotamia and the Indus Valley (1936: 142). That the granaries neatly dovetailed the functional needs of 'an evolved and disciplined civic life' (Wheeler 1953: 183) is well highlighted in Vats' comments on the presumed specimen from Harappa. He believed that 'when there was no currency and taxes had to be paid in kind, the public treasury must have taken the form of great store-houses which are known from other countries as well, such as the long and narrow store-houses attached to the Minoan Palaces at Cnossus and Phaestus in Crete' (1940: 16).²⁷

In the presentations of the Indianised version of the Indus Civilisation, the Sindhu Saraswati Sabhyata, which has been conjured by archaeologists such as Lal and Gupta at present, one finds cruder instances of the ways in which archaeological evidence is being tailored to condition knowledge. A glaring example is the material proof currently offered, to establish the boundaries of a river Saraswati within and near the boundaries of modern India. The reference to this river can be found in the *nadistuti* hymn which appears in a chronologically later *mandala*, i.e. the tenth, of the *Rgveda*. The archaeological evidence, now being presented to situate the Vedic Saraswati on the boundaries of the Indo-Pakistan border, is largely based on speculations that were published in the 1980s, viz., that a river system ran parallel to the Indus during the third millennium B.C., and was drained by, among others, three rivers, the Satluj, the

²⁷ It is interesting to note that like Rakhal Das Banerji before him, Vats too used the Aegean civilisation as his frame of reference for comparison.

Yamuna, and the 'lost' Saraswati. These now flow into the Gangetic system (Misra 1984). The physical evidence for this river system, the Ghaggar-Hakra, was established through maps of landsat images taken during 1972–7, and remote-sensing the courses of three palaeochannels which were observed on these maps. It was also asserted at that time, that tectonic events in the Himalayas could have changed the courses of the rivers Satluj and Yamuna, and caused the Saraswati (and subsequently a part of the entire river system) to dry up (Yash Pal *et al.* 1984).²⁸ Rivers known in Pakistan today as the Hakra, Nara, Waihinda, Raini, and in India as the Ghaggar (which one also finds interchangeably mentioned as the Drishadvati and Saraswati within the Indian territories), were theoretically accepted as being original to the lost and partly dried up river system.

However, even before this 'evidence' was collated, the Saraswati had made its appearance within the Indian territories as an extension of the Chautang, on maps illustrating the distribution of Harappan sites, and in drainage systems for the northern region of the Indus Civilisation (e.g. B.K. Thapar 1982). Many excavators had also speculated on its flow (e.g. Bisht 1982).²⁹ So, it is not a coincidence that the launching of an archaeological campaign from the late 1990s that demanded a public recognition for the physical existence of the Vedic Saraswati running parallel to the Indus and towards the boundaries of modern India, has juxtaposed the hardening of Hindu politics. By corroborating inconclusive geo-morphological observations with the *nadistuti* hymn, relegating the chronology of the entire *Rgveda* to the fifth and the fourth millennium B.C., and thereby seeking chronological overlaps between the 'Rgvedic age' and the Indus Civilisation, Lal has demonstrated that the *Saraswati Flows On* (2002).³⁰ Others have sought to show that the distribution of 'Harappan' sites is concentrated mainly on the now dry channels of the Ghaggar Hakra, and in offering their proof that the latter is the physical relic of the Rgvedic Saraswati weave the tautology that 'the

²⁸ For an incisive analysis on how the evidence for the Saraswati has been archaeologically established see Habib (2000–2001).

²⁹ Jonathan Mark Kenoyer and Gregory Possehl also accept this geography, believing that the 'archaeological data supports the textual information that proclaims the ancient Saraswati as a great river with many populous settlements along its course' (Kenoyer 1997: 52), and that 'the Saraswati began to dry up at the beginning of the second millennium, but that seems to have taken a lot of time' (Possehl 2002: 36). They have not as yet speculated on its exact course from the source to mouth.

³⁰ See also Misra (1984, 1994) and Gupta (1995).

vivid description of the Saraswati as a perennially flowing, mighty and most sacred river in the many hymns of the Rigveda, and the largest concentration of Harappan sites on the Ghaggar-Hakra course, clearly establish that the Rigvedic Saraswati and the present Ghaggar-Hakra are one and the same river, as has been argued by many geographers, geologists, historians, and archaeologists for more than a century' (Misra 2001: 132).

The success of the involved archaeologists to make this 'mighty' river flow once more in India is best exemplified in the initiative taken by the Bharatiya Janata Party in October 2003, to develop 'The Saraswati Heritage Project', and it is not surprising that the Directorship of the project has been offered to an archaeologist. Different opinions, which needless to say are intellectually sound and relate to the ambiguities that are inherent in the textual references and in the geographical location of the ancient palaeo channels (cf. Thapar 2001: 19; Habib 2000–2001; Bhan: 2001: 45–6), are denounced by many archaeologists as being mere 'assertions of western linguists and historians and their more vociferous Indian counterpart that the Rigvedic Saraswati was the Helmand of Afghanistan' (Lal 2002: ix). However, the geographical extent of the Indus Civilisation is over a million sq km (Possehl 2002), the chronological span of the so-called mature and urban phase itself is over a thousand years, and reveals vastly differential increments of 'development' within the associated regions that are also vastly different in terms of their physical extent and material contents. To impose another category, in this instance the Sindhu Saraswati Sabhyata, neither refines the existing strategies through which artifacts associated with this archaeological phenomenon have been interpreted throughout the twentieth century, nor offers any framework for extracting better histories on early India.

V

The nationalistic jingoism that has clearly dictated the archaeological manufacture of an indigenous 'Sabhyata', has made significant inroads in the Indian archaeology of the Indus Civilisation throughout the twentieth century. The finds of a prehistoric civilization comparable in richness and scale to ancient Mesopotamia and Egypt had initially stoked national pride within British India, and Marshall's attempts at restricting its publicity to the foreign (and British) media was received by a sharp response from the Indian press that had

reasonably asked him ‘to whom does the credit of pointing out the real importance of these excavations go—to the officers of the Archaeological Department, or to scholars and antiquarians abroad?’ (*Amrita Bazar Patrika*, 1 January 1925). The demi-official letters between Marshall and Banerji in February and March 1925, articles which appeared in the Indian newspapers during the latter half of 1924, and parliamentary questions raised by Indian members of the Council of State in January 1925,³¹ offer a glimmer of the nature and extent of the conflict between Marshall, the British Director-General, and his Indian officers. Their aspirations to publicize their own past was severely thwarted through Marshall’s dictatorial control of the colonial bureaucracy within which they worked. The intense explorations to locate sites related to the Indus Civilisation along the Ghaggar-Hakra, mostly by the Archaeological Survey of India immediately after Indian independence (from the 1950s through the ‘70s), although ostensibly following Sir Aurel Stein’s explorations in 1942, were to a large extent initiated by a patriotic zeal to compensate for the loss of this more ancient civilisation by the newly freed nation; as apart from Rangpur (Gujarat) and Kotla Nihang Khan (Punjab), the sites remained in Pakistan.³² Subsequently, the divisive politics of India and Pakistan has prevented archaeologists in both countries from working on one another’s territories to clear perspectives on this archaeological entity, which literally straddles the two nations.

³¹ Marshall’s official circular dated 29 May 1925, asking Vats and Banerji to explain why they gave interviews to Indian newspapers, and the tone of Banerji’s reply (National Archives, New Delhi, D.O. 89/116); e.g. *The Statesman* 25 September 1924; and Khan Bahadur Ebrahim Harun Jaffer’s question to Marshall on 22 January 1925, asking ‘will the government please state the reasons why the Director-General of Archaeology did not publish an account of the recent discoveries in the Punjab and Sind in the press of India’ and ‘is it a fact that the Archaeology department refused to provide local papers with copies of photographs which were first published in England, even though the Indian papers offered to pay for them?’ By the end of the year, Marshall could comment that ‘the legislative assembly with whom the final say in these matters rests are now fully alive to the value of the work to be done’ (1927: 60). He could secure two and a half lakhs of rupees for carrying out his excavations in Mohenjodaro in 1925–26 (*ibid*).

³² A total of 917 Harappan sites were found in India by the 1970s through the efforts of the Archaeological Survey, the departments of Ancient Indian History, Culture and Archaeology in the Universities of Kurukshetra and Baroda, Deccan College (Pune), and the State department of archaeology in Gujarat. The explorations, initiated by Ghosh between 1950 and 1953, established the presence of approximately twice the number of sites that are now known in Pakistan. See Misra for details (1994: 512).

Since the excavations at Mehrgarh, in the Kachi district in Baluchistan, have offered a continuous chrono-cultural sequence from the seventh millennium B.C. to the third millennium B.C., the general consensus of the Pakistani, American and European archaeologists who have worked on sites in Pakistan have been that there is ‘no need to look outside the subcontinent to find analogies when we have such strong cultural and historical continuities in the actual region of study’ (Kenoyer 1997: 70, also Mughal 1991 and Jarrige *et al.* 1995: 94). Their view is similar to Marshall’s, who noted that ‘it may be, nay it is more than likely, that this civilization was the offspring of several—born perhaps rather of the soil itself and of the rivers than of the varied breeds of men which they sustained’ (1931: 109). Yet, despite their presentations, which oppose foreign roots for the civilisation, its sub-continental ethos is being fiercely re-iterated through claims made by Indian archaeologists such as Chakrabarti, that there was ‘no break whatsoever in the Indian prehistoric development; the basic story is that of adaptation of pre-historic people to different areas’ and that the ‘conscious aim of nationalist research should be to understand this adaptation in all its regional varieties’ (2003: 276). He is also of the opinion that ‘mercifully, this aim can remain uncluttered by the ambiguities of our literary tradition’ (*ibid.*), thus swinging his logic to accommodate a programme for historicising identities through a discrete category of sources, compiled neatly within the domain of the ‘archaeological’ alone.

In this respect, it is interesting to note that despite expressing sentiments such as ‘if [archaeology] has to strike deeper roots in... [the] educational system, it can do [so] far more effectively as a part of the general province of history than in isolation’ (*ibid.*: 217), archaeologists in India have increasingly begun to voice their anxieties when ‘mainstream’ historians write on India’s prehistoric past.³³ Curiously, individuals who do not exactly qualify as scholars of

³³ By way of example I quote Nayanjyot Lahiri’s comment on Irfan Habib’s book, *Prehistory* (2001) to highlight the clear distinctions that are now being made. She writes ‘that a State textbook corporation agreed to support such a project—a book on prehistoric archaeology written by a renowned scholar of medieval India—can be explained in, at least, two different ways. Either such institutions imagine that there are no archaeologists who can write this kind of books—a possibility that one hopes can be rejected outright. Or they believe that archaeology is only an extension of history and thus, a text book-like treatise on prehistory can be written by any historian’ (2001). It is curious that even those who teach archaeology within departments of history (in this case the University of Delhi) perceive such neat divisions of authority for writing on the past.

the field, and those sharing sectarian views, such as David Frawley, Koenraad Elst, Navaratna S. Rajaram, Bhagwan Singh and Michel Danino, are being allowed to present their non-scholarly opinions on Indian history through the once prestigious journals devoted to Indian archaeology, *Man and Environment* and *Puratattva*.³⁴ Considering that the distinction between archaeological and historical enquiry in the South Asian context can essentially be on choices of perspectives, and even these overlap as the two emanate from similar aims, to demand the authority of ‘archaeological’ sources and methods over others (e.g. *ibid*: 265) can only comply with one of the earliest observation made on Edward Said’s formulation of orientalist-imperialist relationships, that ‘the relation of any scholar to his material is essentially and necessarily a relation of power over it. No scholarly effort ever sees the light of the day without it being a sign of control over a certain body of material’ (Musallam 1979: 24).

Schemes for establishing a national consciousness through archaeology in India has involved casting slander, randomly calling western and many established Indian scholars, especially historians and indologists, colonialists, neo-colonialists, elitists and activists, and maligning their scholarship as ‘simplistic, pompous’ and ignorant of ‘the local and regional complexities’ (Chakrabarti 2003: 219). The bellicose tones of some of the recent publications (e.g. Chakrabarti 1997) seem to prove the point made by one historian of science, Mary Poovey, rather explicitly. Researching on a medical ‘discovery’ (chloroform) and its effects on representation (on Victorian women), she has remarked that the ‘interpretation of representation’ requires ‘the debates about how to tell a story’ and what it is of, since both are ‘about authority . . . to determine and legislate the “true” and who has the right to speak it’ (1986: 138).

This can certainly be one of the underlying reasons why Indian archeologists may wish to project the invincibility of their sources. It helps them keep ‘mainstream historians’ at bay, although, and not surprisingly, it is the latter who discard positivist perspectives while fashioning evidence out of excavated finds, and in the case of the

³⁴ According to one archaeologist, D.P. Agrawal, ‘the neo-converts to archaeology-coming from diverse backgrounds, from physics to fiction-seem to have taken over, and the older archaeologists who have devoted a life time in their disciplines are being taught how to interpret the past’ (2001: 21). Agrawal, however, does not comment on the complicity of the older generation (i.e. his), who accept the opinions of these neo-converts by frequently quoting them. For a recent example, see Chakrabarti (2003: 220–1).

Indus Civilisation, incorporate overlooked historical dimensions, such as human interference in the making of environment (Habib 2000–2001: 73, 84), and demonstrate how social changes encapsulated in the *Rgveda* preclude conferring concrete identities, be they linguistic or physical, on the *arya* and the *dasa* (Thapar 2001).

The rhetoric of finding proof through archaeology offers means of foreclosing dissent by invoking the authority of performing ‘science’, although one has to appreciate that the creation and appraisal of ‘scientific’ evidence established through archaeological methods require an intellectual grasp on historiography, since it is historical conditions that offer excavated artifacts the legitimacy of a proof. This makes a narrative for an aspect of the past derived purely through the archaeological methodology as contingent as that acquired through other intellectual means of investigation. Of the many unfortunate repercussions of national jingoism on interpretations related to the Indus Civilisation, the absence of a coherent professional disavowal for establishing a pseudo-Hindu culture in the third millennium B.C. through ‘archaeological’ sources has been the most glaring. It may well be a sign of a dismissal of historical consciousness by those who are involved in the excavation of Indian ‘grass roots’³⁵ that they can argue on the one hand that ‘the pursuit of a nationalist goal in archaeology is entirely justifiable as long as this does not bend archaeological data and is not used as an intellectual support for chauvinism’, and yet suggest that ‘in such multi-ethnic nation states as India the notion of the ancient past should not be based on the perceived and historically evolving and changing interactions, linguistic and other differences, but on the story of their interactions and common achievements’ (Chakrabarti 2003: 279). Indian archaeologists can perhaps remind themselves that nationalist histories do not achieve anything beyond an inversion of hegemonic premise, as ‘aspirations towards hegemony is the only possible response to hegemony’ (Chattopadhyaya 2002: 117). The senseless attack on the Bhandarkar Oriental Research Institute in Pune in January 2004, ostensibly provoked by an outrage to what a foreigner thought about Shivaji’s lineage, is just one

³⁵ An example is Chakrabarti’s statement that ‘the communal atmosphere in Indian politics in the 1930s and [the] 1940s was a product of the political forces of the day; nationalist image of ancient India had hardly any fundamental relationship with it’ through which he denounces Romila Thapar for proposing links between nationalist writings on ancient India and the emergence of Hindu communal aspirations during the early half of the twentieth century (1997: 12). See also Habib’s comments (in Muralidharan 2003: 9).

example of the destructive and opportunistic hegemony which has been imposed through this new nationalism on academic research on India.

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