

A Guide Through Some Recent Sukhothai Historiography
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The present article was intended first of all as a review of *Mom Chao Chand Chirayu Rajani's Guide Through the Inscriptions of Sukhothai*,¹ but since much of *Guide* is based on highly original interpretations of inscriptions and chronicles, and since Prince Chand's interpretations had as their point of departure some rather severe criticism of the work of Mr. A. B. Griswold and Dr. Prasert na Nagara in their "Epigraphic and Historical Studies", the latter work must also be discussed.²

The controversial nature of Prince Chand's *Guide* would not be at all evident to the ordinary reader, nor even to an historian of Thailand who had not devoted more than casual attention to Sukhothai sources. In the entire work there is hardly any indication of the controversies or of the difficult nature of most of the sources; and Prince Chand's readings of the latter are presented as clear-cut history about which there should no longer be any serious argument.³

In reviewing *Mom Chao Chand's Guide* it is therefore essential to show how Prince Chand differs from G/P, which will involve detailed references to his own and their earlier writings, and will also include some opinion by the reviewer as to the quality of the arguments on both sides. On some points I prefer Prince Chand's solutions, on others Griswold's and Prasert's, while on still others I will suggest interpretations differing from anything hitherto proposed; and this will involve extended discussion of some of the sources themselves.

Because of the unavoidable length of this undertaking, quotations to illustrate the arguments will be kept at a minimum, but there will be thorough references to the relevant articles and source collections, and I must assume that the interested reader will have them at hand for consultation.

Some remarks on method

1. Hereafter cited as *Mom Chao Chand Guide*.

2. Abbreviated as *EHS* with number, and the joint authors will be cited hereafter as G/P. For full bibliographic data on all abbreviated references, see the appended bibliography. Prince Chand's earlier critical studies were three review articles in *JSS* which I shall cite as "Review 1972", "Review Jan 1973", and "Review July 1973".

3. On p. 12 of *Guide* the controversies are briefly noted, but since *Guide* contains not a single footnote nor any specific reference to the various 'chronicles' cited for support of Prince Chand's arguments, the reader will not find it easy to check or thread his way through the difficulties.

Prince Chand once suggested to me that since Sukhothai history was written by people who were not primarily historians, it might be useful to examine the writing on Sukhothai history from the point of view of the trained, practicing historian.

I agree that this is a useful starting point for a review of *Mom Chao Chand's Guide* and *EHS*, but a difficult problem which arises at the very beginning is to define what a historian is, or what kind of historian is to be the model against which to judge the writings in question.

This is not the place for an essay on history and the historian, but since I have accepted for myself the role of historian and intend to follow this suggestion of the principal author under review, it is proper that I set out my own views on what historians should do with the type of evidence available for the history of Sukhothai.

Most simply, of course, a historian is anyone who studies the past in a methodical manner, and since anything that occurred before the lifetime of living persons is not directly knowable, the historian studying earlier times depends on documents, which may be written records, more or less faithful to events, or non-written remains, such as buildings, works of art, or anything else resulting from human activity.⁴

It should be obvious to the reader without adducing detailed evidence that the farther back one searches into the past, the less numerous are the documents and the more tendentious those which have been preserved. There is also near mathematical certainty that when old literature was recopied by hand in the days before printing became common, errors accumulated with each generation of copying; and this complicates the task of studying Southeast Asian chronicles.⁵ Thus for the historian of early times, such as the Sukhothai period, doing history means first of all the discovery of documents and their interpretation, and after that the construction of the most plausible synthesis of the information derived from the documents.

Both the interpretation of the documents and the final synthesis must conform to general rules of scientific method concerning formation of hypotheses, search for evidence, confirmation or negation of hypotheses, and so on, and in the study of the past it is the adherence to such rules of evidence which distinguishes history from historical fiction.

Dr. Prasert, who in his own writings has shown more sensitivity to questions of method than either Griswold or Prince Chand, at one time remarked that Thai historians should not blindly copy the formulations of

⁴ Of course, strictly speaking, non-written remains belong to the discipline of pre-history/archaeology, not history proper, but historians legitimately make use of the interpretations derived from non-written records by specialists in those disciplines.

⁵ See A. C. Clark, *The Descent of Manuscripts*, Oxford, 1918.

their predecessors and fit new evidence into them, but should search for new inferences to link new pieces of evidence.⁶ Dr. Prasert was probably thinking of the acceptance of everything written by Prince Damrong and George Coedès as ultimate truth, and in that context his remark was useful, but the distinction between a historian and a writer of historical fiction is drawn along a narrow line which separates legitimate and illegitimate inferences connecting the various pieces of evidence.

The area of history in which the most attention has been given to method and rules of evidence is archeology, and this is fortunate for our purposes since the main evidence for Sukhothai history consists of inscriptions, which are also archeological artifacts just as much as the buildings which sometimes accompany them or the potsherds which may be found near them. Summarized as simply as possible, the rules of archeological evidence are that the place of each artifact (record) in its temporal and spatial context should be determined as closely as possible. Then each piece must be examined (read) individually for whatever information it can reveal about itself. After that, pieces which appear to be closely related are fitted into sequences or patterns by means of objective techniques such as stratigraphy and seriation,⁷ and this process of fitting together ever more pieces is carried out both spatially and temporally until boundaries are reached beyond which the material seems to be unrelated. Only as a final step is the information obtained directly from the original material itself compared with other types of evidence, such as ancient literature, purporting to deal with the same time and place.

With respect to the Sukhothai inscriptions, these rules of evidence mean that each inscription must be read as literally as possible without regard to what we imagine we know from some other source. Words which have a common, well-known, meaning must not be arbitrarily given some unusual meaning without a full argument of justification including recorded contexts showing the unusual usage; and lacunae must not be filled in, either in the original text or translation, without showing that the restoration involves precisely the number of characters missing from the stone and fits the metre if the composition is in verse. When this has been done for all inscriptions we then see whether they all fall together into a consistent story or pattern. The technique of seriation may play a part here. For example, No. 9⁸ placed in series with earlier and later inscriptions proves that the king

6. Prasert ṅa Nagara, "Researches in Thai history", p. 33.

7. James Deetz, *Invitation to Archaeology*, The Natural History Press, N.Y., 1971, chap. II, for an introductory description.

⁸. The inscriptions of the Thai corpus will be designated as 'No.' plus Arabic numeral. Nos. 1-15 are in George Coedès, *Recueil des inscriptions du Siam I, Inscriptions de Sukhodaya*. Bangkok: Bibliothèque Nationale, Service Archeologique, 1924. ; nos. 30-84

known as Sai Līdaiy was grandson of Līdaiy; and a seriated comparison of vowel or tone marks may indicate the date of an inscription which lacks chronological indications.

If the several inscriptions do fit a pattern or immediately provide a consistent story, we may presume our translations and preliminary interpretations to be correct, and we may proceed to write history. Most frequently there will appear to be contradictions among some of the sources, and we must **always** assume this to mean that we have not understood them properly. We must not assume that the author did not know what he was doing.⁹

Some of the apparent contradictions will be due to lack of information about people, events, and whole time periods not mentioned in the inscriptions, and if we feel the best translation of the extant documents has been achieved we may try to link the epigraphic evidence together by interpretation and inference. This is where we must beware of slipping over the boundary into fiction. Construction of a plausible story is not sufficient to write history and if the gaps to be filled by inference are too large or susceptible to too many different inferences, then it may be better to leave them unfilled, as I have suggested below with regard to Nos. 11 and 40.

Important rules for inferential reconstruction concern fidelity to the evidence, consistency, systematic use of evidence, and economy of explanation. All inferences must be somehow embedded in the evidence.

That is, when an inscription mentions a vague *pū brañā* it **may** (note the emphasis) be legitimate to identify him with a real person known from other material; but it is not legitimate to link two sets of information by postulating the existence of a person who has no existence in any other sources, or to infer marriage or blood ties not implied by any source, or to justify an interpretation on the basis of what some fourteenth-fifteenth century individual might have thought or felt. When all the sources have been exhaustively studied they may provide some possibility of understanding fourteenth-fifteenth century thought, but assumptions about the latter will do more harm than good in the interpretation of the former.¹⁰

With respect to consistency, we must not say that a certain phrase or title ‘means’ one thing in one context and something else in another without a full justificatory argument; and inferences which may be systematically

are in *Śilā cāru’k III*; and nos. 85-130 plus a fragment of no. 40 are in *Śilā cāru’k IV*⁹. Mom Chao Chand, *Guide*, p. 20.

10. It should already be clear that I reject the ideas associated with R.G. Collingwood’s *The Idea of History*, p. 228, about all history being the “re-enactment of past thought in the historian’s mind”, or that the historian’s “proper task (is) penetrating to the thought of the agents whose acts they are studying”. What the Sukhothai sources still require is the ‘positivistic’ type of analysis against which Collingwood so strongly argued.

applied to more than one situation are better than those which fit only one. Finally, in our inferential reconstructions we must not unnecessarily multiply assumptions. In principle the simplest reconstruction will always be the best, and although plausible stories may be constructed by means of elaborate assumptions, or ‘epicycles’, this process may easily result in, and in any case cannot be distinguished from, historical fiction.¹¹

The first general criticism I would make of all the work under review here is that it is often methodologically unsound, generally proceeding from interpretation to fact, rather than the other way around. In nearly every one of their *EHS*, G/P start off with a historical background picture based on a synthesis, sometimes rather arbitrary, of the various chronicles, and then they fit - sometimes really force - the inscriptions into it. Prince Chand often criticizes their reconstructions on matters of detail, but apparently does not object to their method, and himself seems to favour the epicyclical method of reconstructing situations to fill the gaps in the inscriptional information.

In the present review I intend to concentrate on the inscriptions. Space will not permit me to analyze fully the use which has been made of the chronicles. Thus I shall not attempt to prove that G/P are always wrong in the background pictures they draw from the chronicles; in fact, they may sometimes be right. Right or wrong it is the opposite of proper method, and what I intend to show is that even the potentially accurate background pictures may not be related to the inscriptions in the way G/P believe, and that where arbitrary or inconsistent interpretations of inscriptional details are involved the scenarios are almost certain to be wrong.

Some preliminary discussion

Prince Chand’s *Guide* follows a chronological plan with nine chapters, most of which cover, according to their titles, specific periods, from

11. Even within the realm of pure science Kenneth Boulding, *History and Theory* VII, 1 (1968), p. 89, said “one has the uneasy thought that if the computer had been around then, we could have handled the Ptolemaic system quite easily with computerized epicycles, and the great Copernican simplification might never have happened...”. The best concise statement I have seen on scientific method and history is in Gordon Leff, *History and Social Theory*, Anchor Books, N.Y., 1971, pp. 109-110: “[Although] the incompleteness of knowledge accessible to the historian Restricts his conclusions to being at most inferentially probable (he) is subject to the same canons of correct reasoning and technical competence which apply to all intellectual disciplines. If he omits or distorts or makes a faulty implication his failure will be just as palpable as similar shortcomings in the exact sciences.... The historian is as accountable to his evidence and the correct way of reasoning from it as the practitioners of any body of knowledge: it is that obligation which makes history a branch of knowledge and not of the creative arts”.

‘Background to the Sukhothai story’ (chap. 1) to ‘Reign of Ramatipati II [of Ayutthaya] and after’ (chap. 9). In my review I shall also follow the chronological scheme, comparing Prince Chand’s work with the relevant *EHS* of G/P, and discussing the sources themselves where necessary.

Before proceeding directly with discussion of the chronological periods, it is necessary to devote some attention to a few matters of general interest to several parts both of Prince Chand’s and Griswold’s and Prasert’s writings.

(a) *The historical character of Sukhothai inscriptions*

G/P, and Coedès before them, concluded that each inscription was erected to commemorate a specific event or act. This is an opinion which I share, but about which Prince Chand seems to entertain some doubt,¹² and it also seems to be true of Angkorean epigraphy, which undoubtedly influenced Coedès in his work on Thai inscriptions. In Angkorean inscriptions it is a general rule that the date of the event commemorated, and the ‘official’ date of erection of the inscription, is contained in the opening lines of the text.¹³ In Griswold’s and Prasert’s work on Sukhothai inscriptions, there seems to be a tacit assumption that such was the norm in Sukhothai, although they naturally do not ignore the cases in which a series of dated events is clearly indicated.

It is interesting, though, that of the 28 inscriptions discussed by G/P in their *EHS* on Sukhothai history, only 10 are unequivocally texts referring to a single event at a specified date.¹⁴ In addition, Nos. 40 and 64 were probably single-event texts, but they are fragmented and contain no date; No. 10 is too fragmentary to be judged; and the very time period of No. 11 is a matter of controversy.¹⁵ On the other hand, 12 inscriptions are clearly historical¹⁶: that is, the opening date, or first date found in the text, is not the date at which the inscription was erected, but only marks the first of two or more events recorded. Sometimes the events are rather far apart, as in No. 9, and at other times they are close together, as in No. 62, where the two specified dates, only a year or so apart, are in the middle of the text after a

12. George Coedès “Notes critiques sur l’inscription de Rama Khamheng”, p. 21; G/P *EHS* 9, p. 189; Chand, “Review 1972”, p. 264.

13. There are exceptions. For example, the famous inscription of Sdok Kak Thom in which the date is near the end. See G. Coedès and P. Dupont, “Les stèles de Sdok Kak Thom, Phnom Sandak et Prah Vihar”.

14. Nos. 3, 6, 7, 12, 13, 14, 38, 45, 94, and the inscription from Nan reproduced in *EHS* 3, p. 105.

15. See discussion below.

16. Nos. 1, 2, 4, 5, 8, 9, 46, 49, 102, 62, and the gold plate reproduced in *EHS* 10, p. 148.

historical narrative leading up to them.¹⁷ Even a very short inscription may have this character. In the four-line, gold plate text reproduced in *EHS* 10, pages 147-48, there is a very specific opening date at which time a *vihāra* was finished. Then there is mention of another event four months later; and finally it is stated that an eighteen-cubit Buddha image was made - something which could well have taken several months. The true date of the inscription may therefore be over a year later than the date recorded in it.

This historical-narrative character of over half the Sukhothai inscriptions is interesting in its own right, but I wish to emphasize it because I shall seek to prove that two others, No. 93 (Asokārām) and No. 106 (Vat Traḃaṅ Jān Phōak), are also of that type and that their dating has been misunderstood by G/P. The argument will be more convincing if it is clear that the structure I impute to Nos. 93 and 106 was common throughout Sukhothai epigraphy.

(b) *Two confusing inscriptions, Nos. 11 and 40*

Inscription No. 11 has provoked extreme disagreement among scholars as to its date, and this has naturally led to very divergent interpretations of its meaning. Coedès believed the mention of “Braḥ Rāma, his younger brother”, proved that the inscription was the work of Mahādharmarāja IV who together with his younger brother Baṅṅ Rām is mentioned in the *Hlvaṅ Prasro ’th Chronicle (LP)* in 1419, and that face I of the inscription was later than that date.¹⁸ Coedès considered face II to be even later.

In *Towards* Griswold said face I was from the time of Līdaiy, and he based his argument on its style - “direct, orderly and vivid” - a bit too subjective, it would seem to me. He considered that the “beloved younger brother” was Rāmādhīpat I of Ayutthaya.¹⁹

In Prasert’s earlier work on this inscription he suggested that face I was from the time of Līdaiy, around 1359-61, and that face II was later, due to the mention of Braḥ Mahāthera Śrīśraddhārājacūlāmūṅī, and was in content similar to No. 2. The last point is important for the dating of No. 2 as well. Prasert’s reasoning with respect to face I was based on systematic paleography, a much more solid foundation than ‘style’; No. 11 does not contain the mark ไม้หันอากาศ/*mai han ākāṭ*, which was first used by Līdaiy around 1361, and it must therefore be earlier than that date. As for face II,

¹⁷ No.9 is in *EHS* 12 and No. 62, From Lamphun, but with references to Sukhothai, is in *EHS* 13.

¹⁸ Coedès, *Recueil*, pp. 145-49: *LP* at date *cula* 781/A.D. 1419.

¹⁹ A.B.Griswold, *Towards a History of Sukhodaya Art*. Bangkok: The Fine Arts Department, 1967., pp. 38-39.

his argument was based on its contents, and he considered ‘Brah Rāma’ to be Bañā Gāṃhaeñ Brah Rām, mentioned in No. 2.²⁰

Following this Prince Chand published a critique of these ideas in which he apparently argued that the Mahādharmarāja of No. 11 was Lo’daiy who was offering merit to his father Braḥyā Rām, that is Rām Gāṃhaeñ (RK),²¹ which as Prasert rightly answered does not square with the explicit statement of the inscription that Rām was ‘younger brother’.²² Prasert’s second argument, that Mahādharmarāja was the personal name of Līdaiy, is weaker in view of the fact that it was also given to the three succeeding kings of Sukhothai, and could well have been used by Lo’daiy, too.²³ Returning to face II, Prasert here added paleographical evidence for his choice of date, that it mixes *mai han ākāt* with doubled final consonants, a feature found in other inscriptions between 1361 and 1392.

In *Guide* Prince Chand has apparently accepted Prasert’s paleographic argument and agrees that both sides were written, one after the other in Līdaiy’s reign, but both by Śrīśraddhā. He still insists, however, that ‘Brah Rāma’ refers to RK, younger brother of Bān Mo’añ, but it is unclear why this attribute of RK should have been emphasized, and I find Prince Chand’s proposal unconvincing.²⁴

The most recent discussions of No. 11 are in *EHS* 10 (face II) and *EHS* 11-1 (face I), and they do little to clear up the controversies because they are at times mutually contradictory.²⁵ It is stated in both that Līdaiy was author of face I and Śrīśraddhā of face II. But in *EHS* 10, G/P express uncertainty about the relative dates of the two sides, and suggest that face II was written in the 1350s, which contradicts all of Prasert’s earlier paleographic arguments. In *EHS* 11-1, on the other hand, they return to the theory that face II is later and was written sometime in the 1360s, and they remain agnostic on the identity of the younger brother, Brah Rāma.

My own view is that face I is simply too fragmentary even to permit determination of its author, and that there is no way to determine the identity of the Brañā Mahādharmarāja who is mentioned, although I find Prasert’s paleographic arguments the most convincing for the rough dating of the inscription itself. As for the identify of ‘Brah Rāma’, all the proposals to

20. Prasert, *op. Cit.*, pp. 48, 50. The name ‘Śrīśraddhārājaculāmūni’ will be abbreviated henceforth as ‘Śrīśraddhā’.

21. Prince Chand’s article was in the journal *Sam Thahan* (สามทาน), February 2511 B.E. (1968 A.D.). I have not seen it, and am relying on the information in Prasert’s answer.

The name ‘Rām Gāṃhaeñ/Khāmhaeñ will be abbreviated ‘RK’.

22. Prasert, *op. Cit.*, p. 55.

23. Mahādharmarāja II, III, IV.

24. *Guide*, pp. 19, 95-98.

25. *EHS* 10, pp. 135-44; *EHS* 11-1, pp. 112-118.

date seem implausible, and I would say that speculation about it on the basis of extant evidence is useless. Concerning face II, its content certainly does resemble part of No. 2, and the opinions of G/P and Prince Chand that it refers to Śīśraddhā are acceptable, although the assertion that he was author is not certain.

I would say in conclusion that all historical reconstructions based on No. 11 must be set aside pending further study.²⁶

Inscription No. 40 was the subject of *EHS* 5, which has been criticized by Prince Chand both in a review article and in *Guide*.²⁷ This inscription, whose year date has disappeared, is apparently some kind of pact between two princes who are described as uncle and nephew, the former ruler of Sukhothai, and the latter given the title *samtec cau brañā*. G/P assume that ‘*samtec cau brañā*’ means he was king of Ayutthaya, although their reasons for this assumption are not clear; they further assume that ‘uncle’ and ‘nephew’ are to be taken literally,²⁸ and then, on the basis of extrapolation from Wolters’ bi-polar theory,²⁹ they erect a story about Sukhothai-Ayutthaya relations which, with all due respect, I find quite unconvincing. As for the identification of the ‘uncle’ and ‘nephew’ they offer three possibilities, all of which imply a date between 1369 and sometime after 1400.³⁰

Prince Chand, on the other hand, considers that the inscription dates from 1438, and that the uncle and nephew were respectively the ruler of Chaliang - according to him the Bañā Rām of *LP*’s entry for 781/1419 - and Prince Rāmeśvara, later King Trailokanāth.³¹ Prince Chand’s story involves just the sort of filling in which historians should avoid - in particular there is no evidence that the Bañā Rām of *LP* became Bañā Chaliang - but of most importance for critical scholars of Sukhothai history, it is based on a misconception which has long gone undetected, the idea that King Trailokanāth’s mother, queen of Paramarāja II, was a Sukhothai princess. According to Prince Chand, when King Indarāja of Ayutthaya imposed his will on the north in 1419 he “very likely” married his son, Sām Brañā, to a daughter of Sai Līdaiy, and thus Rāmeśvara/Trailokanāth would have been nephew of Sai Līdaiy’s son Bañā Rām.³²

26. In addition to the above, see G/P, “On kingship”, p. 62.

27. Chand, “Review January 1973”, pp. 289-92; *Guide*, p. 79.

28. This is noteworthy since in *EHS* 4, p. 129, n. 15; *EHS* 1, p. 218; and *Towards*, p. 39, they emphasize that such kinship terms **should not** be taken literally.

29. See discussion below and compare with their remarks in *EHS* 5, pp. 94-95.

30. *EHS* 5, pp. 98-102.

31. Chand, “Review January 1973”, pp. 289-92.

32. *Ibid.*, p. 289. [*Of course, the very conception of King Indarāja of Ayutthaya imposing his will on the north in 1419 is now out of favor. Chris Baker, “Ayutthaya Rising: From the Land or From the Sea”, *Journal of Southeast Asian Studies* 34/1 (2003), pp. 41-62.*]

Charnvit Kasetsiri has also repeated this story as though it were common knowledge, saying “a Sukhothai princess was given to the city’s [Chainat] ruler, Chao Samphraya”, and “one result was the future King Trailok”. Charnvit also adds, “Trailok...claim[ed] that as a lineal descendant of the Sukhothai family through his mother...”³³

This story is also found in some other works of Thai history, but I had considerable difficulty tracing any other origins for it until I happened on a reference in Prince Chula Chakrabongse’s *Lords of Life* where Prince Damrong’s *Nidān Porāṇagati* is given as the source.³⁴

In the latter work, first printed in 1944, Prince Damrong cited *Rājādhirāj*, the Mon history, which says Dhammaceti of Haṅśāvātī received from Trailokanāth a gold plate inscribed with the title *braḥ mahādharrmarāja*, which had been the name of Trailok’s paternal grandfather. In reading this passage Prince Damrong “newly found out that King Param Trailokanāth was the grandson of Braḥ Mahādharrmarāja of Sukhothai”.³⁵ He then added that Indarāja had asked for a daughter of Mahādharrmarāja IV (sic) for his son Sām Bañā, and she was the mother of Trailokanāth.

A check of *Rājādhirāj*, however, reveals quite a different situation. There it only says that the king of Ayutthaya, plausibly Trailokanāth, sent to the king of Haṅśāvātī a gold plate inscribed with a new title, *braḥ mahā rājādhirati* (note the difference), saying, “this royal name was the royal designation (สํัญญา) of our paternal grandfather who ruled Kruṅ Śrī Ayudhayā previously”.³⁶ Thus there is no reference to Sukhothai antecedents for Trailokanāth, and all reconstructions based on such an idea are to be rejected. Furthermore the passage from *Rājādhirāj* is in a section which seeks to show

33. Charnvit Kasetsiri, *The Rise of Ayudhya, A History of Siam in the Fourteenth and Fifteenth Centuries*, pp. 131-3. Several things need to be said about Charnvit’s statements, but they are not directly relevant to the present subject and I will save them for a forthcoming review of Charnvit’s book. [* 2012 "A New *Tāṃnān* About Ayudhyā", in this volume, pp. 00-00*] It is only necessary to mention here that no extant source shows Trailok ‘claiming’ anything, least of all about his ancestry. Charnvit cites only an obscure passage in *Yuon bāy/Yuan Phai* (see See Chanthit Krasaisen, *YP*, p. 41), which does not necessarily have to be interpreted, it seems to me, as Charnvit, and others, have done. The key word, *dvītiya* (correct *dvitīya*), both in Sanskrit and standard Thai, is glossed as ‘second’, suggesting that the author of *YP* wished to say that Trailok was second son, not that he was born of two royal dynasties. [*See Prince Damrong’s commentary to *RA*, pp. p. 256, 259, and coment below, p. 00*]

34. H.R.H. Prince Chula Chakrabongse, *Lords of Life*, p. 31, n. 30.

35. Damrong Rajanubhab, *Nidān Porāṇagati*, pp. 238-39, *Nidān* 19, sections 2-3. Prince Damrong apparently paid no attention to the passage of *YP*, perhaps like G/P, not considering it good evidence.

36. *Rājādhirāj*, Bangkok, 2513 (1970), p. 697.

that all the titles attributed to Dhammaceti had been granted by neighbouring rulers and its purpose is to explain details of Hañśāvafī history, not of its neighbours. There is nevertheless some evidence that Trailokanāth's Ayuttayan grandfather, Indarāja, may really have had the title *rājādhipatī* which reinforces the Ayutthayan, not Sukhothai, allusion expressed in the passage.³⁷

So far as I can determine, G/P nowhere allude to any Sukhothai ancestry in Trailok's family, not even in their study of *Yuan Phai*, nor in Griswold's "Yudhiṣṭhira, where the subject should naturally arise."³⁸ This can only be termed an avoidance of the issue, which must mean that G/P reject, correctly, such a story, but for some reason do not wish to reject it explicitly.

[*Prince Damrong also, in his commentary to Trailokanāth's reign in *RA*, said nothing about his mother's origin in Sukhothai, but included an interesting reconstruction, based on use of the Cambodian chronicles, according to which Trailokanāth was second son of his father, an elder brother being Indarāja, who was assigned to rule in Angkor after it was conquered by Ayutthaya in 1431. This may help to elucidate the statement in *Yuan Phai*, above note 31, concerning the term *dvītiya*, but the Cambodian chronicles are hardly reliable evidence for such conclusions. There are thus two different opinions by Prince Damrong, in his commentary to *RA*, written between 1912 and 1914, and in *Nidān porāṇagatī*, first published in 1944.³⁹ Both may depend on misapprehensions about sources of dubious value, and neither is strong support for Trailokanāth's supposed maternal Sukhothai ancestry. [See further discussion of this in note on Yuan Pai]

Although the reconstruction of Prince Chand must be rejected, there is some evidence favouring a relatively late date for No. 40. That is the 𑄓𑄚𑄚𑄚/*mai tho* mark which "appears several times....unusual among the Sukhodayan inscriptions".⁴⁰ Well, it is unusual for the period in which G/P wish to place No. 40, but appears later. In his own studies Prasert first

37. See Vickery, review of van Vliet/vV, Jeremias van Vliet, *The Short History of the Kings of Siam*, p. 22. [*There are two inscriptions recording the royal title *rājādhipatī*, No. 49, dated 1418, which, although found in Sukhothai, G/P have interpreted as belonging to Intharacha (vV's Nakhon In), and a gold plate found in Suphanburi and dated 1?5?, reasonably restored as *śaka* 1357/1435 AD. Thus the title *rājādhipatī* would have been used by both Trailokanāth's grandfather and father. Even if the title *rājādhipatī* was also used in Sukhothai, as I have argued, the evidence of the *Rājādhiarāj* chronicle is that the Ayutthayan king was alluding to Ayutthayan antecedents.*]

38. G/P, "*Yuan phai*"; and Griswold, "Yudhiṣṭhira". [*In fact, *Yuan Phai* seems to support the inferences about Trailokanāth's Sukhothai mother, and avoidance of the matter by G/P suggests, intriguingly, that they had some doubts about that work. So do I, see above, note 33, but discussion must await a later occasion. *]

³⁹ Prince Damrong, in *RA*, pp. 256, 259

40. *EHS* 5, p. 90.

considered No. 40 to represent a treaty between Rāmeśvara I and Lī daiy, but in a footnote stated that the use of modern *mai tho* means it must be placed somewhere between the reigns of Mahādharrmarāja II and Trailokanāth;⁴¹ and in another context he considered that precise identification of the ‘uncle’ and ‘nephew’ was not possible, but that No. 40 was at least proof of family ties between the dynasties of Sukhothai and Ayutthaya.⁴²

This brings us back to the question of *samtec cau brañā*, which Prasert considered to be proof that the ‘nephew’ was Ayutthayan. His identification of the title as Ayutthayan was based on Chinese sources, and indeed the Chinese seem to have used *samtec cau brañā* for certain rulers of early Ayutthaya.⁴³ Ayutthaya, though, was not the only polity to use this title, and G/P should have seen that there was a much more plausible source within the context of their corpus of inscriptions. In their *EHS* 4, page 105, they have reproduced a short inscription from Nan in which the ruler of that polity is entitled *samtec cau brañā*. The date is 1426, within the period suggested both by Prince Chand and by paleographic considerations for No. 40, and the long history of close relations between Nan and Sukhothai adds to the plausibility of such an interpretation.⁴⁴ With this in mind one might even suggest that the “[oath] to the grandfather Brañā”⁴⁵ referred to the earlier Nan-Sukhothai treaty recorded in No. 45 (see below).

There is, however, a further difficulty. The text of No. 40 ends with “... *rājassa yasodharādhipassa*, etc.”, which G/P realize might mean ‘King of Yasodhara’.⁴⁶ They suggest that Yasodhara, part of the classical name for Angkor (*yaśodharapura*), would here mean Ayutthaya, a suggestion which I find attractive at that date,⁴⁷ but the phrase is too fragmentary to permit interpretation.

We must conclude, I am afraid, that No. 40 is simply too fragmentary to permit identification of its date and authors or interpretation of its meaning, that it therefore cannot be used in syntheses of Sukhothai history, and that all such syntheses so far proposed by G/P and Prince Chand are not

41. Prasert, *op. cit.*, p. 48, dated 1967. The footnote apparently dates from 1971, the date of latest publication. I have found no statement about the **first** use of modern *mai tho*.

42. Prasert, *op. Cit.*, p. 58.

43. Prasert, *op. Cit.*, p. 55; T. Grimm, “Thailand in the light of official Chinese historiography”; and see Vickery, “Cambodia after Angkor”, pp. 271-72.

44. See *EHS* 3.

45. *EHS* 5, p. 112, lines 14-16.

46. *EHS* 5, p. 113, n. 56.

47. I believe it would have been connected with strong Angkorean influences in early Ayutthaya, but discussion of this would go far beyond the present subject. [*And if Yaśodhara could then refer to Ayutthaya, it could imply that the reference to Yaśodhara in Inscription II means the region of later Ayutthaya, not Angkor. *]

sustained by the evidence.⁴⁸

(c) *The bi-polar theory of early Ayuttayan policy*

In several of their studies G/P have made use of a bi-polar theory of Ayutthayan policy, first enunciated by O.W. Wolters, as a basis for their conjectural reconstructions of the historical background to the inscriptions.⁴⁹ According to this theory Rāmādhipatī I of Ayutthaya and his son Rāmeśvara, continuing the policy of the earlier kings of Lavo from whom they possibly descended, gave precedence to warfare against Angkor, while the competing, and ultimately victorious, Suphanburi line of Ayutthayan royalty preferred a policy of expansion against Sukhothai. Wolters based this interpretation on the apparently contradictory chronicle traditions of *LP* and the long Ayutthayan chronicles,⁵⁰ the former of which records only one early invasion of Cambodia in 1431 while the latter have three - in about 1352, between 1384-86, and in 1421. Wolters further ‘refined’ these dates, on the basis of the Cambodian *Ang Eng Fragment*, to 1369 and 1389, the first at the very end of the reign of Rāmādhipatī and the second in the reign of Rāmeśvara.

G/P’s use of this reconstruction is rather strange, since throughout their work they emphatically rely on *LP* alone for early Ayutthayan events, while Wolters’ theory depends on modified acceptance of the long versions’ scenario. Moreover, G/P accept *LP*’s 1431, in the reign of Paramarāja II of the Suphanburi line, for the sole conquest of Angkor by Ayutthaya; and they also consider *Jinakālamālī*’s invasion of Kamboja in the 1360s, which Wolters equated with his first war against Cambodia, to refer only to events in central Siam.⁵¹

Thus what G/P have really done is to rely on Wolters’ bi-polar theory when it suits their purposes for conjectural reconstruction but to reject it implicitly when it would be unfavourable to their argument. This is one of the types of inconsistency which is contrary to good historical method. If a hypothesis such as Wolters’ does not fit all relevant cases, it means either that the hypothesis is to be rejected or that the cases in some other respect have not been properly understood.

It can now be stated with virtual certainty that Wolters’ bi-polar theory,

48. See also *Guide*, pp. 79, 81.

49. O.W. Wolters, “A Western teacher and the history of early Ayudhya”, pp. 96-97: and O.W. Wolters, “The Khmer king at Basan (1371-3) and the restoration of the Cambodian chronology during the fourteenth and fifteenth centuries”, pp. 83-84. The theory is explicitly noted by G/P in *EHS* 10, pp. 26-32, 28, n. 11, 38 and elsewhere.

50. For the latter Wolters referred to the ‘Paramanujit’ recension, or the version of Samtec Braḥ Banarat. Insofar as the present subject is concerned, this version is also represented by the ‘Royal Autograph Chronicle’ (*RA*), the so-called ‘British Museum’ version, and *Bāncādanumās*.

51. See G/P, “*Yuan phai*”, p. 130; *EHS* 11-2, pp. 107-08. For the invasion of 1384-86, see Vickery, “Cambodia after Angkor”, pp. 384-93.

and all of G/P's interpretations based on it, must be rejected. In a recently completed dissertation I have demonstrated (1) that Wolters misunderstood the *Ang Eng Fragment*, and that invasions in 1369 and 1389 cannot be reconstructed either from it or from the Chinese records Wolters used in support; (2) the chronology of the long Ayutthayan chronicles as a whole derives from that of *LP* and thus only the latter may be used in further historical study; and (3) the first two invasions of Cambodia in the long versions, in about 1352 and the 1380s, are entirely misplaced.⁵² Thus the *LP* entry of 1431 is the only direct statement of Ayutthayan attacks on Cambodia in the fourteenth-fifteenth centuries, although, as I have shown elsewhere, the *LP* entry of 771/1409 may disguise an earlier Ayutthayan intervention in Cambodia at that date, but not one which would support the bi-polar theory as it now stands.⁵³ As for the other pole of the theory, warfare against Sukhothai, there is ample evidence for it in *LP*, but the only possible interpretation is that expansion northward was a constant feature of Ayutthayan policy from the fourteenth century - a policy which was maintained until King Chulalongkorn unified all of present-day Thailand at the end of the nineteenth century.

Before concluding this matter, I should like to discuss briefly one of the contexts in which G/P made use of the bi-polar theory, their *EHS* 5. They start out by saying, "Īdaiya was on very friendly terms with Rāmādhīpatī I", which must be an extrapolation from the bi-polar theory since nowhere, either in inscriptions or chronicles, is there information about the feelings of these two rulers. "Rāmādhīpatī, whose grand design was to conquer Cambodia", is strictly bi-polar theory, and a strange statement for G/P, all of whose work implicitly rejects the chronicle entries on which it is based. Equally bi-polar is "Rāmeśvara was no less amicably disposed toward Sukhodaya", and it is equally without foundation in any other evidence. Of course, the attacks against Sukhothai which followed under Paramarāja I are explicit in *LP*, but outside of the bi-polar hypothesis there is no evidence that they were in contrast to another policy favoured by Rāmādhīpatī. Whether "Rāmeśvara quickly reverted to (a) policy of friendship with Sukhodaya" is quite unknown, since there is no information at all about his reign from his coronation in 1388 to his death in 1395.⁵⁴ Elsewhere G/P propose that the "reticence" of the chronicles for this period suggests that "Ayudhya was on the verge of civil war", presumably because of the bi-polar tensions,⁵⁵ but gaps of seven years or more occur all through the fourteenth century (for example 1351-1369, 1378-1386, and 1395-1409),

52. Vickery, "Cambodia after Angkor", pp. 167-200, chapter IX, and pp. 369-93.

53. Vickery, "'The 2/k.125 Fragment, a Lost Chronicle of Ayutthaya", pp. 55-61.

54. *EHS* 5, pp. 94-95; *LP*, pp. 132-33.

55. *EHS* 1, p. 213.

and I would say they imply nothing more than a lack of early records when the original *LP* was composed in 1680-81.

As I said earlier, space forbids full analysis of the totality of G/P's background reconstructions, and some of them may turn out to be accurate, but all of those based on the bi-polar theory, and extrapolations from it about ideas of rulers in Sukhothai or Ayutthaya, must certainly be rejected.⁵⁶

Sukhothai 'protohistory'

Discussion of *Mom Chao Chand's Guide* and *EHS* should logically begin with what I shall call, with some distortion of the term, 'protohistory', using it to mean the period for which there is no contemporary written evidence and which has been reconstructed solely from writings of a later period.⁵⁷

This subject is also treated briefly in Prince Chand's first chapter, although this chapter continues with short descriptions of some of the sources for later periods of Sukhothai history as well.

The main source for 'protohistory' is inscription No. 2, apparently written in mid-fourteenth century but part of which describes events of about 100 years earlier.⁵⁸ In addition to No. 2, mention must be made of Braḥ Ruoṅ/Phra Ruang, sometimes treated as a specific king of Sukhothai and sometimes as a legendary ancestor.

I concur with Prince Chand in his opinion that the legends of Braḥ Ruoṅ developed after the end of the Sukhothai period and that the name is generic, but would suggest that it is even more generic than he imagines and is not limited to Indrādity, Rām Gāṃhaeṅ, Lo'daiy and Līdaiy.⁵⁹

The name Ruoṅ/Ruang, written รวง, has apparently been accepted as ร่วง, equivalent to รุง and เรือง, 'bright', 'shining';⁶⁰ and the Pali translation *rocarāja* of *Jinakālamālī* indicates that it was understood the same way in the sixteenth century.⁶¹ In various chronicles and legendary stories written much later than the period under discussion the name has been loosely applied to kings of Sukhothai, but it is never found in Sukhothai inscriptions in reference to any contemporary ruler. Moreover, the sole epigraphic occurrence so far recognized is in No. 13, as the name of a vague ancestor,

56. See also *EHS* 3, pp 64-67; *EHS* 11-1, pp. 75-76, *EHS* 11-2, p. 108; "On kingship", pp. 53-54.

57. A more proper definition of 'protohistory' will be found in Grahame Clark, *Archaeology and Society*, pp. 24-25.

58. *EHS* 10, p. 87; *Guide*, p. 2.

59. *Guide*, p. 1.

60. *Bacanānukram rājapaṇḍityasathān/Royal Institute Dictionary (RID)*, p. 755.

61. Coedès, "Documents", p. 88, n. 3.

entitled *pū brañā* (ปู่พรญา), of a local ruler in 1510.⁶² It seems clear that ‘Ruon’ was never the title of a living, or even a recently dead, king.

I would like to suggest further that ‘Ruon’, in a slightly disguised form, also occurs in No. 45, in a context which reinforces its legendary aspect. In line 4, the first legible name in the list of ‘ancestral spirits’ is *pū ro’ñ*, (ปู่เร็ง) ‘ancestor ro’ñ’, with the vowel เอิ which was used frequently for เอิอิ in early texts. That is, *ruon* is equivalent to *ro’an*, for which *ro’ñ* could have been a common spelling in the fourteenth-fifteenth centuries. It is also interesting that *Pū Ro’ñ* heads the list of Nan, not Sukhothai, ancestors; and if we could find ‘*ruon*’ or some other recognizable variant of the name in still another Thai mythology it would be proof that the Braḥ Ruon stories are not specific for Sukhothai, but are part of a body of ancient common folklore which was carried and modified by the different Thai peoples after they split and moved from their original homeland.

This is a complex and largely unexplored subject. It is accepted today that the Thai languages are descended from a common stock which can be located in prehistoric times in what is now southeastern China and northeastern Viet Nam, and a corollary of that is that there would have been a rather homogenous population speaking the dialects of that common stock at that time. We can then postulate that those people had a common religion or common stock of cosmological beliefs. As they gradually separated through migrations which resulted in greater diversity of their languages they would have carried their old beliefs with them, but just as languages changed and were influenced by languages of different stocks, so too the ancient beliefs of each Thai group would have been modified internally to each group over time and by external influences from other belief systems. Some, but not necessarily the same, elements of the old common cosmology would have been preserved in each Thai group, and it is a reasonable hypothesis that when we find similar stories in two or more widely separated Thai groups they are relics of the common mythology, or that when a story of the ancient history of one group is duplicated in that of another, the stories are in fact common mythology.⁶³

In fact, some support for the hypothesis that the Braḥ Ruon stories might derive from common Thai mythology can be found in the Ahom chronicles. There *Khun Lung* and *Khun Lai*, brothers, are the first ancestors

62. Coedès, *Recueil*, pp. 157-60; *EHS* 14; note *EHS* 1, p. 218, that *pū*, ‘grandfather’, does not need to be taken literally; and see also Fiffinger, “Kinship terms of the Black Tai people”, for various uses of ‘*pū*’ in another Thai language.

63. G/P, “On kingship”, 30-31; James R. Chamberlain, “The origin of the Southwestern Tai” *Bulletin des amis du royaume Lao*, 7-8 (1972), pp. 233-43. So far as I know the only writing on old Thai mythology is in Henri Maspero, *Oeuvres posthumes*, “Religions des anciens chinois”, pp. 170-84.

who descend from heaven to be kings on earth, and *Khun Lung* had a son called Tao Leu. Furthermore, *Khun Lung* and *Khun Lai* were considered by the Ahom to have been grandsons of Indra, and Indra, who was ‘Lord of Thunder’ and ‘Lord of Heaven’, gave *Khun Lung* a special sword as a palladium.⁶⁴

Now given the circumstance that in Thai languages ‘l’ often alternates with ‘r’, ‘*lung*’ is a plausible doublet of ‘*ruñ*’, and even the most casual student of Sukhothai history cannot fail to note the resemblance between the Ahom legend and the early history of Sukhothai, where Rām Gāṃhaeñ (RK), one of the putative *brah ruoñ*, is son of Indrā(dity) and has a son Lo’(daiy), and where one of the remote ancestors of Sukhothai history received a sword from the ‘Lord of Heaven’ (สัพพา), so far identified, (perhaps prematurely), as the king of Angkor.⁶⁵

The differences in the structures of the two stories would be due, at least in part, to the fact that part of the Sukhothai structure may be true, but it seems likely that old common Thai mythology was strong enough that the leaders of Sukhothai wished to adapt their early history to it.⁶⁶

The first protohistorical figures who look like real persons are found in No. 2. There seem to have been two families involved, one the family of the inscription’s probable author, Śrīsraddhā, and the other the family of the kings of Sukhothai, ancestors of Rāmarāj, the true name of the king in the Rām Gāṃhaeñ time period.⁶⁷ Prince Chand thinks these two families were closely related, but this is an extrapolation which is impermissible for a historian, even if it should have been true.⁶⁸ Prince Chand also feels that some of these ancestors should be considered true kings of Sukhothai, contrary to the more common opinion which begins that office with Indrādity, after the presumed liberation of Sukhothai from the Khmer,⁶⁹ and, more controversially, Prince Chand denies that Sukhothai was ever subordinate to Angkor, an argument which Griswold has energetically countered.⁷⁰ Griswold is justified, I would say, in his contention that the

64. *Ahom Buranji*, Calcutta, 1930, pp. 4-11, 14-15.

65. See Nos. I and II, in *EHS* 9 and 10; and see note 76 below

66 In the original I wrote “... fact that part, or even all, of the Sukhothai structure is true...”, but now I do not accept the historicity of Rām Gāṃhaeñ, which does not mean denial that Sukhothai mythology may have claimed a mythical ancestor named ‘Ruoñ’, Ro’añ, etc

67. In the original I wrote “ancestors of RK”. Justification for this argument about ancestry will be presented below. See text with n. 139.

68. *Guide*, p. 3.

69. *Guide*, pp. 2-4, 6, 21.

70. Chand, “Review 1972”, pp. 257-59; *Guide*, pp. 6-7, 21; Griswold, “Notes and comments”, pp. 150-51. [*Now I am more inclined toward the view of Prince Chand, although my arguments are different. See notes 76-77 below and associated text.

grant of a sword-palladium and daughter by a king, in the conventional interpretation a king of Angkor, is rather good evidence of suzerain status, but I believe Prince Chand is closer to the mark in his remarks about the Khòm, Khloñ Lāmbaṅ/Lampong, and the evidence does not permit establishment of the latter's identity so long as the full meaning of *khòm* is unclear. Even if *khòm* is equivalent to 'khmer', this is not sufficient to make him an Angkorean official.⁷¹

As for the rest of the story, the interpretation of Coedès, followed by G/P, seems to be about as far as one can carry the evidence,⁷² and the only other comment I would like to add concerns still more elements of the story which may be due to the influence of common Thai mythology.

The earliest ancestor mentioned is Brañā, or Ba Khun, Sri Nāv Nām Thaṃ. 'Nām thaṃ' is one of the readings of the name meaning 'submerged' 'drowned', which I discuss in detail below. Within Sukhothai mythology submerging, or drowning, is one of the attributes of Braḥ Ruoñ, and it is possible that the author of No. 2 was using it as a claim that his line descended from Braḥ Ruoñ, the most prestigious earthly ancestor.

This may be important in interpreting the information about the enormous extent of his kingdom. As G/P note, the indications of distance are unclear. Moreover, the inscription is so badly damaged that it is not even certain what the original episode was. Prince Chand, however, wished to interpret it literally in the largest sense, which in addition to the other objections, may be meaningless if the very identity of Nām Thaṃ is mythical.⁷³

In the next generation we have two individuals, Phā Mo'añ, son of Nām Thaṃ and Bāñ Klāñ Hāv, of indeterminate ancestry. 'Klāñ hāv' means 'middle of the sky', and one of the elements of old Thai mythology identified by Henri Maspero was a belief that the first rulers of the first Thai kingdom came from a region outside the vault of heaven to settle and organize the first *mo'añ* 'under the sky', which then became the homeland of the Thai.⁷⁴

The Ahom stories also make an interesting contribution. Just as in the Sukhothai genealogy, where there are two possibly rival lines (on this point see further below), in the Ahom cosmology Khun Lung was accompanied to earth by his brother Khun Lai and the latter's descendant in the third

71. He could have been an official of a non-Angkor Khmer state in central Siam. See my remarks on Khòm in *JSS*, January 1972, pp. 409-10; *JSS*, July 1973, pp. 208-09; and Vickery, "The Lion Prince and Related Remarks on Northern History", pp. 332-33, n. 24.

⁷² See, however, following discussion.

73. *EHS* 10, p. 86, n. 17; *Guide*, pp. 20-21.

74. Maspero, *op. cit.* This accounts for the obviously mythical 'Fā Ngum' ('sky covers'), founder of the Luang Prabang monarchy, and also 'Ngam Muong' in Payao.

generation was Chao Pha (*cau fā?*) Phan Klang Jeng Klang Rai (= *rau?*). Ahom ‘*rau*’ is cognate with Sukhothai ‘*hāv*’, ‘air’, and Ahom ‘*j*’ corresponds to Sukhothai and modern Thai ‘*y*’; and therefore *phan klang jeng* is unavoidably reminiscent of Sukhothai’s *mo’an pān yān*, the home of Bān Klān Hāv, part of whose name, in the form ‘*klang rau*’, is also found in the Ahom context.⁷⁵

The author of No. 2 says that the ancestor (possibly his own) Phā Mo’an received a sword from the god (*phī fā*, literally ‘sky spirit’) of Sodhara, which as I have already indicated bears some resemblance to the Ahom story. Given the close cultural relations which may be presumed between Sukhothai and Angkor, it seems doubtful that a real king of Angkor would have been called *phī fā*.⁷⁶ However, sometime after the classical Angkor period Yaśodharapura, name of the Khmer capital from the 10th century, came to be known as Indrapraṣṭha, ‘establishment of Indra’,⁷⁷ and the intention of No. 2 may have been to make a connection, not with the real Angkor, but only with its new, legendary, status as the city of Indra, the chief god, who gave a sword to one of the first Thai chiefs on earth.

There are clearly mythological elements in the account of the protohistorical period, but it is impossible at this point to draw the line between pure myth and myth adapted to true history. One of the possibly mythical elements is the connection made in No. 2 between Sukhothai and Angkor and which has occasioned disagreement between Thai and Western scholars. I would say that if that element, grant of a sword and daughter, is true history, then Sukhothai was a real vassal of Angkor, at least at the time of Phā Mo’an. If, however, the mythical structure I have postulated is accepted, then such a connection between Sukhothai and Angkor may be

⁷⁵ *Ahom Buranji*, p. 4; “Ahom language”, pp. 84-85, 118, 136. What I am arguing here is rather speculative. It is that the Ahom symbols for vowels ‘*ai*’ and ‘*au*’ are easily confused, and that ‘*rai*’ may have been written for ‘*rau*’ in the title concerned.

⁷⁶ Griswold and Prasert (EHS 10, p. 111, n. 41) assumed that *phī fā* was “the *T’ai* equivalent of the Skt *devarāja*”. This is hardly possible, and is not even acceptable as a rough translation. Moreover, it is unlikely that the Thai of Sukhothai had ever heard the term *devarāja* which was never a title in use, and occurs in only one Angkor Sanskrit inscription, K.235 of Sdok Kak Thom, as a translation of the Khmer title *kamrateñ jagat ta rāja*, but has been blown out of all proportion by careless modern scholarship, which assumed that the title was created at the time of Jayavarman II and was universally used. See Coedès, *Etats hindouisés*; pp. 00-00; Claude Jacques, (“The Kamrateñ Jagat in Ancient Cambodia”. Finally, Coedès “Le véritable fondateur du culte de la royauté divine au Cambodge”, proposed that the Khmer title in question developed in the time of Jayavarman IV.

⁷⁷ The first use of ‘Indrapraṣṭha’ for Angkor cannot be known. It is not found in any of the Angkor-period inscriptions, and its first extant contemporary appearance is in the 16th century *IMA*, 3, dated 1579, in the form *indrapraṣṭha*.. See discussion of this in Vickery, “Cambodia after Angkor”, pp. 214, 291, 485-86.

rejected, for the other arguments put forward by Griswold, for example, are not sufficient. Neither artistic nor linguistic influence implies political subjection, and both could have been transmitted through peaceful intercourse between sovereign polities.⁷⁸

There are still interesting things to note about the Sukhothai royal genealogy in this period. Phā Mo'añ is said to have transferred his title, 'śrī *indrapatīndrāditya*', to his contemporary, Bāñ Klāñ Hāv. In the following passage of No. 2 it says Śrī Indrāditya had a son named Rāmarāja, and this makes a connection between No. 2 and No. 1, in which RK is said to have been son of Śrī Indrāditya, and it also allows the identification of Rāmarāja with RK.

All students of Sukhothai history have assumed that '*indrapatīndrāditya* and '*indrāditya*' were equivalent, and that for some reason the original title had simply been abbreviated. The difference, though, must not be ignored. The form *indrāditya*, found in two inscriptions set up within one or two generations, must be accepted as a probably true title, especially since titles including a god's name followed by *āditya* were rather common in the Angkor period and in particular for the dynasty of Jayavarman VII.⁷⁹

The title *indrapatīndrāditya*, however, is quite another matter. It suggests, superficially, an embellishment of the title *indrāditya*, implying perhaps an even higher rank; and according to No. 2 it was this more impressive title which had been granted by the *phī fā* of Angkor to Phā Mo'añ, the direct ancestor of the inscription's protagonist.

The title consists of the elements *indra+pati+indra+āditya*, or *indra+* 'chief, lord' + *indra+* 'sun'. Titles of this form, with *pati*, are known in Angkorean usage--for example, *Nṛpatīndrāditya*, *Bhūpatīndrāditya*, *Mahīpatīndrāditya*, *Rājapatīndrāditya*⁸⁰ and perhaps others, but nowhere else is there an *Indrapatīndrāditya*. The reason is not hard to conjecture. In all such genuine titles *pati* is used as 'lord of', and compounded as above gives the meanings, respectively, *Indrāditya* who is 'lord of men' (*nṛ*), 'lord of the earth' (*bhū*, *mahī*), and 'lord of kings' (*rāja*). Since, however, Indra is the chief god it makes no sense to say 'lord of Indras' (*Indrapati*), and I suggest therefore that the title is the invention of a later generation and never really in use.⁸¹

The titular embellishment, I suggest, was a deliberate invention of Phā

78. Griswold, "Notes and comments", pp. 150-51.

79. See *EHS* 10, o. 112; the index of names in George Coedès, *Inscriptions du Cambodge* (IC), VIII; and Coedès' remarks in *IC*, V, p. 281.

80. See index to Coedès, *IC*, VIII.

81. The alternative, grammatically legitimate, translation, 'Indra the lord', is not acceptable here since the sense of the form is established by the several genuine examples in which 'Nṛ the lord', 'Bhū the lord', etc., are impossible.

Mo'an's descendants who were responsible for No. 2, in order to enhance the prestige of their line over the family of Indrāditya, father of 'RK'-Rāmarāja. It is noteworthy that No. 2 barely mentions the contemporary royalty of Sukhothai and nowhere gives them conspicuous praise. It suggests a rivalry between two families with the author of No. 2 retrospectively surrounding his ancestors with spurious attributes. This might explain Nām Tham as the most ancient ancestor. According to one cycle of legends, Braḥ Ruoñ himself was the king who drowned, and the author of No. 2 would have been claiming direct descent for his line from the most important legendary hero of the Thai.

While on the subject of Sukhothai royal ancestors we should again consider No. 45, which starts out by invoking 'ancestral spirits' as witnesses to a treaty, apparently between Sukhothai and Nan, and to which Prince Chand gives attention immediately after his remarks on the protohistorical period.

Ever since No. 45 was discovered in 1956 it has caused considerable excitement among Sukhothai scholars, prompting them to add, at the very least, two new names, unknown to Prince Damrong and Coedès, to the list of Sukhothai kings. At the extreme Kachorn Sukhabanij has apparently argued that all the names of No. 45 should be considered kings of Sukhothai; Prasert, in one of his early studies, accepted them all as some kind of *cau mo'an*, but only those with the title *pū brañā* as kings; and G/P together still accept only the latter as true kings.⁸²

Prince Chand argues that both Saiy Saṅgrām and Nua Nām Tham were true kings of Sukhothai, but his explanation involves the assumption that the *vān hnā* (*mahā uparāj*, 'second king') institution of Bangkok times already existed at Sukhothai, and he provides a table of family relationships which is pure conjecture.⁸³ Even if his reading of 'Sueng' in place of 'Lueng'/lo'an is correct it does not justify the setting up of a two-branch family as he has done.

In an earlier article I argued that both Nua Nām Tham and Saiy Saṅgrām were legendary figures, that is not real people, and based this argument to a great extent on the unlikely circumstance that both are also found in similar relative positions in the chronicular history of Chiang Mai. Prince Chand has challenged my argument, which I wish to renew with further evidence at the present time.⁸⁴

In his note to my "Lion Prince", pages 285-87, Prince Chand says that

82. Kachorn Sukhabanij, "Thai Beach-head States; Prasert, *op. cit.*, pp. 39-40; p. 45; and G/P, *EHS* 3, p. 82, n. 20.

83. Chand, "Review 1972", pp. 265, 267-69; *Guide*, p. 5.

84. Vickery, "Lion Prince", pp. 371-76; M.C. Chand Chirayu Rajani, "Remarks on 'Lion Prince'", pp. 281-91.

the names Nāṃ Thuom, Nāṃ Thum, Nāṃ Duom (น้ำท่วม, น้ำตม, น้ำท่วม) mean, not that the person concerned was drowned, but was ‘flooded’ that is, he was born during a flood; that there “must have been hundreds of such people, both princes and commoners”, with such a name; and that the two so-called princes of Sukhothai and Chiang Mai were therefore real persons. He adds, moreover, that the terms in Thai for ‘flooded’ and ‘drowned’ are not synonymous, and that the latter would be จมน้ำตาย. It would be presumptuous of me to argue a point of Thai language with Prince Chand on my own authority, and so I shall merely demonstrate that scholars, both Thai and foreign, and including Prince Chand himself, who have studied the names in question, have generally agreed that they mean ‘drowned’, or in some way ‘covered over with water’, at least until I showed the unexpected conclusion that could be derived from this.

First there was Ratapaññā of Chiang Mai who in the sixteenth century wrote *Jinakālamāli*. In the admittedly confused Sukhothai genealogy found in that work he inserted the name ‘Udakajjotthatarāja’, obviously corresponding to the Nāṃ Thaṃ of the No. 45 list and which Coedès translated as ‘the king who plunged into the water’.⁸⁵ There is no ambiguity about the Pali term. It cannot mean ‘flooded’ in the sense that a flood occurred, but only that the person in question, by accident or voluntarily, went below the surface of the water. A modern Thai scholar, Saeñ Manvidūr, in his Thai translation of the same story, has explained the same Pali term as พ่อขุนจมน้ำ, literally ‘the king who sunk into/was submerged/in water’, or พ่อขุนน้ำท่วม (*pho khun nāṃ duom*), indicating that at least one Thai authority considered the two concepts synonymous, that is the king in either case was covered over with water.⁸⁶

As for the Nāṃ Thuom of Chiang Mai history, this name was rendered in *Jinakālamāli*, by ‘Najjotthara’, which Khun Saeñ agrees was meant to be Nāṃ Thuom, although he seems to wonder whether the Pali translation is accurate⁸⁷. Coedès in this case gave the equivalent ‘Nam Thuem’, and cited *Baṅśāvatār Yonak*, a late and often inaccurate synthesis, for the explanation, ‘born during a flood’⁸⁸. My innovation here was to suggest that the Pali ‘Najjotthara’ was intended to mean ‘covered by water’ and that there was a legendary connection between him and the Sukhothai Nāṃ Thaṃ Thuom.

Prasert in one of his early writings has accepted that the Nua Nāṃ Thaṃ of No. 45 and the Nāṃ Thum of *Mūlasāsanā* are equivalent to one

85. George Coedès, “Documents”, p. 99.

86. Saeñ Manvidūr, trans., *Jinakālamālipakaranam*, p. 110, n. 5.

87. Saeñ, *op. cit.*, p. 103, n. 3.

88. Coedès, “Documents”, p. 92, n. 4; A.B. Griswold, “Thoughts on a centenary”, *JSS* L11, 1 (April 1964), p. 33.

another and to Nāṃ Duom (น้ำท่วม), and that they may be compared with the ‘Udakajjotthata’ of *Jinakālamāli*, while G/P together explain Nāṃ Duom as either ‘submerged in water’ or ‘flooded’, showing that one more Thai authority admits the possible synonymity of the terms in this case. G/P go on to relate Nāṃ Duom to the relevant Pali terms found in *Jinakālamāli* and *Sihinganiḍāna*, which they translate respectively as ‘covered with water’ and ‘overflowing with water’⁸⁹.

As for Prince Chand, on the first page of his *Guide*, he refers to ‘Nguā Nam Thom’, “said to have been Bān Mueng’s son or grandson’ in the chronicles, and he explains the name as ‘the king who was drowned [or] the wording here could also mean the king who was flooded, that is, he was born when there was a flood’. The reference to descent from Ban Mueng shows Prince Chand is thinking of the passage in *Jinakālamāli* mentioned above, and his explanation proves that before reading “Lion Prince” he was ready to accept ‘Nāṃ Thom/Thuom’ as containing both meanings, ‘drowned’ and ‘flooded’ and he preferred the former. The same opinion comes forth clearly on his page 5, where, discussing No. 45, he says, “...Pu Phya Nguā Nam Thom... called the ‘king who was drowned’”, with no mention of ‘flooding’⁹⁰.

So much for the literal readings of these terms. For the hypothesis that I put forward in “Lion Prince”, the literal meaning in **modern standard Thai** is less important than the acceptance by Prince Chand, along with other authorities, that all the various spellings of ‘Nāṃ Thuom’ in the different sources are equivalent, and the agreement of qualified scholars that ‘Nāṃ Thuom’ has been rendered in Pali works by terms which mean in some sense ‘covered by water’ or ‘submerged’. As Coedès realized, and as I emphasized in ‘Lion Prince’, these names very likely have some relationship to the legend of Braḥ Ruoṇ disappearing in a river. Prince Chand refuses to admit such a relationship, and says simply that there were hundreds of real people both princes and commoners, who were named ‘Nāṃ Thuom’ through birth at flood time. All I can say to this is that if all the people born during floods were so named, names for those generations would no longer serve their primary purpose of distinguishing one individual from another. Of course I cannot prove that certain princes were not named after floods, and, emphasizing again that historical reconstruction is based on probabilities, I will only reassert that (a) the consensus of expert opinion before I wrote “Lion Prince” was that the names in question more likely meant ‘submerged’ than ‘[born] flooded’, which is sufficient answer to Prince Chand’s

89. Prasert, op cit, pp. 25, 40: *EHS* 11-1, p. 72, n. 6.

90. See also *Guide*, p. 57, “Nguā Nam Thon died (possibly by drowning as the chronicles state)”.

argument; (b) this theme is clearly related to a legend of Braḥ Ruoṅ; (c) the Nāṃ Thuom-type names occur in similar structural sequences in both Sukhothai and Chiang Mai sources, and in the latter in a reign sequence which is suspect in other ways; and (d) therefore we are entitled to suspect interpolation of common legendary material in both places.

There is also other evidence that the theme of a prince submerging in water was part of Thai mythology, even when the name ‘Nāṃ Thuom’ was not used. In the eighteenth century there were political upheavals in the northern Thai states and for a time there was no ruler at Lampang. According to the Chiang Mai chronicles the chief monk recommended a certain Nai Tip’a Chak (นายทิพย์จ๊ก), possibly *devacakra*⁹¹ as leader against the Burmese, and the latter was later crowned as P’raya Sula Leü Xai (พระยาสุลละลือไชย). He was succeeded by a son, Xai Keo (ไชยแก้ว) who was given the title P’raya Xai Songkram (พระยาไชยสงคราม). The latter was also succeeded by a son named Chao Xai Keo (เจ้าไชยแก้ว) who settled a conflict with a rival by means of an ordeal to see who could survive submersion in water. Chao Xai Keo won easily; “he descended and sat peacefully at the bottom of the water”⁹².

The reader cannot have failed to notice here a familiar structural sequence: a king Lo’ (ลือ, and in fact ลือไชย might be a corruption of ลือไทย/Lo’ daiy) followed by a king Jaiy Saṅgrām, followed by a submersible king, a veritable Nāṃ Thuom. Perhaps Prince Chand would argue that this is merely a straightforward recounting of events which happened just as stated. I would find such an explanation utterly incredible, but of course there is no proof. There is strong probability, though, that a persistent old myth surrounding the beginnings of Thai history has influenced the writing of history for a period when another new dynasty was being formed.

Prince Chand also took issue with my interpretation of the names Saiy/Jaiy Saṅgrām, arguing that they are quite different, Saiy meaning ‘4’ and Jaiy ‘victory’. If he is correct, then my argument about similar structures in the Sukhothai and Chiang Mai reign sequences is considerably weakened. *Saiy* occurs in a number of traditional titles in a variety of sources and G/P as well as Chand have sought to interpret it as ‘4’, meaning the person concerned was the fourth son of his father⁹³. As evidence they

91. ‘*Devacakra*’, ‘divine disk’, might possibly mean the sun, which would relate it to one of the names of Braḥ Ruoṅ, ‘*aruna*’, for which see *PN*, pp. 8-11. Another possible etymology for ‘T’ip’a Chak’ would be *divya cakṣu*, ‘supernatural vision’.

92. Camille Notton, *Annales du Siam* III, “Chronique de Xieng Mai”, pp. 192-195; *Tāmnān*, pp. 87-90. Romanization here is according to Notton. The story will henceforth be referred to as *CMC*.

93. Prasert, *op cit*, p. 57; G/P, *EHS* 2, p. 38 n. 5.

can point to the list of the brothers of King Tilokarāj of Chiang Mai where all were given apparently numerical names with Tilokarāj himself as ‘lok’, number six, and where the fourth son is designated ‘sai’.⁹⁴ On the other hand one can see in the Nan chronicle that the personal name Saiy need not have this connotation. In an undated episode of the very early history of Nan there is a *cau khun sai* who has no brother, and in *cula* 684 we find a *cau sai* who was sixth son. Prince Damrong, moreover, although not in a context concerning Chiang Mai, considered that ‘tilok’ was equivalent to ‘trailok’, and if so, the the entire conception of these names deriving from numerals is weakened.⁹⁵

Whatever the practice with respect to the use of *saiy* alone as a personal name, *saiy* in this usage is not the point of interest. What I am concerned with is the combination *saiy saṅgrām*, which is something quite different. It appears to be a title belonging to a class of titles of which the second element is *saṅgrām*, ‘warfare’. Among such titles are not only *jaiy* and *saiy saṅgrām*, but *bijaiy saṅgrām*, *mahā saṅgrām*, *rāj saṅgrām*, *kra:setr saṅgrām*, *rāmraṅaraṅ saṅgrām*, *bejrāt saṅgrām*, *kāṃhaeṅ saṅgrām*, and others⁹⁶; and there is no title in which *saṅgrām* alone follows a personal name or a *yaś* rank. *Saṅgrām* is always the second element of a two or more-element title. Thus even if *saiy* is not equivalent to *jaiy*, neither is it likely to mean ‘fourth son’ in such a context.

The proof that *saiy* and *jaiy* in this context are equivalents is rather lengthy and wide-ranging. First, in certain Thai dialects, including some in the north, the sound of ‘j/๒ has become /s/, pronounced like ‘s/๓, although it is written with a special consonant equivalent to standard Thai๒. Thus *jaiy saṅgrām* in the north is pronounced /sai saṅgrām /, and this is seen in Coedès and Notton’s translations and transcriptions by the use of ‘x’, as in *xai*, *xaya*, whereas ‘s’ is used for๓.⁹⁷ The difficulty is that in No. 45 we find *sai* (๓) *saṅgrām* instead of the ๒ we would expect if my hypothesis is correct, and the only certain confusion between๒ and ๓ which I have found concerns the particle ๒๓, sometimes written ๓.⁹⁸ Otherwise the writing of whatever period seems to distinguish between /s/ (๓) and /s/ (๒).

94. *Tāṃnān*, p. 48; and for a firm expression of this belief see Prince Dhani Nivat, “Recent Siamese Publications, 434, *The Yuan Pāi*”, p. 277.

95. *Nan Chronicle* (NC), pp. 294, 298. Prince Damrong, “Commentary to the reign of Trailokanath”, *RA*, p. 263, remarking that some texts called Trailokanāth ‘Trailokanāyak’ or ‘Tiloka’, but “it is all the same”.

96. “Laws of Civil and Military Hierarchies”, *Laws* I, pp. 280, 282, 296, 320.

97. The indication /.../ is to show rough phonetic approximation; underlining indicates transliteration of the written symbols.

98. *Śilā cāru’k*, III, pp. 83, 137, 148 for examples.

The purely linguistic evidence, then, is inadequate, and indirect proof must be derived from the usage in various texts. In inscription 86⁹⁹, set up in 1528, the most important official mentioned is *brahṇā srī sairanaṅ samgrām*, apparently someone of *cau mo'an* level in the Sukhothai-Phitsanulok area. The language is Thai but the script Khmer, which has no symbol for 𑜀, and thus all /s/ sounds are written with the Khmer equivalent of 𑜀; but it would seem that in the dialect of that place and time the sounds of j/𑜀 and s/𑜀 had not coalesced, and thus *sai* still seems anomalous. The title I have cited from the inscription is not found as such in other sources, but if corrupted in later texts or usage it could easily devolve to *sairanaṅ*, *saiṅaraṅ*, or *sai saṅgrām*.

In the reign of King Nareśuor, during a war with the Burmese, the troops of 23 third and fourth-class provinces were placed under command of *Bra:yā śrī saiṅaraṅ* and *Bra:yā rājṛddhānant*, and the linking of these two officials shows that they were from Phitsanulok, proving, I would say, that the old title had devolved in one of the ways I describe. A bit later Ayutthaya conquered Tenasserim and Tavoy and *Bra:yā śrī saiṅaraṅ* was made governor of the former; and the use of this title for governors of Tenasserim continues in several more entries of the chronicle¹⁰⁰.

Now the real titles of Tenasserim governors under the Ayutthayan system are known from the Hierarchy Law and contemporary seventeenth-century letters which show them as *òkṇā jaiyyādhīpati naraṅ ṛ jaiy*.¹⁰¹

These titles are easily abbreviated to *jaiyyādhīpati naraṅ*, which with the exception of 'j' for 's' is the title of the official whom Nareśuor moved from Phitsanulok to Tenasserim and which was continued in the new location after the original holder's death.

Thus, for whatever reasons, and even though the linguistic evidence is defective, it is clear that Thai bureaucrats and scholars of the time when these titles were in use considered *saiy* and *jaiy* in such a context as equivalents; and my earlier argument about the structural similarities of the king lists of No. 45 and the Chiang Mai chronicle still holds.

There are still other things to say about the 'ancestor' lists of No. 45 and their value for historical reconstruction. The 'ancestors' are explicitly invoked as *ārakṣ*, 'spirits'¹⁰², and while genuine ancestors may be venerated

99. In *Śilā cāru'k* IV.

100. *RA*, p. 158; and see *Laws* 1, p. 317, where both *Braḥ rājṛddhānant* is still listed as Balat of Phitsanulok. *RA*, pp 160, 170, 183-85; *Bāncāndanumās*, p 251.

¹⁰¹ Dhani Nivat, H.H. Prince and Maj. E. Seidenfaden, "Early Trade Relations between Denmark and Siam"

102. G/P feel that the *ārakṣ* are unnamed 'guardian spirits' distinct from the 'ancestral spirits'. See *EHS* 3, p 80.

as protective spirits, it is by no means certain that all spirits invoked in a given context were real people. I mentioned above that the first one in the list of Nan spirits seems to be the legendary Ruoñ. Following him are several more names which G/P try awkwardly

to assimilate to the list of early Nan kings found in the Nan Chronicle.¹⁰³ This is an extremely risky procedure and it would be preferable to recognize that we are simply faced with two quite contradictory lists of Nan ‘ancestors’, one in the chronicle and one in the inscription, either one, or both, of which could be fictional (one of them **must** be fictional).

The Sukhothai list in inscription No. 45 starts with *khun cit* and *khun còt*, according to G/P, with no evidence, father and uncle of Śrī Indrādity, but whom even Prince Chand apparently dismisses as mythical, the “Humpty Dumpty predecessors of Phra Ruang and Phra Lue”.¹⁰⁴ Prince Chand might have given thought, however, to the reason why the name ‘Phra Ruang’ is missing from the Sukhothai side of No. 45. We should also note that in Lao history there was a *khun cet*, son of the mythical *khun bulom*, and later on several more *cet*, including one called *cet cot/čēt cõt*.¹⁰⁵ Part of Lao mythology is thereby included in No. 45, strengthening the argument that some of it may be common Thai mythology.

Finally, it should be pointed out to those who, like G/P and Prince Chand, wish to consider the list of spirits in No. 45 as genuine ancestors, that No. 45 and No. 2, for the period before Śrī Indrādity, are contradictory, and it is thus impossible that both are true. Again we must acknowledge that, beyond the father of RK (really Rāmarāj), Sukhothai history, from whatever source, has been mixed with common Thai mythology to such an extent that the identification of real personalities may remain impossible.

The spirit list of No. 45, then, is a mixture of former genuine kings and mythical figures and it is not in itself sufficient to modify the Sukhothai reign sequence determined from other inscriptions. Specifically, if Sai Saṅgrām and Nua Nām Thaṃ continue to prove difficult to fit into the picture it is best to reject them.

The RāmGāṃhaeñ period

The first historical, as opposed to protohistorical, period of Sukhothai history is the reign of the king who has come to be known as RāmGāṃhaeñ/Khamhaeng (RK) even though all sources but No. 1 call him Rāmarāj. Prince Chand devotes little attention to this reign, he seems to feel that it is a period of few problems, and he apparently disagrees with the

103. *EHS* 3, p. 81, n. 15.

104. *EHS* 3, p. 82, n. 20; *Guide*, p. 5.

105. Charles Archambault, “Les annales de l’ancien royaume de S’ieng Khwang”, pp. 557-674. Some of the names occur in different order in the different versions.

standard treatment only with respect to the date of RK's death. The RK inscription, however, No. 1 of the Thai corpus, is discussed in his chapters 2 and 3, and it is one of the great merits of Prince Chand's work to emphasize that inscription No. 1 may not be from the reign of RK at all. This is a line of discussion which only a Thai scholar could initiate, and when it is raised by such figures as Prince Chand and *Khun* Prida Srichalalai, whose nationalist spirit is beyond question, it deserves serious attention¹⁰⁶

[*This discussion here of inscription no. 1 and 'Rām Gāṃhaeñ' has of course been superseded by later work, in particular, my own studies "The Ram Khamhaeng Inscription, A Piltdown Skull of Southeast Asian History?"; "Piltdown Skull--Installment 2"; and "Piltdown 3: Further Discussion of the Ram Khamhaeng Inscription".¹⁰⁷ It should be obvious that when I wrote the present article I was already convinced that inscription no. 1 was an early modern composition, not a genuine Sukhothai work, but I was reluctant to go that far in my discussion until the ground had been broken by a Thai. The opportunity came with the work of Dr. Piriya Krairiksh.

Betty Gosling, mischievously used my evocation above of the "first historical period" to claim that I had supported the historicity of 'RāmGāṃhaeñ', although even in this context it should have been clear that for me the historical king of the period was Rāmarāj. Readers should beware of the article by Hans Penth, which, although adding some interesting comments which undermine the traditional view of the inscription, and which strongly support the argument that inscription No. 1 is not a genuine Sukhothai work, concludes that the inscription is in fact from the lifetime of 'RāmGāṃhaeñ, not later than 1316. It is difficult to imagine that this represented Penth's opinion at the time.¹⁰⁸.*]

The standard view of No. 1 has always been that it is a production of RK's reign, and its statements that he invented the system of writing used in it and that certain territories were subject to Sukhothai have been taken at face value. Thus Sukhothai, the first Thai state, appears under its greatest king nearly as vast as present-day Thailand. This standard version of the RK

106. *Guide*, p. 30; Prasert, *op cit*, p. 83.

¹⁰⁷ Published in respectively *The Ram Khamhaeng Controversy*, Collected Papers, Edited by James R. Chamberlain, Bangkok, The Siam Society, 1991, pp. 3-52; *Ibid.*, pp. 333-418; and *Journal of the Siam Society*, Volume 83, Parts 1 & 2 (1995), pp. 103-198. The last was marred by an inexcusable number of typographical errors by the editors, and is included with corrections in the present collection, pp. 00-00.

¹⁰⁸ Piriya Krairiksh, "A New Dating of Sukhothai Art"; Elizabeth Gosling, "Architecture at Sukhothai Prior to the Mid-Fourteenth Century and Its Relation to Data in Inscription I", expanded and published as "Sukhothai Religious Architecture and its Relevance to the Authenticity of Inscription One", p. 231, citing p. 203 in Vickery, "Guide"; Hans Penth, "Difficulties with Inscription No. 1", pp. 91-100, reprinted in Chamberlain, ed., pp. 523-552.

story has had to ignore, or explain away in an *ad hoc* manner, certain peculiar features of the composition and writing of No. 1, and to neglect the question of whether a government located at Sukhothai could plausibly have exercised control over such a wide area given the contemporary possibilities for wealth accumulation and communication in the thirteenth century. This standard view has proved embarrassing in the face of certain other evidence, such as mention of a *kamrateñ* (ruler, a Khmer title) of Phetchaburi at a time when Sukhothai is supposed to have dominated that area, and the apparently unequivocal statement of contemporary Chinese writers that *Hsien*, assumed to mean Sukhothai, was not far from the coast; and these details have had to be dismissed by means of more or less ingenious assumptions¹⁰⁹.

In *EHS* 9, G/P have provided a massive new treatment along standard lines and within that framework it at least has the merit of being accessible to many more readers than Coedès' French translation in the old first volume of Thai inscriptions¹¹⁰. It is regrettable, given the problems still remaining in the interpretation of No. 1, that G/P did not see fit to provide facsimile plates instead of the various copies reproduced from old works which now have only curio value. Prince Chand has earlier criticized several details of G/P's translation, but not in a way which affects the historical significance of the inscription, and I do not intend to touch on this matter in the present review.¹¹¹

Prince Chand's main substantive disagreement with G/P for the RK period, and the major point of controversy within the standard treatment, is the date of RK's death. The inscription itself throws no light on this point. It contains dates equivalent to 1283, 1285, 1292, presumably in RK's maturity near the end of his reign, and the dates for its beginning and end have always been calculated on the basis of remarks in other sources of unequal value. G/P suggest his date of accession to be 1279, based on information in the inscription, and that his death occurred in 1298, based on a remark in the Chinese history of the Yuan dynasty¹¹².

Prince Chand would place RK's accession a couple of years earlier and his death not until about 1316, and both conclusions are based on comparisons with the Mon chronicle *Rājādhirāj* and on Prince Chand's denial that Sien/Hsien in the Chinese records ever meant Sukhothai¹¹³. As for *Rājādhirāj*, it is one of the sources which may not, at least for that early

109. See George Coedès, *The Indianized States of Southeast Asia*, p. 205; E. Thadeus Flood, "Sukhothai-Mongol relations", pp. 223, 244-46; Wolters, "A Western teacher", pp. 95-96.

110. Coedès, *Recueil*.

111. Chand, "Review July 1973", pp. 167-74.

112. *EHS* 9, p. 214, n. 99; *EHS* 10, p. 21, n. 2: G/P, "On kingship", p. 38.

113. *Guide*, pp. 15-16.

period, be accepted without full analysis, and although Prince Chand may be right about RK living beyond 1298, *Rājādhirāj*, which in any case speaks of Braḥ Ruoṅ, not RK or *Rāmarāj*, is not good evidence for it¹¹⁴.

Whatever the true date of RK's death, which in my opinion is not determinable, I agree with Prince Chand that Hsien did not mean Sukhothai and that therefore the Yuan records cannot be used to determine dates of Sukhothai history. The full argument to support this position would be rather long and will not be attempted here since it only affects the dates of RK's death and Lo'daiy's accession, neither of which is very important at this stage in the reconstruction of Sukhothai history. I will do no more than point out that confidence in the Hsien/Sukhothai equation has been decreasing in recent years. All are now agreed that by 1349 in any case Hsien meant, not Sukhothai, but some place in the lower Menam basin, and the most recent writer to devote a full study to the Yuan records on Siam recognizes "problems remaining in the unswerving identification of 'Hsien' with Sukhothai".¹¹⁵

The Hsien-Sukhothai problem has now been definitively laid to rest, with a new consensus that Hsien/sien/siem in the early Chinese records never meant Sukhothai but always designated some part of the Gulf Coast and one or more capital cities not far from the coast.¹¹⁶ This means that the surmises in G/P's *EHS* 10, p. 21 and note 2 and the alleged evidence of *rājādhirāj* are of no relevance for the death of a Sukhothai king, whether identified as Rām Khamhaeng or Rāmarāj.

We may now return to the RK inscription itself and to its value as direct testimony for the situation of Sukhothai at the end of the thirteenth century. Prince Chand thinks the inscription is really due to Līdaiy, basing his argument on the third-person references to RK and to Līdaiy's need for No. 1 and its three putative companion inscriptions for propaganda value¹¹⁷

Of more interest to the question of the authenticity of RK is its writing system, which has several curious features. First, the vowel signs for 'i', 'ī', 'u', 'ū', and all those signs now written above and below the consonant symbols, are written on the line to the left of the associated consonant and

114. *EHS* 10, p. 41.

115. *EHS* 10, p. 21; Flood, *op cit*, p. 241, n. 82; Wolters, "A Western teacher", pp. 95-96; *EHS* 1, p. 209, n. 5.

¹¹⁶ See Yoneo Ishii, "A reinterpretation of Thai history with special reference to the pre-modern period"; and Chris Baker, "Ayutthaya Rising: From the Land or From the Sea". Interestingly, Jit Phumisak maintained the view that *Hsien/sayam* was Sukhothai (*Thai Society in the Menam Chao Phraya Valley Before Ayutthaya* [in Thai]), Bangkok, Maingam publishers, 2526/1983, p. 187

117. *Guide*, p. 30. See discussion of the problem of three inscriptions in Vickery, "Piltdown 3--Further discussion of the Rām Khāmhaeng Inscription", [in this volume, pp.00-00](#).

are of the same size. Such a system is not found in any other southeast Asian alphabet, and perhaps not even in any alphabet of the Indic type anywhere¹¹⁸, and more attention should have been given to the anomaly of such an arrangement in the ‘first system of Thai writing’, and to the reasons why it was not carried on into later inscriptions. Thus the RK inscription is absolutely unique; and even if it was the first Thai writing, later scripts could not have been based exclusively on it, but must have adopted traits from other systems as well.¹¹⁹ [*In fact, since the serious undermining of inscription No. 1 cited above, true believers trying to save the phenomena have resorted to this argument, as I describe below*] Another curious feature of RK is that tonal marks are used very much as in modern Thai, while later inscriptions have a defective tonal marking system. That is, tone marks are either entirely lacking or are used irregularly, and there is only a gradual development towards the complete system in use in modern times. The same is true of the vowel system, which in No 1 is more complete than in inscriptions which were supposedly erected later.¹²⁰

Such anomalies did not disturb traditional scholars who would have accepted the idea of a perfect writing system suddenly produced by a great culture hero only to degenerate under weaker successors; but it is a commonplace of modern archeology and art history that the perfect creation comes at the end of a long period of development, not at the beginning.

Still another difficulty for the traditional interpretation of No. 1 is that spelling is inconsistent from one face to another, something which is common in Thai texts of later periods but unexpected in the first, consciously created, Thai script.

G/P take no cognizance of this point of view and accept that No. 1, with the possible exception of ‘Epilogue II’, was composed by RK himself and written while he was still alive. They also seem to accept that the script was truly invented by RK as the first Thai script,¹²¹ and thus their accounting for certain anomalies seems tortuous. They believe that RK composed the entire inscription in his mind, then dictated it to several scribes who prepared drafts of different sections for the engraver, and they assume that the scribes, “then as now, were allowed to use any spelling they liked”, that some of

118. I have not been able to make a complete survey. If one is found, it would be valuable for dating the possible influence on the RK alphabet.

119. It is misleading to speak of the RK script spreading to other areas, as G/P did in their paper, “Remarks on relations between Keng Tung and Chieng Mai before the mid-16th century”, read at the Seventh Conference of the International Association of Historians of Asia, Bangkok, 22-26 August 1977. It was the Sukhothai script as represented by the other inscriptions, not the script of No. 1, which eventually spread.

120. Coedès illustrated this long ago in “L’inscription de Nagara Jum”, *JSS* XIII, 3 (1919), pp. 5-6.

121. *EHS* 9, pp. 192-94, 196, 217, nn. 124-125.

them “may have been more progressive in their spelling than others, or they may have pronounced certain words differently and based spelling on their own pronunciation”.¹²² However, if RK had just devised a new system of writing surely he would have taken pains to see that his scribes remained faithful to it; and if it was a new system of Thai script, where would the ‘progressive’ scribes have imitated the features of spelling which are characteristic of later periods? What does ‘more progressive’ mean when there is supposedly only the one, newly devised, system of Thai writing at hand? G/P’s explanation is appropriate for texts of later periods when in fact scribes were educated in different traditions, but not for the putative period of RK; and if the inscription is accepted as a genuine composition of RK’s reign, then the statement in its face IV/8-11 about the invention of Thai writing must be accounted false.

What needs to be done now is to cease speculation and *ad hoc* rationalization about the anomalies of No. 1, and to set up a comparative analysis of tone marks, vowels, and consonants to see precisely where the system of No. 1 fits in the chain of development of all Sukhothai inscriptions. As for the odd position of vowel marks on the line, a thorough search must be made for such a system among all the Indic alphabets, and if another one is found we must determine when it might have influenced the script of Sukhothai. Such drastic innovations do not just spring forth from a great mind. Changes in writing, as in art styles, are gradual and due to complex influences, and had there been no such outside influences, the author of No. 1 would certainly have placed his vowels in the manner of other alphabets used earlier in the areas surrounding Sukhothai.

The only problem in the content of No. 1 which I wish to discuss in detail is that of the epilogues. Epilogue I lists several Thai peoples between Sukhothai and present-day Laos who were subject to RK. As G/P remark, the area covered is relatively small, but it is credible given the material circumstances of the time and the dispersion routes of the Thai peoples¹²³. Epilogue II gives a much longer list of vassal territories covering all of central Siam, parts of Laos, the Malay Peninsula as far as Nakhon Si Thammarat, and extending westward as far as Hañsāvafī or Pegu, in present-day Burma. This is the list which has hitherto been used to define the true extent of political control of the Sukhothai kingdom in RK’s time. G/P, believe, or at least believed in 1971, that Epilogue I had been composed in 1292, and Epilogue II soon after RK’s death; and thus the enormous expansion of his realm would have been carried out in the last seven years of his reign¹²⁴.

122. *EHS* 9, p. 194.

123. *EHS* 9, p. 195, and see note 63, above.

124. *EHS* 9, pp. 194-95.

In a more recent article, however, G/P accept that Epilogue II does not mean physical conquest or the exercise of continuous political control, but only that rulers of those outlying regions voluntarily submitted or became vassals. “A good many rulers, observing his prowess as a warrior, may have called him in to dislodge an enemy and then made an act of vassalage to gain his permanent protection; and others, even though not facing any immediate danger, might think themselves secure under his suzerainty.”¹²⁵ Unfortunately they resort to pure speculation to account for such submission, at times basing their arguments on chronicles of uncertain reliability,¹²⁶ including *Rājādhirāj* which they rightly rejected on the question of RK’s death.

Besides this, they have derived proof of the vassals’ submission from ideas about Buddha images which were palladia and which were sent to the suzerain’s capital as signs of submission. The difficulty with this argument is that there is not a single instance in which such a practice can be demonstrated. “The chronicles” which “tell stories of certain images that were palladia”, are very weak evidence, as G/P themselves emphasize in other contexts, and they do not in any case tell of palladia being sent voluntarily by vassals to suzerains;¹²⁷ there is no evidence whether the colossal Dvāravatī image was ever a palladium at all or how it came to Sukhothai,¹²⁸ and the importation of a learned monk from Nakhon Si Thammarat to Sukhothai is in no way a sign of the former city’s political submission. Furthermore, the Bayon cult of images to which G/P refer was something quite different. Although some have considered that images at the Bayon may have represented provinces of the realm, there is no sign that such images were made anywhere but in the capital.¹²⁹

Whatever the final decision on these arguments, an important point for the present review is that G/P now admit that Epilogue II is probably more of a propaganda statement than a description of political fact; and it should no longer stand in the way of modification of received ideas about the history of those areas included in the list of Sukhothai vassals. Furthermore, everyone has always recognized that Epilogue II is a real epilogue, that it was written later than the rest of No. 1, and by a different hand.¹³⁰ Once this is admitted, there is no way to give any precise date for it, and it can only be assigned to

125. G/P, “On kingship”, p. 39.

126. *Ibid*, pp. 40-43: *EHS* 10, pp. 26-47.

127. On the lack of reliability of the 'inserted' stories of Buddha images see *EHS* 11-1, p. 73, n. 6; and *EHS* 12, p. 117.

128. G/P “On kingship”, pp. 41-42. Chand, “Review 1972”, pp. 273-74, has some pertinent remarks on this theory of 'palladia'.

129. G/P, “On kingship”, p. 41, n. 9; George Coedès, *Pour mieux comprendre Angkor*, English translation, *Angkor*, chapter VI. This idea of Coedès is now obsolete.

130. Coedès, *Recueil*, pp. 37-38: *EHS* 9, p. 196.

a time period within the very broad limits established by the use of certain place names such as Sraḥluoṅ, Braek, and Sòṅ Gvae, which later became obsolete.¹³¹ The epilogue, which still places all vowel symbols on the line, would then have been a deliberate attempt to mislead, and its list of vassals would have no historical value at all.

It should be clear to the reader that I have a good deal of sympathy for Prince Chand's view that No. 1 really dates from later than the RK period. I also think there is some circumstantial evidence for Prince Chand's choice of the L̄idaiy period, although this still fails to explain the writing of vowel symbols on the line. [*This was written at a time when I was not ready to publicize my view that RK was a 19th-century fake. See now my three 'Pilltdown' papers*]

Above I have tried to show that No. 2 reflected a rivalry between two families of Sukhothai nobility with No. 2 representing the viewpoint of a family other than that of the legitimate kings. I suggest that No. 1, if it is of L̄idaiy's reign, represents the response of L̄idaiy to his rivals. Thus, the list of vassals duplicates more precisely the area roughly claimed for Śrīsraddhā's ancestor, Nām Tham; L̄idaiy's grandfather Rāmarāj duplicates the elephant duel feat of No. 2's hero; and Rāmarāj, son of Indrādity, is given a prestigious-sounding title, Braḥ Rām Gāṃhaeṅ, which nearly duplicates the title, Gāṃhaeṅ Braḥ Rām, given to the son of Indrapatīndrādity in No. 2. These features seem to support Prince Chand's contention that No. 1 was a propaganda effort, and that it was written after No. 2, thus plausibly sometime in the reign of L̄idaiy. [*Obviously I no longer accept "circumstantial evidence for ... the L̄idaiy" period, nor the ensuing rationalizations above. All of the anomalies are better explained by the argument that inscription no. 1 is a 19th-century *tāmnān*, the composition of which was influenced by other known inscriptions. See my "Pilltdown 3."*]

It is likely that when Prince Chand's opinion reaches a wider public its opponents might wish to make use of certain art historical evidence to disprove it. According to G/P the purpose of No. 1 was to commemorate the erection of Rām Gāṃhaeṅ's stone throne [*khdār hin*, Face3, line 12, 15, 17, 26, interestingly spelled in Khmer rather than in Thai fashion (*kra:tān*), and indeed the first dated passage of No. 1 refers to this throne. The stone seat is decorated with a band of stylized lotus petals, which in comparison with other art work might serve to date it, and these lotus petals have been given considerable attention in the recent dissertation of the art historian Hiram W. Woodward, Jr.¹³²

131. Such limits have not yet been determined. For some remarks about them see Vickery, review of van Vliet, pp. 221-22; and Vickery, "The 2/k.125 Fragment," pp. 52-53.

132. Hiram Woods Woodward Jr., "Studies in the art of central Siam, 950-1350 A.D.".

According to Woodward, “the lotus petals . . . are of an unusual and complicated type”, and the throne itself “stands apart from almost everything else in Sukhothai”.¹³³ There is only one other place where a lotus petal anything like those on Rām Khāmhaeng’s throne appears”, at the Mahādhātu in Lop Buri. Even there, it is only a single petal, amidst stucco decor of several styles, which seems to resemble the petals of the RK throne, and moreover, this petal “is entirely different from any of the other examples [at the Mahādhātu]”.¹³⁴ It seems to be chiefly on the evidence of this single petal that Woodward dates the Mahādhātu close to 1292, which he accepts as the date of the throne and No. 1.¹³⁵

Were it not for that petal, which is the primary evidence “for narrowing the time span [of the Mahādhātu] down to the closing decades of the 13th century”, the Mahādhātu could otherwise be “restricted merely to the 13th century, or possibly, to the first half of the 14th”, in Sukhothai terms possibly very close to the beginning of Līdaiy’s reign. Furthermore, Woodward sees close similarities between certain elements of decor at the Mahādhātu and at Wat Som, which “can be placed with some confidence in the second half of the 14th century”.¹³⁶ Since, however, he has decided that the date 1292 is a fixed datum, he is forced to postulate a second generation artist who created the element in question at Wat Som in imitation of an element at the Mahādhātu.

What this means for the history of Sukhothai is that if Prince Chand is right about No. 1, and I am inclined to think he is [*note above the change in my opinion as detailed in the Piltdown papers*], then the date 1292 for the stone throne is no longer certain, and the only other closely related piece of art can, in its other relationships, be placed from the middle to the second half of the fourteenth century, precisely in the reign of Līdaiy.

A further consideration is that Woodward sees Burmese influence in the lotus petal of the Mahādhātu,¹³⁷ and since communication between Sukhothai and Burma must have been quite easy, and the lotus petals of the throne and the Mahādhātu in each case unique motifs surrounded by quite different art styles, is it too much to postulate independent imitation of Burmese art, which would mean that neither the throne nor the Mahādhātu could be dated by comparison with one another?

[There is now a further complication in these ‘epicycles’ (they can

133. *Ibid*, pp. 158-59. Note that here again we find the anomaly seemingly characteristic of everything associated with RK. See also Prince Chand’s remarks on the throne in his “Review 1972,” pp. 263-65, and Griswold’s response in “Notes and comments”, p. 153.

134. Woodward, *op cit.*, p. 172.

135. *Ibid*, pp. 174, 176; vol. II, p. 12.

136. *Ibid*, vol. II, p. 15, pp. 26-28.

137. *Ibid*, p. 12.

hardly be called anything else) designed to keep RK and his putative inscription in its traditional orbit. Woodward's former student Betty Gosling has emphatically denied that the *khdār hin* recorded in inscription 1 meant the so-called stone throne, but was rather a plural expression designating several platforms. I have not yet seen any reaction to this from Woodward or other RK traditionalists. If they simply opt out and refuse to engage in debate on this question they provide implicit support for radical revision of RK]¹³⁸

Although Prince Chand has made an important contribution in suggesting that No. 1 needs to be redated, he ignored the implications this could have for a whole series of important questions in the history of Sukhothai, and he appears equivocal as to the veracity of the content of No. 1. Although stating that Līdaiy put up No. 1 and three other identical, undiscovered, inscriptions, he nonetheless believes that the autobiographical part had at sometime been written by RK himself and “Li Thai [Līdaiy] copied it from somewhere”, utilizing RK's own way of writing.¹³⁹ Thus Prince Chand is able to have his new interpretation while still preserving the old beliefs.

The reign of Lo'daiy

The reign of Lo'daiy is one of the most difficult of the Sukhothai periods to study because of the paucity of factual information which may be imputed to it. Lo'daiy himself appears in four inscriptions, none of which is really concerned with him or his reign.¹⁴⁰ Even his regnal dates are uncertain, and the one proposed by G/P for his enthronement, 1298, depends on the Hsien/Sukhothai equation which has finally been revised by Baker and Ishii, as noted above.

Prince Chand proposes 1322 for Lo'daiy's accession, having accepted circa 1318, based on the inadequate evidence of *Rājādhirāj*, for the death of RK; and his justification for 1322 merits discussion. Prince Chand says that according to a ‘chronicle’, Braḥ Ruoñ, which in this case means Līdaiy, “erased” or diminished the era (*śakarāj*) and that this is confirmed by inscription No. 4.¹⁴¹ That is, since No. 4 is from the reign of Līdaiy, and says something about changing the calendar, the reference in *PN* must also be to Līdaiy. Prince Chand then says that Līdaiy's new era began in 1322 “for reasons that will become apparent later”. These reasons, apparently, are that in the story of *Nān Nabamās* (*NN*), Braḥ Ruoñ, who in that context is supposed to be Lo'daiy, ruled until the eighteenth year of the new *śaka* era,

¹³⁸ See Gosling, *A Chronology*, pp. 18-20, 232,-233

¹³⁹ *Guide*, p. 30.

¹⁴⁰ No. 1, No. 2, No. 2I, No. 45.

¹⁴¹ *Guide*, p. 55. The ‘chronicle’ is probably *PN*; see its pp. 11-13.

or 1340, and “in the latter year Phra Ruang (Loe Thai) died after being on the throne 18 years”, or from 1322, Prince Chand goes on to say, “Loe Thai died after 18 years on the throne”, apparently basing this on *NN*.¹⁴²

Firstly, Coedès, in his version of No. 4, showed clearly that the calendrical reform mentioned there involved the calculation of days and months, not years, and it thus has nothing to do with changing an era, and does not prove that the Braḥ Ruoṅ who (in certain legends) supposedly changed the era was L̄idaiy.¹⁴³

Furthermore I have demonstrated in an earlier study that the stories of Braḥ Ruoṅ changing an era have to do with the establishment of the *cula* era, 600 years earlier.¹⁴⁴ Of course Prince Chand realizes that this cannot be any of the postulated Braḥ Ruoṅ of Sukhothai, and he would apparently agree that no Braḥ Ruoṅ established the real *cula* era. The stories, however, are explicit, although it is possible that L̄idaiy's calendrical reform contributed to the legend, and it will simply not do to say that they ‘really meant’ a change of era in the Sukhothai period. We might also ask why such a new era, with all the importance attributed to it, was never used in any contemporary source. The whole series of Sukhothai inscriptions shows nothing but *śaka*, later *cula*, and occasionally a few Buddhist era dates, but no example of a date resembling the so-called new *śaka* era of L̄idaiy.¹⁴⁵

Nothing then may be said about Lo'daiy's accession date except that he probably succeeded RK. Prince Chand, however, states that his calculations prove an interregnum between RK and Lo'daiy which must have been filled by the reign of Saiy Saṅgrām. That is, Lo'daiy took the throne in 1322 and RK died in either 1298/99 or about 1318, leaving a period of 5 to 25 years to be accounted for.¹⁴⁶ As I have indicated, both those dates for RK's death are highly conjectural; the first is based on evidence which may possibly not even concern Sukhothai, and even if the evidence for the second is accepted as relevant, there is a possible margin of error of several years either way. Whatever the date of Lo'daiy's accession we do not need to postulate an interregnum; and as for Saiy Saṅgrām, found only in the spirit list of No. 45, I maintain my contention that he was not a real ancestor or king. G/P consider Saiy Saṅgrām to have been a real ancestor, but not a king of Sukhothai, and they also accept that Lo'daiy succeeded RK.¹⁴⁷

The major contemporary document of Lo'daiy's reign is No. 2, which

142. *Guide*, pp. 55-56.

143. Coedès, *Recueil*, pp. 98-99, n. 3. This interpretation has been accepted by G/P, *EHS* 11-1, p. 138.

144. Vickery, “A note on the date of the *Traibhūmikathā*”.

145. See Vickery, “Lion Prince”, for more information on changes of eras.

146. *Guide*, pp. 56-57. On his p. 15 Chand took 1316 for RK's death.

147. See above and G/P, *EHS* 3, p. 82, n. 20.

I used above for evidence of Sukhothai prehistory, noting that my use of it would require further justification¹⁴⁸.

There has been a good deal of controversy surrounding the authorship and meaning of this inscription, controversy which to my view has been somewhat disguised in the literature to date. Coedès considered that the object of the inscription was the foundation of the Mahādhātu of Sukhothai and that the author was not the Śrīsraddhā whose activities figure prominently all through it, but another person who begins to speak in the first person mid-way through face II.¹⁴⁹ Griswold accepted the second point and added that Lo'daiy was the author. He also noted correctly that No. 2 speaks of **rebuilding** a Mahādhātu, not its foundation, and he thought the Mahādhātu was probably founded by Indrādity, the postulated first King of Sukhothai.¹⁵⁰

At this point I would only like to note that the change of person in the narrative of No. 2 is not a strong argument, since all agree that its style lacks clarity, and it is a particularly poor argument for those who have never considered the change of person in No. 1 to be an obstacle to its attribution in entirety to RK.

By the time G/P studied No. 2 together they had become less certain about its authorship, and they stated that the author was “either Lōdaiya or Śrīsraddhā, thus implicitly rejecting the argument about change in the style of narrative.”¹⁵¹ They still maintain that its purpose was the **construction** of the Mahādhātu at Sukhothai, and they followed Coedès in breaking up the text into a ‘primary text’ and ‘postscript’ which alternate back and forth from face to face of the stone.¹⁵²

Prince Chand will have none of this. For him the “inscription has nothing to do with the Mahā Dhātu at Sukhothai after the author left the country”; and Prince Chand also feels that G/P’s rearrangement of the text is unnecessary, although he fails to argue these points in detail.¹⁵³

I am in full agreement with Prince Chand on the first point, but I am afraid that, as Coedès already made clear, the division of the inscription into two parts is indicated on the stone itself.

As to the purpose of the inscription, G/P accept that several passages up to face II, line 40, relate Śrīsraddhā’s activities in Ceylon. Then the inscription says, “the Samtec Braḥ Mahāsāmī, leaving Sihala, etc”, which would seem to mean ‘leaving Ceylon’, and would also mean that the

148. See above, nn. 63 ff.

149. Face II, line 45 ff: Coedès, *Recueil*, pp. 49-50.

150. Griswold, *Towards*, pp. 17, 3, respectively.

151. *EHS* 10, p. 75.

152. *EHS* 10, pp. 77-78.

153. *Guide*, p. 2.

Mahādhātu mentioned in face II, lines 45-48, must be somewhere else, possibly at Sukhothai. The difficulty with this interpretation, relegated to the fine print of footnotes, is that between “leaving Sihala” and mention of the Mahādhātu, we find Śrīsraddhā busy near the river ‘Māvalikagaṅga’, at the Mahiyaṅgana Mahāceti, which are famous locations in Ceylon; and G/P accept that “the author has jumped from Ceylon to Sukhodaya ... [and] back again to Ceylon before returning to Sukhodaya”.¹⁵⁴

The way out of this difficulty appears farther along in the same footnote. There we have a clear explanation that Ceylon at that time was divided into two parts, that Śrīsraddhā probably considered Sihala to mean Anurādhapura, and that “leaving Sihala” simply meant going to another part of the island where the Mahiyaṅgana Mahāceti was located.

It is thus clear that the Mahādhātu mentioned in face II/45-48 is the latter edifice in Ceylon, where the Kesadhātu and Gīvadhātu of face II/49-65 were really located. This interpretation would seem confirmed by Śrīsraddhā’s explicit remarks about the “religion **in** [emphasis added] Laṅkadīpa”, which G/P have revised to “religion of Laṅkadīpa”, and “the natives of Sihala”;¹⁵⁵ and it is now definitely clinched by the identification of Kambalai as a location in Ceylon. Even so, however, G/P refuse to draw the obvious conclusion and propose to cut and rearrange the inscription to replace mention of Kambalai in a context less embarrassing for their theory.¹⁵⁶

The importance of this analysis of No. 2 is to demonstrate that its evidence for Sukhothai history lies only in the protohistorical period, not the reign of Lo’daiy; that Lo’daiy had nothing to do with it; and that to the extent the author, probably Śrīsraddhā, dealt with Sukhothai history at all, it was to glorify a family other than that of the Sukhothai kings and who may plausibly be seen as his ancestors and their rivals.¹⁵⁷

[*Another thought about No. 2 and its anomalies has been suggested by Condominas’s work on the Thai societies of North Vietnam (Georges Condominas, "Essai sur l' évolution des systèmes politiques thaïs", , p. 20)

He wrote of 5 social categories; (1) *phia tao*, hereditary chiefs of muó’ng; (2) notables, 4-13 persons; (3) *mo chang*, [‘chanter’] s’occupent du culte pour chefs et people (‘in charge of the cult for chiefs and people’). "A l’occasion des fêtes publiques, les *mo chang* évoquent les origines des *phia tao*, l’histoire des migrations des Thai; ils font l’éloge de leurs exploits guerriers qui sont l’origine de la fondation des muó’ng, ils contribuent ainsi

154. *EHS* 10, p. 127, n. 149.

155. *EHS* 10, p. 131, n. 164; pp. 132-33, and n. 184.

156. Addendum to *EHS* 10, *JSS* LXI, 1 (Jan 1973), pp. 179-80.

157. *Pace* Griswold, *Towards*, p. 17, n. 46 Śrīsraddhā’s notice of Dharmarāja (Lo’daiy) is hardly a ‘eulogy’, but merely a perfunctory compliment, the very least that would have been due the reigning king.

renforcer l'aspect divin de leur pouvoir....(le plus hautes fonctions chez les notables comme chez les *mo chang* reviennent aux \nobles)" ("On the occasions of public festivals the *mo chang* describe the origins of the *phia tao*, the history of the Thai migrations; praise the military exploits at the origin of the foundations of the muó'ng; thus contributing to the reinforcement of the divine aspect of their power...(the highest positions among the notables and among the *mo chang* are reserved for nobles)".

I would like to propose that No. 2 was the chant of such a bard in the person of Śrīśraddhā, and that this accounts for the difficulties of the text if read as an attempt to record factual history.*]

[*New work on No. 2 and on Vat Śrī Jum which I have not been able to integrate into the present republication, but to which serious readers should give attention include Betty Gosling, "Once more Inscription II--an Art Historian's View", *JSS* volume 69, 1981, pp. 13-42; Hiram Woodward, "Observations on Wat Si Chum"; Review of Skilling, *et. al.*, *Past Ten Lives of the Buddha*, pp. 153-161 and 221-225 respectively in *JSS* Volume 97, 2009; and work cited in note 251 below. Gosling insisted that the *jataka* reliefs of Vat Śrī Jum were originally for the Sukhothai Mahādhātu and that no. 2 listed other architectural creations at Suikhothai, whereas Woodward with equal emphasis states that "Inscription II and Wat Si Chum are indissolubly linked and that the inscription has nothing to do with Wat Mahathat at Sukhothai". "George Coedès's assignment of inscription II to Wat Mahathat and the ingenious theories of A.B. Griswold and Betty Gosling concerning Jātakas and building activities there in the future need not play a part in scholarly discourse". *]

The next controversial aspect of G/P's reconstruction of the reign of Lo'daiy is their reliance on interpretations of a variety of secondary chronicle sources. Without them, of course, the reign of Lo'daiy would be nearly blank, since so few inscriptions from that time have been preserved. All of these chronicles are, or contain sections, of dubious accuracy, and G/P's method of extracting parts of them as literal truth to combine, sometimes with *ad hoc* emendation, overt or covert, into a historical synthesis is entirely contrary to proper method,¹⁵⁸ although this does not necessarily mean that all of their reconstructions are false.

The chronicles used by G/P are, in order of their appearance in *EHS* 10 pages 29-70: the *Sāṅkhep* chronicle, *Rājādhirāj*, *Mūlasāsanā* and *Jinakālamālī*. A general criticism of these sources cannot be undertaken here, and I shall confine myself to some of the more obvious weak points.

158. Such chronicles should not be used in synthesis until they have been carefully dissected, and the sources of their information identified. See my "Lion Prince" for an attempt at such analysis, and Vickery, "Cambodia after Angkor", for a complete study of the Cambodian chronicles up to about A.D. 1600.

G/P use *Sankhēp* to establish certain facts about the town of Traitriṅśa, assumed to have belonged to RK because it is **not** mentioned in his list of territories, and to establish certain details about the family background of Rāmādhīpatī I of Ayutthaya. *Sankhēp* was written only in 1850 as a summary of the Ayutthayan chronicle of 1795, but with certain details incorporated from the van Vliet/ *Sāṅgītiyavaṅś* tradition.¹⁵⁹ Its chronology is known to be inaccurate from early in the fourteenth century to about 1630, it incorporates a list of Ayutthayan tributaries which G/P recognize as a later interpolation,¹⁶⁰ and given such serious defects it is simply not permissible to leap onto another part of *Sankhēp*, concerning the antecedents of Rāmādhīpatī I, and treat it as historical truth.

The sources for the *Sankhēp* story are unknown, but it is significant that it makes Rāmādhīpatī I descend from northern princely families and that it was composed by a prince of the Bangkok dynasty, one of whose major preoccupations in the nineteenth century was the subjection of northern Siam to Bangkok authority.

After G/P had produced their EHS 10, Charnvit Kasetsiri collected a number of different stories about the background of Rāmādhīpatī and concluded that he originated from Petchaburi, not the north. I find Charnvit's reasoning no more convincing than that of G/P, but at least his study proves that there are many versions of the antecedents of Rāmādhīpatī, all possibly of equal validity (or none), and the historian may not choose any one of them without offering strong evidence for the rejection of the others. My own view at this writing is that all such stories may be legend, that Rāmādhīpatī I did not have to come from anywhere, and that he may have been of a strictly local family in the lower Menam basin.¹⁶¹ [This is the implication of the new consensus on 'Hsien/Sien', as noted above (n. 116)]

Mūlasāsanā and *Jinakālamālī* are used together for the story of the propagation of the Sihlabhikku community in Siam and this story is also relevant for dating certain parts of the reigns of Lo'daiy and Līdaiy. The whole story seems to begin in 1331, a date read into *Mūlasāsanā* by G/P, and ends in 1369 with the monk Sumana's arrival in Lamphun as recorded in

159. See David K. Wyatt. "The abridged Royal Chronicle of Ayudhya of Prince Paramanuchitchinorot"; Vickery, "Cambodia After A ngkolr"; review of van Vliet, p. 235.

160. *EHS* 10, p. 37. Griswold's reliance on such chronicles in *EHS* is surprising, since in his "Thoughts on a centenary", *JSS* LII, 1 (April 1964), pp. 21-56, he took a very critical view of certain chronicles such as *PN* and *Baṅśāvātār Yonak*, and to the extent that *Sankhēp* purports to break new ground it is no more reliable than the former two.

161. Charnvit Kasetsiri, *The Rise of Ayudhya*, pp. 58-68. This would be awkward for those who insist that Rāmādhīpatī I was Thai, since it is now admitted that the delta population in the 14th century was probably more Mon or Khmer. See G/P, "On kingship", pp. 29, 33-34; *EHS* 3, p. 62.

inscription No. 62 of Wat Phra Yū'n.¹⁶² In between these dates, according to G/P, Sumana would have studied in Martaban in the early 1330s, would have returned to Sukhothai in the late 1330s, would have been reordained in Martaban in 1339-40, and would soon thereafter have gone to reside in the Mango Grove in Sukhothai while his companion resided at the Red Forest Monastery in Śrī Sajjanālai. These dates are based on *Mūlasāsanā*. *Mūlasāsanā* also indicates the existence of separate rulers of Sukhothai and Sajjanālai, the first of whom was entitled Dharmarāja; and G/P identify them as Lo'daiy and Līdaiy, respectively king of Sukhothai and viceroy of Sajjanālai, which accords with the view, supported by No. 4, that Līdaiy was appointed chief of Sajjanālai in 1340 and resided there until 1347.¹⁶³

In general, but with an important difference, *Jinakālamālī* relates the same story. A difficulty with this story is that epigraphic evidence seems to indicate that the Red Forest Monastery and the Mango Grove Monastery were not founded until 1359 and 1361 respectively.¹⁶⁴ G/P struggle unconvincingly to rationalize their way around this difficulty, and conclude with a remarkable bit of special pleading: "While epigraphic evidence is certainly preferable to chronicular, is the conjectural interpretation of an inscription preferable to the straightforward testimony of two chronicles whose accounts are plausible in themselves and internally consistent enough?"¹⁶⁵

Besides the fact that utterly fictional accounts may be 'plausible' and 'internally consistent', there are other important considerations which tend to support the evidence of the inscriptions. *Mūlasāsanā*, from which G/P's dating derives, contains an extremely confusing chronology, as even G/P admit,¹⁶⁶ and even there a much longer time period than 1331-1340 could easily be read into the events it describes. Finally, and this is what I meant by 'covert emendation', G/P have ignored the explicit indication of *Jinakālamālī*, which they otherwise accept as the chronologically most accurate northern chronicle,¹⁶⁷ that the story of Sumana should be placed between 1355 and 1369,¹⁶⁸ which fits very well with, and reinforces, the statements of the inscriptions.

This last point has relevance for certain other problems in Sukhothai history. *Jinakālamālī*, like *Mūlasāsanā*, also mentions two separate rulers in

162. *EHS* 10, p. 55, n. 3; p. 60 and n. 24; *EHS* 13.

163. *EHS* 10, pp. 61, 64.

164. *EHS* 10, p. 71.

165. *EHS* 10, p. 72. It should be emphasized that the chronicles, especially *Mūlasāsanā*, are anything but straightforward, and the evidence of the inscriptions on the dates of these monasteries is not conjectural.

166. *EHS* 10, pp. 53-54, n. 2, and p. 55, n. 3.

167. *EHS* 1, p. 211, n. 12; p. 226, n. 39; G/P, "Yuan phai", p. 129, n. 2.

168. Coedès, "Documents", pp. 95-102.

Sukhothai and Sajjanālai, ‘Dhammarāja’ and his son ‘Lideyyarāja’; and in the 1350s the former could only have been Līdaiy, which would mean that the latter was the king thus far known to historians as Mahādharmarāja II.

G/P returned to this question again in an appendix to EHS 12, pages 114-19, and they finally recognized that the testimony of *Jinakālamālī* poses a real problem. Still they do not face the evidence squarely. Although noting that the account of Sumana falls between 1355 and 1385, they say *Jinakālamālī* does not give a precise date for the time when Dhammarāja was reigning at Sukhothai. The wording of *Jinakālamālī*, however, is, following the coronation of Kilana in 1355 and the installation of his brother in Chiang Rai, “at that time [emphasis added] King Dhammarāja was reigning in Sukhodayapura”¹⁶⁹ It is clear that the writer of *Jinakālamālī* meant this to correspond to the time of Kilana, and there is no reason, within the context of *Jinakālamālī*, to invoke the possibility, as G/P do, on page 116, that *Jinakālamālī*’s account mentions two different Dhammarājas.

G/P attempt to get around the problem in another way (p. 117), by admitting first that *Mūlsāsanā* is corrupt. They then say the account of Sumana in *Jinakālamālī* “seems to be based largely on *Mūlsāsanā*” - something not hitherto apparent to other scholars and which requires demonstration - and that *Jinakālamālī* is therefore not good independent evidence. But if *Jinakālamālī* is based on *Mūlsāsanā* why does it have quite different dates? They also say the story in question is one of the inserted narratives of *Jinakālamālī* which are not so reliable as the basic text, even though it is precisely this inserted narrative which is most frequently used by G/P as a basis for other conclusions in their EHS.¹⁷⁰

Finally though, on page 119, they are almost forced to admit that Sumana may not have come to Sukhothai or discovered the relic until 1361, that therefore the Dhammarāja of *Jinakālamālī* could not have been Lo’daiy, but was Līdaiy I, and that the Lideyya of *Jinakālamālī* would thus be one of his sons, “perhaps the ‘Father Lōdaiya’” of No. 45 who “may have been an elder half-brother of Mahādharmarāja II”.

This last explanation still involves covert assumptions - that Mahādharmarāja II could not have been named Līdaiy/Lo’daiy, or that in 1361 Mahādharmarāja was too young to have been viceroy in Sajjanalai - and I intend to show below that the explicit time period of *Jinakālamālī* fits together with epigraphic evidence to prove that the personal name of Mahādharmarāja II, hitherto undiscovered,¹⁷¹ was something which could be Palicized as ‘*lideyya*’, that he was the Lideyya of *Jinakālamālī*’s Sumana episode, and that this enables us to modify the readings of certain

169. *Ibid*, p. 95.

170. *EHS* 1, p. 212; *EHS* 3, pp. 62-66. See also n. 119, above.

171. See p. 00 below.

inscriptions which have caused difficulty.

Prince Chand is also in disagreement with G/P's treatment of this period. In an earlier critical review he stated that the "contemporaries" of Lo'daiy discussed by G/P were really contemporaries of Ḷīdaiy, after 1347, and that the three important monks mentioned in *Mūlasāsanā* and *Jinakālamālī* were involved in Ḷīdaiy's ordination in 1361/62¹⁷². In *Guide* Prince Chand renews this criticism, referring briefly to the discrepancies in the chronicles which I noted above.¹⁷³ He also, in chapters 4 and 5, gives a long detailed version of the history of Buddhism in Siam up to the fourteenth century, and he describes Ḷīdaiy's ordination in detail as a major event. This history of Buddhism depends on a wide range of sources and more attention will be given to it below.

On the specific point of the arrival in Siam of certain important monks, Udumbara, Anomadassi, and Sumana, my critique above would imply the same conclusions as Prince Chand's but pending a full analysis of the chronicles, I would prefer to reserve judgement on the true factual details of these events and merely suggest how certain of the sources may or may not be used.

One final comment may be made on the only political aspect of Lo'daiy's reign which G/P felt was clear, the loss of the vast territories subordinate to RK.¹⁷⁴ Prince Chand has suggested that No. 1 was in fact a propaganda effort of Ḷīdaiy and I have emphasized that Epilogue II is very weak evidence for real conquests of RK at all. Thus those territories may never have been Lo'daiy's to lose. G/P themselves in one of their latest articles have given an entirely new meaning to Epilogue II, and one which loosens the ties between the outlying territories and Sukhothai to nothing more than vague vassalage; and already in EHS 10 they were hard put to show when or how certain places were lost.¹⁷⁵ We are thus no longer obliged to accept that Lo'daiy lost RK's realm, that his reign was therefore politically traumatic, or that later apparent expansions of Sukhothai were undertaken in a revisionist spirit.

The reign of Ḷīdaiy

Perhaps the only fact of Lo'daiy's life about which there is solid evidence is the approximate date of his death, 1347, at which time, according to No. 4, his son Ḷīdaiy marched from Sajjanālai to take the throne in Sukhothai. Thus began another reign for which, in spite of abundant

172. Chand, "Review July 1973", p. 175.

173. *Guide*, pp. 44-45.

174. *EHS* 10, pp. 25-47.

175. *Ibid*, and G/P, "On kingship", pp. 39, 41-43.

inscriptions, relatively little political detail is certain.¹⁷⁶ Since No. 4 says he moved from Sajjanālai to be crowned at Sukhothai in 1347, and that in 1361 he had “ruled and reigned at Śrī Sajjanālaya Sukhodaya for 22 years”, or since 1340, historians have reasonably inferred that he was appointed to some position such as *uparāj* in Sajjanālai in 1340 during the lifetime of his father.¹⁷⁷

Less reasonably, I would say, G/P assert that when Lo’daiy died “the throne was usurped by a man called Nua Nām Thaṃ, and that this accounts for the apparent violence of L̄idaiy’s move to Sukhothai.¹⁷⁸ I have already elaborated my view of Nua Nām Thaṃ, and here I would only add that it would seem unlikely for the list of spirits in No. 45, which was meant to be auspicious, and which was set up by the direct heir of Lo’daiy and L̄idaiy, to include an ancestor who had tried through usurpation to upset the accepted chain of succession to the detriment of the line responsible for the inscription. That there was some obstacle to L̄idaiy’s accession in Sukhothai seems clear from No. 4, but there is absolutely no way, in the realm of history rather than romance, to ascribe this difficulty to Nua Nām Thaṃ.

For Prince Chand, Nua Nām Thaṃ’s reign would have been even longer, from 1322 to 1340, on the grounds that Lo’daiy died at the earlier date. Prince Chand’s calculation, however, is based on the information of *Nān Nabamās* which I have already shown to be unacceptable.¹⁷⁹

According to G/P, one of the important political achievements of L̄idaiy was to reunify a large part of RK’s old realm which had been lost under Lo’daiy. I have noted above the difficulty of accepting the territorial statement of No. 1, and the lack of evidence for a genuine loss of Sukhothai territory, but G/P base their argument on a very badly damaged portion of No. 2 which seems to refer to divisions of the realm¹⁸⁰. Coedès, however, seemed to feel it concerned the administrative divisions of the kingdom, and G/P’s version is only achieved through massive emendations of the mutilated passages without even demonstrating that the emendations would fit properly into the destroyed portions of the stone.¹⁸¹ Given the extremely fragmentary character of the passage, either Coedès or G/P may well be correct in their general interpretation of the original meaning, but this is a case in which it would have been better to give only the literal translation of the remaining

176. *EHS* 11-1, p. 73.

177. In *EHS* 11-1, p. 71, G/P says he was definitely *uparāja*, but in the absence of that term from the inscriptions we may not be certain of it, or even that the institution of *uparāja* existed at Sukhothai.

178. *EHS* 11-1, pp. 72, 136.

179. *Guide*, pp. 16, 57; Vickery, “A note, etc,” (see n. 144 above).

180. No. 3, face II, lines 12-25; *EHS* 11-1, p. 91-92, 106-07.

181. Coedès, *Recueil*, pp. 78, 88; *EHS* 11-1, pp. 106-08, nn. 109, 114, 117, for ex.

sentence fragments and remain agnostic about the original. In particular I see no way to derive “acting independently” from face II, line 23,¹⁸² and in face II, line 25, the phrase **เปน เจา เปน ขุน** could just as well refer to Līdaiy himself and there would be no need to translate “forced [all] those lords and rulers [to submit]”¹⁸³

The only other political acts which may be understood from the inscriptions are a campaign against Nan and Prae and a seven-year sojourn in Phitsanulok which G/P plausibly place respectively in 1362 and between 1362 and 1370.¹⁸⁴

Again Prince Chand is in disagreement over basic details. For him Līdaiy’s sojourn in Phitsanulok was between the years 1347 and 1359 but was broken by visits to other localities. His reasoning is again based on acceptance of a passage in a “chronicle”, this time *PN*, and assimilation of the passage to information in the inscriptions; and the whole thing is related to his interpretation of a detail of art history discussed in his chapter 4.¹⁸⁵

According to *PN* the “Jinarāj trio”, the Buddha images Jinarāj, Jinasīh and Śāsdā at Phitsanulok, were all cast at the same time with some difficulty arising in the casting of the Jinarāj. Prince Chand accepts this story, believes the date to have been 1359, in the reign of Līdaiy, at which time No. 9 also seems to be telling of an image which caused difficulty, although the latter image would seem to have been in Śrī Sajjanālai.¹⁸⁶ Most art historians deny that the three statues were cast at the same time, and Griswold places all of them at later dates¹⁸⁷.

Whatever the dates of the images, the story of *PN* cannot be accepted,¹⁸⁸ the passage of No. 9 must be taken as referring to Śrī Sajjanālai, and, I would add, there is even some doubt that it refers to casting an image at all. The phrase is **บนนจุ พระ เจา เเรา** of which the last three terms mean ‘our lord’, possibly a royal person rather than an image, and the first, according to modern dictionaries, ‘to put into’, ‘to fill up with’, ‘to insert’.¹⁸⁹ Coedès, however, felt the passage referred to the setting up of an image, although he had some doubts about **บนนจุ**;¹⁹⁰ and it is not the term generally used in

182. *EHS* 11-1, pp. 92, 107 and 107, n. 114.

183. *EHS* 11-1, pp. 92, 108 and 108, n. 117. I have enclosed G/P’s emendations in brackets.

184. *EHS* 11-2, pp. 103-04, 109.

185. *Guide*, pp. 26, 31, 59; see *PN*, pp. 24-27.

186. *EHS* 12, p. 105.

187. Griswold, *Towards*, pp. 54-55, 58: Griswold, “Notes and comments”, pp. 162-163.

188. See Griswold, “Thoughts on a centenary”, which also cites Prince Damrong’s opinion (n. 151, above).

189. George Bradley McFarland, *Thai-English Dictionary*, p. 486 (modern spelling °ĀĀ°Ø)

190. Coedès, *Recueil*, p. 136, line 15, ‘mettre en place (?)’.

passages clearly relating to casting. This is the type of case, mentioned at the beginning of this review, when those proposing an unusual meaning for a term must give explicit evidence that the new meaning is justified. Still another usage of **บนนจุ**, spelled **บนนจุ**, and not included in all the standard dictionaries, but which can easily be attested in the reliquaries of modern Thai temples, is the ‘deposit of ashes’ (**บนนจุ อัฐ**) after cremation; and it is worth noting that in Khmer, which is recognized as relevant for the interpretation of Sukhothai inscriptions, the term is used particularly for the deposit of the ashes of royalty.¹⁹¹

As in the case of Lo’daiy, there seems to be much more information about Līdaiy’s religious activities, in particular his ordination in 1361. The basic details of this event are found in inscriptions;¹⁹² and other details, including identification of the monks involved, have been derived from the same chronicle passages used by G/P for the religious activities of Lo’daiy and concerning which Prince Chand is in disagreement.

Nearly all of Prince Chand’s chapters 4 and 5 are devoted to this subject, and while I agree that the interpretation of G/P is much too arbitrary, and that Prince Chand is probably correct about the time period to which the stories of *Jinakālamālī* and *Mūlasāsanā* refer, his own further interpretations are just as arbitrary as those of G/P. I shall not try to sort out these arguments in detail, for it would take an enormous amount of space, and because I believe the first step must be a thorough critical study of the chronicles concerned.

Prince Chand’s chapter 4 is entitled “Biographies of three Sukhothai monks”, but it is less about three monks than about interpretations of certain inscriptions that differ from those of G/P without making the differences explicit. Four monks are mentioned with equal emphasis, and I am not sure which are the three of the title.

Among the original interpretations are the question of the Jinarāj trio, already discussed, the identity of some of the names of No. 45, and the date of Līdaiy’s death, which will be discussed below.

Chapter 5 is a whole new history of Buddhism in Siam, the purpose of which is to show that Theravada Buddhism was introduced there by Līdaiy at the time of his ordination in 1361, and that Buddhism in Siam before that time had been non-Theravada.

This argument leads Prince Chand through almost the whole religious history of central and peninsular Siam, Burma, and Ceylon as well as into sources which cannot be discussed here due to lack of space and insufficient

¹⁹¹ The use of **บนนจุ** (**บนนจุ**, **ปจจุ**) for the deposit of relics in the early fifteenth century is attested in inscriptions nos. 50 and 51, from Chainat. See *Śilā caru’k* III, pp. 89, 90 192. *EHS* 11-1, pp. 119-176.

competence of the reviewer in the fields of Ceylonese and religious history. Nevertheless, a few definite remarks must be made. Much of Prince Chand's argument is based on some of the last writings of Senarat Paranavitana in his *Ceylon and Malaysia*, and it is now known that the new 'discoveries' revealed in that work were an elaborate hoax. That is, certain inscriptions, in small script between the lines of other inscriptions, seen only by the eyes of S. Paranavitana, and which linked the history of Ceylon and the peninsula in unsuspected ways, are now admitted to be nonexistent.¹⁹³

Thus all of Prince Chand's interpretation which depends on that part of Paranavitana's work, and in particular, all statements about peninsular origins of certain branches of Ceylonese royalty, must be rejected,¹⁹⁴ and critical judgement of Prince Chand's interpretation must await a complete reworking of the entire body of evidence.

Prince Chand and G/P also have very divergent ideas about Līdaiy's death, and both interpretations are based on secondary sources, there being no clear statement in any inscription.

In an article written in 1966 Prasert suggested Līdaiy had died in 1368 on the basis of *Jinakālamālī*, which says a certain Dhammarāja died before Paramaraja's campaign against Sukhothai in 1371, and statements in Chinese sources about the submission of Hsien to Lo-hu by 1368.¹⁹⁵ We know now of course that the latter information is irrelevant for Sukhothai history (see note 116 above)

Two years later Prasert took up the same question again and decided Līdaiy's death had occurred at some time between 1368 and 1373. He provided no further reason for the earlier limit, but based the later date on No. 106, which seems to indicate that Līdaiy was still alive in the 1370s but had died before 1377.¹⁹⁶ Apparently Prasert at the time discounted the value of *Jinakālamālī's* testimony.

In *Towards* Griswold interpreted the *Jinakālamālī* evidence to mean simply that Līdaiy had died after Rāmādhipatī and he added that there was evidence for believing Līdaiy had survived until 1374. This latter evidence seems to be oral tradition about what was still visible on No. 102 in the days

193. See W.H. McLeod, "Inter-linear inscriptions in Sri Lanka", *South Asia* III (August 1973), University of Western Australia Press, pp. 105-06. This information has been part of the 'oral tradition' of Southeast Asian specialists for some time. O.W. Wolters, *The Fall of Srivijaya in Malay History*, chap. VI, n. 56 shows he was aware of the dubious character of Paranavitana's work as early as 1970, although he made use of it to illustrate other points in chap. I, n. 7 and chap. VI, n. 43.

194. *Guide*, pp. 39-40.

195. Prasert, *op cit*, p. 37. This assumes that the statement in *Jinakālamālī* refers to the LP entry of 733/1371 concerning an Ayutthayan campaign against the north.

196. Prasert, *op cit*, p. 26. The reason for the choice of 1373 is still not clear; and see below, my treatment of No. CVI.

of King Vajiravudh, and of course this cannot be accepted¹⁹⁷.

Since a certain passage of *Jinakālamālī* has been interpreted in two different ways by G/P, it is useful to note precisely what that passage contains. First, it is part of one of the inserted stories of miraculous Buddha images which G/P more than once admit to be of dubious value.¹⁹⁸ Moreover, the Sukhothai genealogy is manifestly jumbled, being given there as Rocaraja (Braḥ Ruoñ), Rāmarāja (RK), Pālarāja (Bān Mo’añ), Udakajotthatarāja (Nām Tham), and Lideyyarāja (Līdaiy). RK and Bān Mo’añ are reversed and Lo’daiy is not mentioned at all. It is thus difficult to attribute any exactitude to any of the reigns as found in *Jinakālamālī*. Finally, the campaign of Paramarāja (Vattitejo) to which Prasert alluded, does not correspond precisely to any detail of *LP*, and one may wonder whether it is an event not recorded there, or pure invention made necessary by the story of the Sihing Buddha.¹⁹⁹

G/P’s final statement on Līdaiy’s death is that it occurred “between 1370, when Paramarāja seized the throne of Ayudhya, and 1375, when Paramarāja took Sòn Gvè.²⁰⁰ This interpretation is based on *LP* plus *Jinakālamālī*, but even here there are certain assumptions which merit discussion. Neither *Jinakālamālī* nor *LP* mention Sòn Gvè. *LP* speaks of Phitsanulok, which in fact might suggest that this entry of *LP* is an inaccurate late interpolation since it seems certain that in 1375 the name ‘Phitsanulok’ had not yet replaced the older name Sòn Gvè meaning ‘two branches’ of the river;²⁰¹ and *Jinakālamālī* gives the location as Jayanāda (Chainat).

G/P lose no opportunity to assert that Jayanāda in this context really means Sòn Gvè/Phitsanulok²⁰², even though this forces them to assume serious errors in *Jinakālamālī* and *Sihingānidāna*, which they otherwise try to use to support their interpretation. Thus, “in both *Sihingānidāna* and *Jinakālamālī* Rāmādhipatī seizes Sòn Gvè But it would be out of keeping with everything we know about Rāmādhipatī’s policy for him to lay hold of the city.”²⁰³ In fact, *Jinakālamālī* doesn’t say Sòn Gvè, but Jayanāda, while *Sihingānidāna* says Dvisakhanagara, ‘two branch city’ or ‘confluence city’, which fits Chainat just as well as Phitsanulok (but which

197. Griswold, *Towards*, p. 39, n. 108; *EHS* 7, pp. 158-60.

198. See notes 119 and 161 above.

199. Coedès, “Documents”, pp. 99-100.

200. *EHS* 11-2, p. 109.

201. See *LP*, entry for cula 737. The earliest date for the usage of ‘Phitsanulok’ has not yet been determined.

202. *EHS* 3, p. 63; *EHS* 11-2, p. 108; G/P, “*Yuan phai*”, pp. 143-144; G/P, “On kingship”, p. 64, n. 22.

203. *EHS* 11-2, p. 108.

also translates *sòñ gvè*), and a seizure of Chainat could easily be presumed to fit Rāmādhīpatī's policy even if a seizure of Phitsanulok could not²⁰⁴. The only apparently good evidence that Jayanāda ever really meant Phitsanulok is found in *Yuan Phai*, concerning events nearly 100 years later,²⁰⁵ but there is very good nearly contemporary proof in No. 48, dated 1408, that modern Chainat (Jayanāda) was known by a Jaya-type name, 'Jayasthān'.²⁰⁶

I think it should be clear that all of the relevant evidence is very complex and that all of the various dates put forward by G/P for Līdaiy's death are little more than speculation.

Prince Chand's proposal is entirely different. He ignores the statements of *Jinakālamālī* and *Sihingānidāna*, and at one time asserted that Līdaiy died sometime between 1378 and 1388 and, more precisely, that he must have died shortly before the writing of No. 94 in 1384.²⁰⁷ In *Guide* Prince Chand said Līdaiy's death was between 1378 and 1384, and then pinpointed it to 1379.²⁰⁸

His only evidence for this supposition appears to be No. 102, containing the date 1379 and indicating that a Mahādharmarāja, presumably Līdaiy, was already dead. Prince Chand had to settle on the latest date permitted by this inscription because of his conviction that the Mahādharmarāja who, according to *LP*, surrendered to Ayutthaya in 1378 was Līdaiy, whereas for G/P that Mahādharmarāja was Mahādharmarāja II.²⁰⁹

Thus the end of Līdaiy's reign, for which there is no unequivocal evidence anywhere, depends for both Prince Chand and G/P on their interpretations of the reign of Mahādharmarāja II and it, as I will demonstrate, depends on their respective views of the evidence concerning Sai Līdaiy. For G/P, Mahādharmarāja II, of unknown personal name, was the son and successor of Līdaiy, was the king who surrendered to Ayutthaya in 1378, and was the father of Sai Līdaiy', the next ruler of Sukhothai.²¹⁰ Prince Chand, however, while admitting that there was a prince who

204. I would deny that we know anything about 'Rāmādhīpatī's policy'. This seems to derive from the bi-polar policy discussed above.

205. YP, p. 00. Even then one could argue against the identification by pointing out that the Chiang Mai attack on Jayanāda in YP corresponds to a CMC passage which says they went as far as Pāk Yom, generally accepted as meaning Nakhon Sawan, and which is only about 30 miles north of Chainat. See G/P, "*Yuan phai*", p. 143; and Notton, p. 113. Inscription No. XII, depending on how it is translated, is also relevant to this question. See Coedès, *Receuil*, pp. 151-56; PCSA, p. 27; and my review of PCSA pp. 321-22.

206. *Śilā cāru'k* III, p. 78. There are also problems with YP itself. See note on it.

207. Chand, "Review Jan. 1973", pp. 296-97.

208. *Guide*, pp. 33, 49, 67, 71, 76.

209. *Guide*, pp. 32, 33, 67; *EHS* 1, pp. 210-11.

210. *EHS* 1, pp. 210-11, 214.

corresponds to G/P's Mahādharmarāja II, denies that he was ever king or that he was father of Sai L̄idaiy.²¹¹ In view of such basic disagreement let us turn directly to the evidence.

Mahādharmarāja II

Mahādharmarāja II is indeed the most mysterious character among the indentifiable figures of Sukhothai history. He is mentioned, in the view of G/P, in only one inscription, and that posthumously²¹², no monument or statue may be ascribed to him²¹³, and except for his surrender to Ayutthaya in 1378, which is after all only a conjecture, nothing is known of his political or religious activities. It would seem that his reign is nothing more than a necessary inference to fill the space between L̄idaiy's death and the reign of Sai L̄idaiy which could not have begun, according to G/P, until circa 1400. G/P have given the reign of Mahādharmarāja II as 1368/1374 to around 1399, adding that the only certainties are that he was alive in 1390 and dead by 1399 when No. 93 was written²¹⁴. It is presumed that he was king in 1393 when No. 45 was set up, since he has not been recognized among the 'ancestral spirits', but since Sai L̄idaiy is apparently the acting ruler, assuming No. 64 to be coeval with No. 45, G/P are forced to speculate about a temporary retirement of Mahādharmarāja II,²¹⁵

Since all these conjectures about Mahādharmarāja II are based on inscriptions featuring, or in the reign of, Sai L̄idaiy, it is necessary to review that evidence before discussing in detail the interpretations of G/P or Prince Chand regarding Mahādharmarāja II.

The Sai L̄idaiy period

There are more inscriptions available for the Sai L̄idaiy period than for any other segment of Sukhothai history, but I am going to contend that G/P's historical method, setting up a scenario on the basis of heterogenous chronicles and then fitting the inscriptions into it, has distorted the meaning of the inscriptions, the details of Sai L̄idaiy's reign, and the reign of his predecessor. Even the name, 'Sai L̄idaiy', which I shall continue to use because of its familiarity, is foreign to the inscriptions, which call him Mahādharmarāja [implicitly III], L̄idaiy, Lideyya, and, I will argue, by other titles as well. 'Sai L̄idaiy' is the name found in the Chiang Mai chronicle for a king of Sukhothai who apparently belongs to the appropriate time period and G/P have decided to adopt it as his personal name.

211. *Guide*, pp. 69, 72, 73, 76.

212. No. 93, *EHS* 2, pp. 37, 44.

213. Griswold, *Towards*, p. 49.

214. *EHS* 2, p. 37. The date '1390' is based on their interpretation of No. 93, face II.

215. *EHS* 1, pp. 217-18; *EHS* 3, pp. 66-67.

G/P's background story for this period of Sukhothai history, based on the chronicles, is the story of Sukhothai-Ayutthaya relations and the gradual expansion of the latter to absorb the former. This expansion of Ayutthaya is of course the major development of Thai history throughout several centuries, and, strangely, there has been little effort to explain it. It has apparently been viewed as the natural, inevitable order of events, and it has not been thought necessary to ask why Sukhothai failed to expand and form a modern Siam controlled from the old inland capital.²¹⁶

A sketchy, and probably true, outline of this story for the fourteenth-fifteenth centuries is found in the laconic entries of *LP*. G/P have attempted to put some flesh on the bare bones of this outline through combining *LP* with other chronicles such as *Jinakālamāli* and those of Chiang Mai (*CMC*) and Nan (*NC*), and also by bringing the content of the Sukhothai inscriptions, particularly those for the period of Sai Līdaiy, into the picture of Ayutthayan expansion.

G/P's story is found in *EHS* 1, the main purpose of which was to present Sukhothai's "Declaration of Independence" in 1400 from a first Ayutthayan conquest (No. 46), and an Ayutthayan reaction 1417 (No. 49). Their decision about the purpose of the two inscriptions has forced them to load the evidence in favour of their interpretations of several other inscriptions which were only presented in later *EHS*, but the conclusions from which form part of the story of *EHS* 1.²¹⁷

Since the main interest in the present review is the epigraphy, the chronicles cannot be discussed in detail, but it is useful to demonstrate some of the more tendentious points of Griswold's and Prasert's synthesis in *EHS* 1 before going on to the inscriptions themselves.

G/P make much of the submission of Mahādharmarāja II to Paramarāja of Ayutthaya in 1378, an event mentioned **without any additional detail** in *LP*, and only there²¹⁸. They add gratuitously that Paramarāja "made him swear allegiance and sent him back to rule in Sukhothai as his vassal", and in later *EHS* they use Mahādharmarāja's presumed fidelity to his oath to account for certain mysterious features of the

216. A few hints and suggestions for the resolution of this problem may be found in O.W. Wolters, *The Fall of Śrīvijaya*, pp. 66-67; Charnvit Kasetsiri, *The Rise of Ayudhya*; Vickery, "The 2/k.125 Fragment", pp. 79-80; Vickery, "Cambodia after Angkor", pp. 222-23, 509-22; [*but the most convincing treatment is Chris Baker, in his "Ayutthaya Rising: From the Land or From the Sea", noting pertinently that the union of inland central 'Thailand' and Ayutthaya came about under Sukhothai and Phitsanulok after the 1569 war with Burma. See text with note 230a, in Vickery, "A New Tāṃnān About Ayudhya"[*] 217. *EHS* 1, pp. 213-220.

218. *EHS* 1, p. 210: *LP*, entry for *cula* 740. Moreover, it depends on an assumption, probably legitimate in this case, that the title 'mahārāja', usually meaning kings of Chiang Mai, has been substituted for 'mahādharmarāja'.

inscriptions, such as Mahādharmarāja's apparent absence from the action of Nos. 45 and 64 at a time when he was presumably ruler of Sukhothai.²¹⁹

It looks as though G/P are imputing an idealized version of European feudal culture to fourteenth-century Thailand; and besides the generally risky nature of assumptions which go so far beyond the evidence, it is now commonly accepted by historians that one may not project the attitudes or political morality of one period onto the actors of another, particularly when the culture of the latter is different or largely unknown. We really have no evidence that Mahādharmarāja II swore an oath at all, and even if he did, the political culture of later centuries in the Thai area, which is a more relevant standard for comparison than European practices, permitted weaker countries to form multiple liens of vassalage and in such a system it would have been possible for Mahādharmarāja II to swear an oath to Ayutthaya and at the same time enter into a treaty with Nan²²⁰.

Along the same line of reasoning one may also take issue with G/P's remarks that in 1390 the title 'Samtec Mahādharmarājādhirāja' of the Sukhothai king was "reserved for a sovereign monarch with vassals of his own", whereas in 1426, when Sukhothai seems clearly to have been under Ayutthayan suzerainty, "evidently *rājādhirāja* by that time no longer denoted a sovereign monarch and was hardly more than part of a proper name".²²¹ This violates the criterion of consistency which I evoked earlier, for it is unlikely that the political significance, if any, of '*rājādhirāja*' changed so drastically in 36 years, and it would be better to conclude that the term is not diagnostic at all with respect to vassal/suzerain status. Again, later Thai usage shows that Bangkok vassals were given titles that sounded fully royal and the Cambodian King Ang Eng, 1779-1796, perhaps one of the weakest vassals of all time, was granted one of the longest and most impressive royal titles on record²²².

For the events following the surrender of Mahādharmarāja II, G/P have combined *LP*, *Jinakālamālī* and *CMC* in ingenious ways. The story of difficulties in Kamphaeng Phet, followed by the machinations of a certain Mahībrahma of Chiang Rai and warfare among Ayutthaya, Sukhothai and Chiang Mai occurs both in *Jinakālamālī* and *CMC*, but in *Jinakālamālī* it all comes before 1371 while in *CMC* it is inserted after 1385. Usually G/P accept the chronology of *Jinakālamālī* as superior, but for some reason, perhaps *LP*'s entry of 748/1386 which 'must be' equivalent to one of the

219. *EHS* 1, pp. 216-18; *EHS* 3, pp. 66-67.

220. *Pace* G/P, *EHS* 1, pp. 217-18. On southeast Asian vassalage in later times, see Steinberg *et al*, *In Search of South-East Asia*, pp. 64, 66-67, 120-21.

221. *EHS* 1, pp. 216, 241.

222. Michael Vickery, "Accounts of the Appointments of Vassal Princes in the First Reign of the Bangkok Period", p. 7.

episodes of the *CMC* story, they have opted for *CMC*'s chronology in this section. This is another example of inconsistency in the use of evidence and requires at the very least a full justificatory argument²²³.

Furthermore, the activities of Mahābrahma, both in *Jinakālamālī* and *CMC*, are part of the inserted story of the Sihing Buddha, are centered around efforts to acquire it for the northern kingdom, and may therefore be fiction, at whatever date they are placed²²⁴. G/P are not even faithful to the sources they choose. In summarizing the complex relations among Ayutthaya, Sukhothai and Chiang Mai, they say “Mahādharmarāja II felt bound by his vassal's oath not to take any action against Paramarāja”, ignoring *CMC*'s explicit statement that the king of Sukhothai attempted to conquer Ayutthaya²²⁵. At the present stage of investigation it would be best to frankly recognize that the chronicles are at times contradictory and may not yet be used in synthesis.

G/P's story goes on with events in Sukhothai in 1390, neglecting to warn the reader that '1390' is based on a controversial interpretation of No. 93, not presented until *EHS* 2;²²⁶ and all through this background story we find remarks about the personalities and opinions of individuals which derive from the bi-polar theory discussed earlier.

This is as far as I intend to go in the analysis of G/P's use of the chronicles, the main purpose being to show that their synthesis is not firmly enough grounded to justify any attempt to interpret the inscriptions into it if the meaning of the latter in that respect is not absolutely clear.

Returning to the inscriptions the first fact to note about the life of Sai Līdaiy is that according to No. 9 he was grandson of Līdaiy. Or, more precisely, at a date between 1388 and 1406 No. 9 refers to Mahādharmarāja 'the grandson' and at the dates 1359 and 1361 Mahādharmarāja 'the grandfather'. There is no reference to the intervening generation presumably represented by Mahādharmarāja II²²⁷. The importance of this unequivocal information of No. 9 is that there must have been a generation between Līdaiy and Sai Līdaiy, and any interpretation of other inscriptions which would tend to make Sai Līdaiy son of Līdaiy is to be rejected. It does not prove though that the individual of the intervening generation ever ruled.

The earliest inscription which seems to refer to Sai Līdaiy is No. 102 (*EHS* 7), dated explicitly 1379/80. At that time, according to G/P, Mahādharmarāja II was king; but in Prince Chand's view it was Līdaiy. The protagonist of the inscription is a certain *pā nān gām*, 'Aunt Princess Gām',

223. Coedès, “Documents”, pp. 100-101; Notton, pp. 86-89; *EHS* 1, pp. 212-13.

224. See notes 119 and 161 above.

225. *EHS* 1, p. 212; Notton, p. 89.

226. *EHS* 1, pp. 213-15; *EHS* 2, pp. 50-51, n. 55; and see below on No. XCIII.

227. *EHS* 12, pp. 96, 104, 106, 109.

who was responsible for the construction of a temple and erection of the inscription. Since nearly all Sukhothai inscriptions are concerned with royal family affairs, it is safe to assume along with G/P that she was the aunt of a Sukhothai ruler,²²⁸ or, but this has not been suggested by G/P, aunt of some other royal prince figuring in the inscription.

At two points in the inscription Aunt Gāṃ is mentioned together with the ruler's uncle (*luñ khun* [interpreted as 'uncle' (*luñ*) of the *khun* (the ruler)]), which would be further evidence that she is aunt of that *khun*, whom G/P identify with Mahādharmarāja II, the presumed king of Sukhothai. Of course '*khun*' has had different degrees of meaning, all the way from full king down to low-level chief, and if there were another more plausible candidate, '*khun*' in this context would not necessarily be Mahādharmarāja II. It would also be legitimate to interpret *luñ khun* as 'uncle *khun*', that is the *khun* who was uncle.

Immediately after the paragraph announcing the object of the inscription there is mention that the monastery had been falling into ruin, then a lacuna, and then the phrase, *pun* ('merit') *bī āy* ('eldest brother') *dān* ('sir') *braḥ śrī auras* ('the royal son') *cau mo 'aṅ sukhodai nī* ('lord of this 'mo 'aṅ Sukhothai'). Because of the lacuna it is not clear what *pun* refers to. G/P say it could be either 'merit to' or 'meritorious work of' the royal son. They opt for the former, implying that he is dead. They also inexplicably consider *braḥ śrī* to be his personal name, rather than merely an appropriate title preceding mention of any royal personage²²⁹. Thus the 'royal son' for G/P becomes a son of Mahādharmarāja I [Līdaiy], or even of Lo'daiy.²³⁰

In my opinion the first important consideration is that use of *rāj auras*, 'royal son', implies that the father, presumably the reigning king, is still alive, or only very recently dead. Since every male is also a son, the only reason to emphasize this quality in a title is in opposition or reference to the father, and I believe it is very unusual to find mention in chronicles or inscriptions of a prince specifically designated 'son' as part of his title except when his father was still alive or very recently dead. 'Royal son' in such a context may be taken as equivalent to 'crown prince'. The royal son here would then be son of the reigning king, for G/P Mahādharmarāja II. Another point of interest is that the royal son is described, [in their translation], as 'lord of this *mo 'aṅ* Sukhothai', which, without unnecessary interpretation, would seem to mean 'living lord of this *mo 'aṅ* Sukhothai at the present

228. *EHS* 7, p. 162.

229. *EHS* 7, p. 163. They accept *śrī* as simply part of a title in *brañā śrī nāv nām thaṃ* (*EHS* 10, pp. 108, 112: and *EHS* 8, p. 203).

²³⁰ Note that in "Piltdown 3", page 00, discussing the anomalous use of *nī* 'this' in inscription No. 1, I proposed that the phrase under discussion here could be translated not as "lord of this Mōaṅ Sukhodai", but "this lord of Mōaṅ Sukhodai"

time'. He would thus have been governing for his father who was either absent or present but retired from an active role in government.

Of course the phrase *bī āy* remains to be explained, but because of the lacuna only speculation is possible. Any restoration proposed must indicate the precise number of characters which would fit into the lacuna and the precise Thai words to be inserted. The *bī āy* could be an entirely different person, and because of another lacuna at the beginning of line 24, we could also assume that the original intention was 'the royal son, Princess Aunt Gāṃ and the ruler's uncle assigned', etc.

Finally, 'the ruler' (*Khun*) would most plausibly be, not Mahādharmarāja II, nor Līdaiy, but the 'royal son', 'lord of this *moa'ñi* Sukhothai' of the preceding line, and Aunt Gāṃ would be his own aunt, not the aunt of his father.²³¹ As son of Mahādharmarāja II, we might speculate further that the 'royal son', who is also *khun*, is in fact the future Sai Līdaiy, something I shall try to demonstrate below.

We should also note, for purposes of interpreting another inscription later on, that the 'overseer' of the construction was a certain *nāy* named *āy ind*.²³²

With respect to Griswold's and Prasert's treatment of this inscription a few more remarks are required. (a) The supposed original opening date, due to the circumstances of the stone's discovery, may not be accepted as more than a hypothesis, and may not be used in further reconstruction, such as the date 1375 for the beginning of restoration work on the temple²³³. (b) It is not good reasoning to suggest that Mahādharmarāja II was called *khun* rather than something more elaborate just because he was at the time a vassal of Ayutthaya. I have shown that the *khun* was probably not Mahādharmarāja II, and earlier I adduced some evidence that royal titles are not significant for the indication of vassal or suzerain status; and G/P themselves in another context showed that another supposedly vassal ruler had elaborate titles usually associated with an independent monarch²³⁴. (c) All the hypotheses about relationships among the royal family on their pages 162-64 are untenable in so far as they are based on No.102.

Prince Chand's most serious criticism of *EHS 7* concerned the translation of the phrase, *pun bī āy*, etc.²³⁵ Prince Chand considers that at that date Līdaiy was king, but that Sai Līdaiy was not his son, and he thus had to attempt a translation which would fit that picture. According to Prince Chand, *bī āy dān braḥ śrī rāj auras* means, 'the royal son of the

231. *EHS 7*, p. 166, lines 22-24, and p. 168.

232. *EHS 7*, p. 166, line 24, and p. 168; and see below, n. 261.

233. *EHS 7*, pp. 158-59.

234. *EHS 7*, pp. 161-62; *EHS 1*, p. 241.

235. Chand, "Review Jan. 1973", pp. 292-94; *EHS 7*, p. 166, line 23, and p. 168.

eldest brother’, and he insists that *bī āy* and *dān braḥ śrī rāj auras* are two separate persons. I have already indicated that this is possible, if not certain, and Prince Chand, like myself, believes the royal son to be Sai Līdaiy; but with all due respect I venture to suggest that as a translation ‘royal son of the eldest brother’ violates Thai syntax. If the phrase indicates two persons it should mean ‘eldest brother of the royal son. It transpires, however, that Prince Chand did not mean it as a direct translation, but as an interpretation, after hypothetical restoration of the lacuna.²³⁶ Then it becomes, “... the eldest brother *bī āy* [died], his royal son (*dān braḥ śrī rāj auras*) [became] king...”. However, Prince Chand’s, like Griswold’s and Prasert’s, *ad hoc* restorations may not be taken as anything but more or less plausible hypotheses, and they are all of the type best avoided by historians. I am also skeptical of Prince Chand’s identification of *bī āy* as “Poh Loe Thai”, which depends on his reading of No. 45, but I shall discuss this elsewhere.

A second point separating Prince Chand from G/P is the phrase, (น)งไคเสยราช, which would seem to mean, just as Prince Chand says, ‘the queen came to the throne,’ although G/P have reinterpreted it as ‘[whatever kings] succeed to the throne’, an interpretation which I agree with Prince Chand to be entirely inadmissible. Since the phrase is in a passage full of lacunae, I would prefer to dismiss it as incomprehensible and not interpretable. Prince Chand’s interpretation is that Līdaiy’s queen had become regent for Sai Līdaiy, but this does not come forth from the remainder of the phrase, ไสใหญ่.....”, which G/P translate, plausibly, as ‘may they [or he, she] uphold ...’, the type of injunction with which many inscriptions conclude. I thus feel that both Griswold’s and Prasert’s and Prince Chand’s interpretations of this phrase are dubious, but I intend to show later that Prince Chand is probably correct about unusual circumstances during the reign of Sai Līdaiy, although not necessarily a regency under Līdaiy’s queen.

The preliminary conclusions to draw from this inscription are that Sai Līdaiy in 1379/80 was already acknowledged as either king, or governor of the city of Sukhothai during the lifetime of his father or shortly after the latter’s death. One might wonder if Sai Līdaiy were not too young for such a position, since according to the interpretations of Prince Chand and myself he would have been only 12 years old,²³⁷ while for G/P he was perhaps even younger,²³⁸ Age 12 for a crown prince, or viceroy, should be acceptable for G/P since they postulate elsewhere that Rāmeśvara was sent as viceroy to

236. Chand, “Review Jan. 1973”, p. 294; *Guide*, p. 32.

237. See below, discussion of No. 93 of Wat Asokaram.

238. I have found no precise statement by G/P on the birth of Sai Līdaiy, but they at least believe he was old enough to father a son in 1390-91, the year when, according to No. 12, Mahādharmarāja IV was born.

Phitsanulok at the age of 7 in 1438; and in nearly contemporary Europe the future Henry VIII of England was appointed Lord Lieutenant of Ireland at the age of 3, and his brother Arthur was made a royal commissioner dealing with the government of Wales at the age of 6.²³⁹

EHS 8 concerns No. 106, the next piece of evidence, which was erected by a person whom G/P call ‘Foster-Father Sai Tām, with ‘foster-father’ their translation of **ພໍ່**, literally ‘breast-father’, which seems absurd (or was Sukhothai prefiguring 20th-century Pattaya), but is justified by them on the grounds that he was husband of ‘breast-mother’ (**ແມ່**) Det, who could well have been a wet-nurse or foster-mother. An alternative interpretation, favoured by Prince Chand, is that **ພໍ່** (*banam/’phanom*, ‘mountain’), was simply a proper name.²⁴⁰

It is my contention that G/P have missed important features of the structure of this inscription and that properly understood it helps build up a picture of the reign of Sai Līdaiy in which several inscriptions fall into place.

As G/P have recognized in their footnotes 33 and 40, the text of the inscription is poorly organized and full of digressions. It opens with a B.E. date equivalent to 1384, which G/P take to be the date of its execution; and at that time the author had some reason to think of his future existences after death. There follow (a) a flashback to events of 1361, (b) mention (lines 20-28) of the death of a Mahādharmarāja, apparently Līdaiy, which G/P believe occurred in 1368-74, and the death of his queen, (c) a note (lines 28-32) that a certain *brañā* had taken the author out of monkhood to serve the state, (d) in lines 32-42 description of religious works performed by the author in a snake year which G/P consider must be 1377, earlier than 1384, although they note the unusually long time between the beginning of the work and its dedication, (e) in face II, lines 12-18 another mention of the *brañā*, (f) in face II, lines 19-31, more religious works, (g) in face II, lines 31-37, a digression, and (h) a continuing description of religious works up to the end of the inscription.

G/P recognize that the section of face II, lines 12-19, is a digression which should belong with the first mention of the *brañā*. They also recognize that lines II/11-12 and II/19 are nearly identical and should be connected. Thus the story of the author’s religious works, in a Snake Year, continues to the end of face II and is later than the episode of the *brañā*. If that is so, then the Snake Year is probably the latest date in the text and would be 1389. That 1384 is not the real final date of the inscription would also seem

239. Griswold, “Yudhiṣṭhira”, pp. 215, 221; G/P, “*Yuan phai*”, p. 130; *EHS* 1, p. 242; *The Oxford History of England, VII, The Earlier Tudors, 1485-1558*, pp. 651, 204.

240. *EHS* 8, p. 189; Chand, “Review Jan. 1973”, p. 298. It is a plausible loanword from Khmer.

confirmed by the final section, clearly the conclusion of its story (face III, lines 12-35),²⁴¹ for there Banam Sai Tām is abbot of the monastery dedicated in a Snake Year, whereas in 1384 he was still a layman contemplating reentry into the monkhood.

I would therefore like to suggest the following absolute chronological slots for the different sections of the inscription: face I/10-20, 1361; I/20-28, 1370s; I/28-32 and II/12-19, between the death of Līdaiy and 1384; I/3-10, 1384; I/32-42, Snake Year 1389; II/remainder and all of face III, after 1389, perhaps even several years later.

The most interesting remark about contemporary events contained in the inscription is the statement that Banam Sai Tām was taken out of the monkhood by a certain Brañā Debāhurāja, “to help build the kingdom”. For G/P the title *brañā* ‘certainly means a king’, and according to their reign sequence it must have been Mahādharmarāja II, even though the title is not attested elsewhere²⁴². Proof one way or another is impossible, but I am going to argue that there were internal difficulties in Sukhothai at that time, possibly a regency during the early years of Sai Līdaiy, and that Brañā Debāhurāja might have been an otherwise unknown member of the nobility active in state affairs in the reign of a very young and weak king.

Prince Chand is convinced that there indeed was a regency government, that Brañā Debāhurāja was a member, and that he succeeded Līdaiy’s Mahā Devī as chief regent around 1381²⁴³. As I have pointed out above, his belief that Mahā Devī was regent is based on the last line of No. 102, and is untenable, and thus there can be no conclusion about a change of regent in 1381, even though I agree with Prince Chand about the possibility of a regency government.

A further intriguing detail is that in another connection Banam Sai Tām consecrated merit to a certain Cau Brahm Jai, a name also mentioned in *Jinakālamālī*. This has led G/P into complex speculations. I find it impossible on the evidence available to account for Cau Brahm Jai, and prefer to remain entirely agnostic on the matter²⁴⁴.

In his remarks about this period Prince Chand is also in error about a date in No. 9 at which time Sai Līdaiy appears to be full king. He says it was 1388, but the correct date in that inscription was 768/1406, at which time Mahādharmarājādhirāj, Śrī Rājamāta (his mother), and an unidentified *pū brahñā* settled a monastic dispute.²⁴⁵

The next relevant inscription is No. 94, described by G/P in *EHS* 11-2,

241. *EHS* 8, p. 208.

242. *EHS* 8, p. 193.

243. Chand, “Review Jan. 1973”, p. 295.

244. *EHS* 8, p. 207, Prasert, *op cit*, pp. 63-64; Coedès, “Documents”, p. 100.

245. Chand, “Review, Jan. 1973”, p. 296; *EHS* 12, pp. 109-113.

pp. 124-25, and also noted by Prince Chand.²⁴⁶ It is a very short gold plate text, dated 746/1384, in which a Saṅgarāja, who had been teacher (*grū*) of a certain Mahādharmarāja has made a stupa for the relics, *braḥ dhātu* of that same Mahādharmarāja, which implies, Prince Chand thinks, that the Mahādharmarāja in question was either Līdaiy (Griswold's and Prasert's choice) or Mahādharmarāja II, recently dead. I intend to refer to this again below.

Prince Chand also cites No. 95, which mentions both a Mahādharmarāja, “the grandfather” (*phū pū*), and a Braḥ Mahādharmarāja.²⁴⁷ He considers it important in proving his contention that Sai Līdaiy succeeded Līdaiy, but since the inscription is undated I would say it cannot be integrated into the story.

There seems to be full agreement among G/P and Chand on the basic meaning of the next evidence, Nos. 45 and 64.²⁴⁸ The two inscriptions together are considered to be separate records of a treaty between Sukhothai and Nan in 1393. This is based on several assumptions which deserve to be made explicit, although my insistence on this does not necessarily mean that I find the assumptions impermissible.

The first assumption is that No. 64, although undated, is of the same time period as No. 45 and refers to the same events. So far there is no serious objection to this assumption. The second assumption is that the Līdaiy named in 64, but not in 45, as representative of the Sukhothai side, must have been Sai Līdaiy, since Līdaiy was dead; and this depends on the subassumption that the name ‘Līdaiy’ can only refer to one of those two persons. I intend to offer arguments against this last belief. Finally it has been assumed that Mahādharmarāja II was the reigning king and that he must have been at the time a monk, or retired, or for some other reason had delegated authority to Sai Līdaiy. I hope to show that Mahādharmarāja II was probably already dead and that no other hypotheses are necessary to account for his apparent absence from the action.

In fact, since so much of these inscriptions has been destroyed, their purpose is no longer determinable²⁴⁹ and the most important point for discussion at this stage is the identity of the Sukhothai ruler. The problem is centered on the title ‘Līdaiy’, and the discussion relates both to No. 45 and to certain other inscriptions which are discussed below. ‘Līdaiy’ (ลี้ดัย) occurs unequivocally as the name of two kings, Līdaiy I and Sai Līdaiy, while ‘Lo’daiy’ (ลือดัย, เลอดัย) is found as the name of Līdaiy’s father and, written

²⁴⁶ Chand, “Review Jan. 1973”, p. 297; *Guide*, p. 33

²⁴⁷ Chand, “Review Jan. 1973”, p. 299; *Guide*, p. 68

²⁴⁸ EHS 3; *Guide*; *PP.* 5-6, 68

²⁴⁹ EHS 1, p. 217; EHS 3, p. 67.

เล็ไทย, as the name of a person in the ‘ancestor’ list of No. 45 whose identity has occasioned some disagreement²⁵⁰. Whatever the latter’s identity, the two names appear to have alternated throughout four generations of Sukhothai royalty. I will not try to speculate on the origin or meaning of the two terms, but since the use of vowel signs in the early Sukhothai inscriptions indicates that there was frequently confusion in the use of vowel symbols,²⁵¹ the two titles may have been synonymous, but written differently from generation to generation in order to distinguish the bearers.

Furthermore the syllable เล็ in those days, in Thai, Khmer and Pali was often written เล, Ledaiy, Ledaya, or Ledeyya; and Līdaiy in Pali is Lidaya or Lideyya. There is even one context in which G/P consider Lo’daiy to have been rendered in Pali by ‘Lideyya’,²⁵² which could easily lead to confusion if contexts were not absolutely clear. For some reason it has always been assumed that Mahādharmarāja II could not have had one of these names and that the *Ba Lo’daiy* of No. 45 must have been some non-reigning member of the dynasty, even though his name is of a type that was in other cases given only to reigning kings.

The main point to remember is that the names Līdaiy and Lo’daiy could be confused, particularly when written in Pali; and this will permit us a bit later to see that one of these terms, probably ‘Lo’daiy’, was the name of Mahādharmarāja II, and that he was the *ba lo’daiy* of No. 45. This last point was almost admitted by G/P in one of their later *EHS*, and perhaps they were only held back by the assumptions which they had upheld in their earlier studies.²⁵³

The records of Sukhothai history continue in No. 38, and here I am going to suggest that both Griswold’s and Prasert’s interpretation, and Prince Chand’s critique, have totally confused the context. The date is 1397²⁵⁴ and the opening statement reads: “this capital [Sukhothai] is under the authority of *samtec pabitr mahā rājaputra... rāja śrī paramacakrabarrtirāja* who has

250. *EHS* 3, p. 82, n. 20; Chand, “Review Jan. 1973”, pp. 286-87; *Guide*, pp. 33, 58, 68.

251. *Śilā cāru’k* V, where the vowel system in the inscription of the Jataka panels, Vat Śrī Jum, is incomplete in modern terms. Note in particular the evidence of plates 27 and 34 that the independent vowels ‘ī’ and ‘ī’ (็, ็) may not have been in use at all. See also *EHS* 9, p. 193; *EHS* 11-1, pp. 79-80, 113, 145-6; Prasert, in “*Deuxième Symposium Franco-Thai*”; Bauer, “The Wat Sri Chum Jataka Glosses”.

252. *EHS* 11-1, p. 72, n. 6.

253. See above, text between notes 161 and 162.

254. See *EHS* 4, n. 8 for discussion; and it must be emphasized that the systematic calculation of the several elements of the date proves conclusively that 759/1397 is the correct year and that the day must be *mu’n hmau*. The reader may check this for himself by working through the formulae of Roger Billard, “Les cycles chronographiques chinois dans les inscriptions thaïes”, *BEFEO* LI, 2 (1968), 403-31.

succeeded to the throne in accordance with the royal wish. This sacred kingdom [แดน พระ ธรรมราชสีมานี้] ...” Here, I suggest, G/P have forced the evidence. As their own note 11 admits, **แดน**¹ etc, should really mean, in a Sukhothai context, ‘the *dharmarāja* realm’, that is, the kingdom of Sukhothai, [not Ayutthaya, as G/P would have it.]

In addition to this, G/P neglected to translate **ทดแทน** preceding **แดน** etc, and although it may appear presumptuous to question G/P’s translation, the point is extremely important. In the standard dictionaries **ทดแทน** is glossed, ‘to reward, recompense, replace, a substitute’²⁵⁵ which suggests the idea of succeeding to a realm also²⁵⁶. Modern dictionary definitions would not alone be sufficient basis for asserting this, but in a Thai version of the fifteenth-century *Mūlasāna* we find the expression, **แทน สัมปัติ** meaning ‘succeed to the realm’.²⁵⁷ Thus I feel that lines 2-4 from “this capital” to “Tabatiṅsa” should be read as a single sentence, and the translation should be revised to “.... in accordance with the royal wish succeeding to this *dharmarāja* realm [Sukhothai] like the Tabatiṅsa”. The meaning then is that the *rājaputra* (‘royal son’, ‘crown prince’) succeeded to the throne of Sukhothai, not Ayutthaya.

This *rājaputra* went to Kamphaeng Phet with his retinue, which included his maternal uncle (*mātulā*, *lun*), who had reared him, and this mention of *lun* repeats the familiar structure of No. 102 in which a *lun* figured prominently along with a *rājauras*, synonym of *rājaputra*. It is thus most probable that the *rājaputra* was Sai Līdaiy who was announcing his assumption of full control over the government of Sukhothai.

Also included in the king’s retinue were four officials entitled *brañā* + *baññ* (**พง** with the name of a place, and who seem to have been connected with rivers. Such titles have not been found in any other source. The geographical location of all four is in strictly Sukhothai territory, and the last-named, from Nagar Daiy, is called *bī brañā dān baññ nagar daiy*,²⁵⁸ ‘the elder brother (*bī brañā*) [of] the ruler (*dān*) who *baññ* Nagar Daiy’, [or, possibly, ‘the elder brother ruler who, etc.’, in either case] indicating that *baññ* is to be separated from *brañā* and is probably a verb acting on the following noun. Thus *brañā* + *baññ* could be ‘the *brañā* who *baññ* a certain location. In the absence of other comparable examples no further interpretation is possible, but it is clear that we are faced with a peculiar

255. McFarland, *Thai-English Dictionary*, p. 394; RID, pp. 441, 475.

256. G/P’s ‘succeeded to the throne’ is the translation of “**ขึ้นเสวย**”

257. *Tāmnān Mūlasāsanā*, ed. by Sommai Premchit, Translation Series IX, Dept. of Sociology and Anthropology, Chiang Mai University, Jan. 1976, p. 23.

258. The term *dān* (**ทณ**) here could refer either to *bī brañā*, with the relative sense, ‘who’, or could refer to the king, giving the sense, ‘the elder brother of the king’.

Sukhothai institution which has not appeared from other sources.

G/P found the passage baffling since they had already assumed the inscription to be Ayutthayan,²⁵⁹ but without such an assumption there is no mystery about the context as a whole, even if the details are incompletely understood. The ruler of Nagar Daiy, as ‘elder brother’ of the new ruler of Sukhothai, not Ayutthaya, presents no structural problem, particularly if, accepting G/P’s reasoning, the term need not be taken literally; and it may only indicate respect for an older person.

G/P’s assumption that the inscription had to be Ayutthayan is based first on the text of the law, which bears some resemblance to the “Law on Abduction” (ไต่ขการลักกา) dated B.E. 1899/1355 of the 1805 Ayutthayan law collection²⁶⁰, and perhaps also on the title *śrī paramacakrabartirāja*, a title sometimes used by Ayutthayan kings.²⁶¹

The first consideration is certainly not sufficient, since laws concerning fugitives would have been equally necessary in the societies of both Sukhothai and Ayutthaya, and could have developed independently, although regardless of the degree of interdependence of the two polities, reciprocal influences in all aspects of their higher cultures may reasonably be supposed. The law text itself would only be evidence of Ayutthayan action if it were virtually identical to the Ayutthayan code and if it could be demonstrated that the influence could not have gone the other way. G/P themselves note that the two law texts are quite different, but they attribute this to later interpolations in the Ayutthayan code, which is a reasonable hypothesis but one which must be tested by thorough textual study of the laws before any general historical conclusions may be drawn. G/P might wish to argue that Sukhothai society was not such as to promote the promulgation of such a law, an idea which seems to appear in one of their latest articles.²⁶² Otherwise there is little point in discussing the text of the law here except for one passage which is virtually identical to a passage of the Ayutthayan law.²⁶³

The inscription lists, after Sukhothai, the towns of Jalyañ, Kāmben Bejra, Duñ Yāñ, Pāk Yam, and Sòn Gwae, all squarely within the Sukhothai realm, and prescribes what to do if a slave in one of those places runs away

259. *EHS* 4, p. 129, n. 15.

260. *Laws* III, pp. 1-20.

²⁶¹ On the frequency and patterns of Ayutthayan royal titles see Michael Vickery, “Prolegomena”, and Michael Vickery, “The Constitution of Ayutthaya”.

262. G/P, “On kingship”; their analysis of Sukhothai society is drawn almost entirely from No. 1, which because of its anomalous character cannot be taken as authoritative on such matters. The other Sukhothai inscriptions and the material remains of Sukhothai culture indicate that Sukhothai society may not have been much different from Angkor or later Ayutthaya.

263. *EHS* 4, p. 132; *Laws* III, pp. 1-2.

to another person's house. The Ayutthayan law text also lists Jaliañ, Sukhothai, Duñ Yāñ, Pāñ [Pāk?] Yam, Sòñ Kaev [Gwae], and Kāmbeñ Bejr, plus two other Sukhothai towns, Sahluoñ and Jāvtañrāv, and prescribes what to do if slaves run away to them.²⁶⁴ In the inscription such locations fit logically into the picture of runaway slaves, but they seem quite out of place in the Ayutthayan law text. Since Sukhothai was still independent in 1355, its territory might have been a goal of runaway slaves from Ayutthaya, at least the strongest and most enterprising of them, but surely most runaways would seek refuge in areas less distant; and a meaningful Ayutthayan law would deal with areas within the Ayutthayan king's jurisdiction, not the far-off Sukhothai towns over which he had no authority. If we laid aside all preconceptions, we might offer the interpretation that the Sukhothai law, preserved in the inscription, is the older text; and that later, when Sukhothai had been incorporated into the Ayutthayan system, part of its legislation was directly incorporated into the Ayutthayan code, attributed to earlier Ayutthayan kings, and slight changes made to fit Ayutthayan circumstances.^{264a}

Evidence in favour of careless modification in the original text is the Buddhist Era date of the Ayutthayan law. At that time the common era of Ayutthaya was *śaka*²⁶⁵ and the Buddhist Era date of the law probably indicates tampering with the text at a later date when it was believed that use of the Buddhist Era had preceded the *śaka* and *cula* eras.

For myself a much stronger indication of Ayutthayan identity for the *rājaputra* of No. 38 would be his title *cākṛabarrtirāja*, since I believe patterns in titles are important, whereas G/P do not,²⁶⁶ and *cākṛabarrtirāja* seems to have been used much more consistently at Ayutthaya than elsewhere. However, it was also apparently used at Chiang Mai.²⁶⁷ and patterns of titles did change over time through borrowing of auspicious

²⁶⁴ 'Pāñ' is the Mon equivalent of Thai 'pāk', not unexpected in old Ayutthaya. For another instance of confusion of 'Gwae/Khwae' and 'Kaev' see the review of Jeremias van Vliet, *The Short History of the Kings of Siam*, in this volume, pp. 00-00.

^{264a} For some discussion of this subject, northern influences in Ayutthaya, see "Chris Baker, "Ayutthaya Rising: From the Land or From the Sea", pp. 10-11

²⁶⁵. Nearly all of the extant contemporary Ayutthayan records dating from before the Burmese invasion of 1569 use *śaka*. Most of them are listed or cited in Vickery. "The Khmer inscriptions of Tenasserim".

²⁶⁶. See Vickery, "Cambodia after Angkor", chap. VII; G/P, "Devices and expedients", *In Memoriam Phya Anuman Rajahon*, The Siam Society, 1970, p. 151; and G/P, "A Pali inscription from Vat Śrīratnamahādhātu, Subarnapuri", *Art and Archaeology in Thailand*, The Fine Arts Dept, Bangkok, 1974, p. 4.

²⁶⁷. Coedès, "Documents", pp. 108, 109, 110, 129, where one of Tiloka's titles was 'Siridhammacakkavattibilakarājādhirāja'.

elements from other polities. Sukhothai in the reign of Sai Līdaiy was apparently often weak and subject to influences both from north and south, and its elite could well have been experimenting with borrowed titles and institutions. Thus the title *cākrabarrtirāja* is not sufficient to prove that the *rājaputra* could only have been from Ayutthaya, and it may have been taken purposely by Sai Līdaiy at a time when he was apparently trying to increase, or consolidate, his power within the realm. Moreover the epithet ‘royal son’ is scarcely credible for the Ayutthayan Rāmarāja, who had become full king two years earlier,²⁶⁸ but it and other details fit a Sukhothai pattern which begins with No. 102. The emphasis on ‘royal son’ probably indicates that for some reason Līdaiy had not been allowed to assume full authority at the death of his father and that, as Prince Chand emphasizes, some sort of regency council had prevailed.

Inscription No. 38, then, continues the dynastic picture beginning with No. 102, is solely concerned with Sukhothai matters, and all inferences from it about Ayutthaya-Sukhothai relations are misplaced (**except as noted above, text with note 264a**).

Prince Chand’s main argument over No. 38 concerns the date, which he thinks should be about 100 years later,²⁶⁹ but he also notes pertinently that G/P’s interpretation is essential to their theory of Ayutthayan interference in the affairs of Sukhothai, a theory which fully merits the skepticism Prince Chand appears to feel. He accepts, however, along with G/P, that the *rājaputra* is Ayutthayan, but that he was either Trailokanāth or the latter’s son, Rāmādhīpati II. He says the information on the king’s relatives fits only these two princes, something I fail to understand unless it is based on the idea of Trailokanāth’s mother being a daughter of Sai Līdaiy, a theory I have already shown untenable²⁷⁰.

Prince Chand also says the language of the introductory part is of the fifteenth century and that anyone who can read Thai should be able to see it, although he neglected to point out the diagnostic details. He apparently offered these arguments in an earlier article, for Prasert answered them in two of his own studies²⁷¹. Prasert wrote that he was unable to judge the date of the language, which would indicate that Prince Chand’s reasoning on this point is unclear even to Thai experts.

The next relevant inscription is No. 46 dated 1404. The individuals given prominence are Sai Līdaiy, called *Samtec mahādharrmarājādhīpatī śrī sūryavaṇś*, and his mother; and it appears that they had recently increased the

268. *EHS* 4, p. 128, n. 9.

269. Chand, “Review Jan. 1973”, pp. 287-89; *Guide*, p. 92.

270. See above, pp. 00-00, “Two confusing inscriptions”.

271. Prasert, *op cit*, 49, 52-54. He referred to an article by Prince Chand in the journal **สยามสาร**, Feb 2511 (1968), which I have not read.

territorial extent of the Sukhothai kingdom. G/P have called this a declaration of independence, but such an inference seems exaggerated, particularly if the supposed oath of Mahśdharmarśja II is only an imaginative reconstruction and if No. 38, as I propose, has no connection with Ayutthayan interference in Sukhothai. It is possible, however, that at the beginning of the fifteenth century weakness in Ayutthaya gave Sai Līdaiy the chance to enlarge his kingdom²⁷².

We should again note No. 9, which two years later in 1406 gives the same political prominence to the queen mother that is found in No. 46.

According to G/P and Prince Chand the next inscription of interest, No. 49, depicts the visit of an Ayutthayan king to Sukhothai in 1417, although Prince Chand thinks it was a mere social visit while G/P claim it represents the reassertion of Ayutthayan suzerainty over Sukhothai.²⁷³

Among the persons figuring in the inscription we find the king of Sukhothai, Sai Līdaiy, who is first entitled *bò ayū hua cau da òkyā dharrmarāja*, then more simply *cau bra:yā* and, *bò ayū hua cau*, all of which G/P take to mean he was a vassal ruler. In 1412 he gave permission to build a temple. Then in 1417, when the temple had been built, there was a visit by *braḥ param rājādhipatī śrī mahā cāk[ra]battirāj* together with his queen mother, and his maternal aunt. According to G/P and Prince Chand this was the royal family of Ayutthaya, but is such an interpretation necessary? Among the royal titles we find *rājādhipatī*,²⁷⁴ which was among the titles of Sai Līdaiy in No.46, and the element *cāk[ra]battirāj* which, I have argued, was adopted by Sai Līdaiy in No. 38. Moreover, Sai Līdaiy's mother was given prominence in Nos. 46 and 9, and here again the queen mother accompanies the ruler. The third royal person is a maternal aunt, and in No.102 an aunt seemed to be a guardian of the much younger Sai Līdaiy. Furthermore, a maternal uncle of the king (the *òkyā dharrmarāja*), who was a monk, is also given an important role in the inscription, and in both Nos. 102 and 38 we have noted such an uncle, not yet a monk, as one of the major figures close to Sai Līdaiy. Thus all of these inscriptions seem to show a similar royal family structure which is strictly that of the family of Sukhothai.

One more character of No. 49 who deserves a note is Nāy Inda Saraśakti, the man in charge of the construction of the temple, and whom G/P, but not Prince Chand, call the 'Ayudhyan Chief Resident' in Sukhothai. We should recall, however, that in No. 102 there was a certain "nāy named

272. See *EHS* 1.

273. *EHS* 1; Chand, "Review Jan. 1973", pp. 273-74; *Guide*, pp. 74-75.

274. *rājādhipatī* was also an Ayutthayan title: see Vickery, review of van Vliet, p. 00 in [this volume](#), where I did not argue against G/P's interpretation of No. 49. Since it is clear that *rājādhipatī* in 46 refers to Sai Līdaiy, it is proof that certain titles at that time were shared by Sukhothai and Ayutthayan royalty.

Āy Ind” who also took charge of temple construction over 30 years earlier, and it is quite likely that it is the same individual with a higher rank denoted by the title *saraśakti* who figures in No. 49. Rather than ‘Ayudhyan Chief Resident’ he was probably something like chief municipal engineer of Sukhothai.²⁷⁵

Skeptics might still argue that the different royal titles show that two different rulers are involved in the story. I do not think this is a necessary conclusion. Inscription No. 49 is admitted to contain several peculiarities of language, and as a matter of method, until they are explained in general no single one of them may be picked out and arbitrarily given a special meaning.²⁷⁶ We should also remember, as I noted earlier, that the weight of later evidence is that vassal rulers continued to hold full royal titles. Finally, the inscription was explicitly erected by the Mahāthera, uncle of the Sukhothai king, and Nāy Saraśakti, who had served the royal family for half a lifetime. It would be quite natural for these men to refer to Sai Līdaiy by short titles equivalent to ‘the king’ in the sections devoted to the preparations for construction and to then use his full titles when recording his official visit.

I would thus conclude that No. 49 bears no evidence for Sukhothai-Ayutthaya relations. [*I would now (2012) modify this statement. No. 49, in my interpretation, at least shows that in both polities some of the same royal titles, and not those found nearly everywhere, but relatively unusual titles such as *rājādhipatī* and *cākrabarrtirāj*, were in use, perhaps indicating cultural borrowing--but in which direction? The traditional view is that Ayutthaya was imposing its influence on Sukhothai, but as Chris Baker has convincingly argued, it may have been that the Sukhothai region, including not only that city, but also Phitsanulok and Kampaeng Phet, was seen as a model worthy of imitation by an Ayutthaya intent on reinventing itself as an inland kingdom rather than as merely a coastal trading region.²⁷⁷ *]

An inscription which will be more difficult to fit into the revisionist picture is No.93 of Wat Asokārām concerning which there are serious differences of opinion between Prince Chand and G/P. It includes the date

275. See above n. 232. In a review of Yoneo Ishii *et al*, *A Glossarial Index of the Sukhothai Inscriptions*, I argued against his being ‘Ayutthayan Chief Resident’ on other grounds, which I now realize may not be so solid as I once thought, due to the higher rank of such titles as ‘*nāy*’ and ‘*khun*’ in earlier times (see Vickery, “The 2/k, 125 Fragment”, p. 54). I still deny that he was Ayutthayan Chief Resident, but think the comparison of Nos. 102 and 49 provides a better argument, and my earlier remarks about ‘*saraśakti*’ would still hold.

²⁷⁶ *EHS* 1, pp. 232, 238, n. 62; Chand, “Review Jan/ 1973”, p. 272; Vickery, review of *A Glossarial Index of the Sukhothai Inscriptions*, pp. 58-59. Note that in No. 93, *EHS* 2, p. 46, n. 32, G/P accept ‘*bò ayū hua*’ as the title of an independent king

²⁷⁷ Chris Baker, “Ayutthaya Rising, From the Land or from the Sea”.

1399 at which time all the authorities agree that Sai Līdaiy was king of Sukhothai.

The principal personage is the founder of the monastery, entitled *samtec braḥ rājadebīsrī cuḷalakṣaṇa*. G/P have decided that she was wife and half-sister of Mahādharmarāja, Sai Līdaiy's mother, and thus identical to the *rājamātā* of No. 46, while Prince Chand says she was Līdaiy's second queen and mother of Mahādharmarāja II.²⁷⁸

In the inscription this lady is called *jāyā*, 'wife', or as G/P say, 'consort', of Samtec Mahādharmarājādhirāja. In their note 10 G/P also say 'consort' really means 'widow', since they have decided that she must have been consort of Mahādharmarāja II, already dead in 1399, but there is no precedent for such an interpretation of *jāyā*. The term *jāyā* plus *arrgarājamahesī*, 'chief queen', of her titles, can only mean she was chief queen and consort of the reigning Mahādharmarāja, that is Sai Līdaiy²⁷⁹.

This identification is supported by two other phrases which G/P have treated in a somewhat arbitrary manner. Within the space of a few lines there is mention of a *mae ayū hua* building temples and a *bò ayū hua* becoming a monk. G/P translated these titles, normally, as 'queen' and 'king' and identified them respectively as Śrī Cuḷalakṣaṇa and Sai Līdaiy. However, if *bò ayū hua* thus means the reigning king, which is easy to accept, then *mae ayū hua* should logically be his consort, not his mother, and if *mae ayū hua* is, as G/P say, Śrī Cuḷalakṣaṇa, then by a second chain of reasoning she was consort of Sai Līdaiy.²⁸⁰

I would also suggest that G/P have distorted the meaning of *samtec bò òk*.²⁸¹ As they admit, this title would normally refer to the lady's own father, but this will not do in view of their decision about her own identity; and the distortion is carried through to their identification of the deceased Samtec Braḥ Rājamātā and the translation of the list of people receiving merit from Śrī Cuḷalakṣaṇa's good works²⁸². On their reading of this passage depend also their remarks, page 37, about the sibling relationship between Śrī Cuḷalakṣaṇa and Mahādharmarāja II.

Here Prince Chand's translation, if not his interpretation, is definitely superior. The merit was dedicated to *samtec pū braṇā* (Līdaiy), *bò òk* (her own father), *mae òk* (her own mother), *samtec mahādharmarājādhirāja*, and *braḥ śrī dharmarājamātā*. G/P consider the last two to be Mahādharmarāja

278. *EHS* 2, pp. 37, 48, n. 41; Chand, "Review Jan. 1973", p. 274; *Guide*, pp. 69-71.

279. *EHS* 2, p. 44. The reader should not be misled by '*mādapravara*', which G/P left untranslated. I have no suggestion for it either, but *māda* is not the word for 'mother', which is *mātā*.

280. *EHS* 2, p. 46.

281. *EHS* 2, p. 47, n. 35.

282. *EHS* 2, p. 48.

II, the donor's husband, and the latter's mother, both dead; while Prince Chand says they were Sai Ḷīdaiy and **his** mother, both alive.

In most cases transfer of merit only concerns those who are already dead, and thus Prince Chand's suggestion would be unlikely. He argues that a clear case of transferring merit to someone still alive is found in No. 49, but there Nāy Ind only states that he **would** transfer to the king (perhaps only after the latter's death) the merit accruing from a temple which he **intended** to build. The situation is thus quite different and in fact in No. 49, which is complete and which records the completion of the temple, there is no statement about really transferring merit, perhaps because the king was still alive²⁸³.

I think we must accept that the Mahādharmarāja in question cannot be the living Sai Ḷīdaiy, but must be an earlier Mahādharmarāja, and the Brah Śrī Dharmarājamātā must be Sai Ḷīdaiy's mother²⁸⁴.

This revision, which seems fairly straightforward from face I, becomes much more complicated by the evidence, and lacunae, of face II. The latter is in Pali and there is considerable controversy over its interpretation.

The first difficulty is in Griswold's and Prasert's rendering of lines 5-12, which is an interpretive paraphrase rather than a translation, the latter being partially provided in their note 53.²⁸⁵ Taking the important elements in literal order we have, "in 730/1368 Dharmarājādhirāja of Lidayarāja's... queen Śrī Dharmarājamātā... was born" (lines 5-9). Now G/P and Prince Chand have assumed that 'Lidaya' can only be one individual, the king reigning between *circa* 1347 and 1374, and they also implicitly assume, with a logic that quite escapes me, that since the name of Mahādharmarāja II has not been preserved it could not have been Ḷīdaiy. As I have shown above Pali 'Lidaya'/'Lideyya' could represent either 'Lo'daiy' or Ḷīdaiy, and there is no reason why Mahādharmarāja II could not have had one of these names. If the Ḷīdaiy of this inscription was Mahādharmarāja II then the Dharmarāja who was born in 1368 would have been Sai Ḷīdaiy, and his mother is seen to have had the same basic titles as in the unequivocal No. 46.

The date fits very well into the known period of Sai Ḷīdaiy's life and so do some of the other passages of the inscription. It continues, "when he was sixteen years old", and then there is a one-line lacuna so we do not know what happened in 745-46/1383-84, but two other inscriptions allude to the importance of that year and thus mention of the date fits an already established pattern without hazarding guesses as to its precise meaning²⁸⁶.

283. Chand, "Review Jan. 1973", p. 276; *EHS* 1, p. 233, lines 5-6, p. 236.

284. Prince Chand, however, does not accept Mahādharmarāja II as father of Mahādharmarāja III, which is discussed further below.

285. *EHS* 2, p. 50.

286. *Pace* G/P, *EHS* 2, p. 50, n. 54. The other two inscriptions are Nos. 106 and 94.

Then in line 13 we find “when he was thirty-eight, in the year seven hundred...”, followed by a lacuna where the rest of the date should be. I would accept the metrically correct interpolation of *aṭṭasatti*, ‘sixty-eight’, making a date 768/1406. G/P cannot accept that because of their belief that the inscription was written in 1399, and their insistence that the Mahādharmarāja concerned was not Sai Līdaiy, but Mahādharmarāja II, already dead by that date²⁸⁷. Prince Chand calls attention to another reason why 1368 would be unacceptable for the birth of Mahādharmarāja II in G/P’s genealogical scheme. It would make him only 23 years old in 1391 at the birth of his grandson Mahādharmarāja IV, son of Sai Līdaiy, and such a genealogy is impossible²⁸⁸.

Prince Chand’s own argument is that “1399 was the foundation date of the *wat*, but not necessarily the date of the inscription”, and he implies that the latter date could have been as late as 1406, on both of which points I am in full agreement. He also insists, rightly, that reconstruction of the lacuna in lines 13-14 of the Pali face must obey the metre, and that the date must therefore be 768/1406.²⁸⁹ Since Prince Chand, however, like G/P believes the Mahādharmarāja concerned to be the second, and since he realizes that a genealogy Līdaiy - Mahādharmarāja II - Sai Līdaiy - Mahādharmarāja IV is not possible with this assumption and with the dates of No. 93, he postulates an entirely different family structure in which Līdaiy - Ba Lo’daiy (No. 45) - Sai Līdaiy would represent the main line of the family with Sai Līdaiy taking the throne in 1379, the year of Līdaiy’s death, and in which Mahādharmarāja II would be a half-brother of Ba Lo’daiy and a contemporary of Sai Līdaiy.²⁹⁰

This solution, which is a speculative reconstruction, forces Prince Chand into further epicyclic reasoning to tie up loose ends, such as the question why Ba Lo’daiy never reigned and is not mentioned anywhere but in No. 45, and the problem of Mahādharmarāja II being given such a title at birth, even though in Prince Chand’s own explanation he did not reign at all. For an answer to the first problem he produces a story in which Ba Lo’daiy, and also Nāṃ Mo’aṅ, another figure of No. 45, are killed off in the war of 1378,²⁹¹ but this is precisely the type of story which, having no supporting evidence at all, is best avoided by the historian. For the second problem Prince Chand postulates a rivalry between the women whom he has identified as the two queens of Līdaiy, Rājamātā of No. 46, mother of Sai

287. *EHS* 2, p. 50, n. 55.

288. Chand, “Review Jan. 1973”, p. 278; *Guide*, p. 69. The date of birth of Mahādharmarāja IV may be inferred from No. 12.

289. Chand, “Review Jan. 1973”, pp. 278-80; *Guide*, pp. 68-70.

290. *Guide*, pp. 71-73.

291. *Guide*, pp. 33, 73.

Līdaiy and Śrī Culālakṣaṇa of No. 93, mother of the contemporary Mahādharmaṛāja II; and he says that the title ‘mahādharmaṛāja’ for the prince whom we know as the second of that name, and who never really became king, is a false claim by his mother which is found only in No. 93. As evidence for such rivalry, he points to different styles in the temples they built and supposedly different territories claimed by Nos. 46 and 93²⁹². But differences in architectural style in no way imply political rivalry, and with respect to the claims of territory, the passage of No. 46 is fragmentary, that of No. 93 is poorly understood and probably adjusted to the exigencies of Pali metre, and the two statements, both made about 1406 according to Prince Chand’s and my readings, could easily be understood as referring to the same general area.²⁹³ Of course Prince Chand’s main point here was to argue against G/P’s contention that the queens of Nos. 46 and 93 were the same person. To that extent I agree with him, but otherwise I feel that his reconstruction is based on very tenuous speculations and must be rejected.

One other suggestion of Prince Chand, that the Prince Asoka of No. 93 went to Laṅka to obtain the relics, must also be rejected.²⁹⁴ He has evidently been misled by the Thai translation of the inscription in *Śilā cāru’k* IV, which in fact makes it appear that Asoka went to Laṅka, but the structure of the Pali text and G/P’s translation show that this is impossible. [the inscription only says that there were two relics which had come from a place which G/P, via an arbitrary emendation (not justified by a break in the text) identify as Ceylon.²⁹⁵

Griswold’s and Prasert’s reconstruction is based on an assumption about the identity of ‘Lidaya’ shared by Prince Chand; a belief that all the dates of the inscription must be earlier than 1399, which both Prince Chand and I reject;²⁹⁶ and a conviction that Mahādharmaṛāja II was dead in 1399, which I, but not Prince Chand, would also accept.

Let us first take the question of the date. As Prince Chand pointed out, 761/1399 is only mentioned as the date at which the temple was built, and in face I other things are mentioned as having occurred later. Thus, (a) four

292. *Guide*, pp. 70-71, 73.

293. See *EHS* 1, p. 225, lines 7-9, p. 227; and *EHS* 2, p. 51. Note also that in Angkorean inscriptions such mundane details are often incomplete in the Sanskrit portion due to metrical requirements and are more accurately rendered in the Khmer passages.

294. Chand, “Review Jan. 1973”, p. 280; *Guide*, pp. 70, 73.

295. *Śilā cāru’k* IV, p. 55, line 10 from the top; *EHS* 2, p. 42, lines 33-41 and p. 52, from II/29-34 through II/35-42. [In the inscription the toponym in question is *pañṇipūra*, which G/P said was *tambapañṇipūra*.] I should make clear here that I have no pretensions to be a Pali scholar, and only claim sufficient basic Pali to identify passages in a translation with the corresponding passages of the original.

296. Chand, “Review Jan. 1973”, pp. 278, 280.

months later an image of the Buddha (lacuna, “was erected”), and four months after Phagguna/Phalguna is already into the next year, 762/1400, (b) then sacred texts were assembled, and (c) then people and land were donated to the temple. Following this the inscription relates that the same lady also built six other temples and a monastic residence; and, *pace* G/P, there is no necessity to assume that they were all built earlier than the Asokārāma (their note 37 is based on all the assumptions about identities of individuals which I am attempting to disprove). It is clear that the inscription was set up later than the date it includes, in the absence of any further date in face I the intervening time could have been a matter of years, and there is no conflict with the metrically correct conjecture that the missing date of face II should be 768/1406. Indeed the boundaries mentioned at that time, although in Palicized form and not entirely clear, fit the picture of Sai Līdaiy expanding or taking stock of his realm as in No. 46, dated 1404. Once this feature of the dating is recognized there is no longer any temporal objection to identifying Sai Līdaiy as the prince whose birth is recorded in 1368 and Culālakṣaṇa as his queen.

We must now return to the two passages transferring merit to other members of the donor’s family.²⁹⁷ Prince Chand feels that G/P have ‘doctored’ their version and I consider his criticism to be just. However, I do not agree with Prince Chand that both passages should contain the same details, and that the Pali text should be interpreted to conform to the Thai.²⁹⁸ As a literal translation, and that is what we must first work with, Prince Chand’s rendering of the Thai passage, illustrated above, is far superior to that of G/P. However, since literal translation forces us to understand the donor, Śrī Culālakṣaṇa, as queen of Sai Līdaiy, and since standard practice forces us to assume that all the persons mentioned were dead, we must take the last two individuals mentioned as a Mahādharmarāja preceding Sai Līdaiy, presumably his father, and his mother, who is given part of the same titles as found in No. 46. Since No. 49, as I read it, shows this lady still alive in 1417, No. 93 could only have been written after that date.

As for the passage in the Pali face, which according to both Prince Chand’s and my own readings would have been later than 1399, and perhaps even much later, the donor, still the same lady, refers to her parents, and then to her husband in a phrase, *sāmiko me mahādharmarājādhirājanāmako*’, and finally to the “royal mother”, using the same ‘Śrī Dhammarājamātā’ as found in the Thai face and in No. 46. In this case Prince Chand’s interpretation that *sāmiko* and *mahādharmarāja* are to be separated and made into two persons is just not tenable.²⁹⁹ The Pali phrase, literally “my husband named

297. *EHS* 2, pp. 48 and 55 for G/P’s translations.

298. Chand, “Review Jan. 1973”, p. 276; *Guide*, pp 69-70.

299. *Guide*, p. 70.

mahādharmarājādhirādhirāja, is quite clear and we must prefer the rendition of G/P, who also assume that the individual named was dead.

Where my reading differs from G/P is in identifying the donor's husband as Sai Līdaiy instead of Mahādharmarāja II; and since we know the date of Sai Līdaiy's death, 1419, this leads to the conclusion that the Pali side, but not the Thai side in which Sai Līdaiy is clearly alive, was not written until after that date. With this conclusion there is no longer any problem with Queen Culālakṣaṇa's son being called Mahādharmarāja.³⁰⁰ He was Sai Līdaiy's son, Mahādharmarāja IV, who would have been on the throne at the date the Pali inscription was written.

Thus without any interpretive paraphrases instead of translation, without any epicycles, and with only two assumptions (since 1399 records the beginning of the activities rather than their end), the inscription could have been erected within any time period which could reasonably fit into the lifetime of Śrī Culālakṣaṇa; and since no other name has been recorded for Mahādharmarāja II, there is no objection to accepting him as the 'Lidaya' of the Pali face), we can achieve a reading of No. 93 which fits it into the picture based on earlier inscriptions and which is, I submit, much more acceptable from the point of view of historical method.

As for the Prince Asoka of No. 93, speculation about him and the significance of his name is not very helpful in the absence of all evidence. It is in any case not a unique instance of the name. 'Asoka' had been used by royalty in central Siam since the twelfth century,³⁰¹ and just because so few individuals are named in Sukhothai inscriptions we may not therefore assume that there were not many more princes in each generation.

Some conclusions

I stated earlier that all conclusions about the end of Līdaiy's reign and the reign of Mahādharmarāja II depend on the inscriptions of the Sai Līdaiy period, and since the latter have been discussed it is time to go back to the earlier problems.

Since there is no unequivocal statement about the death of Līdaiy, and since there is hardly any unequivocal evidence for Mahādharmarāja II at all, the reader might still find part of Prince Chand's theory attractive: Līdaiy died in 1379 and was succeeded by Sai Līdaiy, still a minor. The *rāj auras* of the latter's title would refer to his relationship with Ba Lo'daiy and there

300. That is, the problem of why he was apparently given such a title at birth. See Chand, "Review Jan. 1973", p. 280.

301. My original published text had, erroneously, 'thirteenth' century. See Coedès, "Nouvelles données épigraphiques sur l'histoire de l'Indochine centrale", *Journal Asiatique* CCXLVI, 2 (1958), pp. 125-42, [including the 1167 AD Nakon Sawan inscription in Pali and Khmer]; and Coedès, *Recueil*, Nos. XIII and XIV.

would have been no Mahādharmarāja II at all. No. 9 precludes that scenario, for it unequivocally calls Līdaiy ‘grandfather’ and Sai Līdaiy ‘grandson’; and there is as yet no permissible way to reinterpret these terms. There was thus an intervening generation whose representative must have been considered king in order for his son Sai Līdaiy to be called ‘*braḥ śrī rāj auras*’; and there is no objection if we continue to use the title ‘Mahādharmarāja II’.

The only thing we can say for certain about the time period of his reign is that it would have begun at the death of Līdaiy in 1368, or 1374, or 1379, and that it must have ended by 1397 when Sai Līdaiy in No. 38, still entitled ‘crown prince’, appears to be asserting his full authority. The total lack of information about Mahādharmarāja II and the uncertainty in No. 102 about who was ruling in Sukhothai between 1379 and 1389 lend credence to Prince Chand’s supposition that, for a large part of Sai Līdaiy’s reign, power was in the hands of a regency.³⁰² However, since Sai Līdaiy was still ‘crown prince’ or ‘heir apparent’ long after attaining his majority, and only asserted full authority in Sukhothai in 1397, perhaps together with a regency or without it, there were serious conflicts among factions of Sukhothai royalty or among the Sukhothai cities, and Mahādharmarāja II may have been a real ‘*roi fainéant*’. This is a speculative reconstruction, based on very incomplete evidence, and I shall carry it no further. All we can say for certain is that Sukhothai history between the 1360s and the 1390s is very unclear, the kingdom appears to have suffered a decline, and nothing of importance was accomplished by Mahādharmarāja II. This supposed decline of Sukhothai may well have been related to the constant Ayutthayan pressures recorded in *LP* and other chronicles, but I think it should be clear now that the inscriptions are concerned with strictly internal Sukhothai affairs and their factual content may not be integrated into any scenario of Ayutthayan expansion.

As a positive contribution it is perhaps possible to establish more closely the date of death of Mahādharmarāja II. This will be based on three inscriptions, Nos. 106, 94, and 93, all of which give a special importance to the year 1383-84.

In No. 106 a certain gentleman whose wife may reasonably be assumed to have been foster-mother to a prince is seen giving thought to basic existential problems early in the year 1384. Although not explicit, this year must have had some special significance, since, as I have shown, it was not the date of the inscription.

Inscription No. 94, the short gold plate reproduced in *EHS* 11-2, pp. 124-25, says a Saṅgharāja, who had been teacher of a certain

302. *Guide*, pp. 71, 76; and Chand, “Review Jan. 1973”, p. 295.

Mahādharmarāja, had just built a stupa for the relics of that Mahādharmarāja, whom Prince Chand considers could have been either Līdaiy or Mahādharmarāja II. If Līdaiy died between 1368 and 1374 (G/P), or even in 1379 (Chand), 1384 seems rather late to consecrate his relics. But if Mahādharmarāja II had just died this circumstance would fit the picture of the inscription much better.

Finally, No. 93 notes some important occurrence for the life of Sai Līdaiy in 1383-84, although the details have disappeared from the stone. None of these statements are at all specific, and although it gets dangerously close to the type of speculative reconstruction against which I have been warning, I would like to offer the hypothesis that the important event of 1384, or slightly earlier, was the death of Mahādharmarāja II.

With respect to No. 94, however, Prince Chand's preference, as well as Griswold's and Prasert's is that it refers to Līdaiy. Could we then not suppose that Līdaiy himself had lived until 1383-84, and that the reign of Mahādharmarāja II was from 1384 until sometime in the 1390s? Then the *braḥ śrī rāj auras* of No. 102 would be Mahādharmarāja II, he might still have been alive, but retired, as G/P say, in 1393, date of No. 45, and he would only have died shortly before 1397 at which time **his** *mahārājaputra*, Sai Līdaiy, asserted his authority in Sukhothai.

This would be an acceptable reconstruction if we wished to ignore, as Prince Chand does, the statement of *Jinakālamālī* that a certain Dhammarāja had died during events which seem to be dated by *LP* in the 1360s or 1370s. I have discussed this portion of *Jinakālamālī* above, and pointed out some of its weaknesses, but it is so close to other, better, sources, that its statements may not be ignored. I therefore agree with G/P that on the basis of *Jinakālamālī* we must date the death of Līdaiy somewhere between 1368 and 1374, and in such case the *braḥ śrī rāj auras* of 1379 can only be Sai Līdaiy.

One more positive contribution centers on the personal name of Mahādharmarāja II. No. 93 shows that it was something which could be Palicized as 'Lidaya', and Pali *lidaya* could represent both the Thai Līdaiy and Lo'daiy. G/P have already accepted that the mysterious *bo lo'daiy* of No. 45 was of the same generation as Mahādharmarāja II, perhaps an 'elder half-brother'.³⁰³ The only reason for not identifying him directly as Mahādharmarāja II was Griswold's and Prasert's view of No. 93, which I have attempted to show untenable, and I believe we must accept that the Lidaya of No. 93, hitherto known only as Mahādharmarāja II, was also the *ba lo'daiy* of No. 45, with the term 'ba' indicating that he was of the father generation to the reigning king Sai Līdaiy.

303. See note 241 above.

Later Sukhothai

Prince Chand's chapters 8 and 9 continue beyond the reign of Sai Līdaiy into a period which G/P have treated, but not exhaustively, in their study of 'Yuan Pai' (YP)³⁰⁴ and in Griswold's "Yudhiṣṭhira". Since Prince Chand generally approves of those two studies, and since there are few inscriptions, there is much less controversy, which is not to say that the history of this late Sukhothai period has been explained in a more satisfactory manner than the early reigns. Details about which there is general agreement are the death of Sai Līdaiy in 1419 as recorded in *LP* and the ensuing reign of his son, Mahādharmarāja IV, who finds mention in No. 12, dated 1426, and whose reign may have come to an end in 1438 when *LP* seems to be noting direct Ayutthayan interference in Sukhothai affairs, although the nature of this Ayutthayan contact with Sukhothai may have been different from its portrayal in the standard histories.³⁰⁵ Beyond this the scarcity and nature of the sources permit conflicting conjectures which cannot yet be resolved.

Part of Prince Chand's chapter 8 concerns art and architectural matters which I deliberately ignore in the present review, but most of it is Prince Chand's own narrative of the "Union of Ayudia and Sukhothai", a history of the relations among Sukhothai, Ayutthaya and Chiang Mai in the last three quarters of the fifteenth century. It is based on the chronicles and its acceptability will depend on a full analysis of those texts.

Only a few critical remarks need to be made here. In the "Genealogical Table VII", page 78, Prince Chand again shows Trailokanāth born of a Sukhothai princess, something I discussed above; and he indicates that 'Phya Rām', mentioned in the *LP* entry of 781/1419, was appointed as 'Phya Chalieng', a detail for which there is no evidence. He also states unequivocally that Yudhiṣṭhira was son of this Phya Rām, and he seems to believe that the inclusion of 'rāmarajissara' (in Pali) among Yudhiṣṭhira's titles shows that his father's name was Rāma.³⁰⁶ However, the frequent occurrence of 'rāma' in the names or titles of numerous kings in several polities eliminates the possibility of attributing any specific significance to it. Griswold originally settled for "scion of the old ruling family", and G/P have more recently suggested he was "son of the deceased king", Mahādharmarāja IV;³⁰⁷ and we must conclude that his parentage may not be determined with any certainty. Prince Chand also repeats his ideas about No. 40, which I

304. G/P, "Yuan phai".

305. *LP*, entries for *cula* 781, 800; *EHS* 1, p. 241; Vickery, "The 2/k. 125 Fragment", pp. 75-76; Baker, "Ayutthaya Rising, from the Land or from the Sea"

306. *Guide*, pp. 78-79.

307. Griswold, "Yudhiṣṭhira", p. 221; G/P, "Yuan phai", p. 132.

have discussed above³⁰⁸.

Although Prince Chand accepts Griswold's "Yudhiṣṭhira" for much of the fifteenth-century story, he gives somewhat more emphasis to a difficulty which Griswold tended to gloss over and which illustrates the pitfalls of reconstructing history from the chronicles without first undertaking their full critical analysis.

The problem is a battle between the northern and southern forces recorded in *CMC* in 819/1457 and in *LP* in 825/1463³⁰⁹. It is obvious that one or the other date is wrong, which both Griswold and Prince Chand admit, but Griswold considered it of "little importance", while Prince Chand realizes that it spoils the whole reconstructed story.³¹⁰ Besides this, if a comparative table of events in *LP* and *CMC* is set up it will be seen that the *LP* entry of 825 may also be related to the *CMC* entries of 821 and 813, which latter are also related to *LP* entries of 822/23 and 813 respectively. If *LP* is taken as the most accurate framework, as has been done by most historians, then *CMC* for this period is a confusing jumble and its details may not be inserted directly into the *LP* structure.

A small error occurs in Prince Chand's reference to the Wat Chulamani/*cuḷāmaṇī* inscription. It is not just the "last two paragraphs", but the entire inscription, which dates from 1680-81 (not 1679) even though it purports to record information from the fifteenth century.³¹¹

In Prince Chand's chapter 9, he first reviews the history of Ayutthaya as recorded in *LP* and notes appropriately that some of its entries are obscure.³¹² He again insists that No. 38 belongs to this period.³¹³ but otherwise his remarks on the inscriptions of the late fifteenth and early sixteenth centuries, which are too few to provide a coherent historical picture, are not controversial, and his only excursion into unsupported conjecture is in the final paragraph, on events of the first quarter of the sixteenth century, which he admits is only a hypothesis.

As a final comment I can only emphasize that the history of Sukhothai, in a form acceptable to modern historians, remains to be written. The difficulty is not in the literal understanding of the sources, for surely Prince Chand and Dr. Prasert could come to terms on satisfactory readings of even the most difficult Thai texts. The barrier to an acceptable history of Sukhothai has so far been in certain rather traditional attitudes about what is

308. *Guide*, pp. 79, 81; and see above, "Two confusing inscriptions".

309. Griswold, "Yudhiṣṭhira", pp. 223-24; *Guide*, pp. 83-84.

310. See also G/P, "*Yuan phai*", p. 135.

311. *Guide*, p. 86; *PP I* (Guru Sabha ed.), pp. 160-61; *PCSA*, pp. 32-33; Vickery, review of *PCSA*, 323-24.

312. *Guide*, pp. 91-92.

313. See above, n. 242.

important in history, and to what extent the historian may give rein to his imagination in recreating the events of the past. It may be that the sources are altogether insufficient to write history at the biographical and political level, at which attention is devoted to the actions and interactions of individuals or to the attitudes and policies of the ruling families of Sukhothai, Chiang Mai and Ayutthaya. It is undoubtedly the concentration on this aspect of history which has led to the very speculative, conjectural reconstructions which characterize the works reviewed here. I would suggest that it may be necessary to move to a more abstract, and possibly more interesting, level and to devote our attentions to the implications of material remains for social and economic life; and some of the important problems would be (a) whether new economic forces affecting all of Southeast Asia caused the growth of Ayutthaya and its expansion against Sukhothai,³¹⁴ (b) whether control of pottery manufacture and trade routes played an important role in the fifteenth-century wars between Sukhothai, Ayutthaya, and Chiang Mai; and (c) to what extent Ayutthaya changed as it gradually absorbed the more Thai areas in north-central Siam in the fifteenth-sixteenth centuries.³¹⁵ The inscriptions and chronicles will have an important role to play in the study of all such questions, but they must be treated in a much more rigorous manner than has been done to date, and many of the problems to which Mr. Griswold, Dr. Prasert and prince Chand gave greatest attention may not be soluble due to lack of sufficient evidence in extant sources; and, in fact, they may no longer be of much interest.

314. See Wolters, *The Fall of Srivijaya in Malay History*, chaps. 3, 4, 5, 12; Vickery, "Cambodia after Angkor", pp. 509-22; Vickery, "The 2/k.125 Fragment", pp. 79-80.

³¹⁵ An important contribution to this subject is Chris Baker, "Ayutthaya Rising, from the Land or from the Sea." The ethno-linguistic character of 15th-century Ayutthaya was certainly Khmer and Mon, not Thai

