

Book Reviews

Ronald Inden, Jonathan Walters & Daud Ali. *Querying the Medieval: Texts and the History of Practices in South Asia* Oxford: Oxford University Press (2000). 235 pp., index.

This book contains three substantial essays dealing with the history and texts of South Asia between the sixth and twelfth centuries. As the title indicates, the authors have attempted to re-think these centuries in significant ways and to write new histories of the post-Gupta period. The result is stimulating, challenging and important; however much one may take issue with specific points or the handling of certain themes, this is a book which historians of medieval India will find difficult to ignore.

The book focuses on different geographical areas, dynasties, texts and religious dispensations but forms a coherent whole. This is due to the fact that the contributors share a methodological and theoretical position, the most important aspects of which are: (1) that texts in South Asia are not static descriptions of external historical realities but were part of a 'scale of texts', that is, they were composed and re-worked in response to other texts and a variety of surrounding historical events, (2) that texts not only articulated and responded to particular circumstances but were part of the process by which situations were constituted, that is, texts were seen as having a power to create new social, religious and historical realities, (3) that texts were not composed by single individuals in the modernist sense but were produced by 'complex authors', that is, a tangle of sages, scholars, scribes, readers and listeners, all of whom, directly or indirectly, helped shape the texts that are preserved for us to study. To support this theoretical framework, the authors cite Collingwood, Volōšinov, Barthes, Foucault and others. In addition to these shared theoretical principles, the present volume is held together by the idea of 'imperial formation', an historical model developed by Inden to explain the political constitution of India from the Gupta period to the rise of the Sultanate.

Within this framework Inden, in the first essay 'Imperial Purāṇas: Kashmir as Vaiṣṇava Center of the World', argues that the *Viṣṇudharmottara-purāṇa* (VDhP) was a key text of the Pāñcarātrins who developed this massive compilation to achieve a number of ends, principally to absorb and surpass Vedic forms of ritual, to assert the supremacy of the Pāñcarātra 'disciplinary order', to forge a special relationship with the Kārkoṭa

Nāga dynasty of Kashmir and, through all this, to establish a new vision of the world in which a huge temple of Vaikuṅṭha, the special form of Viṣṇu venerated by the Pāñcarātrins, would stand at the centre of a Kārkoṭa imperial formation embracing all of India. In cultural, historical and religious terms, the Pāñcarātrin's was no small achievement and Inden's description of it is likewise. His arguments about how the VDhP construed its relationship to a 'scale of texts', that is, the orthodox traditions that the 'complex author' of the VDhP felt obliged to variously accommodate, critique or ignore, are themselves complex and carefully constructed. The implications of Inden's work for the study of medieval India are many. One interesting point to emerge is that the Pāñcarātra campaign to establish temple building and image worship as the central concern of Indian society was a salient cultural marker of the seventh and eighth centuries and one which particularly distinguishes this period from that of the Guptas.

Those who have read some of Inden's work previously will be familiar with his polemical style and his habit of addressing a host of issues along the way; the polemic is counterproductive and inclines us to doubt his reading; the asides, often expressions of disdain or amazement at how earlier scholars got it all wrong, represent an idiosyncratic approach to historiographical issues and ultimately weaken what is an important argument. Particularly curious is the author's insistence that he is not an Indologist. The uninitiated will be baffled by much of this, but Inden's theoretical position helps us understand: the book under review belongs to its own 'scale of texts' and is concerned not simply with describing Indian history in an objective way but with constituting a distinct position for the author within the American academy. Some understanding of the sociology can be had from Richard Eaton, '(Re)imag(in)ing Other²ness: A Postmortem for the Postmodern in India,' *Essays on Islam and Indian History* (New Delhi, 2000), pp. 133-55 and Kopf's deliciously vitriolic review of Inden's *Imagining India* in *JAOS* 112 (1992), pp. 674-77.

This writing helps us understand the current Indological divide but hardly excuses a failure to use correct scholarly apparatus. Thus it is odd that the VDhP, the centre-piece of Inden's essay, is not cited; in a staggering display of bibliographic vagueness, it is first referenced as the 'printed version' (p. 42, note 20). Certain key works bearing on the subject are also absent

from the notes. For example, reference is made to the *History and Culture of the Indian People*, no doubt a useful secondary survey, but R. S. Tripathi's *History of Kannauj* (Benares, 1937) and S. M. Mishra's *Yaśovarman of Kannauj* (Delhi, 1977) are not given in the appropriate place (for example p. 82). Is this because by citing them one is less able to make a mockery of Indological scholarship and thus less able to cut a dashing profile as a radical intellectual? As pointed out with unflinching precision by Richard J. Evans, *In Defence of History* (London, 1997), American post-modernists tend to represent previous scholarship in highly selective ways in order to exaggerate its apparent flaws and thereby advance their own reading of the past. This might explain why Inden has decided to ignore relevant theoretical work within Indology. The most notable omission is any reference to the writing of Hermann Kulke in the interesting discussion (pp. 54-5) of how the *VDhP* takes us beyond the regional horizon of the *Nilamata* toward a quest for pan-Indian dominion on the part of both the Pāncarātrins and their royal patrons. One would happily sacrifice much of the space given to structuralists and post-structuralists for some critical engagement with what Kulke has contributed to directly analogous problems in eastern India. While one would not like to make Inden's compelling analysis any more complex than it is, my own view is that some cross-regional comparisons are needed to develop and substantiate the arguments put forward. Another point which might benefit from elaboration is Inden's understanding of the *VDhP* as re-ordering the relationship between Pāncarātra forest-dwellers and Bhāgavata householders and residents of the plains (pp. 64-7). This is an important distinction but there is more to the contrast of forest and plain than first meets the eye: the radical renovation of ritual and kingship advocated by the Pāncarātrins also had physiological and environmental implications, some of which are hinted at in F. Zimmermann, *The Jungle and the Aroma of Meat* (Delhi, 1999). Thinking of the environmental setting of the *VDhP* with categories provided by the Indian medical tradition is part of the process of placing this and related texts 'on the ground' in actual places occupied by tangible 'human agents'. The process of moving the *VDhP* towards its geographical and historical framework also involves the Pāncarātra temples themselves, the (dare I say) empirical centre piece of Inden's argument (pp. 60, 66, especially 86). When we look at the actual remains, however, we find huge Buddhist buildings (an anomaly from the *VDhP* perspective) and so much destroyed otherwise that we can safely conclude nothing.

The second chapter in this book by Walters is entitled 'Buddhist History: The Sri Lankan Pali *Vaṃsa* and Their Commentary'. In this stimulating essay, Walters attempts to situate the *Dīpavaṃsa*, *Mahāvāṃsa*

and *Vaṃsatthappākasinī* (VAP) in their political, social and religious milieu. Here the idea of a 'scale of texts', beginning with the earliest canon but more particularly with the *Buddhavaṃsa*, shows itself to be an especially effective analytical tool. Walters demonstrates, convincingly in my view, that the *Vaṃsa* texts articulated specific phases in the long conflict between the Māhavihāra and Abhayagirivihāra at Anurādhapura, a conflict which the 'complex authors' of the *Vaṃsa* texts were able to turn into a struggle for the control of history. The final intellectual victory of the Theravādins and their subsequent domination of historical questions is shown by the fact that Tibetans first derived their calculations for the date of the Buddha from Sri Lankan monks resident at Bodhgayā. The *Vaṃsa* texts could be described as concerned with the politics of authenticity because the question of which school or 'disciplinary order' was the authentic heir of the first Buddha *saṅgha* ultimately determined which order was worthy of royal protection and patronage. The triumph of the Māhavihāra turned not just on historical matters but also on the way in which the 'complex author' used the VAP to articulate the imperial ambitions of the Okkāka dynasty. Here there seems to be a parallel to what the Pāncarātrins were attempting in Kashmir, a parallel not explored in this volume but one which suggests that this was part of a significant change in the ideology of South Asian kingship. As with Inden's essay, the arguments in this chapter are nuanced and intricate; the digressions and footnotes are a mine of interesting references and information.

The tradition of review-writing necessitates quibbles. Some minor points might be made about what Walters says of the Hemavatas, on which see 'Buddhist Saints in Ancient Vedisa', *JRAS* 11 (2001); Walters also teases us by citing (p. 105, note 6) his PhD dissertation 'Rethinking the Buddhist Missions', no doubt an important work but one which is, of course, not readily available. Let us hope that the key findings will be published without delay. More troublesome is the discussion (pp. 108-10) of the Śuṅgas and early Sātavāhanas, 'Buddhist emperors' who 'constituted their empires on the basis of a homology between the cosmic polity represented in the *Buddhavaṃsa*... and the disciplinary practices of the Buddhists living in India at the time'. This is contrasted with what took place later under the Ikṣvākus in the Andhra country. They 'allowed their queens and daughters to sponsor Buddhist activities' while they shaped their empires with a Theist (i.e. Vaiṣṇava and Śaiva) vision. The problem here, aside from the existence or otherwise of the Śuṅgas, is that there is no evidence of a 'shift' in the inscriptional records at early Buddhist sites; in the sculptural record (not discussed by Walters) just who and what is intended in the reliefs of Cakravartin kings from Amāravatī and

Jaggayyapeta is also something that has not been fully resolved. Particularly relevant in this regard is the Heliodorus pillar inscription at Vidishā which dates to circa 100 BC (which see Salomon, *Indian Epigraphy* [Oxford, 1998] p. 266). This mentions a Bhāgavata shrine and a king named Bhāgabhadra. He was not a Śuṅga. Just ten or eleven kilometres from the pillar is Sānchī, the best preserved early Buddhist complex in India. We may safely assume that Sānchī was in Bhāgabhadra's kingdom. Although there are something like 800 early inscriptions at the site recording donations from all manner of people, there is not single inscription recording the gift of a king. This evidence can be read in a number of ways. One possibility is that Theism was always present and that 'queens and daughters' may have always been the most visible patrons of the Buddhist dispensation. If the 'Buddhist emperors' of ancient India vanish into thin air then the situation in Sri Lanka becomes especially unique and interesting. To my mind this actually strengthens Walter's analysis of the VAP and underlines the remarkable attempt this text makes to articulate an imperial Okkāka vision with Theravāda Buddhism at its centre.

The ambitions of the Okkākas brought them into alliance and conflict with the dynasties of south India, their most notable encounter being with the Cōlas. This takes us to the final chapter by Daud Ali, 'Royal Eulogy as World History: Rethinking Copper-plate Inscriptions in Cōla India'. This essay deals with an astonishing set of thirty-one sheets of copper, weighing nearly two hundred pounds and engraved with 816 lines of writing. This is largest set of copper-plates ever found and one of the most extraordinary documents relating to medieval India. Ali persuasively argues, however, that this is much more than a simple 'document'. By restricting themselves to the documentary aspect of this and other inscriptions, historians have, in Ali's view, missed much of what they have to offer. In addition to recording a complicated property transaction, the plates provide information about the composition, distribution and use of texts; more interestingly the plates also provide an imperial history which articulates the Cōla's understanding of their place in the world. The 'scale of texts' Ali explores begins with the Purāṇas and ends with inscriptional eulogies (*praśasti*), the link between the two being made by genealogies which trace the Solar descent of the Cōlas and the Lunar descent of their rivals. The case for reading inscriptions as texts or inscriptions with texts is not, I think, something with which anyone would take issue, yet Indologists have had to struggle to bridge the epigraphic-textual divide. Some effort to address this problem has been made recently, notably by Schopen and Trainor in their analysis of early Buddhist material. Ali's essay is a landmark in this regard for it takes us into a period when there are a substantial number of

inscriptional texts with which to work.

Ali insists that inscriptions need to be taken as an expression of a particular world view and that we should not unwittingly infect our reading of them with preconceived ideas about the political, social and economic relationships that existed at the time. By accepting, at least intellectually, the concept of the universe, definition of time and nature of life as articulated in these texts we can start to understand why they were written and what they were trying to do. Inden and Walters share this view. This is not so radical as the authors would have us believe. Could one really find an active scholar who would take issue with this approach? Where we might part company with the present authors is their attempt to rationalise the claims which kings made in their texts about 'ruling the whole earth', 'conquering the world' and so forth. The idea of 'imperial formation' is used in this book to engage with these claims. This model was developed by Inden in *Imagining India* (Oxford, 1990) and is summarised here on pp. 24-5. The starting point is a critique of 'colonial' historiography which generally assumed that the medieval period was a time of cultural decadence and political fragmentation, a time when a confusing parade of dynasties interacted like so many 'repellent molecules'. In contrast to this, Inden has asserted that major dynastic powers were able to exercise political and ritual hegemony by surrounding themselves with a 'circle' of subordinate kings. Broadly speaking, the Kārkoṭas of Kashmir were the first imperial formation after the Guptas; they were followed by the Cālukyas, the Rāṣṭrakūṭas and, finally, the Cōlas. Imperial formations of this type were not static. Each dynasty struggled constantly to rebuild and remake its imperial formation lest it be overtaken by ambitious neighbours who were always ready to remake the world according to their own vision. This is, in my view, a very compelling historical model. It usefully accounts for the political dynamics of medieval India, a good instance being Ali's description of the events that followed the sack of Mānyakheṭa in 972 (pp. 192-3). The imperial model also helps us explain the rhetoric of inscriptional texts and various military and ritual undertakings. For these and other reasons Inden's model has been influential beyond the present volume, for example André Wink, *Al-Hind: The Making of the Indo-Islamic World* (Leiden, 1990), chapter 5.

But does this model stand for the whole medieval period? Going back to *Imagining India* (pp. 214ff.) where the Rāṣṭrakūṭas are used as a foundational test case, we can see that Inden has not been altogether impartial in his handling of the evidence. The essence of the problem is that Inden, in a laudable effort to take his sources seriously, assumes that Arab accounts and Rāṣṭrakūṭa epigraphic claims should be read at face value. While

most scholars would agree that we need to accept the statements of our sources as a starting point, difficulties soon emerge with the eighth and ninth-century material because several dynasties articulated a world vision using conventions of rhetoric that are very close to those found in Rāṣṭrakūṭa inscriptions. Reading these partisan and highly politicised claims against each other is a long-standing tradition within Indology, a good example being Tripathi's *History of Kannauj*. Schooled at the University of London and guided by L. D. Barnett of the British Museum, Tripathi's classic exemplifies a method that is still going strong because it works so well (for example see p. 200 in the volume under review). So the question of whether the imperial formation model can stand scrutiny boils down to whether it can co-exist with a 'critical cross-reading' of dynastic records. A brief survey of the ninth century highlights the problem; a few but not all of the relevant inscriptions will be cited in the course of this summary.

At the beginning of the century the Rāṣṭrakūṭa king Govinda III (c. 794-814) was able to upset the Pāla scheme to install a puppet on the throne of Kannauj and thereby build an imperial formation that embraced all of north India from Kashmir to the Bay of Bengal (*EI* 18 [1925-6], p. 245). These events are mentioned in Pratihāra records but they tell us that their king Nāgabhaṭṭa II (c. 810-33) played a key part in bringing down the Pāla pawn (*EI* 18 [1925-6], p. 108). The Pratihāra inscriptions are predictably silent about Rāṣṭrakūṭa involvement in the matter. During Nāgabhaṭṭa's reign and that of Mihira Bhoja (c. 836-85), the Pālas remained a vital and dangerous power. On the southern flank of the Pratihāra kingdom, there was a stand-off with the Rāṣṭrakūṭas. Neither enjoyed hegemony in practical or symbolic terms: land-granting powers and the territories of subordinate kings remained intact (*IJ* 39 [1996], p. 133; *ToG*, map 6 for find-spots of relevant records). During this time cross-border raids were frequent but not decisive (*Ibid*, map 6 and *IoG*, p. 2). It was not until the time of Indra III (c. 915-28) that the Pratihāra heartland was attacked in earnest (*EI* 7 [1902-03], p. 43). This seems to have had a devastating effect on the Pratihāras and allowed subordinate rulers, such as the Candellas, Kalacuris and Paramāras, to carve out independent kingdoms from the Pratihāra dominion. In the ninth century there was, therefore, no Rāṣṭrakūṭa imperial formation in the strict sense of the term because neither the Pratihāras nor Pālas were subordinate to the Rāṣṭrakūṭas. Inden passes over this century and the Pratihāras because this would require certain qualifications of his model. Of course it could be argued that the Pālas were a minor and peripheral power (a dangerous assumption) and that Nāgabhaṭṭa II was defeated and thus symbolically subordinate to the Rāṣṭrakūṭas (*EI* 18 [1925-6], p. 245). But with these

possibilities we are brought full circle. Whether the medieval period was marked by political fragmentation or imperial formation is based on a number of *a priori* working assumptions which govern how we read and construe the surviving evidence. Inden has effectively shown that medieval kings had the political theory and ambition to create imperial formations; precisely when and where these kings achieved them is another matter.

Certain distortions can come from an overly insistent application of the imperial model, for example in this volume where Ali states that in their northward campaign the 'Cōḷa generals, in alliance with a Paramāra force from Mālwa, defeated the Oḍra (Utkala, Orissa), Kaliṅga (coastal Andhra) and Somavaṃśi kings of South Kosala (p. 200)'. While the imperial model necessitates seeing the Paramarās as subordinate allies, the Nāgpur Museum inscription of Paramāra Naravarman describes the encounter somewhat differently (H. V. Trivedi, *CII*, volume 7 [Delhi, 1978]: number 33, vv. 35-54). Similarly, Walters says that Mahinda IV of Sri Lanka was 'genuinely in a position to claim paramount overlordship' (p. 140). Although they were undoubtedly powerful beyond the island, it is simply unbelievable that the Okkāka dynasty was anywhere close to a paramount position in the tenth century. This shows how a model, rather than evidence, can start to dictate conclusions.

In closing I would like to turn to Inden's introductory remarks at the front of the volume. This touches a number of issues and almost every reader will find something there to irritate them. I will not dwell on details but rather on some general themes which I regard as key. Inden quotes avant-garde thinkers in an attempt to radicalise this volume, but the results are curiously old fashioned. In the end we are dealing with texts, the long-standing centre piece of Indological study. But just what is a 'text' in South Asia? Does this overarching category cover everything that was written down or are there useful subdivisions which can be made in the material? Does the word *sāstra* cover all this activity? There is no extended discussion of this problem and no engagement with extant writing on the subject, for example Pollock, 'The Theory of Practice and the Practice of Theory in Indian Intellectual History', *JAOS* 105 (1985), pp. 499-519. Pollock has put forward some serious arguments about the nature of knowledge and *sāstra*-texts in south Asia; Inden can hardly proceed by pretending that Pollock's influential work does not exist. The remarkably different vision that we have of India from Pollock and Inden perhaps springs from the fact that one reads *Mīmāṃsā* and the other *Purāṇa*; this is a problem that merits further work. A different but similar problem relates to the discussion of periodisation. In a book that calls itself 'Querying the Medieval' we might hope to find some engagement with relevant literature, for example Green, 'Periodisation in European and World

History, *Journal of World History* 3 (1991), pp. 13-53. The sole reference to U. N. Ghoshal's 1965 'Periods in India History' (cited p. 16) is hardly adequate.

Despite many guises, Inden is essentially an institutional historian concerned with kingship. His work on ritual, caste, Vedism and so forth all comes back to kingship which sits at the core of his historical analyses. But other forces were at work and other 'agents' (to use his terminology) had concerns to nurture. The dynasties discussed in this book have been swept away, but Theravāda monks still recite the *pātimokka* in Sri Lanka and the Theist mendicant orders of India, inscrutable as they often are, still dedicate body and soul to their supreme Lord. Could it be that the 'disciplinary orders' put great historical processes in motion for their own special ends, harnessing kings to their schemes? The radical transformation of the Tibetan kingdom by Padmasambhava and Śāntarakṣita in the eighth century suggests this might be one way of querying the momentous events of this period. Like this book, the medieval is at once fascinating, infuriating, illuminating, perplexing, seminal and irrelevant. We can never be done with it, however much we try.

Michael Willis

Abbreviations

CII	<i>Corpus Inscriptionum Indicarum</i>
EI	<i>Epigraphica Indica</i>
IA	<i>Indian Antiquary</i>
IJ	<i>Indo-Iran Journal</i>
IoG	<i>Inscriptions of Gopakṣetra</i> (London, 1996)
JAOS	<i>Journal of the American Oriental Society</i>
JRAS	<i>Journal of the Royal Asiatic Society</i>
ToG	<i>Temples of Gopakṣetra</i> (London, 1997)

Anila Verghese, *Archaeology, Art and Religion: New Perspectives on Vijayanagara*. New Delhi: Oxford University Press (2000). xvii + 342 pp., 10 maps, 17 figures, 80 plates.

This book of essays deals with a far wider range of topics than its title suggests. Anila Verghese provides innovative analyses of Vijayanagara and its numerous monuments by looking at data gathered by the Vijayanagara Research Project alongside inscriptions and travellers accounts. Besides looking at temples, she considers the importance of memorial stones, as well as monuments connected with food consumption, the performing arts and martial display. Beginning with general essays about the site, the book then telescopes into detailed studies of specific themes and their cultural implications.

The first four chapters provide an overall picture

of the site, its history, the role of royal patronage and temple morphology. The introductory chapter establishes Vijayanagara's historical context and gives a cursory description of its different monuments. The chapter that follows considers Vijayanagara's archaeological history beginning with the antiquarian investigations of Colin Mackenzie in the early nineteenth century, and ending with a detailed survey of work done between 1975 and 1995 connected with the Vijayanagara Research Project. The third chapter traces the evolution and growth of the city by looking at monumental and inscriptional evidence. Temple growth was connected with patronage, and this patronage extended into other areas such as the gifting of villages to temples and the construction of new suburbs.

Chapter four looks at the development of temple architecture from the fourteenth to the sixteenth centuries by setting up a chronology of inscriptionally dated temples. The oldest temples at Vijayanagara are Deccani style freestanding shrines, while those constructed later are temple complexes built in the Tamil idiom. Verghese concludes that a straight-forward progression from simple to complex architecture did not occur. Although the elements of Tamil style temple architecture and planning became increasingly dominant towards the sixteenth century, smaller Deccani style temples continued to be constructed, showing that one style of temple architecture was not supplanted in favour of another.

The fifteen chapters that follow move beyond the general picture of Vijayanagara and its history. Instead they focus on distinct features of specific monuments. For example, chapter six looks at the iconography of *sati* stones alongside the many sensational travellers' accounts of the practice of *sati* at Vijayanagara. Although *sati* was clearly an important social practice at Vijayanagara, its scale and frequency was definitely not as widespread as the travellers' accounts suggest. Such accounts provide useful information, but are for the most part exaggerations. When considered in conjunction with the iconography and frequency of *sati* stones, the information in travellers' accounts can be more faithfully interpreted.

Chapters seven and eighteen also consider the importance of memorial stones. Chapter seven looks at carved images of the Shaivite folk deity, Mailara, three of which are on freestanding stone slabs. These images show that the cult of Mailara was not widespread. The absence of Mailara images inside larger temples shows that his worship at Vijayanagara never synthesized with the cults of more popular deities. Chapter eighteen looks at two hero stones and five other carvings of an unidentified warrior doing battle with an enraged elephant. There are no identifiable written accounts of this hero, but the iconography and placement of these