

of Manu (mid-second century CE) and gradually in the wider Dharmaśāstra tradition under the rubric of *dharma* of the ruler, a point also reiterated by scholars such as Donald R. Davis, Jr.

The present translation should be seen against the backdrop of all these complexities in scholarship teasing out questions of antecedents and afterlives. Olivelle undertakes his project commendably well. A further enduring underlying concern that runs through the volume is the approach to translation with particular reference to the technical meanings of key terminologies while at the same time engaging with earlier commentaries and translations by modern scholars. Moreover, readers will find the appendices—on fauna and flora (Appendix 1), weights and measures (Appendix 2) and geographical names (Appendix 3)—extremely useful, and the notes adequate and scholarly. Together they facilitate in crucial ways the reader's understanding of the technical terminologies, their usages, later commentarial as well as modern scholarly traditions. In short, Olivelle's book is a project that problematises the passage of a core text to its present received form and in so doing both extends and goes well beyond the remits of a bare literal translation.

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K. Rajan, *Early Writing System: A Journey from Graffiti to Brahmi*. Madurai: Pandya Nadu Centre for Archaeological Research, 2015, XXII + 439 pp., ₹2290 (Hardback).

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There is a serious rift in the field of ancient historical and archaeological research between north and south India. One of the main reasons is the lack of familiarity with south Indian languages on the part of north Indian historians and with north Indian languages on the part of south Indian historians. The situation is not desirable, and I am not aware how it can be rectified unless the students are encouraged to choose their research topics in areas beyond their own regional and provincial linguistic domain. Considering the current state of ancient historical and archaeological research in Indian universities, much of which is due to the abysmally low academic quality of its teachers recruited most shamelessly on the basis of caste and regional/national politics, the situation is unlikely to change in the near future.

One of the manifestations of this dichotomy is the general lack of awareness of the various problems specific to different parts of the country. The beginning of writing is one such issue, on which a great amount of light is thrown by the book under review.

I am not competent to write about the graffiti or about what the author Professor K. Rajan calls Tamil-Brahmi. I shall only try to make clear how this book throws important light on the general issue of the beginning of writing in early historic India. Basically, the book is a report on the author's Kodumanal excavations with special reference to all its excavated data on graffiti and other inscribed material.

The Asokan inscriptions provide a major fixed point of the early historic Indian script in the sense that it is the earliest material available. Not so long ago there were some scholars who believed that they could point out some pre-Asokan cases of Brahmi, especially in the script of the Sohgaura inscription from eastern Uttar Pradesh. It may kindly be noted that ancient historians are usually opinionated persons who operate mostly without firm evidence, that is, evidence which can be corroborated by any independent method or evidence. One of the men who refused to accept any pre-Asokan evidence of Brahmi was the late D.C. Sircar, and I suspect that it is primarily because of his academic eminence that the theory of pre-Asokan Brahmi has been held in abeyance for some time. In fact, a few scholars have emerged arguing that it was the King Asoka who was personally responsible for the discovery of Brahmi. Opinions such as these only show that given half the chance, modern historians of ancient India will not stop at anything!

What about the literary sources regarding the beginning of historic writing in India? The problem about these sources is that scholars are seldom unanimous about their dates. Let us take the case of the Buddhist *Jataka* stories. I believe that they relate to conditions around 500 BC and this I do mainly on the ground that the principality of Kasi was an independent kingdom under Brahmadatta when the *Jataka* stories were being narrated and that this independence was lost when the Magadhan king Ajatasatru annexed it in the sixth/fifth century BC. The logic splitting over the *Jatakas* has never ended; some people argue that their verse portion is earlier than their prose portion, and for some inscrutable reason, D.D. Kosambi, a mathematician who thought he knew how ancient Indian history should be written, took it for granted that the context of the *Jataka* stories included early centuries AD. In any case, the *Jatakas* were familiar with writing and this literary text should suggest that writing was known in historic India around 500 BC. By the way, why there is so much uncertainty regarding the dates of Indian texts? The reason is simple: they are usually undateable. You cannot date a text on the basis of its language. You can put it in a linguistic frame, of whose general date you may have an idea but nothing more than that can be argued. Language *per se* does not date anything. Historians who call for 'interdisciplinary' historical research through language studies simply forget this simple truth. In some quarters of modern archaeology, what is happening in the name of 'archaeo-linguistic' studies is precisely this. The extent to which one can accept or deny the premises of 'archaeo-linguistics' depends on the extent of one's faith in the premises built up by orthodox 'comparative philologists' of the nineteenth century. That 'New Archaeology' of the 1960s and 1970s, which began by swearing in the name of 'science', should reduce itself to linguistic mumbo jumbo of the nineteenth century is possibly an object lesson in how archaeology need not be confused with hard science. Current proponents of 'archaeo-linguists' have also forgotten that 'comparative philology' which is the progenitor of their 'archaeo-linguistics' had a strong smell of racism about it. Proof—hard, solid and verifiable proof—is what any study—historical studies included—needs. At least,

there should be strong circumstantial evidence or a strong circumstantial logic behind our historical premises.

One of the main reasons why such hard proof or hard circumstantial proof is missing from ancient Indian studies is that our writings were generally inscribed on palm leaves or birch leaves and such writings have not survived. We seldom wrote on perishable materials like clay which, once burnt, became well-nigh imperishable. This is how so much of the ancient Mesopotamian and Egyptian literature has survived whereas the surviving Indus civilisation corpus of writing is amazingly incomplete. This must be the reason why the administrative archives of ancient India have not survived. Some inscriptions do, in fact, imply that there were administrative store houses of documents. That the Indians preferred to record most of the things of their lives on palm leaves has been known even as late as the late nineteenth century when the Indian census recorders of the period returned their 'proformas' incised on palm leaves.

Sringaverapura is a site on the bank of the Ganga, not far upstream of Allahabad. Birch leaf fragments have been identified in its Black-and-Red Ware level dated around 800 BC at the site. The nearest source of birch leaf (*Betula utilis* or *bhurja patra*) is the Himalayas, possibly Garhwal hills. What is the point in importing these leaves to Srinagaverapura unless they were used for writing? This is certainly a piece of hard circumstantial evidence in favour of pre-Asokan existence of writing in early historic India.

The volume under review puts forward the direct hard evidence in the form of incised Brahmi script dated around 500 BC at Kodumanal. This hard evidence has taken a long time in coming. Sometime in the 1990s, excavations at the Sri Lankan site of Anuradhapura yielded examples of Brahmi script inscribed on pottery in the radiocarbon-dated context of mid-fifth century BC. There was nothing to doubt this dating. Many examples of the so-called Tamil-Brahmi script they found at Anuradhapura have been found in many places in south India, and what the Anuradhapura discovery ought to have given birth to was the belief that similar inscriptions from the south should go back to the mid-fifth century BC. This regrettably did not happen. Our scholars preferred to ignore the Anuradhapura finding.

In the late 1990s, Professor Rajan was in Cambridge as an Academic Staff Fellow attached to me. We consequently had an opportunity to discuss the dates of his Kodumanal excavations which had yielded no radiocarbon date then. However, Professor Rajan's discussion on the stratigraphy of the site convinced me that its earliest level was certainly pre-300 BC. I believe I supported this opinion in my *India—An Archaeological History* published in 1999 and *The Oxford Companion to Indian Archaeology* (2006). Further, in my *The Ancient Routes of the Deccan and the Southern Peninsula* (2010), I laid down the archaeological and historical basis of my argument that the early historic urban growth in south India should date from about 500 BC. In the context of north India, I called it a process from 800 to 500 BC.

After the two relevant sites excavated by Professor Rajan—Kodumanal and Porunthal—and their radiocarbon dates are taken into account, there is no reason to doubt for a moment that the archaeological evidence of the Brahmi script in Tamil Nadu is about 500 BC. Correspondingly, early historic urban growth in Tamil Nadu should also date from this period. This is an argument which I made in my *The Ancient Routes of the Deccan and the Southern Peninsula* without even the radiocarbon dates.

Archaeological discoveries when they upset the traditional beliefs should be matters of great rejoicings. Professor Rajan's discovery belongs to this category, and I congratulate him on relentlessly pursuing his work and emerging eventually successful.

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**Joan Mickelson Gaughan, *The Incumberances: British Women in India, 1615–1856*.
New Delhi: Oxford University Press, 2013, xv + 279 pp., ₹895 (Hardback).**

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As the title of the book suggests, the highly masculine world of Western men pursuing geographical and mercantile expansion across the globe was extremely unwilling to allow white women—married or single—into the excitement as well as the uncertainties of travelling to strange countries like India. This reluctance—typically represented by the East India Company—was particularly strong when the Company's imperial intentions in the subcontinent were still unformulated while its mercantile interests were also precariously dependent upon the goodwill of the Mughal court as well as of the various Indian princes. Gaughan shows how this institutionalised male resistance to women's presence notwithstanding, women did manage to come to India from the seventeenth century (and perhaps even earlier) and their impressions of India were sometimes very different from the dominant masculine perspective. Though Gaughan concedes that by the nineteenth century the women were so cut off from the indigenous population, their opinions reflected the most blatant imperialist take on the 'natives': yet the exceptions to this again reflected a totally different perspective from the mainstream, right up to the mid-nineteenth century, after which the Revolt of 1857 choked off all communication between the Indian population and European women. The book is helpfully divided, using chronological brackets, into three parts, and each part has essays arranged around a theme. Part I shows the seesawing opinions of the Company Directors in the early seventeenth and eighteenth centuries against the needless expenditure of having to pay for the maintenance of wives and families of employees. Their reluctance was compounded by the fact that the employees often suffered from the hot tropical climate and as a consequence, their mortality rates were high. The dead employees then became the Company's responsibility, something which the pinch-penny Directors did not relish. The Company Directors were also wary of the wives who were not their employees, and thus could become distressingly independent.

Thus, a clearly nervous set of Company Directors frowned upon Frances Steele because she had given her merchant husband a head start over Sir Thomas Roe by managing to find an entry into the Mughal zenana, thereby upsetting Roe's delicate negotiations with the Emperor. The second essay, 'The Incumberance', sharply reveals the Company's consternation at being unable to check a woman's independent forays into areas so far completely managed by men. Wives, though discouraged, did force the Company's reluctant hand if the employee happened to be a very valuable and