

Review Article



Tamil through Epigraphical Lenses

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Abstract

The collection of papers by Indian and western scholars, senior and junior, published in the book *New Dimensions in Tamil Epigraphy* edited by Appasamy Murugaiyan is doubly welcome as it concerns epigraphical sources with a focus on Tamil Nadu. In this contribution are offered a summary of each essay in the volume along with comments and suggestions for deepening the arguments presented.

Keywords

Epigraphy – Tamil – Tamil Nadu – Sri Lanka – Cōla

Appasamy Murugaiyan (ed.), *New Dimensions in Tamil Epigraphy: Select Papers from the Symposia held at EPHE-SHP, Paris in 2005, 2006 and a Few Invited Papers. Edited with an Introduction by Appasamy Murugaiyan*, Chennai: Cre-A, 2012, xvii + 354 pp., with b/w plates, ISBN 978 8 192 13024 8. ₹ 750.

This volume contains fourteen contributions: ten papers presented during two international symposia held at the École pratique des hautes études, Paris in 2005 and 2006, and four invited papers. They are arranged according to five topics: history and philology; archaeology, iconography and history of art; archae-

ology and maritime trade; social anthropology; linguistics. The contributors are Indian and western scholars, senior and junior. The book is published in Chennai at an affordable price—a laudable effort to make research done in the west available to an Indian audience.

The title—which promises to open new ways in using Tamil epigraphical documents—is somewhat misleading as several contributions are only remotely related to epigraphy while others make a conventional—although in no way illegitimate—use of epigraphic sources. The volume is regrettably rather poorly edited from the formal point of view. No uniform scheme of transliteration (see “Note,” p. vi) has been adopted for Tamil.¹ This might not be a problem for modern place-names, but it is unfortunate when technical terms are used without diacritics. Articles are unevenly furnished with abstract, end bibliography and numbering of illustrations. Typographical errors occur on a scale that could have been avoided. Despite these formal defects, the volume contains valuable information and innovative research.

Not being trained in all of the many disciplines represented in the volume, I will provide a summary of each contribution and offer comments of a general reader or of a specialist, depending on the degree to which Tamil epigraphy is used by the authors.

Daud Ali (“The epigraphical Legacy at Gangaikondacholapuram: Problems and Possibilities,” pp. 3–34, with 4 illustrations and 1 table) offers a preliminary study of the epigraphical corpus of the temple at Gangaikondacholapuram (Kaṅkaikoṅṭaṭṭaṭṭapuram, Gangai) founded in the first half of the 11th century by Rājendracōla I. A useful comparison is made with the earlier major Cōḷa royal foundation at Tanjore (consecrated in 1009/10). For its global approach of the epigraphical corpus of a single temple and its attention to its spatial and chronological distribution, this study, mindful of Leslie C. Orr’s recent recommendation, is perfectly suited as the inaugural contribution to the volume.²

The author first traces the history of the epigraphical survey of the temple. He remarks that fourteen inscriptions copied between 1806 and 1818 by the

1 In the present review, the transliteration scheme of the *Madras Tamil Lexicon* is adopted for Tamil.

2 See Leslie C. Orr, “Preface,” in *Pondicherry Inscriptions*, Part I, Pondichéry, École française d’Extrême-Orient & Institut français de Pondichéry, 2006. For a study of the entire epigraphical corpus of a single temple adopting similar methodology, see Charlotte Schmid, “Au seuil du monde divin : reflets et passages du dieu d’Ālanturāi à Puḷḷamaṅkai,” *Bulletin de l’École française d’Extrême-Orient* 92, 2005, pp. 39–157.

Mackenzie team were not found in later surveys by the Archaeological Survey of India (ASI).³ He also recalls that material was retrieved from the temple in order to build the Lower Anicut (a dam-and-bridge on the Kāvēri River) in the mid-19th century (p. 6). This might in fact explain the disappearance of these “Mackenzie” inscriptions. The focus of Ali’s study is the thirty-nine inscriptions collected by the ASI. These are conveniently presented in a table (pp. 23–28), arranged according to their location in the complex. Ali corrects some mistakes about locations (the nine inscriptions collected in 1892 are not on the enclosure wall, but on the central shrine) and reveals that a survey in 2008 resulted in the discovery of four so far unreported inscriptions, while “not all the inscriptions noted by the ASI could be located” (p. 5). After a succinct architectural history of the temple, the author characterises its epigraphical corpus: most inscriptions are fragmentary, on stray and sometimes reused stones; on the central shrine, most are damaged or mutilated. While the city and temple were founded in order to commemorate the victorious campaign of Rājendracōla I in North India—presumably completed in 1023, and from which the king obtained his title Kaṅkaikoṅṭacōla, “the Cōla who took (i.e. conquered) the Gaṅgā”—Ali assesses that the first reference to the city is dated to 1029, that arrangements made in favour of the temple in 1035 by Rājendracōla I are known through a later record (No. 1 in the table, which will be our point of reference here), and that the foundation of the temple is mentioned in the copper plates of Ecālam, dated to 1036. The author gives credit to Pierre Pichard’s theory that the new capital was founded so as to escape from factional politics at Tanjore (pp. 8, 13). Ali underlines the enigmatic fact that we have no inscriptions of Rājendracōla I on this temple, which contrasts with the well-known inscriptions Rājarājacōla I had engraved at Tanjore. Ali then reviews the whole epigraphical corpus.

The inscription No. 1, dated to 1068, is the one that retrospectively mentions orders made by Rājendracōla I in 1035 and 1036, notably reallocating to the temple villages that had earlier, as pointed out by R. Nagaswamy, been donated by Rājarājacōla I to the Tanjore temple. Ali concludes that 1035 might be the year of achievement of the temple of Gangai, the city having been founded earlier. He wonders why these orders of 1035–1036 were not immediately recorded in inscriptions and suggests that the death of Rājendracōla I in 1044 might be the reason. But according to Ali, the mystery remains because the inscriptions of Rājarājacōla I at Tanjore were also posthumous. In my view this posthumous character is in fact not certain. In any case the contrast between the two tem-

3 These fourteen inscriptions were later edited by T.N. Subramaniam in *South Indian Temple Inscriptions*, vol. 2, Madras, 1954, Nos. 671–684, pp. 641–646.

ples is well illustrated: at Gangai, there is no inaugural inscription like at Tanjore,⁴ only three Cōla inscriptions on the central shrine and “no unified agenda or programme for the ‘textualisation’ of the temple” (p. 12). The author rightly contrasts the different treatment of mouldings at both sites, resulting in the fact that there were flat surfaces available—and lots of them—for easy engraving at Tanjore, but not at Gangai. That is to say the patronage of the temple of Gangai by Cōla court is “problematic” as there is “no substantial amount of moveable wealth (images, ornaments) or services (temple personnel or temple supplies) donated to the temple” (pp. 12–13) as in Tanjore. Most of the patronage epigraphically attested dates to the late 11th and 12th century while the existence of a Cōla palace at Gangai is confirmed for the same period (see endnote 38, and also inscription No. 3).

Ali then deals with two purely eulogistic inscriptions that suggest “royal interest in the site for its symbolic potential” (p. 13). The inscription No. 20, so far unpublished, is undated and consists in two mutilated Sanskrit verses also found at Chidambaram. The verses praise Kulottuṅgacōla I,⁵ mentioning his victory over the five Pāṇḍyas and the installation of *vijayastambhas*.⁶ Ali recalls the rivalry that opposed Kulottuṅgacōla I with Adhirājendra (to whose reign some inscriptions at Gangai are dated) and suggests “the presence of factions loyal to both kings” at Gangai (p. 15). In fact, I would suggest that the inscription No. 20, which is undated, has been commissioned by Gangai courtiers seeking to attract the favours of Kulottuṅgacōla I (so that he maintain the capital and its palace there?) once he emerged as the sole Cōla king or/and that it was a statement of allegiance. The inscription No. 2, also so far unpublished, is the most enigmatic: dated to the 4[1]st regnal year of Kulottuṅgacōla I, it opens with his *meykkīrtti* (metrical Tamil royal eulogy) and is followed by four verses which are “a verbatim copy of a portion of the *praśasti* found in the copper plates of the Gāhaḍvāla kings of north India” (p. 16). Ali convincingly suggests that the link between these two dynasties is to be sought in Kulottuṅgacōla I’s early career rather than during Rājendracōla I’s rule, as the first extant Gāhaḍvāla inscription is dated to 1090. The suggestion of a marital alliance is

4 *South Indian Inscriptions*, vol. II, No. 1.

5 Ali translates Kulottuṅga as “one from lofty family(ies)” (p. 15), which is acceptable. It might also be translated as “one lofty in the family;” that is loftier than his rivals and thus deserving the throne.

6 These five Pāṇḍyas are reminiscent of the five Pāṇḍavas, the more so because the Pāṇḍyas seem to have associated themselves with the (five) Pāṇḍavas. See V. Venkayya, “The Pallavas,” *Archaeological Survey of India, Annual Report 1906–1907*, 1909, p. 220, n. 6.

speculative, as Ali himself admits (p. 17). So is that of Gāhaḍavāla influence with regard to the introduction of sun-worship in the Tamil country. The enigma of this inscription lies in the fact that it is not damaged, but does not mention any transaction (it was thus “not completed or did not contain any grant portion,” p. 18). Nevertheless the inscription implies some kind of political alliance between Kulottuṅgacōla I and the Gāhaḍavāla dynasty.

Ali finally deals with the Pāṇḍya records at Gangai which testify to “the continued importance of the city and temple” (p. 19) even as the Cōla empire declined. An important conclusion of this study is indeed that the temple was still “active in the latter half of the thirteenth century, and was important enough to attract the patronage of the Pandya rulers” (p. 20). The temple even retained some importance until the end of the 15th century, as inscriptions No. 9–12 attest the presence of Vijayanagara nobles or rulers at Gangai.

As Ali indicates, work remains to be done in terms of on-site research and fuller comparison with other monuments (Tanjore and Chidambaram). Notable in this respect are the inscriptions by later rulers (the Marathas) that we find at Tanjore. One should also somehow deal with the inscriptions reported to have been copied at Gangai in the Mackenzie collection.

Y. Subbarayalu (“Conception and Understanding of Some Technical Terms in Tamil Inscriptions,” pp. 34–46) discusses the meaning of several terms attested in medieval Tamil inscriptions. A longer version of this article—in which the term *kōṇērīnmaikoṇṭān* is additionally discussed—has appeared elsewhere in the same year.⁷ The author assesses that *vati*, rather than a road, designates a canal and more precisely the branch of a main canal (*vāykkāl*). As for *kuṭumpu*, it should not be translated as “ward,” as this term is related “with the grouping of cultivated land” (p. 37) and was later replaced by the term *kaṭṭalai*. The term *veṭṭappēru* (i.e. *veṭṭappēru*) is not related to *vēṭṭal* (“sacrificing,” according to the *Madras Tamil Lexicon*), but should be understood as *veṭṭippēru*. It designates a land grant to people subjected to *veṭṭi*, a duty of maintenance of irrigation works. The author also discusses fiscal terminology (*vāram*, *pañcavāram*, *devadāna*, *iraiyili devadāna*). The discussion is generally well-informed and the new interpretations acceptable. I was however somewhat surprised when reading endnote 8—not to be found however in the longer version of the article—which states that the term *devadāna* “should be spelt *dēvatāna*, meaning the place or ground of the temple.” It seems that the second term of the compound

7 See “Interpreting Inscriptional Terminology,” in Y. Subbarayalu, *South India under the Cholas*, New Delhi, Oxford University Press, 2012, pp. 59–74.

is understood as the Tamil form of Sanskrit *sthāna*, and not *dāna*. In fact we have plenty of Sanskrit inscriptions mentioning a *devadāna*, which unequivocally means gift (*dāna*) to god (*deva*).

In the last paragraph the author advocates the building of a digital corpus of inscriptional texts and warns us that “picking up terms from inscriptions should not be mechanical: serious thinking would be necessary all along to dissect a text intelligently and cull out the relevant term or passage” (p. 44). In this respect, it may be useful to observe that the tools to do this are now available, in the framework of the Text Encoding Initiative (TEI) with the EpiDoc norm, that is XML format files with mark-up adapted to epigraphical texts. This approach, initially developed for Greek and Latin inscriptions, has already started to be applied to Indic epigraphy.⁸

Osmund Bopearachchi (“Andhra-Tamilnadu and Sri Lanka: Early Buddhist Sculptures of Sri Lanka,” pp. 49–68, with 13 illustrations) focuses on three newly found Buddhist artefacts (two statues and a panel) from Tissamaharama on the southern coast of Sri Lanka. To begin with, the author reminds us that in the first centuries of the Common Era, South India and Sri Lanka, with their ports set up on estuaries of rivers, were part of the international trade network of the Indian Ocean, that Roman traders had no direct connections with Sri Lanka because of the time gap between the two monsoon winds, and that the South Indian traders served as intermediaries between Sri Lanka and the Mediterranean world. Then he describes and interprets the three items. He determines from a study of style and of material that the first statue, carved in white marble, is an import from Andhra Pradesh, while the second one, carved in local stone, “was executed by artists from Andhra living on the island or by Sri Lanka sculptors using an imported statue from the Krishna valley as a prototype” (p. 57). As for the third item—a guard-stone with a central two-armed male figure surrounded by several attendants—Bopearachchi rightly compares it with a very similar piece, now in the museum of Yatala, at Tissamaharama, which had been identified as a Nāgarāja. He argues that it could be a Bodhisattva (Maitreya or Avalokiteśvara/Padmapāṇi) or a Cakravartin. The demonstration is sound, but one wonders why Bopearachchi does not point out that in both sculptures, there is no canopy of *nāga* heads for the central figure. Although the presence of umbrellas indeed makes one think of a Cakravartin, still, as admitted by the

⁸ See <http://en.wikipedia.org/wiki/EpiDoc>, accessed March 7, 2014, and in the field of Indic epigraphy, <http://isaw.nyu.edu/publications/inscriptions/campa/index.html>, accessed March 7, 2014.

author, none of the seven treasures of the Cakravartin are actually represented. Boppearachchi further discusses other guard-stones, which are not Nāgarājas, and which he would identify as Avalokiteśvaras rather than Maitreyas. He concludes that while it is certain that Sri Lankan artists were inspired by sculptures from the Buddhist centres of Andhra Pradesh, they developed their own distinct school. This contribution thus confirms the close connections between Andhra Pradesh and Sri Lanka early schools of sculpture, adding to the demonstration sculptures so far unpublished.

Charlotte de Percin-Sernet (“Why Does Caṇḍeśa Sport a *Jaṭābhāra*?” pp. 69–80, with 7 illustrations) tries to demonstrate that the figure known as Caṇḍeśa is at some periods represented with a *jaṭābhāra*, that is a mass of braided hair normally serving as indication of a wrathful character, with no relation to his actual functions, whereas a *jaṭāmukuta*, mark of a benign character, would be more suitable. She attempts to account for “this mismatch between function and representation” (p. 70) by sketching the successive functions of the god. In 7th-century Andhra Pradesh, at Bhairavakonda, he would be a fierce guardian, paired with a benign Gaṇeśa. In 8th-century Kāñcīpuram, he would have become the model devotee with a chignon in the Caṇḍeśānugrahamūrtis and is no more a fierce deity as he is no longer a doorkeeper. While starting at that time to be the consumer of *nirmālya* (the remnants of offerings touched by the god), this mutation in function would not have been followed in the iconography, as he retains the *jaṭābhāra* when represented seated. Later, in Cōla temples, the difference between the assessor of the god and his devotee would have been marked in the hairdo (*jaṭābhāra* versus chignon).⁹ In the 11th century, while the inscriptions reveal a new function of Caṇḍeśa as supervisor of transactions in Śiva’s temples, iconography would have been in a transitional period for the devotee’s representations. In the 12th century, the *jaṭāmukuta* would have tended to be generalised in accordance to the new functions of the deity. The conclusion is that the *jaṭābhāra* would be inherited from earlier functions as a fierce deity and that “new attributes [i.e. functions] explain a need to adjust [the] iconography” of Caṇḍeśa, by depicting him with a chignon.

This article is very short and I have tried to clarify its argument. If I have correctly understood the author’s views, her argument fails to convince me. Firstly, the exact date of the caves at Bhairavakonda is not clearly established

9 The author mentions an exceptional case of a standing Caṇḍeśa with *jaṭābhāra* at Tanjore (beginning of the 11th century).

and they might in fact be later than the 7th century.¹⁰ In the Kailāsanātha, the seated Caṇḍeśa might still in fact be a guardian as he is still paired with Gaṇeśa.¹¹ No wonder thus that he bears a *jaṭābhāra*, which in fact also fits Caṇḍeśa as supervisor as well as consumer of *nirmālya*: in the first capacity he is to be feared by would-be offenders to transactions concerning Śiva's estates; in the second he accomplishes a dangerous task that only a fierce deity could carry out.¹² I am however not able to comment on the 12th-century corpus of Caṇḍeśa images as I do not know it very well. The author lists in her bibliography a long article by Dominic Goodall, but does not seem to have made extensive of it, although it is, to my knowledge, the best informed recent synthesis on Caṇḍeśa.¹³

Virginie Gazon's contribution ("Iconographic Programme during the Reign of the Chola Queen, Sembiyaṅ Mahādēvi," pp. 81–98, with 1 map, 1 plan, and 14 illustrations) concerns the famous Cōḷa queen Cempiyaṅ Mātēvi. Spouse of a Cōḷa king (Gaṇḍarāditya/Kaṅṭarātittan) and mother of another one (Uttamacōḷa), she is hailed in Tamil historiography as an exemplary devotee for the temples she had built, reconstructed or endowed in the second half of the 10th century. The problem discussed here is whether she can be held responsible for the reworking—that is the addition of sculptures—of a series of temples, as scholars such as B. Venkatarman and S.R. Balasubrahmanyam "seem to feel" (p. 94).

The method of the author is quite plain and sensible. In the first place she analyses the iconographic program of the temples known, through epigraphical evidence, as foundations or re-foundations by our Cōḷa queen.¹⁴ She determines that they present a homogenous program with few variations (listed p. 89). Secondly, she does the same for the set of temples that we know, again

10 Bhairavakonda (Prakasham district, Kanigiri mandal) should in fact be spelt Bhairavakōna, so as to avoid confusion with Bhairavakoṇḍa (Prakasham district, Cumbum mandal), where sculptures and inscriptions have also been found.

11 See Emmanuel Francis, *Le discours royal, Monuments et inscriptions pallava (IV^{ème}–IX^{ème} siècles)*, PhD dissertation, Louvain-la-Neuve, Institut orientaliste, 2009, p. 396, n. 388.

12 See Erik af Edholm, "Caṇḍeśa and the Sacrificial Remnants. A Contribution to Indian Gastrotheology," *Indologica Taurinensia* 12, 1984, pp. 75–91.

13 Dominic Goodall, "Who is Caṇḍeśa?" in Shingo Einoo (ed.), *Genesis and Development of Tantrism*, Tokyo, University of Tokyo, 2009, pp. 351–423.

14 These are, according to the author's list, the Śiva temples at Kōṅērīrājapuram, Tirukkōṭīk-kāval, Aṅaṅkūr, Cempiyaṅ Mātēvi (a village named after the queen), Māyavaram (also Mayilāṭuturai), Kuttālam, Tiruvārūr, and Āṭuturai.

through epigraphical evidence, were founded before Cempiyaṅ Mātēvi but that she supposedly reworked.¹⁵ She finds that they “form a less homogenous ensemble” (p. 94) and that some of their additional sculptures might betray an influence of the model provided by the temples of Cempiyaṅ Mātēvi. Nevertheless, the absence in these temples of any inscription of the period of her son or any inscriptional mention of the queen, as well as the presence of images which are stylistically late (pp. 91–94), leads the author to conclude that the suspected sculptures are additions postdating the period of Cempiyaṅ Mātēvi. Another argument is that we have evidence that Cempiyaṅ Mātēvi took care of having earlier inscriptions recopied when she had temples rebuilt. (The inscriptions are explicitly introduced as copies of older records at Tirukkōṭikkāval for instance.) “As a result, it is difficult to admit that these iconographic additions destroying the inscriptions [i.e. at Tiruppuṅampiyam and Karantai/Karuntaṭṭāṅkuṭi] have been undertaken under her direction” (p. 95; see also p. 82). This conclusion looks solid, and is welcome because it counterbalances a fascination for Cempiyaṅ Mātēvi on the part of certain Tamil scholars, who have been prone to exaggerating her contribution.

One must, however, say that Gazon oversimplifies the opinion of the scholars she criticises. In fact S.R. Balasubrahmanyam speaks of the “impact of Sembiyan spirit” (see endnote 49). One also feels that the author is too much dependent on the secondary literature. A fresh but demanding re-examination of the whole corpus of inscriptions attributed to or mentioning Cempiyaṅ Mātēvi might open new insights, as the ongoing doctoral research by Nicolas Cane (École pratique des hautes études, Paris) reveals. In fact there are some inscriptions not yet fully taken into account in the secondary literature. Moreover, it is simply incorrect that there is no mention of Cempiyaṅ Mātēvi (p. 90) in the six supposedly reworked temples.¹⁶ Gazon’s corpus of temples comprises only fourteen temples. But there are other Cōḷa temples, the inclusion of which in the comparative study might have been relevant.¹⁷ It also appears, again based on earlier works mentioned in the bibliography and on Cane’s

15 These are the Śiva temples at Tiruvēlvikkuṭi, Tiruppuṅampiyam, Tiruvāṭuṭurai, Karantai/Karuntaṭṭāṅkuṭi, Kāmaracavalli, and Puñcai.

16 As Nicolas Cane (personal communication, April 2014) has pointed out to me, Cempiyaṅ Mātēvi offered a silver pot at Tiruvēlvikkuṭi (*South Indian Inscriptions*, vol. XXXII, part II, No. 34) and another one at Tiruppuṅampiyam (*ibidem*, No. 233).

17 See the temples described by G. Hoekveld-Meijer, *Koyils in the Coḷamaṅḍalam: Typology and Development of Early Coḷa Temples*, PhD dissertation, Vrije Universiteit te Amsterdam, 1981, pp. 167 ff., including the one at Māṅampāṭi, which is the subject of Rachel Loizeau’s contribution in the volume under review.

work, that at least one temple (at Viruttācalam) and possibly as second one (at Tirumaṇaṅcēri)—both with six niches on the *maṇḍapa*—can be added to the list of (re)constructions by Cempiyaṅ Mātēvi,¹⁸ while there is reason to doubt that the queen is responsible for the Māyavaram temple. Furthermore, the argument that additions would have destroyed earlier inscriptions is taken from S.R. Balasubrahmanyam (see endnotes 38 and 49), but requires further investigations. With regard to the image of Campantar at Karantai (photo 14, p. 93), Uthaya Veluppillai has recently remarked that it is framed by an uninterrupted inscription dated to 1015.¹⁹

Rachel Loizeau's article ("The Nāganātheśvara Temple at Mānambāḍi: a Chola Monument Dedicated to the Glory of Śiva and His Devotees," pp. 99–125, with 13 illustrations) is welcome, since the temple under study is relatively unknown while displaying noteworthy sculptures.²⁰ The epigraphical corpus of the temple consists in nine inscriptions, dated from the beginning of the reign of Rājendra-cōla I (r. ca. 1012/14–1044) to 1108.²¹ From the iconography and structure of the temple, the author however dates it to the time of Rājārājacōla I (p. 99), that is "to the very end of the tenth century or early eleventh century" (p. 120).²² Rachel Loizeau shows that the structure and iconographic program is influenced by that adopted in temples attributed to Cempiyaṅ Mātēvi (pp. 101–107), which is meant to substantiate her dating. She emphasizes the presence of portraits and devotees (pp. 108–110), hypothesizing that some might be royal portraits, because they frame the Naṭarāja sculpture, an image which "has become the tutelary divinity of the Cholas under the reign of Uttama Cōla" (p. 110). The most interesting panels, in my view, are found in the arches above the niches

18 See *South Indian Inscriptions*, vol. XIX, No. 302 and *Annual Report on Indian Epigraphy* Nos. 9–10 of 1914.

19 See Uthaya Veluppillai, *Cikāli: hymnes, héros, histoire, Rayonnement d'un lieu saint shivaïte au Pays Tamoul*, PhD dissertation, Université Sorbonne Nouvelle-Paris 3, 2013, p. 188. This, however, proves the anteriority of the place designed for a sculpture, not of the actual sculpture there, which might still be a later replacement.

20 The author however does not mention anywhere the few pages devoted to this temple by S.R. Balasubrahmanyam et al. (*Later Chola Temples: Kulottunga I to Rajendra III (A.D. 1070–1280)*, [Madras], Mudgala Trust, pp. 421–425).

21 But see the author's endnote 2 about two possible earlier inscriptions on the northern wall.

22 The author in fact follows Hoekveld-Meijer's tentative dating (op. cit., pp. 175–176) to 988–990 because of "structural and iconographical affinity" with the Kapardīśvara at Tiruvalaṅcuḷi.

and represent exemplary devotees (pp. 110–118). The author convincingly identifies Cīruttonṭar, Kaṇṇappar, and Kōcceaṅkaṇṇāṇ. Finally, she describes the miniature panels on the plinths of the temple (pp. 118–120).

While epigraphy is summoned to supply a *terminus ante quem* for the monument, the author notes that “[t]he plinth iconography ... seems to confirm the hypothesis that the niches must have been added after the construction of the temple and that the reliefs [i.e. those of plinth] must have been sculpted at the same period” (p. 119). In this respect, she could have pointed out that the inscriptions are all on the base (p. 101), which makes one suspect that the unengraved walls are later. Loizeau argues that the dancing Śiva of the plinth dates to this later phase because he is encircled by flames as in the case of bronze figures. If so, one may doubt that all sculptures, including those in the niches and arches, date to the reign of Rājarājacōla I, as implicitly argued by the author. Admittedly from their style, it is difficult to date them to the 10th century or early 11th century. Most of the sculptures could then date to this later reconstruction of the temple, the structure and iconographic programme of which could be inherited from Cempīyan Mātēvi’s model, but recreated. It follows that the representations of devotees on the arches, interesting as they are, might not be as early as ca. 1000.

A plan of the temple showing the location of the different sculptures would have been welcome. As for the possible royal portraits, one should notice that the epigraphical corpus does not seem to attest any direct involvement of the Cōla kings in the affairs of the temple (the content of the inscriptions is summarised, p. 101). On miniature panels, other relevant publications could have been mentioned.²³ Note also that figure 11 is a Kālāri and not, as indicated in the caption, a Kāmāntaka, anyhow represented on another miniature panel.

Bernard Gratuze and Sarah Guillaume (“Analysis of Tamil Nadu Glass Beads: Application to the Study of Inland Glass Trade,” pp. 129–143, with 2 tables and 5 illustrations) aim to refine the recent work of Laure Dussubieux about glass artefacts.²⁴ This work revealed new insights into the Indian trade with

23 See Charlotte Schmid, “Aventures divines de Kṛṣṇa : la *līlā* et les traditions narratives des temples *cōla*,” *Arts Asiatiques* 57, 2002, pp. 33–47, and “Of Gods and Mortals: *Līlā Cōla*,” in Catherine Jarrige & Vincent Lefèvre (eds.), *South Asian Archaeology 2001, Volume 2: Historical Archaeology and Art History*, Paris, éditions Recherche sur les Civilisations, 2005, pp. 623–636.

24 Laure Dussubieux, *L’apport de l’ablation laser couplée à l’ICP-MS à la caractérisation des verres : application à l’étude du verre archéologique de l’Océan Indien*, PhD dissertation, Université d’Orléans, 2011.

Southeast Asia and the Mediterranean world, in the centuries before and after the turn of the Common Era, and made possible distinction between locally produced and imported artefacts. While, as far as Tamil Nadu is concerned, Dussubieux analysed artefacts from Arikamedu (Arikkamētu) only, the corpus of Gratuze and Guillaume consist in 150 artefacts from seven archaeological sites. The model of Dussubieux is confirmed: the same distinction is tenable also within the bigger corpus. As for Arikamedu, it was indeed a bead-making site, but there is no evidence of glass-making. The authors conclude that, since dating the artefacts is imprecise as it cannot be based on typology or physical analysis, a further step to be taken is to study “well-dated glass finds, as only correlation between a precise stratigraphy and the chemical production will allow the understanding of glass production in the Indian Ocean, as has been done in the Mediterranean world” (p. 137).

S. Suresh’s article (“Epigraphical Evidences for the Roman Trade and Roman Artifacts in Ancient India—With Special Reference to South India,” pp. 144–157, with 6 illustrations and 1 map) seems basically a summary of the author’s book.²⁵ The tone is sometimes more emphatic than seems justified by the evidence produced in the article itself.²⁶ The purpose is to critically examine the meagre Indian epigraphical evidence pertaining to Indo-Roman trade between 300 BCE and 300 CE. Thus, in order to complement the studies in the field, which are mostly based on Roman numismatic finds in India and the *Periplus*, it “mainly discusses little-known epigraphical evidences of Indo-Roman trade” (p. 146). The author examines the term *yavana*, mentioning references in inscriptions and in Tamil literature. He asserts that “[i]t is now commonly accepted that the *yavanas* mentioned in [the] inscriptions [from Maharashtra-Andhra] are Roman traders” (p. 148) without giving references for this common view.²⁷ The author himself, however, acknowledges that *yavana*

25 *Symbols of Trade: Roman and Pseudo-Roman Objects Found in India*, New Delhi, Manohar, 2004, which is mentioned several times in notes for further details.

26 See p. 144 (“Thousands of Romans and their African and Arabian representatives came to India. Many of them even settled there, adjusting themselves to the Indian climate and lifestyle”) or p. 145 (“It [contact between ancient Rome and India] extended to exchange of diplomatic embassies and cultural interaction, which led to substantial Roman influence on Indian economy, society, architecture and art. ... Roman artists settled in South India and began to teach their skills to Indians”).

27 This common view might be found in some of the “references to *yavanas* in Indian literature and inscriptions” collected in endnote 4. To this collection an article by Herman Tieken (“The *Yavanas*’ Clothes in Old Tamil Literature,” *Indo-Iranian Journal* 46, 2003, pp. 261–271) should be added.

from “Ionian” came to mean “any foreigner coming to India from West Asia or the Mediterranean world” (p. 147). While Suresh suggests that some donors mentioned in the Tamil-Brāhmī inscriptions may have been *yavanas* (p. 149), he must admit that references to *yavanas* in Caṅkam literature are rare and that, in the extreme south, “the occurrence of the term in the records is extremely limited although this was the region where the Romans came and settled in vast numbers” (p. 151). Suresh however argues that “[p]lausibly, the Tamil speaking people addressed these foreigners by other terms of which we are unaware” (*ibidem*). With such a line of argumentation, one can prove anything. As for the gold coins mentioned in Indian inscriptions he refers to (for instance *suvarṇa* in the Mahākṣatrapa Nahapāna’s inscription at Nāsik, *paḷankācu* in Pallava inscriptions), he favours their identification with the Roman *aurei*, mostly with the argument that gold coins from northern dynasties are not found in the regions concerned. This is highly speculative and disputed, and no new insights are contributed to the debate. The author’s argumentation does not make it necessary to assume that there is a reference to Roman trade and artefacts in these inscriptions. It really does nothing to counter the view that the scarcity of mention of Roman traders in Tamil sources merely reflects the fact that the number of Roman traders involved was not so significant or that only a limited number of them settled in India, even though this view is based on an *argumentum e silentio*.

Iravatham Mahadevan offers a very short article (“Messengers of Trade and Faith: Ancient Pottery Inscriptions from Sri Lanka Found in India: An Annotated and Illustrated Catalogue,” pp. 158–169, with a table, 12 illustrations and 1 map). This is a catalogue of twelve inscriptions in Sinhala-Prakrit and Sinhala-Brāhmī script, dated from the 1st century BCE to the 1st century CE, found in Tamil Nadu (ten items) and Bengal (two items), on pottery (eleven items) and bone (one item). These inscriptions consist only in personal names, which are ownership marks, some with additional titles. The main interest of this contribution is that it is “a ready reference to information gathered from scattered publications, some of the most recent ones in Tamil” (p. 159). Mahadevan also summarises the characteristics of the script and language of the early cave and pottery inscriptions of Sri Lanka (pp. 160–161). Furthermore, information about the twelve items (including readings) is provided in a table, while all of them are illustrated. Unfortunately the map (p. 168) is hardly legible. The material is interesting as it attests to the exchange between Sri Lanka and the continent. It is made easily accessible to researchers interested in evaluating the proposed readings, interpretations and dates.

K. Rajan and V.P. Yatheeskumar (“Cultural Transformation from Iron Age to Early Historic Times: A Case Study of the Vaigai River Valley, Tamil Nadu,” pp. 170–202, with 4 maps, 8 illustrations, and 1 table) present a three-fold argument. First, the so-called Iron Age that preceded the Early Historic period in Tamil Nadu (which conventionally starts with the introduction of script) does not represent a single homogenous culture. Second, and consequently, to call that age Megalithic Culture is a misnomer. Third, the late Iron Age was continued in the early Historic period, meaning that there is no clear-cut rupture between these two periods. The demonstration is based on the archaeological data of the Vaikai river valley, with detailed maps and an extensive table listing and describing 196 archaeological sites. The authors underline the importance of trade routes. They argue for the continuity between the late Iron Age and the Early Historic period: graffiti of the late Iron Age could be discovered to be inscriptions and thus precursors of the earliest identified inscriptions so far, which belong to the Early Historic period. This leads them to affirm that “this region had entered into Early Historic period well before the 5th century BCE” (p. 198) and to place the “transformation from late Iron Age to early Historic period ... around the 6th to 5th centuries BCE” (p. 200). The article also offers a useful presentation of what are considered to be the earliest hero-stones of India (at Pulimāṇ Kōmpai and Tātappaṭṭi), although the date proposed (4th and 3rd century BCE) is questionable (pp. 194–197).

With the contribution of Tiziana Leucci (“South Indian Temple Dancers: ‘Donated’ to the deity & ‘Donors’ for the Deity: Two Tamil Inscriptions on Music and Dance in the Rājarājeśvara Temple at Tañjavūr (11th century),” pp. 205–252) we turn to the use of epigraphical sources by anthropologists. Leucci’s paper is a passionate *disputatio* with Leslie C. Orr about the function of temple women in the Medieval period.²⁸ It focuses on two famous inscriptions engraved on the outer wall of the temple Rājarājacōḷa I had built at Tanjore,²⁹ which institute endowments to reciters of Tamil Bhakti hymns, temple women (*taliccēri penṭukaḷ*) usually identified as female dancers and various male servants such as musicians, drummers, etc. It is also a kind of methodological opposition between a qualitative (Leucci) and a quantitative (Orr) approach. Leucci’s method is multidisciplinary as she relies on various sources (literary

28 Leslie C. Orr, *Donors, Devotees and Daughters of God: Temple Women in Medieval Tamilnadu*, New York & Oxford: Oxford University Press, 2000.

29 *South Indian Inscriptions*, vol. II, Nos. 65–66.

texts, travelogues, ethnographic data), some of much later date—sometimes at the risk of anachronism—in an effort to fill up the “space of silence” (a concept borrowed from Franco Ruffini) inherent in epigraphical sources. Leucci’s conclusion is that the temple women mentioned at Tanjore are dancers and that, if one reads the inscriptional record between the lines, one will find many correspondences with what we know from later sources. One must concede that in any case, the Tanjore inscriptions are exceptional documents which are, as Orr put it, the “product of a spectacular royal gesture”;³⁰ they might in fact have represented, in their time, the exception rather than the rule. It is however perfectly legitimate, as Leucci does, to study them in depth and try to extricate all that is implied, providing rich historic and ethnographic information. Rather than make a generalisation from that case, I would prefer to consider it as an early testimony of a royal practice, which would later be replicated in other segments of the society, by patrons outside of royal circles. It would just be another instance of kingship providing a model, as is also the case, for instance, with built architecture or monumental epigraphy.

Zoé E. Headley (“Of Dangerous Guardians and Contested Hierarchies: An Ethnographic Reading of a South Indian Copper Plate,” pp. 253–281, with 4 illustrations and 1 table), also a (social) anthropologist, delivers a context-sensitive ethnographic reading of a copper plate kept in the custody of a tailor in a village of south-central Tamil Nadu and said to have been discovered “sometime back in a nearby field” (p. 253). The document is fascinating and concerns passed-away members of the present-day subcaste of the Pramallai Kallars (*piramalai kallār*). It is a contract or agreement (*oppantam*) for the construction of a new hall (*maṅṭapam*) in a local temple. It moreover provides a narration about shifting guardianship rights, murders, convictions, and suicides, and it also, importantly, establishes rights of precedence (*mutalmai*) at this temple. The author sketches the main features of the socio-historical background of the Pramallai Kallars, a former “criminal tribe” famous for having been studied by Louis Dumont. According to traditional narratives the Pramallai Kallars settled in the west of Maturai around the 16th or 17th century and exercised the occupation of guardianship (*kāval*) of fields or houses. Headley recalls the existence and importance of other copper plates, some of which were produced in law courts for assessing rights of the Pramallai Kallars (p. 259). She provides an edition and translation of the newly found copper plate. She then comments “on each sequence of the inscription through an ethnographic lens” (p. 264). This

30 See Leslie C. Orr, *op. cit.*, p. 34.

insightful commentary is based on the reactions and comments of the local people who read the inscription with the author and her research assistant, on knowledge about the Pramallai Kallars' oral history and social structure as analysed by anthropologists. Headley focusses on two issues: "the perennial struggle for precedence at the village level" and "the ambivalent representation of this subcaste's former traditional occupation" as guardians (p. 265). She finally discusses the authenticity of the document offering *pro* and *contra* arguments.

From the point of view of the epigraphist and the historian, an important issue is the date and authenticity of the copper plate. I would conclude from the details provided that it is not older than the 19th century. The remark (see endnote 15) about the notation of \bar{e} by Torsten Tschacher is very pertinent in this regard. The use of the *pulli* is also hint to a recent date. As such the artefact cannot be as old as other known plates dated to the 17th century (see pp. 257–259). It would have been useful to have a reproduction of the plate, to enable palaeographical analysis. The late date that I propose does not however mean that the copper plate is a forgery. It could well be an authentic grant of that period. It might even be a "silent" later copy of an earlier grant.³¹ The author presents arguments for its authenticity (pp. 273–274). Some bear much weight: the grant is not dated internally and presents a negative portrayal of the subcaste of the Pramallai Kallars. If Pramallai Kallars were the commissioners of the forgery, one would not expect such characteristics. As for the fact that the author was not able to find the names said to have been carved on the pillars of the temple, I would like to point out that the inscription only mentions that pillars were attributed (*vakuntiru-ttal*), which may mean that what was attributed is a place, more or less near to the god, during temple rituals. This would according to the villagers be marked by an inscription (p. 272). We know of earlier pillars inscribed with personal names, which have been interpreted as names of donors.³² What the Pramallai Kallars today say invites us to reconsider this understanding. Whatever the correct interpretation, this practice might in both cases reflect the same preoccupation: being as close as possible to the god.

Headley (p. 274) does not venture to "give a definite answer to the question of whether this copper plate is authentic or a forgery" but rightly concludes that "the inscription remains of great interest either way: either as a historical artefact worthy of study or as a contemporary reinvention of caste history." She

31 I.e. a benevolent forgery as per Salomon's typology (see below).

32 See Blandine Legrand, *Kīlāiyūr-Mēlappaḷuvūr, Epanouissement d'une dynastie princière en Inde, à l'époque Cōla*, Paris, éditions Recherche sur les Civilisations, 1987, p. 68 and photo 56.

concludes that “if this is a forgery it holds no historical interest” whereas, from the point of view of an anthropologist, “the interest of this copper plate would certainly be enhanced if it is indeed a forgery.” In this regard, Richard Salomon has recently proposed a typology of forgeries, distinguishing between ancient or traditional forgeries (deceiving, but well- or ill-intentioned) and modern forgeries (ill-intentioned).³³ The contribution of Headley is in itself interesting in that the plate has been read with Pramallai Kallars: whether their interpretation is historically authentic or not, it is worth knowing how contemporary Pramallai Kallars read it. Furthermore, recalling Richard Salomon’s opinion that “a modern forgery is by definition historically worthless,”³⁴ Headley nicely illustrates that its value might lie elsewhere, in the way it enables the anthropologist to determine how the forgers represent themselves.

The argument of Christiane Pilot-Raichoor (“Tamil-Brahmi Inscriptions: A Critical Landmark in the History of the Dravidian Languages,” pp. 285–315) is that the corpus of Tamil-Brahmī inscriptions—that is inscriptions written in Tamil language in a southern variety of the Brāhmī inscriptions and dated, for the earliest, to the first centuries BCE—calls for a revision of the theories concerning the characteristics of Proto-Dravidian and Prehistoric Tamil, as this corpus is testimony to a transient stage in the evolution of Tamil. Emphasizing certain features of the Tamil-Brahmī inscriptions (uninflected words; lack of categorial distinction, that is between noun and verb, for instance; isolation of morphemes) which would point to an earlier state of Dravidian, the author develops the theory already put forward by other scholars of a typological shift from an isolating, analytical stage to an agglutinative, synthetic stage.³⁵ She also advocates a double methodological approach to the issue: the study of language

33 Richard Salomon, “The Fine Art of Forgery in India,” in Gérard Colas & Gerdi Gerschheimer (eds.), *Écrire et transmettre en Inde classique*, 2009, defines forgeries as “legal and official documents which were drawn up or altered with an intention to deceive the reader, whether the intention was malevolent, as in the case of fake documents created to justify illegitimate claims to property, or benevolent, as with unsanctioned reproductions of lost copies of genuine documents” (p. 107, n. 1). In the latter case, “the intention to deceive ... lies only in the presentation of the replacement as it were the original, even though the legal claim which the forged replacement embodies may be perfectly legitimate” (p. 111). Salomon further distinguishes between traditional and modern forgeries, the latter “mostly produced for their value, whether financial, political, or professional, as antiquarian objects” (p. 113).

34 Salomon, *op. cit.*, p. 130.

35 It is surprising, in this respect, that Sanford Steever, *Analysis to Synthesis*, New York, Oxford University Press, 1993, is not referred to.

change and the linguistic reconstruction. She identifies three phases for Tamil: I. Protohistoric Tamil (characterised by inference), II. Old Tamil (i.e. Tamil-Brāhmī inscriptions, Caṅkam corpus), III. Modern Tamil. Then, after looking at theoretical models for other families of languages (developed notably because of the necessity of “combining the tree and areal models’ properties,” see p. 307), she concludes that the corpus of Tamil-Brāhmī inscriptions attests to a typological grammatical shift, from an isolating to agglutinative type of language, from polyfunctional uncategorized lexemes to the emergence of morphological paradigms for nouns and verbs. She argues that the development of Dravidian languages “cannot be simply explained by the linear splitting of an original mother-language into daughter-languages leading to the present day of sub-groups of languages” but that it is better explained in terms of “convergence of [a] previous dialect continuum” (p. 310). In other words, in the pre-Tamil-Brāhmī period in South India, a punctuating event (or events) triggered “the development of new convergent grammatical features which spread to the whole Dravidian domain.”

The argument is clearly expounded, but it seems necessary to state some important facts about the corpus of Tamil-Brāhmī inscriptions. Firstly, it is a very small corpus of generally very short inscriptions: 87 inscriptions (from 30 sites) dated from the 2nd century BCE to the 4th century CE according to Iravatham Mahadevan.³⁶ Note in this respect that Pilot-Raichoor favours the date of 3rd century BCE for the upper limit of Tamil-Brāhmī inscriptions. Secondly, this corpus is constituted by a specific type of inscriptions (foundation/ownership statements in relation to Jaina hermitages) with a recurrent sentence structure. Even though the corpus is not as homogenous as the corpus of later hero-stone inscriptions treated by Appasamy Murugaiyan in the present volume, this latter author is cautious enough to draw conclusions only for his corpus. Thirdly, one must also bear in mind that the Tamil-Brāhmī corpus is written language and actually represents the first attempt to write Tamil. That is to say, the writing system is experimental and does not necessarily reflect the structure of the language in accurate manner. Fourthly, even though Mahadevan’s book is a milestone in the study of Tamil-Brāhmī inscriptions, not all his readings and interpretations have been definitively accepted.³⁷

36 Iravatham Mahadevan, *Early Tamil Epigraphy from the Earliest Times to the Sixth Century A.D.*, Cambridge, Harvard University Press, 2003. See p. 97, table 2.1 with a century-wise distribution and a distinction between early and late Tamil-Brāhmī inscriptions, each group spanning three centuries.

37 See Pilot-Raichoor’s endnote 7, where she refers to the uncertainties in interpreting the

For instance, the argument in favour of the analytical or isolating character of Tamil-Brāhmī (pp. 289–290) based on the orthographic feature that “non-initial word-segments and grammatical suffixes commencing with vowels occur separately without sandhi or the glides *y* or *v* in between” (quoted from Mahadevan, 2003, op. cit., pp. 242–243) requires further investigation. Mahadevan lists other instances of “break between the consonant and the following vowel in the same syllable” (p. 244), for instance *atit^oanam* for *aṭṭānām* or *ar^oitan* for *aritan*,³⁸ and proposes explanations for such usages that are more or less convincing. It is however clear that in those cases this is not a question of morphology.³⁹ It is furthermore noticeable that examples of what seems morphological segmentation mirrored in the script are attested in later inscriptions. For instance at Cittaṇṇavācal, we read in a 9th-century inscription:⁴⁰ *aṇṇalvā^oil arivar ko^oin* [i.e. *kō^oin*] *muṇṇāḷ maṇṭakaṅ kallāley* [i.e. *kallālēy*] *nir^oik*, “after having erected with stone the ancient hall of the temple of Arivar at Aṇṇalvā(y) (i.e. modern = Aṇṇavācal).” In the words *aṇṇalvā^oil* and *ko^oin* (i.e. *kōyil* in *sandhi*), the element *il* (literally “place; house”) is still understood as a discrete morpheme: grammaticalized as a locative suffix in *aṇṇalvā^oil*⁴¹ or an independent lexeme in *ko^oin* = *kōyil*, “temple,” that is literally “house” (*il*) of the “king” (*kō*), meaning “palace” and by extension “temple.” One could argue that this is just an archaic spelling, but it is nonetheless intriguing. It seems conceivable that this way of segmenting words reflects consciousness of the morphology of the language, and is not an indication of a transitional

inscriptions and mentions several reviews of Mahadevan (2003) to which one might add the one by Herman Tieken (*Zeitschrift der Deutschen Morgenländischen Gesellschaft* 157, 2007, pp. 507–511).

38 Initial vowel signs are preceded by “^o”.

39 As pointed out to me by Arlo Griffiths (personal communication, November 2013), from Old Javanese, which is written in an Indic script, it would be easy to cite a great number of cases of initial vowel signs which were used inside words that cannot be clearly segmented into constituent morphemes, e.g. *vanu^oa*, “village,” in early inscriptions, later spelled *vanva*, or *la^our*, *laur*, *lor*, all meaning “north.” Consequently it cannot be taken for granted that difference of orthographic practice always reflects a real morphosyntactic difference. Nor can it be argued that morphemes, which later became suffixes, at an earlier stage still had the value of free-standing words, only because they are spelled with initial vowel signs.

40 *South Indian Inscriptions*, vol. XIV, No. 45, lines 8–9. There is, it seems, another example—*paṭai^ooṭu*—in the volume under review itself (p. 334), in an inscription dated to 600 C.E. See also the instances mentioned by Mahadevan (op. cit., p. 244 and n. 1).

41 Alternatively, *aṇṇalvā^oil* can be analysed as a place-name with unmarked locative: “[at] the place [which is] is Aṇṇalvā(y) / [at] Aṇṇalvāyil.”

phase of the morphosyntactic system of the language. In conclusion, I feel that Pilot-Raichoor's argument requires further investigation and elaboration as well as supportive data.

Appasamy Murugaiyan ("Hero-Stone Inscriptions in Tamil (450–650 CE): Text to Meaning: A Functional Perspective," pp. 315–351), starting from the observation that the correct interpretation of an inscription is dependent on extra-linguistic knowledge—i.e. context at large—sets himself to a deep analysis of a corpus of 38 inscriptions on hero-stones (*naṭukaḷ*, literally "planted stones") dated between 450 and 650 CE. The author analyses in detail the structure of these very short inscriptions which function as sort of labels to memorial stones erected for deceased warriors. He rightly identifies the structural patterns, which makes these inscriptions formulaic. The need for extra-linguistic knowledge is well demonstrated in the case of the mention, indeed very elusive sometimes, of regnal years in the preambles of inscriptions (pp. 325–326). Other grammatical elements of the records (noun phrases, verb forms) are analysed with many details and examples. The author observes the following specificities of his type of inscription: rarity of case marking; ambiguity about the grammatical status of elements such as *āḷ* (noun "chief" or verbal root "to rule") and verbal forms, typically *paṭṭān* (from *paṭu-tal* "to experience," i.e. "to die"), which could be analysed as past "indicative" (my terminology) or past participial noun. These conclusions are on the whole acceptable.

The author could have elaborated on *āḷ* in reference to Pilot-Raichoor's point about the precatégorical nature of words. He refers to this article when arguing that hero-stone inscriptions reflect a transitional period of verbal morphology in the case of *paṭṭān* (p. 339, n. 23).⁴² As for the status of *paṭṭān*, let's take an example: *kāvatiṭaṭukan toru iṭuvittup paṭṭān kal*. Murugaiyan translates (p. 339): "This is the memorial stone of Kāvatiṭaṭukāṇ, (the one) who was dead (while he) liberated cattle." A more literal translation would be: "(Of) Kāvatiṭaṭukan, he who died (*paṭṭān*, past participial noun, 3rd person singular masculine) having liberated cattle, the stone." But one could equally translate: "Kāvatiṭaṭukan, having liberated cattle, died (*paṭṭān*, past "indicative", 3rd person

42 Note that in Modern Tamil, such an ambiguity partly remains, that is in the 3rd person singular neuter, as *ceytatu* is either past "indicative" form ("it did") or past participial noun ("that which did"). However for the 3rd person singular masculine we have *ceytān* ("he did") versus *ceytavan* ("he who did"). See Thomas Lehmann, *A Grammar of Modern Tamil*, Pondicherry, Pondicherry Institute of Linguistics and Culture, 2nd ed., 1993, p. 78.

singular masculine). (This is his) stone.” The author favours the past “indicative” option in respect to examples such as *naṭuvitta kal* (p. 323), *paṭṭa kal* (p. 343), where *kal* is immediately preceded by a word syntactically dependent on it. The author’s argument (p. 339) holds good for *paṭṭān* in such cases, when it is followed by *kal*, that is it would be syntactically dependent on *kal*, a configuration possible only if *paṭṭān* is analysed as a participial noun. But even in the case when *paṭṭān* is not followed by *kal* (see examples, p. 341), Murugaiyan supports the participial noun option arguing that “the textual type of the inscription, which basically functions as an informative notice on the stone, as well as the regular relation of identification with the proper name of the hero, favours, even in this case, the participial noun interpretation” (p. 340). I still feel that the ambiguity somehow prevails in those very cases.

While the analyses and translations of inscriptions are on the whole convincing, some alternatives might occasionally be proposed. For ... *nīlakaṇṭaru paṭṭa kal*, Murugaiyan’s translation (p. 343: “... This is the memorial stone of Nīlakaṇṭar who died”) somehow obscures the fact that *paṭṭa* (relative participle) is dependent on *kal* and is the syntactic link between *nīlakaṇṭaru* and *kal*. I would prefer “... [this is] the stone related to Nīlakaṇṭaru having died / to the fact that Nīlakaṇṭaru died / to the death of Nīlakaṇṭaru,” or more literally “... related to the fact that Nīlakaṇṭaru died, the stone” or even “... [this is] the stone [placed] where Nīlakaṇṭaru died.” For ... *ēraṇ* [i.e. *eraṇ*, to be read as *ēraṇ*] *erintu paṭṭān*, which Murugaiyan (p. 344) translates as “Ēraṇ is the one who died being wounded ...”, I wonder about the meaning “to be wounded” attributed to *eri-tal* and I would translate as “Ēraṇ is the one who died having attacked” or “Ēraṇ died having attacked”. The specific meaning of *eri-tal* chosen by Murugaiyan might in fact be indicated in the context (see endnote 21), but the whole inscription is not quoted. That is to say, and to conclude, that I would have benefitted of integral quotations of the texts of the inscriptions, since these are in many cases edited in publications not easily accessible.